Hasse – Davenport

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Manuskripte Hasse–Davenport

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Letters Hasse–Davenport

1.1 07.12.1930, Davenport to Hasse

Davenport introduces himself.

TRINITY COLLEGE
CAMBRIDGE.

7.12.1930

Dear Prof. Hasse,

Prof. Mordell has told me of your letter to him, in which you say you would like to know of an advanced English student of pure mathematics, whom you could invite to Marburg next summer term. May I offer you my services ?

I used to be a student of Prof. Mordell's at Manchester, but for the last three years I have been studying here. I am particularly interested in the analytical theory of numbers — Gitterpunktprobleme, ζ–function, etc. Are you interested in these subjects, or is there anyone else at Marburg who is ? So far I have only written two short papers, which will appear soon in the Journal of the London Mathematical Society; one on the distribution of quadratic residues (mod p), the other on Dirichlet's L–functions.

I am 23 years old, and not at all 'handsome' (as you required in your letter). Also I do not swim or drink beer — and I understand that these are the principal recreations of Germany.

I apologize for writing in English, but my German is rather imperfect.

I am,
Yours faithfully,

Harold Davenport.

1.2 01/1932, Davenport to Hasse

Treatment of $ax^m + by^n \equiv c$. Littlewood's tea party. Not improved on 5/8 for cubic sum. New: 2/3 for generalization of Kloosterman sum.

January 1932[1]

My dear Helmut,

I promised to send you my treatment of the congruence

$$(1) \qquad ax^m + by^n + c \equiv 0 \pmod{p.}$$

Let $\chi_1, \ldots, \chi_{m-1}$ be the non–principal characters for which $\chi^m = \chi_0$, the principal character. It is easily seen that

$$1 + \chi_1(t) + \cdots + \chi_{m-1}(t)$$

is precisely the number of solutions of $x^m \equiv t$. Hence the number of solutions of (1) is

$$N = \sum_t \{1+\chi_1(t)+\cdots+\chi_{m-1}(t)\}\{1+X_1(-\frac{at+c}{b})+\cdots+X_{n-1}(-\frac{at+c}{b})\}$$

where $X_1, \ldots, X_{n-1}$ are the n.–p. characters for which $X^n = \chi_0$. Hence

$$N = p + \sum_{r=1}^{m-1}\sum_{s=1}^{n-1}\sum_t \chi_r(t) X_s(-\frac{at+c}{b}).$$

The sums in t can be easily expressed in terms of generalized Gaussian sums

$$\tau(\chi) = \sum_\nu \chi(\nu) e(\nu), \quad e(x) = e^{\frac{2\pi i x}{p}}.$$

These have the property

$$\overline{\chi}(u)\,\tau(\chi) = \sum_\nu \chi(\nu) e(u\nu).$$

[1]The date is handwritten by Hasse.

Hence

$$\begin{aligned}
\sum_t \chi(t) X(at+c) &= \frac{1}{\tau(\overline{X})} \sum_{t,\nu} \chi(t) e((at+c)\nu) \overline{X}(\nu) \\
&= \frac{\tau(\chi)}{\tau(\overline{X})} \sum_{\nu} \overline{\chi}(a\nu)\, \overline{X}(\nu)\, e(c\nu) \\
&= \frac{\tau(\chi)\,\tau(\overline{\chi}\,\overline{X})}{\tau(\overline{X})}\; \overline{\chi}(a)\, \chi\, X(c)
\end{aligned}$$

Therefore [2]

$$\begin{aligned}
N &= p + \sum_{r=1}^{m-1} \sum_{s=1}^{n-1} \frac{\tau(\chi_r)\tau(\overline{\chi}_r \overline{X}_s)}{\tau(\overline{X}_s)}\, \chi_r(\frac{c}{a})\, X_s(-\frac{c}{b}) \\
&= p + \vartheta\sqrt{p}(m-1)(n-1) \quad \text{since} \quad |\tau| = \sqrt{p}\ ,\ |\vartheta| \le 1 \\
&> 0 \quad \text{if} \quad p > (m-1)^2(n-1)^2\,.
\end{aligned}$$

Quite trivial !

I have just returned from Littlewood's tea-party, which is the only approach we have to a seminar. L. has talked about such diverse subjects as the behaviour of the ζ–fn on $\sigma = \frac{1}{2}$ and $\sigma = 1$ and the Phragmen-Lindelöf theorem and schlicht functions *and* ballistics.

I haven't improved on $5/8$ yet for the cubic sum. [3] But I have extended the $p^{2/3}$ for Kloosterman sums to

$$\sum_x \chi(x)\, e(ax + b/x) \tag{1}$$

for any χ, hence to $\sum_x e(ax^n + bx^{-n})$. (1) is equivalent to

$$\sum_y \chi'(y^2 - k)\, e(y)\ ; \tag{2}$$

[2] In the first line of the following formula Davenport writes $\chi(\frac{c}{a})\, X(-\frac{c}{b})$; we have inserted the proper indices.

[3] Davenport refers to his result on the estimation of exponential sums for cubic polynomials as $\mathcal{O}(p^{\frac{5}{8}})$. This, as well as the next mentioned result with $2/3$, is published in Davenport's Crelle paper [Da:1933] which carries the date of receipt as July 15, 1932.

+ from this I can deduce

$$|\sum_{1}^{N} \chi(ax^2 + bx + c)| < K\, p^{\frac{2}{3}}$$

for $1 \leq N \leq p$, a, b not both zero. But now I've got this I can't find any application for it !

Bemerkungen:

1. In Hasses Tagebuch VIII findet sich der Eintrag:

> "*Davenports Beweis der Lösbarkeit von* $ax^m + by^n \equiv c \bmod p$ *und Bestimmung der Anzahl der Lösungen.*"

Der Eintrag ist undatiert. Er befindet sich zwischen einem auf "Dezember 1931" datierten Eintrag (Vennekohl) und einem auf "Februar 1932" datierten (Primzahlsatz nach Hardy-Littlewood-Landau). Mithin kann der Tagebucheintrag etwa auf "Januar 1932" datiert werden, was übereinstimmt mit der Datierung, die Hasse auf diesem Brief eingetragen hat.

2. In Hasses Tagebuch findet sich unter dem Datum "April 1932" ein Eintrag:

> "*Davenport's Abschätzung der kubischen Exponentialsummen.*"

Dort findet sich eine Reproduktion des Davenportschen Beweises mit dem Exponenten 5/8. Der Davenportsche Beweis mit 5/8, sowie auch der mit 2/3 für die Kloostermanschen Summen, finden sich beide in Davenports Arbeit im Crelleschen Journal [Da:1933]. Vgl. Kommentar **1.** zum Brief vom 25.2.32

3. Davenport war in Marburg bei Hasses ungefähr in der Zeit vom 6.–13. Januar 1932. Siehe die Postcarte von Hasse an Mordell vom 20.11.1931. Wahrscheinlich hat Davenport dabei Hasse von seinen neuen Resultaten erzählt und ihm versprochen, ihm genauere Details zu den Beweisen zu schicken.

1.3 01/1932, Fragment of a Letter by Davenport

A copy of Punch.

a copy of Punch – in fact the first thing I bought in England on Friday afternoon. Perhaps Helmut may care to read it. If there's anything he doesn't get I'll try to explain it.

I hope Juttalein is now quite well again.

Kind regards to Gertrud and 'die Oma' and 'Tante Mims' if she is with you.

Love from

Harold.

Clärle soll diesen Brief lesen, als Übung für ihre(n?) Englisch !

Bemerkungen:

1. Möglicherweise gehört diese Seite zu dem vorangegangenen Brief vom Januar 1932. Dafür spricht, dass jener Brief keine Grußformel und Unterschrift trägt. Allerdings fängt diese Seite mitten in einem Satz an. Vielleicht fehlt eine Seite? Davenport war im Januar 1932 in Marburg bei Hasses, und den "Punch" mag er sich sofort nach seiner Rückkehr aus Deutschland gekauft haben, weil ihn Hasse darum gebeten hatte.

1.4 25.02.32, Davenport to Hasse

Number of solutions of cubic congruences (Mordell) and of $ax^m + by^n \equiv c \bmod p$. *Gaussian sums in terms of their prime decomposition in cyclotomic fields. "Can you help me?" "haven't reduced any exponents recently".*

Trinity College,
Cambridge.
25. February, 1932.

My Dear Helmut,

Thanks awfully for your letter. I was glad to hear that you got a chance of using the brand-new skis after all, and I am sure it would be good fun. Has all the snow melted by now, and did it stay long enough for you to win your bet ? You all look very happy on the photos, did you really find skiing quite easy ?

I quite see your remarks about the disadvantages of an Easter visit to Germany, and I shall reconsider it. If my work goes badly, of course I shall stay at home for the vac. (=vacation), and work. If I *do* come, I may go down into South Germany (what is the climate like there ?), and then spend a few days in Marburg after Prof. and Mrs. Mordell's visit, when I should perhaps see a little of you. I made enquiries about taking the car abroad a few weeks ago, and was surprised to find that several items of the cost (reckoned in sterling) have actually decreased. On the other hand, I suppose the price of petrol in Germany is still excessively high. However, it would be very nice to see all the red circles with black dots on them again [1], and to have decent food to eat !

Last Saturday I caught an early train home, had lunch at home and spent the afternoon there, then came back here in the car. It was extremely pleasant to be behind the wheel again, and the car is running excellently. I came across country from Stockport and joined the Great North Road at

[1] Davenport refers to the traffic signs. At that time traffic signs were not yet internationalized. Several German traffic signs were red circles with $1-5$ black dots in the white interior of the circle. The meaning of the sign depended on the number of those black dots.

Newark. This is the main road from London to Scotland, and a very fine one.

I haven't done anything with Waring's Problem yet; my immediate task is to master the details of the H.-L. papers, and I rather shrink from it. H. and L. say they think I may very well get something very good out of it, but I feel very doubtful. [2]

One little thing I have done recently is to express the number of solutions of $y^3 \equiv ax^3 + bx^2 + cx + d$ (which Mordell discovered to be $p + \mathcal{O}(\sqrt{p})$ [3]) explicitly in terms of u and v, where $4p = u^2 + 3v^2$. The expression is quite simple (though my proof is a little roundabout), and has two forms, according as a certain function of a, b, c, d is a cubic residue or not. (Of course $p \equiv 1 \pmod 3$ throughout). My proof consists in expressing the number of solutions in terms of the Gaussian sums

$$\tau(X) = \sum_n X(n) e^{2n\pi i/p}$$

where X is a cubic character $\pmod p$, then using the fact that the two τ's are known in terms of u and v. What I should like to do is to generalize this to the number of solutions of $ax^m + by^n + c \equiv 0$. (perhaps only for $m = n$). It can similarly be expressed in terms of τ's, whose X's are $m-$ and $n-$th power residue characters. Now what I should like to know is whether these τ's are known in terms of, say, the decomposition of p in the fields of the m'th and n'th roots of unity – or should it be the decomposition of p in the fields of $\sqrt{m}$ or $\sqrt{n}$? I am very ignorant of all this; can you help me at all, or give me some references ? [4]

I have at last acquired a copy of Dirichlet-Dedekind, and am reading it with pleasure. It is all very nicely expressed. [5]

I haven't reduced any exponents recently, I regret to say.

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[2] H. and L. are Hardy and Littlewood.

[3] See Mordell's letter to Hasse of Dec 14, 1931.

[4] This question finally led to the joint paper [Da-H:1934] of Davenport and Hasse, in which the zeros of the ζ-function of the Davenport-Hasse function fields are explicitly determined by means of Gaussian and Jacobi sums. Many years later this was used by A. Weil in the study of the so-called Hasse-Weil global ζ-functions.

[5] Obviously, Hasse had recommended Dirichlet-Dedekind to Davenport for reading, as an introduction to algebraic number theory. It may be noted that Hasse himself, when he was young and serving in the navy, had read Dirichlet-Dedekind on the recommendation of his school teacher. This has been recorded by Frei in [Fr:1985].

It is very annoying about your American paper, they seem to be unreliable people. I suppose you will be sending them a list of corrections to be published in their next number. [6]

I enclose your list, with what words I can explain, explained. As regards "splitting infinitives", perhaps I ought to explain that this is definitely wrong, but not very serious. Yet for some reason it is a thing which everyone notices when it happens, and is very careful not to commit.

The other day I was reading "Das Leben" for February. In it there were some verses which I understood quite well (with the aid of the dictionary), all but the title: "Starker Tobak". What on earth does this mean ?

The very kindest regards and best wishes to you both, from

Harold

[6]The "American paper" is Hasse's paper in English, published in the Transactions of the American Mathematical Society [H:1932b]. Hasse had not been given the opportunity for corrections, and there were quite a number of misprints in this paper. Hasse was very annoyed by this. Indeed he published a 4-page note in the same volume of the "Transactions" containing corrections to [H:1932b].

Bemerkungen:

1. Vgl. die Eintragung in Hasses Tagebuch VIII, Bemerkung **2.** im Brief vom Januar 1932. Es scheint so, dass jene Tagebuch-Eintragung von Hasse nach dem Osterbesuch (oder Besuch kurz nach Ostern) von Davenport gemacht wurde. Im Jahr 1932 fiel Ostern auf den 27. März. Die Hasse-Eintragung ist datiert für April 1932".

1.5 07.03.1932, Davenport to Hasse

D. hopes to come at Easter to Marburg. Hardy-Littlewood conversation class. Nobody seems to be interested in mod p problems. Mordell speaks highly of Siegel as a cook. Aufgabe 125 (Vennekohl).

Trinity College
Cambridge
7 March, 1932.

My dear Helmut and Clärle,

Many thanks for Clärle's letter. It is very good of you to say that you will be able to spare a little time for me if I come at Easter. I hope to be able to do so, though as yet I cannot say definitely. I have been spending my time recently trying to improve on $\mathcal{O}(p^{\frac{2}{3}})$ for Kloosterman's sums, instead of working on Waring's problem, which I don't find half as interesting. Though if I succeeded with the latter I should feel entitled to taking a decent holiday.

Nothing very thrilling has happened recently. Term ends next week, and then I shall go home for a time (address Stockport from Tuesday next.) Last Tuesday I talked to the Hardy–Littlewood "Conversation class" (our feeble English equivalent for a seminar) about "mod. p" problems in general. But I rather attempted to do too much in a short time. Nobody seemed very interested in the "mod. p" problems, but everybody seemed intrigued by an unsolved problem which I mentioned casually a long time ago, and I was quite unable to do anything with it. It is "in what sense is the following formal identity –

$$2\pi \sum \mu(n) n^{-1} \{n\theta\} \;=\; -\sin\, 2\pi\theta$$

where

$$\begin{aligned}\{x\} &= x - [x] - \frac{1}{2} \\ &= -\frac{1}{\pi} \sum n^{-1} \sin\, 2n\pi x \;,\end{aligned}$$

true ?" [1]

Am I right in assuming that Aufgabe 125 in the Jahresbericht der D.M.V. is from a pupil of yours ?

How do you like the enclosure ? I came across it by accident this afternoon in a bookshop and thought it might amuse you.

I had quite a human letter from Mordell this morning. He stayed with Siegel in Frankfurt, and speaks highly of him as a cook!

Do you remember once saying that you and Artin had tried to apply analytic methods to the cubic sum (with a pure power), and get some "dual" for it ? In a sense I have done this, but the result is only approximate (the error term is very small though). But the result is only a curiosity.

The post is about to go, so I will stop. Write when you can spare time.

Yours

Harold.

Will write to Clärle in a day or two.

[1] In the last line of the above formula Davenport writes "θ"; we have changed it to "x". We also have changed Davenport's $-\frac{1}{2\pi}$ to $-\frac{1}{\pi}$ which seems to be what he meant to write.

Bemerkungen:

1. Was meint Davenport damit, wenn er sagt, dass Hasse und Artin versuchten, analytische Methoden für die “cubic sum with a pure power” anzuwenden, und etwas ”Duales” erhalten haben?

1.6 12.03.1932, Davenport to Hasse

Approximate functional equation for cubic sums.

12 March, 1932.

My dear Helmut,

I was in such a hurry when I wrote the other day that I forgot quite a lot of things. To begin with, I forgot to thank you for the two off-prints which you sent me the other week through Littlewood. Let me do so now, very heartily.

Then for the "approximate functional equation" for cubic sums. I don't suppose it is the sort of thing you and Artin had in mind; firstly because it is only approximate, and secondly because the right-hand side involves new difficulties of a quite different (and rather formidable) type. Anyhow, I will give you the results.

1.

$$\begin{aligned}\sum_x \exp(2\pi i a x^3/p) &= A\left(\frac{p}{a}\right)^{\frac{1}{4}} \sum_1^{3ap} n^{-\frac{1}{4}} \exp\left(-\frac{4}{3}\pi i \left(\frac{p}{3a}\right)^{\frac{1}{2}} n^{\frac{3}{2}}\right) \\ &+ B\left(\frac{p}{a}\right)^{\frac{1}{3}} + O\left([\log p]^{\frac{5}{2}}\right).^*\end{aligned}$$

$$^*A = \frac{e^{\frac{\pi i}{4}}}{\sqrt{2}} \frac{1}{3^{\frac{1}{4}}}, \quad B = \left(\frac{\pi}{4}\right)^{\frac{1}{3}} \frac{1}{\Gamma\left(\frac{1}{3}\right)} \left(\frac{1}{\sqrt{3}} + i\right).$$

2. Same for $\sum_x \exp(2\pi i(ax^3 + cx)/p)$ with n in the sum on the right replaced by $n + \frac{c}{p}$.

3. Similarly for $\sum_1^{\lambda} \exp(2\pi i a x^3/p)$ with the summation on the right running up to $3a\lambda\ ^2/_p$ instead of p.

There is nothing essentially new in these results, formulae like this were obtained by Hardy and Littlewood in their Diophantine Approximation papers in the Acta Math. in 1914. But they were concerned with $\sum_{1}^{x} \exp(i\theta n^3)$ as [...] goes to infinity, where θ is fixed.

The error term in 1. is extremely small, isn't it? The formula gives us extraordinarily accurate information about the *right* hand side.

The proof of these is simply by Poisson's formula:

$$\sum_{x} \exp(2\pi i a x^3/p) = \sum_{-\infty}^{\infty} \int \exp(2\pi i a t^3/p + 2\pi i n t) dt$$

then approximating to the integrals on the right by the "method of steepest descents".

I am quite unable to do anything with Waring's problem, my methods are far too feeble. The fact is that Hardy and Littlewood use Weyl's inequality in a form in which it is really very powerful.

Nothing very interesting has happened recently. Tonight I am going to the Commemoration of Benefactors Feast, a big dinner to which many famous old Trinity men come.

The very best wishes,

Yours, *Harold.*

1.7 29.03.1932, Davenport to Hasse

An identity involving the Möbius function.

Stockport, England, 29 March, 1932.

Mein liebes Clärle und lieber Helmut,

Vielen herzlichen Dank für Clärles Brief, den ich gestern erhaltete.[1] Ich hoffe, daß Ihr ein glückliches Ostern gehabt haben, und daß alles Euch gut geht.

Ich habe die Absicht, nächste Woche nach Marburg zu reisen; ich fahre mit dem Schiffe von Harwich nach Antwerp am Montag Abend (4ten April) und werde in Marburg sein wahrscheinlich Mittwoch vormittag. Vielleicht werde ich schon ankommen am Dienstag Abend, daß kommt darauf an, ob ich in einem Tag von Antwerp nach Marburg fahren kann mit dem Wagen. Meine Schwester kommt nicht mit.

Ich würde sehr dankbar sein, wenn Clärle ein Zimmer für mich bestellen könnte – aber ich habe die feste Meinung, dafür selbst zu bezahlen. (Dieser Ausdruck klingt nicht richtig, aber ich kenne keinen besseren.) Vielleicht würde Clärle auch fragen, ob die neu endeckte Garage frei ist.

Ich bin gespannt, zu hören, wie gut Clärles englisch geworden ist. Aber sie muß doch ein bißchen deutsch mit mir „quatschen". Darf ich weiter englisch schreiben?

I have done nothing recently except a little work on an old problem which I had previously abandoned – namely the attempt to prove rigourously the identity

$$\sum_1^\infty \frac{\mu(n)}{n}\{n\theta\} = -\frac{1}{\pi}\sin 2\pi\theta,$$

where

$$\{t\} = -\frac{1}{\pi}\sum_1^\infty \frac{\sin 2m\pi t}{m} = \begin{cases} t-[t]-\frac{1}{2} & \text{if } t \neq [t] \\ 0 & \text{if } t = [t]. \end{cases}$$

[1] An zahlreichen Stellen sind, offenbar von den Empfängern, nachträglich Korrekturen angebracht worden.

The only non-trivial result I have been able to prove is that the partial sums of the series in question are uniformly bounded. Also that the series converges and is equal to the right-hand side for *certain* irrationals θ, namely, those whose a_n in their continued fraction expansions increase very rapidly.

I look forward very much to seeing you both next week.

Kind regards to Prof. and Mrs. Mordell.

Yours,

Harold.

P.S. If you write to me, write to Trinity College, where I shall call on Monday next.

P.P.S. Excuse the dullness of this letter, but I have been leading a dull life lately.

1.8 06.05.1932, Davenport to Hasse

Hardy-Littlewood's tea party. Paley on number of irreducible polynomials modulo 2.

Cambridge, 6 May, 1932.

My dear Helmut and Clarle,

Very many thanks for your letters. As regards corrections to Helmut's, the most important one I have to make is that "taken in" means deceived of swindled, which I suppose is not what was meant.

I should like to have seen the welcoming of May by the students on the Castle hill. There is no such celebration in England : there used to be a custom of dancing round the May-pole, but it died out over a century ago. There remains only the custom of decorating cart-horses with ribbons.

What is a "Renntour by Fuss" which Clarle mentions? Is it some custom associated with Ascension Day?

Was an overwhelming desire to revisit Bad Elster awakened in you by the post-card from the Badedirektion? I must confess that it wasn't in me. The weather forecast is very interesting, and I hope it will prove to have been a sound one. His argument based on sunspots is valid, but his assertion that the probability of a fine summer is further increased by the fact that the last two summers were very wet must be regarded with great suspicion, I think.

I have not indulged in any recreations recently except last Saturday, when Jaeger and I drove down to Brooklands (the motor racing track), and saw the race for the British Empire Trophy. Brooklands is situated near Weybridge, about 20 miles SW of London. The average speed in the race was about 130 miles (208 km) an hour. The distance was 100 miles, formed by 36 circuits of the track. The race was quite interesting to watch, but I found it quite impossible to form a good idea of the difficulties the driver is up against. The tyres seem to give the most trouble, many cars had to retire because of bursts. Also the overtaking seems to be extraordinarily difficult.

I have not got any new results recently, except that if the upper bound of the real parts of the zeros of all L-functions is $< \frac{3}{4}$ then the series

$\sum_1^\infty \frac{\mu(n)}{n}\{n\theta\}$ converges to $-\frac{1}{\pi}\sin 2\pi\theta$ for almost all θ. The last Hardy-Littlewood tea-party was very interesting. Paley (fellow of Trinity, a year or so older than me) talked about polynomials whose coefficients are taken mod 2, which he writes as numbers, e.g. x as 10, $x+1$ as 11, x^5+x^3+1 as 101001, and so on. He has obtained some very interesting results for the Waring problem for these numbers. But the question he raised which interested me even more concerns the prime number theorem for these numbers. The number of primes (i.e. irreducible polynomials) of degrees $\leqq a$ given n can be exactly determined (done by Gauss), and it is asymptotically $2^n/n$. This is the natural analogue of the prime number theorem. But the question raised by Paley is, is it true that the number of primes among the first t numbers of the series is asymptotic to $t/\log t$ for all t, not necessarily of the form 2^n? This appears to me to be a difficult question.

Mr Besicovitch's book on almost periodic functions has just been published. I think it is excellent – a model text-book.

Kind regards from Jaeger. The very best wishes to you both from

Harold.

Grüsse an Juttalein!

1.9 14.05.1932, Davenport to Hasse

Polynomials mod 2. Artin does not deal with the case $p = 2$ and the module composite. "Can you suggest any weapon?"

Trinity Coll. Camb.
Saturday. 14 May '32.

My dear Helmut and Clärle,

I hope you are having a very pleasent holiday in Allendorf, and that you are having the same marvellous weather that we have here. One quite forgets the existence of whitsuntide in Cambridge: it is not observed at all here.

I have sent you a copy of the summer number of Punch, and hope you will like some of the pictures in it, at any rate.

Two problems have interested me lately. The first was given to me by a geometer, in whose investigations it arose. Prove that any permutation of the p residues mod p (prime $\equiv 3$ mod 4) with the following property A must be of the form $x' = a^2x + b$, if $p > 11$. The property is: for every m there exists an n such that the set of numbers $r + m$, where r runs through the quadratic residues, transforms into the set $r + n$ (possibly in another order). This is not true for 7, 11 but is apparently true for larger primes.

The second problem concerns the polynomials mod 2. The suggested prime number theorem I mentioned before turns out to be very easy. If one uses the fact that a prime remains prime when written back to front ($x^n f(\frac{1}{x})$ instead of $f(x)$), the theorem is an immediate consequence of the analogue of Dirichlet's theorem on primes in an arithmetic progression (which is obvious + was proved by Artin). What I am now interested in is proving the Riemann hypothesis for the L–functions constructed with the characters mod 2, x^n. Can you suggest any weapon ? Artin does not deal with the case $p = 2$ nor with the case when the modulus (here x^n) is composite.

Sorry to talk about mathematics. Give my love to Clärle, and my kindest regards to your parents. Is Juttalein with you in Allendorf ?

To–day I am doing without a fine for the first time. I took the car out for a short run this afternoon, it was doing excellently. Nothing has required overhaul since my last trip to Germany, except that luggage grid. The term here is now half–way through.

Very best wishes Yours, Harold.

1.10 25.05.1932, Davenport to Hasse

Einstein in Cambridge

Cambridge, 25 May, 1932.

My dear Helmut and Clarle,

Very many thanks for the post-card, with its picture of the familiar entrance-gate to Bad Sooden, and your letter of last Saturday. Your English is quite above serious criticism. The only little suggestion I have to offer is that you should try to cut down the number of commas to the minimum. It is perhaps rather a matter of opinion, but if I were writing e.g. the sentence (1) of your letter I should omit the first two commas.

The weather here was also delightful last week, and I went on the river once or twice punting. It is a pity you have no holiday in June; if you had I should have tried to persuade you to come over here and see England at the time of the year when it looks its best. I think you would like our river and gardens very much.

The article about why men shave is very amusing. Everyone I have mentioned it to agrees with me that it probably really happened at Cambridge, U.S.A., and that whoever translated it made a slip. Such theses are written in America, but I cannot imagine it here. Though, as a matter of fact, I do not think the thesis is any more nonsensical than any thesis on, say, philosophy or literature probably is – they all seem to be written on much the same plan. By the way, the article does not say that the lady actually got her degree.

When I read one of your questions I simply sat down and roared with laughter. Now I have my revenge on you for all the laughable blunders I have made in German. I.O.M. stands for the Isle of Man, an island about 30 miles by 10 in the Irish Sea, between England and Ireland. Douglas (population 30000) is the principal town. What makes your question so amusing is that it is so perfectly reasonable of you to assume Douglas to be a man's name.

The Isle of Man (70 miles from Liverpool) is a favourite holiday-place for the work-people of Lancashire and Yorkshire. I have been a few times, when a boy. The only other facts I can give you are that (1) the I.O.M. is the envy of England because it has an income tax of only a few pence in the pound, whereas in England it is 4s.3d. in the pound; (2) Manx cats have no tails.

As for your other questions:-

(1) An ABC time-table gives all stations in alphabetic (ABC) order, and under each station the times of arrival and departure of trains to and from London. Similar time-tables exist with other towns as "base" instead of London. Naturally such tables are very convenient, but of restricted utility.

(2) V.A.D. = Voluntary Aid Division (of nurses, during the war.)

(3) "ner" is simply derived by gradual corruption from "than". In ordinary English the "a" is often pronounced merely with a dull "e" sound as in your "bitte". What has happened in the Lancashire and Yorkshire dialects is that the "th" has dropped, and the dull "e" sound slipped to the rear of the consonant. Try them in order.

I am awfully pleased to hear that you are reading The Good Companions. It is a very fine book, and is in much closer correspondence with real and actual English life (except for the happy ending) than anything of Walpole's or Galsworthy's. I look forward to hearing your impressions of it.

You think the polynomials mod. 2 are important? I must admit I have quite lost interest in them. There seems to be nothing whatever to be done with them except note their existence, and to note that all the properties of ordinay L-functions have analogues for these L-functions, and the analogues appear to be all very trivial. Of course I am not looking at them from the right point of view.

The group of the residues modd. $(2, x^n)$ which are prime to x, when combined by multiplication is of rather complex "type", depending on the structure of n, but it can be specified without difficulty.

I am sorry that I omitted to mention that there is a (1,1) correspondence between m and n, it is part of the data. But the theorem remains one with the exceptions 7, 11.

For the last week or so I have been thinking about various series like

$$\sum \frac{r(n)}{n} e^{2n\pi i\theta}, \quad \sum \frac{\chi(n)}{n} \{n\theta\}$$

(where $r(n) = \sum\limits_{n=u^2+v^2} 1$, χ a character, $\{x\} = x - [x] - \frac{1}{2}$). These seem to me to be much more intriguing, but I suppose they do not appeal to you.

Yes, the Einstein who lectured here was the famous Einstein, he has been in England some time. I am ashamed to say I did not attend his lecture I am told he mixed up English and German rather amusingly. I think there was a large audience of “general public”.

I hope you have both returned from your Easter holiday refreshed.

As you say, it is now almost a year since we met. You have made it a very happy year for me.

The very kindest regards and best wishes to both of you.

Yours,

Harold.

1.11 15.11.1932, Hasse to Davenport

Thanks for D's manuscript on exponential sums, will be publ. in vol. 169 of Crelle. Report about court session concerning car accident.

Marburg, Nov. 15th, 1932

My dear Harold,

First of all very many thanks for your letter and the cigarettes. I got the latter allright, i. e., without being bothered by a Zollamt order. That they were rectangular shaped instead of round did not in the least impair their fine flavour. You may be sure that we, i. e. Clärle, Gertrud and myself enjoyed them ever so much and thankfully thought of you while smoking them.

Your manuscript was quite allright, the untidiness (as you put it) in certain parts of it does not matter at all for the printing. I have not got yet the sizing up of its length in print, but I hope I shall get it to–morrow. It will be published at any rate in vol. 169.[1]

You ask me about the lectures which will be given at Göttingen, Berlin and Hamburg next term. I am sorry that I cannot help you now, for the Vorlesungsverzeichnisse usually do not appear before Christmas. I suggest that we write to the Universitätssekretariate of G., B. and H. when you are here at Christmas and order copies of the Vorlesungsverzeichnisse.

Now to the main point! I mean the report about your case which has been up to–day. Clärle and myself attended to the proceedings. It was a so–called Beweisaufnahme, consisting in the examination of the witnesses of both sides. It began with Clärle's examination. We had prepared it the day before by drafting a document plainly answering a series of 10 questions that had been put forward to Clärle on the Ladung form. Clärle's answers were absolutely conform with what she had already laid down at the previous examination before the police. She handed over her draft to the judge who happened to be Gertrud's present employer, and who is a very nice fellow indeed. Clärle particularly emphasized the fact that the cyclist had never been farther on

[1] Es handelt sich um die Arbeit "'On certain exponential sums"', die im Zuge der Arbeiten an der Riemannschen Vermutung für Funktionenkörper wichtig wurde.

the road than your car and that therefore there was no question of *overtaking* him. She said she was definitely *sure* about that, whereas she could only give her *impression* as to most of the other questions. She had to take her answers on oath later on request of the other solicitor. This other solicitor, Schilling of Koch a. Schilling, was not a nice fellow at all. He behaved very fidgety and tried to annoy Clärle by cross–examining her in a rather biting way. Clärle and Krämer kept entirely steady to that and were not put out of contenance. Schilling inveighed even against the judge but got a jolly good snub from him. The judge seemed to be fairly in your favour from the very beginning, though he had already admitted on a former occasion that you could not possibly come off without any damages. He kept this attitude through all of the trial. After Clärle's examination the same procedure took place with three witnesses from Sterzhausen, the brother of the injured man, the fiancé of his sister and another man. The first two are the men who followed Bamberger on their cycles. The third also followed on a cycle but 40 odd meters farther back. He emerged shortly before the accident on the other branch of the secondary road. He stated, he also had nearly been run over by your car, he had hardly escaped crossing the road in front of the car and coming to a short stop in front of a forgery at the opposite side of the main road, he had turned round then in order to make sure of the number of your car, and had noticed then Bamberger being bumped down by you. He estimated your speed to 80 km (!!) and substantiated this by saying he was a motorist himself and could judge the speed fairly exactly. Due to this obvious contradiction to the result of measuring the Bremsspur and to many other incompatibilities in his statements he was not given much belief, though. I will not bore you by enumerating the statements of the other witnesses. They were slightly incompatible with each other, too, and certainly incompatible with Clärle's testimony. After the cross–examining from all sides the trial (Beweisaufnahme) was closed at last about 1.15 p.m. (It had begun about 11.45 p.m., rather quick work, you see.) After a short deliberation with the lawyers, the parties all the while present, the judge put forward a compromise (or agreement), so–called Vergleich, (at/on) the following terms: damages to Bamberger 700 Rm, legal expenses charged to Bamberger (he has Armenrecht, i. e., the state will pay them for him to itself), expenses for lawyers charged to you. Krämer consented, though he told me afterwards there certainly was a jolly good chance to get better terms with this judge. He feared, however, things would not be the same with the appellation court (Landgericht), and he was sure plaintiff would go to it in this case. Schilling consented only with the reservation that a new medical opinion about Bamberger's health should turn out satisfactorily. Bamberger was denoted "quite healthy" by the Kreisarzt lately. The Vergleich will be up three weeks, hence on the 9^{th} of Dec.. Let us hope for the best.

By the way, did you notice the third cyclist emerging in front of your car from the first branch. Clärle was simply baffled. She had not noticed anything of that kind.

I hope to get the information about exponential sums etc. soon, perhaps to–morrow. I am given one hour for this subject.[2]

I attend to a course of Miss Diffené's on Translation and Essay Writing. It is quite amusing. We write essays on subjects suggested by A. Huxley's "Those Barren Leaves". My first Essay was "On Verbal Felicities". I shall show it to you at Christmas, with Miss Diffené's corrections. The next has to be "On the passing of etiquette and modern informality" or "On the change of our susceptibility to flattery". I still do not know which of them I shall choose. The translation works this way: we have got a German translation of Thackeray's "The Face of the Snob" and retranslate it into English.

Now I bothered you enough with all this stuff.
Kindest regards and much love,
from
Helmut

[2] Offenbar bereitet H. seinen Vortrag in Kiel vor, der im November geplant ist.

1.12 20.11.1932, Hasse to Davenport

H. busy with congruences mod p. H. has just received Mordell's paper on exponential sums (Quart. Journ.) In Kiel, H. will treat these problems in detail, further one of D.s cubic proofs. For character sums and general congruences in two variables, H. will give only the most striking results, with short hints as to the methods. H. will not lecture in Hamburg, he will stay there with the Artins only. H. tries to interest D. in a generalization of Artin's lemma for the reciprocity law.

Marburg, Nov. 20^{th}, 1932

My dear Harold,

I was just busy over the congruences mod. p when your cigarette letter arrived. It is awfully kind of you to think of us in such a substantial way. You may be sure that both of us appreciate your precious gift ever so much. Clärle, too, realizes now the considerable difference in quality between the Churchmans and the German Gold Flakes. Thanks awfully. This time, the cigarettes arrived in a perfect state, nearly as round as round can be. By the way, it did not make the slightest difference that they were fairly pressed the first time.

Your two air-mailed letters arrived in due course, though not in the least earlier than they would have arrived by regular mail. The first one, obviously written as a comment to the "Commercial Papers" and dispatched about the same time, left Cambridge at 7 a.m. on Wednesday (according to the stamp) and arrived here on Friday at 12 a.m. And the second one ran correspondingly twenty–four hours later. There seems to be no expedience in air–mailing letters from Cambridge to Marburg.

I am entirely satisfied with the information given by your papers. For the time being, there is only one point I should like to have more fully explained, namely the trivial reduction of the exponential sum with the general quadratic fractional function to a Kloosterman sum. I am sure I could find it myself by worrying intensely about it; my first attempt, however, failed, and since I am a little short with my time, I should be pleased to have your help. It will be easy for you, of course, to give me the clue.

I will give you full details about my lecture at Kiel when I have completed the manuscript. I am not going to lecture at Hamburg. My stay there has the only purpose to be together with the Artins.

I have just received the number of the Quarterly Journal containing Mordell's paper on exponential sums. I think I shall treat these problems in detail, further perhaps one of your cubic proofs, and Mordell's or your treating of the Fermat congruence. As to character sums and general congruences in two variables, I shall only give the most striking results with short hints as to the methods.

May I try to get you interested in a little question which plays a rôle in the proof of Artin's law of reciprocity ? There occurs a Lemma concerned with rational numbers only, namely:

Let n, $a > 1$, k be given integers. Then there always exists an integer m and a group $\mathcal{U}$ of prime residue classes mod. m such that

1.) $(m, k) = 1$,

2.) the class group of all prime residue classes mod. m with respect to $\mathcal{U}$ as unit is cyclic, i. e., it exists a basis element r such that every prime residue b mod. m has a representation

$$b \equiv r^{\beta} u \ \text{ mod. } m \ \text{ with } \ u \ \text{ in } \ \mathcal{U}.$$

3.) the class of a with respect to $\mathcal{U}$ has an order divisible by n, i. e., the least power of a belonging to $\mathcal{U}$ has an exponent divisible by n.

One can prove this Lemma in a thoroughly elementary way.[1] Let us first assume that $n = \ell^{\nu}$ is a prime power. Then the number [2]

$$\begin{aligned} f_{\nu}(a) &= \frac{a^{\ell^{\nu}} - 1}{a^{\ell^{\nu-1}} - 1} = \frac{a^{\ell^{\nu}} - 1}{d} \\ &= \ell + \binom{\ell}{2} d + \cdots + \binom{\ell}{\ell - 1} d^{\ell-2} + d^{\ell-1} \\ &= a^{\ell^{\nu-1}(\ell-1)} + \cdots + a^{\ell^{\nu-1}} + 1 \end{aligned}$$

[1]Hasse hatte diesen Beweis schon im April 1932 in seinem Tagebuch notiert.

[2]In the second row of the following formula Hasse writes $d^{\ell-1}$ and d^{ℓ}; we have changed this into the proper terms $d^{\ell-2}$ and $d^{\ell-1}$.

is obviously greater than ℓ and divisible only [3] by ℓ^1 (if one assumes, without restriction, that $\nu \geq 2$ for $\ell = 2$, $a \equiv 1$ mod. 2), and consequently divisible by a prime $q \neq \ell$. Every such q divides $a^{\ell^\nu} - 1$, but not $a^{\ell^{\nu-1}} - 1 = d$ because obviously $(f_\nu(a), d) \mid \ell$. Hence a has order ℓ^ν mod. q, and 2.), 3.) are satisfied by taking $\mathcal{U}$ as the identical group $u \equiv 1$ mod. q. In order to satisfy 1.), choose primes q, q', q'', ... in the same way for the exponents ℓ^ν, $\ell^{\nu+1}$, $\ell^{\nu+2}$, ... They are all different from each other, hence existing in infinite number, and amongst them is one prime to k. The order of a is no longer exactly $n = \ell^\nu$, but certainly divisible by $n = \ell^\nu$.

The case of a compound $n = \prod_i \ell_i^{\nu_i}$ may be treated by a more or less trivial composition: $m = \prod_i q_i$, where q_i is chosen to $\ell_i^{\nu_i}$ according to the foregoing; $\mathcal{U}$ has to be chosen as the greatest common part of groups $\mathcal{U}_i$, where $\mathcal{U}_i$ (in order to satisfy 2.)) is no longer the group $u_i \equiv 1$ mod. q_i, but the group $u_i \equiv x_i^{\ell_i^{k_i}}$ mod. q_i where $\ell_i^{k_i}$ is the *highest* ℓ_i–power in $q_i - 1$ ($k_i \geq \nu_i$, of course); the order of a with respect to $\mathcal{U}_i$ is unaltered by this substitution for $\mathcal{U}_i$.

My question is now, whether the Lemma can be generalized to more than one number a, for example to the case where a number of primes $p_1, \ldots, p_r$ is given, and one demands in 3.) that *each* of them shall belong to a class with order divisible by n; or to the case where two primes p_1, p_2 are required to belong to the *same* class of order divisible by n. I know proofs for these generalizations using analytical means (Dirichlet series), but I have not been able to prove them elementarily.[4]

Many good wishes and much love,

from Helmut

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[3]Hasse writes "only" but obviously he means "at most".

[4]Hasse hatte einen (nicht-elementaren) Beweis in seinem Tagebuch am 8.6.1932 notiert. Ein elementarer Beweis von van der Waerden findet sich im Crelleschen Journal 171 (1934). Davenport ist offenbar nicht darauf eingegangen.

1.13 07.12.1932, Davenport to Hasse

New results on exponential sums for rational functions. D. gives a time table for an overnight trip by boat and train to Marburg. Marshall Hall: $x^2 - 1 = y^m$.

TRINITY COLLEGE,
CAMBRIDGE.
7 Dec 1932

My dear Helmut,

I'm afraid I have not very much to say in this letter: I had thought I had proved

$$\sum_x e\Big(\frac{ax^3 + \ldots + d}{Ax^3 + \ldots + D}\Big) \;=\; \mathcal{O}(p^{\frac{7}{8}})$$

but it turns out to be a mistake. What I can prove — at least I think so — is that for any n

$$\sum_x e\Big(\frac{a_n x^n + \ldots + a_0}{A_n x^n + \ldots + A_0}\Big) \;=\; \mathcal{O}(p^{1-\frac{1}{n.2^{n+1}}}) \,.$$

The proof is by the usual method of repeated squaring together with the result (proved directly by Mordell's method) that

$$\sum{}^{*} \Big| \sum_x \Big(\frac{x}{p}\Big)\, e\Big(\frac{a_n x^n + \ldots + a_0}{A_n x^n + \ldots + A_0}\Big)\Big|^{2n} \;=\; \mathcal{O}(p^{3n-1})\,,$$

where $\sum{}^{*}$ is a summation over all forms $\frac{a_n x^n + \cdots + a_0}{A_n x^n + \cdots + A_0}$ which are not equivalent by cancelling, adding a constant, and replacing x by kx.

By the way I have studied the Reichskursbuch carefully + I find that if one crosses by the Vlissingen boat one can reach Marburg at 6.29 a.m. instead of 9 a.m. The route is
Vlissingen – Venlo – Viersen – Duisburg – Soest – Kassel – Marburg.
It is only necessary to change at Soest + Kassel. See "Zug– u. Wagen–Verzeichnis, Anlage zum Reichs–Kursbuch" under D189. One reaches Kassel at 3.53 a.m.

I regret to say I am being very lazy.

Much love

Harold.

P.S. Marshall Hall aims at proving that $x^2 - 1 = y^m$ has only one solution $x = 3$, $y = 2$, $m = 3$. Is this trivial or hopeless (or false) ? [1]

[1] Dies ist ein Spezialfall der Catalan-Vermutung, die im Jahre 2002 von Preda Mihailescu bewiesen wurde. Der hier vorliegende Spezialfall wurde 1965 von Chao Ko. bewiesen. Siehe den Artikel von G. Frey [Frey:2002].

1.14 07.12.1932, Hasse to Davenport

H. enjoyed seeing D. again. H. reports on his lectures in Kiel and Hamburg. H. hopes to extend all D.'s and Mordell's results to finite fields. Artin has pointed out the consequences for the zeros of his ζ-function. Functional equation for general algebraic L-series.

7. 12. 32

My dear Harold,

I am very, very busy, in point of fact right in the middle of a giant proof correction (40 pages) for Crelle. Nevertheless you shall have a few lines in answer to your kind letter.

I have awfully enjoyed seeing you again, though only for so short a time.[1]

My lectures found much interest with the Hamburg and Kiel mathematicians.

In Hamburg, I was able to produce a couple of new results, which I had found during my journey back from Kiel in a Personenzug.

I have proved that[2]

$$e(f(x)) \;=\; \frac{1}{q}\sum_{\xi} e(S(f(\xi))) \;=\; \mathcal{O}(q^{-\frac{1}{n}})$$

when ξ runs through all elements of the $GF(q)$ (Galois field of q elements), n is the degree of f (a polynomial with coefficients also in the GF) and S denotes the Spur (trace): $S(\alpha) = \alpha + \alpha^p + \cdots + \alpha^{p^{r-1}}$ $(q = p^r)$. I have also applied my (i. e. Mordell's) method to the character sums and found that in the elliptic case

$$N(\eta^2 \equiv f(\xi)) \;=\; q + \mathcal{O}(q^{1-\frac{1}{6}}), \qquad (\eta,\ \xi \text{ in } GF(q)).$$

[1]Es ist merkwürdig, dass Hasse einen (wenn auch nur kurzen) Besuch von Davenport erwähnt, während doch sein Brief am selben Tag geschrieben ist, an dem ihm Davenport den Fahrplan und das Eintreffen in Marburg signalisiert. Ist vielleicht Davenports Brief irrtümlich falsch datiert?

[2]In the following formula, Hasse writes $\mathcal{O}(q^{-\frac{1}{n}})$ while the reader would perhaps expect $\mathcal{O}(q^{1-\frac{1}{n}})$. But note that Hasse writes the term $\frac{1}{q}$ in front of his exponential sum!

My method has been very rough, and I am pretty sure I can improve this to $\mathcal{O}(q^{1-\frac{1}{4}})$ corresponding to your best–known result.

As Artin pointed out, this means that the zeros of his congruence ζ–function lie all in $\sigma \leq 1 - \frac{1}{6}$, or $1 - \frac{1}{4}$ respectively.[3]

I hope to be able to extend all your and Mordell's results to Galois fields. At present, this is, however, of less interest. The main point must be to get the rational case straight, i. e., to find $\mathcal{O}(p^{1-\delta})$ at least in the hyperelliptic case, and if possible for the general irreducible $f(\xi, \eta)$. I am quite sure that any method that leads to a such result generalizes at once to GF's. The only new idea one has to bring in is that

$$S(\gamma\alpha) \;=\; 0 \text{ for every } \gamma \text{ of the } GF$$

is equivalent with

$$\alpha \;=\; 0.$$

The same idea applies in the general rational case for the exponential sums. I have not carried on my investigations about this case yet. I have come to a deadlock for the time being, consisting in the question whether a certain interpolation problem is uniquely determined. But this difficulty cannot be serious. One must only have patience and a sufficient skill in handling purely algebraic methods. Perhaps we can plunge into this at Christmas.

Many thanks for your help with the integral. I was a fool not to see its value and meaning myself.[*)]

I have got the required proof for the functional equation of the general algebraic L–series straight with it.[4] It will be given in my Seminar on Thursday.

Kindest regards and much love,

from Helmut

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[3] Artin hatte das schon 1921 in einem Brief an Herglotz erwähnt, aber niemals publiziert. Hasse hatte diese Tatsache dann in seiner Übersicht über die Zetafunktionen (unter Berufung auf Artin) publizierte.

[*)] As a matter of fact I had calculated it for the required case $n = 2$ by means of the Residuenkalkül.

[4] Es handelt sich wohl *nicht* um L-Reihen von Funktionenkörpern, sondern von Zahlkörpern. Vielleicht hat Hasse den Beweis der Funktionalgleichung deshalb studiert, um sie auf den Fall von Funktionenkörpern zu übertragen.

1.15 14.12.1932, Davenport to Hasse

Announcing arrival in Marburg on 28. oder 29.12. A result from analytic number theory.

Stockport, 14 Dec. 1932.

My dear Helmut,

Many thanks for your note. I expect I shall arrive either 7.12. p.m. on Dec 28th. (via Hoek v.H. and Niederlahnstein) or 7.27. a.m. on Dec 29th (via Kassel). Are you sure I made a mistake about the 6.29 arrival? It agrees with the $1\frac{1}{2}$ hours taken by an express from Kassel to Marburg, since one leaves Kassel about 5. I will look it up again.

I came home on Monday morning from Cambridge, by the usual route, and thought of our several journeys along it a few months ago.

To-morrow I am going up to London. I have the opportunity of reading my second note on quadratic residues (in which I prove $\varphi(2,7) = \frac{19}{20}$) to the London Math. Soc. (I sent it to them a few weeks ago). Also my sister has a music exam. in London on Friday + Saturday morning, so I shall take her + bring her back.

The only mathematics I have done lately has been to prove (what I have tried occasionally for a long time) that the proportion of numbers less than x for which

$$\frac{\sigma(n)}{n} < \lambda, \qquad \left(\sigma(n) = \sum_{d|n} d\right),$$

tends to a definite limit as $x \to \infty$, and this limit is a continuous function of λ. I have proved that

$$\lim_{x\to\infty} \frac{1}{x} \sum_{n=1}^{x} \left(\frac{\sigma(n)}{n}\right)^s$$

exists for any s, real or complex, and is

$$\prod_p \left[1 + \sum_{\nu=1}^{\infty} \left\{ \left(\frac{1-p^{-\nu-1}}{1-p^{-1}}\right)^s - \left(\frac{1-p^{-\nu}}{1-p^{-1}}\right)^s \right\}\right].$$

From this the first result follows on using the arguments of Schoenberg.

I am extremely sorry to hear that your mother has got pneumonia, and sincerely trust that she will make a good recovery.

Kindest regards to the Oma, + particularly to Gertrüdlein.

Yours

Harold.

1.16 20.01.1933, Circular by Hasse

Announcing H.'s Marburg Lecture Notes.

MATHEMATISCHES
SEMINAR
DER UNIVERSITÄT

MARBURG–LAHN,
DEN 20. Jan. 1933.

Verschiedenen Anregungen Folge leistend beabsichtige ich, eine vollständige Ausarbeitung meiner Vorlesung "Klassenkörpertheorie" vom Sommersemester 1932 vervielfältigen zu lassen und so einem größeren Kreis von Interessenten zugänglich zu machen. Nach einem vorläufigen Überschlag würde sich der Preis eines gehefteten Exemplars, wenn sich genügend viele Abnehmer finden, auf sieben bis acht Reichsmark stellen. Ich wäre Ihnen dankbar, wenn Sie mir bis zum 10. Februar des Jahres mitteilen würden, ob Sie auf ein Exemplar reflektieren. Geht bis zu diesem Termin keine Antwort ein, so nehme ich an, daß Sie keine Bestellung machen wollen. [1]

In vorzüglicher Hochachtung

ergebenst
Hasse

[1] Handschriftlicher Vermerk von Hardy: *"I will certainly take a copy, and I hope that you will get the Univ. library to take one also — G.H. Hardy"*

1.17 02.02.1933, Hasse to Davenport

H. explains to D. the GF-method. H. recommends van der Waerden for D.'s second question. H. has studied F. K. Schmidt carefully and finds it better than Artin.

2. 2. 33

Dear Harold,

The flu has come over all of us. It is terribly spreading all over Marburg, thank heaven not very heavy in most of the cases. Clärle and I seem to be over the worst, the fever is down, only a terrible weakness has remained.

I think I have got all you want in connexion with the number of congruence solutions in Galois Fields. We know that N_1 (which is $N + \mathcal{O}(1)$) is given by

$$N_1 - p \;=\; \beta_1 + \cdots + \beta_m,$$

where the β_μ are the roots of the ζ–function (in $z = p^s$). Futher we know that in the Galois Field of degree r (i. e., of p^r elements)

$$N_1(p^r) - p^r \;=\; \beta_1^r + \cdots + \beta_m^r.$$

Now suppose that for at least one sequence of exponents r tending to ∞

$$N_1(p^r) \leq C\, p^{r\vartheta}\,, \qquad \left\{ \begin{array}{c} C \\ \vartheta \end{array} \right\} \text{ independent on } r\,.$$

Then by the well–known argument all

$$|\beta_\mu| \;\leq\; p^\vartheta$$

and therefore in particular

$$|N_1 - p| \;\leq\; mp^\vartheta.$$

In the case of $y^2 \equiv f(x)$ it is therefore no restriction to deal with such Galois fields only, as base fields, in which $f(x)$ splits into linear factors. For the numbers r belonging to these fields are all the multiples of a number r_0, and hence tend to infinity.

I cannot help you with either of your questions. But you may be sure to get an answer from v.d. Waerden to the second (?) one, about the n congruences in n variables. It would be awfully nice if you could get a general result for the hyperelliptic case.

After carefully studying F.K. Schmidt [1] I am very much satisfied. This paper is really far better than Artin's, [2] for the simple reason that all his formulae and notions are birationally invariant. It is half as difficult as Artin's, once one has really got into touch with it. I am much more hopeful about general birational automorphisms now.

Kindest regards, and many thanks for your letter,

from Helmut.

[1] Hasse refers to the paper of F. K. Schmidt [FK:1931] who had defined the ζ-function of a function field over finite base field.

[2] This means Artin's thesis [A:1924].

1.18 07.02.1933, Hasse to Davenport

H. recommends to D. textbooks for finite fields. H. explains basics for multiplicative characters in finite fields.

7.2.33

Dear Harold,

Thanks awfully for your ciphered wishes. I did it in 5 minutes. Your clue was too obviously given. It contained an ambiguity, though, because it was not defined whether $\sqrt{5} \equiv 2\sqrt{3}$ or $-2\sqrt{3}$ mod. 7. — As to GF's, you will find a good account (a) in Haupt, Algebra II, Kap. 23,1, (b) Steinitz (Baer–Hasse), Alg. Theorie der Körper, §15, (c) v.d. Waerden, Mod. Algebra I, §31. Quadratic residuacity in a GF is not dealt with in those books, though. But it is absolutely trivial: All elements $\neq 0$ of a GF may be represented as powers of a "primitive" element. Those whose exponents are even are squares, the others not. There are $\frac{p^n-1}{2}$ squares $\neq 0$, and as many non–squares, in the $GF(p^n)$. The symbol of quadratic "residuacity" is best given as $\chi(a)$ or $\chi_2(a)$. $1+\chi(a)$ is the number of solutions of $x^2 = a$. Further $\sum_a \chi(a) = 0$. There is no need of representing the abstract GF as a residue class field for a prime ideal $\mathfrak{p}$ in a (suitably chosen) algebraic number field. If you do, you may write $\chi(a) = \left(\frac{a}{\mathfrak{p}}\right)$, where a now means an integer of that algebraic number field. I should not call $\chi(a)$ the "symbol of quadr. res." therefore, rather simply "the quadratic character" in GF. For the generalization of Mordell's and your results it is most convenient to make use of the following notation: $e(u) = e^{\frac{2\pi i}{p}S(u)}$, where $S(u) = u+u^p+\cdots+u^{p^{n-1}}$ (always an element of the $GF(p)$ contained in the $GF(p^n)$). Then $\chi(a) = \frac{1}{G(\chi)}\sum_t \chi(t)e(at)$, where $G(\chi) = \sum_t \chi(t)e(t) = \left\{ \begin{array}{ll} \sqrt{p^n}, & p^n \equiv 1\ (4) \\ i\sqrt{p^n}, & p^n \equiv -1\ (4) \end{array} \right\}$. Proof in the usual way. $G(\chi)^2 = \chi(-1)p^n$ is quite trivial, also $\chi(-1) = (-1)^{\frac{p^n-1}{2}}$; and I do not think you need more with any of your proofs. Similar facts hold, of course, for any m^{th} power character where $m \mid p^n - 1$. One thing is still important for both the proof of the above fact about $G(\chi)$ and your proofs altogether: $\sum_v e(uv) = \left\{ \begin{array}{lll} p^n & \text{for} & u = 0 \\ 0 & \text{"} & u \neq 0 \end{array} \right\}$. $u = 0$ is trivial,

$u \neq 0$ equivalent with $\sum_v e(v) = 0$. Represent here v by a basis $v_1, \ldots, v_n$ of n linearly independent elements (with respect to the rational $GF(p)$):

$$v = t_1 v_1 + \cdots + t_n v_n \ , \ t_i \text{ in } GF(p).$$

Then

$$\sum_v e(v) = \prod_{i=1}^{n} \sum_{t_i} e(t_i v_i).$$

Here $\sum_{t_i} = 0$ when $S(v_i) \neq 0$. Now suppose $S(v_i) = 0$ for $i = 1, \ldots, n$. Then by linear composition $S(v) = 0$ for every v of the $GF(p^n)$. This may be considered as an algebraic equation of degree p^{n-1} for v. Since the $GF(p^n)$ is a *field*, any algebraic equation has no more roots in it than its degree. Therefore $S(v) = 0$ is not true for all the p^n elements in $GF(p^n)$. Hence for at least one i $S(v_i) \neq 0$, $\sum_{t_i} = 0$; $\sum_v e(v) = 0$.

Best wishes, also from Clärle. I should like to learn more of Chowla's proof.

Yours,

Helmut.

1.19 11.02.1933, Davenport to Hasse

D. will be in Göttingen next semester. Chowla. Zeros mod p for the general cubic.

Cambridge, 11.2.1933.

My dear Helmut,

Very many thanks for your letter + card, also the Vorlesungsverzeichnis from Hamburg. I have decided in favour of Göttingen – chiefly on the ground that the place itself is more prepossessing. I am hoping that with the removal of the temptations to laziness which Cambridge offers, and with the change of life I shall get, I shall do more steady work, even if I do not go to any lectures.

As regards Schilling etc., I have not heard from them. Will you please give them my address in case you have not already done so? Perhaps you will ask Krämer what was said about the Ortskrankenkasse in the "Vergleich", if anything. I cannot write to the Insurance Co. until I have some direct demand from Schilling, so the sooner I hear from him the better.

I return your two letters with some corrections and notes.

I have just heard from Walfisz. Chowla's paper falls to the ground: it is quite correct in itself, but a paper of Titchmarsh in Rendiconti ... Palermo **54** (1930) on which it is based is wrong – at any rate unproved. T. quotes a result as proved by Landau but applies it in conditions other than those imposed by L. It is a great pity. It is conceivable, but unlikely, that the matter may be brought into order.

Treatment of general cubic. $f(x, y) \equiv 0$.

There must be at least one solution (x_0, y_0). One can prove this in either of two ways:

(a) Average $(N-p)^4$ over the p^{10} polynomials f, which have $> k.p^9$ automorphisms, whence
$$N - p = O(p^{3/4})$$
hence at least one Soln.

(b) a cubic has precisely one soln if

$$\left(\frac{-D}{p}\right) = -1 \qquad D = \text{disc.}$$

$D =$ sextic in y. Hence there exist values of y for which there is a soln in x (quoting result about $y^2 \equiv f_0(x)$.).

We can take the solution to be $(0,0)$. Then write zx for y + cancel x. Congruence becomes

$$x^2 f_3(z) + x f_2(z) + f_1(z) \equiv 0.$$

Number of solns = number of solns of

$$t^2 \equiv f_2^2 - 4 f_1 f_3$$

+ result $p + O\left(p^{2/3}\right)$ follows from result for $y^2 \equiv f_4(x)$.

$\boldsymbol{y^3 \equiv f_3(x)}$.

(a) Mordell's method.

Average $(N-p)^2$ over the p^4 polynomials, among which there are $> kp^4$ automorphisms. One finds

$$N - p = O\left(p^{\frac{1}{2}}\right).$$

(b) My method which yields precise value.

Sorry, the post is about to go. Will continue this tomorrow.

My warmest thanks to Clärle for her letter, + my love to her. Please tell her anything in this which would interest her.

I have been prevented from writing earlier today by the presence of an old acquaintance whom I have had to entertain.

Best wishes to Gertrudlein + to yourself

From, Your., Harold.

1.20 21.02.1933, Davenport to Hasse

D. explains to H. details for $y^3 \equiv f_3(x)$. D. is excited as to H.'s new idea for $y^2 \equiv f_3(x)$. Proofs for D.'s Crelle paper. Next term D. will go to Göttingen.

TRINITY COLLEGE,
CAMBRIDGE.
21.2.1933.

My dear Helmut,

Many thanks for your card. Excuse my long delay in sending the details of $y^3 \equiv f(x)$. I have been distracted by other things. Here is the proof at last. I feel sure there must be a simpler way of doing it from first principles.

One way of seeing that $y^3 \equiv f_3(x)$ has $p + \mathcal{O}(\sqrt{p})$ solns. is your *G.F.* method. In $G.F.[p^{3r}]$ $f_3(x)$ splits up, and the problem is the same as that for $y^3 \equiv f_2(x)$, and I suppose there will be no difficulty in showing that this has $p^r + \mathcal{O}(p^{\frac{1}{2}r})$ solutions. (In particular this follows from the corresponding result for $ax^2 + by^3 + c \equiv 0$, a case of $ax^m + by^n + c \equiv 0$). The result for p follows from that for $GF[p^{3r}]$, although $f_3(x)$ does not split up $\bmod p$. Have I understood your + Artin's method correctly ? Has any account of it appeared in print, by the way ?

I am much excited as to whether your new idea for $y^2 \equiv f_3(x)$ comes off. The result for $f_3(x, y) \equiv 0$ follows from it without further work. Are you going to get new automorphisms or birational transformations from your method, or what ?

Chowla's paper in itself was quite correct (it had a mistake in it but that was easily put right); it was Titchmarsh's that was wrong, on which it rested.

I received the proofs of my Crelle paper last Wednesday. You will receive them back from me in a few days with, I regret to say, numerous corrections. The printing is, in general, very good.

I have got permission to be at Göttingen next term, also an extra grant of £20 from the College. I expect I could have got more, but decided to be modest for once. A German student from Düsseldorf, called von Körke, is having my rooms.

Very many thanks for your P. C. from Winterberg, which has just arrived. Where have you been picking up all this slang ? Glad you had a good time. Best Wishes to all,

Harold.

1.21 06.03.1933, Davenport to Hasse

D. has a revival of interest in analytic number theory, and is reading Titchmarsh.

TRINITY COLLEGE,
CAMBRIDGE.
6. 3. 1933.

My Dear Helmut,

Many thanks for your letter. But surely the error term is at least $\mathcal{O}(\sqrt{p})$? If N_c is the no. of solutions of $y^2 \equiv f(x) + c$ one proves without difficulty that $\sum_c (N_c - p)^2 > Ap^2$ (A an absolute constant). Hence there exists a value of c for which $|N_c - p| > A_1\sqrt{p}$. Perhaps I have misunderstood you.

Just now I have had a revival of interest in the analytic th. of numbers + am reading Titchmarsh on the ζ–function.

The Insurance Co. are resigned to paying up, but wish to see whether anything was said about it in the terms of the "Vergleich".

Yours,

Harold

1.22 17.03.1933, Davenport to Hasse

D. is waiting with great eagerness to hear what the final results of H.'s work will be. Marvellous achievement. Solutions of other problems, e.g., Kloosterman sums. Question for the form of the ordinates of zeros of Artin's ζ-function. Revised proofs of D.'s Crelle paper. Must be an enormous amount of ingenuity in H.'s method. D. will meet H. in a fortnight.

TRINITY COLLEGE,
CAMBRIDGE.
17.3.1933.

My dear Helmut,

I must first apologize for not having written you anything but short notes for such a long time. The things which have kept me from doing so have been quite trivial — except for my lazyness, which is too serious to be trivial. ("Quite trivial" reminds me of a paper of Littlewood's on Ballistics which begins "The results of this paper were developed a few years ago for quite trivial purposes...", the purposes being in fact the War.)

I am waiting with great eagerness to hear what the final result of your work will be. It will be a marvellous achievement, + should lead to the solution of other problems, e. g. the Kloosterman sums (which are closely connected with $y^2 \equiv f_3(x)$). I re–read your letter in which you explained your method the other day, and can now follow it more or less in so far as it relates to $y^2 \equiv x^2 - 1$. But I do not see how you *discovered* the fact about $c(\frac{\mu}{p-1})$, $c(\frac{\nu}{p+1})$. What is the connection between the solutions as they arise in your method, and the parametric solution $x \equiv \frac{1}{2}(t + t^{-1})$, $y \equiv \frac{1}{2}(t - t^{-1})$?

I hope in a few days I shall be able to congratulate you on a final solution of the problem. What do you think the form of the ordinates of the zeros of Artin's ζ–function will be ?

I have heard from Chowla that he is able, without using Titchmarsh's paper, to prove

$$h(-\Delta) > \Delta^{\frac{1}{2}-\varepsilon}$$

provided Δ is not too composite, and to deduce $h(-\Delta) > 1$ for $\Delta > \Delta_0$.

I received a few days ago the revised proofs of my Crelle paper. I do not know whether to send them to you or to de Gruyter, but think I will send them to you to–morrow.

My mother has bought a house at Harrow–on–the–Hill (where the famous school is), a few miles NW of London. I have seen it, + I think it very nice, though I think our furniture is a little large for it. My father + mother send their very kindest regards + hope they will see you there in the not too distant future.

There must be an enormous amount of ingenuity in your method, when it comes to $y^2 \equiv f_3(x)$. Best wishes for its success, + your own general welfare till we meet in a fortnight.

Yours

Harold.

1.23 15.05.1933, Hasse to Davenport

H. has worked out the case $c_2 = 0$, i.e., $y^2 = x^3 - a$. D. should keep this letter and bring it next time. Basis for their common paper.

MATHEMATISCHES
SEMINAR
DER UNIVERSITÄT

MARBURG–LAHN,
DEN 15. 5. 33

My Dear Harold,

I have just worked out the case $c_2 = 0$. Everything turns out very simple:

$$y^2 = x^3 - a\,; \quad x^3 = y^2 + a \qquad \text{in } E_q \quad (q \equiv 1 \bmod 3)$$

$$\begin{aligned} N &= \sum_y (1 + \chi_3(y^2+a) + \overline{\chi}_3(y^2+a)) \\ &= q + \sum_y \chi_3(y^2+a) + \sum_y \overline{\chi}_3(y^2+a) \\ &= q + \qquad \pi \qquad + \qquad \overline{\pi}\,. \end{aligned}$$

$$\begin{aligned} \pi &= \sum_y \chi_3(y^2+a) = \sum_t \chi_2(t)\,\chi_3(t+a) \\ &= q\,\chi_6(a)\,\frac{\tau(\overline{\chi}_6)}{\tau(\chi_2)\,\tau(\overline{\chi}_3)} \qquad\qquad (\chi_6 = \chi_2\chi_3) \\ &= \chi_6(a)\,\frac{\tau(\overline{\chi}_6)\,\tau(\chi_3)}{\tau(\chi_2)}, \text{ since} \\ &\ \tau(\chi_3)\tau(\overline{\chi}_3) = q\chi_3(-1) = q\,. \end{aligned}$$

Analogously,

$$\overline{\pi} \;=\; \overline{\chi}_6(a)\,\frac{\tau(\chi_6)\,\tau(\overline{\chi}_3)}{\tau(\chi_2)}\,.$$

Hence

$$\pi\,\overline{\pi} \;=\; \frac{\tau(\chi_6)\,\tau(\overline{\chi}_6)\;\cdot\;\tau(\chi_3)\,\tau(\overline{\chi}_3)}{\tau(\chi_2)\,\tau(\chi_2)} \;=\; q\,\frac{\chi_6(-1)\,\chi_3(-1)}{\chi_2(-1)} \;=\; q\,,$$

i. e., π and $\overline{\pi}$ represent the factors of a decomposition of q in the 3^{rd} cyclotomic field.

Furthermore,

(a)

$$\begin{array}{ll} \tau(\overline{\chi}_6) \equiv \tau(\chi_2) \bmod. 1-\varrho & \qquad\qquad (\varrho = e^{\frac{2\pi i}{3}}) \\ \tau(\chi_3) \equiv -1 \quad \bmod. 1-\varrho & \\ \hline \end{array}$$

$$\pi \equiv -\chi_6(a) \;\; (\equiv -\chi_2(a)) \bmod. 1-\varrho\,,$$

(b)

$$\begin{array}{l} \tau(\overline{\chi}_6) \equiv \tau(\overline{\chi_3}) \qquad \bmod. 2 \\ \tau(\chi_2) \equiv -1 \;(\equiv 1) \bmod. 2 \\ \hline \end{array}$$

$$\pi \equiv -\chi_6(a) \;\; (\equiv \chi_3(a)) \;\; \bmod. 2\,.$$

Combined,

$$\pi \equiv -\chi_6(a) \bmod. 2(1-\varrho)\,.$$

Since, in the third cyclotomic field, $\phi(2(1-\varrho)) = 6$ and ± 1, $\pm\varrho$, $\pm\varrho^2$ represent all the prime residue classes mod. $2(1-\varrho)$, this congruence normalizes the factor π uniquely amongst the 6 associates.

I first tried to normalize mod. 3 instead of $2\,(1-\varrho)$ but without success.

The case $c_3 = 0$ must turn out quite analogous, though I wonder whether here the suitable modulus is $2\,(1-i)$ or something with 3, corresponding to the "strange" factor 2 in the above case.

Perhaps it is worth while checking my above normalization numerically. Or can you check by comparison with your result in the Crelle Paper ?

Best wishes and kindest regards,

yours,

Helmut

1.24 17.05.1933, Davenport to Hasse

From Göttingen. About cubic congruences. Heilbronn. Davenport participatess at Noether-Spaziergang.

$\sqrt{-3}$
$2(1-\rho)$
$(1-\rho)(1-\rho)$
$\sqrt{-3}\quad\sqrt{-3}$ *

Göttingen, Wed. 17 May 1933.

My dear Helmut,

Very many thanks for your letter. I do not see why the modulus $2(1-\rho)$ should be necessary. The normalisation is given completely ($q = p$ being assumed) in Bachmann, Kreistheilung, as follows :-

1) $p \equiv 1 \pmod 6$, χ a cubic character. Then

$$\sum_x \chi(x(x+1)) = a + b\rho$$

where a, b are determined uniquely [except that a, b may be replaced by $a-b, -b$, which happens when χ is replaced by $\overline{\chi}$] by:

$$\begin{aligned} p &\equiv (a+b\rho)(a+b\rho^2) \\ a &\equiv -1 \pmod 3 \qquad b \equiv 0 \pmod 3 \end{aligned}$$

Now if we take our congruence to be $y^2 \equiv x^3 + a$ ($\longrightarrow$ not the same as a above!), the number of solutions is

$$\begin{aligned} p &+ \sum_y \chi(y^2-a) + \sum_y \overline{\chi}(y^2-a) \\ = p &+ \chi_6(4a).\sum_y \chi\big(y(y+1)\big) + \overline{\chi}_6(4a)\sum_y \overline{\chi}\big(y(y+1)\big) \end{aligned}$$

[0]Diese Symbole sind mit Bleistift eingetragen, von Davenport's Hand.

(as is easily seen). *Hence the roots of the ζ-function are* ($p^s = z$) $z = c + d\rho, c + d\rho^2$ *where* $p = (c + d\rho)(c + d\rho^2)$ *and*

$$c + d\rho \equiv -\chi_6(4a) \bmod 3.$$

2) $p \equiv 1 \pmod 4$, χ a primitive quartic character. Then

$$\sum_x \chi\big(x(x+1)\big) = a + bi$$

where a, b are determined uniquely (except for the sign of b) by $p = a^2 + b^2$,

$$a \equiv -(-1)^{\frac{p-1}{4}} \bmod 4, \quad b \equiv 0 \bmod 2.$$

Now if we take our congruence to be $y^2 = x^3 - gx$, the number of solutions is

$$p + \sum_x \left(\frac{x(x^2-g)}{p}\right) = p + S + \overline{S}$$

where

$$\begin{aligned} S = \sum_x \chi(x)\left(\frac{x-g}{p}\right) &= \sum_x \chi^2(x)\chi(x+g) \\ &= \overline{\chi}(g)\sum_x \chi\big(x(x+1)\big) \end{aligned}$$

(on replacing x by $\frac{g}{x}$). Hence the roots of the ζ-function are given by

$$z = \overline{\chi}(g)(a + bi), \quad \chi(g)(a - bi)$$

with a, b normalised as above.

The proofs of Bachmann are quite trivial, I will consider how they go for E_q ($q = p^f$ $f > 1$).

Yesterday I had Heilbronn to supper: this afternoon I have been on the Noether-Spaziergang from which you received the postcard.

Yours in haste

Harold

1.25 17.05.1933, Hasse to Davenport

H. has simplified the case $c_2 = 0$, and treated $c_3 = 0$ in similar way.

MATHEMATISCHES
SEMINAR
DER UNIVERSITÄT

MARBURG–LAHN,
DEN 17. 5. 33

My dear Harold,

I have still simplified the case $c_2 = 0$, and I have treated the case $c_3 = 0$ entirely the same way. No other knowledge about Gaussian sums is required than the absolute value and the reduction with the character.

Here are the details:

I. $\underline{y^2 = x^3 + a}$, in any E_q with $q \equiv 1 \bmod. 3$, $a \neq 0$.

$$\begin{aligned} N &= q + \sum_y \chi_3(y^2 - a) + \sum_y \overline{\chi}_3(y^2 - a) \\ &= q - \qquad \pi \qquad - \qquad \overline{\pi}\,. \end{aligned}$$

π and $\overline{\pi}$ are conjugate complex numbers in the third cyclotomic field.

$$\begin{aligned}
\pi &= -\sum_y \chi_3(y^2-a) = -\sum_t \chi_2(t)\,\chi_3(t-a) \\
&= \frac{-1}{\tau(\chi_2)\,\tau(\overline{\chi}_3)} \sum_{u\,,\,v} \chi_2(u)\,\overline{\chi}_3(v) \sum_t e(tu+(t-a)v), \\
&\qquad\qquad e(\xi) = e^{\frac{2\,\pi\,i}{p}\,S(\xi)} \\
&\qquad\qquad \tau(\chi) = \sum_\xi \chi(\xi)\,e(\xi)\,, \\
&= \frac{-q}{\tau(\chi_2)\,\tau(\overline{\chi}_3)} \sum_u \chi_2(u)\,\overline{\chi}_3(-u)\,e(au), \\
&\qquad\qquad \text{since } \sum_\xi e(\alpha\xi) = \left\{ \begin{array}{lll} q & \text{for} & \alpha = 0 \\ 0 & \text{for} & \alpha \neq 0 \end{array} \right\} \text{ in } E_q\,, \\
&= -\chi_6(a)\,\frac{q\,\tau(\overline{\chi}_6)}{\tau(\chi_2)\,\tau(\overline{\chi}_3)}\,, \\
&\qquad\qquad \text{since } \chi_2(u)\,\overline{\chi}_3(-u) = \chi_2(u)\,\overline{\chi}_3(u) = \overline{\chi}_6(u)\,, \\
&\qquad\qquad \text{and the character reduction of the Gaussian sum.}
\end{aligned}$$

Hence $|\pi| = q^{\frac{1}{2}}$, i. e.,

$$\underline{\pi\,\overline{\pi} = q}\,.$$

Furthermore,

$$\pi = -\sum_t \chi_2(t)\,\chi_3(t-a) \equiv$$

$$\equiv \left\{ \begin{array}{lllll} -\sum_{t\neq 0} \chi_3(t-a) & = \chi_3(-a) & = \chi_3(a) & \equiv \chi_6(a) \bmod. 2\,, \\ -\sum_{t\neq a} \chi_2(t) & = \chi_2(a) & \equiv \chi_6(a) & \bmod. 1-\varrho\,, \end{array} \right.$$

hence

$$\underline{\pi \equiv \chi_6(a) \bmod. 2(1-\varrho)}\,.$$

Since, in the third cyclotomic field,

$$\left\{ \begin{array}{lll} \phi(2) & = 3 & \text{with } 1,\ \varrho,\ \varrho^2 \text{ as the prime residues mod. } 2 \\ \phi(1-\varrho) & = 2 & \text{with } 1,\ -1 \text{ as the prime residues mod. } 1-\varrho \end{array} \right\},$$

this congruence gives a unique normalization for π amongst the associates $(-1)^\mu \varrho^\nu \pi$.

II. $\underline{y^2 = 4x^3 + ax}$, in any E_q with $q \equiv 1 \bmod. 4$, $a \neq 0$.
$y = xz$ gives

$$xz^2 = 4x^2 + a\,; \text{ i. e.}, \quad 4x^2 - xz^2 + a = 0$$

with $N' = N - 1$ solutions, since to $x = y = 0$ no solution x, z corresponds, whereas the transformation is a "one–one one" for $x \neq 0$.

$$\begin{aligned} N' &= q + \sum_z \chi_2(z^4 - 16a) = q + \sum_z \chi_2(z^4 - a) \quad (\text{ by } z' = 2z) \\ &= q + \sum_t \chi_2(t-a)\Big(1 + \chi_2(t) + \chi_4(t) + \overline{\chi}_4(t)\Big) \end{aligned}$$

Now

$$\begin{aligned} &\sum_t \chi_2(t-a)(1 + \chi_2(t)) = \sum_t \chi_2(t^2 - a) = \\ &= N(t^2 - a = u^2) - q = (q-1) - q = -1\,. \end{aligned}$$

Hence

$$\begin{aligned} N &= N' + 1 \\ &= q + \sum_t \chi_2(t-a)\,\chi_4(t) + \sum_t \chi_2(t-a)\,\overline{\chi}_4(t) \\ &= q - \qquad \pi \qquad - \qquad \overline{\pi}\,. \end{aligned}$$

π and $\overline{\pi}$ are conjugate complex numbers in the fourth cyclotomic field.

$$\begin{aligned} \pi &= -\sum_t \chi_2(t-a)\,\chi_4(t) \\ &= \frac{-1}{\tau(\chi_2)\,\tau(\overline{\chi}_4)} \sum_{u\,,\,v} \chi_2(u)\,\overline{\chi}_4(v) \sum_t e((t-a)u + tv) \\ &= \frac{-q}{\tau(\chi_2)\,\tau(\overline{\chi}_4)} \sum_v \chi_2(-v)\overline{\chi}_4(v)\,e(av) \\ &= \frac{-q}{\tau(\chi_2)\,\tau(\overline{\chi}_4)} \sum_v \chi_4(v)\,e(av), \\ &\qquad\qquad \text{since } \chi_2(-1) = 1 \ (q \equiv 1 \bmod. 4) \\ &\qquad\qquad \text{and } \chi_2(v)\,\overline{\chi}_4(v) = \chi_4(v) \\ &= -\overline{\chi}_4(a)\frac{q\tau(\chi_4)}{\tau(\chi_2)\,\tau(\overline{\chi}_4)}\,. \end{aligned}$$

Hence $|\pi| = q^{\frac{1}{2}}$, i. e.,

$$\underline{\pi\,\overline{\pi} = q}\,.$$

Furthermore,

$$\begin{aligned}
\pi &= -\sum_{t} \chi_2(t-a)\chi_4(t) \\
&= -\sum_{t\neq a} \chi_2(t-a)\chi_4(t) \\
&= -\sum_{t\neq a} \chi_4(t) + \sum_{t\neq a} \Big(1-\chi_2(t-a)\Big)\,\chi_4(t) \\
&= \chi_4(a) + \sum_{t\neq a} \Big(1-\chi_2(t-a)\Big)\,\chi_4(t) \\
&= \chi_4(a) + \sum_{t\neq 0\,,\,a} \Big(1-\chi_2(t-a)\Big)\,\chi_4(t) \\
&\equiv \chi_4(a) + \sum_{t\neq 0\,,\,a} \Big(1-\chi_2(t-a)\Big) \bmod .\, 2(1-i)\,, \\
&\quad \text{since } 1-\chi_2(t-a) \equiv 0 \bmod .\, 2\,, \\
&\quad \chi_4(t) \equiv 1 \bmod .\, 1-i \text{ for } t\neq 0\,,\ a\,, \\
&\equiv \chi_4(a) + q - 2 + \chi_2(-a) \equiv \chi_4(a) - 1 + \chi_2(a) \bmod .\, 2(1-i)\,, \\
&\quad \text{since } \chi_2(-1) = 1\,,\ q\equiv 1 \bmod .\, 4\,, \text{ and } 2(1-i) \text{ divides } 4\,.
\end{aligned}$$

Now, for $a \neq 0$,

$$\begin{array}{rcl}
\chi_2(a)-1 & \equiv & 0 \bmod .\, 2 \\
1-\chi_4(a) & \equiv & 0 \bmod .\, 1-i \\
\hline
\Big(\chi_2(a)-1\Big)\Big(1-\chi_4(a)\Big) & \equiv & 0 \bmod .\, 2(1-i) \\
\chi_2(a)-1+\chi_4(a) & \equiv & \chi_2(a)\,\chi_4(a) = \overline{\chi}_4(a) \bmod .\, 2(1-i)\,.
\end{array}$$

Hence

$$\underline{\pi \ = \ \overline{\chi}_4(a) \bmod .\, 2(1-i)}\,.$$

Since, in the fourth cyclotomic field, $\phi(2(1-i)) = \phi((1-i)^3) = 4$ with ± 1, $\pm i$ as the prime residues mod. $2(1-i)$, this congruence gives a unique normalization for π amongst the associates $i^{\mu}\pi$.

Please keep this letter and bring it here the next time. It may serve us as a basis for our common paper designed. I am, of course, ready to accept any improvements you may be able to suggest. One slight improvement of the notation may be convenient: in order to get rid of the bar in the normalization of the second case, one ought to define π as the above $\overline{\pi}$. The sign $-$ before the sums defining π and $\overline{\pi}$ makes those numbers the exact roots of the ζ–polynomial. It ought to remain therefore.

We spent a jolly day yesterday and received quite a few presents and congratulations. Clärle will have told you more about it in last night's letter. Otherwise "Im Westen nichts Neues". What about the "Osten"?

Yours,

Helmut.

1.26 18.05.1933, Hasse to Davenport

Referring to Bachmann. In the case $c_3 = 0$ H. has learned from D. to begin more simply. As H. has said to D. and also to his american friend Albert: Work with algebraic numbers instead of with their coordinates. Politics: Times cutting and Papen's speech. Say thanks to Emmy Noether. Question about Neugebauer.

MATHEMATISCHES
SEMINAR
DER UNIVERSITÄT

MARBURG–LAHN,
DEN 18.5.33

My dear Harold,

Many thanks for your letter. You will have got mine by now. Although the case $c_3 = 0$ is now also settled by referring to Bachmann, I do not think it will be convenient "to actually refer" to Bachmann in either case. For we would have to point out how Bachmann's argument generalises to E_q. Although this argument is quite trivial, as you affirm, it will not be shorter than my few–lined congruence considerations, I am sure. It may even prove to be identical with my argument.

In the case $c_2 = 0$, your (i. e., Bachmann's) modulus $3 \sim (1-\varrho)(1-\varrho)$ is just as good as my modulus $2(1-\varrho)$. For both, a complete system of prime residues is given by ± 1, $\pm\varrho$, $\pm\varrho^2$. There is no question of one of them being "necessary", or the other "sufficient". One may choose between them on equal terms according to suitability. I would readily accept 3 for the final form provided you can derive the normalisation congruence for it just as simply as I have done for $2(1-\varrho)$. On the other hand, $2(1-\varrho)$ fits better in with the general case, where the normalisation depends on congruences mod. 4.

In the cases $c_3 = 0$, I have learned from you now how to begin more simply. My transformation of $y^2 = 4x^3 + a$ to $4x^2 - xz^2 + a = 0$ is not at all elegant. Your way of getting down to a character sum:

$$\begin{aligned} N &= q + \sum_x \chi_2(x(4x^2+a)) \\ &= q + \sum_t (1+\chi_2(t))\,\chi_4(t)\chi_2(4t+a) \\ &= q + \sum_t \chi_4(t)\,\chi_2(4t+a) + \sum_t \overline{\chi}_4(t)\chi_2(4t+a)\,, \end{aligned}$$

is obviously *the* one. I had some difficulty in identifying this representation of N with my former:

$$N \;=\; q + \sum_t \chi_4(t)\chi_2(t-a) + \sum_t \overline{\chi_4}(t)\chi_2(t-a)\,,$$

until I discovered the plain fact: $\chi_4(-4) = 1$ for $q \equiv 1 \mod. 4$, which shows at once that both representations are termwise identical. As to that plain fact, one has indeed $\chi_4(-4) = \chi_4(-1)\,\chi_2(2)$, and $\chi_4(-1) = (-1)^{\frac{q-1}{4}}$, $\chi_2(2) = (-1)^{\frac{q^2-1}{8}} = (-1)^{\frac{q-1}{4}}$ for $q \equiv 1 \mod. 4$. The equation $y^2 = 4x^3 + a$ is therefore equivalent to $y^2 = x^3 - a$. We should better start with the latter now, though the former is better in accordance with the general case. Bachmann's normalisation:

$$q = a^2 + b^2 = (a+bi)(a-bi) = \pi\overline{\pi} \text{ with } a \equiv -(-1)^{\frac{q-1}{4}} \mod. 4\,,$$

$$b \equiv 0 \mod. 2\,,$$

is clumsily expressed, because there is a dependency between the character

$$(-1)^{\frac{q-1}{4}} \;=\; \chi_4(-1)$$

and the congruence value of $b \mod. 4$. Obviously,

$$\begin{aligned} &b \equiv 0 \mod. 4 \quad \text{for} \quad q \equiv 1 \mod. 8\,, \text{ i. e., for } (-1)^{\frac{q-1}{4}} = +1\,, \\ &b \equiv 2 \mod. 4 \quad \text{for} \quad q \equiv 5 \mod. 8\,, \text{ i. e., for } \qquad \text{"} \qquad = -1\,. \end{aligned}$$

On account of this, the normalisation is simply:

$$\pi \equiv -1 \mod. 2(1-i)\,.$$

With my argument, one is lead to this type of congruence at once, without having first to go down to the rational coordinates a, b and introducing the above alternative.

Here is a very striking example for a general remark that I have often made to you and also to my American friend Albert: One has learnt to–day operating with algebraic numbers as "real entities", whereas in those times one did not know yet to make full use of all the advantages brought about by the algebraic number theory even when one was dealing with problems involving algebraic numbers.

Many thanks for the Times cutting. Though I can understand the mentality out of which it was written, I dare say it is rather saucy, in particular the point about our going to war in 1914 having been an "ambitious folly". To–day, one knows only too well that Germany was far more threatened by the encircling policy of your King Edward and all its consequences than England was by our going through Belgium. Although it is certainly bad that Germany is losing all sympathy from the other countries, she had to show some grit and backbone at least after all those years of willingly bending down under the dictate of Versailles. That is what I liked in v. Papen's speech. We have now the great answer from Hitler to all the stir round our borders and in particular to the Geneva proceedings. I quite agree with every single word of it. This speech, and the foreign policy which I hope will follow it accordingly, is exactly what I hoped for together with so many of my countrymen when I gave my vote to the Nazis. It is somehow tragical that this sort of foreign policy could not be brought about without the personal drawbacks for learned men in Germany. As it is, one has to take them as a sacrifice and to hope that reason will come back in due course. As to v. Papens speech, I must say that the Times is certainly wrong by taking it as any sort of a new–war–fanfare.

I have got on with "Faraway". I like it very much. The Lancashire man reminds me of what I saw there last "fall".

Please say E. Noether my warmest thanks for the delightful post–card. Could you make out whether Neugebauer will be in Göttingen on Wednesday 24^{th} or Thursday 25^{th} ? A telephone call will do.

You would do me a great favour by helping me with the proof correction of Wilton's new Crelle paper. I have got the Revision to–day, and I should like you to look it over. May I send on the thing to you ?

I enclose our account. Could you possibly return half of your debt in cash now ? We will put the other half off our account from last year.

Kindest regards,

yours,
Helmut

1.27 21.06.1933, Davenport to Hasse

Relation between Gaussian sums can be proved very easily.

Göttingen, Wed. 21 June 1933.

My dear Helmut,

The relation we spent so much time looking for is incredibly simple :-

$$\frac{\tau(\psi)\tau(\chi\psi)\tau(\chi^2\psi)\ldots\tau(\chi^{n-1}\psi)}{\tau(\psi^m)} = \varepsilon \cdot p^{\frac{m-1}{2}}. \tag{1}$$

Here χ, ψ are characters of order m, n resp., where $(m,n) = 1$, and ε is an mn th root of unity depending on ψ and m; $\varepsilon = \varepsilon_m(\psi)$.

I use your notation. □□□ $\vartheta = \zeta\eta$. Define $\mathfrak{R}_{\mu,\nu}$ by

$$\vartheta \equiv R^{-\frac{(p-1)}{mn}(n\mu+m\nu)} \pmod{\mathfrak{R}_{\mu,\nu}} \qquad \begin{array}{ll} 1 \leqq \mu \leqq m-1, & (\mu,m) = 1. \\ 1 \leqq \nu \leqq n-1, & (\nu,n) = 1. \end{array}$$

Then one can prove without difficulty that for any j (including 0 and other numbers not necessarily prime to m) we have

$$\left(\tau(\chi^j\psi)\right)^{mn} = \prod_{\substack{\mu,\nu \\ \text{as above}}} \mathfrak{R}_{\mu,\nu}^{[nj\mu+m\nu]_{mn}}.$$

Also

$$(\tau(\psi^m))^{mn} = \prod_{\substack{\mu,\nu \\ \text{as above}}} \mathfrak{R}_{\mu,\nu}^{m[m\nu]_n}.$$

We therefore have to prove the following identity: if $(m,n) = (m,\mu) = (n,\nu) = 1$, then

$$\sum_{j=0}^{m-1}[nj\mu + m\nu]_{mn} = nm\frac{(m-1)}{2} + m[m\nu]_n.$$

This is immediate: the sum on the left is the same as

$$\sum_{j=0}^{m-1}[jn + m\nu]_{mn},$$

and

$$[jn+m\nu]_{mn} = \begin{cases} jn+m\nu & \text{if} \quad jn+m\nu < mn \\ jn+m\nu-mn & \text{if} \quad jn+m\nu \geqq mn. \end{cases}$$

Hence the sum is

$$n\frac{m(m-1)}{2} + m^2\nu - mn \sum_{j=m-\frac{m\nu}{n}}^{m-1} 1 = n\frac{m(m-1)}{2} + m^2\nu - mn \sum_{j=1}^{\frac{m\nu}{n}} 1$$

$= nm\dfrac{m-1}{2} + m^2\nu - mn\left(\dfrac{m\nu}{n} - \dfrac{1}{n}[m\nu]_n\right)$ as stated.

□□□

The only values of m for which I have been able to determine ε are powers of 2 : for m a power of 2 and any ψ ε is expressibly easily in terms of $\psi(2)$ and $\dfrac{\tau(\chi_2)}{\sqrt{p}} = i^{\left(\frac{p-1}{2}\right)^2}$.

It may be that ε in the general case is also determinable, but I do not see how to do it.

I cannot believe that (1) is not already well known.

This afternoon I read "The Motor Rally Mystery", a moderately good detective story.

The very best wishes,

Yours,

Harold

P.S. I wonder if I shall receive a letter from you in the morning with roughly the same contents!

1.28 21.06.1933, Hasse to Davenport

Relation about products of Gaussian sums.

MATHEMATISCHES
SEMINAR
DER UNIVERSITÄT

MARBURG–LAHN,
DEN 21.6.33

My dear Harold,

Your relation is alright. It generalises at once to

$$\frac{\tau(\psi)\,\tau(\chi\psi)\cdots\tau(\chi^{m-1}\psi)}{\tau(\psi^m)\,\tau(\chi)\cdots\tau(\chi^{m-1})} \sim 1\,,$$

where m, n are any numbers prime to each other.

I use our recent notations with a slight alteration, namely $mm'+nn'=1$, which seems better.

We know already

$$\exp_{\mathfrak{p}_\mu}\tau(\chi^\alpha)^m \;=\; [\mu\alpha]_m \quad \text{for } (\alpha,\, m)=1\,.$$

I first prove that this also holds when $(\alpha,\, m)\neq 1$. For, when $m=m_0 d$,

$$\tau(\chi^d)^{m_0} \sim \prod_{\mu_0} \mathfrak{p}_{\mu_0}^{(0)\mu_0} \quad (\mu_0 \text{ reduced mod. } m_0)\,.$$

Further, by the usual argument,

$$\mathfrak{p}_{\mu_0}^{(0)} \;=\; \prod_{\mu\equiv\mu_0(m_0)} \mathfrak{p}_\mu\,.$$

Hence

$$\tau(\chi^d)^{m_0} \sim \prod_{\mu} \mathfrak{p}_\mu^{\mu_0}$$

where μ_0 denotes the reduced residue of μ mod. m_0 for every μ, i. e.,

$$\tau(\chi^d)^m \sim \prod_\mu \mathfrak{p}_\mu^{\mu_0\, d} = \prod_\mu \mathfrak{p}_\mu^{[\mu\, d]_m},$$

since $\mu_0 d$ is the reduced residue of μd mod. m. Starting from $\tau(\chi^{\alpha_0 d})^{m_0}$ with $(\alpha_0, m_0) = 1$ instead of $\tau(\chi^d)^{m_0}$, one finds

$$\tau(\chi^{\alpha_0 d})^m \sim \prod_\mu \mathfrak{p}_\mu^{[\mu_0\, \alpha_0]_{m_0} d} = \prod_\mu \mathfrak{p}_\mu^{[\mu\, \alpha_0 d]_m},$$

which proves the above assertion. Notice that this holds also for the trivial case $m_0 = 1, d = m$. Here $\mu_0 = 0$, and $\tau(\chi^m) = \sum_{x \bmod p} \chi^m(x)\, e(x) = \sum_{x \not\equiv 0 \text{ mod.} p} e(x) = -1 \sim \prod_\mu \mathfrak{p}_\mu^\sigma$. It is very convenient to allow this case in the following argument.

Applying our former argument to $\tau(\chi^\alpha \psi)^{mn}$, and observing that it does not matter there whether α is prime to m or not because of the above argument, we have[1]

$$\begin{aligned}
\exp_{\mathfrak{r}_{\mu,\nu}} \tau(\chi^\alpha\psi)^{mn} &= [\alpha\mu\nu]_{mn} \Big(\text{reduced residue } \textit{mod. } mn, \\
&\qquad \text{which is } \equiv \alpha\mu\,(m) \text{ and } \equiv \nu\,(n) \Big) \\
&= n[n'\alpha\mu]_m + \nu - n[n'\nu] + \\
&\quad + \begin{cases} 0 & \text{for } [n'\nu]_m \le [n'\alpha\mu]_m \\ mn & \text{''}\quad [n'\nu]_m > [n'\alpha\mu]_m, \end{cases}
\end{aligned}$$

since $nn' \equiv 1\,(m)$ and therefore this expression is obviously $\equiv \alpha\mu\,(m)$, $\equiv \nu\,(n)$, and since it is obviously reduced mod. mn. Summing up over $\alpha = 0, \ldots, m-1$ and noticing that the second case in the alternative happens for exactly $[n'\nu]_m$ values of α, we have

$$\exp_{\mathfrak{r}_{\mu,\nu}} \prod_{\alpha=0}^{m-1} \tau(\chi^\alpha\varphi) = n\sum_\alpha [n'\alpha\mu]_m + m\nu\,.$$

On the other hand,

$$\exp_{\mathfrak{r}_{\mu,\nu}} \prod_{\alpha=0}^{m-1} \tau(\chi^\alpha)^{mn} = n\sum_\alpha [n'\alpha\mu]_m$$

[1] Hasse schreibt in der folgenden Formelzeile $[\alpha\mu, \nu]_{mn}$, und wir haben das durch $[\alpha\mu\nu]_{mn}$ ersetzt.

$$\exp_{\mathfrak{r}_{\mu,\,\nu}} \tau(\psi^m)^{mn} \;=\; m\nu\,.$$

This proves the above relation.

The value of the quotient in question is therefore an algebraic unit, hence a root of unity by Hilbert, Satz 48.

That quotient is unaltered by $\mathsf{Z} \longrightarrow \mathsf{Z}^R$, since it takes the factor

$$\frac{\overline{\psi}\cdot\overline{\psi}\overline{\chi}\cdots\overline{\psi}\overline{\chi}^{m-1}}{\overline{\psi}^m\cdot\overline{\chi}\cdots\overline{\chi}^{m-1}}(R) \;=\; 1\,.$$

And it is symmetric in the $m^{\underline{th}}$ roots of unity with regard to its structure in the χ^α's. It is therefore an $n^{\underline{th}}$ root of unity, when n is even. When n is odd, it is at any rate a $(2n)^{th}$ root of unity.

Suppose now first m is a prime power, $m = \ell^\mu$. Then $1-\zeta$ is the prime divisor of ℓ in k_m, the field of the primitive $m^{\underline{th}}$ root of unity ζ.

$$\overline{\chi},\, \overline{\chi}^2,\, \ldots,\, \overline{\chi}^{m-1} \equiv 1 \text{ mod.}\, 1-\zeta$$

(except the argument is naught which may be excluded in the Gaussian sums) Therefore every $\tau(\psi\chi^\alpha) \equiv \tau(\psi) \text{ mod}\, 1-\zeta$, and every $\tau(\chi^\alpha) \equiv -1 \text{ mod}\, 1-\zeta$.

Furthermore,

$$\begin{aligned}\tau(\psi^m) \;&=\; \sum_{x\not\equiv 0\,(p)} \psi(x)^m e(x) \equiv \Big(\sum_{x\not\equiv 0\,(p)} \psi(x) e(\frac{x}{m})\Big)^m \text{ mod.}\,\ell\\ &\equiv\; \psi(m)\,\tau(\psi)^m \text{ mod.}\,\ell\,.\end{aligned}$$

Hence

$$\frac{\tau(\psi)\,\tau(\chi\psi)\cdots\tau(\chi^{m-1}\psi)}{\tau(\psi^m)\,\tau(\chi)\cdots\tau(\chi^{m-1})} \equiv (-1)^{m-1}\overline{\psi}(m) \text{ mod.}\, 1-\zeta\,.$$

This congruence holds, of course, also for the least power of $1-\zeta$ representing an ideal in k_n, i. e., for ℓ as modulus. Since the roots of unity in k_n (i. e., the n^{th} or $2n^{th}$ roots of unity) are incongruent mod. ℓ, the congruence must be an equality. This argument only fails when m is a power of 2 (and accordingly n is odd), because only the n^{th}, but not the $2n^{th}$, roots of unity are incongruent mod. 2. In this case a sign $\pm$ remains undetermined. Suppose n also is a prime power, and $1-\eta$ the corresponding prime divisor in k_n. Then

$$\begin{aligned}\tau(\psi) \;&\equiv\; -1 \text{ mod } 1-\eta\\ \tau(\psi^m) \;&\equiv\; -1 \text{ mod } 1-\eta\\ \tau(\chi^\alpha\psi) \;&\equiv\; \tau(\chi^\alpha) \text{ mod } 1-\eta\,,\end{aligned}$$

hence the quotient $\equiv 1 \bmod 1-\eta$. On the other hand it is $= \pm(-1)^{m-1}\overline{\psi}(m)$. Since $\overline{\psi}(m) \equiv 1 \bmod 1-\eta$, one has $\pm(-1)^{m-1} \equiv 1 \bmod 1-\eta$, i. e. $\pm(-1)^{m-1} = 1$. When n is composite, and n_0 arises from n leaving out one of its prime powers with corresponding divisor $1-\eta$, I split ψ up into ψ_{n_0} and ψ'. Since $\psi' \equiv 1 \bmod. 1-\eta$, the quotient reduces mod. $1-\eta$ to the quotient for ψ_{n_0}. Thus one finds that the above holds also for composite n (using induction).

We know now

$$\frac{\tau(\psi)\,\tau(\chi\psi)\cdots\tau(\chi^{m-1}\psi)}{\tau(\psi^m)\,\tau(\chi)\cdots\tau(\chi^{m-1})} = \overline{\psi}(m) \quad \text{when } m \text{ is a prime power.}$$

The same procedure for the prime powers of m, as we have just applied to the prime powers of n, leads to extending this result for any m.

I hope this is alright. Perhaps you will be able to test it in some special cases.

Please excuse me for writing so disorderly as regards both handwriting and argument. I am fairly short with my time and had to take down the last argument as it occured to me while writing.

The next step in this matter must be to investigate the connexion between the Gaussian sums belonging to powers of the same prime ℓ as orders of the characters.

I am very sorry I opened by accident the enclosed notice to you from the bank. I hope you will forgive me intruding thus upon your business.

I think I have got an idea for the determination of the exponents for Gaussian sums which belong properly to Galois fields. Perhaps more about this one of the next days.

Much love,

yours

Helmut.

1.29 23.06.1933, Hasse to Davenport

A more general relation between Gaussian sums.

MATHEMATISCHES
SEMINAR
DER UNIVERSITÄT

MARBURG–LAHN,
DEN 23. 6. 33

My dear Harold,

In a great hurry the following results:

1.) The formula

$$Q_m(\psi) \;=\; \frac{\prod_{\mu=0}^{m-1}\tau(\chi^\mu\psi)}{\tau(\psi^m)\cdot\prod_{\mu=1}^{m-1}\tau(\chi^\mu)} \;=\; \varepsilon$$

(root of unity in the n^{th} cyclotomic field) holds for any m, $n \geq 1$. The proof is much simpler, than my first in the special case, and somewhat simpler than yours. I profited by yours in finding my new one.

It is convenient, to consider the prime decomposition for all τ's in the $(p-1)^{th}$ cyclotomic field k_{p-1}. One can restrict the investigation to only one prime ideal $\mathfrak{p}|p$ in k_{p-1} which stands for all.

When $\tau(\chi) = \sum_a \overline{\chi}(a)\mathsf{Z}^a$ (the bar is convenient) and α is the uniquely determined reduced residue mod. $p-1$ with

$$\chi(a) \equiv a^\alpha \text{ mod. } \mathfrak{p} \text{ for every } a\,,$$

(this α is a function of χ and $\mathfrak{p}$; it may also be considered as a normalisation of $\mathfrak{p}$), then

$$\tau(\chi) \text{ contains exactly the power } \mathfrak{p}^\alpha\,.$$

From this and your reduction of

$$[\mu\alpha+\nu\beta]_{p-1} = [\mu\alpha]_{p-1} + [\nu\beta]_{p-1} \quad \begin{cases} -0 \\ -p-1 \end{cases}$$

(summing up over μ mod. m and putting $\nu = 1$) the above result follows in a now quite familiar manner.

2.) I suppose,

$$Q_m(\psi) = \psi^m(m)\,.$$

I can prove this in a number of cases, and I suppose my method will do it in due course. Only I have not had much time yet. My last letter was slightly wrong, so far as I remember, in that I forgot the exponent m to ψ.

3.) Do you think there are further relations, not covered by the above ? It would be very interesting to have the *complete* system of relations.

Much love,

Helmut.

1.30 25.06.1933, Postcard Hasse to Davenport

Exponent to which the higher Gaussians contain a prime ideal.

25.6.33.

Dear Harold,

After finishing with the relation problem I have tried to determine the exponent to which the higher Gaussians contain a prime ideal. Although I cannot give a complete proof, I think I know the truth by now. Let χ be a character of E_{p^f} , and $\tau(\chi) = \sum_x \overline{\chi}(x) \mathsf{Z}^{S(x)}$, x running through E_{p^f} . $\tau(\chi)$ belongs to $k_{p^f-1}(\mathsf{Z})$, $\tau(\chi)^{p-1}$ to k_{p^f-1} itself, because $\chi(b)^{p-1} = 1$ for rational b . In k_{p^f-1} , p splits into prime ideals $\mathfrak{p}$ of degree f , i. e. , the residue classes mod. $\mathfrak{p}$ form a field of type E_{p^f} . Identifying the given E_{p^f} with the residue system of one of the $\mathfrak{p}$'s, $\chi(x)$ has a representation $\chi(x) = \xi^{-\alpha}$, where α is a constant, and ξ the $(p^f - 1)^{th}$ root of unity belonging to the residue class x . Also $S(x) \equiv S_{\mathfrak{p}}(\xi)$ mod. p , where $S_{\mathfrak{p}}(\xi)$ is the trace determined from the congruence mod. $\mathfrak{p}$ of degree f satisfied by ξ . Hence $\tau(\chi) = \sum_\xi \xi^{-\alpha} \mathsf{Z}^{S_{\mathfrak{p}}(\xi)}$ where ξ runs through all $(p^f - 1)^{th}$ roots of unity (ordinary algebraic numbers). Let α be reduced mod. $p^f - 1$ and $\alpha = \alpha_0 + \alpha_1 p + \cdots + \alpha_{f-1} p^{f-1}$. Then $\tau(\chi)^{p-1}$ contains exactly $\mathfrak{p}^{s_\alpha}$ where $s_\alpha = \alpha_0 + \cdots + \alpha_{f-1}$. Notice that α is not uniquely determined by χ and $\mathfrak{p}$, because a correspondence between E_{p^f} and the residues mod $\mathfrak{p}$ may be modified by applying any of the automorphisms $x \longrightarrow x^{p^i}$. Therefore only the set αp^i (mod. $p^f - 1$) is uniquely determined by χ and $\mathfrak{p}$. But s_α is invariant replacing α by αp^i . I can give the proof only for special cases. A nearly "rational" formulation is:

$$\sum_{a \text{ mod. } p} \sum_{\kappa=1}^{p-1} (-1)^\kappa \binom{a}{\kappa} \Pi^\kappa \sum_\xi \xi^{-\alpha} (\xi + \cdots + \xi^{p^{f-1}} - a)^{p^f(p-1)} ,$$

where ξ runs through all $(p^f - 1)^{th}$ roots of unity, is divisible exactly by Π^{s_α} . Here $\Pi = 1 - \mathsf{Z}$ is the well–known prime divisor of p in $R(\mathsf{Z})$, i. e. , $\Pi^{p-1} \sim p$.

When my proof is complete, it will furnish a new proof of your f^{th} power theorem, for I require no restriction on χ with this line of argument. But the whole thing looks rather complicated so far. Anyhow, I will not give in until the matter is definitely settled.

Much love,

Helmut.

1.31 20.07.1933, Davenport to Hasse

D. criticises H.s review of Mordell's paper. Discussion with Tsen.

Göttingen. Thursday. 20.7.33.

My dear Helmut,

Many thanks for the proof of your report on Mordell's paper, which I return herewith. I cannot find any mistake in it. But I am afraid, I must criticise it severely, on the ground that *nothing whatever* is said about Mordell's method. I do not blame you in the least for devoting half the report to the connection with F.K. Schmidt's Zetafunctions. But I do think something should have been said about the method of attack on these congruences by transforming to exponential sums, averaging over the coefficients, and collecting equivalent polynomials. However familiar these ideas are to us now they were great discoveries on Mordell's part, and remain the *only* method of attacking many of the problems to date. □□□ On the other hand the F.K. Schmidt theory has not *in itself* provided anything at all in the way of *proof*. Your own proof of $\rho = \frac{1}{2}$ in the case $g = 1$ hardly uses the F.K.S. theory at all. I do not wish to belittle the F.K.S. Theory, no doubt it is of greater *permanent* value than Mordell's work, but □□□ the latter has a definite + considerable value at present.

Excuse my criticism, but I felt it my duty to give you my actual impression! Also, remember that the aim of the Zentralblatt is to save the reader reading the original paper!

One thing I have discovered in the last day or two (of no *value*, but of some little interest to me) is that the exponential sums behave very simply on passage to Galois field. Take for instance the cubic exponential sum:

$$\begin{aligned} s &= \sum_x e(ax^3 + bx^2 + cx) \\ s^{(f)} &= \sum_{\xi \text{ in } GF(p^f)} e\left(\mathrm{Sp}(a\xi^3 + b\xi^2 + c\xi)\right) \end{aligned}$$

Then $s^{(f)}$ is a simple polynomial in s, and in fact

$$s^{(f)} = \lambda^f + \mu^f$$

where

$$\lambda + \mu = s, \quad \lambda\mu = p.$$

(You see the very close analogy with the Artin Zetafunction.) Exactly the same is true for the Kloosterman sum. My method is the same as that by which I proved the result for the Gaussian sums, and applies to any expression

$$s = \sum_x \chi(x) e\left(a_1 x^{\ell_1} + \cdots + a_n x^{\ell_n}\right)$$

but the result is then a little more complicated, + I have not yet worked it out fully.

This afternoon in the course of conversation with Tsen, I told him you were interested in his work on Funktionenkörper, and I thought you and Chevalley had a new proof of his result. Is this the case? I hope I was not indiscreet. Incidentally he would like to have any separata you can spare him.

What I told you on Monday about the Reichsmark was perfectly correct. My last remittance gave me 17.75 M. for £1[1] instead of 14. This makes Germany considerably cheaper for me.

Yesterday I bought + read "Hot Water", the latest Wodehouse.

Much love, Yours

Harold.

[1] undeutlich

1.32 22.07.1933, Davenport to Hasse

Manuscript on abundant numbers for Sitzungsberichte der Preussischen Akademie Wiss. Mordell has obtained a 2-year fellowship for Baer. Exponential- und Kloosterman sums.

Gottingen, Sat. 22.7.33

My dear Helmut,

There are so many little things to put in this letter that I think I shall have to divide it into paragraphs.

1) I enclose the MS on abundant numbers, and I should be much indebted to you if you would forward it to Bieberbach for publication in the Sitzungsber. Preuss. Akad. Wiss. There is just one thing I should like you to do first, and that is to add a sentence (forming a new para.) on page 3 to say that I wish to express my thanks (*hearty* thanks, we had better make it) to Herrn Behrend for many helpful corrections and suggestions. [As a matter of fact I am still more indebted to you and Heilbronn in that direction, but I think I ought to mention B. in some way, as he had also proved half of the results independently, but later than and not so simply as me.]

2) Suppose $p_1, \ldots, p_n$ are different primes, and ℓ an integer > 1. Let $f(x_1, \ldots, x_n)$ be a polynomial with rational integral coefficients, not of the form $g(x_1^\ell, \ldots, x_n^\ell)$. Then I conjecture that

$$f(p_1^{\frac{1}{\ell}}, \ldots, p_n^{\frac{1}{\ell}}) \neq 0.$$

How can one prove this? (is it true?) Is it true that the Kummer fields $K(\sqrt[\ell]{\mu})$ with different values of μ[1] but the same ℓ are independent? For my purposes it would suffice to have the result for ℓ a prime too. I expect the question seems ridiculously trivial to you, but I should be grateful for your help.

3) I have had a p.c. from Mordell (in Scotland), he has succeeded in getting Baer a two-year fellowship.

4) (everybody seems to be going to bed in the house, so I had better stop typing.) My proof that for a polynomial f of degree 6 the exponential

[1] μ is not divisible by any ℓ'th power, μ rational and integral.

sum is $O\left(p^{\frac{13}{16}}\right)$ is balderdash. I am very sorry: the result was pretty and non-trivial.

5) With the exponential sums; I am afraid I have made no progress. If $f(x)$ is a polynomial of degree n with coefficients in $GF\,p$, and

$$S = S^{(1)} = \sum_x e\bigl(f(x)\bigr) \qquad S^{(r)} = \sum_{\substack{\xi \\ \text{in } GF\ p^r}} e\,\mathrm{Sp}\,\bigl(f(\xi)\bigr),$$

there exist $n-1$ numbers $\lambda_1, \ldots, \lambda_{n-1}$ such that for all r

$$(-1)^{r-1} S^{(r)} = \lambda_1^r + \ldots + \lambda_{n-1}^r.$$

If $n \leqq 5$, the λ's are expressible in terms of S (and other trivial things), for $n \geqq 6$ this does not seem to be the case. For example, if $f(x)$ is a quartic, o.B.d.A.

$$f(x) = ax + bx^2 + cx^4,$$

then

$$(-1)^{r-1} S^{(r)} = \lambda^r + \mu^r + \nu^r$$

where

$$\begin{aligned} \lambda + \mu + \nu &= S \\ \lambda\mu + \mu\nu + \nu\lambda &= \chi_2(2c) \cdot \tau(\chi_2) \cdot e\left(-\frac{b^2}{2c}\right) \cdot \overline{S} \\ \lambda\mu\nu &= \chi_2(2c) \cdot e\left(-\frac{b^2}{2c}\right) \cdot p \cdot \tau(\chi_2) \end{aligned}$$

As I said, my method applies generally to $\sum_x \chi(x) e\,\mathrm{Sp}\,\bigl(f(x)\bigr)$[2], and I have worked out a number of cases, but there seems to be no order about the results. However in every case there is a finite number of λ's with the above property. You must bring your algebraical mind to bear upon the matter.

I can show that for the Kloosterman sums to be $\leqq 2\sqrt{p}$ it is necessary and sufficient that the number of solutions of

$$\mathrm{Sp}\,\xi = \mathrm{Sp}\,\eta, \quad \mathrm{Sp}\,\xi^{-1} = \mathrm{Sp}\,\eta^{-1}, \quad \xi \text{ and } \eta \text{ in } GF\ p^r$$

[2] $f(x)$ now any sum of powers, not necess. a polynomial

should be

$$p^{r-2} + O\left(p^{\frac{r}{2}}\right) \quad \text{as } r \to \infty.$$

This is hardly very exciting. The analogue for the cubic exponential sum can be transformed into this form:

$$\sum_{\substack{\xi \\ \xi \text{ in } GF\ p^r \\ \mathrm{Sp}\,\xi=0}} \chi_2(\xi \cdot \mathrm{Sp}\,\xi^3) = O\left(p^{\frac{r}{2}}\right) \quad \text{as } r \to \infty.$$

6) The general plane algebraic curve of genus 2 is birationally equivalent to a curve of the form $y^2 = f_6(x)$. So I suppose it may easily be proved that the roots of the F.K.S. Zetafunction arising from any Funktionenkörper of genus 2 have real part $\leqq \frac{7}{8}$ (by Mordell's result.)

7) Now I cannot think of anything else. I hope you are not doing too much work (which is hypocritical of me, since I am giving you more work by this letter).

Very best wishes,

Yours, Harold.

1.33 23.07.1933, Hasse to Davenport

H. explains to D. his ideas which governed his review of Mordell's paper. D.'s discovery is most *remarkable. Discussion of Artin-Schreier extensions and their characters.*

MATHEMATISCHES
SEMINAR
DER UNIVERSITÄT

MARBURG–LAHN,
DEN 23. 7. 33

My dear Harold,

Many thanks for your letter. Excuse me for not writing earlier. I had a lot of trouble with several questions in connection with my lecture on quadratic forms.

I quite agree with you that I ought to have mentioned something about Mordell's method instead of laying the main stress upon my own point of view. I most certainly appreciate the high value and the ingenuity of his line of attack and I do not in the least shut my eyes to the fact that his argument is at present the only one leading to definite results with the overwhelming lot of all these problems. On the other hand, the difference between us is that I do not consider the asymptotic questions as the original problem, particularly not when p is considered variable, perhaps a little more when f in $q = p^f$ is variable for fixed p. From my present point of view the analogue to Riemann's hypothesis lies in the middle of interest, and the asymptotic, or rather non–asymptotic, behaviour of certain numbers of congruence solutions is the rational expression for this problem. From this point of view the question whether the F.K. Schmidt function contributes by itself to the solution of the congruence questions or not is quite unimportant. The line of idea is:

$$\begin{array}{lll} \text{F.K. Schmidt's function} & \longrightarrow & \text{its zeros and their Riemann hypothesis} \\ & \longrightarrow & \text{connection with character sums} \\ & & \text{or numbers of solutions} \\ & \longrightarrow & \text{sizing up of them by Mordells method} \\ & & \text{(or, if possible, exact determination by} \\ & & \text{uniformization or arithmetical argument)} \end{array}$$

I will not say that, by putting F.K. Schmidt's function at the beginning, I confess myself as a decided analyst. On the contrary: F.K. Schmidt's function again is only a formal expression for the arithmetic and algebraic properties of the field K of algebraic functions, and it is the study of this field, which I consider as the original problem. In particular, the number of solutions, slightly filled up by the "infinite solutions", appears from here as the number of prime divisors of degree one, i. e., the analogue to the well–known densities in the common algebraic number theory. The analogy of the algebra and arithmetic in a congruence field K to the common algebraic number theory is perhaps the deepest reason for my own interest in all those questions as well as for their permanent significance altogether.

That is exactly the line of ideas which I am going to follow in my great paper on fields K of genus 1. Although it may not be very [?] towards Mordell and you who started those questions from the more analytic and elementary point of view*), you must allow me the right of putting my discoveries my own way, even on the danger of deteriorizing the whole thing in your eyes by laying your plain questions on a level on which hardly anything of their simple elementary arithmetic apparel is discernible. I think, however, to serve you by this in the long run. For, while it is certain that Mordell's and your publications will find due interest with mathematicians of your own tendency, they must certainly run the risk of being overlooked or even regarded as uninteresting special casual inconnected calculations, which have no bearing to the present systematical development in modern algebra and arithmetic, by a great school of mathematicians that undoubtably forms an integrating and most active part of contemporary mathematics altogether.

That is the reason why I dared bringing my own point of view even in a review on Mordell's paper. It seemed to me far more important to review for

*) "analytic" in the sense of : "How great is a thing ?", "elementary" in the sense of referring to rational integers.

those who are liable to overlook the golden core in his paper by considering it as one of the legion papers of only casual interest with which unfortunately contemporary periodicals teem, than for those who, as you, already know the essence of it. I do not agree with you that a Zentralblatt review ought to save the reader reading the original paper, at any rate not in general. It ought to show the interested reader that there is something which deserves his particular interest. It should give therefore the ideas (in words) more than the details (in formulae), and of course no proofs at all. It should show where a result belongs in the system of knowledge. And it should be written with the intention to interest as far a circle of mathematicians as possible for the thing, provided that the thing deserves interest altogether.

I agree with you, however, that in this particular case I ought to have mentioned something about Mordell's methods; for, as it is, my review contains certainly too much Hasse and too little Mordell, and the method itself deserves a great interest. It was only, because I had already written a too large paragraph, that I made an end after giving Mordell's results. Looking at the printed text, realizing that it did not seem so long as I had feared, and having your criticism, I decided on making an addition at the end. I inserted a sentence giving the leading ideas of his method, as you pointed them out in your letter, rather short though, without formulae. I hope you will be soothed now, though I realize that in your very heart you think I ought to have laid the main stress on the asymptotic questions themselves instead of swerving off at once to Riemann's hypothesis.

While writing this large apology, I am handed your big letter. I will try to answer a few of its points.

First of all, your discovery on the exponential sums are *most remarkable*. I believe I see now "where they belong in the system of knowledge", although I cannot prove it yet.

I suppose that $y^p - y = f_3(x)$ has genus $(p-1)$. The ζ–function for the corresponding cyclic field is the product of $p-1$ L–series and the trivial rational ζ–function. Let $X, \dots, X^{p-1}$ the corresponding characters. I do not know their explicit expressions yet. But I have much reason for supposing that

$$X(\mathfrak{p}) \;=\; e^{\frac{2\pi i f_3(a)}{p}}$$

for the prime divisor $\mathfrak{p}$ of degree 1 corresponding to the prime function $x - a$. If this is the case, the exponential sum for $f_3(x)$ is essentially the next but highest coefficient in the polynomial for the L–series $L(s, X)$. This L–

series has 2 non–equivalent zeros, in accordance with the product $\zeta_K(s) = \zeta_k(s)\prod_{r=1}^{p-1} L(s, X^r)$ having $2(p-1)$ zeros.

I will try to prove all this very soon. I see now a way how to determine the genus. That is the main task. Once one knows it as $p-1$, one knows that $L(s,X)$ has two zeros only, and your result on the behaviour for Galois Fields shows that they are your λ and μ. Nothing, of course, follows from this argument for the magnitude of them. For this question, one will have to investigate the field of $y^p - y = f_3(x)$ by uniformization or any other method. Similar results seem to hold for general $f(x)$. I am looking forward to discussing all this with you next week–end.

By the way, Saturday next I am going to Frankfurt for a lecture of Siegel on "Klassenzahlen binärer quadratischer Formen" (11.a.m.). Perhaps you would also like to hear it.

Your argument about $y^2 = f_6(x)$ is perfectly right. One has only to carry out the generalisation to Galois fields which seems a trifle.

I will add the thanks to Behrend in your Ms. and forward it to Bieberbach.

Now to your question about the independency of Kummer fields. For any $\ell > 1$ the fields $k(\sqrt[\ell]{\mu_1}), \ldots, k(\sqrt[\ell]{\mu_r})$, over the ℓ^{th} cyclotomic field k, are independent, when from a relation $\mu_1^{x_1}\cdots\mu_r^{x_r} = \alpha^\ell$ (α in k) follows $\mu_1^{x_1} = \alpha_1^\ell, \ldots, \mu_r^{x_r} = \alpha_r^\ell$. Suppose now too, the equations $x^\ell = \mu_1, \ldots, x^\ell = \mu_r$ are irreducible in k, i. e., none of the $\mu's$ is a power of any exponent $\ell' \mid \ell$, $\ell' > 1$ with basis in k. Then a relation $\varphi(\sqrt[\ell]{\mu_1}) = 0$, where $\varphi(x)$ polynomial in $k(\sqrt[\ell]{\mu_2}, \ldots, \sqrt[\ell]{\mu_r})$, implies $x^\ell - \mu_1 \mid \varphi(x)$, because then $x^\ell - \mu_1$ is irreducible also in $k(\sqrt[\ell]{\mu_2}, \ldots, \sqrt[\ell]{\mu_r})$.

For $\mu_1 = p_1, \ldots, \mu_r = p_r$ (different rational primes, none of them $= \ell$) all suppositions are true, hence the above statement. You only ask about the case where $\varphi(x)$ has coefficients in $\underline{R}(\sqrt[\ell]{p_2}, \ldots, \sqrt[\ell]{p_r})$. That is a special case. It follows $x^\ell - p_1 \mid \varphi(x)$.

Now suppose $f(\sqrt[\ell]{p_1}, \ldots, \sqrt[\ell]{p_r}) = 0$ with rational coefficients and, w.l.o.g., f of degree $< \ell$ in each argument. If

$$f = \varphi_0 + x_1\varphi_1 + \cdots + x_1^{\ell-1}\varphi_{\ell-1},$$

it follows that

$$\varphi_i(\sqrt[\ell]{p_2}, \ldots, \sqrt[\ell]{p_r}) = 0 \quad (i = 0, \ldots, \ell-1).$$

By replying the same argument, one finds that $f = 0$ (as a polynomial). If the degrees of f are not reduced, one has non trivial relations, for $x^\ell - p_1 \mid \varphi(x)$

does *not* imply $\varphi(x) = \varphi_0(x^\ell)$. For example,

$$\sqrt[\ell]{p}^{\ell+1} - p\sqrt[\ell]{p} = 0\,, \quad \text{corresponding to } x^\ell - p \mid x^{\ell+1} - px\,.$$

I also had a post–card from Mordell, indicating that Baer has been given a fellowship. I am very glad for him.

I am very glad that you have succeeded in getting a better rate of exchange. Your German term must have cost you a lot on the official rate. Lucky fellow having been able to do it in spite of that !

Very best wishes,

Yours,
Helmut.

1.34 24.07.1933, Hasse to Davenport

H. has computed the genus for $y^p - y = f_3(x)$. He can give explicitly the characters for any $y^p - y = f(x)$ (polynomial). Corresponding L-series, is a polynomial in p^{-s}. For $n = 3$ it has degree 2; roots are Davenports λ, μ. Exponential sum is fully analogous to character sum. H. hopes to determine the genus also for rational functions. – H. has posted D.'s Ms. to Bieberbach. – A. Weil had come over from Frankfurt for a day. It is not clear whether Siegel's lecture will take place at all. If so, H. will go there by arrangement with his seminar "in the usual way".

MARBURG–LAHN, DEN
24.7.33

My dear Harold,

I have succeeded in proving my presumption on the exponentials. For $y^p - y = f_3(x)$ the genus is really $p-1$. Further I can explicitly give the characters for any $y^p - y = f(x)$ (polynomial). Let $P(x)$ be a prime function in x of degree r. Then the residue class

$$S_P(f(x)) \equiv f(x) + f(x)^p + \cdots + f(x)^{p^{r-1}} \bmod. P$$

is representable by a definite element of E_P. Let $S_P(f(x))$ denote this element. I define then

$$\left\{\frac{f(x)}{P(x)}\right\} = e^{\frac{2\pi i}{p} S_P(f(x))}$$

I can prove: When $f(x)$ has degree n, $\left\{\frac{f(x)}{P(x)}\right\}$ depends on the first n coefficients of $P(x) = x^r + p_1 x^{r-1} + \cdots + p_n x^{r-n} + \cdots$ only; and if $P(x)$ has the same first n coefficients as $P_1(x)\, P_2(x)$, then $\left\{\frac{f(x)}{P_1(x)}\right\}\left\{\frac{f(x)}{P_2(x)}\right\} = \left\{\frac{f(x)}{P(x)}\right\}$. Further $P(x)$ splits in the field $K = E_p(x, y)$ if and only if $\left\{\frac{f(x)}{P(x)}\right\} = 1$.

Consequently $\left\{\frac{f(x)}{P(x)}\right\}$ is the exact analogue to Artin's symbol $\left(\frac{f(x)}{P(x)}\right)$ for $y^2 = f(x)$. One can define $\left\{\frac{f(x)}{A(x)}\right\}$ by decomposing $A(x)$ into prime functions. Then this symbol is a generating character $\chi(A(x))$ for the field K. The corresponding L–series is

$$L(s,\chi) \;=\; \sum_A \left\{\frac{f(x)}{A(x)}\right\} \frac{1}{|A(x)|^s}$$

The product over the $p-1$ L–series, corresponding to χ, χ^2, $\cdots$, χ^{p-1} is $\frac{\zeta_K(s)}{\zeta(s)}$. $L(s,\chi)$ is a polynomial in $\frac{1}{p^s}$. For $n=3$ it has degree 2 . Its roots are your λ, μ .

The exponential sum is

$$\sum_{|P|=p} \left\{\frac{f(x)}{P(x)}\right\} \;=\; \sum_{|P|=p} e^{\frac{2\pi i}{p}\, S_P(f(x))} \;=\; \sum_a e^{\frac{2\pi i}{p}\, f(a)} \,,$$

fully analogous to Artin's symbol and the character sums.

I have reason to suppose that my method for determining the genus applies to all cases where $f(x)$ is a rational function whose "degree" does not reach to p . This would give the number of non–equivalent zeros of the corresponding L–functions, and so determine the "order of difficulty" for the different exponential sums.

I have looked over your Ms. again, corrected some trifles and posted it to Bieberbach with a few lines. I hope it will be willingly accepted.

I learn from Clärle that partaking on Siegel's lecture would mean interfering with already fixed plans between you. I have further learned from André Weil, who came over from Frankfurt for a day, that it is not yet certain whether Siegel's lecture will take place at all. I should on no account like your taking my suggestion as a hint for driving me there or for your partaking at all. We are going together by an arrangement from our Seminary in the usual way, if Siegel's lecture takes place at all. I did not mean anything else than informing you that there was a lecture of Siegel.

Much love,

Yours,

Helmut

1.35 25.07.1933, Hasse to Davenport

Artin-Schreier extensions of rational function fields, its genus and its L-functions. Kloosterman sums.

25.7.33

My dear Harold,

I have got much more general results on

$$y^p - y = C$$

than I first thought.

As a matter of fact, I have determined the genus, and with it the number of zeros of the corresponding L–function for *every* C (integral or fractional).

If $C = C_1 + \cdots + C_r$ is the decomposition of C into "Partialbrüche", and if all terms out of these C_i which are pure p^{th} powers are removed by an easy transformation of y, and if $n_1, \ldots, n_r$ are the degrees of those $C_1, \ldots, C_r$, then

$$g = \frac{(p-1)(n+r-2)}{2} \quad (n = n_1 + \cdots + n_r),$$

and therefore the L–functions have $n + r - 2$ zeros.

In particular the Klosterman sums belong to L–series with $2 + 2 - 2 = 2$ zeros. Same for the cubic exponential sums, as you discovered. I think I can also determine the last term of the L–polynomials, that is the analogue to your $\lambda\mu = p$ in the cubic case. It follows from the Riemann–Roch theorem for congruence classes.

Of course, your other question about the characters for the class division with fixed first $n + 1$ digits is also answered by my results. Take all polynomials of degree n and all their characters

$$\left(\frac{C}{P}\right) = e(S_P(C)),$$

or rather the composite characters $\left(\frac{C}{A}\right)$ arizing this way. Then the class division in question arizes. It belongs to the composite of all those fields

$y^p - y = C$ as class field. This is the "analogue" to the usual cyclotomic field.

I hope to tell you more when you come next week. Particularly I hope to find some *results* in your sense until then.

Very best wishes,

Yours,
Helmut.

1.36 06.10.1933, Hasse to Davenport

Automorphism group of elliptic function fields over finite fields and over their algebraic closure. Automorphisms "given by the addition theorem." F. K. Schmidt's class field theory comes in. H. hopes the proofs will allow a purely algebraic treatment. – "I have got the knack of it now, in particular of where complex multiplication and imaginary quadratic fields come in."

6.10.33

My dear Harold,

I am awfully sorry your father's health is so bad. It must be terrible for all of you seeing him suffer so seriously. It is really a hard lot, quite undeserved for all his goodness. My very best wishes to all of you, in particular himself.

I have been rather busy with the elliptic case and made some progress in the direction indicated in my last letter.

I have proved that the automorphism group given by the addition theorem is of "dimension 2" . For the "infinite finite field" (composite of all finite fields to p) it is isomorphic to the additive group of all pairs (r_1, r_2) of rational numbers, with denominator prime to p , considered mod. 1 . This is entirely in accordance with what I proved analytically. That automorphism group is, on the other hand, isomorphic to the group of all algebraic solutions with composition according to the addition theorem. I am now going to proceed to the study of a "properly finite" field of coefficients. Here F.K. Schmidt's theory of the general class fields comes in, for $K = E(x, y)$, is unramified abelian, i. e. class field, over $K_n = E(x_n, y_n)$, where x_n, y_n arise by multiplication with n in the sense of the addition theorem. The properties of the generating equation for K over K_n are well–known from the ordinary Teilungstheorie. They are expressable in purely algebraic terms, and I hope the proofs will also allow a purely algebraic treatment. I am not far enough to tell you more at present. I have, however, got the knack of it now, in particular of where complex multiplication and imaginary quadratic fields come in.

Kindest regards and best wishes,

Yours,
Helmut.

1.37 15.10.1933, Davenport to Hasse

Question about algebraic treatment of elliptic case. List of Refugees from Germany. Could Hasse help giving information about their status? In particular Richard Brauer. – Cambridge is very peaceful, after Germany. D. enjoyed the months he spent in Marburg.

TRINITY COLLEGE,
CAMBRIDGE.
Sunday 15.10.33

My dear Helmut,

Very many thanks for your note. It will be splendid if you obtain a purely algebraic treatment of the elliptic case. What is the starting–point for your proof that the automorphism group given by the addition th$^{\underline{m}}$ is of "dimension 2" ?

I seem to have *over*stated the seriousness of my father's illness in my first letters to you. I did not anticipate his becoming seriously worse in the near future. But I am arranging for the specialist (who is indirectly in charge of his case, and who has not seen him for some time) to make an examination of him.

I am afraid I have not settled down to work yet. I have made some desultory attempts to attack the Kloosterman sums analytically, but without success. I do not expect to get anything except some approx. functional equation. In the next few days I will get our paper on Gaussian sums into shape.

German refugees are quite in evidence at Cambridge. Max Born has received a regular lectureship, and Courant will probably be coming here too. T. Rado is here, with a fellowship of some kind for two years. He is about 27, and married. I find him a very pleasent fellow. From his appearance I should not have thought there was a drop of Jewish blood in him. He looks like a typical German — which I suppose from his name he really cannot be. Bernard Neumann, one of the Berlin people is here, also Kaufmann of Heidelberg. S. Bochner was here, but is not now.

Hardy has received a list of German mathematicians in difficulties from the Academic Assistance Council, the list is drawn up uncritically by non mathematicians, and our task is to sift it. We should very much welcome any information you could give us about the present circumstances of the following: —

Max Dehn

Blumenthal ?

E. Fischer (Köln) ?

Hartogs (München) ?

Hausdorff

Hellinger

E. Jacobsthal (Techn. Hochsch. Berlin) ?

v. Mises ?

Schur (I) (Berlin)

Toeplitz.

These are all wellknown people, on the elderly side. I have put Schur on the list because Rado, who is in touch with Berlin, had not heard of his being reinstated, so I thought I would ask whether you were sure. Similarly Toeplitz. What we particularly want to know[n] of the above is: which are "beurlaubt", which are "ausgeschmissen", of these which will get pensions, and whether there are any cases of serious hardship in the above list.

We should also like to hear about *Neugebauer* (to what extent his being prevented from lecturing will affect his financial position, and what his future prospects are), *Hamburger* (Köln) and *Richard Brauer*. I seem to remember you saying there was hardship in the last case. Perhaps you would tell us all you can about R. Brauer, his circumstances, whether married and with children, whether he has any other resources etc. Perhaps you might write a statement of the quality of his work, which Hardy could then use, there being probably none in England competent to assess R. Brauer's work.

Apart from R. Brauer and the people in the above list (about whose circumstances we are not sufficiently informed) the most deserving case appears to be that of Remak. If you know of any other cases you might mention them, as I am not sure that our list is complete. Excuse my giving you all this trouble.

Cambridge is very peaceful — so is England as a whole, after Germany. My rooms are a little grimy, but I cannot afford to have them decorated — anyhow they would soon get dirty again.

I need hardly assure you how very much I enjoyed the months I spent in Marburg. My only regret is that I have not been able to bring you back to England with me. Last Tuesday was the Fellows Admission Dinner; I cannot say that I enjoyed it as much as the one a year ago.

There are four new fellows, one, Chandrasekhar, an Indian, is an astro–physicist, pupil of Milne + Eddington. He is the first Indian fellow of Trinity apart from Ramanujan. The other three new fellows consist of two botanists + a zoologist, which is a very unusual distribution.

Kindest regards, + very best wishes,

Harold.

Kind regards to Gertrud.

1.38 15.10.1933, Hasse to Davenport

A letter which D. had sent from Marburg has been returned because of wrong address. H. had not heard from D. for a long time. H. has carried on his investigations on elliptic function fields mod p. All turns out very nicely. H. has not got through to the end. Wants a solid foundation before carrying on to the determination of the class number. – Question about an elementary proof for the existence of a Dirichlet character for which given integers have order divisible by a given k and $\chi(-1) = -1$: vdW. now has a simple proof. Is based on Chevalley's proof for $r = 1$. H. gives details.

MATHEMATISCHES
SEMINAR
DER UNIVERSITÄT

MARBURG–LAHN,
DEN 15. 10. 33

My dear Harold,

To–day, the enclosed letter was brought to me from the post–man. Owing to the fact that you made a blunder with one highly important letter in the address, it travelled to a wrong district of London, then back to Marburg, then to the Oberpostdirektion at Kassel (since you forgot to put the sender on back of it), was opened there by an official, and the sender found to be "Harold" bei Prof. Hasse, Weissenburgstr. 22, Marburg–L. , and eventually handed to me, all of which took 4 weeks precisely ! What a shame !

I have not heard from you for a long time. I was glad, though, to hear from your mother that your father is better now. I hope he will soon recover entirely.

I have carried on my investigations on elliptic functions mod. p . All turns out very nicely. I have not got through to the end, however, because I wanted a solid foundation first before carrying on to the determination of the class number.

Perhaps you remember my question about an elementary proof for the existence of a Dirichlet character χ for which given integers $a_1, \dots, a_r > 1$ have order divisible by a given k and $\chi(-1) = -1$. It may interest you that v.d.Waerden found a very simple elementary proof. It bases upon Chevalley's proof for the case $r = 1$: Let k (w.l.o.g.) be a prime power $\ell^{\nu+1}$ ($\nu \geq 0$; for $\ell = 2$ even $\nu \geq 1$). Then

$$Q = \frac{a^{\ell^{\nu+1}} - 1}{a^{\ell^\nu} - 1} = \frac{(b+1)^\ell - 1}{b} =$$

$$= b^{\ell-1} + \binom{\ell}{1} b^{\ell-2} + \cdots + \binom{\ell}{\ell-2} b + \binom{\ell}{\ell-1}, \quad (b = a^{\ell^\nu} - 1)$$

has the properties: every prime $p \neq \ell$ dividing Q does not divide b; the prime ℓ may divide Q but ℓ^2 does not divide Q. Since $Q > \ell$, there exists at least one $p \neq \ell$ dividing Q. For this p,

$$p \mid a^{\ell^{\nu+1}} - 1, \quad p \nmid a^{\ell^\nu} - 1.$$

Hence, when ℓ^μ is the highest power of ℓ dividing $p-1$, any Dirichlet character χ mod. p of order ℓ^μ has the properties

$$\chi(a)^{\ell^\nu} \neq 1, \quad \chi(a)^{\ell^{\nu+1}} = 1.$$

Let now $a_1, \dots, a_r > 1$ be given integers and ℓ^ν as before. For every $\omega > 0$ there exists a set of primes $p_1, \dots, p_r$ such that

$$\chi_\rho(a_\rho)^{\ell^{\nu+\omega}} \neq 1, \quad \chi_\rho(a_\rho)^{\ell^{\nu+\omega+1}} = 1, \tag{1}$$

where χ_ρ is a Dirichlet character mod. p_ρ of order ℓ^{μ_ρ}, the highest power of ℓ dividing $p_\rho - 1$. By chosing ω sufficiently high one can exclude that any of the p_ρ divides $a_1 \cdots a_{\rho-1} a_{\rho+1} \cdots a_r$. We put now

$$\chi(x) = \prod_\rho \chi_\rho(x)^{c_\rho}$$

with certain exponents c_ρ mod. ℓ^{μ_ρ}, and try to fix the c_ρ in such a way that for all $\delta = 1, \dots, r$ at any rate

$$\chi(a_\delta)^{\ell^{\nu+1}} \neq 1 \quad \text{(whereas also } \chi(a_\delta)^{\ell^{\nu+2}} \neq 1 \text{ is allowed)}. \tag{2}$$

For given $c_2, \dots, c_r$, $(2)_{\delta=1}$ is *not* satisfied for the solutions c_1 of

$$\chi(a_1)^{\ell^{\nu+1}} = 1, \quad \text{i. e.,} \quad \chi_1(a_1)^{c_1 \ell^{\nu+1}} = \prod_{\rho \neq 1} \chi_\rho(a_1)^{-c_\rho \ell^{\nu+1}}.$$

Since $\chi_1(a_1)$ is a primitive $\ell^{\nu+\omega+1}$–th root of unity (by (1)), $\chi_1(a_1)^{\ell^{\nu+1}}$ is a primitive ℓ^ω–th root of unity. If the right–hand side is also a ℓ^ω–th root of unity, there is precisely one solution c_1 mod. ℓ^ω, i. e., precisely $\ell^{\mu_1-\omega}$ solutions c_1 mod. ℓ^{μ_1}. If not, there is no solution c_1 for the given $c_2, \dots, c_r$. Hence there are at most

$$\ell^{\mu_1-\omega}\ell^{\mu_2} \dots \ell^{\mu_r} = \ell^{-\omega} \cdot \ell^{\sum_\rho \mu_\rho}$$

sets c_ρ mod. ℓ^{μ_ρ} for which $(2)_{\delta=1}$ is *not* satisfied, and therefore at most

$$\frac{r}{\ell^\omega} \cdot \ell^{\sum_\rho \mu_\rho}$$

sets c_ρ mod. ℓ^{μ_ρ} for which at least one of the r conditions (2) is *not* satisfied. When ω is now chosen also so large that $\ell^\omega > r$, there is at least one set c_ρ mod ℓ^{μ_ρ} for which all the r conditions (2) are satisfied.

The character χ formed with these c_ρ has the property that every a_ρ is at least of order $\ell^{\nu+2}$ for it. For $\ell = 2$ one can reach in addition that $\chi(-1) = -1$. For if $\chi(-1) = +1$, the character $\chi^*(x) = \chi(x)(\frac{-1}{p_{r+1}})$, where p_{r+1} is a divisor of $4a_1 \cdots a_r - 1$ with $p_{r+1} \equiv -1$ mod. 4, satisfies the same conditions and $\chi^*(-1) = -1$.

There is now no difficulty in forming a character χ for which given integers $a_1, \dots, a_r > 1$ have orders divisible by a given composite k and $\chi(-1) = -1$.

Though I know you are not particularly keen on "elementary" proofs, I think this proof is a jolly good piece for itself, and certainly preferable to a proof which deals with Dirichlet series and even to an extent that surpasses the circuit of the classic theorem on arithmetic progressions.

I hope to hear from you very soon.

Much love from

Helmut.

P.S. Many thanks for "Contemporaries and Makers". What is the point of it. Is it meant to be funny ??

1.39 20.10.1933, Hasse to Davenport

About several mathematicians in Germany who had been dismissed: Rado, Courant, Frank, W. Roepke, Schur, Toeplitz, Hausdorff, Hellinger, Dehn, Fischer, Hartogs, v.Mises, Jacobsthal, Blumenthal, Hamburger, Neugebauer, Landau, Remak, Herzberger, Jaeger. – H. proved the "2-dimensionality" of the group of points of finite order of an elliptic curve. (There seems to be an error as regards points of p-power order; H. claims there are none.) – Baer and Mahler will translate H.s Klassenkörper-Ausarbeitung under Mordell's supervision. – H. is looking forward to D.'s draft of a common paper on Gaussian sums.

Marburg, 20.10.33

My dear Harold,

Thanks for your kind letter. I was very much interested in your report about what I may call Cambridge Concentration Camp for German Refugees (C.C.C.G.R.). I do not know T. Rado personally. I think he is Hungarian, anyhow his name suggests this. I heard from Hensel that Courant together with Frank[†] (the physicist) will be going to Angora[‡]. There they may seem to gather quite a few German scholars, for instance W. Roepke (the Marburg national economist) who got beurlaubt last spring.

About your list: *I. Schur* is really reinstated. I had a letter from him this morning telling me that he received the official acknowledgement of his reinstalment (or reinstation). *Toeplitz* was never beurlaubt, neither *Hausdorff*, *Hellinger*, *Dehn*. There may hang a sword of Damocles over each of them though. I am not sure about *E. Fischer*, *Hartogs*, *v. Mises*, *Jacobsthal*. *Blumenthal* was and is still beurlaubt so far as I know. As long as a man is only beurlaubt he gets his full salary. Shall I collect information about the last five by writing to friends ? (Also about *Hamburger* where I do not know anything) ?

Neugebauer and *Landau* are not beurlaubt but prevented from lecturing for the time being. This does not affect their financial position. Neuge-

[†]gemeint ist wohl James Franck

[‡]Ankara(?)

bauer has a fairly good income from the Zentralblatt, too. I have written to *R. Brauer* and asked him to give me all possible information about himself. I have also written to I. Schur for information about *Remak*. There is further *M. Herzberger*, who wrote some papers on algebra and a lot on geometrical optics. He had a position with Carl Zeiss, Jena, which he lost so far as I know. I am very sorry Jaeger did not get the fellowship. What will he be doing now ?

I have carried through my proof for the two–dimensionality of the automorphism group given by the addition theorem now. The proof is as follows: I show first that the number of points which become infinite (i. e. zero in the sense of the addition theorem) after n–fold addition (multiplication with n) is n^2 when n is prime to p and n_0^2 when n_0 is the greatest divisor of n prime to p. [1] This follows by studying the numerators and denominators in the explicit multiplication formula for a multiplicator n by induction: Let x_n, y_n arise from x, y by symbolic multiplication with n. Then $x_n - x$ vanishes of first order for every point P_{n-1} whose $n-1^{th}$ iteration is infinite, and also for every point P_{n+1}, and becomes infinite of second order for every point P_n. Once the above statement is proved, the two–dimensionality follows at once from the exponent *two* in the number n^2 or n_0^2.

I had a letter from Mordell asking me permission to publish my Klassenkörperausarbeitung in England. I think I will consent. Baer and Mahler are going to translate it under Mordell's supervision.

I am looking forward to your draft of our paper on Gaussian sums.

Kindest regards to everybody known to me there, in particular to yourself.

Yours,

Helmut

[1] Hasse himself found later that this is not quite correct. In general, there are points of p-power order, except in the "supersingular" cases.

1.40 24.10.1933, Hasse to Davenport

German language letter. H. reports news about German mathematicians who had lost their job: R. Brauer, A. Brauer, v. Mises, Frau Dr. Pollaczek, Remak, Stefan Bergmann, Weyl, Courant, Landau, Levy, Fenchel, Neugebauer, Heilbronn, Heesch, Fritz Noether. – H. has completed his Aufgabensammlung.

MARBURG–LAHN, DEN 24.10.33

Lieber Harold !

Heute der Eile halber einen deutschen Brief ! Ich habe eine ganze Reihe von Neuigkeiten über deutsche "notleidende" Mathematiker gehört.

R. Brauer hat durch Veblen eine Einladung für ein Jahr (1934) als visiting professor nach Lexington (Kentucky) bekommen. Seine Stelle ist ihm gekündigt und die venia entzogen. Er bekommt noch Gehalt bis zum 1.4.34, ist also zunächst sichergestellt. Sein Gehalt war netto etwa 240 Rm monatlich, dazu früher sehr erhebliche Kolleggelder. Außerdem unterstützt ihn seine Mutter mit 100 Rm monatlich, wird das aber wohl nicht mehr lange können.

A. Brauer ist als Kriegsteilnehmer, Inhaber des Verwundetenabzeichens und des Eisernen Kreuzes nicht in Gefahr. Allerdings ist ihm, wie allen nichtarischen Assistenten seine Stelle vorsorglich gekündigt worden (zum 1.1.34), aber gleichzeitig ist Verlängerung beantragt, und die Entscheidung wird erwartet. Man sieht seine Lage als gesichert an. Immerhin ist es unter den heutigen Verhältnissen für Nichtarier grundsätzlich kein Vergnügen, hier auf Gnade eine geduldete Stelle zu haben. Leicht haben es diese Leute nicht. Und ich könnte mir denken, daß auch A. Brauer den Wunsch hat, über kurz oder lang Deutschland den Rücken zu kehren.

v. Mises hat freiwillig um seine Entlassung gebeten. Er geht als Direktor des neuzugründenden Mathematischen Instituts an die Universität Stambul, unter sehr günstigen Bedingungen.

In Berlin ist zudem *Frau Dr. Pollaczek* (bisher v. Mises' Assistentin) und *Remak* die venia entzogen worden. Frau Pollaczek geht auf zwei Jahre nach

Brüssel, als Assistentin des dortigen Prof. van Dungen. Über Remak schreibt Schur nichts weiter. *Stefan Bergmann* (soviel ich weiß noch polnischer oder russischer oder rumänischer Staatsangehöriger) ist von Hadamard gebeten worden, nach Paris zu kommen. Er wird wahrscheinlich eine Stellung an der Universität Calcutta bekommen.

In Göttingen hat *Weyl* sein Amt niedergelegt, er geht wohl nach Amerika. *Courants* Beurlaubung ist vorgestern aufgehoben worden. Courant ist mit seinen Nerven sehr herunter, vor allem wegen der Sorge um die Zukunft seiner Kinder. Er hat sofort weiteren Urlaub beantragt. Übrigens ist er ja wohl überhaupt dort in Cambridge für ein Jahr verpflichtet. Ich glaube er denkt daran, später in die Türkei (Angora oder Stambul) zu gehen, obwohl man davon in Göttingen noch nichts weiß. Ich hörte es nur durch Hensel, der es von Berlin mitbrachte. *Landau* ist zur Zeit in Berlin. Er hat seine Vorlesungen in üblicher Weise angekündigt und gedenkt offenbar zu lesen. Irgendeine Entscheidung der Universitätsbehörden in dieser Hinsicht liegt bisher nicht vor.

Von den jüngeren Göttingern ist *Levy* in Amerika, *Fenchel* in Kopenhagen, *Neugebauer* hat ebenfalls eine sehr gute Forschungsprofessur in Kopenhagen in Aussicht, doch liegen bindende Abmachungen noch nicht vor. *Heilbronn* gibt an, er wolle vorerst Privatassistent von Landau bleiben. Dann ist noch *Heesch* da, der Privatassistent von Weyl war, und der offenbar von Weyl privat bezahlt wurde; dadurch ist er jetzt in schwieriger Lage. Ob er Arier ist, weiß ich nicht, glaube es aber.

Ferner höre ich noch, daß E. Noethers Bruder, *F. Noether,* mit *dreiviertel* seiner gesetzlichen Pension in den Ruhestand versetzt worden ist, weil er den republikanischen Studentenbund unterstützt haben soll. Er will dagegen protestieren, da die Begründung nicht zutreffe, und hofft, seine *volle* Pension durchzusetzen. Er würde nach E. Noethers Angaben sehr gerne auf einige Zeit Gastvorträge im Ausland halten. Die Kürzung seiner Einnahmen trifft ihn als Familienvater sehr.

Das ist alles, was ich heute aus verschiedenen Briefen erfuhr. Vielleicht kannst Du dort doch für diesen oder jenen etwas tun.

Schur schickte mir auch die Revision Deiner Note für die Berl. Sitz. Ber. Er bittet Dich um Rücksendung unmittelbar an ihn (Berlin–Schmargendorf, Ruhlaer Str.14).

Heute habe ich endlich meine Aufgabensammlung abgeschlossen und das Ms. nach Berlin abgesandt. Ich bin sehr froh, das endlich vom Halse zu haben. Nun sind glücklich noch gerade 4 Ferientage, wo ich meinen eigenen Interessen nachgehen kann, und auch das noch nicht einmal, denn inzwischen hat sich wieder so viel anderes angehäuft, daß ich abarbeiten muß. Immerhin hoffe ich in den nächsten Tagen ein bischen weiter zu kommen.

Gelesen habe ich The Black Arrow und bin jetzt bei Three Men in a Boat (to say nothing of the dog). Das macht mir jetzt doch ganz viel Spaß. Über mein Mißverständnis mit den Contemporaries and Makers wirst Du gelacht haben.

Laß bald mal wieder von Dir hören.

Herzlichst Grüße

Dein
Helmut

1.41 28.10.1933, Hasse to Davenport

"Progress comes very slowly indeed." – H. has no copy of his Marburg Lecture Notes left for Rado. H. hopes that English translation will appear soon. – H. looking forward to visit D. in England next spring. H. speaks of his "Nazi colleagues".

28.10.33

My dear Harold,

I wish you many happy returns of that delightful day. [1] I hope you will enjoy your time there even when being continually lazy. I cannot say the same of myself, I am hard at work. Progress comes very slowly indeed. Next week term will prevent me from taking further steps in this matter. I am afraid the thing will not be finished until then.

Unfortunately, I have not got another copy of my Ausarbeitung for Rado. Presumably it will not last so very long until the English translation appears.

I am looking forward to our visit in England next spring which will be greatly favoured by all my Nazi colleagues here. They consider it almost as a national duty that every German who has friends in England should go there and make "Kulturpropaganda" for Germany.

Kindest regards to all people known to me there.

Much love,

from

Helmut.

[1] On 30 October 1933 Davenport had his 26th birthday.

1.42 05.11.1933, Hasse to Davenport

H. would welcome the publication of joined paper, but will not press D. – H. ought to make treatment of elliptic case by means of elliptic functions ready for print. But cannot get himself working at it. H. would prefer giving a purely algebraic proof. H. made some progress during past weeks: Purely algebraic criterion for the roots being simply $\sqrt{-p}$ *:* $A = 0$ *. –* $A = 1$ *iff* h *divisible by* p *. R.H. is* $(h - (p+1))^2 < 4p$ *. This depends on a certain identity for operators. H. has not quite finished the proof. – Defining* π *as an operator which represents complex multiplication in char.* p *.* $\pi + \overline{\pi} = p + 1 - h$ *. – H. thinks this proof of R.H. will please D. better. H. tries to find operators* $\pi \pm 1$ *.*

5.11.33

My dear Harold,

I am awfully sorry for all you wrote to Clärle about your father. It is a great burden for all of you and in particular for yourself. It must be terrible to know such a destiny impending and have no means to avoid it. My heartiest sympathies are with you.

I can quite understand that days like these are not liable to yield much work and many results. Though I should most heartily welcome the publication of a joined paper from both of us after so long a time of our acquaintanceship, I will not press you in the least to finishing the thing now. I can also understand that pursuing new questions is often far more alluring than polishing off old matter.

It is the same with me now. I ought to make my treatment of the elliptic case by means of elliptic functions ready for print. But I cannot get myself to working at it. I rather should like to avoid this publication at all by giving a pure algebraic proof. I have made some definite progress in this direction in the last weeks. First of all, I can give a purely algebraic criterion for the case where the roots are simply $\sqrt{-p}$. This depends on the following: Let $y^2 = 4x^3 - g_2x - g_3$ be the equation over E_p. Put $t = \frac{-2x}{y}$ and develop $y\,\frac{\mathrm{d}t}{\mathrm{d}x}$ formally in a power series:

$$y\,\frac{\mathrm{d}t}{\mathrm{d}x} = 1 + a_1t + a_2t^2 + \cdots$$

It is easy to see that a_1, a_3, $a_5, \ldots = 0$ (and also $a_2 = 0$, but this does not matter). The development begins with

$$y\,\frac{\mathrm{d}t}{\mathrm{d}x} \;=\; 1 + \frac{g_2}{2}t^4 + \frac{3g_3}{4}t^6 + \cdots$$

and the coefficient of $t^{2\nu}$ is a polynomial in g_2, g_3 with rational coefficients whose denominators are powers of 2 only (i. e. which are definite elements in E_p), and which is homogeneous of dimension -2ν, when g_2, g_3 are as usual reckoned of dimensions -4, -6 (x, y of dimensions -2, -3; t of dimension $+1$).

Now let $A = -a_{p-1}$ the coefficient of t^{p-1}. Then the alternative $A = 0$ or $A \neq 0$ decides whether the roots are simply $\sqrt{-p}$ or not.

You may consider this from the following point. t is a "uniformizing variable" for the point $x = \infty$, $y = \infty$. In the common theory of elliptic functions one can find another uniformizing variable u for this point such that $y\,\frac{\mathrm{d}u}{\mathrm{d}x} = 1$, i. e. $\frac{\mathrm{d}x}{\mathrm{d}u} = y$. This is not possible for characteristic p, on account of the well–known denominators in the development of $x = \wp(u)$, $y = \wp'(u)$ in power series in u or of the reciprocal series of u in $t = \frac{-2x}{y} = -\frac{2\wp(u)}{\wp'(u)}$. But one can get an approximation, by taking

$$u \;=\; t + \frac{a_1}{2}t^2 + \frac{a_2}{3}t^3 + \cdots + \frac{a_{p-2}}{p-1}t^{p-1}\,.$$

Then

$$y\,\frac{\mathrm{d}u}{\mathrm{d}x} \;=\; y\,\frac{\mathrm{d}t}{\mathrm{d}x}\bigg/\frac{\mathrm{d}u}{\mathrm{d}t}$$

$$= \;(1 + a_1 t + \cdots + a_{p-2}t^{p-2} + a_{p-1}t^{p-1} + \cdots)/(1 + a_1 t + \cdots + a_{p-2}t^{p-2})$$

$$= \;1 + a_{p-1}t^{p-1} + \cdots \;=\; 1 - At^{p-1} + \cdots \;=\; 1 - Au^{p-1} + \cdots\;,$$

i. e.

$$\frac{1}{y}\,\frac{\mathrm{d}x}{\mathrm{d}u} \;=\; 1 + Au^{p-1} + \cdots$$

It is the occurence of $A = -a_{p-1}$ here that brings in the connection mentioned above. A is an invariant of the field; its being $\neq 0$ puts a stop to carrying the approximation further on.

Another thing which depends on A is the question whether the class number $h = N_1 = N + 1$ is divisible by p. This is the case if and only if $A = 1$ (as before I throughout restrict myself in this letter on the rational case, E_p). Thus, for example, for $p = 5$, where $A = 2g_2$, only for $g_2 = 3$ the class number h is divisible by 5 , i. e. only for $y^2 = 4x^3 - 3x \pm 2$ this is the case. In point of fact, $h = 5$ here.

I have also made considerable progress towards the proof of $(h-(p+1))^2 < 4p$ which is Riemann's hypothesis. This depends on the following relation:

$$p(x, y) + (x^{p^2}, y^{p^2}) \;=\; (p+1-h)(x^p, y^p)\,.$$

Here the brackets denote solutions of $Y^2 = 4X^2 - g_2X - g_3$ and the integer factors mean the iteration of the addition formula [if $p+1-h < 0$, $(p+1-h)(x^p, y^p) = (h-(p+1))(x^p, -y^p)$]. Also the $+$ is to understand in the sense of the addition theorem. I have not quite finished the proof of this identity. But I clearly see its truth. The above inequality follows from it by comparing degrees on both sides. For, the x–term of $p(x, y)$ is a rational function of degree p of x^p ; so is also the x–term of (x^{p^2}, y^{p^2}) . From the addition formula it follows thus that the x–term of the left hand side has degree $4p$ in x^p at most; on the other hand, the x–term of the right hand side is a rational function of degree $(p+1-h)^2$ of x^p .

As to the above relation, it is quite easy to see that it holds for every rational solution (a, b) and also for every solution of (a, b) with rational a and b of degree 2 . In both cases, the left hand side is $(p+1)(a, b)$. For a and b rational, the right hand side is $(p+1)(a, b) - h(a, b)$; but $h(a, b) = 0$, because the rational (a, b) form a group of order h (0 means the "infinite" solution, which represents "nought" in the sense of the addition formula). For a rational, b of degree 2 , $(a^p, b^p) = (a, -b)$; further $(2(p+1)-h)(a, b) = 0$, because those solutions, together with the solutions with $b = 0$, form a group of the complementary order $2(p+1) - h$; hence the right hand side becomes $(p+1-h)(a^p, b^p) = (p+1-h)(a, -b) = -(p+1-h)(a, b) = (p+1)(a, b)$. In order to complete the proof, one has to verify the relation also for the higher algebraic solutions (α, β) ; then it actually holds identically in (x, y) . — Introducing $(x^p, y^p) = (x_0, y_0)$, this relation may be considered as $\pi(x_0, y_0) + \overline{\pi}(x_0, y_0) = (p+1-h)(x_0, y_0)$, where π is an operation which represents the "complex multiplication" in the elliptic field, and $\overline{\pi}$ its "conjugate" operation. $\pi\overline{\pi} = p$ in the sense $\pi\overline{\pi}(x_0, y_0) = p(x_0, y_0)$. And $\pi + \overline{\pi} = p + 1 - h$ in the sense of the relation in question.

I think this method of proving Riemann's hypothesis will please you better than my original one. It is, by the way, not quite the translation of my analytical method into algebraic language. For, there I consider complex multiplication with $\pi \pm 1$, whereas here with π. I hope, however, to find the algebraic translation also for the operators $\pi \pm 1$, and to get by this nearer to the actual solutions, not only to their number h.

Kindest regards and much love,

yours,
Helmut.

1.43 06.11.1933, Postcard Hasse to Davenport

H. just finished the new proof in the elliptic case. The whole proof is much shorter now. It is nothing else than a translation of every step of old proof into algebraic language.

6.11.33

My dear Harold,

I have just finished the proof of the identity stated in yesterday's letter. As I thought, one has to consider also the operators $\pi \pm 1$. Everything pans out quite satisfactor(il?)y then. The case of a higher E_{p^f} makes no difficulties at all. No preliminary irrational extension is necessary, as in my analytical proof, because there is no need of bringing the formulae to dimension 0. The whole proof is much shorter now. I could write it down on 20 pages of my usual (small) size, though I am afraid there is very much text and very few formulae in it. I have to consider a lot of isomorphisms, automorphisms, homomorphisms, and meromorphisms, the latter being a new conception very useful for this subject, meaning an isomorphism between $K = k(x, y)$ and a sub–field $K' = k(x', y')$. I really think, I ought to publish this new proof, and not my old analytical one, particularly with regard to the fact that the new proof is nothing else than a translation of every step (really every !) of my old proof into algebraic language.

The new proof is better than the old one also because I now need not introduce the assumption that the original base–field E_q is of odd degree f, which was necessary on account of certain difficulties in the theory of the J–transformation.

The main fact $(p + 1 - h)^2 < 4p$ may also be derived without the rather complicated comparison of degrees, namely from the fact that Pell's equation has an infinity of solutions for positive discriminant. I rather prefer this other argument.

Best wishes and much love,

yours,

Helmut

1.44 11.11.1933, Hasse to Davenport

Further information about mathematicians suffering under present circumstances. Blumenthal. Hensel has written to him. Neugebauer. Landau. Heilbronn and Lüneburg. Feller. Kindest regards to Courant. More detailed account about H.s recent work in the elliptic case. Torsion.

11. 11. 33

My dear Harold,

The enclosed letter means only a formality which I am bound to fulfil. I do not quite remember whether it was last year's or this year's amount that I got from you by way of our account running. Do you ?

I have got further information about mathematicians suffering under the present circumstances. *Blumenthal* has got the sack now, and as I am told without a pension. Hensel has just written to him to learn further details as to his actual position and his intentions. I shall give you the answer in due course. *Neugebauer* will be actually going to Kopenhague. He has resigned his post. *Landau* was badly treated by a troop of students. They assembled in the corridor leading to his class–room, just before his first lecture, prevented everyone from going in, and escorted him eventually into the empty room. I do not think they were mathematicians. I am awaiting further information as to whether he has given up lecturing for this term on account of this or not. He is entitled to and even bound to lecture by the government.

Quick help would be welcome in particular for *Heilbronn* and *Lüneburg*. Heilbronn is *not*[,] as I wrote in a former letter[,] paid by Landau from a private source. He was paid by the government as Landau's assistant and has lost this job now, because he is of Jewish descent. He has no means to live on. Lüneburg, though Arian, has got the sack because he was suspicious from the political point of view. He has also no means to live on. There is also *Feller* who had a position in Kiel. He is in Kopenhague now but still without a job. He has a small fund saved from his salary in Kiel, but this will not last very long.

I was interested to hear about Courant's settling down for the term there. Please give him my kindest regards.

I owe you a more detailed account about my recent work. I consider the Weierstrass equation

$$y^2 \;=\; 4x^3 - g_2x - g_3\,, \quad \Delta = g_2^3 - 27g_3^2 \neq 0$$

in a fixed finite field k of $q = p^f$ elements, $p \neq 2$. By Gothic letters I denote solutions of this equation, in particular by $\mathfrak{x}$ the indeterminate solution $(x,\, y)$, by $\mathfrak{x}_1, \ldots$ solutions whose two terms are rational functions of the indeterminates $x,\, y$, and by $\mathfrak{a}, \ldots$ constant solutions. The latter may be rational (in k) or algebraic. The algebraic basis of my proof is Abel's theorem in its original form. It may be stated as follows:

In the whole of all solutions (constant or not) there exists a unique, commutative, associative, and uniquely invertable operation, called addition, denoted by:

$$\mathfrak{x}_1 + \mathfrak{x}_2 = \mathfrak{x}_3\,.$$

In order to make this generally true it is necessary to introduce formally the infinite solution $\mathcal{O} = (\infty,\, \infty)$. This solution plays the rôle of zero for the addition. Opposite solutions are such that differ by the sign of the y–term only. I denote therefore the opposite solution $(x_1,\, -y_1)$ to $\mathfrak{x}_1 = (x_1,\, y_1)$ by $-\mathfrak{x}_1$. The actual rational addition formulae are not necessary for my proof. I only need the properties mentioned before.

The formal introduction of $\mathcal{O} = (\infty,\, \infty)$ becomes more than only formal by the following fact: Let $\mathfrak{x}_1,\, \mathfrak{x}_2,\, \mathfrak{x}_3$ be three solutions with

$$\mathfrak{x}_1 + \mathfrak{x}_2 = \mathfrak{x}_3\,,$$

and $\mathfrak{a}$ a constant solution. By replacing the $x,\, y$ in $\mathfrak{x}_1,\, \mathfrak{x}_2,\, \mathfrak{x}_3$ by the two terms $a,\, b$ of $\mathfrak{a}$ one gets three constant solutions $\mathfrak{a}_1,\, \mathfrak{a}_2,\, \mathfrak{a}_3$ with

$$\mathfrak{a}_1 + \mathfrak{a}_2 = \mathfrak{a}_3\,,$$

and this is true including the cases where one or more of the $\mathfrak{a}_1,\, \mathfrak{a}_2,\, \mathfrak{a}_3$ become $\mathcal{O}$. This must be strictly defined and proved, of course. I do it by using the arithmetic theory of F.K. Schmidt ($\mathfrak{a}_1 = 0$, when the prime divisor $\mathfrak{Q}$, defined by $\mathfrak{x} \equiv \mathfrak{a} \bmod. \mathfrak{Q}$, occurs in the reduced denominator of $\mathfrak{x}_1$).

I prove now by induction (following approximately Weber, Algebra III, §58) the following theorem:

a) *When* $(n,\, p) = 1$, *there are exactly* n^2 *solutions* $\mathfrak{a}_n$ *with* $n\mathfrak{a}_n = \mathcal{O}$.

b) *When* $n = p^v$, *there are exactly* n *solutions* $\mathfrak{a}_n$ *with* $n\mathfrak{a}_n = \mathcal{O}$, *if the invariant* A , *defined in my last letter, is not zero.*

b') *When* $n = p^v$ *and* $A = 0$, *there is only* 1 *solution* $\mathfrak{a}_n$ *with* $n\mathfrak{a}_n = 0$, *namely* $\mathfrak{a}_n = \mathcal{O}$.

Notice that A depends on $\mathfrak{J} = \frac{g_2^3}{\Delta}$ (and p) only, not on the exponent v .

The solutions $\mathfrak{a}_n$ are, of course, constant and generally algebraic. They form a sub–group $\mathfrak{G}_n$ of the group G of all algebraic solutions. Conversely, every algebraic solution $\mathfrak{a}$ is of finite order, i. e. , a solution $\mathfrak{a}_n$ with a suitable n .

From this theorem one finds immediately the structure of the group G of all algebraic solutions. G is the direct product of two groups G_1 and G_2 , G_1 consisting of all solutions with order prime to p , and G_2 of all solutions with order a power of p .

a) G_1 *is isomorphic to the group of all pairs of rational numbers* (r_1, r_2) *mod.* 1 *with denominators prime to* p . *The sub–groups* $\mathfrak{G}_n$ *of the* $\mathfrak{a}_n$ $((n, p) = 1)$ *are bicyclic of type* (n, n) .

b) G_2 *is* 1 *for* $A = 0$. *For* $A \neq 0$, G_2 *is isomorphic to the group of all single rational numbers* r *mod.* 1 *with denominator a power of* p . *The sub–groups* $\mathfrak{G}_n$ *of the* $\mathfrak{a}_n$ $(n = p^v)$ *are cyclic of order* n .

The algebraic solutions of order prime to n may therefore be represented by the points with rational relative coordinates in a parallelogramm (there is no reason for fixing the proportion of its sides or the magnitude of its angles at present).

Analogously the solutions of order a power of p may be represented by the points with rational relative coordinate on a line (with two ends). Now, with the relation $\mathfrak{x}_1 + \mathfrak{x}_2 = \mathfrak{x}_3$ of Abel's theorem one has simultaneously the differential equation

$$\frac{\mathrm{d}x_1}{y_1} + \frac{\mathrm{d}x_2}{y_2} = \frac{\mathrm{d}x_3}{y_3} .$$

Hence in particular

$$\frac{\mathrm{d}x_n}{y_n} = n\,\frac{\mathrm{d}x}{y} \quad \text{for } n\mathfrak{x} = (x_n, y_n) ,$$

and consequently

$$\frac{\mathrm{d}x_p}{y_p} \;=\; 0 \quad \text{for} \;\; p\mathfrak{x} = (x_p,\, y_p)\,.$$

From this it follows easily that x_p, y_p are rational functions of x^p, y^p. Therefore in $q\mathfrak{x} = (x_q,\, y_q)$ the terms x_q, y_q are rational functions of x^q, y^q. I denote by $q\mathfrak{x}^{q^{-1}}$ the pair of rational functions of x, y which arises by replacing x^q, y^q by x, y in the two terms x_q, y_q of $q\mathfrak{x}$. *This pair* $q\mathfrak{x}^{q^{-1}}$ *is again a solution, as well as the pair* $\mathfrak{x}^q = (x^q,\, y^q)$. For the coefficients of Weierstrass' equation remain unaltered by raising to the q^{th} power.

Hence the operation giving $q\mathfrak{x}$ from $\mathfrak{x}$ may be decomposed into two operations π and $\overline{\pi}$, π giving $\mathfrak{x}^q$ from $\mathfrak{x}$, and $\overline{\pi}$ giving $q\mathfrak{x}$ from $\mathfrak{x}^q$, i. e., $q\mathfrak{x}^{q^{-1}}$ from $\mathfrak{x}$. I write for this:

$$\pi\mathfrak{x} \;=\; \mathfrak{x}^q\,, \quad \overline{\pi}\mathfrak{x} \;=\; q\mathfrak{x}^{q^{-1}}\,.$$

Then obviously

$$\pi\overline{\pi}\mathfrak{x} \;=\; \overline{\pi}\pi\mathfrak{x} \;=\; q\mathfrak{x}\,.$$

I study now the effect of those operations on the group G_1 of all algebraic solutions of order prime to n. One sees immediately that they effect automorphisms of this group. Hence there exist two matrices $P = \begin{pmatrix} a & b \\ c & d \end{pmatrix}$, $\overline{P} = \begin{pmatrix} \overline{a} & \overline{b} \\ \overline{c} & \overline{d} \end{pmatrix}$ such that the algebraic solution $\mathfrak{a}$ represented by the point $(r_1,\, r_2)$ of the paralle[lo]gramm is changed by π to $\pi\mathfrak{a} = \mathfrak{a}^q$ corresponding to the point

$$(r_1,\, r_2)\, P \;=\; (ar_1 + cr_2,\, br_1 + dr_2)$$

and analogously is changed by $\overline{\pi}$ into $\overline{\pi}\mathfrak{a} = q\mathfrak{a}^{q^{-1}}$ corresponding to the point

$$(r_1,\, r_2)\, \overline{P} \;=\; (\overline{a}r_1 + \overline{c}r_2,\, \overline{b}r_1 + \overline{d}r_2)\,.$$

These matrices P, $\overline{P}$ have necessarily integer coefficients (and determinant only a power of p). The actual value of the determinant follows from $\pi\overline{\pi} = q$ (in the above sense), i. e., from $P\overline{P} = qE$, hence $\overline{P} = q\,P^{-1}$, and $|P| = |\overline{P}| = q$, where E denotes the unit matrix. From this one has

$$\overline{P} \;=\; \begin{pmatrix} d & -b \\ -c & a \end{pmatrix},$$

hence

$$P + \overline{P} \;=\; m\,E \quad (\text{with } m = a + d).$$

I shall prove that this number m is the "error term", and

$$m^2 < 4q\,,$$

which is Riemann's hypothesis.

First of all, I re–translate the relation $P + \overline{P} = m\,E$ into an identity for $\mathfrak{x}$, namely the identity

$$\pi\mathfrak{x} + \overline{\pi}\mathfrak{x} \;=\; m\mathfrak{x}\,, \quad \text{i. e. } \; \mathfrak{x}^q + q\mathfrak{x}^{q^{-1}} = m\mathfrak{x}\,, \tag{1}$$

mentioned in my last letter. This is done at once by stating first that this identity is true for every algebraic solution of order prime to p (every element of G_1) from the meaning of π and $\overline{\pi}$ as P and $\overline{P}$ within G_1, and observing then that a rational relation which is true for an infinity of values holds identically.

I now prove

$$m^2 < 4q\,,$$

i. e., that the discriminant

$$D \;=\; m^2 - 4q$$

of the quadratic polynomial

$$f(t) \;=\; |tE - P| \;=\; t^2 - mt + q$$

whose (formal) roots are π, $\overline{\pi}$, is *negative.*

This may be proved by comparing degrees in (1) as I pointed out in my last letter. m^2 is the degree of the x–term x_m on the right–hand side of (1). This is, by the way, a rational process for calculating the error term m (except for its sign). Another proof follows from playing about with identities in $\mathfrak{x}$ and automorphisms of G_1. Suppose $D > 0$. Then there are integer solutions g, n of $|g\,P + n\,E| = \pm 1$ (Pell's equation). Putting accordingly $\mathfrak{x}' = g\,\mathfrak{x}^q + n\,\mathfrak{x}$, one finds by considering the behaviour of the prime divisors that $\mathfrak{x} \to \mathfrak{x}'$ means an automorphism of $K = k(\mathfrak{x})$ which leaves the point $\mathcal{O} = (\infty, \infty)$ invariant. Therefore necessarily $\mathfrak{x}' = \pm\mathfrak{x}$, i. e.,

$$g\,\mathfrak{x}^q + n\,\mathfrak{x} \;=\; \pm\mathfrak{x}\,.$$

Then, going back to G_1,

$$g\,P + n\,E \;=\; \pm E\,,$$

which is impossible except for $n = \pm 1$, $g = 0$. (For the cases $g_2 = 0$ or $g_3 = 0$ where K has more automorphisms (6 or 4) which leave $\mathcal{O} = (\infty, \infty)$ invariant, the same argument slightly modified holds). Similarly $D = 0$ is excluded by noticing that then $P = gE$, hence $\mathfrak{x}^q = g\,\mathfrak{x}$ with $g^2 = q$, which is easily contradicted.

I consider finally the group $\mathfrak{G}$ of all *rational* solutions $\mathfrak{a}$, whose number h is to be determined. The $\mathfrak{a}$ are characterized by

$$\mathfrak{a}^q \;=\; \mathfrak{a}, \quad \text{i. e.} \quad (\pi - 1)\mathfrak{a} \;=\; \mathcal{O}.$$

Similarly

$$\mathfrak{a}^q \;=\; -\mathfrak{a}, \quad \text{i. e.} \quad (\pi + 1)\mathfrak{a} \;=\; \mathcal{O}$$

characterizes a group $\mathfrak{G}'$ of certain rational or algebraic solutions, number h'. One easily sees that $\mathfrak{G}'$ corresponds uniquely to the "conjugate" equation

$$ry^2 \;=\; 4x^3 - g_2x - g_3\,, \qquad (r \text{ no square in } k\,).$$

Therefore,

$$h + h' \;=\; 2(q+1)\,.$$

Now $\mathfrak{G}$ is the direct product of $\mathfrak{G}_1$ (order h_1 prime to p) and $\mathfrak{G}_2$ (order h_2 a power of p), and similarly $\mathfrak{G}'$, $\mathfrak{G}'_1$, $\mathfrak{G}'_2$, h'_1, h'_2.

$$\begin{aligned} h &= h_1h_2 \\ h' &= h'_1h'_2\,. \end{aligned}$$

By translating $\mathfrak{G}_1$ into the paralle[lo]gramm one sees that h_1 is the number of solutions (r_1, r_2) with denominators prime to p of

$$(r_1, r_2)(P - E) \equiv 0 \text{ mod. } 1\,.$$

Hence

$$h_1 \;=\; \text{the factor prime to } p \text{ of } \Big||P - E|\Big| \;=\; q + 1 - m$$

$$(\text{this is} > 0 \text{ since } m^2 < 4q\,)$$

$$\boxed{\text{Nb. } |P - E| = f(1) = 1^2 - m\,1 + q}$$

On the other hand, every solution in $\mathfrak{G}$ satisfies (1); hence

$$\mathfrak{a} + q\,\mathfrak{a} \;=\; m\,\mathfrak{a}$$

$$(q+1-m)\mathfrak{a} \;=\; \mathcal{O}\,.$$

I apply this to the solutions in $\mathfrak{G}_1$. They form a *cyclic* group by theorem **b)** above. With regard to the last equation it follows:

$$h_2 \leq \text{ the highest power of } p \text{ in } q+1-m\,.$$

Hence,

$$h \;=\; h_1\,h_2 \leq q+1-m\,.$$

An analogous argument leads to

$$h' \;=\; h_1'\,h_2' \leq q+1+m\,.$$

Now $h+h' = 2(q+1)$. Hence exactly,

$$\begin{aligned} h &= q+1-m \\ h' &= q+1+m\,, \end{aligned}$$

q.e.d.

The special case $A = 0$, i. e. **b')** above, is equivalent with $m = 0$, as is easily stated.

It remains still to distinguish between the two conjugate numbers h, h'. I hope this may be done by congruences, as we already know for the special cases $g_2 = 0$ or $g_3 = 0$. It would suffice to get a connection between the congruence value of h or m mod. 4, and the quadratic character $\chi(g_3)$ which distinguishes between those two conjugate equations. Perhaps it is possible to get such a connection by considering the equation

$$-m \;=\; \sum_a \chi\Big(f(a)\Big)\,, \quad f(x) \;=\; 4x^3 - g_2x - g_3\,,$$

as a congruence mod. 4. I did not succeed, though, until now. Perhaps you can help me there. I always think the decomposition

$$f(x) \;=\; (4x^3 - g_2x) - g_3$$

where the first term changes sign with $x \to -x$ should give the congruence value required.

Of course

$$\begin{array}{lcll} h \equiv & 0 \text{ or } 2 & \text{mod.}\,4 & \text{when } n_0 = 1 \\ h \equiv & 0 & \text{mod.}\,4 & \text{when } n_0 = 3 \\ h \equiv & +1 \text{ or } -1 & \text{mod.}\,4 & \text{when } n_0 = 0 \end{array} \quad \left(\begin{array}{l} \text{this follows from con–} \\ \text{sidering the } \mathfrak{a}_2 \text{ (in } \mathbf{a}\text{)} \\ \text{above) contained in } \mathfrak{G} \end{array}\right)$$

where n_0 is the number of solutions of $f(x) = 0$.

I have also written to Mordell about all this. I think he will be able to appreciate it.

I still long for a letter from you as in the good old times.

Much love and best wishes,

Yours,
Helmut.

13.11. P.S. *Landau has just resigned his post on account of what happened – The enclosed "Stimmschein" may interest you.*

1.45 28.11.1933, Davenport to Hasse

Details on the elliptic case. It seems odd that the case $N = p$ cannot arise for $f > 2$. D. cannot follow H.'s remark that this is clear from $\pi = \sqrt{-q}$. D. studies generalization of abundant numbers. On the (ordinary) zeta function. Ritt. Hardy's Conversation class.

Cambridge. 28 Nov. 1933.

My dear Helmut,

Very many thanks for your letters. I am painfully conscious that our correspondence lately has been very onesided; I do not know that there is any explanation I can advance except my inveterate laziness, and the absence of anything of great importance for me to communicate. I am very much impressed indeed by your account of your recent work on the Weierstrassian equation in a finite field. It really is marvellous that it should have been so systematized and simplified. I do not think I shall properly understand it until you explain it to me in person – and probably not then – I am so stupid when any question of groups or automorphisms arises. At the moment I have not got Weber III (which is also not in the University Library, incredible though that seems), so I cannot follow what seems to be the most important part of the treatment. I also do not follow your remark about the addition theorem: "*this must be strictly defined and proved, of course. I use the arithmetic theory of F.K. Schmidt.*"

It certainly seems odd at first sight that the case $N = p$ cannot arise when $f > 2$. I do not follow your remark that this is clear from $\pi = \sqrt{-q}$. Why should this be impossible? I am really very stupid.

As regards the mean square over g_3 of the error term, this is quite easily obtained: -

$$\begin{aligned}
\sum_{g_3}\left(\sum_x \chi\big(f(x)\big)\right)^2 &= \sum_x\sum_y\sum_g \chi\left((4x^3-g_2x-g)(4g^3-g_2y-g)\right)\\
&= \sum_{\substack{x,y\\4x^3-g_2x=4y^3-g_2y}} (p-1) + \sum_{\substack{x,y\\4x^3-g_2x\neq 4y^3-g_2y}} (-1)\\
&= p^2+p\sum_{\substack{x,y\\4(x^2+xy+y^2)=g_2\\x\neq y}} 1 \quad -p^2
\end{aligned}$$

It is quite simple to evaluate this exactly, and still easier to see that it is $p^2+O(p)$. But I cannot prove this for the sum over those g_3 with $\chi(g_3)=1$.

I feel very flattered that my paper is being considered in your Seminar. I should think it has almost lost interest now that you are on the way to a complete solution of all these questions. Indeed I do not suppose there is a single method used in it which is genuinely "appropriate".

I have not thought about mod p problems at all this term. For some time I worried a great deal about the following two problems (generalising the abundant numbers):

1) $a_1, a_2, \ldots$ is a given sequence of increasing positive integers. Has the sequence of all integers divisible by at least one of the a's necessarily got a density? (*I conjecture no.*)

2) $a_1, a_2, \ldots$ is a sequence of incr. pos. integers with the property that if m, n are unequal, then a_m does not divide a_n. Does it follow that the sequence a_n has zero density? (*I conjecture yes.*)

These problems look very simple, but I cannot solve them.

More recently I have been thinking about the ζ-function. A problem which had not been solved was that of proving that

$$\lim_{T\to\infty}\frac{1}{T}\int_0^T |\zeta(\sigma+it)|dt = \sum_1^\infty \left(d_{\frac{1}{2}}(n)\right)^2 n^{-2\sigma}$$

$$(\textit{where } \sqrt{\zeta(s)} = \textstyle\sum_1^\infty d_{\frac{1}{2}}(n)n^{-s} \textit{ for } \sigma > 1)$$

holds for $\sigma > \frac{1}{2}$. I succeeded in proving this, also the corresponding result with $\int |\zeta|^\lambda dt$ for $0<\lambda<4$, but I then discovered that a paper is in course of publication by Mr Ingham, in which this is proved. Another result I have obtained is to extend the range for which

$$\lim_{T\to\infty} \frac{1}{T}\int_0^T |\zeta(\sigma+it)|^{2k}dt = \sum_1^\infty \big(d_k(n)\big)^2 n^{-2\sigma}$$

(k integral) can be proved to hold from $\sigma > 1 - \frac{1}{k}$ to (roughly) $\sigma > 1 - \frac{\log k}{k}$. This is, of course, not frightfully important, but it is pleasant to get anything new about the ζ-function.

I take this opportunity of returning the letter you sent me some time ago with the reference to a paper by Ritt. I do not understand the theorem enunciated, but I presume it is not relevant to our exponential sum.

We have had some rather good talks at Hardy's Conversation Class this term: Besicovitch on the distribution of the digits in large integers, James (of Pasadena and Chicago) on Waring's problem, Hardy on bilinear forms in an infinity of variables, Hardy on von Staudt's theorem on Bernoulli numbers, Ingham on Tauberian theorems for general Dirichlet series, Miss Cartwright on functions which take no value more than p times in the unit circle, and (to-day) Rado on regular equations (Combinatorik).

Thank you very much indeed for your sympathy in connection with my father's illness. At the moment I believe he is slightly better.

Thank you very much for the "Stimmenschein". The sentiments expressed on it are very pacific, but nevertheless the fate meeted out to any genuine German pacifist remains an unpleasant one!

I hope I shall have an opportunity of seeing you sometime during the winter. My love to Clarle, and to yourself,

Yours,

Harold.

1.46 Manuscript by Hasse, no Date

The zeros of zeta function have absolute value > 1 *and* $< q$.

About November 1933[1]

Let $\omega_j \quad (j = 1, \ldots, 2g)$ be the roots of the ζ–function of an algebraic function field K of genus $g \geq 1$ over a finite field k of q elements, and $K_n = K\, k_n$ where k_n is the finite field of q^n elements. Then ω_j^n are the roots of the ζ–function of K_n. The following facts are known:

(1) $\displaystyle\sum_j \omega_j^n = q^n + 1 - N_1^{(n)}$, $N_1^{(n)}$ the number of prime divisors of degree 1 of K_n/k_n,

(2) $\displaystyle\prod_j (1 - \omega_j^n) = h^{(n)}$, the class number of K_n/k_n (number of divisors classes of any fixed degree of K_n/k_n), hence $\neq 0$; therefore no ω_j is a root of unity,

(3) $1 \leq |\omega_j| \leq q$, from Euler's product and the functional equation,

(4) the ω_j can be arranged into pairs ω_j, ω_j' with $\omega_j\, \omega_{j'} = q$.

Theorem. $1 < |\omega_j| < q$.

It suffices to prove $|\omega_j| < q$. Since $N_1^{(n)} \geq 1$ for all multiples n of a certain n_0, we may suppose $N_1^{(n)} \geq 1$ for all n. Then simply

$$\sum_j \omega_j^n \leq q^n .$$

Let

$$\omega_j = q^{\vartheta_j} e^{2\pi i \varrho_j}, \quad \varrho_j \text{ mod.}^+ 1 .$$

[1]The manuscript is written in English language by Hasse's hand. It has been found in Davenport's legacy. It appears that Hasse sent it to Davenport, some time in November 1933.

For a given integer N there are integers $n \geq 1,\ m_j$ so that

$$|n\,\varrho_j - m_j| < \frac{1}{N}\,.$$

Elementary proof. Take the N^{2g+1} multiples $0\,(\varrho_j),\ 1\,(\varrho_j), \ldots, N^{2g}\,(\varrho_j)$ mod.$^+$ 1 of the vector (ϱ_j) and distribute them in the N^{2g} cubes with side–length $\frac{1}{N}$, into which the unit cube decomposes. There are at least two multiples in the same cube. Their difference $n\,(\varrho_j)$ gives a solution of the inequalities in question.

Remark. Since $\varrho_{j'} \equiv -\varrho_j$ mod.$^+ 1$, it suffices to take g of the ϱ_j only. Then $1 \leq n \leq N^g$ instead of $1 \leq n \leq N^{2g}$. This is irrelevant, though. No knowledge on the magnitude of n is required.

1.) Now

$$|e^{2\pi i n\varrho_j} - 1| \;=\; |e^{2\pi i(n\varrho_j - m_j)} - 1| \;=\; 2|\sin\pi(n\varrho_j - m_j)| < \frac{2\pi}{N}\,,$$

since $|\sin x| \leq |x|$ for all real x. Therefore

$$\begin{aligned}
|\sum_j \omega_j^n - \sum_j q^{n\vartheta_j}| &= |\sum_j q^{n\vartheta_j}(e^{2\pi i n\varrho_j} - 1)| \\
&\leq \sum_j q^{n\vartheta_j}|e^{2\pi i n\varrho_j} - 1| \\
&< \frac{2\pi}{N}\sum_j q^{n\vartheta_j}\,, \\
\sum_j q^{n\vartheta_j} - \sum_j \omega_j^n &< \frac{2\pi}{N}\sum_j q^{n\vartheta_j}\,, \\
(1 - \frac{2\pi}{N})\sum_j q^{n\vartheta_j} &< \sum_j \omega_j^n \\
&\leq q^n
\end{aligned}$$

where n is determined as above to a given N. Let

$$\vartheta_1 \geq \vartheta_2 \geq \cdots$$

Suppose $\vartheta_1 = 1$. Then

$$(1 - \frac{2\pi}{N})(1 + \frac{1}{q^{n(1-\vartheta_2)}}) < 1 \,.$$

Taking $N \geq 4\pi$,

$$1 + \frac{1}{q^{n(1-\vartheta_2)}} < 2 \,,$$

hence

$$\begin{aligned} q^{n(1-\vartheta_2)} &> 1 \,, \\ 1 - \vartheta_2 &> 0 \,, \\ \vartheta_2 &< 1 \,. \end{aligned}$$

Remark. Only $n \geq 1$ is needed; no limit $n \to \infty$.

2.) For any n,

$$\sum_j q^{n\vartheta_j} \sin 2\pi n \varrho_j \;=\; 0 \,,$$

hence

$$-q^{n\vartheta_1} \sin 2\pi n \varrho_1 \;=\; \sum_{j \geq 2} q^{n\vartheta_j} \sin 2\pi n \varrho_j \,.$$

Suppose $\vartheta_1 = 1$. Then $\vartheta_2, \ldots < 1$, hence

$$|\sin 2\pi n \varrho_1| \leq \sum_{j \geq 2} \frac{1}{q^{n(1-\vartheta_j)}} \;\longrightarrow 0 \;\text{ for }\; n \to \infty \,.$$

To complete the proof, it suffices to show that there is a sequence of integers n, for which $n\varrho_1$ mod.$^+$ 1 tends to $\pm\frac{1}{4}$, say.

Now ϱ_1 is irrational. For, $\omega_1 = qe^{2\pi i \varrho_1}$, and $\omega_{1'} = e^{-2\pi i \varrho_1}$ is no root of unity. Hence, given N and determining $n_0 \geq 1,\ m_0$ by

$$|n_0 \varrho_1 - m_0| < \frac{1}{N} \,,$$

one has necessarily

$$\varrho_0 = n_0 \varrho_1 - m_0 \neq 0 \,.$$

Therefore there is an integer ν, for which

$$\left|\nu\,|\varrho_0| - \frac{1}{4}\right| < |\varrho_0|\,, \quad \text{i. e.,} \quad |\nu\varrho_0 \mp \frac{1}{4}| < |\varrho_0|\,.$$

This means

$$|\nu\, n_0 \varrho_1 \mp \frac{1}{4} - \nu\, m_0| < |\varrho_0| < \frac{1}{N}\,.$$

Hence, given N, there are integers n, m with

$$|n\, \varrho_1 \mp \frac{1}{4} - m| < \frac{1}{N}\,.$$

Then $|\sin 2\pi n \varrho_1| \to 1$ for $N \to \infty$, as required.

1.47 02.01.1934, Hasse to Davenport

H. has no information from Göttingen or from Königsberg. Tomorrow H. will visit Göttingen to talk with Courant about negotiations in Berlin, in case H. will obtain an offer for Göttingen. H. worries about the health condition of Mrs. Hasse. H. thanks for subscription of Manchester Guardian, which D. has presented to him. H. hopes to meet D. in the New Year.

2.1.34

My dear Harold,

I have still to thank you for the nice Times Calendar. I like it very much and will enjoy it throughout the year. The examination paper has greatly interested me. I did not try, though, to solve the problems. It would be nice to discuss some points in them with you on the occasion of our next meeting. I have nothing heard from Göttingen yet, neither from Königsberg. To–morrow I am going to G. for a short talk with Courant about things there and the policy for my negotiations in Berlin, should I be offered the position in G.

I am rather troubled by Clärle's present condition of health. There seems to be something wrong with the kidneys, although I do not think it is another stone. She has a continuous pain in the back. We are going to have the opinion of Prof. Boenninghaus on the case after a new detailed investigation.

I enjoy reading the Manch. Guard. Weekly. It is a very "substantial" paper. Thanks very much for your subscription on our behalf.

The very best wishes for a happy New Year, though a little belated. I hope we shall meet in the course of it very often.

Much love,

yours,
Helmut.

1.48 09.01.1934, Hasse to Davenport

H. had a short telephone conversation with D. Detailed report on the health condition of Mrs. Hasse. Joint paper on Gaussian sums? Irreducibility of radical equations over $\mathbb{Q}$. Report on trip to Göttingen and discussion with Courant. Should H. get an offer for Göttingen, it will be very difficult to do things correctly under the eyes of the mathematical world.

MARBURG–LAHN, DEN
9.1.34

My dear Harold,

I was very glad indeed to have a short talk with you over the 'phone last night, though your voice seemed extraordinarily distant and I had the utmost difficulty to catch your words. I hope you did not mind our telegraphic request for a trunk call.

I had a long interview with Prof. Boenninghaus over the 'phone this afternoon. He did not give great importance to a cure in Bad Wildungen, because the only effective thing in it, namely drinking Wildunger Wasser, could just as well be done here. He asserted quite positively that the idea of Nierenschrumpfung was "absurd". There was not the slightest symptom for it. The only thing that could be said was that Clärle's right kidney was a trifle lower down than the other. This was quite common with tall and slender women — sort of a compliment for Clärle. As to the two stones, they have approximately the size of a barley grain. He can by no means give any positive opinion as to the time when they are going off. Also it is not necessary that they grow. Much can be done to prevent the latter by drinking immense quantities and avoiding the forbidden victuals, though there is no certain guarantee against trouble by living up to all this carefully. There is also a possibility for loosening the stones, in particular the upper; by mechanical process, i. e., gymnastics, dancing, driving, etc., though again nothing is an absolute certain means to this effect. For the case of a colique he recommends going to the clinique provided the stone does not come off within a day or two. He can alleviate the pain only by morphine. The pain is chiefly due to the "Stauung" (Stockung) of the urin, not only to mechanical effects of the stone to the uraetra. It can also be alleviated by cathedrisising which is, of course, only possible in the clinique.

All this does not sound very reassuring. We have to put up with it and try to do our best for Clärle. I am ever so sorry for her, and so will you be.

What about our common paper on Gaussian sums ? I should be very glad, if you could manage to get down to work on it in not so remote a future.

Perhaps you are interested in the following question which is closely connected with my Aufgabe 147 in the D.M.–V.:

Let a be a rational number for which the radical $\sqrt[m]{a}$ cannot be reduced to a lower radical by cancelling prime factors of m with the exponents of the prime decomposition (the factor -1 included) of a. For which such $\sqrt[m]{a}$ is $\sqrt[m]{a}$ irreducible in the usual sense, i. e., the polynomial $x^m - a$ irreducible ?

The example $x^4 + 4$ shows that this polynomial may be reducible even if the radical is not reducible in the first sense.

I have taken up lectures to–day.

My trip to Göttingen was rather troubling. Courant was in an extremely sad mood. He is really in a very dreadful position. I could not make him hope for taking up his teaching in Göttingen in not to[o] far a time. The students are absolutely set on letting no Jew ever ascend again the “Lehrstuhl” there. Also apart from this questions things look rather entangled there. There are as many wills as heads. If I get the position there I will have the greatest difficulties to please all those different people, and to do the right things before the eyes of the mathematical world at the same time.

Best wishes to you and to the Mordells.

Yours,
Helmut.

1.49 16.01.1934, Hasse to Davenport

Solution of problem about radical equations. Mahler in Groningen.

16. 1. 34

My dear Harold,

Thanks for your kind letters of Monday and Thursday. I had a heavy attack of tooth–ache in the meantime. The dentist had put a drug into the root and closed it hermetically. As there was some kind of infection at the bottom, I got a very bad time until, on Monday morning, the tooth was opened again. I had to take refuge to veronal in order to get sleep for a few hours.

The solution of my problem about $x^m - a$ is: Suppose $a = (-1)^\nu \prod_{i=1}^r p_i^{\nu_i}$ and no odd prime divisor ℓ of m divides all ν_i, further, for $2 \mid m$, not all the ν_i und ν are even. Then $\sqrt[m]{a}$ is not reducible to a lower radical. The polynomial $x^m - a$, however, may be reducible. It is reducible, if and only if $4 \mid m$ and $a = -4c^4$. Then there are two irreducible factors $x^{2m_0} \pm 2cx^{m_0} + 2c^2$ ($m = 4m_0$). I am surprised at such a purely elementary problem "not suggesting anything to you".

I am glad Mahler has got a chance at Groningen. I think a lot of him, though I should not like having him permanently near me.

Very best wishes,

from

Helmut

1.50 29.01.1934, Hasse to Davenport

British newspapers are prejudiced against National Socialism. H. has found an error in his proof for the elliptic case, on the brink of his departure for his lectures in Hamburg. Is it possible that the Frobenius operator is transcendental? H. expects from D. the paper on Gaussian sums in finite fields. Thanks for the paper on "numeri abundant".

MARBURG–LAHN, DEN
29.1.34

My dear Harold,

Thanks awfully for your letter, the New Statesman, and the cutting. From those things, and from reading the M.G.Weekly for a longer period, I must say that English public opinion seems to be badly prejudiced against Germany. The papers pick up every bit of *bad* news they can get hold of about Germany, but they hardly think it worth while to write about *good* things brought about by National Socialism. One would not expect enthusiasm, since the whole movement has a certain and very outspoken tendency against the Allies because of the Versailles Treaty. But one *would* expect a certain detachment and aloofness, otherwise so strong in the English. It is not *their* business after all.

I am very troubled at present because I found a gap in my proofs about the elliptic case while drawing up my lectures for Hamburg. The whole thing seems too sensible for being *wrong*. But it may be that the proof of the actual result lies a bit deeper than my argument went so far. The possibility I have to exclude is that the operation π is transcendental. I can prove that *if it is algebraical* it must be imaginary quadratic, because a unit operation cannot exist.

Thanks also for the University lecture list. You have indeed a pretty good show there. I look forward to getting the paper on Gaussian sums in G.F. Your Times Calendar adorns my writing–desk and enjoys us all with a new picture and a lyrical rhyme every week.

Very best wishes,

Yours,
Helmut

Thanks for your paper on Num. Ab. I shall give the other copy to Franz. I am afraid there will be no use in a copy of my lectures at Hamburg, before the gap in the proof is not closed. Unfortunately I have written the text in German letters again — somehow I cannot get rid of this habit —. But perhaps they will make a copy at Hamburg of what I actually say.

1.51 10.02.1934, Davenport to Hasse

H. has posted the manuscript for joint papwer on Gaussian sums. D. will visit Marburg at the end of March.

Cambridge, 10 Feb. 1934.

My dear Helmut,

I posted to you late last night the MS I have made for our paper on Gaussian sums in finite fields (excluding the relations). I enclosed with it your MSS on Gaussian sums; perhaps you might let me have back that one (or those ones) relating to the relations (excuse the construction!), if you do not need it (them) at the moment. I did not send the MS we drew up for the first half of the paper, as it has been all embodied in this new MS.

1) I have put our names in the title in alphabetical order. But of course I shall not be offended or surprised if you alter them to the order of importance!

2) I only discovered too late that s was being used in two different senses, as a divisor of r, and as the complex variable (p^{-s}). Provisionally I altered the latter to S, but I would much prefer to alter the former instead.

3) I *think* everything is valid when $p = 2$. ((11) is then meaningless, but is unnecessary.) But I should not like to rely on my judgement alone.

4) There are doubtless a large number of mistakes, both of principle and detail, in the thing. Do not hesitate to revise it drastically. You will see that I have omitted most of your remarks on pages III, IV of your MS. I thought I understood them. But it is quite probable that I have missed the essential point of them, and that they ought to be put in in full. If so, do so.

5) I expect the length of the paper will exceed 8 pp, hence it may not be possible to get it in the Journal of the London Math. Soc. In this case, the best thing would be to send it to the Oxford Quarterly Journal (this being quicker than *Proc.* L.M.S.).

I hope you had a pleasant time at Hamburg, and that you have overcome the flaw about the possibility of the operation π being transcendental.

Mordell was here last weekend and is here now. He has given two lectures each time. So far he has sketched the theory of ideals and has proved that all the rational solutions of a cubic indeterminate equation in two variables can be derived by rational operations from a finite number of solutions.

I am very pleased indeed to hear that Clärle's symptoms have disappeared, and hope that it is a genuine recovery.

English public opinion is certainly against most of the developments in Germany, and with reason, I think. But I think no assertions about Germany have been made which are so false and ridiculous as some of the statements about England which have been made by prominent Nazis.

I hope to see you again in the not too distant future, – preferably in England, but if (as now seems likely) you will not be coming here, I hope to come over to Marburg before the end of March – if you will put up with me.

Very best wishes,

Yours,

Harold.

I am not used to typing letters "out of my head", that is the only excuse I can offer for the bad style of this letter.

1.52 12.02.1934, Davenport to Hasse

Mordell pointed out Stickelberger's result!

12.2.34

My dear Helmut,

I regret to say that the second part of our paper (prime ideal decomposition of the gen. Gaussian sums) was done 39 years ago by Stickelberger (Math. Ann. 37(1890)). Mordell reminded me of this paper, which is cited in Hilbert. Apparently S. does succeed in putting through the method of the first of the two proofs. He does not say anything about the connection with sub-fields–as far as I can see from a casual glance–but I expect he thought it trivial. Or anyhow, I feel sure someone will have done it.

Sorry to be a bearer of this ill news,

Yours, Harold.

1.53 12.02.1934, Hasse to Davenport

Hamburg was a full success from every point of view. H. was able to fill the gap. As to higher genus, from what Artin and H. found it becomes only a matter of patience. H. is going to carry through all the details without bothering about special cases now. H. has received D.'s paper on Gaussian sums. H. proposes to publish the promised continuation of the L-functions of $y^p - y = x^m$ and $y^n = 1 - x^m$ in the Berliner Sitzungsberichte. Also, H. plans to publish there his general theory for for $y^p - y = R(x)$.

12.2.34

My dear Harold,

I am at home again from Hamburg and have settled down to business to–day after a day in Winterberg with Gertrud.

Hamburg was a full success from every point of view. I was able to fill up the gap in my proof shortly after I wrote you how depressed I was. The new proof is, as Artin meant, even more adequate than the old would have been, were it consistent. I give a direct proof of the identity*)

$$\mathfrak{x}^p + p\mathfrak{x}^{p^{-1}} \;=\; v\,\mathfrak{x} \quad (v = p+1-h \;\text{ the error term})$$

on the following lines:

The identity holds for all "rational" solutions $\mathfrak{x} \equiv \mathfrak{a}$, since $\mathfrak{a}^p = \mathfrak{a}$ and the identity may be written as

$$(\mathfrak{x}^p - \mathfrak{x}) + p(\mathfrak{x}^{p^{-1}} - \mathfrak{x}) + h\,\mathfrak{x} \;=\; \mathfrak{u} \;\; \text{(zero element)}\,.$$

Notice $h\,\mathfrak{a} = \mathfrak{u}$, because the $\mathfrak{a}$'s form a group of order h.

The identity also holds for all solutions $\mathfrak{a}'$ with $\mathfrak{a}'^p = -\mathfrak{a}'$ (i. e. $a'^p = a'$, $b'^p = -b'$), which form a group of order $h' = 2(p+1) - h$. For the identity may also be written as

$$(\mathfrak{x}^p + \mathfrak{x}) + p(\mathfrak{x}^{p^{-1}} + \mathfrak{x}) - h'\mathfrak{x} \;=\; \mathfrak{u}\,.$$

*) p denotes the prime *power* order of the GF.

Finally the identity holds for all the sums $\mathfrak{a} + \mathfrak{a}'$, which are at least $\frac{h\,h'}{4}$ different solutions of $f(x) = y^2$. For $\mathfrak{a} = \mathfrak{a}'$ holds only for $\mathfrak{a} = \mathfrak{u}$ and the three solutions $\mathfrak{a} = (e,\, 0)$, where $f(e) = 0$.

We have therefore $\frac{h\,h'}{4} = \frac{p^2}{4} + o(p)$ different constant solutions of the identity. $o(p)$ comes simply from Artin's result, that the upper limit ϑ of the real parts of the zeros is < 1. It refers to increasing p for a fixed prime basis p_0 $(p = p_0^r,\ r \to \infty)$. Now the multiplication theory shows that the degree of the x–component of $\mathfrak{x}^p + p\mathfrak{x}^{p^{-1}} - v\mathfrak{x}$ is $o(p^2)$, since for $\mathfrak{x}^p$ and $p\,\mathfrak{x}^{p^{-1}}$ it is p exactly, and for $v\mathfrak{x}$ it is $v^2 = o(p^2)$. For sufficiently large p the denominator of the x–component of $\mathfrak{x}^p + p\mathfrak{x}^{p^{-1}} - v\mathfrak{x}$ has therefore more zeros than is allowed by the degree. This is a contradiction when $\mathfrak{x}^p + p\mathfrak{x}^{p^{-1}} - v\mathfrak{x} \neq \mathfrak{u}$. Therefore the identity holds for sufficiently large p. This is sufficient for all the rest.

From what Artin and I found when considering the possibilities of generalisation to higher genus, it becomes only a matter of patience to do this. The general line is fully obvious now. The addition theorem is generalisable in a purely algebraic form. If $f(x,\, y) = 0$ has genus g, then for each two sets of g solutions

$$\begin{array}{l}(x_{11},\, y_{11}),\ \ \ldots\ \ ,(x_{1g},\, y_{1g})\\ (x_{21},\, y_{21}),\ \ \ldots\ \ ,(x_{2g},\, y_{2g})\end{array}$$

there exists a third such set (uniquely determined, except the arbitrary arrangement)

$$(x_{31},\, y_{31}),\ \ \ldots\ \ ,(x_{3g},\, y_{3g})$$

with all the algebraic properties of an "addition" of the two sets.

The number of automorphisms of the field K of all symmetrical functions of g independent solutions

$$(x_1,\, y_1),\ \ \ldots\ \ ,(x_g,\, y_g)$$

is finite. This gives the fact that the abstract operation π (defined as p^{th} power) is algebraic of degree $2g$, and that the field of π as an algebraic number contains only $g-1$ independent units, i. e., is totally–imaginary.

I am going to carry through all the details without bothering about any more special cases now.

I received your manuscript on Gaussian sums. With all the accumulated work I found here on my return, I have not been able to give it more than a very superficial glance. It seems perfectly alright, though. I will look at it more carefully to–morrow.

Would you mind publishing the promised continuation on the L–functions for $y^p - y = x^m$ and $y^n = 1 - x^m$ in the Berliner Sitzungsberichte ? I will publish my general theory of $y^p - y = R(x)$ there, and it seems appropriate to let the other paper follow immediately.

I enclose a clipping from a Hamburg paper which may interest you. I intended going to this film on Saturday, but was prevented from doing so by the possibility of going to Winterberg on Sunday.

Excuse my rather hurried letter. You will hear from me soon about our paper.

Very best wishes,

yours,
Helmut

1.54 17.02.1934, Hasse to Davenport

It is rather a pity that Stickelberger already proved what took us so many hours.

Feb. 17^{th}, 1934

My dear Harold,

It is rather a pity that Stickelberger already proved what took us so many hours. But it is better this has turned up now than later. So far as I can see, Stickelberger's proof is exactly on the lines of my first unsuccessful attempts, namely Π–power development of

$$\tau(\chi) = \sum_{\xi} \xi^{-1}(1-\Pi)^{Sp_{\mathfrak{p}}(\xi)} .$$

Stickelberger succeeds in getting not only at the exact power of Π but also at its coefficient, by a very cumbersome study of congruence properties of binomial and polynomial coefficients. From my point of view, our proof is by far simpler and easier to understand. But I suppose we had better not publish[ed] the paper in its present form now, even not with the necessary historical corrections.

I have looked through it, however, and found it O.K. except for a few remarks and for the last §. Here you have missed my point indeed. By writing

$$\left(\tau(\chi)^{p-1}\right) = \prod_{\mathfrak{p}|p} \mathfrak{p}^{q(\alpha(\chi,\,\mathfrak{p}))}$$

one states the result in an incomplete form. For one does not say what is the connexion between the $\alpha(\chi,\,\mathfrak{p})$ for fixed χ and different $\mathfrak{p}$. The definition of $\alpha(\chi,\,\mathfrak{p})$, as you gave it, is of course alright, and also the formula as just written is an immediate consequence from what was proved before for a fixed $\mathfrak{p}$. But the "recipe" for getting at $\alpha(\chi,\,\mathfrak{p})$ is too vague. It consists only in prescribing: take an isomorphism of E_{p^r} into the residue classes mod $\mathfrak{p}$ for each $\mathfrak{p}/p$, etc. What I intended by my more detailed statement was to give the connexion between the different $\mathfrak{p}$'s and the different $\alpha(\chi,\,\mathfrak{p})$.

The $\mathfrak{p}$ arise from one of them, say $\mathfrak{p}_1$, by applying the automorphisms $\xi - \xi^c$ of k_{p^r-1}, where c runs through all prime residues mod. p^r-1. The sub–group $c = 1, p, \ldots, p^{r-1}$ leaves $\mathfrak{p}_1$ invariant. Let c run through a complete system of "Nebengruppen" with respect to this subgroup. Then the different $\mathfrak{p}$'s may be written as $\mathfrak{p}_c$, where $\mathfrak{p}_c$ arises from $\mathfrak{p}_1$ by $\xi - \xi^c$.

Now my supplementary statement was

$$\alpha(\chi, \mathfrak{p}_c) \equiv c^{-1}\alpha(\chi, \mathfrak{p}_1) \text{ mod. } p^r - 1\,,$$

hence more detailed

$$\Big(\tau(\chi)^{p-1}\Big) \;=\; \prod_c \mathfrak{p}_c^{q(c^{-1}\alpha(\chi,\,\mathfrak{p}_1))}\,,$$

where c runs through the residue system defined above. This form of the result is what one actually requires for the applications. It leads to the statement that the symbolic power

$$C^{\sum_c s_c q(c^{-1}\alpha)} \;=\; 1$$

for each α and each ideal class C in k_{p^r-1} containing a prime ideal of degree r. s_c denotes the automorphism $\xi \to \xi^c$. Relations of this type lead to criteria for Fermat's Last Theorem.

What I propose is to abandon the original plan of a "snappy" paper on Gaussian Sums, but to give our new and simpler proof of Stickelberger's result in our planned paper on $y^n = 1 - x^m$ and $y^p - y = x^m$ as an appendix. As to your first theorem, $-\tau(N_s\psi) = (-\tau(\psi))^{\frac{r}{s}}$, we can give your extremely nice elementary proof also as an appendix there, relating to the proof yielded by the more general theory.

Or do you think we ought to publish our "snappy" paper after all with the necessary historical revision ? I am retaining the Ms. here until I have your answer. For, in case you decide on publishing, I should like to rewrite the last § after the lines indicated.

It seems by the way as if the result is already due to Kummer (Crelle 44, quoted in Stickelberger). I have not been able yet to look at Kummer's paper, because it is not in our Seminar and "verliehen" from the University Library. Even Hensel does not possess either Crelle 44 or Kummer's Separatum of this paper. I presume Crelle 44 from the Library is in the hands of one of my students who lives in Mainz at present.

bad English, sorry !

Therefore it may last a considerable time before I can lay hands on this paper. Perhaps you could do it with less difficulties.

As to the other points in your letter, concerning the Ms., I will deal with them even if it is useless.

The alphabetical order of our names is the only possibility in question. There is no scale of importance !

Why not alter the letter s (divisor of r) into something else, say ϱ ?

Of course, everything is valid also for $p = 2$, even the argument with $\Pi = 1 - \mathsf{Z}\ (= 2)$.

There was a mistake in (4):

$$\tau(\chi) \;=\; r' \sum_{|F|=p^{r'}} \wedge_1(F)\,\Phi(F)\,.$$

You had omitted the factor r'. Further I have added a short remark concerning the uniqueness of E_{p^r}. This field is more than unique in the abstract sense of isomorphism. It is *normal*, i. e., two isomorphic representations contained in the same field are identical. I take $\wedge_1(1) = 0$; I hope, rightly. It should be mentioned. Instead of "We define norm and trace..." I find it better to say "Norm and trace are defined...".

That's about all.

Thanks very much for your kind advise concerning Göttingen. It goes without saying that I shall not take the position there, unless I can get absolute and definite full power to do what *I* like in every respect. At present it seems unlikely that anything will happen before next term. I heard from F.K. Schmidt that the Ministry has complied with Weyl's and Landau's Entlassungsgesuchen. Ignoring all steps already taken by the Faculty on my behalf the Ministry has asked from the Faculty the usual "list" with 3 names for each post. Landau's successor will presumably be "applied". Trefftz seems to be a favourite. Also Knopp would be welcomed by the Faculty. Please do *not* mention this to Courant because I am not authorized to give this information. F.K. Schmidt could get into difficulties for having things let out. The Faculty will of course pursue their first course as regards myself as successor to Weyl.

I very much hope that I shall have a quiet summer here before I am called upon the battle field. Otherwise I doubt whether I shall be able to think on

$f(x,\, y) = 0$ for higher genus before long. At present I develop all the details about the addition theorem and the Abelian functions in my lecture, for the field of all complex numbers. The most important point is that one must consider the field $k(x_1,\, y_1\,;\ldots;\, x_g,\, y_g)$ of the symmetric functions of g independent solutions. Whereas $k(x,\, y)$ has only a finite number of automorphisms for $g > 1$, that field has again a $2g$–dimensional translation group of automorphisms, given by the translations in the period–parallelotope of the Abelian integrals. What one has to expect, therefore, is the number N_g of all systems of g solutions (for finite k). Its main value is p^g. My method will lead to error term $|N_g - p^g|$. I think this will trivially settle the corresponding question for N itself. What about it ? (In N_g systems which arise by permutation are *not* counted as different). Since Clärle has written in detail about our plans for the coming vacation, I need not say anything to this point. We are looking forward to your answer.

I forgot to mention that I could not find your Theorem 1 ($-\tau(N,\, \psi) = -\tau(\psi)^{\frac{r}{s}}$) in Stickelberger. I think it[']s possible that you have the priority with it, though I would not like to have my hand burnt for it after the other discovery. Anyhow, this Theorem *alone* is not so important from my point of view as to justify a separate publication.

Kindest regards and very best wishes,

yours,

Helmut.

1.55 20.02.1934, Davenport to Hasse

On the proposed joint paper. Relations between Gaussian sums should be published separately.

Thanks for the review of the film about Henry VIII. The reviewer took it seriously, as also the English public did. It was intended to be a burlesque.

Tuesday 20.2.34.

My dear Helmut,

Very many thanks for your letter. I am sorry I did not appreciate your final §. I quite saw the object of it, but I did not realize that the final form of the prime ideal decomposition was really any less 'vague' than the previous form.

I cannot decide what is the right way in which these things ought to be published. I am not sure that I prefer our (or rather your) proof of the law of decomp. to Stickelberger's. S.'s is 'purer' in conception – that ought to appeal to you – it avoids the reference to: "$\sum a_i = \sum b_i$ and $a_i \geqq b_i$ implies $a_i = b_i$".

I should like a 'snappy' paper on relations between Gaussian sums, proof consisting in defining 'ad hoc'

$$\log L_\chi(s) = \sum_{\nu=1}^{\infty} \frac{p^{-\nu s}}{\nu} \sum_{\substack{\xi \\ \text{in } E_{p^\nu}}} \chi(\xi)\psi(1-\xi)$$

and

$$\log L(s) = \sum_{\nu=1}^{\infty} \frac{p^{-\nu s}}{\nu} \sum_{\substack{\xi \\ \text{in } E_{p^\nu}}} \psi(1-\xi^n) \qquad (n = \text{order of } \chi)$$

so that

$$L(s) = \prod_\chi L_\chi(s)$$

(I am writing from memory), and comparing coefficients. My reason is that the relations between Gaussian sums are 'concrete' relations which will be of interest to a large number of people who know nothing about algebra and alg. functions (like myself), and *a* proof ought to be published, readable to such people with a minimum effort. The existence of a great number of such people is deplorable, but is a fact.

I will look at Kummer's paper but probably shall not be able to understand it.

I spoke at Hardy's class today on abundant numbers. I was not at all nervous, in spite of having an audience which included Landau, Courant, + Hardy. Landau arrived here on Saturday for a fortnight.

Besicovitch has solved the problem: if a_i is a sequence of positive integers such that $a_i \nmid a_j$ if $i \neq j$, is the density of the sequence 0. The answer is in the negative. Erdos says he has proved that the *lower* density is 0.

Thanks for your news about G. I will keep it to myself. Trefftz I have never heard of. Knopp is not a mathematician of the same rank as Landau or Courant or Weyl. What I feel about G. is that if it is in the power of one man to restore the prestige of G., you are the man – but *perhaps* it is not within the power of one man.

If N_g is the no. of solutions *not* counting permutations separately, surely the principal term in N_g will be $\binom{p}{g}$ or something like it. It will be a very great triumph for you if you prove the R.H. in the general case.

Mother is here now for a few days.

I look forward to the vac., I hope we shall have a pleasant trip somewhere.

Don't overwork.

Yours ever,

Harold

1.56 22.02.1934, Hasse to Davenport

H. advocates their new proof of Stickelberger's results. Plan for a joint paper on Gaussian sums. Parallel to this plan for the joint paper in the Berlin Academy. H. expects D.'s visit next month.

22.2.34 My dear Harold,

I received your kind letter to–day. It would be rather nice, after all, to publish a paper about the Gaussian sums from a more elementary standpoint. When we include the relations, as you suggested, I think there is no harm in giving our proof of Stickelberger's (or Kummer's) theorem as well. You are quite right with your criticism of our proof in favour of the old. But on the other hand ours is more concise. Moreover the old proof and the whole matter seems to have slipped from the minds of our generation, presumably owing to Hilbert's inconceivable not giving it in his Zahlbericht. The projected paper would then consist of three things:

(1.) Your theorem on $\chi = N_\psi$

(2.) the new proof of Stickelberger's (or Kummer's) theorem

(3.) the relations

I should like, though, to deal shortly with all those things from the higher standpoint in our projected paper in the Berliner Akademie. You write nothing about my suggestion in this direction. Please let me know whether you agree with my suggestions in this and in my last letter.

I will post the Ms. and the concepts of mine concerning the decomposition and the relations to–morrow. Would you mind writing the new Ms.? We could discuss it then next month when you are here.

I am looking forward to that time with great pleasure. Please remember me to everybody that cares for me in Cambridge.

Very best wishes,

yours,

Helmut.

1.57 02.03.1934, Davenport to Hasse

Proposal: Two papers. Kummer. Landau. Travel plans for D.'s visit in Germany.

Cambridge, 2 March 1934.

My dear Helmut,

Very many thanks for your letter of the 22 Feb. and the improved MS and your own MSS. As regards our papers, I favour distributing the material as follows:

1) a paper on the relations only, in the Journal L.M.S. or the Oxford Q.J.

2) everything else in the paper in the Berlin Academy.

In 1) I would suggest doing nothing except proving the relations, by defining ad hoc the functions

$$\log L_\chi(s) = \sum_{\nu=1}^{\infty} \frac{p^{-\nu s}}{\nu} \sum_{\xi \text{ in } E_{p^\nu}} \chi(\xi)\psi(1-\xi),$$

$$\log L(s) = \sum_{\nu=1}^{\infty} \frac{p^{-\nu s}}{\nu} \sum_{\xi \text{ in } E_{p^\nu}} \psi(1-\xi^m).$$

We should of course refer in 1) to the fuller treatment of these functions from the general point of view in 2), but not actually do anything which is unnecessary for the proof of the relations. I suggest also giving in 1) the elementary verification that the exponents of the prime ideals in the decomposition of the various i's[1] add up to the right result, and indicate briefly how the value $\overline{\psi}^m(m)$ of the unit can be determined by congruence considerations.

If you approve, I will try to write 1) about the end of next week. This would be of course almost the same as the MS which I return herewith, but expressed entirely in elementary language.

[1]undeutlich

I have looked at Kummer's paper, and it seems to me that the prime ideal decomposition is there. Certainly the Galois field is there, and a good deal is said about the Gaussian sums in the G.F., all written out beautifully. Also the sums of the coefficients of an integer A expressed in the scale of p occurs, and he states explicitly the lemma that the power of p dividing $A!$ is $\frac{A-s}{p-1}$. But I did not recognise the final result in his language.

Of course the relations would also be referred to briefly in 2). I do not feel keen on publishing either of the two items composing the paper we have just abandoned. If we adopt the plan I suggest, of course the elementary proof of $\tau(\chi) = \big(\tau(\psi)\big)^{r/s}$ is a more trivial application of the method we should use in 1).

As regards Stickelberger's paper, there is a reference to it in the bibliography in Hilbert. Mordell says he drew our attention to it when you were compiling the references for your Bericht. Anyhow there is no disgrace in having been anticipated by Kummer!

Landau is here still. He spoke at Hardy's class on Tuesday, very well indeed. He makes a better impression on me now than when I saw him in Gottingen. He is an extraordinary personality. He dined in Hall last night, and afterwards Hardy told us the Littlewood murder case, which I must tell you about sometime.

I am very interested in some things arising out of Khintchine's work on Additive Zahlentheorie at the moment. I am also rewriting Erdos's paper on abundant numbers – or supposed to be doing. But as you will see, it is a long time since I used a typewriter.

I look forward very much indeed to seeing you again, and to making a trip southwards. I heard the other day a vague rumour of the possibility of the German tourist ban on Austria being extended to Italy, but I do not suppose there was anything in it. By the way, if we visit the Riviera, we should certainly want to visit the French side. But I suppose you could get a visum in San Remo. The German Ausreisevisum has been abolished.

Could you please pay my 5 RM subscription to the D.M.V. I am sure I do not owe them 10 Marks, for it cannot be more than a year since you payed 5 Marks on my behalf.

Let me know (through Cl., if you are busy) what you think about the rival attractions of the Lakes, the Riviera, and Venice. There seems to me to be three routes to the Riviera: Gothard-Milan-Genoa: Gothard-Turin-Cuneo: Geneva-Grenoble-Digne. The Riviera seems the most attractive possibility to me, provided we had a little time there when we got there. Of course there is no need to decide until I have arrived in Marburg.

Very best wishes, + love from

Harold

Don't overwork!

1.58 24.04.1934, Hasse to Davenport

It appears that D. had recently been in Marburg. H. reports about negotiations in Berlin. H. has obtained the assurance that Göttingen will keep the same number of positions as of 1929. Courant. H. had no time for mathematics. Plans for visiting Finland in September.

24. 4. 34

My dear Harold,

In addition to quite a few letters I had to write to–day, all more or less connected with the Göttingen business, I will spend a couple of decades of minutes writing to you.

First of all I am awfully sorry I am no longer able to speak to you personally: it would be much simpler, much more convenient, and much more interesting, too. I wonder, though, if my own degree of regret for that being impossible reaches your's for not being in the Weissenburgstr. any longer. My negotiations (I am rather astonished at your queer spelling "negociations" which is not allowed by the Oxford standard) in Berlin were quite satisfactory. As to personal terms I have reached all I could reasonably expect, being now only by ε below the absolute maximum in the 3^{rd} Realm, though presumably considerably below it with regard to the period between the 2^{nd} and 3^{rd} Realm. I hope we shall be able to realize our pet idea of owning a nice car in due course. Further I was able to "secure" this by getting the promise of new negotiations about the personal terms, should the Civil Service salaries be cut by further laws.

As to the Institute, the Ministry appeared to be particularly keen on rebuilding Göttingen as it was. They promised at once and explicitly to have the professorships of Landau and Bernstein succeeded with all possible expediency. There are still some points about the number of regular and irregular assistants and employees of the Institute to be cleared. My own information from F.K. Schmidt and Weber does not agree with what the Ministry had in its lists. But those points will undoubtably be cleared up to my satisfaction, i. e. , in such a way that the Göttingen Institute does not lose a living soul nor a shining penny either from what it had in 1929 when the Institute was inaugurated. My position is rather strong at present, so long as I have not subscribed, and I am going to make every possible advantage out of it, before it is too late.

I also touched the rather intricate question of Courant's position. I have written a letter to him about this. They will on no account grant him the long leave asked for, but will insist upon a quick definite solution. With other words, they will put him before the alternative of taking up his lectures, as he is bound to do by his position, and so inflicting a new opposition by the students upon himself, or renouncing, in which case they will promise him a financial compensation. They seem to hope that Courant will take the latter decision, so to say by sound reason. I personally doubt that the matter can be solved this way. Nobody in Berlin and Göttingen seems to think it possible that Courant will be able to take up his lectures at Göttingen again on account of the very strong opposition to him from all quarters. The intention of refusing a long leave and having done with the whole question before long was pronounced by the Minister himself, though not in my presence.

Of course there was no time available for mathematical work. I hope I will have some time soon, on account of the usual delay in getting my "Ernennung" and the inexpediency of taking up lectures here for the term to come.

As to Finnland, I secured my trip there for the end of September with the only difference, that now the Prussian Minister will pay for it, instead of Finnland which will pay for another Marburg professor.

Kindest regards and very best wishes,

yours,
Helmut

1.59 01.05.1934, Davenport to Hasse

Comments to H.'s introduction to joint paper. D.'s proof of **functional equation** *for L-functions not yet finished. D. cannot understand the language in Bieberbach's article.*

Trin. Coll. 1 May 1934.

My dear Helmut,

Very many thanks for your post-card and the Introduction to our joint paper. Here are my remarks (they are none of them points to which I attach real importance, and I might very well change my mind if I could hear your opinion):

1) It would seem to me preferable not to mention the prime ideal decomposition of $\tau(\chi)$ until *im zweiten Falle* has been given. As it is, it seems to intrude unnecessarily into the flow of thought. Also the prime ideal decomposition is relevant to the τ's and π's equally.

2) "Wir werden beilaufig einen Beweis... bringen." I should make it even clearer that it is "beilaufig" by putting the section of the paper in which it is done at the end of the paper perhaps as "Anhang" – not that I think it the least important thing in the paper.

3) (Last sentence on p. 4.) I had understood that the proof of the relations by Stickelberger + congruence would come in the other paper (our joint paper on the relations, which I am supposed to be writing).

4) (Line after equation (4)) I do not see why is at all relevant. The result is solely a consequence of

$$\sum_x e(cx) = \begin{cases} 0 & c \neq 0, \\ q & c = 0. \end{cases}$$

5) The proof of $\pi \cdots = \frac{\tau \cdots \tau \cdots}{\tau \cdots}$ (which I have marked in pencil (A) might

be given a little more shortly as follows):

$$\begin{aligned}
\tau(\chi)\tau(\psi) &= \sum_x \sum_y \chi(x)\psi(y)e(x+y) \\
&= \sum_z e(z) \sum_{x+y=z} \chi(x)\psi(y) \\
&= \sum_z e(z)\chi\psi(z) \sum_{x+y=1} \chi(x)\psi(y) \\
&= \tau(\chi\psi)\pi(\chi,\psi).
\end{aligned}$$

6) I am glad to note that you have only defined the generalised Gaussian sums for non-principal characters. This is in accordance with my own practice. Note (though the point is of no importance) that (5) presupposes $(m,n)=1$, whereas (9) presupposes only $m \nmid n$.

7) [Footnote 2] I like to have $\chi(0)=0$ for all χ, as otherwise the usual properties of characters do not hold, but perhaps the question will not arise.

All these silly trivial criticisms must not in any way obscure the expression of my view that this "Einleitung" is very nicely written indeed. [Excuse the bad style: perhaps it is the result of trying to translate Bieberbach's lecture, which becomes more obscure and nonsensical every time I read it. What does "So etwas ist geistig bestimmt" mean?]

I have not got my proof of the functional equation into suitable form for writing up yet. It is simply elementary algebra, and that is something I never was good at.

I hope the paper I forward is what you wanted.

Very best wishes, Yours,

Harold.

1.60 02.05.1934, Hasse to Davenport

Report on negotiations in Berlin. H. cannot do more for Courant. Would D. join H. an the way to Finland? Plans for trip to England in August. On Bieberbach's article. (Hecke has written something on this.) H. awaits D.'s proof of the **functional equation** *for L-functions. Is D. able to treat exponential sums too? Would D. be able to obtgain examples for $A \neq 0$ in elliptic function fields?*

2.5.34

My dear Harold,

Although I suppose there is another letter from you on its way to me, I will answer the one I have got the other day.

My negotiations with Berlin have not proceeded yet. It seems as if the change in the administration of our Kultusministerium has been delaying all matters running. Two days ago the Prussian Minister für Wissenschaft, Kunst und Volksbildung, Herr Rust, was appointed the Reichsminister für Wissenschaft, Erziehung und Volksbildung. Nobody knows yet what this change is going to bring about. I wonder whether the universities will be administered by the Reich now, or things will be essentially the same as before, only that the Reich will ex[c]ercise a sort of control over the Education Boards of the single Länder.

I suppose I did not yet mention the principal moot point in my negotiations. You will have heard about the Dozentenschaften. These are the new bodies representing all university Dozenten except the ordentliche Professoren. They include in particular all assistants, even if they are not Dozenten. The Dozentenschaft has a Führer who is appointed by the Rector. The Rector himself is superior to this Führer, and so indirectly superior also to the members of the Dozentenschaft. The question is now whether the Rector is authorized to co–operating in the appointment of an asistant. Formerly the appointment of an assistant was in the hands of the Ministry. It was given on the proposal of the Institutsdirektor, the Institutes being subordinated to the Ministry and not to the University. In Marburg no change in this has come about, whereas in Göttingen the Rector holds that he is entitled to co–operation in the appointment of assistants, because the assistants once appointed belong to the Dozentenschaft and are subordinated to him as members of this body. What all this means in my case is much more obvious than the theoretical administrative question: the appointment of assistants would be controlled by the Dozentenschaft, i. e., by a body whose main task is political. You will understand that I am not willing to submit myself to this regulation, the more so as I know that it must not necessarily be so, as for instance in Marburg. I have made no mistake about my opinion about his point in Berlin, and I was greatly encouraged not to give in by the Marburg Universitätskurator (the representative of the Ministry at our University).

I had a letter from Courant the other day. I am sorry I cannot do more for him.

It would be exceedingly nice to have you coming with us to Finland. What nonsense about your company being “irksome” to us !

Claerle and I had a long talk about our projected trip to England in August. From our trip two years ago we know, how expensive motoring about in the county and staying at a new hotel every night would be. What with all the expenses of our moving to Göttingen and settling down in a new flat, I do not think we could manage that for a couple of weeks. On the other hand, staying most of the time at Cambridge would not be the right thing for many reasons. What would you think upon our settling down at some nice place, after the fashion of our stay in Nice, and touring about from there ? We could of course first stay for a couple of days at Cambridge. Can you think of a lovely place to go to then ? And what do you think the pension will be ? I will on no account have you pay the whole lot on account, as it was the last time.

I have not seen Bieberbach’s lecture nor the account of it yet, but I have heard a great deal of deprecatory comment on it by German mathematicians, and no approving comment at all so far. Hecke, for example, wrote me the other day, he hoped there would soon be an opportunity to show that Bieberbach does *not* represent the opinion of the D. M. V.

I agree entirely with you on Mahler. He is certainly a very clever mathematician, though.

I am looking forward to your proof of the functional equation of the L–functions arising from character sums. I hope you will succeed in mastering the exponential sums, too. I wonder, whether the method can be carried through for cyclic equations over arbitrary algebraic function fields, or is by its nature restricted to the rational function field.

Can you prove, that there always are g_2, g_3 such that the error term is *not* 0 or $\pm 2\sqrt{q}$, and that for $p \geq 13$ there are always $g_2 \neq 0$, $g_3 \neq 0$ for which the error term is 0 or $\pm 2\sqrt{q}$? I should like to have these statements, in particular the latter, which means that my invariant modular form A is not identically 0. I cannot prove this directly, by studying the coefficients in the power series of $\frac{1}{y}\frac{\mathrm{d}x}{\mathrm{d}t}$ in $t = \frac{-2x}{y}$, A being the coefficient of t^{p-1}. What I can prove is only, that apart from the trivial cases $g_2 = 0$, $g_3 = 0$ there are *at most* $\frac{p-k}{12}$ values of $J = \frac{g_2^3}{g_2^3 - 27g_3^2}$ for which $A = 0$, k being the least residue of p mod. 12.

Nb. $A = 0$ is equivalent with: error term $= 0$ or $\pm 2\sqrt{q}$.

The very best wishes from

yours,

Helmut

1.61 04.05.1934, Hasse to Davenport

H. gives detailed replies to D.'s comments to H.'s introduction. The new proof of Stickelberger's theorem should perhaps go into the "low brow" paper. H. has written to Bieberbach concerning his paper which had aroused big protest. Bieberbach has sent the original Ms.– H. cannot agree with it. But H. has often thought about the matter except the Aryan/non-Aryan side of it. Explanation of details. – H. had a letter from Donald.

4.5.34

My dear Harold,

Your letter of May 1^{st} turned up yesterday noon as I expected. There was no need of sending the Einleitung back, as I had kept 3 more copies here. As it is, I do not think it necessary to send it back to you once more. You will certainly remember what's what in my ensuing comment on your criticism. I deal first with your points 1) – 7).

1) I quite agree that the prime ideal decomposition of $\tau(\chi^{\mu})$ had better be mentioned at a later place, after the introduction of the zeros $\pi(\chi^{\mu},\ \psi^{\nu})$ for case (2.). — See, however, **3)** to this point.

2) It would be all the same to me emphasizing the "beiläufig" by putting the new proof of Stickelberger's prime ideal decomposition theorem as an "Anhang" at the end. — See, however, **3)** to this point.

3) Now you have mentioned it, I remember our original intention of bringing the arithmetical proof of the relations between generalized Gaussian sums in our "low–browed" paper. — It fits there much better, indeed.

But then perhaps the new proof of Stickelberger's theorem mentioned in **1)** und **2)** above had also better be given in that "low–browed" paper, as an appendix if you prefer this. For in the "high–browed" paper no real application of Stickelberger's theorem is to be made, whereas the arithmetical proof of the relations between generalized Gaussian sums, which shall be given in the "low–browed" paper, bases on Stickelberger's theorem. In this case the “high–browed” paper would contain only a short reference to Stickelberger’s result and a few remarks to the effect that the roots $\tau(\chi^\mu)$ and $\pi(\chi^\mu,\, \psi^\nu)$ are arithmetically characterized by their Stickelberger prime ideal decomposition and certain congruence properties. I still have to work that out more clearly. Please let me know with your next letter whether you agree with this arrangement. In that case I do not need the copy of yours you sent me on request, because then *you* are supposed to write the paragraph in question.

4) Of course $\sum_c \chi^\mu(c) = 0$ is irrelevant for $\sum_c \chi^\mu(c) \sum_b e^k(b(c-1)) = q$. One only needs $\sum_b e^k(b\,c) = \left\{ \begin{array}{ll} q & \text{for } c = 0 \\ 0 & \text{for } c \neq 0 \end{array} \right\}$; this again follows from $\sum_b e^k(b) = 0$.

5) Thanks very much for your considerably simpler version of the proof for

$$\pi(\chi^\mu, \psi^\nu) \;=\; \frac{\tau(\chi^\mu)\tau(\psi^\nu)}{\tau(\chi^\mu\psi^\nu)}\,.$$

6) and 7) I restricted myself on non–principal characters in the sums τ and π in order to avoid the silly question what is the “right” value of the principal character for 0. Since my further developments shall be such that this question will not arise, I would have no objection to extending the definition $\chi^\mu(0) = 0$ in the footnote in question also to $\mu = 0$. Perhaps the best thing to do is not to mention $\chi^\mu \neq 0$ at all (neither that $\mu = 0$ is included). — I do not understand why you think that in $-\sum_a \chi^\mu(1-a)\,\psi^\nu(a) = \pi(\chi^\mu,\, \psi^\nu)$, $\left(\begin{array}{c} \chi^\mu \neq 1, \quad \psi^\nu \neq 1 \\ \chi^\mu\psi^\nu \neq 1 \end{array} \right)$ the condition $(m,\, n) = 1$ is implicitly presupposed. As I see it, the conditions in brackets state quite clearly that all and only those exponents $\mu,\, \nu$ are allowed, for which neither of the 3 characters is the principal character. Such $\mu,\, \nu$ exist also when $(m,\, n) > 1$; only in the trivial case $m = 2,\ n = 2$ no such $\mu,\, \nu$ exist (in this case $\zeta_Z(s)$ has *no* zeros, the genus of Z is 0). Perhaps I should have been a little more explicit here, and should have mentioned that exceptional case for the sake of clearness. Similarly, for the relations $\prod_{\nu=0}^{n-1} \tau(\chi\,\psi^\nu) = \chi^n(n)\tau(\chi^n) \prod_{\nu=1}^{n-1} \tau(\psi^\nu)$ the trivial case $m \mid n$ ought to be excluded explicitly. I did not do that, because I thought it was not necessary in the Einleitung, and could be mentioned later. But now it seems better to me to strive for absolute exactness already in the Einleitung.

There are two more points you raised by remarks on the margin of p.1.

a) I wrote "p–Potenz q" in order to spare a notation $q = p^r$ which can be avoided through the whole paper. I have not introduced a letter for $\log_p q$ in all my other papers, because I did not need it. I should prefer holding the same line here. But if you think it better to introduce a letter for $\log_p q$, I should not strongly object. Only let us avoid the letter "r", because this is always used for the relative degree ($k^{(r)}$ of q^r elements) in my other papers, and is going to be used in the same sense here. Perhaps $q = p^\ell$ would do.

b) You put "oBdA" to the conditions $m \mid q-1$ and $n \mid q-1$. This is alright so far the *character sums* are concerned. But I do not see quite clearly what "oBdA" means for the *field* Z. I must consider this point.

I shall re–write the Einleitung according to all this when I have got your answer, in particular to point **3)** above, and shall carry on with the main part then.

I wrote Bieberbach about the comment roused by his lecture. He denotes the account in the Deutsche Zukunft tainted and biased. He sent me the Ms. proper; have you seen the lecture itself or only the account in the D.Z. ? I have read the lecture itself. I cannot say that I agree with it, neither with the whole trend nor with the single arguments. You know, though, that I have often thought about the matter myself, except the Aryan/non–Aryan side of it. I mean, I also hold that mathematics of different nationalities have different peculiar traits. My own experience is chiefly based on the manifestations of this in English, American, and German mathematics. Where I do *not* agree with Bieberbach is that race and blood are the essential factors. I think the whole thing is more a question of surroundings and upbringing, not only mathematical surroundings and upbringing, but also the whole complex of all things that are taught to be valuable and mattering. One of your favourite standards, for example, is the question of "pounds, shilling and pence" in politics. That is a part of your good old English "common–sense". It seems to me somehow connected with your preference of questions of "magnitude" in mathematics, though the connexion may be dim and subconscious. Whereas my preference of "structure" in mathematics seems in the same way connected with my trend to introduce irrational notions as "nationalism", "race", "honour" in politics. You will agree that those differences of opinion of ours, both in mathematics and politics, are somehow typical for our countries, every exception granted of course. I hope I have not expressed myself too vaguely. We had a long letter from Donald yesterday. He is really a very nice chap. One simply must like him for his letters, let alone his amiable personality.

Kindest regards and best wishes,

yours,
Helmut.

Many good wishes from Clärle.

1.62 12.05.1934, Davenport to Hasse

The new proof of Stickelberger's results. It has no definite function in either paper.

T.C.C. Sat. 12.5.34.

My dear Helmut,

Very many thanks for your letters of the 2nd and 4th., and humble apologies for not replying sooner. I have not been idle during this period, but have devoted a good deal of energy to some problems in analytic numbertheory and in "Kombinatorik", but without any results to show for it. Rado and I are reading van der W. together – when we do not digress from it to something more amusing.

About the Einleitung. I really have no feelings as to which paper the new proof of Stickelberger should come in. It has no definite function in either paper. Would you consider making a separate note of it? – in that case under your name alone? Each of the two papers we are writing is genuinely new (at least, we hope so), whereas the value of the proof of S. lies in its intructiveness and general interest, rather than novelty. But whatever plan you prefer will suit me.

I apologise for my obtuseness in thinking $(m, n) = 1$. Of course it is irrelevant.

My objection to "p-Potenz q" was rather to the phrase, which seemed strange to me. But it is of no importance.

□□□

(above is nonsense – I must suspend judgement!)

I have been distracted from work in the last few days by several things. Donald came on Thursday, to the Feast of the Ascension, and I took him back to town yesterday afternoon. (During the morning we punted on the river – extremely pleasant in this weather). Then I went out to Harrow to see Mother and Father, while Donald went to an important meeting of the Royal Astron. Soc.

Sunday evening

I had just finished the preceding sheet when I was interrupted by another visit – from Page, a contemporary of mine at Manchester, now lecturer at a London teachers' training College. He has written a few papers on the theory of numbers. He stayed until to-day, and now is my first opportunity to finish this letter.

I have only seen the report of Bieberbach's lecture in the D.Z. It may be distorted, but I take it it is correct in principle. Of course, *your* views on the effect of national characteristics on mathematical outlook are very reasonable. I should say, though, simply that there exist different mathematical outlooks, each with its own contribution to make, and that the flourishing of particular ones in particular countries is the result of a combination of circumstances – national tendencies one of them, but the presence of the right teacher at the right time a more important one still.

Bieberbach's lecture, like many German utterances at the moment, is ridiculous because of its crudity: Jew: black, German: white, rest of universe ignored.

I cannot think of anything more to write at the moment, though I am sure there is a lot which I should remember if we were conversing together. Sorry all is not settled about G. Look forward to having you here in Aug. Very best wishes,

Yours, H.

1.63 15.05.1934, Hasse to Davenport

H. has started to write the joint paper. Remarks: high-brow versus low-brow paper. H. will do the Appendix containing Stickelberger's proof. H. will cpmplete the "cyclic paper" in a few days. – Schneider's theorem.

MARBURG–LAHN, DEN
15. 5. 34

My dear Harold,

Just a few remarks to our joint paper which I have begun to write on the arrival of your letter to–day.

From my point of view, the two relations

$$\tau(\chi_r) = \tau(\chi)^r \quad \text{for} \quad \chi_r = \chi\Big(N_r(a_r)\Big), \ (a_r \text{ in } k^{(r)}) \tag{1}$$

$$\prod_\nu \tau(\chi\psi^\nu) = \chi^n(n)\tau(\chi^n)\prod_\nu \tau(\psi^\nu) \tag{2}$$

are so much the same with respect to the source they are coming forth from, that I should not like to give the second a preference before the first. I propose to treat them both as co–ordinated, in *both* the high–browed and the low–browed paper. Therefore I *implore* you to drop all uneasiness about the first being less interesting and "perhaps already done by somebody else" (as you wrote me some time ago) and to give in to my systematical reasons by giving it its proper place in the low–browed paper to be written by you.

The same craze for "systematicity" (or "systematicalness") induces me to give the arithmetical verification (Stickelberger + congruences) for *both* (1) and (2). The only question is, whether we ought to do this in the low–browed or in the high–browed paper. I rather incline to do it in the *latter*, because I am going to give the "arithmetische Charakterisierung" of the $\tau(\chi)$ and $\pi(\chi, \psi)$ *there*, and that arithmetical verification is very much on the same line. I should of course agree, though, if you felt it rather belonged to the low–browed paper, in particular if you thought this arithmetical verification would be a desirable matter to fill this l.–b. paper up with. I shall work it out anyhow and provide, for the time being, its being inserted in the h.–b. paper.

I will take the "Anhang", giving Stickelbergers proof, to the h.–b. paper. I cannot agree with you — being a *German* mathematician — that this new *proof* is devaluated by the discovery that Stickelberger proved the *result* years ago. Since *you* feel this way, the proper conclusion is: it ought to be published in Germany.

By the way, I was right after all with my reference to $\sum_c \chi(c) = 0$ as necessary for $|\tau(\chi)| = \sqrt{q}$. For, $|\tau(\chi)|^2 = \sum_a \sum_{b\neq 0} \chi(\frac{a}{b})\, e(a-b) = \sum_c \chi(c) \sum_{b\neq 0} \chi(b(c-1)) = \sum_c \chi(c) \sum_b \chi(b(c-1)) = \cdots$, and for the step from "$b \neq 0$" to "just b" one requires the relation in question. Or can you avoid it by a better arrrangement ? I should see no point in this at all, though.

$m \mid q-1$ is not "oBdA", at any rate not in the sense you presumably implied, that replacing an arbitrary m by $m_0 = (m,\, q-1)$ gives essentially the same field Z_0 as Z. On the contrary, Z_0 and Z have in general even not the same genus ! Of course one can understand "oBdA" in that sense, that there is no harm in presupposing the field k so large from the beginning that $m \mid q-1$. But I had rather no "oBdA" at all in the introduction. Simply presupposing $m \mid q-1$ and $n \mid q-1$ will do. Later on, when it comes to applications on congruences and character sums, such as $ax^m + by^n + c \equiv 0$ mod. p, one can put in a remark that $m \mid q-1$, $n \mid q-1$ is "oBdA".

A pupil of Siegel, named Schneider, has proved: Of three complex numbers

$$a \neq 0\,,1\,; \quad b \text{ irrational}\,; \quad a^b$$

at least one is transcendental. Is that not extremely nice, indeed ?

Also Nevanlinna thought Landau a rather poor mathematician every allowance for his technical ability and criticisms duty made.

Thanks for your remarks on mine on Bieberbach's lecture. I do not think you will desire to have a look at the original, though I could send you a copy now.

I shall write the "cyclic paper" within the next days and send you a copy then, also a copy of another note I have written recently on unramified separable cyclic fields over an elliptic function field (part of the details for my great proof).

Very best wishes,

yours,
Helmut

1.64 20.05.1934, Hasse to Davenport

H. has been busy writing the joint paper. He writes details about the proof of Stickelberger's result. One point he wishes to discuss with D. Next Thursday H. will go to Berlin for further negotiations.

MARBURG–LAHN, DEN
20. V. 34

My dear Harold,

I have been busy writing our paper. All went well, except one point which I will lay before you. This point concerns the arithmetical proof of the relations:

$$\begin{array}{llcll} \textbf{(I)} & \tau(\chi_r) & = & \tau(\chi)^r & \text{for } \ \chi_r(a_r) = \chi(N_r(a_r)) \ \ (a_r \text{ in } \ k^{(r)})\,, \\ \textbf{(II)} & \prod_{\mu=0}^{m-1} \frac{\tau(\chi^\mu \psi)}{\tau(\chi^\mu)\tau(\psi)} & = & \frac{\tau_m(\psi^m)}{\tau(\psi)^m} & \text{for } \ \chi^m = 1 \ \ \text{(principal character)}. \end{array}$$

Notations:

$$\begin{array}{rcl} \tau_k(\chi) & = & -\sum_a \chi(a)\, e^k(a) \ \ (k = 1, \ldots, p-1)\ , \ e(a) = e^{\frac{2\pi i}{p}\gamma(a)} \ \ (a \text{ in } k\,) \\ \tau(\chi) & = & \tau_1(\chi) \\ \tau(1) & = & 1\,. \end{array}$$

Since $|\tau_k(\chi)| = \sqrt{q}$ for $\chi \neq 1$, those relations hold for the absolute values. From Stickelberger's result, they hold for the prime ideals occurring in the τ's. Hence the quotient of both sides in (I) and (II) is an algebraic unit with all its conjugates of absolute value 1, i. e. a root of unity. This root of unity is invariant under the automorphisms $\mathsf{Z} \to \mathsf{Z}^k$ of the field of the p^{th} root of unity $\mathsf{Z} = e^{\frac{2\pi i}{p}}$. It belongs therefore to the field of the $(q-1)^{th}$ roots of unity (which, at all events, contains the characters χ, ψ), and is therefore a $\left\{ \begin{array}{ll} 2(q-1)^{th} & \text{root of unity for } p = 2 \\ (q-1)^{th} & \text{" \ " \ " \ "} \ p \neq 2 \end{array} \right\}$. Suppose $p \neq 2$. Since the $(q-1)^{th}$ roots of unity are incongruent for any prime ideal $\mathfrak{P}/p$, it suffices to prove that that root of unity is $\equiv 1 \bmod \mathfrak{P}$, in order to show that it is $= 1$. (For $p = 2$ one has to prove $\equiv 1 \bmod 2\mathfrak{P}$. I will not bother here about this case, though.)

Now reading Stickelberger's proof, I discovered that he proves exactly what is necessary to complete the arithmetical proof of the relations (I) and (II) on the lines indicated above. For he not only determines the exact power of $\mathfrak{P}$ contained in $\tau(\chi)$, but also gives the congruence value for the next higher power of $\mathfrak{P}$.

I found this proof very nice indeed, and much simpler than I expected from my first scanning of St.'s paper. I have adapted this proof to our notations and simplified it a little. Here goes:

$\tau(\chi) = -\sum_\zeta \zeta^{-\alpha} \mathsf{Z}^{\gamma_\mathfrak{p}(\zeta)}$, where ζ runs through all $(q-1)^{th}$ roots of unity and $\gamma_\mathfrak{p}(\zeta)$ is the rational residue of $\zeta + \zeta^p + \cdots + \zeta^{p^{f-1}}$ mod. $\mathfrak{p}$; $\mathfrak{p}$ a prime ideal dividing p in the field of the $(q-1)^{th}$ roots of unity ($q = p^f$), and α the exponent of χ with respect to $\mathfrak{p}$.

W.l.o.g.

$$0 \le \alpha < p^f - 1$$
$$\alpha = \alpha_0 + \alpha_1 p + \cdots \cdots + \alpha_{f-1} p^{f-1}, \qquad \left\{ \begin{array}{c} 0 \le \alpha_i \le p-1 \\ \text{not all } \alpha_i = p-1 \end{array} \right\}.$$

Let

$\mathsf{Z} = 1 + \Pi$, $\Pi^{p-1} \cong p$, Π the prime divisor of p in the field of Z,

and

$\mathfrak{P} = (\mathfrak{p}, \Pi)$, $\mathfrak{P}^{p-1} = \mathfrak{p}$, $\mathfrak{P}$ the prime divisor of $\mathfrak{p}$ in the field of the $(q-1)p^{th}$ roots of unity.

Since $\mathfrak{p}$ is of degree 1 in the field of all symmetrical functions of $\zeta, \zeta^p, \ldots, \zeta^{p^{f-1}}$ (Hilbert's Zerlegungskörper for $\mathfrak{p}$), the expression $\zeta + \zeta^p + \cdots + \zeta^{p^{f-1}}$ is congruent to a rational number for every power of $\mathfrak{p}$, and a fortiori for every power of $\mathfrak{P}$. Let $\gamma_\mathfrak{p}(\zeta)$ be the rational residue of $\zeta + \zeta^p + \cdots + \zeta^{p^{f-1}}$ mod. $\mathfrak{P}^{s(\alpha)+1}$ where

$$s(\alpha) \;=\; \alpha_0 + \alpha_1 + \cdots + \alpha_{f-1}.$$

Now

$$\mathsf{Z}^{\gamma_\mathfrak{p}(\zeta)} = (1+\Pi)^{\gamma_\mathfrak{p}(\zeta)} = \sum_{k=0}^{\gamma_\mathfrak{p}(\zeta)} \binom{\gamma_\mathfrak{p}(\zeta)}{k} \Pi^k =$$

$$= \sum_{k=0}^{\infty} \binom{\gamma_{\mathfrak{p}}(\zeta)}{k} \Pi^k \equiv \sum_{k=0}^{s(\alpha)} \binom{\gamma_{\mathfrak{p}}(\zeta)}{k} \Pi^k \quad \text{mod. } \mathfrak{P}^{s(\alpha)+1}$$

$$\equiv \sum_{k=0}^{s(\alpha)} \gamma_{\mathfrak{p}}(\zeta)\Big(\gamma_{\mathfrak{p}}(\zeta)-1\Big)\cdots\Big(\gamma_{\mathfrak{p}}(\zeta)-(k-1)\Big)\,\frac{\Pi^k}{k!} \quad \text{mod. } \mathfrak{P}^{s(\alpha)+1}.$$

<u>**Lemma.**</u> $\frac{\Pi^k}{k!}$ *is integral for* Π, *namely containing* Π^{s_k} *as the exact power of* Π.

s_k denotes the "Quersumme" of the p–adic representation of k.

Proof. One knows that $k!$ contains exactly $p^{\frac{k-s_k}{p-1}} \cong \Pi^{k-s_k}$. Hence, in the last congruence, $\gamma_{\mathfrak{p}}(\zeta)$ may be replaced by any expression congruent to it mod. $\mathfrak{P}^{s(\alpha)+1}$; we replace it by $\zeta+\zeta^p+\cdots+\zeta^{p^{f-1}}$ which fulfills this condition by the definition of $\gamma_{\mathfrak{p}}(\zeta)$. One has then:

$$\mathsf{Z}^{\gamma_{\mathfrak{p}}(\zeta)} \equiv \sum_{k=0}^{s(\alpha)} \binom{\zeta+\zeta^p+\cdots+\zeta^{p^{f-1}}}{k} \Pi^k \text{ mod. } \mathfrak{P}^{s(\alpha)+1},$$

where $\binom{\omega}{k}$ for any ω is defined by $\frac{\omega(\omega-1)\cdots(\omega-(k-1))}{k!}$. This polynomial has the well–known property

$$\binom{\omega_0+\cdots+\omega_{f-1}}{k} = \sum_{\substack{k_0+\cdots+k_{f-1}=k \\ k_i\geq 0}} \binom{\omega_0}{k_0}\cdots\binom{\omega_{f-1}}{k_{f-1}}.$$

Hence

$$\mathsf{Z}^{\gamma_{\mathfrak{p}}(\zeta)} \equiv \sum_{\substack{k_0+\cdots+k_{f-1}\leq s(\alpha) \\ k_i\geq 0}} \binom{\zeta}{k_0}\binom{\zeta^p}{k_1}\cdots\binom{\zeta^{p^{f-1}}}{k_{f-1}} \Pi^{k_0+\cdots+k_{f-1}} \text{ mod. } \mathfrak{P}^{s(\alpha)+1}$$

Let

$$\binom{\omega}{k} = \frac{1}{k!}\sum_{0\leq\nu\leq k} c_{k\nu}\omega^{\nu} \qquad c_{kk}=1\,, \quad (c_{k0}=0 \text{ für } k\geq 1\,)$$

(the c's are integers).

Then

$$\mathsf{Z}^{\gamma_{\mathfrak{p}}(\zeta)} \equiv \sum_{\substack{k_0+\cdots+k_{f-1}\le s(\alpha)\\ 0\le\nu_i\le k_i}} c_{k_0\nu_0}\cdots c_{k_{f-1}\nu_{f-1}}\zeta^{\nu_0+\nu_1 p+\cdots+\nu_{f-1}p^{f-1}}\cdot$$

$$\cdot\frac{\Pi^{k_0+\cdots+k_{f-1}}}{k_0!\cdots k_{f-1}!} \text{ mod. } \mathfrak{P}^{s(\alpha)+1}$$

$$\tau(\chi) \;=\; -\sum_{\zeta}\zeta^{-\alpha}\mathsf{Z}^{\gamma_{\mathfrak{p}}(\zeta)} \equiv -\sum_{\substack{k_0+\cdots+k_{f-1}\le s(\alpha)\\ 0\le\nu_i\le k_i}} c_{k_0\nu_0}\cdots c_{k_{f-1}\nu_{f-1}}\cdot$$

$$\cdot\sum_{\zeta}\zeta^{(\nu_0-\alpha_0)+(\nu_1-\alpha_1)p+\cdots+(\nu_{f-1}-\alpha_{f-1})p^{f-1}}\frac{\Pi^{k_0+\cdots+k_{f-1}}}{k_0!\cdots k_{f-1}!} \text{ mod. } \mathfrak{P}^{s(\alpha)+1}$$

Here $\sum_{\zeta}=0$, except for

$$(*)\qquad (\nu_0-\alpha_0)+(\nu_1-\alpha_1)p+\cdots+(\nu_{f-1}-\alpha_{f-1})p^{f-1}=\mu_0(p^f-1)$$

with μ_0 an integer, when it is p^f-1. With this last relation one has simultaneously

$$(\nu_{f-1}-\alpha_{f-1})+(\nu_0-\alpha_0)p+\cdots+(\nu_{f-2}-\alpha_{f-2})p^{f-1} \;=\; \mu_1(p^f-1) \quad \text{with } \mu_1 \text{ an integer}$$

$$\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots$$

$$(\nu_1-\alpha_1)+(\nu_2-\alpha_2)p+\cdots+(\nu_0-\alpha_0)p^{f-1} \;=\; \mu_{f-1}(p^f-1) \quad \text{with } \mu_{f-1} \text{ an integer}$$

Those f equations may be solved with respect to the $\nu_i-\alpha_i$:

$$\nu_i-\alpha_i \;=\; p\mu_{f-1-i}-\mu_{f-i} \qquad \text{(the indices mod. } f\text{)}.$$

From the inequalities for the α_i and from $\nu_i\ge 0$:

$$\mu_i(p^f-1) > -\Big((p-1)+(p-1)p+\cdots+(p-1)p^{f-1}\Big) = -(p^f-1)\,,$$

i. e.

$$\mu_i > -1\,,\qquad \mu_i\ge 0\,.$$

Further

$$\begin{aligned}\sum_i(\nu_i-\alpha_i)(1+p+\cdots+p^{f-1}) &= \sum_i\mu_i(p^f-1)\,,\\ \sum_i\nu_i-\sum_i\alpha_i &= (p-1)\sum_i\mu_i \;\geq\; 0\\ \sum_i k_i \;\geq\; \sum_i\nu_i &\geq \sum_i\alpha_i \;=\; s(\alpha)\,.\end{aligned}$$

Hence the only solution of the condition $(*)$ occurring in the multiple sum above is:

$$\text{all } \nu_i=k_i \quad \text{with } \sum k_i=\sum \alpha_i=s(\alpha)$$

$$\text{and with this } \sum \mu_i=0\,,\ \text{hence all } \mu_i=0\,,$$

$$\text{hence all } \nu_i=\alpha_i\,,\ \text{hence all } k_i=\alpha_i\,.$$

Therefore

$$\tau(\chi)\equiv-(p^f-1)c_{\alpha_0\alpha_0}\cdots c_{\alpha_{f-1}\alpha_{f-1}}\frac{\Pi^{\alpha_0+\cdots+\alpha_{f-1}}}{\alpha_0!\cdots\alpha_{f-1}!}\ \ \text{mod. } \mathfrak{P}^{s(\alpha)+1}$$

$$\boxed{\tau(\chi)\equiv\frac{\Pi^{s(\alpha)}}{\alpha_0!\cdots\alpha_{f-1}!}\ \ \text{mod. } \mathfrak{P}^{s(\alpha)+1}}$$

This is Stickelberger's proof and result. One can write the result in another form, noting that the above result on $k!$ may be extended to:

$$\frac{k!}{(-p)^{\frac{k-s_k}{p-1}}}\equiv k_0!\,k_1!\cdots k_{n-1}!\ \text{mod.}\,p$$

$$\text{for } k=k_0+k_1p+\cdots+k_{n-1}p^{n-1}$$
$$(0\leq k_i\leq p-1\,).$$
$$(s_k=k_0+k_1+\cdots+k_{n-1}\,).$$

Since

(0) $\dfrac{\Pi^{p-1}}{-p} \equiv 1 \text{ mod.}\,\Pi$ $\Bigg($ from $p = (1-\mathsf{Z}) \cdots (1-\mathsf{Z}^{p-1})$:

$$\begin{array}{rcl} 1-\mathsf{Z}^k & = & 1-(1+\Pi)^k \equiv -k\,\Pi \text{ mod.}\,\Pi^2 \\ \dfrac{1-\mathsf{Z}^k}{\Pi} & \equiv & -k \text{ mod.}\,\Pi \\ \dfrac{p}{\Pi^{p-1}} & \equiv & (-1)^{p-1}(p-1)! \equiv -1 \text{ mod.}\,\Pi\Bigg) \end{array}$$

one has

$$\frac{k!}{\Pi^{k-s_k}} \equiv k_0! \cdots k_{n-1}! \quad \text{mod.}\,\Pi\,,$$

hence, since α reduced mod. $p^f - 1$, i. e., $s(\alpha) = s_\alpha$:

$$\tau(\chi) \equiv \frac{\Pi^{s_\alpha}}{\frac{\alpha!}{\Pi^{\alpha - s_\alpha}}} \equiv \frac{\Pi^\alpha}{\alpha!} \quad \text{mod. } \mathfrak{P}^{s(\alpha)+1}.$$

If α is not reduced mod. $p^f - 1$, one has

$\tau(\chi) \equiv \dfrac{\Pi^{\varrho(\alpha)}}{\varrho(\alpha)!}$ mod. $\mathfrak{P}^{s(\alpha)+1}$,	where $\varrho(\alpha)$ denotes the least non–negative residue of α mod. $p^f - 1$, and $s(\alpha)$ the p–adic Quersumme of $\varrho(\alpha)$.

Proof of the relations **(I)** and **(II)** on this line.

Relation **(I)**. Let χ have exponent α for $\mathfrak{p}$ and χ_r exponent α_r for $\mathfrak{p}_r$, a prime divisor of $\mathfrak{p}$ in the field of the $(q^r-1)^{th}$ roots of unity ($\mathfrak{p}$ splits into a product of *different* $\mathfrak{p}_r$). For a $(q^r-1)^{th}$ root ζ_r one has

$$\chi_r(\zeta_r) = \zeta_r^{-\alpha_r}.$$

Again by definition

$$\chi_r(\zeta_r) = \chi(\zeta_r^{\frac{q^r-1}{q-1}}) = \zeta_r^{-\frac{q^r-1}{q-1}\alpha}.$$

Hence

$$
\begin{array}{lcl}
\alpha_r & \equiv & \dfrac{q^r-1}{q-1}\,\alpha \mod. q^r-1 \\
& \equiv & \alpha_0+\alpha_1 p+\cdots+\alpha_{f-1}p^{f-1} \\
& + & \alpha_0 p^f+\alpha_1 p^{f+1}+\cdots+\alpha_{f-1}p^{f+(f-1)} \\
\multicolumn{3}{c}{\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots} \\
& + & \alpha_0 p^{(r-1)f}+\alpha_1 p^{(r-1)f+1}+\cdots+\alpha_{f-1}p^{(r-1)f+(f-1)} \mod. q^r-1\,.
\end{array}
$$

$$
\tau(\chi_r)\equiv\frac{\Pi^{r(\alpha_0+\cdots+\alpha_{f-1})}}{(\alpha_0!\cdots\alpha_{f-1}!)^r}\equiv\Big(\frac{\Pi^{\alpha_0+\cdots+\alpha_{f-1}}}{\alpha_0!\cdots\alpha_{f-1}!}\Big)^r\equiv
$$

$$
\equiv\tau(\chi)^r \mod. \mathfrak{P}^{r\,s(\alpha)+1}
$$

$$
\frac{\tau(\chi_r)}{\tau(\chi)^r}\equiv 1 \mod. \mathfrak{P}\,, \qquad\qquad \textbf{q.e.d.}
$$

Relation **(II)**. Here my question will arise. I *can* show that the function $s(\alpha)$ satisfies the relation. But I *cannot* show that the function $\alpha_0!\cdots\alpha_{f-1}!$ mod. p satisfies the relation.

As to $s(\alpha)$, we have to prove

$$
\sum_{\mu=0}^{m-1}\Big(s(\mu\alpha+\beta)-s(\mu\alpha)-s(\beta)\Big)=s(m\beta)-ms(\beta)\,,\quad \text{when } m\alpha\equiv 0 \mod. q
$$

Now from considerations as on p. 182, one sees easily:

$$
\frac{p^f-1}{p-1}\,s(\alpha) \;=\; \sum_{i=0}^{f-1}\varrho(\alpha p^i)\,.
$$

For the function $\varrho(\alpha)$ we have already proved the analogous relation. From this the relation for $s(\alpha)$ follows by passing from α, β to the αp^i, βp^i and summing up. It remains to prove the corresponding multiplicative relation:

$$
(A)\quad \prod_{\mu=0}^{m-1}\frac{f(\mu\alpha+\beta)}{f(\mu\alpha)\,f(\beta)} \;\equiv\; m^{-s(\beta)}\frac{f(m\beta)}{f(\beta)^m} \mod. p \;\text{ for } f(\alpha)=\alpha_0!\cdots\alpha_{f-1}!\,,
$$

or using the above second
form of St.'s result:

$$
(B)\quad \prod_{\mu=1}^{m-1}\frac{\varrho(\mu\alpha+\beta)!}{\varrho(\mu\alpha)!\,\varrho(\beta)!} \;\equiv\; m^{-\varrho(\beta)}\frac{\varrho(m\beta)!}{(\varrho(\beta)!)^m} \mod. p\,.
$$

The factor $m^{-s(\alpha)}$ or $m^{-\varrho(\alpha)}$ arises from the fact that $\tau_m(\psi^m)$ (not $\tau(\psi^m)$) occurs in **(II)**. For τ_m one has to replace

$$\begin{aligned} \Pi = \mathsf{Z} - 1 \ \text{ by } \ \Pi^{(m)} \ &= \ \mathsf{Z}^m - 1 \ = \\ &= \ (1+\Pi)^m - 1 \ \equiv \ m\,\Pi \ \ \mathrm{mod.}\,\Pi^2, \end{aligned}$$

hence

$$\begin{aligned} \frac{\Pi^{(m)}}{\Pi} \ &\equiv \ m \ \ \mathrm{mod.}\,\Pi \\ \tau_m(\chi) \ &\equiv \ \frac{\Pi^{(m)\,s(\alpha)}}{f(\alpha)} \ \equiv \ \frac{\Pi^{(m)\,\varrho(\alpha)}}{\varrho(\alpha)!} \ \ \mathrm{mod.}\ \mathfrak{P}^{s(\alpha)+1} \\ &\equiv \ m^{s(\alpha)}\frac{\Pi^{s(\alpha)}}{f(\alpha)} \ \equiv \ m^{\varrho(\alpha)}\frac{\Pi^{\varrho(\alpha)}}{\varrho(\alpha)!} \ \ \mathrm{mod.}\ \mathfrak{P}^{s(\alpha)+1}. \end{aligned}$$

I have tried to prove (A) or (B) on the lines of Stickelberger §4 where apparently a similar thing is done. But I did not succeed. Anyhow, the whole thing is reduced now to an elementary arithmetical question.

As to the exceptional case $p = 2$, it seems inconvenient to do it in the way indicated on p. 179 (by enhancing the modulus $\mathfrak{P}$). So far as I see, one can very easily get round the difficulty by considering the congruence behaviour of the unity in question for prime divisors of m and n. The above argument leaves only a factor ± 1 undetermined. Since the τ's are $\equiv 1 \ \mathrm{mod.}\,\mathfrak{s}$ for a character of order $\ell^{\mathfrak{s}}$ ($\mathfrak{s}$ dividing ℓ), this factor must be $+1$ for characters of prime power order. For composite order the τ's are congruent mod. $\mathfrak{s}$ to τ's with characters of order prime to ℓ. The assertion $+1$ follows then by induction.

I should be very glad, indeed, if you could help me with settling the remaining question (A) or (B) above.

The very best wishes,

yours,
Helmut

Claerle thanks for your letter. She is going to write to–morrow. Kindest regards from Gertrud. I will be going to Berlin for further negotiations on Thursday.

1.65 23.05.1934, Davenport to Hasse

Reply and questions to the preceding letters. D. wishes to get the original text of Bieberbach's lecture. D. is surprised that the case $A = 0$ always occurs, at least for $p \geq 13$. D.'s proof of the **functional equation** *for $y^n = f(x)$ is complete but not yet written down – direct calculation with polynomials. Courant has obtained a letter from Berlin.*

T.C.C. 23.5.34.

My dear Helmut,

Very many thanks for your letters of the 15th and 20th. To take the latter first; I am quite impressed by the directness and simplicity of this method, at any rate as far as the proof of $\tau(\chi_r) = \tau(\chi)^r$ is concerned. As regards the more difficult relation, I am unable to get a proof of the elem. congruence even in the case $r = 1$. I am afraid I must have misunderstood it in some way. You write[1]

$$\prod_{\mu=0}^{m-1} \frac{f(\mu\alpha+\beta)}{f(\mu\alpha)f(\beta)} \equiv m^{-s(\beta)} \frac{f(m\beta)}{f(\beta)^m} \bmod p \quad (m\alpha \equiv 0 \bmod (p-1))$$

where $f(\alpha) = \alpha_0! \ldots \alpha_{f-1}!$ With $r = 1$, $f(\alpha) = s(\alpha) = \alpha$ reduced mod $p-1$. Take $p = 7$, $\alpha = 2$, $m = 3$, $\beta = 1$. Then this is:

$$\frac{1!3!5!}{0!2!4!} \equiv 3^{-1} \cdot \frac{3!}{(1!)^3} \bmod 7,$$

which is incorrect. I must have made some stupid slip, but I can't find it.

Returning to your earlier letter: I do *not* think that the new proof of S. is *devaluated* by the fact that S. proved the result years ago – at least, I do think it is devaluated, but not so much so as not to make it well worth publishing. In fact, the new proof is much more to my taste than S.'s, because it has more "snap" and less "structure" – so our ...

I have more reason to advocate it than you should have – if we followed out

[1]Korrektur in anderer Schrift: $s(\beta)$ im Exponenten ersetzt durch $s(m\beta)$

the views about mathematics which we have attributed to one another! I agree to putting the proof of $\tau(\chi_r) = \tau(\chi)^r$ in the "low-brow" paper (that is to say, the disguised L-series proof; perhaps the new one too, if you like).

I should very much like to have the MS of Bieberbach's lecture, if you could send it me.

In a previous letter you suggested two results about the possibility of

$$S(g_2, g_3) = \sum_x \left(\frac{4x^3 - g_2 x - g_3}{p} \right) \quad \text{or rather } \chi_2 \text{ in } E_q$$

being 0 or $\pm 2\sqrt{q}$. I find the conjecture that this always happens (at least once) for $p \geqq 13$ very surprising. Is there any evidence in favour of it? [2] The other statement, – that for all q there exist g_2, g_3 for which this is not the case, cannot be difficult to prove. But the only way I can see of doing it at the moment is to calculate

$$S_2 = \sum_{g_2, g_3} (S(g_2, g_3))^2, S_4 = \sum_{g_2, g_3} (S(g_2, g_3))^4$$

(this can be done exactly, but is a little tedious for S_4) and to prove that

$$S_4 \neq 4qS_2,$$

(as I am quite sure it is not). This would suffice. But there must be some simpler way.

I have got the proof of the functional equation for $y^n = f(x)$ into a simple form – direct calculation with polynomials. You may not like the look of it, but it could easily be translated into more elegant languages, I should think. I have been intending to write it up + send it you, but there have been so many distractions. Visitors keep coming to Cambridge in this nice summer weather, and if they have ever known me they come and dig me out. Then, of course, there are always lots of new and more "amusing" problems turning up, in the course of conversation with Rado + other people.

I hope the outcome of your visit to Berlin was satisfactory. Courant had a letter from the Ministry about a fortnight ago telling him

1) he was granted leave of absence for this summer term, without any pay,

[2] Es handelt sich um die sog. supersingulären Fälle in der (späteren) Terminologie von Deuring.

2) no decision had been reached about his future, but

3) they had “no objection” to his going to America.

Cambridge is very pleasant just now. I am sorry you cannot see it at this time of the year.

Very best wishes, Yours,

Harold

1.66 24.05.1934, Hasse to Davenport

From Berlin. H. has found very nice solution of question concerning Gaussian sum relation. H. presents this in detail. H. just settled all questions with the Ministry. "The Rektor in Göttingen has been made give in." Next week H. will take up lectures in Göttingen.

HOTEL KIELER HOF,
BERLIN, 24. 5. 34

My dear Harold,

I have found a very nice solution of my question concerning the Gaussian sum relation, i. e., a very nice way of arranging Stickelberger's rather clumsy method of proving the factorial congruences connected with them, and the exponent relations at the same time. You will find the right idea already in my former letter (the long one). I proved there

$$\tau(\chi) \equiv \frac{\Pi^{\varrho(\alpha)}}{\varrho(\alpha)!} \mod. \mathfrak{P}^{\delta(\alpha)+1},$$

where α the exponent of χ for $\mathfrak{p} = \mathfrak{P}^{p-1}$, $\mathsf{Z} = 1 + \Pi$,

$$\alpha \equiv \varrho(\alpha) = \alpha_0 + \alpha_1 p + \cdots + \alpha_{f-1} p^{f-1} \mod. q-1, \quad 0 \leq \varrho(\alpha) < q-1.$$

$$\left(\substack{0 \leq \alpha_i \leq p-1 \\ \text{not all } \alpha_i = p-1}\right)$$

$$\delta(\alpha) = \alpha_0 + \alpha_1 + \cdots + \alpha_{f-1}.$$

Introducing multiplicative congruence (instead additive) this may be written as

$$\tau(\chi) \equiv \frac{\Pi^{\varrho(\alpha)}}{\varrho(\alpha)!} \mod.^* \mathfrak{P},$$

in the sense that the quotient of both sides is $\equiv 1 \mod. \mathfrak{P}$. Also

$$\tau_k(\chi) \equiv k^{\varrho(\alpha)} \frac{\Pi^{\varrho(\alpha)}}{\varrho(\alpha)!} \mod.^* \mathfrak{P}, \qquad \text{since } \Pi^{(k)} \equiv k\,\Pi \mod.^* \Pi.$$

Now the relation may be written as

$$\frac{\prod_{\mu=0}^{m-1}\ \tau(\chi^\mu\psi)}{\tau_m(\psi^m)} \;=\; \prod_{\mu=0}^{m-1} \tau(\chi^\mu)\,, \quad \text{for any character } \psi$$

i. e., *the quotient on the left hand side,* in which χ^μ runs through the solutions of $(\chi^\mu)^m = 1$, *is independent of* ψ *;* notice that the right hand side is its value for $\psi = 1$.

From the principle of "arithmetic characterisation" it suffices to prove the corresponding multiplicative congruence

$$\frac{\prod_{\mu=0}^{m-1}\ \frac{\Pi^{\varrho(\mu\alpha+\beta)}}{\varrho(\mu\alpha+\beta)!}}{m^{\varrho(m\beta)}\,\frac{\Pi^{\varrho(m\beta)}}{\varrho(m\beta)!}} \;\equiv\; \prod_{\mu=0}^{m-1} \frac{\Pi^{\varrho(\mu\alpha)}}{\varrho(\mu\alpha)!} \ \text{mod.}^*\ \mathfrak{P}\,,$$

which reduces to

$$\textbf{(I)}\quad \sum_{\mu=0}^{m-1}\ \varrho(\mu\alpha+\beta) - \varrho(m\beta) \;=\; \sum_{\mu=0}^{m-1} \varrho(\mu\alpha)$$

$$\textbf{(II)}\qquad m^{\varrho(m\beta)}\,\frac{\prod_{\mu=0}^{m-1}\ \varrho(\mu\alpha+\beta)!}{\varrho(m\beta)!} \;\equiv\; \prod_{\mu=0}^{m-1} \varrho(\mu\alpha)!\ \ \text{mod.}^*\ p\,.$$

Again **(I)** and **(II)** state the independence of β of the left hand side.
W.l.o.g. one may take

$$\alpha \;=\; \frac{q-1}{m}\,, \quad 0 \le \beta < \frac{q-1}{m}\,.$$

Then the function ϱ for all arguments in question is identical with its argument. Hence **(I)** is trivial. (I wonder why we missed this simple method of proving **(I)**!) In order to show **(II)**, I consider the effect of $\beta - 1 \longrightarrow \beta$ on the left hand side. The effect is the supplementary factor

$$m^m\,\frac{\beta(\beta+\frac{q-1}{m})\cdots(\beta+(m-1)\frac{q-1}{m})}{m\beta(m\beta-1)\cdots(m\beta-(m-1))} \;=$$

$$=\; \frac{m\beta(m\beta+(q-1))\cdots(m\beta+(m-1)(q-1))}{m\beta(m\beta-1)\cdots(m\beta-(m-1))}\,.$$

It suffices to show that this factor is $\equiv 1$ mod. p for $\beta = 1, \ldots, \frac{q-1}{m} - 1$. Now

$$m\beta + \mu(q-1) \;\equiv\; m\beta - \mu \text{ mod.}\, p^f \,, \text{ since } q = p^f \,.$$

$$\text{Since } \beta + \mu\frac{q-1}{m} < p^f - 1 \quad (\mu = 0, \ldots, m-1)\,,$$

the highest power of p contained in both sides of this congruence may [...] p^{f-1} only. Hence

$$m\beta + \mu(q-1) \;\equiv\; m\beta - \mu \text{ mod.}^* \; p \quad (\mu = 0, \ldots, m-1)$$

what proves the statement required.

Stickelberger proves a congruence between factorials amounting to essentially the same. Only his way of expressing himself and leading the proof is extremely clumsy. He expresses the α_i above as

$$\alpha_i \;=\; \left[\frac{\mu\varrho(p^{f-1-i}\alpha)}{q-1}\right] \;=\; \left[\frac{\mu\varrho(p^{f-1-i}\overline{\alpha})}{m}\right] \qquad (\text{where } \alpha = \overline{\alpha}\,\tfrac{q-1}{m}\,).$$

He gives the result for $\tau(\chi)$ in terms of those functions in the exponent of Π and their factorials in the denominator (not with my reduction, but $\frac{\Pi^{\alpha_0+\cdots+\alpha_{f-1}}}{\alpha_0!\cdots\alpha_{f-1}!}$). Without close connection with this result he gives a congruence relation mod. p for his function

$$P(x) \;=\; [x]! \quad (x \text{ real}, 0 \le x < p\,)$$

which is essentially identical with the congruence **(II)** above, and in which he sees the analogue to the third functional equation of $\Gamma(s)$ in the theory of residues mod. p.*) It is really astonishing that he has not discovered the τ–relations, although he had the principle of "arithmetic characterization" (explicitly given !), the congruence property of $\tau(\chi)$ *and this factorial relation*, all in one paper.

I have just settled all questions with the Ministry. The Rektor in Göttingen has been made give in. I shall be going there next week and take up lectures. Not a very hopeful prospect as to finishing my work on our joint paper. I hope I shall be able to spare some time for it, though, once the first rush of duties is over.

The very best wishes,

yours,
Helmut

*)That is, obviously, the only reason for his mentioning it.

1.67 27.05.1934, Hasse to Davenport

H. corrects a mistake in former letter. "Snap and Structure" of Stickelberger's proof. Disposition of joint paper. Witt remarked that **functional equation** *for L-functions is simple consequence of Riem.Hyp. But H. would like to know D.'s proof without Riem.Hyp. Now H. has signed the Vereinbarung with Ministry. Will go to Göttingen next week. H. encloses Bieberbach's lecture. Courant again. Vorträge von Nevanlinna und Ahlfors. Tomorrow Schneider.*

27. 5. 34

My dear Harold,

Your letter of May 23^{rd} reached me when I was back from Berlin. You will have got, in the meantime, my letter from Berlin. Obviously I have made a mistake by writing $m^{-s(\beta)}$ instead of $m^{-s(m\beta)}$ and so given you trouble. The whole thing is settled by my last letter.

I am surprised, you find that Stickelberger's proof has less "snap" and more "structure". As to "snap", I am not quite sure. But as to "structure", I cannot agree with you. I find, our proof has decidedly more structure.

I have written about half of our joint paper. The disposition of the whole is as follows:

Einleitung.

Allgemeines über L–Funktionen.

§1. Die Nullstellen von $\zeta_Z(s)$ für $Z = K(\sqrt[p]{x^m}) = K_0(\sqrt[m]{t},\ \sqrt[p]{t})$.

§2. Die Nullstellen von $\zeta_Z(s)$ für $Z = K(\sqrt[n]{1-x^m}) = K_0[(]\sqrt[m]{t},\ \sqrt[n]{1-t})$.

§3. Relationen zwischen verallgemeinerten Gaussschen Summen

I. Beweis der Relation $\tau(\chi_r) = \tau(\chi)^r$

II. Beweis der Relation $\prod_\mu \pi(\chi^\mu,\, \psi) = \frac{\tau(\psi)^m}{\tau_m(\psi^m)}$

§4. Arithmetische Charakterisierung der Nullstellen

§5. Arithmetischer Beweis der Relationen zwischen v.G.S.

§6. Die Anzahl der Lösungen von $ax^m + by^n = c$

§7. Die elliptischen Spezialfälle $y^2 = 1 - x^3$ und $y^2 = 1 - x^4$

Anhang: Stickelbergers Beweis und ein anderer Beweis für die Primidealzerlegung der v.G.S.

As to $\sum_x \chi_2(4x^3 - g_2x - g_3) = 0$ or $2\sqrt{q}$ for $q \geq 13$, I have very strong reason to believe that this always happens for at least one pair $g_2 \neq 0$, $g_3 \neq 0$ for $q \geq 13$. For the condition is $A_p(I) = 0$ where A_p is a polynomial in the absolute invariant I, and there is no reason to believe that $A_p(t)$ vanishes identically for any p.

Witt remarked that the functional equation of the congruence L–functions is quite generally a simple consequence from the Riemann Hypothesis. I should very much like to know your proof without R.H.

I have subscribed the Vereinbarung with the Ministry to–day. Everything is settled now. I am going to Göttingen on Tuesday next. The long delay in May was due to a letter from me having gone astray in the Ministry while the referee was abroad. So they waited for an answer from me while I was waiting for them to answer.

I am enclosing a copy of Bieberbach's lecture. Please return it to me to Göttingen. I am also enclosing an envelope for the sake of interest. It shows what the post can achieve in spotting the right place when an adress is completely wrong, and without considerable delay, too.

I was asked by the Ministry to induce Courant to resign from his post. As I must fear that they will dismiss him from their side if he does not resign from his side, I think I have to tell him so when I meet him in Göttingen. Please do not mention anything about this to anybody ! I know I can trust you.

We had Nevanlinna and another Finlandish mathematician, Ahlfors, here recently. They both gave excellent lectures on modern theory of functions. To–morrow Schneider (Frankfurt) is going to lecture here about the transcendency of a^b. Unfortunately, his result was discovered by Gelfond approximately at the same time.

Kindest regards,

yours,
Helmut.

1.68 27.05.1934, Davenport to Hasse

D. is glad that things are settled in Berlin. New proof of relations (about Gauss sums) very simple and elegant. Rado and D. work on a Minkowski conjecture.

27.5.34.

My dear Helmut,

Many thanks for your note in Clärle's letter, and your letter from Berlin. I am very glad that everything has been settled satisfactorily with the Ministry.

The new proof for the relations is very simple and elegant.

As regards χ_0, I prefer to avoid χ_0 altogether, when possible. But otherwise, I prefer to write always $\chi_0(0) = 0$ and to say that the no. of solutions of $x^m = a$ is $1 + \sum_{r=1}^{m-1} \chi^r(a)$.

Recently Rado + I have been working on a Minkowski conjecture, which asserts that if (a_{ij}) (where $|a_{ij}| = 1$) is a boundary case in the Minkowski theorem for homog. linear forms, then $(a_{ij}) = I \cdot B$ where I is a unitary matrix with integer elements, and B has the form:

$$B = \begin{pmatrix} 1 & 0 & \dots & 0 \\ b_{21} & 1 & \dots & 0 \\ & & & \\ b_{n1} & \dots & & 1 \end{pmatrix}.$$

We have some ideas for attacking it, but are not yet "through" with it.

Very best wishes,
Yours

Harold

1.69 06.06.1934, Hasse to Davenport

H. has finished the ms. of joint paper and sends it to D. for comments. Rademacher. What happened in Göttingen? On the principal character.

6. 6. 34

My dear Harold,

I have finished the Ms. of our joint paper at last. I am sending the whole matter to you as "Drucksache". Please keep all of it except the Ms. itself and the copy marked $\otimes$, because I have no second copy of either at hand.

I hope you will be content with the general outline. Please do not spare with your comment on details. You may use the margin or even empty space between the lines, for I intend typewriting the whole thing after having reached an agreement with you about everything.

Rademacher wrote me the other day from some seaside resort at the Baltic Sea. He has been definitely dismissed by the Government. As he will get a job at Philadelphia University, he asks me to inform you of his final dismission and his going to Philadelphia, and to inform the Academic Assistance Council through you. He did not get an offer from Philadelphia itself yet, but they let him know through E. Noether this offer would surely come the moment they knew about the Prussian Government's final decision, which has been given in the meantime.[†]

You must not mind our not writing about what happened in Göttingen last week.[‡]

Thanks for your letter. As to the principal character, I am afraid I could not do without it. I am not in favour of $\chi^0(0) = 0$ from general principles. I will explain those principles for the Dirichlet–characters mod. m. Such a character is defined as a function $\chi(a)$ of an integer argument a prime to m with the properties

[†]Der Brief von Rademacher an Hasse war datiert am 2. .6. .1934.

[‡]Am 29.5.1934 war Hasse in Göttingen gewesen, um die Leitung des Instituts zu übernehmen. Ihm wurde jedoch durch *Weber* die Herausgabe der Institutsschlüssel verweigert.

(1.) $\chi(a) \neq 0$ for at least one argument a

(2.) $\chi(a) = \chi(a')$ when $a \equiv a' \bmod. m$

(3.) $\chi(ab) = \chi(a)\,\chi(b)$.

If the least positive integer m_0 satisfying **(2.)** is equal to m, χ is called a *proper* character mod. m. Otherwise the manifold of the arguments a of χ may be uniquely extended to all integers m_0 in such a way that **(1.)**–**(3.)** are satisfied in this wider manifold, namely by

(4.) $\chi(b) = \chi(a)$ when b is not prime to m (but prime to m_0) and a is any integer $\equiv b \bmod. m$ and prime also to m.

χ is then a proper character mod. m_0. With the supplementary extension

(5.) $\chi(b) = 0$ when b is not prime to m_0,

(1.)–**(3.)** are satisfied in the manifold of all integers.

Now suppose $m_0 = 1$. Then, at any rate,

$$\chi(a) \;=\; 1 \quad \text{for all integers } a \neq 0\,,$$

as an easy consequence from **(1.)**–**(4.)**. But also

$$\chi(0) \;=\; 1\,,$$

by **(4.)**, since the integer 0 is $\equiv 1 \bmod. 1$, or by **(5.)**, since the integer 0 is prime to 1.*)

The question whether the principal character χ^0 of an abstract finite field k has the property $\chi^0(0) = 0$ or 1 or anything else, is of course a question of a suitable definition. But the moment we consider the characters of k as special values of Dirichlet–characters of the field $K = k(t)$, which is in every respect mattering analogous to the rational field, the above argument leads to $\chi^0(0) = 1$. And this point of view lies at the bottom of our whole paper. Hence I decided on $\chi^0(0) = 1$. As you will see, any actual trouble can be

*) There are 3 definitions for “a is prime to m”: (1) a and m have no common prime divisor; (2) from $d \mid a$ and $d \mid m$ it follows $d \mid 1$, i. e., $d = \pm 1$; (3.) from $m \mid ab$ it follows $m \mid b$. Each of those definitions (which are *not* equivalent in the general theory of ideals, m an ideal, a an element) is satisfied for $a = 0$, $m = 1$. More generally, 0 is prime to m only for $m = \pm 1$, and 1 is prime to every integer a.

avoided by excluding $a \neq 0$ from the summation in $\tau(\chi) = \sum_a \chi(a)\, e(a)$. Notice further, that even from *your* point of view ($\chi^0(0) = 0$) the relation

$$\frac{\tau(\chi)\,\tau(\psi)}{\tau(\chi\psi)} \;=\; \pi(\chi,\psi) \;=\; \sum_{a+b=1} \chi(a)\,\psi(b)$$

does *not* hold for $\chi \neq 1$, $\psi \neq 1$, $\chi\psi = 1$. From *my* point of view ($\chi^0(0) = 1$) it does *not* hold for $\chi = 1$ or $\psi = 1$ or $\chi\psi = 1$, though, and it cannot be made valid by excluding 0, 1 from the summation on the right–hand side for $\chi \neq 1$, $\psi \neq 1$, $\chi\psi = 1$ again. I am afraid the last remarks are rather intricate. You must read the introduction of the Ms. first, in order to grasp what I mean by them. After all, I have found my way in the Ms. in a consistent and clear manner, and I do not think it is necessary to alter anything in this respect.

Many best wishes, also from Claerle, my father,
Gertrud, and Juttalein.

Yours,

Helmut.

1.70 15.06.1934, Davenport to Hasse

D. was in Bristol. Heilbronn there. Heilbronn+Linfoot have joint paper on class number one problem. On trouble in Germany. No doubt things will be all right in the long run. D. plans to bring H. back in Aug. or Sept.

Friday. 15.6.34.

My dear Helmut,

I got back from Bristol last night, and have not yet done any mathematics, but I thought I would write you a few lines.

I had a very pleasant time at Bristol. Heilbronn lives in Wills Hall, a University Hall of residence endowed[1] by the tobacco magnates, and seems very happy. He is certainly a new man compared with what he was six months ago when he came to England. Bristol is not much of a mathematical centre, but Linfoot is there, who is a theory of numbers expert, so it might be worse. Heilbronn + Linfoot have written a joint paper proving that there is at most one neg. discriminant $< -10^4$ with class-number one. Nine are known before -10^4, I think. Heilbronn seems to have a lot more good ideas too.

We saw some of the country round Bristol, which is very fine. On[2] driving about in the last few days I have been much impressed by the general leisure and prosperity now in England. The depression is bad in certain areas, but in Southern England everything seems to be going well. The marvellous weather – and Summer Time – help in giving a favourable impression. The two fine summers have had quite an effect on England. Frequently one sees cafés with tables + parasols outside which gives quite a Continental air. The roads are crowded with motorists and cyclists – the girls almost all wearing shorts or trousers – which would have been impossible a few years ago.

I have heard some details about the trouble in G., and extend to you my hearty sympathy. No doubt things will be all right in the long run. I still hope to bring you back here for a visit in Aug. or Sept.

Very best wishes

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[1]undeutlich
[2]undeutlich

Yours in haste

Harold.

1.71 21.06.1934, Hasse to Davenport

Proof corrections of D.s Manuskript on exponential and character sums. – H. has found a very nice law of reciprocity for Artin-Schreier extensions. New insight: the norm residue symbol is explicitly expressed by means of the residuum.

21. 6. 34

My dear Harold,

Many thanks for your last letter. I am looking forward to your criticism on the Ms. on character and exponential sums. You need not bother about returning the copy marked $\otimes$ now that the proofs are in my hands. You will have got them, too. *Please help me with finding out the exact quotation of Hardy–Littlewood on sheet* 9. The dots after your first two papers have no significance.The printer put them in errorneously.[†]

I have found a very nice law of reciprocity for equations of type $y^p - y = A$. It is

$$\prod_{\mathfrak{p}} \left(\frac{B,\, A}{\mathfrak{p}}\right)_p = 1$$

where $\left(\frac{B,A}{\mathfrak{p}}\right)_p = e(\gamma_{\varrho_{\mathfrak{p}}}(A\,\frac{\mathrm{d}B}{B}))$ and $\varrho_{\mathfrak{p}}$ denotes the residuum of the differential $A\,\frac{\mathrm{d}B}{B}$. The new insight lies in the fact that the norm–residue–symbol $\left\{\frac{A,B}{\mathfrak{p}}\right\}$ is explicitly expressed by means of a residuum, and the law of reciprocity therefore is a simple consequence from the residuum theorem $\sum_{\mathfrak{p}} \varrho_{\mathfrak{p}}(A\,\frac{\mathrm{d}B}{B}) = 0$.

Very best wishes,

yours,

Helmut

[†]Es handelt sich offenbar um D.s Manuskript "On certain exponential sums" in Crelle 169 (1933).

1.72 13.10.1934, Davenport to Hasse

Triangle problem from Erdös.

13.10.34.

My dear Helmut,

I much regret to say that I have not done any serious work yet. Erdos gave me an elementary problem to solve, which he and other Hungarians had been unable to do. It is the following: ABC is any triangle, P any point inside. PD, PE, PF are the perpendiculars from P on to the sides. Then

$$? \quad PA + PB + PC \geqq 2(PD + PE + PF) \quad ?$$

I really have spent a week on this + haven't done it yet. Try it, or get one of your students to!

I hope you are keeping up with the Times. I haven't done the crosswords since I came here, because I read only our communal copy.

Very best wishes

Yours

Harold

Am sending you the latest Jeeves[1] to cheer you up. Personally I think it better than the previous one.

[1] undeutlich

1.73 22.10.1934, Hasse to Davenport

D. may have proof corrections for joint paper by now. Nagell's problem. D.'s triangle problem. Witt has made headway towards the **functional equation**. *H. will go to Berlin. Things do not look too rosy.*

Göttingen, 22. 10. 34

My dear Harold,

You will have got the proof corrections of our joint Crelle paper by now. Please let me have your corrections as soon as possible. I am just working on it and having it supervised by Witt.

Nagell sent a problem for the DMV: The congruence

$$x_1^n + \cdots + x_n^n \equiv a \bmod. p$$

has at least one solution for every prime p, every integer a, and every integer $n \geq 1$. I cannot do it. I found that

$$a_1 x_1^{n_1} + \cdots + a_r x_r^{n_r} \equiv 1 \bmod. p \quad \begin{pmatrix} n_1, \ldots, n_r \mid p-1 \\ a_1, \ldots, a_r \not\equiv 0 \bmod. p \end{pmatrix}$$

has

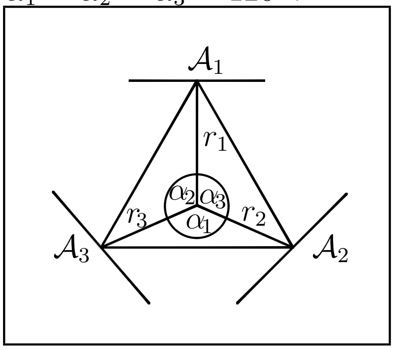
$$N = p^{r-1} + \sum_{\chi_1, \ldots, \chi_r \neq 1} \chi_1(a_1) \cdots \chi_r(a_r) \frac{p^{r-1} \tau(\chi_1 \cdots \chi_r)}{\tau(\chi_1) \cdots \tau(\chi_r)}$$

solutions where $\chi_1, \ldots, \chi_r$ run through all non–principal characters mod. p of orders $n_1, \ldots, n_r$ respectively, and $\tau(\chi) = \sum_a \chi(a)\, e(a)$. For the special case $r = n$ this gives

$$|N - p^{n-1}| \leq (n-1)^n p^{\frac{n-1}{2}},$$

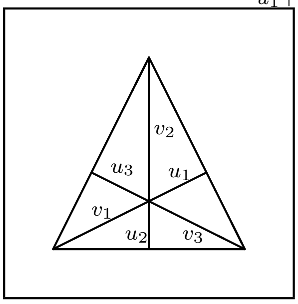
which is not sufficient to prove $N > 0$. Perhaps one can show $N \not\equiv 0$ mod. $\mathfrak{P}$ for a prime divisor of p by Kummer's congruence for the τ's. There will be an elementary method for proving Nagell's statement, of course, but I am not greatly interested in such a casual way of getting to a special result.

I have laid your triangle problem before some mathematicians. The only result I got from one of them is a proof for the case of an equilateral triangle. Steiner proved that, for an arbitrary triangle, $r_1 + r_2 + r_3 = \text{Min}$ when $\alpha_1 = \alpha_2 = \alpha_3 = 120°$.



This follows easily from the fact that the triangle formed by the perpendiculars in $\mathcal{A}_1$, $\mathcal{A}_2$, $\mathcal{A}_3$ on r_1, r_2, r_3 is equilateral when $\alpha_1 = \alpha_2 = \alpha_3 = 120°$. From this theorem on arbitrary triangles your proposition follows easily for equilateral triangles, because the sum of the perpendiculars on the sides is constant in these.

Perhaps you can carry on by means of the following nice theorem: The necessary and sufficient condition for three transversales of a triangle to go through one point is $\frac{u_1}{u_1+v_1} + \frac{u_2}{u_2+v_2} + \frac{u_3}{u_3+v_3} = 1$. For the centre of gravity, and only for it, is $\frac{u_1}{u_1+v_1} = \frac{u_2}{u_2+v_2} = \frac{u_3}{u_3+v_3} = \frac{1}{3}$.



Witt has made headway towards the functional equation. Here is one of his results (only an elementary special case of a general result): Let ϑ be a generating element of a finite field of p^n elements ($n > 1$) and χ a non–principal character of this field. Then

$$\sum_{a_0, a_1, \ldots, a_{n-2}} \chi(a_0 + a_1\vartheta + \cdots + a_{n-2}\vartheta^{n-2}) \;=\; (q-1)q^{\frac{n-2}{2}}.$$

I am going to Berlin on Wednesday. Things do not look too rosy.

Many thanks for the Wodehouse. I found it a great solace. Last night I began reading it to Clärle.

Kindest regards,

yours,
Helmut

1.74 24.10.1934, Davenport to Hasse

Nagell's question is classical theorem (Landau). More on triangle problem. D. has got out the proof of **functional equation** *for congruence L-functions. D. has not yet received proofs of joint paper.*

T.C.C. 24.10.34.

My dear Helmut,

I am glad to be able to "catch you out". Nagell's suggested question for the D.M.V. is a classical theorem, and is a special case of Satz 300 of Landau's Vorlesungen Vol 1. The proof given there is extremely elegant, especially in Nagell's simple case. I suppose the theorem originated in one of the H.-L. P.N. papers. So I think the question is a very unsuitable one for the D.M.V. – unless it is desired to establish the theorem on a purely Aryan basis. But it will be difficult to get a better proof than that in Landau.

I came across the result again as an application of my theorem on addition of sets mod p, which will appear in J.L.M.S. shortly. This states that if $\alpha_1, \dots, \alpha_m$ are m different residue classes mod p and $\beta_1, \dots, \beta_n$ are n different residue classes mod p, and $\gamma_1, \dots, \gamma_\ell$ are all those residue classes which are representable as $\alpha_i + \beta_j$, then

$$\ell \geqq m + n - 1$$

provided $m + n - 1 \leqq p$, and otherwise $\ell = p$. If we apply this to Nagell's problem, and also note that the number of non-zero residue classes which are representable as $\sum_1^N x_i^k$ $(x_i \not\equiv 0)$ is for any $k \mid p-1$ (which is no restriction) and any N a multiple of $\frac{k-1}{p}$, we get the result required [with the additional fact, as in Landau, that $x_i \not\equiv 0$].

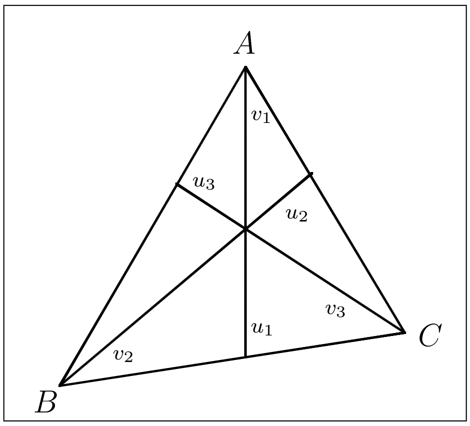
This method shows how little the result depends on the particular nature of n th powers[†)]

[†)] Your suggestion of showing $\mathcal{N} \not\equiv 0 \bmod \mathfrak{P}$ by Kummer's congruences may have great potentialities. The problem is of no great importance for the genuine Waring Problems, but has for the problem of what is $\Gamma(k)$. Of course you might have $\mathcal{N} \equiv 0 \bmod p$ without $\mathcal{N} = 0$

I also had already a proof of the triangle-inequalities for equilateral triangles. The analytical formulation can then be deduced easily from Cauchy's inequality.

I do not see how to make headway with the idea you suggest. It is *not* true that

$$v_1 + v_2 + v_3 \geqq 2(u_1 + u_2 + u_3)$$



Also it must be borne in mind that the original suggested inequality is only asserted for points *inside* the triangle: + is *not* always true for points outside.

I have now got out the proof of the functional eqn. for congruence L functions, + will send it you soon.

I have not yet received the proofs of the joint paper.

I hope you continue to read the Times. General Smut's speech at Glasgow (reported in last Thursday's Times) was excellent. In Saturday's Times there was a nice account of a night drive by car.

My love to Clärle. Excuse this awful writing.

Much love

Harold.

Regarding the Australian air race, when they landed at Melbourne the winners said: "It was a lousy trip." All the papers gave this verbatim, except the Times, which reported them as saying: "It was a dreadful trip."

1.75 26.10.1934, Davenport to Hasse

D. sends rough ms. on **functional equation**. *Replacement of p by p^r is entirely trivial. Other restrictions cannot be important.*

Cambridge 26.10.34.

My dear Helmut,

Here is a rough MS on the functional eqn. It is all really very simple, though concealed by a mass of suffixes.

The replacement of E_p by a general E_{p^f} is naturally entirely trivial. The other restriction made about the h's[1] cannot be important.

Yours in haste

H.

[1] undeutlich

1.76 27.10.1934, Davenport to Hasse

Glad to hear that Berlin was satisfactory. D. hopes to send ms. on **functional equation** *for L-fctns. of exponential sums soon. Any L-fctn. of degree 3 has at least one zero on critical strip.*

Cambridge 27.10.34.

My dear Helmut,

Glad to hear that your visit to Berlin was satisfactory. I hope to send you an M.S. on the functional eqn. for the L-functions arising from exponential sums in a day of two. Have you noticed the following amusing consequence of the functional equation: Any L-function of degree 3 has at least one zero on $\sigma = \frac{1}{2}$.

Very best wishes

Yours

Harold.

An undergraduate here claims to have solved the triangle problem, but I have not yet seen his solution.

1.77 27.10.1934, Postcard Hasse to Davenport

H. will get Nagell's question out of the Jahresbericht. Spare copies of joint paper. Looking forward to D.'s proof of **functional equation** *for L-series. Next Thursday H.'s term in Göttingen begins. Will lecture on Integral Equations and Linear Algebra.*

27. 10. 34

My dear Harold,

Thanks awfully for your catching me out in the wheeze of Nagell's question. I will get the thing taken out of the Jahresbericht. Now I remember you telling me about this very question — or rather theorem — in connexion with your paper on the game of Snakes and Adders.

I am sending you a spare copy of our joint paper. You did not get one through my fault: I forgot putting your address on the Ms. I am writing to the Publishers to send you the usual 3 copies.

I am looking forward to your proof of the functional equation for the congruence L–series.

I am afraid I cannot afford reading the Times regularly because it is rather expensive. Thus I missed Smuts' speech and the other article you mentioned. I enjoy reading the M.G.W. every Sunday, though.

Kober made a rather obscure remark about you in a recent letter of his. He wrote me you had given up your former method for investigating certain questions in the theory of numbers and would follow a method suggested by me. I have not the slightest idea what this is all about.

I have read "The Claverton Mystery" by John Rhode and am reading "Desire to kill" by Alice Campbell, also "Babbitt" by L. Sinclair.

Next Thursday my first full term in G. begins. I am going to lecture on "Integral Equations" and on "Linear Algebras", Seminar on Matrices.

I hope I shall hear from you soon.
Much love,
yours,
Helmut

1.78 30.10.1934, Hasse to Davenport

*H. got D.'s proof of L-***functional equation***. Extremely fine achievement. Witt decided not to be told about it. H. wishes the thing for Crelle. Behrbohm at present working with Redei on real quadratic fields with Euclid algorithm.*

30. 10. 34

My dear Harold,

First of all, many happy returns of this day. I hope we shall be friends for each single one.

I got your proof of the L–functional equation immediately after posting my last communication. I devoured it greedily. My heartiest congratulations on this extremely fine achievement. I find your proof absolutely oke, and more than this: a precious gem. (I hear your reply to this: don't overdo it; but I cannot help, its very simplicity and naturalness fascinated me.) I think I can do the general case (base–field $E_q(x, y)$ algebraic instead the rational field $E_q(x)$, order of χ arbitrary instead of prime to p) after the same lines.

I put Witt before the question whether I should tell him your proof or not. He decided on not being told. Would you mind my trying to do the generalization indicated ? Please write me frankly; I won't interfere with your own intentions. I shall wait at any rate until I get the case χ of order p you announce in your letter of to–day, though I feel able to give the proof for this case without difficulty.

May I have the two things for Crelle ?

As I put the triangle problem before quite a few friends, I should like to know the solution when it turns up at Cambridge.

I got Kober's Ms. on ζ–transformations. What do you think of it ? I take it he told you of his results.

Herr Behrbohm is going to investigate the elliptic case ($g = 1$) for $p = 2$. At present, he is working on real quadratic fields with an Euklid Algorithm together with Rédei. They have determined all discriminants $d > 0$ with $d \equiv 2, 3 \bmod. 4$ in question, in particular proved that their number is finite. The case $d \equiv 1 \bmod. 4$ leads to rather tricky questions concerning the distribution of quadratic residues.

Many good wishes, and much love,

Yours,
Helmut

1.79 05.11.1934, Davenport to Hasse

H. had sent spare copy of (proofs of) joint paper. H. looks forward to complete D.'s results concerning **functional equation**. *Kober. Mordell's proof of triangle problem.*

5 Nov 1934.

My dear Helmut,

Very many thanks for your two cards and your good whishes. I am glad you approve of the M.S. Of course it will require considerable revision before it is fit for publication.

I have not been feeling well for a week now (a recurrence of the old trouble), and have done practically no work. I have not got the case χ of order p out satisfactorily yet : I can do it (in the case of a polynomial) by the obvious slogging-out method which I thought of over a year ago. But I hope to get this out, and to write a paper on the subject of "The functional eqns of the congruence L-functions" – but in some English Journal, largely because this "pays me better" from the point of view of crude advertisement. I have not sufficient papers in view for it to be worth while spreading them out.

The extension to an algebraic base field is certainly in your line and not in mine. I'm afraid I have no great interest in it.

Kober's remark, which mystified you, no noubt arose in this way. He asked me whether I was continuing my work on quad. residues and exp. sums. I replied no, not really, since it had become clear that the methods I had used were not the most suitable to the problems, and that the right method was yours, or a development of it.

Rado was in Manchester last week and put the triangle problem to Mordell, which Erdos apparently had not done, and M. very quickly produced the following beautiful solution. Let x, y, z be the lengths of the perpendiculars, A, B, C the angles of the triangle ($A > 0$, $B > 0$, $C > 0$, $A + B + C = \pi$). The inequality is

$$S = \frac{1}{\sin A}\sqrt{y^2 + z^2 + 2yz\cos A} + \cdots \geqq 2(x + y + z)$$

$$y^2 + z^2 + 2yz\cos A = (y\sin C + z\sin B)^2 + (y\cos C - z\cos B)^2$$

Hence

$$
\begin{aligned}
S &\geqq \frac{y\sin C + z\sin B}{\sin A} + \dots \\
&= x\left(\frac{\sin A}{\sin B} + \frac{\sin B}{\sin A}\right) + \dots \\
&\geqq 2(x+y+z).
\end{aligned}
$$

Kober seems to be a good man on his specialities. How *deep* his work is I don't know. But it is sound, and is of at least formal interest. As a rule I suspect formal complication as being generally associated with 'shallow' mathematics.

What are 'the tricky questions on distrib. of quad. residues' which Behrbohm + Redei's work raises? I mean, of what nature.

Thanks for the 3 more copies of proof sheets. Am getting on with the reading gradually. I keep trying to get an elem. proof of the relations.

Today is Guy Fawkes Day, and there is a perpetual noise of fireworks to be heard.

Your writing "Snakes and Adders" is the best joke I have come across for a long time. The game is really called "Snakes + Ladders". Were you pulling my leg?

Much love, H.

1.80 12.11.1934, Davenport to Hasse

L-functions corresponding to mixed characters.

12 Nov 1934.

My dear Helmut,

As far as I can see, this letter will consist simply of my excuses for not having done any work. In fact I have done some work, on exponential sums, but it has been non productive.

This result may amuse you: it is not deep at all. Let $L(s)$ be the L-function corresponding to $\sum_x e(ax^3+bx)$. Let $M(s)$[1] be the L function corresponding to the mixed character + exponential sum $\sum_x \left(\frac{x}{p}\right) e(2ax^3+2bx)$. (It is obvious how to define L-functions corresponding to mixed sums, and what their properties will be.) Then:

$$\boxed{\boxed{M(s) = \left(1 - p^{\frac{1}{2}-s}\right) L\left(\frac{1}{2}s + \frac{1}{4}\right) L\left(\frac{1}{2}s + \frac{1}{4} + \frac{\pi i}{\log p}\right)}}\ ^*$$

* This is not quite correct. for $M(s)$ read $M\left(s - \frac{i\kappa}{\log p}\right)$ where
$$e^{i\kappa} = -i^{\left(\frac{p-1}{2}\right)^2} \left(\tfrac{6a}{p}\right).$$

This is only the translation into L-functions of the identity (13) of my Crelle paper. The really interesting thing about it is : how to generalise?

The October number of the Journal L.M.S. was exceptionally good.

I spent the weekend at Harrow – this is all I can produce as excuse for my unproductivity. I hope to spend next weekend elsewhere!

Much love from

H.

[1] undeutlich

1.81 13.11.1934, Davenport to Hasse

Behrbohm-Redei problem may be really difficult. On primes in arithm. progressions and the generalized R.H.

T.C.C. 13.11.34.

My dear Helmut,

Very many thanks for your letter. The Behrbohm-Redei problem does not look very difficult at first sight – there being so many "degrees of freedom", but I am afraid it really is difficult. For one thing, the obvious method of attack presupposes some knowledge about small quadratic nonresidues (ie. nonresidues $< \sqrt{p}$), and in fact we know very little about these. But the problem is one after my own heart. I should advise them to examine whether some slightly weaker theorem would not suffice (e.g. $kp - r$ instead of $p - r$). Then there might be some hope.

As regards the primes in an A.P. on the generalised R.H. I cannot find the result actually in print, but it must be known to everyone who has thought about the subject. The obvious result is:

$$\text{I u.A.d.}v\text{.R.V.:-} \qquad \pi(x;k,\ell) = \frac{1}{\varphi(k)} \operatorname{li} x + O\left(k^{\varepsilon} x^{\frac{1}{2}+\varepsilon}\right)$$

for all $\varepsilon > 0$.

The only thing required for the proof is the result given in Landau Vorles. Sätze 241-244 :-

$$\text{II u.A.d.}v\text{.R.V.,} \qquad \left|\frac{L'(s,\chi)}{L(s,\chi)}\right| < A(\varepsilon)k^{\varepsilon}\left(|t|+1\right)^{\varepsilon}$$

for all χ mod k and for $\sigma \geqq \frac{1}{2} + \varepsilon$ and $|1 - s| > A$ (to avoid the pole at 1 when $\chi = \chi_0$).

From this things follow in the usual way. Letting

$$(1) \qquad \vartheta(x;k,\ell) = \sum_{\substack{n=1 \\ n\equiv\ell \bmod k}} \wedge(n) = \sum_{p^r \leqq x} \log p$$

we have

$$(2) \qquad \vartheta(x;k,\ell) = \frac{1}{2\pi i}\int_{2-i\infty}^{2+i\infty} \frac{x^s F(s) ds}{s}$$

where

$$F(s) = -\frac{1}{\varphi(k)}\sum_{\chi} \frac{L'(s)}{L(s)}\overline{\chi}(\ell) \qquad \text{summed over all } \chi \text{ mod } k \text{ incl. } \chi_0$$

By II we have

$$|F(s)| < A(\varepsilon)k^{\varepsilon}(|t|+1)^{\varepsilon} \qquad \text{for } \sigma \geqq \frac{1}{2}+\varepsilon \quad \oplus \quad |1-s| > A.$$

$F(s)$ is also regular for $\sigma \geqq \frac{1}{2}+\varepsilon$ except for a simple pole at $s=1$ with residue $\frac{1}{\varphi(k)}$. Thus in (2) we can move the path of integration to the line $\frac{1}{2}+2\varepsilon-i\infty$, $\frac{1}{2}+2\varepsilon+i\infty$, + get

$$\vartheta(x;k,\ell) = \frac{x}{\varphi(k)} + O\left(x^{\frac{1}{2}+\varepsilon}k^{\varepsilon}\right) \qquad (\text{new } \varepsilon)$$

There is one correction I must make here; we cannot quite use (2) because we don't know the integral converges. We must use $\int\limits_{2-iT}^{2+iT}$ with an error term, sized up by means of Landau Vorles. Satz 449, choosing T a suitable function say x^3. This does not affect the truth of (2). I follows from (2) in the usual way : see e.g. Vorles. Satz 382.

All this must be in print somewhere, but I don't know where, nor does Hardy offhand.

I will try to do the proof sheets soon.

Very best wishes

Yours

H.

I suppose you will get this on Thurs. before Clärle leaves, but if not, please forward the enclosed to her.

1.82 23.11.1934, Davenport to Hasse

D. has read the proof sheets for the joint paper. 99% is due to H. Erdös.

23.11.34.

My dear Helmut,

The whole paper is marvellously written, and I can find practically nothing to correct.

The proof of (5.12) is very nice.

I have not read sheets 4, 5, 6, carefully. I am rather surprised at the length of the proofs of the relations by means of the L-series. The comparative simplicity of the proof by means of "ad hoc" definitions of the L-series makes me reluctant to study the general theory of L-series.

Thank you for allowing my name to appear at the top of this paper – 99% of which has been done by you, and done extremely well too.

I have not yet recovered from the tiring journey. Erdös is coming from Manchester tomorrow : he will tire me out still more efficiently !

Yours with much love

Harold

1.83 27.11.1934, Hasse to Davenport

H. thanks D. for doing the proofs at last. H. could not think about generalizing D.'s functional equation for the polynomial L-series. H asks D. to cooperate with Enzyklopädie. General program.

Göttingen, den
27. November 1934

My dear Harold !

Thanks very much for doing the proofs at last. I fell in with almost all your suggestions, as you will see from the copy with my remarks I returned to you yesterday. You will get a copy of the second proofs very soon. Please return them to me immediately. The number in question of the journal is to be published before X–mas.

I could not give another thought to the problem of generalising your functional equation for the polynomial L–series. Although I spent considerable energy on finding the algebraic principle lying behind your curious functional equation connected with a cubic–polynomial, I have not found anything that elucidates this rum thing.

Now I have to ask you a favour. The first value of our German Enzyklopädie der Mathematischen Wissenschaften is to be having a second edition. Prof. Hecke and me have been appointed Editors of this. We have made a plan for the whole thing giving due regard to the enormous development algebra and arithmetic have taken since the first edition (about 30 years ago). I enclose you a copy of our plan. We have decided to ask you for the last item, i. e., D 5 Binäre Diophantische Gleichungen und Kongruenzen. We should be delighted if you would care to write this number after the lines given in the second bunch of printed sheets enclosed. This number D 5 shall contain all that has been done on Diophantic equations and congruences. If you wish, we could make Diophantic equations a second part of number D 4 (which is to be written by Mahler) and leave you the Diophantic congruences only. For D 3 we shall ask Prof. Mordell. D 1 is the only number, which is nearly up to date in the first edition. We shall ask Bohr–Camér to add the few supplements required. D 2 will probably be written by Prof. Rademacher.

As to the other numbers, in A,B,C, you will hardly be interested to know whom we are going to ask. C 5 and C 7 will probably be written by me.

Please consider the matter and let me know your answer as soon as possible.

As I am badly pressed in time, forgive me for not dealing more detailedly with your remarks to our paper. You will gather the gist of what I mean from my remarks on the proofs. Although I have certainly done a lot in shaping the thing after my peculiar taste, your share on the proofs weighs considerably with me. I am glad to appear in print with you at last.

Much love,

yours,
Helmut

P.S. The whole of the articles in the Enz. Vol. I shall not exceed 80 “Bogen”, the “Bogen” à 16 pages. You may gauge from this the order of magnitude for the required number D 5 .

1.84 no date, Davenport to Hasse

Season's greetings

(Harrow, at the moment)

My dear Helmut,

Just a few lines to wish you retrospectively a Merry Christmas and anticipatively a Happy New Year.

I haven't done any work at all and the only news I have is that Littlewood has proved that $\sum \frac{1}{n} \sin \frac{x}{n}$ is *unbounded*.

Jaeger was successfully married yesterday.

Kind regards to your father + all the very best wishes

from Harold.

1.85 03.02.1935, Davenport to Hasse

Khintchine problem. Bilharz problems. Donald. Landau. D. looks forward to meet H.

3. 2. 35.

My dear Helmut,

I am sorry that it has taken me so long to write a letter to you directly, but I suppose Clärle will have told you anything there was to tell.

I have now been back in Cambridge almost three weeks. Lecturing I find rather pleasant, though with my usual procrastination I prepare each lecture the previous evening. I am now doing quadratic residues, so feel at home.

At the beginning of the term I thought long about an 'elementary' proof of the $\tau(\psi^m)$ relation, and thought I had 'almost' got it. But now I have been occupied with the 'Khintchine problem'. That is as follows. a_ν, b_ν are sequences of positive integers such that for *all*[1] n,

$$\sum_{a_\nu \leqq n} 1 \geqq \alpha n, \qquad \sum_{b_\nu \leqq n} 1 \geqq \beta n.$$

c_ν is the sequence obtained by taking all a's, all b's, and all numbers $a_i + b_j$. The conjecture is that

$$\sum_{c_\nu \leqq n} 1 \geqq (\alpha + \beta) n \quad \text{for all } n,$$

provided $\alpha + \beta < 1$. This was proved by Khintchine when $\alpha = \beta$, and something very close to it has been proved in the general case by Besicovitch. The problem is not 'important', but is rather fascinating.

I have been unable to make any progress with the Bilharz problems – either the large B. problem (to find something for his doctor dissertation) or the small B. problem (to do something further with primes for which a is prim. root).

[1] undeutlich

Donald was here yesterday and today. Last night he was my guest at the feast, and after the feast we found Landau, who had just arrived, talking to Besicovitch. He told us dozens of funny or witty stories. He is giving the Rouse Ball lecture next Wednesday – a lecture without proofs, for the first time (almost) in his life.

I wonder whether you cared for Queen Victoria – or does biography bore you? I read the book some weeks ago in the Union Library. The Times I sent you for the picture from the Scilly Isles: the Weekend Review for the article "Jack Horner". (I suppose you know Jack Horner?) The Weekend Review is good in many ways: I hope it isnt still banned in Germany.

I hope you have satisfactory visits to Marburg and Berlin, and I hope I shall be able to meet you on the quay at as early a date as possible.

My very best wishes, also to Gertrud,

Yours, Harold

1.86 19.02.1935, Davenport to Hasse

Formal invitation to a course of lectures in the Seminar on class field theory.

19 February 1935

Dear Prof. Hasse,

I wish to invite you as cordially as possible to give a course of lectures here at our Seminar on classfield theory, preferably between March 5 and March 20. I trust that we shall also be able to collaborate in research work in continuation of that in our previous joint paper.

It will give me great pleasure to see you here in the course of the next fortnight.

Yours sincerely,

H Davenport

M.A., Fellow of Trinity College

1.87 09.04.1935, Hasse to Davenport

H. has been in England. Most pleasant stay. Behrbohm-Redei. Bilharz is downhearted. H. has written to Erdös. H.'s seminar is about Gaussian sums. Eventually to the relations, all without knowledge of algebraic functions mod p. H. interested in Caliban problems, opened a contest in the Institute. On primitive roots in function field case. Bilharz. Reduced the whole thing to R.H.

9. 4. 35

My dear Harold,

I feel very much ashamed for not having written to you since we left you in the English Channel. But as I know you don't like large apologies, I won't make efforts to put the necessary amount of such in King's English. I would not achieve that, anyhow !

Again you know without many words on my part, how infinitely grateful I am to you for all you did to our benefit during our most pleasant stay in England. So I will leave it at that.

I delayed writing to you, after the first rush of work was over, chiefly because I intended giving you an account of *Behrbohm–Rédei's* work on *Euklid's Algorithm* for $d \equiv 1 \bmod. 4$, $d > 0$. These people, however, have not written down yet what they did. I asked them for it immediately on my return here.

Billharz is rather downhearted about his prospects. I pity the poor chap sincerely. I have *written to Erdös* for a detailed account about his result. If it turns out as you told me, I do not think Billharz will find anything to do for himself in this subject. It occured to me, though, that *there might be some hope of getting through* for the equivalent problem in a *field* $R = k(x)$ where k is a finite field. Here the ζ–functions are explicitly known. *I think I can carry everything through* for the special case, where the given $A(x)$ in R (which is to be primitive root) is x itself. This would answer the question, for how many irreducible polynomials $P(x)$ mod. p the generating element ξ of the field $k(\xi)$ with $P(\xi) = 0$ is a basis of the cyclic group of all elements $\neq 0$ of $k(\xi)$. I think this is not quite trivial.

The subject of my Seminar this term is "Gaussian sums". I gave a short account on the ordinary Gaussian sums and their connexion with cyclotomy in the first Seminar. Next time a student shall tell us Mordell's proof for the sign of $\sum_{\nu=0}^{n-1} e^{\frac{2\pi i\nu^2}{n}}$ (Mess. 48), which is a little nicer than Kronecker's proof (which you gave when I was there). We shall then proceed to the prime ideal decomposition, and eventually to the relations, all without knowledge of algebraic functions mod. p , quite elementarily.

By the way, the Manchester Guardian has been sent to me through all the time of my absence, and is still coming every Saturday. I think, though, I can do without it now. I have ordered the Times weekly from the News Agency here. If you will do me a favour, could you order the New Statesman for me ? I begin to take an interest in the Caliban Problems, and I cannot get the N. St. here. Last night I solved Crackrib's Diary (Week–end Pr. Bk.), the thing with the logarithms. I don't understand why I did not see the trick immediately when I worked so hard on it two years ago. I now found it immediately by starting with the *second* column, in which only very few of the last two digits exceed 26 .

I have been re–reading a few chapters of "Tess" with great pleasure. The lovely Dorset Countryside is still alive within me, and that makes reading about it a renewed pleasure. I further read Margery Allingham's detective story in which an art–dealer Max Fustian kills an artist Dacre and a woman in connexion with the will of a great painter. I forgot the title. You will know it presumably. The authoress also wrote "Police at the funeral" whose plot is chiefly laid down at Cambridge.

Many kind regards, old boy, and much love, from
yours,

Helmut

1.88 12.04.1935, Hasse to Davenport

H. and Bilharz work hard on primitive roots.

12. 4. 35

My dear Harold,

I apologize for the triviality I overlooked in my last letter. *There is of course always a prime polynomial* $P_n(x)$ mod. p of given degree n for which x is a primitive root, namely simply the prime polynomial to which a given primitive root of $GF(p^n)$ belongs as a root. As there are $\frac{\varphi(p^n-1)}{n}$ non–conjugate primitive roots in $GF(p^n)$, the number of $P_n(x)$ is $\frac{\varphi(p^n-1)}{n}$. *It cannot be difficult to give* an expression for the density of those $P_n(x)$ among all prime polynomials when $n \to \infty$.

Billharz and I are working hard on the problem for a general given $A(x)$ as primitive root. I think we have reduced the whole thing to Riemann's hypothesis mod. p and the following question:

Let f_q denote the least exponent with $p^{f_q} \equiv 1 \text{ mod. } q$. Does then $\sum_q \frac{1}{f_q \cdot p^{\frac{1}{2} f_q}}$ converge ? The factor $\frac{1}{2}$ by f_q arises from Riemann's hypothesis.

I have been trifling about a little with the Week–End Problem Book. I believe I have found a flaw in Time Test 32. Besides the solution given by the author there seems to be another:

"My brother who *died* on the 15^{th} of June 1899 would have been 29 years old last $\left\{ \begin{array}{c} \text{week} \\ \text{month} \\ \text{year} \end{array} \right\}$." (Stated 1^{st} of July 1927).

I cannot understand Time Tests 30, 34. The former, because I do not know how the score in Bridge is converted into payments. The latter has no point for me. Perhaps you can help me.

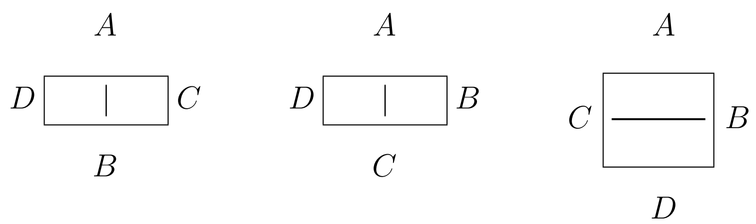
I have opened a contest in Caliban problems in the Institute. Each week I announce one of them (suitably arranged according to the differences implied by the other language and metric systems). The solutions are to be put in a box until Friday night, and the solvers are awarded by publishing their names on the notice board together with the solution and the new problem.

I began with "who killed Popoff ?"

Kindest regards and much love,

Yours,
Helmut

Claerle will be coming home to–morrow morning with Juttalein.



$$\begin{array}{llcl} B & 8-13+11 & = & 6 \\ C & -8+13+11 & = & 16 \\ D & -8-13-11 & = & -32 \end{array}$$

1.89 14.04.1935, Hasse to Davenport

On Bilharz problem. H. has got together all details. H. sends extended manuscript to D. Erdös announced sending his proof for the rational field in a few days.

14. 4. 35

My dear Harold,

You will be interested *in my progress in the now transformed Billharz problem*: primitive roots in $k(x) = R$. I have now *got together all the essential details, and I am going to let Billharz "discover" them gradually.*

Let k be a finite field of p elements (p power of prime p_0).

$R = k(x)$ the field of all rational functions of x over k.

$A \neq 0$ a given element in R, for no prime $q \neq p_0$ a q^{th} power of a divisor of R, in particular not an element in k.

$\mathfrak{p}$ prime divisors of R; $\mathfrak{N}_p = p^n$ (number of residue classes mod $\mathfrak{p}$)

q primes $\neq p_0$; f_q the order of p mod. q, i. e., $p^{f_q} \equiv 1$ mod. q as the first power.

k_q the finite field of p^{f_q} elements (q^{th} roots of unity over k); $[k_q : k] = f_q$.

$R_q = k_q(x)$; $[R_q : R] = f_q$.

$\overline{\mathfrak{p}}$ prime divisors of $\mathfrak{p}$ in R_q; then $\mathfrak{N}_{\overline{\mathfrak{p}}} = \mathfrak{N}_{\mathfrak{p}}^{d_q^{(\mathfrak{p})}}$, where $d_q^{(\mathfrak{p})}$ the order of $\mathfrak{N}_{\mathfrak{p}}$ mod. q, i. e., $\mathfrak{N}_{\mathfrak{p}}^{d_q^{(\mathfrak{p})}} \equiv 1$ mod. q as the first power; the number of $\overline{\mathfrak{p}}$'s dividing $\mathfrak{p}$ is $e_q^{(\mathfrak{p})}$ with $d_q^{(\mathfrak{p})} e_q^{(\mathfrak{p})} = f_q$.

In particular

$$d_q^{(\mathfrak{p})} = 1 \longleftrightarrow \mathfrak{N}_{\mathfrak{p}} \equiv 1 \bmod. q \qquad \text{(necessary and sufficient condition for } \mathfrak{p} \text{ decomposed fully in } R_q \text{)}$$

$K_q = R_q(\sqrt[q]{A}) = k_q(x,\, y)$ with $y^q = A$; $[K_q : R_q] = q$, since R_q/R is not ramified, hence A is no q^{th} power of a divisor also in R_q.

The set $\mathfrak{M}$ of all those $\mathfrak{p}$ for which A mod. $\mathfrak{p}$ is a primitive root is characterised as the common part of all sets $\overline{\mathfrak{M}}_q$, where $\overline{\mathfrak{M}}_q$ is the complement to the set $\mathfrak{M}_q$ defined as follows:

$\mathfrak{M}_q$ is the set of all $\mathfrak{p}$ (prime to A) for which

(a.)	$\mathfrak{N}_\mathfrak{p} \equiv 1 \bmod. q$,	i. e., $\mathfrak{p}$	fully decomposed in R_q,
(b.)	$\left(\frac{A}{\overline{\mathfrak{p}}}\right)_q = 1$,	i. e., $\overline{\mathfrak{p}}$	fully decomposed in K_q.

We are concerned with the "frequency function" of $\mathfrak{M}$:

$$w_\mathfrak{M}(s) \;=\; \frac{\sum_{\substack{\mathfrak{p} \text{ in } \mathfrak{M} \\ m\geq 1}} \frac{1}{m\mathfrak{N}_\mathfrak{p}^{ms}}}{\sum_{\substack{\mathfrak{p} \\ m\geq 1}} \frac{1}{m\mathfrak{N}_\mathfrak{p}^{ms}}} \qquad (s > 1)\,,$$

and the question whether the "density"

$$w_\mathfrak{M} \;=\; \lim_{s\to 1} w_\mathfrak{M}(s)$$

exists and what its value is. The denominator of $w_\mathfrak{M}(s)$ is simply $\log\,\zeta(s)$, where $\zeta(s) = (1-\frac{1}{p^{s-1}})(1-\frac{1}{p^s})$ is the ζ–function of R.

It is known that for each normal (Galoisien) field K over R the set $\mathfrak{M}_K$ of all $\mathfrak{p}$ fully decomposed in K has a density

$$w_{\mathfrak{M}_K} \;=\; \frac{1}{n} \qquad \text{where} \quad n = [K : R]\,.$$

Moreover the numerator of the corresponding frequency function is part of the Dirichlet series

$$\frac{1}{n}\,\log\,\zeta_K(s) \;=\; \frac{1}{n}\sum_{\substack{\mathfrak{P} \\ m\geq 1}} \frac{1}{m\mathfrak{N}\,\mathfrak{P}^{ms}} \qquad (\,\mathfrak{P} \text{ prime divisors of } K\,).$$

Hence in particular (since $\mathfrak{M}_{K_q} = \mathfrak{M}_q$ and $[K_q : R] = [K_q : R_q] \cdot [R_q : R] = q\,f_q$):

$$\lim_{s\to 1} w_{\mathfrak{M}_q}(s) = \frac{1}{q\,f_q}\,, \tag{1}$$

$$w_{\mathfrak{M}_q}(s) \leq \frac{1}{q\,f_q}\,\frac{\log\,\zeta_{K_q}(s)}{\log\,\zeta(s)}\,. \tag{2}$$

We form the common part $\mathfrak{M}$ of all $\overline{\mathfrak{M}}_q$ by taking first the common part $\mathfrak{M}^{(n)}$ of the first n sets $\overline{\mathfrak{M}}_{q_\nu}$ $\;(\nu = 1, \ldots, n)\;$ (for any fixed order of all primes $q \neq p_0$), and then taking the limit for $n \to \infty$.

By purely combinatory considerations one finds easily:

$$w_{\mathfrak{M}^{(n)}}(s) = 1 - \sum_{1\leq\nu\leq n} w_{\mathfrak{M}_{q_\nu}}(s) + \sum_{1\leq\nu_1<\nu_2\leq n} w_{\mathfrak{M}_{q_{\nu_1},\,q_{\nu_2}}}(s) - \cdots \tag{3}$$

$$\cdots + (-1)^n w_{\mathfrak{M}_{q_1,\ldots,q_n}}(s)\,,$$

where $\mathfrak{M}_{q_{\nu_1},\ldots,q_{\nu_\varrho}}$ denotes the set of all those $\mathfrak{p}$ which are fully decomposable in all fields $K_{q_{\nu_1}}, \ldots, K_{q_{\nu_\varrho}}$ (or — what is usually the same — in their composite field $K_{q_{\nu_1}} \cdots K_{q_{\nu_\varrho}}$). Those sets $\mathfrak{M}_{q_{\nu_1},\ldots,q_{\nu_\varrho}}$ have densities $w_{\mathfrak{M}_{q_{\nu_1},\ldots,q_{\nu_\varrho}}}$, namely the reciprocals to the degrees $n_{q_{\nu_1},\ldots,q_{\nu_\varrho}}$ of the $K_{q_{\nu_1}} \cdots K_{q_{\nu_\varrho}}$. Hence the $\mathfrak{M}^{(n)}$ have densities $w_{\mathfrak{M}^{(n)}}$, namely

$$w_{\mathfrak{M}^{(n)}} = \lim_{s\to 1} w_{\mathfrak{M}^{(n)}}(s) = 1 - \sum_{1\leq\nu\leq n} \frac{1}{n_{q_\nu}} + \sum_{1\leq\nu_1<\nu_2\leq n} \frac{1}{n_{q_{\nu_1},\,q_{\nu_2}}} - \cdots \tag{4}$$

$$\cdots + (-1)^n\,\frac{1}{n_{q_1,\ldots,q_n}}\,.$$

Again, by purely combinatory considerations one finds from (3):

$$w_{\mathfrak{M}^{(n)}}(s) \quad \text{decreases monotonously with increasing } n\,,$$

and

$$w_{\mathfrak{M}}(s) < w_{\mathfrak{M}^{(n)}}(s) \qquad \text{for all } n\,.$$

Hence there exists

$$\lim_{n\to\infty} w_{\mathfrak{M}^{(n)}}(s) \geq w_{\mathfrak{M}}(s)\,. \tag{5}$$

Finally, by purely combinatory considerations, one finds from (3):

$$w_{\mathfrak{M}^{(n)}}(s) - w_{\mathfrak{M}}(s) \leq \sum_{\nu > n} w_{\mathfrak{M}_{q_\nu}}(s)\,. \tag{6}$$

We shall prove

$$\underline{\sum_q w_{\mathfrak{M}_q}(s) \quad \text{converges, uniformly for } 1 < s \leq s_0\,.} \tag{7}$$

Hence, by (5),(6):

$$\lim_{n\to\infty} w_{\mathfrak{M}^{(n)}}(s) \;=\; w_{\mathfrak{M}}(s)\,, \qquad \text{uniformly for } 1 < s \leq s_0\,,$$

and therefore there exists the density:

$$\begin{aligned} \underline{\underline{w_{\mathfrak{M}} \;=\; \lim_{s\to 1} w_{\mathfrak{M}}(s)}} \;&=\; \lim_{s\to 1}\lim_{n\to\infty} w_{\mathfrak{M}^{(n)}}(s) \;= \\ &=\; \lim_{n\to\infty}\lim_{s\to 1} w_{\mathfrak{M}^{(n)}}(s) \;=\; \underline{\underline{\lim_{n\to\infty} w_{\mathfrak{M}^{(n)}}}}\,, \end{aligned} \tag{8}$$

where the right–hand side may be evaluated from (4) by arithmetical methods. I shall leave that to Billharz. It cannot be difficult.*)

The main point for the proof of (8) is the proof of (7). The latter will be proved, when the following statement has been shown to be true:

$$\sum_q \frac{1}{q\,f_q}\,\frac{\log\,\zeta_{K_q}(s)}{\log\,\zeta(s)} \quad \text{converges, uniformly for } 1 < s \leq s_0\,. \tag{9}$$

For then (7) follows from (2).

Now

$$\zeta_{K_q}(s) \;=\; \zeta_q(s) L_{K_q}(s)\,,$$

where $\zeta_q(s)$ is the ζ–function of R_q and $L_{K_q}(s)$ a polynomial of degree $2g_q$ in $\frac{1}{p^{f_q s}}$ with constant term 1; g_q denotes the genus of K_q.

*) Other than for the rational number field, the $n_{q_{\nu_1},\dots,q_{\nu_\varrho}}$ are *not* simply the products of the $n_{q_\nu} = q_\nu f_{q_\nu}$, since the fields K_{q_ν} have common parts greater than R. The common part of K_{q_1} and K_{q_2} is $R_{(q_1,\,q_2)} = k_{(q_1,\,q_2)}(x)$, where $k_{(q_1,\,q_2)}$ is the common part of k_{q_1}, k_{q_2} (of $p^{f(q_1,\,q_2)}$ elements; $f(q_1,\,q_2)$ the g.c.d. of f_{q_1} and f_{q_2})

Hence (9) reduces to the following two statements:

$$\begin{array}{lll} \text{(10a.)} & \sum_q \frac{1}{q\,f_q}\frac{\log \zeta_q(s)}{\log \zeta(s)} & \text{converges, uniformly for } 1 < s \le s_0 \\ \text{(10b.)} & \sum_q \frac{1}{q\,f_q}\frac{\log L_{K_q}(s)}{\log \zeta(s)} & \text{"} \qquad \text{"} \qquad \text{"} \qquad \text{"} \end{array}$$

Proof of (10a.)

$$\begin{array}{lllll} \zeta(s) & = & (1-\frac{1}{p^{s-1}})^{-1}(1-\frac{1}{p^s})^{-1} & = & \sum_{n=0}^\infty \frac{1}{p^{n(s-1)}} \cdot \sum_{n=0}^\infty \frac{1}{p^{ns}} \\ \zeta_q(s) & = & (1-\frac{1}{p^{f_q(s-1)}})^{-1}(1-\frac{1}{p^{f_q s}})^{-1} & = & \sum_{n=0}^\infty \frac{1}{p^{f_q n(s-1)}} \cdot \sum_{n=0}^\infty \frac{1}{p^{f_q ns}} \end{array}$$

$$\begin{array}{llllll} (1<) & \zeta_q(s) & \le & \zeta(s) & \text{for} & s>1 \\ (0<) & \log \zeta_q(s) & \le & \log \zeta(s) & \text{for} & s>1 \end{array}$$

$$\frac{1}{q\,f_q}\,\frac{\log \zeta_q(s)}{\log \zeta(s)} \le \frac{1}{q\,f_q} \le \log p \cdot \frac{1}{q\,\log q} \qquad \text{for } s>1\,,$$

the latter, since

$$\begin{array}{rcl} q & | & p^{f_q}-1 \\ q & < & p^{f_q} \\ f_q & > & \dfrac{\log q}{\log p}\,. \end{array}$$

As $\sum_q \frac{1}{q\,\log q}$ converges, (10.a) is true.

Proof of (10b.) under the generalized Riemann hypothesis, or even less, namely the hypothesis, that the zeros ω_ν of

$$\left(\frac{\zeta_{K_q}(s)}{\zeta_q(s)} \;=\;\right) L_{K_q}(s) \;=\; \prod_{\nu=1}^{2g_q}(1-\frac{\omega_\nu}{p^{f_q s}})$$

have the property

$$|\omega_\nu| \le p^{\vartheta f_q} \qquad \text{with } (\tfrac{1}{2}\le)\,\vartheta < 1 \textit{ independent of } q\,.$$

Then

$$(0<)\,L_{K_q}(s) \le \left(1+\frac{p^{\vartheta f_q}}{p^{f_q s}}\right)^{2g_q} < \left(1+\frac{p^{\vartheta f_q}}{p^{f_q}}\right)^{2g_q} =$$

$$= \left(1+\frac{1}{p^{(1-\vartheta)f_q}}\right)^{2g_q} \qquad \text{for } s>1\,.$$

Now

$$2g_q \;=\; (q-1)(m_q-2)\,,$$

where m_q is the number of the different prime divisors $\overline{\mathfrak{p}}$ occuring in A with an exponent not divisible by q (see my first paper in Crelle 172). This m_q is certainly not greater than the sum of the degrees in x of numerator and denominator of A plus 1 (allowing for the prime divisor $\mathfrak{p}_\infty = \overline{\mathfrak{p}}_\infty$, the denominator of x), hence

$$m_q - 2 \le m\,, \qquad \text{where } m \text{ depends on } A \text{ only (not on } q\text{)},$$

and

$$2g_q < mq\,.$$

Therefore

$$\log\, L_{K_q}(s) < 2g_q\, \log\left(1+\frac{1}{p^{(1-\vartheta)f_q}}\right) < mq\cdot\frac{1}{p^{(1-\vartheta)f_q}} \qquad \text{for } s>1\,.$$

On the other hand there exists an $s_0 > 1$ such that

$$\log\,\zeta(s) \ge 1 \qquad \text{for } 1 < s \le s_0\,.$$

It follows

$$\frac{1}{q\,f_q}\,\frac{\log\, L_{K_q}(s)}{\log\,\zeta(s)} < m\,\frac{1}{f_q p^{(1-\vartheta)f_q}} \qquad \text{for} \quad 1 < s \le s_0\,.$$

(10b.) will be proved, when

(11) $$\sum_q \frac{1}{f_q p^{(1-\vartheta)f_q}} \quad \text{converges}$$

has been shown.

Proof of (11). We divide all q's into two classes:

Class I. $q < p^{(1-\vartheta)f_q}$.

Since always

$$f_q > \frac{1}{\log\, p}\,\log\, q\,,$$

$$\frac{1}{f_q p^{(1-\vartheta)f_q}} < \log\, p\,\frac{1}{q\,\log\, q} \qquad \text{for Class I,}$$

Hence (11) converges for class I.

Class II. $\quad q \geq p^{(1-\vartheta)f_q}$.

Let $q_1, \ldots, q_r$ be r different primes of class II, belonging to the same $f = f_{q_1} = \cdots = f_{q_r}$. Then the product $q_1 \cdot \cdots \cdot q_r \mid p^f - 1$, hence

$$p^{r(1-\vartheta)f} \leq q_1 \cdots q_r < p^f ,$$

and therefore

$$r(1-\vartheta) < 1$$

$$r < \frac{1}{1-\vartheta} .$$

Hence for each given f there are at most $[\frac{1}{1-\vartheta}]$ primes q of class II with $f_q = f$. Arranging sum (11) for class II according to the values $f = 1, 2, \ldots$ of the f_q one has therefore

$$\sum_{q \text{ in II}} \frac{1}{f_q p^{(1-\vartheta)f_q}} < \frac{1}{1-\vartheta} \sum_{f=1}^{\infty} \frac{1}{f p^{(1-\vartheta)f}} = < \frac{1}{1-\vartheta} \log \frac{1}{1 - \frac{1}{p^{1-\vartheta}}} .$$

Hence (11) converges also for class II.

This finishes the proof of (10b.), hence (9), (7), (8) .

Since the hypothesis about the zeros of $\zeta_{K_q}(s)$ involves characters χ of all the orders q , none of the special results of you, Mordell, and others seem to guarantee its truth for any A other than

$$A = x^m, \quad 1 - x^m, \quad x^p - x ,$$

of which *the first yields only* a trivial result, as I pointed out in my last letter. The latter two, however, seem to give non–trivial results.

Erdös announced sending his proof for the rational field R within a couple of days.

Many kind regards,

Yours,
Helmut.

1.90 16.04.1935, Davenport to Hasse

H. had been in England. Khintchine problem. New Bilharz problem. L-functions built with characters modd $(f(x), p)$. *Tine Tests and Caliban problems. D. asks H. to return letter last autumn on* **functional equation**.

Tuesday. 16.4.35.

My dear Helmut,

Many thanks for your two letters. I also am ashamed at not having written. Since your departure I have oscillated between Cambridge + Harrow, and the only mathematical subject I have thought about has been "$\alpha + \beta$" (Khintchine), which I have tormented myself over without any success.

Your new Bilharz problem sounds quite interesting. The series

$$\sum_q \frac{1}{f_q p^{\frac{1}{2} f_q}}$$

which you wrote down is obviously strongly cgt., perhaps you made a slip? For the number of values of q for given f_q is $\leqq$ number of prime factors of $p^{f_q} - 1$ which is $\leqq \dfrac{\log\left(p^{f_q} - 1\right)}{\log 2} = O(f_q)$ and the series $\displaystyle\sum_f \frac{1}{p^{\frac{1}{2} f}}$ is strongly cgt.

Did you ever consider the subject of the characters modd $\big(f(x), p\big)$, where, for example, $f(x) = x^k$? Does the Riemann hypothesis for the L-functions built with these characters, i.e.

$$L_\chi(s) = \sum_g \chi(g) p^{-s(\text{degree } g)}$$

where $g = g(x)$ runs through all polynomials mod p, follow from any other type of Riemann hypothesis?

I quite agree with you about the alternative solution to Time Test 32. As regards Time Test 30 you are told that 100 points = 1 shilling (a common but extravagant method of scoring – of course prohibitively high with Contract scoring). In Time Test 34, the only point is that to smoke a cigar with *a band on*, ie. without removing the band, is in bad taste. It is very feeble.

I shall be very interested to hear how the □□□ Caliban problems catch on in Göttingen.

I hope you do not come under suspicion owing to possessing the New Statesman. It is a left wing periodical; one of the best weeklies. I found last week's Caliban problem very easy.

The new term at Cambridge starts a week today. Summer Time came in on Sunday, bringing an impression of Summer with it.

Could you let me have back sometime the letter I wrote you (I suppose last autumn) containing a proof of the functional eqns of the exponential-sum L functions.

I have read all the novels of Jane Austen in the past few weeks. I used to think them very dull, but now like them.

I am only sorry that your visit was so short. I look forward to seeing you in Summer.

All best wishes from

Harold.

1.91 19.04.1935, Davenport to Hasse

Referring to a letter from H. which is not preserved.

Good Friday. 19.4.35.

My dear Helmut,

Your M.S. arrived on Tuesday evening, just after I had taken my letter to you to the post. It is very interesting and suggestive.

I don't quite see what you mean at the end by taking $A(x) = x^m$. Surely this is inadmissible, except for $m = 1$?

One can certainly take

$$A(x) = \text{quadratic} \quad [\text{but this is covered by } 1 - x^m]$$

□□□ and

$$A(x) = \text{cubic}$$

and

$$A(x) = \frac{\text{quadratic}}{\text{linear}},$$

because I once proved (J.L.M.S.) that (in the first place mod p, but proof is same for any G.F.)

$$\left| \sum_x \chi_1(x + a_1)\chi_2(x + a_2)\chi_3(x + a_3) \right| < Kp^{3/4}$$

(K abs. const.), and this, I suppose, implies the result one requires, by going [...] to a G.F. in which the cubic or quadratic splits up.

Also one could take

$$A(x) = (x + a_1)^{h_1}(x + a_2)^{h_2} \quad \text{if } (h_1, h_2) = 1,$$

for the roots of the L fn are then our sums $\pi(\chi, \psi)$.

I have nothing interesting to report, except that our Austrian maid is leaving us. She says she has been offered a better situation.

I was much reminded of past conversations with you by re-reading part of the Forsyte Saga today.

All good wishes for Easter.

Much love,

Harold.

1.92 28.04.1935, Davenport to Hasse

D. thanks for H.'s letter which is not known but appears to be about primitive roots modulo p.

Sunday. 28.4.35.

My dear Helmut,

Many thanks for your letter. I shall be very interested to hear whether anything comes out with the primitive roots mod p. I should think you would come up against some real difficulties.

The Joan problem gave me no difficulty. There are simply an unknown number of ages, which turns out, by considerations of magnitude, must be 3, + one is led to the solution. With the other problem, I agree with you that it seems much too easy. It follows, from the use of the phrase "all of those" by the man who knows what k is, that $k \geqq 3$. There are the obvious solutions

$$\begin{array}{lll} x = 7, & y = 3, & k = 3, \\ x = 7, & y = 4, & k = 4, \\ x = 35, & y = 34, & k = 34. \end{array}$$

So the answer appears to be, trivially, negative.

I sent the Times a few days ago because of the pictures.

Very best wishes

Yours
Harold

1.93 10.06.1935, Davenport to Hasse

H.'s mathematical query is of schoolboy standard. Repeating request that H. may return D.'s letter concerning **functional equation**.

Whit Monday 1935.

My dear Helmut,

Many thanks for your letter. I am very glad indeed to hear that Gertrud is getting on so well.

Your mathematical query is quite of schoolboy standard! For any $k < n$ we have

$$\begin{aligned}
\sum_1^n \frac{p^\nu}{\nu} &= \sum_1^{n-k} \frac{p^\nu}{\nu} + p^n \sum_0^{k-1} \frac{1}{p^\nu (n-\nu)} \\
&= \sum_1^{n-k} \frac{p^\nu}{\nu} + \frac{p^n}{n} \sum_0^{k-1} \frac{1}{p^\nu} + \frac{p^n}{n} \sum_0^{k-1} \frac{\nu}{p^\nu (n-\nu)} \\
&= S_1 + S_2 + S_3 \quad \text{say.} \\
S_1 &= O(p^{n-k}); \\
S_2 &= \frac{p^{n+1}}{n(p-1)} + O(p^{n-k});
\end{aligned}$$

and provided $k < \frac{1}{2}n$, say,

$$S_3 = O\left(\frac{p^n k^2}{n^2}\right).$$

Choosing $k = 2\log n$,

$$\sum_1^n \frac{p^\nu}{\nu} = \frac{p^{n+1}}{n(p-1)} \left\{1 + O\left(\frac{\log^2 n}{n}\right)\right\}.$$

In fact one can easily get an asymptotic expansion in decreasing powers of n, if required. (Partial Summation!)

Tomorrow I am going to Bristol with Heilbronn to our little mathematical meeting. Today I must think of something to say about the congruence ζ-fns.

Don't forget to let me have my letter on the functional eqn. back sometime.

I am still trying the $\alpha + \beta$ problem without success.

I hope you are having a pleasant Whitsun with the car.

Love to all

Harold

1.94 21.06.1935, Davenport to Hasse

On li(x) *and* $\sum p^\nu/\nu$.

21 June 1935

My dear Helmut,

The position re $\sum p^\nu/\nu$ is quite analogous to that of $\operatorname{li} x$. $\operatorname{li} x$ is *not* $\frac{x}{\log x} + o(\sqrt{x}\log x)$ – you ought to know this! $\operatorname{Li} x$ has the asymptotic expansion

$$\operatorname{Li} x = \frac{x}{\log x} + \frac{x}{\log^2 x} + \frac{2!x}{\log^3 x} + \cdots$$

$\sum_1^n \frac{p^\nu}{\nu}$ has the asymptotic expansion

$$\sum_1^n \frac{p^\nu}{\nu} = \frac{1}{p-1}\frac{p^n}{n} + \frac{1}{(p-1)^2}\frac{p^n}{n^2} + \frac{2!}{(p-1)^3}\frac{p^n}{n^3} + \ldots$$

(for fixed p). Proof: partial integ. or summ.n resp.

I have ordered the New St. permanently + also the odd copy I forgot to send.

All best wishes + much love

from Harold

Sorry to have to write in such haste.

1.95 11.07.1935, Hasse to Davenport

Seminar on Gaussian sums. H.L. Schnid has found an elementary proof of the first relation on Gaussian sums. Proof is given. Looking forward to see D. end of July.

Göttingen, den
11. 7. 35

My dear Harold,

My Seminar on Gaussian sums has had one outcome at least: a research student of mine, H. L. Schmid, has found an elementary proof for the relation

$$\tau^{(r)}(\chi) \;=\; \tau(\chi)^r\,.$$

His **proof** proceeds by induction. Suppose the relation is true for r. Then

$$\begin{aligned}
\tau(\chi)^{r+1} &= \tau(\chi)\cdot\tau(\chi)^r \;=\; \tau(\chi)\cdot\tau^{(r)}(\chi^{(r)}) \\
&= \sum_{\substack{x\neq 0 \text{ in } k\\ y\neq 0 \text{ in } k^{(r)}}} \chi(x\,N_r(y))\cdot e(x+S_r(y))\,, \\
&\qquad\qquad \text{where } \left\{\begin{array}{lll} k & \text{the field of } q=p^f \text{ elements} \\ k^{(r)} & \text{''}\quad\text{''}\quad\text{''}\quad q^r & \text{''} \\ N_r & \text{''}\quad \text{norm for } k^{(r)}/k \\ S_r & \text{''}\quad \text{spur}\quad \text{''}\quad\text{''}\quad\text{''} \end{array}\right. \\
&= \sum_{u\neq 0 \text{ in } k} \chi(u) \sum_{v \bmod p} \mathsf{Z}^v M(u,v)\,,
\end{aligned}$$

where $\mathsf{Z} = e^{\frac{2\pi i}{p}}$, and $M(u,\,v)$ denotes the number of solutions y of

$$\mathfrak{S}\left(S_r(y)+\frac{u}{N_r(y)}\right) = v, \qquad y\neq 0 \quad \text{in} \quad k^{(r)}. \tag{1}$$

$\mathfrak{S}$ denotes the absolute spur for k.

On the other hand,

$$\begin{aligned}\tau^{(r+1)}(\chi^{(r+1)}) &= -\sum_{z\neq 0 \text{ in } k^{(r+1)}} \chi(N_{r+1}(z))\, e(S_{r+1}(z)) \\ &= -\sum_{u\neq 0 \text{ in } k} \chi(u) \sum_{v \bmod p} \mathsf{Z}^v\, N(u,v)\,,\end{aligned}$$

where $N(u,v)$ denotes the number of solutions z of

$$(2) \qquad \mathfrak{S}(S_{r+1}(z)) = v \qquad \text{with} \quad N_{r+1}(z) = u\,, \qquad z\neq 0 \quad \text{in} \quad k^{(r+1)}.$$

Hence

$$\tau(\chi)^{r+1} - \tau^{(r+1)}\left(\chi^{(r+1)}\right) = \sum_{u\neq 0 \text{ in } k} \chi^{(u)} \sum_{v \bmod p} \mathsf{Z}^v \left(M(u,\,v) + N(u,v)\right)\,.$$

The relation will be proved for $r+1$, when the sum $M(u,v)+N(u,v)$ is shown to be independent of *either* u *or* v. The following argument proves at once its independence of *both* u and v.

(1) and (2) may be written as:

$$(1') \quad f_u(y) = \textstyle\sum_{i=0}^{f-1} \left(y + y^q + \cdots + y^{q^{r-1}} + \frac{u}{y^{1+q+\cdots+q^{r-1}}}\right)^{p^i} = v\,,$$

$$y \neq 0 \text{ in } k^{(r)}$$

$$(2') \quad f_u(z) = \textstyle\sum_{i=0}^{f-1} \left(z + z^q + \cdots + z^{q^{r-1}} + \frac{u}{z^{1+q+\cdots+q^{r-1}}}\right)^{p^i} = v$$

$$\text{with} \quad z^{1+q+\cdots+q^r} = u\,, \quad z\neq 0 \text{ in } k^{(r)}$$

with the same rational function $f_u(t)$ on the left–hand sides. This $f_u(t)$ becomes a polynomial in t with absolute term $\neq 0$ by multiplying with the highest denominator $t^{p^{f-1}(1+q+\cdots+q^{r-1})}$, and the degree of this polynomial is

$$\begin{aligned}p^{f-1}\left(q^{r-1} + (1+q+\cdots+q^{r-1})\right) &= \frac{1}{p}\left(q^r + (q+q^2+\cdots+q^r)\right) \\ &= \frac{1}{p}\Big((q^r-1) + (1+q+\cdots+q^r)\Big)\,.\end{aligned}$$

Since each common solution $t = y = z$ of (1') and (2') belongs to $k^{(r)}$ and hence satisfies $t^{q^r} = t$, each such common solution annuls the derivative

$$f'_u(t) \;=\; 1 - \frac{u}{t^{(1+q+\cdots+q^{r-1})+1}}\,,$$

and therefore is at least a double root of $f_u(t) - v = 0$. Hence $M(u,\, v) + N(u,\, v)$ is less than or equal to the number of all linear factors $\neq t$ of $f_u(t) - v$, i. e., less than or equal to the above degree:

$$(3) \qquad M(u,\, v) + N(u,\, v) \leq \frac{1}{p}\Big((q^r - 1) + (1 + q + \cdots + q^r)\Big)\,.$$

On the other hand the number of arguments y available in (1') is $q^r - 1$, and the number of arguments z available in (2') is $1 + q + \cdots + q^r$, the latter since $z^{1+q+\cdots+q^r} = u$ has exactly $1 + q + \cdots + q^r$ solutions z in $k^{(r+1)}$. Hence

$$(4) \qquad \sum_{v \bmod . p} \Big(M(u,\, v) + N(u,\, v)\Big) = (q^r - 1) + (1 + q + \cdots + q^r)\,.$$

(3) and (4) together give:

$$M(u,\, v) + N(u,\, v) \;=\; (q^r - 1) + (1 + q + \cdots + q^r)\,, \quad \text{independent of } u \text{ and } v\,,$$

q. e. d.

It rather surprised me that induction for r works in this proof. It seems so unnatural from the first look.

Schmid tried hard to prove the other relation by a similar procedure but did not succeed yet.

We are going to Marburg to–morrow afternoon and will be back on Sunday night. We are looking forward to seeing you towards the end of July. We shall be very kind to you and try making you forget all you have been through.

Much love,

Yours,
Helmut

1.96 04.10.1935, Davenport to Hasse

D. thanks for splendid time in Göttingen. Meromorphisms. Heilbronn.

4 October 1935.

My dear Helmut,

I am sorry I have not written to you directly sooner, but I have had little to say. The ten days I was at home I did no mathematics, + in the week I have been here all I have done has been to get a little more familiar with the elliptic case.

I have found it quite easy to prove that if $k = E_p$ itself, and not a higher field, then every meromorphism is of the form $a + b\pi$ where a, b are integers; and the same method proves that π is algebraic (if $k = E_p$). The fact is that a necessary + suff. condition for all meromorphisms to be of the form $a + b\pi$ is that for all meromorphisms μ, $\dfrac{dx_\mu}{y_\mu} = c\dfrac{dx}{y}$ where c is a rational integer. I feel doubtful whether this is true if $k \neq E_p$.

Did you solve the cipher problem in last week's Statesman? I was unable to do so.

Heilbronn is quite settled here, and we spend a good deal of time together.

My very heartiest thanks for the splendid time I had in Göttingen.

All good wishes from

Harold.

1.97 09.10.1935, Hasse to Davenport

On D.'s communication about meromorphisms in the elliptic case. Number of solutions for multiplication with n. Watson in Gö.

9. 10. 35

My dear Harold,

Thanks very much for your kind letter. I am very glad you are spending some further energy on the subject of our common interest.

Your communication about the nature of the meromorphisms in the elliptic case seems to me extremely important and interesting. I should very much know to have a more detailed account of your proofs. I do not quite see how you can get back from the behaviour of the differentials to the elements of the field. For in $\frac{\mathrm{d}x_\mu}{y_\mu} = c_\mu \frac{\mathrm{d}x}{y}$ the element c_μ is by no means an ordinary integer (when $k = E_p$), but only an element of E_p, i. e. an integer mod. p. If your argument is consistent, it will enable me to give a considerable simplification of my proof of Riemann's hypothesis. I very much hope so.

I think I can simplify another part of that proof, namely the determination of the number of solutions of $n\mathfrak{a} = \mathfrak{u}$ for $n \not\equiv 0$ mod. p. Instead of Weber's recurrent polynomials I use the determinant

$$\begin{vmatrix} 1 & x_2 & \cdots\cdots & x_n \\ 0 & \frac{\mathrm{d}x_2}{\mathrm{d}x} & \cdots\cdots & \frac{\mathrm{d}x_n}{\mathrm{d}x} \\ \cdots & \cdots\cdots & \cdots\cdots & \cdots\cdots \\ 0 & \frac{1}{(n-1)!}\frac{\mathrm{d}^{n-1}x_2}{\mathrm{d}x^{n-1}} & \cdots\cdots & \frac{1}{(n-1)!}\frac{\mathrm{d}^{n-1}x_n}{\mathrm{d}x^{n-1}} \end{vmatrix}$$

where $1, x_2, \ldots, x_n$ is a basis for the integral multipla of $\frac{1}{\mathcal{O}^n}$, say 1, x, y, x^2, xy, $y^2, \ldots$ I have not quite surmounted the difficulties arising for $n > p$ in handling the higher differential coefficients.

I have not solved last week's Caliban problem either. I must forego this pleasant hobby for a while, since I am extremely busy in mathematics. I just think about the generalisation of the determination of the number of solutions of $p\mathfrak{a} = \mathfrak{u}$ to higher genus g.

I have not finished Bleak House yet. There are still 100 pages left. I try to put in a chapter every day.

I am sorry to hear Grace is not well. Let us hope she will recover soon.

Kindest regards to all people I know there, yourself included,

Yours,

Helmut

P.S. Watson and wife have been here for three days. They stayed in the Institute and were our guests several times. I had already met them at the Stuttgart meeting of the DMV.

1.98 16.10.1935, Davenport to Hasse

On meromorphisms in elliptic case. Question of addition theorem for $g > 1$.

16 October 1935

My dear Helmut,

Very many thanks for your letter. I send the proof you asked for, + hope it is correct. As you see, I have not been able to prove that every μ is $a + b\pi$, but only that $c\mu = a + b\pi$. There was a mistake in the step from the latter to the former, + I can't see how to do this part, but am considering it.

It seems to me that we have two possible defns. of a meromorphism, (1) defining a mer. for a particular equation $y^2 = 4x^3 - g_2x - g_3$ (2) defining a mer. as a rational operation with coeffts depending on g_2, g_3 rationally, valid for all equations (or all equations for which the rational operation has a sense). Perhaps this distinction is of importance in the higher Galois Fields. It might be that there are meromorphisms of the first kind for which $\frac{dx_\mu}{y_\mu} = c\frac{dx}{y}$ (c not in E_p) in which case μ is not of the form $a + b\pi$. At present however I can prove nothing at all about the higher Galois fields.

Grace is quite better now, I understand. I hope Clärle's improvement in health continues.

I expect Clärle has told you that I have got a new Riley – a saloon, which is perhaps an advantage in winter; I don't know how I shall like it in summer.

Glad to hear you are persevering with Bleak House.

Have you cleared up the question of the addition theorem (for what pairs of solns it is unique etc.) in the case of genus > 1.?

Very best wishes + much love to both from

Harold.

1.99 16.10.1935, Hasse to Davenport

New and simpler proof for the number of n-division points. This is not trivial!

Göttingen, den
16. 10. 35

My dear Harold,

I have now finished my new and simpler proof for the number n^2 of prime divisors $\mathfrak{p}$ with $\frac{\mathfrak{p}^n}{\mathfrak{o}^n} \sim 1$, $\quad n \not\equiv 0 \bmod. p$.

It bases on the following remarks about higher differential coefficients. For a power series

$$x = x(\pi) = \sum_{\mu} a_\mu \pi^\mu\,, \qquad \text{with coefficients in any field } k\,,$$

I define

$$D_\pi^{(k)} x \;=\; \sum_{\mu} \binom{\mu}{k} a_\mu \pi^{\mu-k},$$

or $D_\pi^{(k)} x$ is the coefficient of t^k (t an indeterminate) in

$$(1) \qquad x(\pi + t) \;=\; \sum_{k=0}^{\infty} \left(D_\pi^{(k)} x\right) t^k \;=\; x + \sum_{k=1}^{\infty} \left(D_\pi^{(k)} x\right) t^k\,.$$

Notice that $D_\pi^{(k)} x\Big|_{\pi=0} = 0$ for $k = 0, \ldots, \mu - 1$ $\quad D_\pi^{(\mu)} x\Big|_{\pi=0} \neq 0$ is a necessary and sufficient condition for x vanishing of the μ^{th} order for $\pi = 0$, *unrestricted by any "characteristic" condition for the field k of coefficients.* (Here x must be an integral power series, whereas for the definition of $D_\pi^{(k)} x$ there may as well be a finite number of negative exponents.)

I further define, for a polynomial

$$f(x,\, y) = \sum_{m,n} a_{mn} x^m y^n\,, \qquad \text{with coefficients in } k\,,$$

the partial derivatives by

$$\Delta_{\mu,\nu}^{(\mu+\nu)} f \ = \ \sum_{m,n} \binom{m}{\mu}\binom{n}{\nu} a_{mn} x^{m-\mu} y^{n-\nu} \,,$$

or $\Delta_{\mu,\nu}^{(\mu+\nu)} f$ is the coefficient of $t^\mu u^\nu$ (u, v indeterminates) in

$$f(x+u,\, y+v) \ = \ \sum_{\mu,\nu=0}^{\infty} \left(\Delta_{\mu,\nu}^{(\mu+\nu)} f\right) \, u^\mu v^\nu \,. \tag{2}$$

Then from (1) and (2),

$$\sum_{k=0}^{\infty} \left(D_\pi^{(k)} f\right) \, t^k \ = \ \sum_{\mu,\nu=0}^{\infty} \left(\Delta_{\mu,\nu}^{(\mu+\nu)} f\right) \left(\sum_{r=1}^{\infty} \left(D_\pi^{(r)} x\right) \, t^r \right)^{\mu} \cdot \tag{3}$$

$$\cdot \left(\sum_{s=1}^{\infty} \left(D_\pi^{(s)} y\right) \, t^s \right)^{\nu}$$

for any power series $x = x(\pi)$, $y = y(\pi)$, where on the left–hand side f means the power series $f(x(\pi),\, y(\pi))$.

Let now x, y be elements of an algebraic function field K over the constant field k, and $f(x,\, y) = 0$ a polynomial relation between them, with $\frac{\partial f}{\partial y} = \Delta_{0,1}^{(1)} f \neq 0$ (as an element of K). Such an equation exists for every given y with a suitably chosen x (x must be chosen such that $\mathrm{d}x \neq 0$, or $K/k(x)$ separable). For k a finite or infinite Galois field, this condition is automatically satisfied if $f(x,\, y) = 0$ is the *irreducible* equation between x and y. Let further $\mathfrak{p}$ be any prime divisor of degree 1 of K and π a (local) prime element for $\mathfrak{p}$, so that all elements of K have unique developments $x = x(\pi),\ y = y(\pi)$ into power series with coefficients in k.

Then (3) gives the identity (since all $D^{(k)} f = 0$):

$$\sum_{\mu,\nu} \left(\Delta_{\mu,\nu}^{(\mu+\nu)} f\right) \left(\sum_{r=1}^{\infty} \left(D_\pi^{(r)} x\right) \, t^r \right)^{\mu} \left(\sum_{s=1}^{\infty} \left(D_\pi^{(s)} y\right) \, t^s \right)^{\nu} \ = \ 0 \,. \tag{4}$$

I need not bother about the explicit formulae arising from this by equating coefficients in t, which are the equivalent of the well–known formulae for total differentiation. I need however the explicit formulae arising by solving these latter formulae with respect to the $D_\pi^{(s)}$. As I once told you, it took me a great deal of trouble to get this solution. To–day, I am able to do it in a very simple and elegant way.

The equation

$$\sum_{\mu,\nu=0}^{\infty} \left(\Delta_{\mu,\nu}^{(\mu+\nu)} f\right) u^\mu v^\nu \;=\; 0$$

with $\Delta_{0,0}^{(0)} f \;=\; f \;=\; 0$ and $\Delta_{0,1}^{(1)} f \;=\; \frac{\partial f}{\partial y} \neq 0$ has, as is well–known, a unique solution with respect to v of the form

$$v \;=\; \sum_{\mu=1}^{\infty} (\Delta_\mu f)\; u^\mu$$

with coefficients $\Delta_\mu f$, which are rational functions of the $\Delta_{\mu,\nu}^{(\mu+\nu)} f$ and contain only $\frac{\partial f}{\partial y}$ in their denominators. With those coefficients $\Delta_\mu f$, one has from (4)

$$\sum_{s=1}^{\infty} \left(D_\pi^{(s)} y\right) t^s \;=\; \sum_{\mu=1}^{\infty} (\Delta_\mu f) \left(\sum_{r=1}^{\infty} \left(D_\pi^{(r)} x\right) t^r\right)^{\mu} . \tag{5}$$

Equating coefficients in t, one is led to

$$D_\pi^{(k)} y \;=\; \sum_{\substack{\varrho_1,\varrho_2,\cdots,\varrho_k=0 \\ \varrho_1+2\varrho_2+\cdots+k\varrho_k=k}}^{k} (\Delta_{\varrho_1+\cdots+\varrho_k} f)\, \frac{(\varrho_1+\cdots+\varrho_k)!}{\varrho_1!\cdots\varrho_k!} \left(D_\pi^{(1)} x\right)^{\varrho_1} \cdots \tag{5'}$$

$$\cdots \left(D_\pi^{(k)} x\right)^{\varrho_k} .$$

This is the solution required.

Note. Taking formally $\pi = x$, hence $D_x^{(1)} x = 1$; $D_x^{(2)} x, \ldots = 0$ one has

$$D_x^{(k)} y \;=\; \Delta_k f .$$

Hence the $\Delta_k f$ are the representations of the higher differential coefficients of the algebraic function $y(x)$ by the partial derivations of $f(x, y)$.

Further Note. Do not think all this is trivial and contained in elementary books. For it is *not.* Left alone that I could not find a book that contained the explicit formulae (5'), even if such a book existed, it would have been of no use whatsoever. For it surely will prove them by using the *recurrent* definitions

$$\frac{1}{(k+1)!}\,\frac{\mathrm{d}^{k+1}x}{\mathrm{d}\pi^{k+1}} \;=\; \frac{1}{k+1}\,\frac{\mathrm{d}}{\mathrm{d}\pi}\left(\frac{1}{k!}\,\frac{\mathrm{d}^k x}{\mathrm{d}\pi^k}\right), \quad \text{i. e.} \quad D_\pi^{k+1}x \;=\; \frac{1}{k+1}\,D^{(1)}D_\pi^{(k)}x$$

$$\Delta_{\mu+1,\nu}^{(\mu+1+\nu)}\,f \;=\; \frac{1}{\mu+1}\,\frac{\partial}{\partial x}\,\big(\Delta_{\mu,\nu}^{(\mu+\nu)}\,f\big)$$

which are senseless for fields of prime characteristic p, when $k+1,\ \mu+1\equiv 0$ mod. p.

I only require the following structure of the formulae (5')

$$D_\pi^{(k)}y \;=\; (\Delta_k f)\left(\frac{\mathrm{d}x}{\mathrm{d}\pi}\right)^k + \sum_{\lambda=1}^{k-1}(\Delta_\lambda f)\,X_\pi^{(k,\lambda)}\,, \tag{5"}$$

where $D_\pi^{(1)}x=\frac{\mathrm{d}x}{\mathrm{d}\pi}$ and the $X_\pi^{(k,\lambda)}$ are polynomials in $D_\pi^{(1)}x,\dots,D_\pi^{(k)}x$ with integer coefficients. From this structure it becomes obvious that the $D_\pi^{(2)}x,\dots,$ $D_\pi^{(k)}x$ (in fact the terms $X_\pi^{(k,\lambda)}$) may be disregarded in a determinant

$$|D_\pi^{(k)}\,y_i| \;=\; \begin{vmatrix} D_\pi^{(1)}y_1 & \cdots\cdots & D_\pi^{(n)}y_1 \\ \cdots\cdots & \cdots\cdots & \cdots\cdots \\ \cdots\cdots & \cdots\cdots & \cdots\cdots \\ D_\pi^{(1)}y_n & \cdots\cdots & D_\pi^{(n)}y_n \end{vmatrix}$$

for n elements $y_1,\dots,y_n$ of K. Hence the following **fundamental theorem**:

Let K be an algebraic function field over the constant field k of any characteristic, and let x be an element of K such that $K/k(x)$ is separable, i. e. $\mathrm{d}x\neq 0$; let further $y_1,\dots,y_n$ be any elements of K. Let finally be $\mathfrak{p}$ any prime divisor of degree 1 of K and π a (local) prime element for $\mathfrak{p}$. Then

$$\frac{|D_\pi^{(k)}y_i|}{\left(\frac{\mathrm{d}x}{\mathrm{d}\pi}\right)^{1+2+\cdots+n}} \qquad (i,\,k=1,\dots,n)$$

is an element of K. It is independent of the choice of $\mathfrak{p},\pi$.

It may therefore be denoted by

$$\frac{|D^{(k)}y_i|}{(\mathrm{d}x)^{1+2+\cdots+n}}$$

In fact

$$\frac{|D^{(k)} y_i|}{(\mathrm{d}x)^{1+2+\cdots+n}} \;=\; |\Delta_k f_i|\,,$$

where the f_i are polynomials such that $f(x,\,y_i) = 0\,,\quad \frac{\partial f}{\partial y_i} = 0\,.$ They may be *any* such polynomials (not necessarily the irreducible), since the left–hand side — already for *one* fixed system $\mathfrak{p}$, π — shows the independence of the element in question of the choice of the f_i.

(To be continued)

Much love,

Yours,
Helmut

1.100 18.10.1935, Davenport to Hasse

Is $\deg n = n^2$ $(p \nmid n)$ *so difficult? Refers to Weber. Meromorphisms.*

18 Oct 1935

My dear Helmut,

Many thanks for your letter. I understand it so far as it goes, but do not see the application to $n\mathfrak{A} = \mathfrak{U}$. Is the result $\deg n = n^2$ $(p \nmid n)$ so difficult? It seems to me one reaches it by the Weber method for $\wp(nu)$ very easily (middle of p. 198 vol 3) without the recurrence formulae, and surely it is clear that x_n is the same fn. of x, g_2, g_3 as $\wp(nu)$ is of $\wp(u), g_2, g_3$ because both follow from the same addition formula.

As regards my letter I forgot to add that it follows from the rep. $n\mu = a + b\pi$ that all meromorphisms commute (of course $k = E_p$). By the way, it is not true that every n.mer. is representable as $a + b\pi$. For example, if e_1, e_2, e_3 are in E_p, there is a n.mer. μ such that $\pi - 1 = 2\mu$, and μ is clearly not representable as $a + b\pi$. Nor is 2 any exception here.

Much love to both, Yours

Harold

1.101 20.10.1935, Davenport to Hasse, postcard

Meromorphisms are commutative.

Sunday 20 Oct. (*1935*)

My dear Helmut,

Here is a general proof that meromorphisms are commutative. I look forward to hearing from you whether my previous MS was correct; of course, if it wasn't then this isnt either.

Yours

Harold.

1.102 21.10.1935(?), Davenport to Hasse, postcard

Proof which D. sent yesterday is wrong.

Monday 21 Oct.

My dear Helmut,

Many thanks for your MS, which I will study. I made an obvious blunder in my MS I sent yesterday. For $k > E_p$ it is not true that $\frac{dx_\mu}{y_\mu} = 0$ implies $\mu = \pi\nu$. So the proof is quite wrong. I still have a little hope of putting it right.

Yours

Harold.

1.103 21.10.1935, Hasse to Davenport

Norm inequality for meromorphisms. H. has achieved total elimination of any normal form. Weber's arguments are not applicable. Thanks for D.'s proof that π is algebraic. Detailed discussion of R.H. for elliptic case.

Göttingen, den
21. 10. 35

My dear Harold,

Thanks very much for your kind letter and post–card. Your proofs for the case $k = E_p$ are perfectly correct. Please excuse my doubting them first. I find them extremely nice and important. I very much hope that the difficulty with c_μ in $\frac{\mathrm{d}x_\mu}{y_\mu} = c_\mu \frac{\mathrm{d}x}{y}$ for $k = E_{q^r}$ or E_{q^∞} will be overcome soon. For the time being I have given up my efforts to prove c_μ is always in E_p.

Let me add some remarks to the whole thing.

1.) *The inequality* $|\mu_1 + \mu_2| \leq 2|\mu_1| + 2|\mu_2|$.

First of all I quite appreciate your very convenient notation. This inequality ought to be proved properly. I am sure your proof — I never saw or heard it — is quite correct. I have given the following proof, using divisors instead of rational functions:

Let us assume $\mu_1 \neq \pm\mu_2$. Then (in an obvious notation)

$$\overline{x} \;=\; -x_1 - x_2 + \frac{1}{4}\left(\frac{y_1 - y_2}{x_1 - x_2}\right)^2 .$$

Now one easily sees that the following 2 facts are true:

(1.) When a prime divisor $\mathfrak{q}$ divides the denominators of x_1, y_1, say, then it divides them to the exact exponents 2α, 3α with a certain α

(2.) Let $\mathfrak{p}$, $\overline{\mathfrak{p}}$ be a pair of conjugate prime divisors with respect to x (i. e. the two prime divisors of the numerator of a linear factor $x - a$). ($\overline{\mathfrak{p}} \neq \mathfrak{p}$ or $\overline{\mathfrak{p}} = \mathfrak{p}$). When $\mathfrak{p}^\alpha$ divides the numerator of $x_1 - x_2$, then also $\overline{\mathfrak{p}}^\alpha$ divides this numerator, and one of them, say $\overline{\mathfrak{p}}^\alpha$, divides the numerator of $y_1 - y_2$.

From (1.) one has — all German letters denote integral divisors —

$$x_1 \cong \frac{\mathfrak{a}_1}{\mathfrak{u}_1^2} \quad , \quad y_1 \cong \frac{\mathfrak{b}_1}{\mathfrak{u}_1^3}$$
$$x_2 \cong \frac{\mathfrak{a}_2}{\mathfrak{u}_2^2} \quad , \quad y_2 \cong \frac{\mathfrak{b}_2}{\mathfrak{u}_2^3} \, ,$$

and from (2.) one has

$$x_1 - x_2 \cong \frac{\mathfrak{d}\overline{\mathfrak{d}}}{\mathfrak{u}_1^2\mathfrak{u}_2^2} \, , \qquad y_1 - y_2 \cong \frac{\overline{\mathfrak{d}}\mathfrak{e}}{\mathfrak{u}_1^3\mathfrak{u}_2^3} \, .$$

Hence

$$\overline{x} \;=\; -x_1 - x_2 + \frac{1}{4}\left(\frac{y_1 - y_2}{x_1 - x_2}\right)^2 \;=\; \frac{\mathfrak{g}}{\mathfrak{d}^2\mathfrak{u}_1^2\mathfrak{u}_2^2} \, .$$

Now

$$\deg \mathfrak{u}_1 \;=\; |\mu_1| \qquad \deg \mathfrak{u}_2 \;=\; |\mu_2|$$

and

$$\deg \mathfrak{d}\overline{\mathfrak{d}} \;=\; \deg \mathfrak{u}_1^2\mathfrak{u}_2^2 \, , \qquad \text{hence } \deg \mathfrak{d} \;=\; \deg \mathfrak{u}_1\mathfrak{u}_2 \;=\; \deg \mathfrak{u}_1 + \deg \mathfrak{u}_2 \, .$$

Therefore

$$|\mu_1 + \mu_2| \;=\; \deg \mathfrak{d}\mathfrak{u}_1\mathfrak{u}_2 \leq 2\deg \mathfrak{u}_1 + 2\deg \mathfrak{u}_2 \, .$$

I have omitted obvious considerations as to common divisors of $\mathfrak{u}_1$, $\mathfrak{u}_2$.

2.) You will have got my proof for $h_n = \left\{ \begin{array}{ll} n^2 \, , & n \not\equiv 0 \text{ mod.}\, p \\ p^\nu \, , & n = p^\nu \end{array} \right\}$ in the mean time. What I have achieved with it is the *total elimination of any normal form.* This seems to me of high importance. For I have *no* hope of mastering the case $g > 1$ by discussing the degrees in the rational functions of the addition formulae.

I do not see how you will get at the same results by an argument like that on your post–card (Weber III, middle of p. 198). For, first of all, it is not obvious that the rational function x_n of x ($\wp(nu)$ of $\wp(u)$) has not p in the denominator of its numerical coefficients, and second, one can*not* argue as Weber does with his function $\psi_n(u)$. This would be a "petitio principii", for this function is based upon the "transcendental" knowledge that there are exactly n^2 essentially diff. *point*s $u = a_{\nu_1\nu_2} = \frac{\nu_1\omega_1 + \nu_2\omega_2}{n}$ with $nu \equiv 0 \text{ mod.}\,(\omega_1, \omega_2)$, and the algebraic analogue of this is, that there are exactly n^2 different *prime divisors* $\mathfrak{p}_i$ with $\mathfrak{p}^n \sim \mathfrak{o}^n$ (or solutions $\mathfrak{a}_i$ of $n\mathfrak{a}_i = \mathfrak{u}$). The only way of applying the idea behind this proof of Weber's is the one indicated in my Hamburg paper, i. e., going into the rational recurrent process for the construction of the P_n (in Weber's notation). I hope my very short and strict argument, based on higher differentials, particularly short for $n \not\equiv 0 \bmod. p$, will convince you, that this new method is better. But my main point is, as you will realize, that it seems by far more appropriate for the generalisation to higher g. This is what I am just thinking about; and also a definition of the meromorphisms and a development of their properties which does not base on a normal form or explicit addition formulae.

I know, of course, that your intention — at least so far as the elliptic case is concerned — lies rather in the opposite direction: to "rationalize" the whole argument. Naturally, a purely formal algebraic proof (with only rational functions and their degrees) in the elliptic case would be rather nice to have, even if only to see it is possible without too many complicated formulae.

3.) Your new method leads very nicely to the fact: "π satisfies an algebraic equation" in the case $k = E_p$. From this the way is not long to Riemann's hypothesis. I will indicate it in short. It is the way I took originally, but discovered a flaw in my direct proof for the algebraicity of π later.

If π is algebraic, it is necessarily an algebraic *integer*, i. e., satisfies at least one algebraic equation with highest coefficient 1 and integer coefficients (the irreducible equation has this property). For suppose this was not the case. Then consider the algebraic field generated by the irreducible equation satisfied by π. In this field, as in every algebraic number field, to every *fractional* number α there exists a polynomial $f(\alpha)$ with integer coefficients and an integer $s > 1$ such that $sf(\alpha) = 1$. The proof is an easy generalisation of the following argument for the rational number field: Let $\alpha = \frac{r}{s}$ be a fractional rational number, $(r, s) = 1$. Then $rr' \equiv 1 \bmod. s$, $f(\alpha) = r'\alpha - \frac{rr'-1}{s}$, $sf(\alpha) = 1$. This gives a contradiction for $\alpha = \pi$.

Further, by the same reason, the algebraic number field generated by the irreducible equation for π, is either the rational number field, or imaginary quadratic (Dirichlet's theorem on algebraic units).

Therefore π satisfies necessarily an equation

$$\pi^2 - v\pi + p \;=\; 0$$

with an integer π.

It remains to prove that $v = p+1-h$, where h is the number of solutions $\mathfrak{a}$ in E_p.

Now the $\mathfrak{a}$ are characterised by

$$(\pi - 1)\mathfrak{a} \;=\; \mathfrak{u}\,.$$

Consider the additive group of all solutions $\overline{\mathfrak{a}}$ in E_{p^∞}. Its structure is known by the theorem in my Hamburg paper or in my last Ms. Every $\overline{\mathfrak{a}}$ has a unique decomposition

$$\overline{\mathfrak{a}} \;=\; \overline{\mathfrak{a}}_p + \sum_{q \neq p} \overline{\mathfrak{a}}_q\,,$$

where only a finite number of $\overline{\mathfrak{a}}_q \neq \mathfrak{u}$, and $\overline{\mathfrak{a}}_p$, $\overline{\mathfrak{a}}_q$ have (additive) orders a power of p, a power of q.

The group of the $\overline{\mathfrak{a}}_p$ is of type: all rational integers r mod. 1, with denominator a power of p.

The groups of the $\overline{\mathfrak{a}}_q$ are of type: all pairs of rational integers $\binom{r_1}{r_2}$ mod. 1, with denominators a power of q. Correspondingly

$$h \;=\; h_p \prod_{q \neq p} h_q\,,$$

where only a finite number of the $h_q \neq 1$.

h_q is the number of solutions of $(\pi - 1)\mathfrak{a}_q = \mathfrak{u}$ with $\mathfrak{a}_q$ an $\overline{\mathfrak{a}}_q$; h_p of $(\pi - 1)\mathfrak{a}_p = \mathfrak{u}$ with $\mathfrak{a}_p$ an $\overline{\mathfrak{a}}_p$.

<u>**(1.)** $\quad h_p \leq (p+1-v)_p$</u> $\qquad$ (power of p contained in $\cdots$)

Proof.

$$\overline{\pi}\mathfrak{a}_p - v\mathfrak{a}_p + \pi\mathfrak{a}_p \;=\; \mathfrak{u}$$

gives

$$(p - v + 1)\mathfrak{a}_p \;=\; \mathfrak{u}\,.$$

Now the group of the $\mathfrak{a}_p$ is *cyclic.* Hence

$$h_p \leq (p+1-v)_p\,,$$

for h_p is the *least* power of p annihilating every $\mathfrak{a}_p$.

<u>**(2.)** $\quad h_p \;=\; (p+1-v)_q$</u>.

Proof. Consider the application of π to the $\overline{\mathfrak{a}}_q$ isomorphically represented by the $\binom{r_1}{r_2}$ mod. 1. One easily sees that this application is described by a matrix $M_q = \begin{pmatrix} a & b \\ c & d \end{pmatrix}$ of q–adic integers such that

$$\pi\mathfrak{a}_q \quad \text{corr. to} \quad M_q\begin{pmatrix} r_1 \\ r_2 \end{pmatrix} \equiv \begin{pmatrix} ar_1 + br_2 \\ cr_1 + dr_2 \end{pmatrix} \text{ mod. } 1\,,$$

$$\text{when} \quad \overline{\mathfrak{a}}_q \quad \text{corr. to} \quad \begin{pmatrix} r_1 \\ r_2 \end{pmatrix} \text{ mod. } 1\,.$$

(Verify this first for the sub–group of all $\overline{\mathfrak{a}}_q$ with $q^N\overline{\mathfrak{a}}_q = \mathfrak{u}$; then $M_q^{(N)}$ is a matrix mod. q^N; and with increasing N the matrix $M_q^{(N)}$ converges q–adically.)

Now

$$(\pi - 1)\mathfrak{a}_q \;=\; \mathfrak{u} \qquad \text{corr. to} \qquad (M_q - E)\begin{pmatrix} r_1 \\ r_2 \end{pmatrix} \equiv \begin{pmatrix} 0 \\ 0 \end{pmatrix} \text{ mod. } 1$$

The number of solutions of the congruence ($\uparrow$) is obviously equal to the power of q contained in $|M_q - E|$.

Now let $\overline{M}_q$ be the matrix corr. to $\overline{\pi}$. Then

$$\begin{array}{rlcl} \pi + \overline{\pi} = v & \text{corr. to} \;\; M_q + \overline{M}_q & = & vE \\ \pi\overline{\pi} = p & \text{corr. to} \;\;\; M_q\overline{M}_q & = & pE \end{array}$$

Hence

$$t^2 - vt + p \;=\; |M_q - tE|$$

and therefore

$$1 - v + p \;=\; |M_q - E|\,.$$

This gives (2.).

A similar argument leads to

$$\begin{array}{lrcl} (1.\text{'}) & h'_p & \leq & (p+1+v)_p \\ (2.\text{'}) & h'_q & = & (p+1+v)_q\,, \end{array}$$

for the "conjugate" class–numbers.

From all those relations:

$$\begin{aligned} h &\leq p+1-v \\ h' &\leq p+1+v \end{aligned}$$

Since $h + h' = 2(p+1)$, one has equality throughout, **q. e. d.**

Please excuse my disorderly writing. I am in a great hurry on account of many duties, as usual.

But I thought, all this would interest you.

I have finished "Bleak House". I particularly liked the great scene with Bucket and Sir Leicester.

Much love. I hope to hear from you soon.

Yours,
Helmut

1.104 22.10.1935, Davenport to Hasse, post-card

D.'s proof is easily corrected.

22 Oct. 1935.

My dear Helmut,

My proof is easily corrected. Instead of $\mu = A(\tau) + \pi\mu_1$ read:

$$p^{f-1}\mu = p^{f-1}A(\tau) + \pi\mu_1 \qquad (k = E_{p^f}).$$

Then everything is O.K. I think one can also deduce that all n.mer. are algebraic, but have not time to do this today.

Yours

Harold.

1.105 22.10.1935, Davenport to Hasse, postcard

Unfortunately, the proof cannot be corrected easily.

22 Oct 1935.

My dear Helmut,

Unfortunately the mistake cannot so easily be corrected. One will have to study the rational operations which transform a solution of $y^2 = 4x^3 - g_2x - g_3$ into a soln of $y^2 = 4x^3 - g_2x - \overline{g_3}$ where g_2, g_3 is a pair of conjugates of g_2, g_3. I have not yet been able to do this.

Yours

Harold

1.106 23.10.1935, Hasse to Davenport

H. received D.'s second communication on normalized meromorphisms. Whole idea seems sound. May lead to commutativity and algebraicity. H. thinking intensely about higher genus. Referring to Siegel.

Göttingen, den
23.10.1935

My dear Harold,

I received your second communication on n.Mer. Unfortunately, there seems to be a flaw in it. When a n.mer. μ has coeff. 0, it does not follow that μ is divisible by π, because π now means the power $q = p^f$ and not only p. Of course μ^f is divisible by π.

But the whole idea seems sound, and I think with due consideration to my above remark you will be able to prove the commutativity and algebraicity at once. Then the non–existence of units leads at once to:

The n.mer. are formally algebraic integers from a fixed imaginary quadratic field.

Also Riemann's hypothesis follows by purely formal considerations, as I pointed out in my last letter.

You will perhaps be interested in the enclosed copy.

I am thinking rather intensely on a method of approaching higher genus. Siegel's paper gives one certain ideas how to proceed.

Much love,

Yours,
Helmut

1.107 27.10.1935, Hasse to Davenport

H. smoothing the proofs in the elliptic case. For higher genus: Generating the field of abelian functions? Better inequality for norms of meromorphisms. Chevalley found a purely arithmetical proof for the whole class field theory, including Dirichlet's thm. for arithmetical progressions.

27. 10. 35

My dear Harold,

First of all, many happy returns and all good wishes $\left\{ \begin{array}{c} \text{at} \\ \text{to} \\ \text{for} \\ \text{on} \end{array} \right\}$ your birth–day. I hope you will continue making remarkable progress with our common problem. At present I am looking forward with the greatest interest to your further efforts on meromorphisms in the elliptic case.

As I already wrote you, I am concerned with smoothing the proofs in the elliptic case in such a way that as little use is made of a normal–form as possible. I think I can manage that quite satisfactorily. As a test for the sufficiency of my smoothing process I take the inclusion of the case $p = 2$, hitherto excluded. The only point where a completely new argument has to be introduced is the study of the automorphisms of K leaving a fixed prime divisor $\mathfrak{o}$ invariant. I have managed that part, without going into complicated discussions of a normal form and its substitutions.

All things concerned with automorphisms $S_{\mathfrak{o},\mathfrak{p}}$ (translations from $\mathfrak{o}$ to $\mathfrak{p}$), meromorphisms, addition formulae can be done without the actual formulae. My "basis" is always *any* basis x, y, z of the integral multipla of $\frac{1}{\mathfrak{o}^3}$ for a fixed prime divisor $\mathfrak{o}$, and the homogeneous relation $f(x, y, z) = 0$ of dimension 3 which "generates" K.

As to higher genus g, I have hit upon a rather surprising method. I think, with reason — the proof is not completed yet — that a field K of genus $g > 1$ with algebraically closed constant–field k may always be generated in the following form: Let $\mathfrak{o}_1, \ldots, \mathfrak{o}_g$ be a set of g different prime divisors of K, for which there is no element

$$z \cong \frac{\mathfrak{g}}{\mathfrak{o}_1 \cdots \mathfrak{o}_g}$$

in K with integral divisor $\mathfrak{g}$, except constants. (It is easy to see, that for any given $\mathfrak{o}_1$ one can choose $\mathfrak{o}_2,\ \mathfrak{o}_3, \ldots, \mathfrak{o}_g$ successively so that this is the case, and one has only a finite number of exceptions for each $\mathfrak{o}_i$. Hensel–Landsberg call that a *non–special* set. Then there exist elements

$$x_i \cong \frac{\mathfrak{g}_i}{\mathfrak{o}\mathfrak{o}_i} \quad (i = 1, \ldots, g)\,, \qquad y_i \cong \frac{\mathfrak{h}_i}{\mathfrak{o}\mathfrak{o}_i^2}$$

where $\mathfrak{o} = \mathfrak{o}_1 \cdots \mathfrak{o}_g$, with integral divisors $\mathfrak{g}_i\,, \mathfrak{h}_i$. I think now, one can choose those $x_i,\ y_i$ so that the field S of all symmetric functions of them is of genus 1, generated by

$$x = \sum_i x_i\,, \qquad y = \sum_i y_i\,, \quad \text{with denominators } \mathfrak{o}^2,\ \mathfrak{o}^3\,,$$

that further K is a Galoisien field of degree g over S, generated by the pairs $x_i,\ y_i$ which are conjugate with respect to S.

S represents the field of the Abelian functions (so far as that is possible within K). The translations of S will be essential, they represent the group of the classes of divisors in K. For as in the elliptic case, any class C of degree 0 is representable by a quotient $\frac{\mathfrak{p}}{\mathfrak{o}}$, where $\mathfrak{p} = \mathfrak{p}_1 \cdots \mathfrak{p}_g$ is a product of g prime divisors. And this expression is unique except for the classes C for which $\mathfrak{p}_1, \ldots, \mathfrak{p}_g$ turns out to be a *special* set.

I hope you will see what I aim at with all this. It sounds almost too good to be true. But I think it is. Once it is proved, one has an obvious starting point for mastering $g > 1$.

As to the inequality $|\mu + \nu| \leq 2|\mu| + 2|\nu|$, I think it ought to be replaced by the actual truth:

$$\sqrt{|\mu + \nu|} \leq \sqrt{|\mu|} + \sqrt{|\nu|}\,.$$

(Better write $|\mu|^2$ for your $|\mu|$, hence $|\mu|$ for $\sqrt{|\mu|}$ in your sense.) I think I can prove that by the same methods.

Another remark that will interest you: For Weierstrass' $\wp$–function one easily sees:

$$\wp\left(u;\ M^{-1}\binom{w_1}{w_2}\right) \;=\; \wp\left(u;\ \begin{matrix} w_1 \\ w_2 \end{matrix}\right) + {\sum_{\omega}}' \left(\wp\left(u - \omega;\ \begin{matrix} w_1 \\ w_2 \end{matrix}\right) - \wp\left(\omega;\ \begin{matrix} w_1 \\ w_2 \end{matrix}\right)\right)$$

where M is an integral matrix $\begin{pmatrix} a & b \\ c & d \end{pmatrix}$ with positive determinant m, and ω runs through a complete system of incongruent solutions of

$$\omega \equiv 0 \text{ mod. } M^{-1}\binom{\omega_1}{\omega_2}$$

with the exception of $\omega \equiv 0$ mod. $\binom{\omega_1}{\omega_2}$. Particular cases:

a.) $M = \begin{pmatrix} n & 0 \\ 0 & n \end{pmatrix}$

$$n^2\wp(nu) \; = \; \wp(u) + \sum_{\nu_1,\nu_2 \text{ mod. } n}{}' \left(\wp\left(u - \frac{\nu_1\omega_1 + \nu_2\omega_2}{n}\right) - \wp\left(\frac{\nu_1\omega_1 + \nu_2\omega_2}{n}\right)\right)$$

b.) ω_1, ω_2 imaginary–quadratic, μ a complex number with

$$\begin{aligned} \mu\binom{\omega_1}{\omega_2} &= M\binom{\omega_1}{\omega_2} \\ N(\mu) &= |M| \; = \; m \\ \mu^2\wp(\mu u) &= \wp(u) + \sum_{\omega}{}' \left(\wp(u-\omega) - \wp(\omega)\right) \end{aligned}$$

where ω runs through a complete system of incongruent solutions of

$$\mu\omega \equiv 0 \text{ mod. } \binom{\omega_1}{\omega_2}.$$

The algebraic analogue of this is obvious. Let $p \neq 2,\, 3$ and $K = k(x,\, y)$ with Weierstrass' normal form.

a.) Let $n \not\equiv 0$ mod. p, and $\mathfrak{a}_\nu$ run through the n^2 solutions of $n\mathfrak{a}_\nu = u$. Let further $x^{(\nu)}$ be the x–components of $\mathfrak{x} - \mathfrak{a}_\nu$, x_n of $n\mathfrak{x}$, $a^{(\nu)}$ of $\mathfrak{a}_\nu$. Then

$$n^2 x_n \; = \; x + \sum_{\nu}{}'(x^{(\nu)} - a^{(\nu)})$$

b.) Similarly for a normalized meromorphism μ; I do not see, however, what here takes the place of the factor μ^2.

I was interested in what happens for $n = p^k$. I found in special cases that the x–component of $p^k \mathfrak{x}^{p^{-k}}$ is the analogous sum:

$$\bar{\mathfrak{x}}_{p^k} \;=\; x + \sum_{\nu}{}' (x^{(\nu)} - a^{(\nu)}) \,,$$

without a numerical factor before it. Here may be another foundation for the whole theory, although it does not seem easy to prove directly that those sums and the corresponding for the y–components satisfy Weierstrass' equation. Anyhow, these sums take my fancy by far more than the formulae by which x_n is given from the addition theorem or Weber's recursion.

Perhaps you are also interested in them and able to do something in this line.

Now enough of mathematics. Or rather another trifle: Chevalley found a purely arithmetical proof for the whole class–field theory, including in particular Dirichlet's theorem on arithmetic progressions.

I hope you are enjoying term in Cambridge, but also longing for Germany and our company.

Best wishes and much love,

Yours,
Helmut.

1.108 28.10.1935, Davenport to Hasse

Details of proof for the structure of ring of meromorphisms.

Monday 28 Oct. 1935.

My dear Helmut,

Many thanks for your two letters. Your proof of the R.H. from the fact that π is algebraic is very nice indeed. Thank you for sending it.

As regards $|n| = n^2$, it is easy to deduce this from $|m+n| \leqq 2|m| + 2|n|$. Suppose we have a function $f(n)$ which satisfies:

$$\begin{aligned} f(mn) &= f(m)f(n), \\ f(2) &= 4, \\ f(m+n) &\leqq 2f(m) + 2f(n). \end{aligned}$$

Then it follows that $f(n) = n^2$. Heilbronn and I invented the following simple proof:

(1) If $n = 2^{k_0} + 2^{k_1} + \cdots$, $k_0 > k_1 > \cdots$, then

$$\begin{aligned} f(n) &\leqq 2^{k_0+1} + 2^{2k_1+2} + \dots \\ &\leqq 2^{2k_0+1}\left(1 + \frac{1}{2} + \frac{1}{4} + \cdots\right) \\ &\leqq 2^{2k_0+2} \leqq 4n^2. \end{aligned}$$

But now

$$f(n)^r = f(n^r) \leqq 4n^{2r}$$

and so, making $r \to \infty$, $f(n) \leqq n^2$.

(2) For any n there exists an $n' < n$ such that $n + n' = 2^k$. Then:

$$\begin{aligned} f(n) &\geqq \frac{1}{2}f(2^k) - f(n') \\ &\geqq 2^{2k-1} - n'^2 \geqq 2^{2k-1} - 2^{2k-2} \\ &= 2^{2k-2} \geqq \frac{1}{4}n^2. \end{aligned}$$

Now by the same trick $f(n) \geqq n^2$.

Hence $f(n) = n^2$.

Of course, I know that the 'structural' value of this proof is nil, but it makes what is to me a rather dull question amusing.

I think I have now got the proof of the commutativity of all n[ormalized] mer[omorphisms] and the algebraicity of π and τ (if there is any τ), into order. I show that for any μ there exist integers $a_0, \ldots, a_{f-1}$, satisfying $0 \leqq a_i < p^f$ such that

$$\mu = a_0 + a_1\tau + \ldots + a_{f_1-1}\tau^{f_1-1} + \pi\mu_1. \tag{1}$$

For this I have to start with

$$\mu = a'_0 + \cdots + a'_{f_1-1}\tau^{f_1-1} + \pi_1\rho$$

where ρ is a quasimeromorphism, ie. a pair of rat. fns x', y' of x, y which satisfy identically

$$y^2 = 4x^3 - \overline{g_2}x - \overline{g_3},$$

where $\overline{g_2}, \overline{g_3}$ is a pair of conjugates of g_2, g_3 (in fact $g_2^{p^{f-1}}, g_3^{p^{f-1}}$). $\pi_1 = (x^p, y^p)$. Then I show that quasimeromorphisms have also coefficients, the field of coeffts. being the same as that of meromorphisms. Then one can get round the cycle of f types of quasimeromorphisms + get back to μ_1.

From (1) the proof is the same as before. The algebraicity of π, τ follows from

$$(\pi^n - 1)\mu = A_0(\tau) + A_1(\tau)\pi + \ldots$$

by taking (1) $\mu = \overline{\pi}$, (2) $\mu = \tau^f$ and seeing that the result of eliminating π or τ is not an identity, which is easily done.

Thank you for your remark about π being an algebraic *integer*. I had forgotten this fact.

Will write again when I have written out my proofs.

I read a good new detective story:

Crime at Guildford, by Freeman Wills Crofts,

which you should get if it appears in Tauchnitz or Albatross.

Much love

Yours

Harold.

1.109 08.11.1935, Davenport to Hasse

Again: Details of proof. Work with Heilbronn on quadratic forms.

8 Nov 1935

My dear Helmut,

I hope you got my MS alright, and found it reasonably correct. No doubt it is possible to do it more elegantly using more 'highbrowed' language, but I wanted above all to be sure not to make too many mistakes.

The case $\overline{\pi} = \pm\pi$ is easily settled when f is odd, but I have not yet done it when f is even.

I am doing some work with Heilbronn on quadratic forms (ζ-functions connected therewith).

Are you doing any reading at present? I am just playing with 'The Seven Pillars of Wisdom', T.E. Laurence's work now published for the first time.

Very best wishes from

Harold

1.110 11.11.1935, Hasse to Davenport

H. busy writing down detailed account for the elliptic case. Witt.

Göttingen, den
11. November 1935.

My dear Harold,

thanks very much for your Ms. As I am pretty busy writing down a detailed account of the general elliptic case, without using a normal form nor the explicit addition formulae I have only looked through your Ms. rather superficially. I hope to reach the point where I have to use your very important result before long. As I have plenty of work connected with my lectures and the Institute, I hope you will not mind my delaying the examination of your paper, until I have reached the point mentioned in my own work. I hope you will agree with my intention of taking over your proof — slightly generalised — in my paper, with all due acknowledgements to its author. This shall not interfere with any plan you may have for publishing your proof wherever you like. If you could agree with publishing your proof in Crelle's Journal and have it slightly generalised so as to fit my general propositions, I could also just refer to it without giving the proof once more myself.

You see from the certainty with which I am scheming about your proof that I have not the least doubt that it is quite oke.

Thanks for your letter, too. You will hear from me again soon. For the time being I will just mention that Witt has found for complex numbers, that every field of genus g may be obtained as the composite of g elliptic fields.We are trying to free his argument from any elements from the theory of conform representation. Once this is done, we will make considerable headway towards Riemann's hypothesis.

Much love,

Y o u r s
Helmut

P. S. I enclose your Mitgliedskarte for the D. M. V. I have settled the amount with Teubner as you told me when you were here.

1.111 14.11.1935, Davenport to Hasse

Witt? On D.'s proof of commutativity and algebraicity.

14 Nov. 1935.

My dear Helmut,

Many thanks for your letter. Witt's result seems very important. Am I correct in supposing that when you say "a field of genus g" you mean "the field of all rational symmetrical functions of g pairs of indeterminate solns. of the defining equation" ? For the composite field of g elliptic fields is surely a field with g independent variables. Or have I misunderstood the meaning of 'composite'?

As regards my proof of commutativity + algebraicity, I must say that I should like to publish it in England (probably Q.J.), assuring it is O.K. Firstly, because it is definitely in my interest at present to publish many moderately good papers in England as possible; secondly because I should not like to see it buried (if you will forgive the word) in a paper whose main emphasis will not be on new results but on new exposition. You know my prejudice that a new exposition is o(a new result).

I hope I am not being selfish. Of course you are quite at liberty to incorporate my proof or your version of it in your paper. As a matter of fact it seems to me that you would be doing it quite unnecessary honour, for I cannot see how it helps with your problem of proving R.H. without normal form – from this point of view it is surely an unnecessary complication.

I haven't settled the case $\overline{\pi} = \pm\pi$ yet; it may need one new idea. But I will kill it eventually.

Much love from

Harold.

1.112 21.11.1935, Hasse to Davenport

H. has got D.'s manuscript on the norm inequality. H. has discovered the norm addition formula. Detailed exposition. D. should certainly publish his proof. Siegel's beautiful paper.

Göttingen, den
21. 11. 35

Lieber Harold,

Der Einfachheit halber, und weil es mir genau auf die Ausdrucksweise ankommt, schreibe ich heute deutsch.

Ich habe Dein Ms. gestern und heute genau gelesen, und alles in Ordnung befunden. Ein paar kleine Bemerkungen habe ich in Bleistift an den Rand gesetzt. Deine Behandlungsweise und Methode ist in sich konsequent, ich meine von Deinem Standpunkt aus, alles auf rationale Formeln zurückzuführen und nur mit solchen zu arbeiten. Wir haben ja schon mehrfach darüber gesprochen, dass dieser Standpunkt zu meinem entgegengesetzt ist. Es wird Dir daher klar sein, dass ich dieselben Dinge auf eine total verschiedene Art ausdrücken würde, hauptsächlich alle die vorbereitenden Hilfssätze. Aber wie gesagt, ich erkenne Deinen Standpunkt als ebenfalls berechtigt an, und will deshalb hier nicht an einzelnen Dingen Kritik üben, weil jede solche Kritik nur einen Versuch bedeuten würde, Deine Schlüsse von dem Operieren mit rationalen Funktionen zu "säubern".

Wie ich schon schrieb, hatte ich die Absicht, Deinen Beweis — in meiner Sprache ausgedrückt — in meiner gerade im Entstehen begriffenen ausführlichen Darstellung der ganzen Schlusskette bis zur Riem. Verm. zu verwenden. Das wird nun aber nicht mehr nötig sein. Zu meiner eigenen grössten Überraschung fand ich nämlich gestern den in meinen Augen "wahren" Beweis für die fraglichen Sätze über Meromorphismen. Ich hatte mich die letzten drei Wochen sehr intensiv mit dem Beweis der Ungleichung

$$N(\mu + \nu) \leq 2N(\mu) + 2N(\nu)$$

beschäftigt, und gerade deshalb hatte ich ja die Lektüre Deines Ms. erst einmal zurückgestellt. Ich wollte erst in meinem eigenen Ms. soweit sein, dass ich an die Bearbeitung Deines Beweises herangehen könnte.

Meine Methode zum Beweis dieser Ungleichung — für beliebige vollkommene Körper k beliebiger Charakteristik — führte mich nun darauf, sofort die folgende Identität zu beweisen:

$$(1) \qquad N(\mu+\nu)+N(\mu-\nu) \;=\; 2N(\mu)+2N(\nu)\,.$$

Ich weiss nicht, ob ich Dir diese Identität früher schon einmal mitteilte. Auch sehe ich im Moment nicht, ob Du sie aus den rationalen Formeln des Additionstheorems ebenso einfach ablesen kannst, wie die schwächere Ungleichung. Für alle Fälle will ich Dir die Grundgedanken meines Beweises skizzieren.

Ich beweise (1) als Folge der Relation

$$\begin{array}{llcll} (2.) & x\mu - x\nu & \cong & \frac{\mathfrak{o}(\mu+\nu)\cdot\mathfrak{o}(\mu-\nu)}{(\mathfrak{o}\mu)^2\cdot(\mathfrak{o}\nu)^2} & \text{für} \quad \mu,\,\nu,\,\mu+\nu,\,\mu-\nu \neq 0 \\ (2.') & (\mathrm{d}x)\mu & \cong & \frac{\mathfrak{o}(2\mu)}{(\mathfrak{o}\mu)^4} & \text{für} \quad \mu \neq 0\,. \end{array}$$

Dabei bezeichnet $\mathfrak{o}$ den zur Normierung benutzten Primdivisor, x irgendein ganzes nicht–konstantes Multiplum von $\frac{1}{\mathfrak{o}^2}$ in K (sodass K über $k(x)$ quadratisch ist) und durch hinteres Anhängen von μ ist die Ausführung der isomorphen Abbildung μ von K auf den Teilkörper $K\mu$ bezeichnet. $x\mu$, $x\nu$ haben also die Nenner $(\mathfrak{o}\mu)^2$, $(\mathfrak{o}\nu)^2$.

Nun kann ich allerdings die Relation (2.) nicht in dieser vollen Allgemeinheit beweisen, sondern nur unter einer gewissen Einschränkung über die Teilbarkeit von μ, ν durch Dein π_1. Ich bezeichne mit $\mathfrak{I}(\mu)$ die genaue Potenz p^r derart, dass $K\mu \leq K^{p^r}$ (also μ genau durch π_1^r teilbar). Dann gilt

$$\begin{array}{rcll} \mathfrak{I}(\mu\nu) & = & \mathfrak{I}(\mu)\,\mathfrak{I}(\nu) & \\ \mathfrak{I}(\mu+\nu) & \geq & \mathrm{Min}\,(\mathfrak{I}(\mu),\,\mathfrak{I}(\nu))\,; & \text{daraus folgt, dass} = \text{gilt,} \\ & & & \text{wenn } \mathfrak{I}(\mu)\neq\mathfrak{I}(\nu)\,. \end{array}$$

Meine einschränkende Voraussetzung lautet dann

$$(\mathfrak{I}) \qquad \mathfrak{I}(\mu+\nu) \;=\; \mathfrak{I}(\mu-\nu) \underset{\uparrow}{=} \mathrm{Min}\,(\mathfrak{I}(\mu),\,\mathfrak{I}(\nu))\,.$$

Grob gesagt, soll sich also bei $\mu\pm\nu$ das "Anfangsglied" in π_1 nicht wegheben. Hat man (2.) unter dieser Einschränkung ($\mathfrak{I}$), so folgt durch Gradvergleich auch (1.) unter der Einschränkung ($\mathfrak{I}$). Um davon loszukommen beachte man, dass für $p \neq 2$ sicherlich $\mu+\nu$, $\mu-\nu$ (statt μ, ν) die Relation ($\mathfrak{I}$)

erfüllen, wenn sie für μ, ν verletzt ist; denn dann ist sicherlich $\mathfrak{I}(\mu+\nu) \neq \mathfrak{I}(\mu-\nu)$, also nach obiger Bemerkung

$$\mathfrak{I}(2\mu) \;=\; \mathfrak{I}(2\nu) \;=\; \mathrm{Min}\,(\mathfrak{I}(\mu+\nu)\,,\, \mathfrak{I}(\mu-\nu))\,.$$

Aus (1) für $\mu+\nu$, $\mu-\nu$, 2μ, 2ν folgt aber (1) für für μ, ν, $\mu+\nu$, $\mu-\nu$ durch Wegdivision des Faktors 2, wenn das aus (2.') durch Gradvergleich folgende Resultat $N(2\mu) = 2^2 N(\mu)$, d. h. $N(2) = 2^2$ bereits feststeht. Nun ist aber (2.') für $p \neq 2$ triviale Folge (durch Anwendung von μ) aus der ohne weiteres feststellbaren Tatsache

$$\mathrm{d}x \cong \frac{\mathfrak{o}^2}{\mathfrak{o}^4}\,.$$

Für $p = 2$ komme ich mit dieser Reduktion auf die Voraussetzung ($\mathfrak{I}$) ebenfalls durch, mit ein paar — allerdings unliebsamen — Ergänzungen.

Ich vermute übrigens, dass (2.) auch ohne die Einschränkung ($\mathfrak{I}$) stimmt.

Genau so, wie Du in allen Deinen Hilfssätzen o. B. d. A. $i = 0$ nimmst, kann ich statt ($\mathfrak{I}$) oBdA sogar

$$(\mathfrak{I}_1) \qquad \mathfrak{I}(\mu+\nu) \;=\; \mathfrak{I}(\mu-\nu) \;=\; \mathrm{Min}\,(\mathfrak{I}(\mu)\,,\, \mathfrak{I}(\nu)) \;=\; 1\,,$$

also oBdA etwa $\mathfrak{I}(\mu) = 1$ annehmen (sonst operiere ich eben im Körper K^{p^r}, wo p^r der gemeinsame Wert in ($\mathfrak{I}$) ist).

Der tatsächliche Beweis von (2.) unter der Voraussetzung ($\mathfrak{I}_1$) verläuft dann durch Algebraisierung der Aufzählung von Nullstellen und Polen von $\wp(\mu\, u) - \wp(\nu\, v)$ inkl. ihrer Vielfachheiten. Dass die Teiler $\mathfrak{p}$ von $\mathfrak{o}(\mu+\nu)$ und $\mathfrak{o}(\mu-\nu)$ genau zur ersten bzw. zweiten Potenz aufgehen, je nachdem sie einen oder beide Divisoren teilen, folgt aus

$$c_\mu \pm c_\nu = c_{\mu\pm\nu} \neq 0 \qquad \text{für} \quad \mathfrak{I}(\mu\pm\nu) = 1\,,$$

wo c_μ der Faktor des ganzen Differentials $\mathrm{d}u$ bei μ ist:

$$(\mathrm{d}u)\mu \;=\; c_\mu\, \mathrm{d}u\,.$$

Aus derselben Quelle folgt, dass (2.) hinsichtlich der Vielfachheit der gemeinsamen Teiler $\mathfrak{p}$ von $\mathfrak{o}\mu$ und $\mathfrak{o}\nu$ stimmt, die allein unter den Nennerprimteilern Schwierigkeiten machen.

Ich bin so lange bei diesem Beweis verweilt, weil er sozusagen der Ersatz ist, den ich für Deine Schlüsse mit π_1 gebe.

Nun kommt die Überraschung, die Dich vermutlich ebenso überrascht wie mich.

(1) liefert zunächst für ganzrationale n durch Induktion leicht

$$N(n) \;=\; n^2 .$$

Ferner liefert (1) durch Induktion im Bereich der linearen Komposita $m\mu+n\nu$ leicht die Formel

$$N(m\mu+n\nu) \;=\; m^2N(\mu)+mn(N(\mu+\nu)-N(\mu)-N(\nu))+n^2N(\nu) .$$

Da die linke Seite nie negativ ist, folgt die Ungleichung

$$(N(\mu+\nu)-N(\mu)-N(\nu))^2 \leq 4\,N(\mu)N(\nu) .$$

Führt man also als *Betrag von* μ die Funktion

$$|\mu| \;=\; \sqrt{N(\mu)}$$

ein, so gilt

$$\begin{array}{lccl} \text{(a.)} & |\mu+\nu| & \leq & |\mu|+|\nu| \\ \text{(b.)} & |\mu\nu| & = & |\mu|\cdot|\nu| \\ \text{(c.)} & |n| & = & |n| \;\; \text{im gewöhnlichen Sinne.} \end{array}$$

NB. Mit Hinblick hierauf wiederhole ich meine auf S.7 Deines Ms. ausgesprochene Bitte nachdrücklichst.
Hiernach ist $|\mu|$, was man eine *archimedische Bewertung* nennt (im Gegensatz zu der oben auftretenden nicht–archimedischen Bewertung $\mathfrak{I}(\mu)^{-1}$).

Sei nun Γ der Bereich der gewöhnlichen ganzen Zahlen und μ irgendein Meromorphismus. Dann betrachte ich den Teilring $\Gamma[\mu]$ aller ganzzahligen Polynome in μ. Er ist kommutativ und nullteilerfrei. Also existiert (in abstracto) sein Quotientenkörper $P(\mu)$ aller rationalen Funktionen von μ mit Koeffizienten aus dem Körper P der rationalen Zahlen. $P(\mu)$ ist a fortiori entweder ein einfach algebraischer oder ein einfach transzendenter Erweiterungskörper von P. Die eingeführte Betragsfunktion setzt sich auf P fort, so dass (a.), (b.), (c.) richtig bleiben. Nach dem berühmten — ganz trivialen — Satz von Ostrowski (Acta math.) 41 lässt sich daher $P(\mu)$ so isomorph auf einen Körper aus gewöhnlichen komplexen Zahlen abbilden, dass dabei die Betragsfunktion in $P(\mu)$ gleich dem gewöhnlichen absoluten

Betrag der als Bilder zugeordneten komplexen Zahlen ist. Wir können also die Elemente aus $P(\mu)$, insbesondere die aus $\Gamma(\mu)$ als gewöhnliche komplexe Zahlen ansehen. Sei in diesem Sinne

$$\mu \ = \ \xi + i\eta$$

in Real– und Imaginärteil zerlegt (ξ, η sind natürlich dann nicht als Meromorphismen erklärt, sondern eben auf Grund jener isomorphen Abbildung). Dann ist

$$N(\mu) = \ |\mu|^2 \ = \ \xi^2 + \eta^2 \ = \ m\,,$$

eine ganzrationale Zahl. Ferner ist auch

$$N(\mu + 1) \ = \ |\mu + 1|^2 \ = \ (\xi + 1)^2 + \eta^2$$

eine ganzrationale Zahl. Daraus folgt, dass

$$2\,\xi \ = \ N(\mu + 1) - N(\mu) - 1 \ = \ g$$

eine ganzrationale Zahl ist. Daher ist μ Nullstelle des Polynoms

$$t^2 - gt + m$$

mit ganzrationalen Koeffizienten, und

$$g^2 \leq 4\,m\,,$$

entweder aus $(2\xi)^2 \leq 4(\xi^2 + \eta^2)$ oder aus $(N(\mu + 1) - N(\mu) - 1)^2 \leq 4N(\mu)$ nach obiger allgemeiner Ungleichung mit $\nu = 1$.

Aus der Kenntnis aller nullteilerfreien hyperkomplexen Systeme über P, deren Elemente sämtlich höchstens vom Grade 2 sind, kann man jetzt leicht schließen:

Der Ring M *aller normierten Meromorphismen eines Körpers* K *vom Geschlecht* 1 *über einem vollkommenen Konstantenkörper* k *ist*

a.) *entweder der Integritätsbereich* Γ *aller ganzen Zahlen*

b.) *oder eine Ordnung eines imaginär–quadratischen Zahlkörpers über* P

c.) *oder eine Ordnung eines imaginären verallgemeinerten Quaternionensystems über* P.

Sei nun insbesondere k *absolut–algebraisch von Primzahlcharakteristik* p, also ein endlicher Körper oder jedenfalls ein ev. unendliches Kompositum aus endlichen Körpern. Dann liegt jedenfalls die Grundgleichung $f(x, y) = 0$ in einem endlichen Körper, sagen wir von q Elementen, und dann ist $K \longrightarrow K^q$ ein Meromorphismus π, mit $N(\pi) = q$.

Wie Du setze ich voraus, dass π *nicht in* Γ liegt (also $\overline{\pi} \neq \pi$; $\overline{\pi} = -\pi$ ist sogar zugelassen). Dann liegt also entweder b.) oder c.) vor.

Nun ist aber π mit allen μ vertauschbar; daher kann c.) *nicht* vorliegen, denn sonst könnte man aus π, μ ganzrational ein Element vom Grade 4 über Γ konstruieren, während doch alle Elemente von M höchstens vom Grade 2 sind. Somit folgt:

Ist k absolut–algebraisch und $\pi \neq \overline{\pi}$, so liegt b.) vor.

Es ist schade, dass der Fall $\pi = \overline{\pi}$ sich nicht auf diese Weise erzwingen lässt. Für die Riemannsche Vermutung, die allein π betrifft, ist das zwar irrelevant, aber es wäre doch interessant, die Struktur von M in allen Fällen zu kennen. — ob wohl c.) bei gewissen Körpern k vorkommt ?

Ich habe lange überlegt, ob ich Dir all' dies im gegenwärtigen Moment mitteilen sollte, oder ob ich nicht lieber meine Erkenntnisse noch eine Weile für mich behalten sollte. Denn ich weiss doch, wie sehr Du gerade jetzt für Dein Fortkommen es nötig hast, mit schönen Ergebnissen vor die Öffentlichkeit zu treten. Schliesslich habe ich es aber doch für richtig gehalten, Dir alles dies gleich zu schreiben. Ich finde nämlich, es besteht *kein* Grund, weswegen Du nicht Deinen sehr originellen und interessanten Beweis doch veröffentlichen solltest. Von Deinem Standpunkt aus wirst Du zwar vielleicht meinen Beweis der Normenidentität (1) als zufriedenstellend anerkennen, und auch noch die Herleitung von (a.), (b.), (c.). Sicherlich wirst Du aber die von da zum Hauptsatz führende begriffliche Schlussweise als mit Deinem „ rationalen" Standpunkt gemäss als unbefriedigend empfinden, und ich kann das gut verstehen. Ich wäre selbst froh, wenn ich um den Ostrowski'schen Satz herum käme. Man darf allerdings auf der anderen Seite nicht vergessen, dass ich jetzt den Dirichlet'schen Einheitensatz gar nicht mehr brauche. Das ist auch von Deinem Standpunkt aus sicher ein Vorteil.

Ich möchte also sagen, dass Du bestimmt Deinen Beweis veröffentlichen sollst, am besten recht bald. Neben allen anderen Gesichtspunkten hast Du doch das fragliche „ neue Resultat" zuerst gefunden, und ich bin erst durch Deine Mitteilungen, und vor allem durch die von Dir gewonnene Erkenntnis über die Tragweite der Normenungleichung dabei, dazu angeregt worden, über diese Normenungleichung eingehender nachzudenken.

Nun viele herzliche Grüsse, auch an alle Bekannten dort.

Was hast Du übrigens zu Siegels schöner Arbeit gesagt ?

Stets Dein

Helmut.

1.113 23.11.1935, Davenport to Hasse

Congratulations + admiration.

Sat. 23 Nov. 1935.

My dear Helmut,

Many thanks for your letter, which I found when I got back from Harrow this evening.

My sincere and hearty congratulations on your new method and new results. I had never thought of your identity, though it seems so reasonable now.

I have not yet completely digested your letter, but of course it is obvious to me that your results go much further than mine. I was quite unable to prove that all mer. are quadratic; in fact I had not dared to conjecture it.

That only $\overline{\pi} = \pi$ (and not also $\overline{\pi} = -\pi$) should be excluded is now a surprise to me, for $\overline{\pi} = -\pi$ can only arise when f is odd, in which case my method works too.

On the whole I do feel doubtful about publishing my comparatively amateurish work.

By the way, I am not a bigoted "rationalist"; the principal reason why I wrote my MS in purely rational language was to avoid making blunders.

Again my hearty congratulations + admiration.

Much love from

Harold.

1.114 27.11.1935, Hasse to Davenport

D. must publish his proof. Witt made a blnder. Matrix generation of function fields. Hyperelliptic case.

Göttingen, den
27. 11. 35

My dear Harold,

Thanks very much for your kind letter. *Of course* you must publish your proof ! I have mentioned the fact, that you first had the idea of considering $N(\mu)$ as a sort of absolute value and proved the algebraicity and commutativity of normalized meromorphisms on this basis, in both my preliminary paper for the Göttinger Nachrichten and my detailed account for Crelle's Journal.

I do not see why $\overline{\pi} = -\pi$ implies f is odd, although I may have proved that at an earlier stage of my acquaintance with the matter. Would you mind letting me know how you prove it ?

I must take back what I wrote some time ago about every field of genus g being a composite of g elliptic fields. Witt made a blunder with his operations of dissecting and reconnecting a Riemann surface.

I have given a few more contributions to the problem of a suitable generation for genus g, and I will let you know how I see the problem at present, and in particular how it looks in the hyperelliptic case $g = 2$.

Let k w. l. o. g. be algebraically closed, and K a field of genus g over k. Let $\mathfrak{o} = \mathfrak{o}_1 \cdots \mathfrak{o}_g$ be a non–special integral divisor of degree g (see one of my former letters), i. e., there is no integral multiple of $\frac{1}{\mathfrak{o}}$ in K except constants or the determinant

$$\left| \frac{\mathrm{d}u_i}{\mathrm{d}\omega_j} \right|_0 \neq 0 \,,$$

where the $\mathrm{d}u_i$ are the g linearly independent integral differentials of K, the ω_j prime elements for the $\mathfrak{o}_j$, and the suffix 0 means taking each $\frac{\mathrm{d}u_i}{\mathrm{d}\omega_j}$ for $\omega_j = 0$.

Then there are elements

$$\begin{array}{rcll} x_i & \cong & \frac{\mathfrak{a}_i}{\mathfrak{o}_i \mathfrak{o}} & ; \ \mathfrak{a}_i \text{ integral, prime to } \mathfrak{o}_i \\ y_i & \cong & \frac{\mathfrak{b}_i}{\mathfrak{o}_i^2 \mathfrak{o}} & ; \ \mathfrak{b}_i \text{ integral, prime to } \mathfrak{o}_i \end{array}$$

in K, i. e., x_i, y_i have exactly $\mathfrak{o}_i^2$, $\mathfrak{o}_i^3$ in their denominators, and otherwise at most (but not necessarily exactly !) the first power $\mathfrak{o}_j \quad (j \neq i)$. As one easily sees from the Riemann–Roch Theorem,

$$\left.\begin{array}{cccccccccc} 1,\, x_i & \text{is} & \text{a} & \text{basis} & \text{for} & \text{the} & \text{integral} & \text{multipla} & \text{of} & \frac{1}{\mathfrak{o}^2} \\ 1,\, x_i,\, y_i & '' & '' & '' & '' & '' & '' & '' & '' & \frac{1}{\mathfrak{o}^3} \\ 1,\, x_i,\, y_i,\, x_i^2 & '' & '' & '' & '' & '' & '' & '' & '' & \frac{1}{\mathfrak{o}^4} \\ 1,\, x_i,\, y_i,\, x_i^2,\, x_iy_i & '' & '' & '' & '' & '' & '' & '' & '' & \frac{1}{\mathfrak{o}^5} \\ 1,\, x_i,\, y_i,\, x_i^2,\, x_iy_i,\, x_i^3 & '' & '' & '' & '' & '' & '' & '' & '' & \frac{1}{\mathfrak{o}^6} \\ \cdots & . & . & \cdots & . & . & \cdots & \cdots & . & \cdots \end{array}\right\} \begin{array}{l}\text{(i=1,..}\\ \text{..,g)}\end{array}$$

Hence all elements of K which have only the $\mathfrak{o}_i$ in their denominators are polynomials of type

$$\sum_{\mu=1}^{m} a_{i\mu} x_i^{\mu} + \sum_{\nu=1}^{n} b_{i\nu} x_i^{\nu} y_i + c\,,$$

and conversely. From this the following facts follow:

(1.) $K = k(x_1, \ldots, x_g;\, y_1, \ldots, y_g)$.

(2.) Let $\mathfrak{p}$ be a prime divisor of K different from the $\mathfrak{o}_i$, and

$$(x_i,\, y_i) \equiv (a_i,\, b_i) \bmod . \mathfrak{p}$$

Then there is no other $\mathfrak{p}'$ in K belonging to the same a_i, b_i. Hence $\mathfrak{p}$ is uniquely characterized by the a_i, b_i.

The elements y_i^2 are representable as linear combinations of the elements of the last basis above given explicitly. These representations are of the following type:

$$y_i^2 \;=\; a_i^{(0)} x_i^{(3)} + \sum_j a_{ij}^{(1)} x_j y_j + \sum_j a_{ij}^{(2)} x_j^2 + \sum_j a_{ij}^{(3)} y_j + \sum_j a_{ij}^{(4)} + a_i^{(5)}\,.$$

Introducing the notation

$$\mathfrak{x} \;=\; \begin{pmatrix} x_1 \\ \vdots \\ \vdots \\ x_g \end{pmatrix}, \quad \mathfrak{y} \;=\; \begin{pmatrix} y_1 \\ \vdots \\ \vdots \\ y_g \end{pmatrix}, \quad \mathfrak{x}^2 \;=\; \begin{pmatrix} x_1^2 \\ \vdots \\ \vdots \\ x_g^2 \end{pmatrix}, \ldots, \; \mathfrak{x}\mathfrak{y} \;=\; \begin{pmatrix} x_1y_1 \\ \vdots \\ \vdots \\ x_gy_g \end{pmatrix},$$

this equation may be written as

$$\mathfrak{y}^2 \;=\; A_0\mathfrak{x}^3 + A_1\mathfrak{x}\mathfrak{y} + A_2\mathfrak{x}^2 + A_3\mathfrak{y} + A_4\mathfrak{x} + \mathfrak{a}_5$$

with constant g–rowed matrices $A_0, \dots, A_4$, in particular A_0 a regular diagonal matrix, and a constant column $\mathfrak{a}_5$. This rather resembles the fundamental equation in the elliptic case, and becomes in fact identical with it for $g = 1$.

To each prime divisor $\mathfrak{p} \neq \mathfrak{o}_i$ there corresponds a constant solution $\mathfrak{a}$, $\mathfrak{b}$ of this equation in $\mathfrak{x}$, $\mathfrak{y}$. $\mathfrak{p}$ is uniquely determined by this solution $\mathfrak{a}$, $\mathfrak{b}$. But not to every given solution a prime divisor exists, for $x_1, \dots, x_g$ are still algebraically dependent.

The addition theorem may be expressed as follows. Let $\mathfrak{p}_1, \dots, \mathfrak{p}_g$ and $\mathfrak{q}_1, \dots, \mathfrak{q}_g$ be two given sets of g prime divisors ($\neq \mathfrak{o}_i$). Then there exists one (and generally only one) set $\mathfrak{r}_1, \dots, \mathfrak{r}_g$ so that

$$\frac{\mathfrak{p}_1 \cdots \mathfrak{p}_g \;\mathfrak{q}_1 \cdots \mathfrak{q}_g \;\mathfrak{r}_1 \cdots \mathfrak{r}_g}{\mathfrak{o}_1 \cdots \mathfrak{o}_g \;\mathfrak{o}_1 \cdots \mathfrak{o}_g \;\mathfrak{o}_1 \cdots \mathfrak{o}_g} \;\cong\; z\,, \quad \text{an element of } K\,.$$

Now let $(\mathfrak{a}_i,\, \mathfrak{b}_i) \leftrightarrow \mathfrak{p}_i$, $(\mathfrak{a}_i',\, \mathfrak{b}_i') \leftrightarrow \mathfrak{q}_i$, $(\mathfrak{a}_i'',\, \mathfrak{b}_i'') \leftrightarrow \mathfrak{r}_i$ in the above sense. And let

$$z \;=\; \sum_j \alpha_j x_j + \sum_j \beta_j y_j + \gamma$$

be the representation of z as an integral multiple of $\frac{1}{\mathfrak{o}^3}$ by the above basis $1,\, x_j,\, y_j$. Then taking the constant residues mod. $\mathfrak{p}_i,\, \mathfrak{q}_i,\, \mathfrak{r}_i$ of z which are 0, one gets the linear equations

$$\begin{aligned} &\textstyle\sum_j \alpha_j a_{ij} \;+\; \sum_j \beta_j b_{ij} \;+\; \gamma \;= 0 \\ &\textstyle\sum_j \alpha_j a_{ij}' \;+\; \sum_j \beta_j b_{ij}' \;+\; \gamma \;= 0 \qquad \text{where } \mathfrak{a}_i = \begin{pmatrix} a_{i1} \\ \vdots \\ a_{ig} \end{pmatrix}, \dots \\ &\textstyle\sum_j \alpha_j a_{ij}'' \;+\; \sum_j \beta_j b_{ij}'' \;+\; \gamma \;= 0 \end{aligned}$$

Now take each of the first $2g$ equations (in $2g+1$ terms) and only one at a time of the last g equations. Then the determinant

$$\begin{vmatrix} a_{11} & \cdots & a_{1g} & b_{11} & \cdots & b_{1g} & 1 \\ \cdot\cdot & \cdots & \cdot\cdot & \cdot\cdot & \cdots & \cdot\cdot & \cdot\cdot \\ a_{11}' & \cdots & a_{1g}' & b_{11}' & \cdots & b_{1g}' & 1 \\ \cdot\cdot & \cdots & \cdot\cdot & \cdot\cdot & \cdots & \cdot\cdot & \cdot\cdot \\ a_{i1}'' & \cdots & a_{ig}'' & b_{i1}'' & \cdots & b_{ig}'' & 1 \end{vmatrix} \;=\; 0$$

for each value of $i = 1, \ldots, g$. From this, and the above fundamental matrix equation, the symmetrical functions of the $(\mathfrak{a}_i'', \mathfrak{b}_i'')$ may be expressed rationally by the symmetrical functions of the $(\mathfrak{a}_i, \mathfrak{b}_i)$ and $(\mathfrak{a}_i', \mathfrak{b}_i')$.

Taking the $(\mathfrak{a}_i, \mathfrak{b}_i)$ and $(\mathfrak{a}_i', \mathfrak{b}_i')$ as indeterminates, this process of elimination gives the rational formulae of the addition theorem. But I suppose one only needs the above implicit formulation; in the elliptic case I need not more.

Perhaps you will care to push forward to explicit formulae in the hyperelliptic case $g = 2$. I will give you therefore what I worked out for this case (characteristic $\neq 2$):

$$K = k(u, v) \qquad \text{with} \quad v^2 = (u - c_1) \cdots (u - c_6)\,, \quad c_i \text{ different}$$

Prime divisors:

$$\begin{array}{llll} \text{ramified } \mathfrak{z}_i: & u - c_i \cong \frac{\mathfrak{z}_i^2}{\mathfrak{o}\mathfrak{o}'}\,, & v \cong \frac{\mathfrak{z}_1 \cdots \mathfrak{z}_6}{(\mathfrak{o}\mathfrak{o}')^3} & \\ \text{general } \mathfrak{p}: & u - a \cong \frac{\mathfrak{p}\mathfrak{p}'}{\mathfrak{o}\mathfrak{o}'}\,, & v - b \cong \frac{\mathfrak{p}\mathfrak{q}_1 \cdots \mathfrak{q}_5}{(\mathfrak{o}\mathfrak{o}')^3} & (\mathfrak{q}_i \text{ different from } \mathfrak{p}) \\ \text{Differentials (integral)} & \frac{du}{v} \cong \mathfrak{o}\mathfrak{o}'\,, & (u-a)\frac{du}{v} \cong \mathfrak{p}\mathfrak{p}'\,. & \end{array}$$

"Special" in the above sense only $\mathfrak{o}\mathfrak{o}'$ and every $\mathfrak{p}\mathfrak{p}'$. Non–special for example $\mathfrak{z}_1\mathfrak{z}_2$. Take this as $\mathfrak{o} = \mathfrak{o}_1\mathfrak{o}_2$ above. Then, say,

$$x_1 = \frac{1}{u - c_1} \cong \frac{\mathfrak{o}\mathfrak{o}'}{\mathfrak{z}_1^2}\,, \qquad x_2 = \frac{1}{u - c_2} \cong \frac{\mathfrak{o}\mathfrak{o}'}{\mathfrak{z}_2^2}$$

$$y_1 = \frac{v}{(u - c_1)^2 (u - c_2)^2} = v x_1^2 x_2 \cong \frac{\mathfrak{z}_3 \cdots \mathfrak{z}_6}{\mathfrak{z}_1^3 \mathfrak{z}_2}\,,$$

$$y_2 = \frac{v}{(u - c_1)(u - c_2)^2} = v x_1 x_2^2 \cong \frac{\mathfrak{z}_3 \cdots \mathfrak{z}_6}{\mathfrak{z}_1 \mathfrak{z}_2^3}$$

Now, by development into partial fractions,

$$y_1^2 = \frac{(u-c_3)\cdots(u-c_6)}{(u-c_1)^3(u-c_2)} = \frac{a_1^{(3)}}{(u-c_1)^3} + \frac{a_1^{(2)}}{(u-c_1)^2} + \frac{a_{11}}{u-c_1} + \frac{a_{12}}{u-c_2} + 1$$

$$y_2^2 = \frac{(u-c_3)\cdots(u-c_6)}{(u-c_1)(u-c_2)^3} = \frac{a_2^{(3)}}{(u-c_2)^3} + \frac{a_2^{(2)}}{(u-c_2)^2} + \frac{a_{21}}{u-c_1} + \frac{a_{22}}{u-c_2} + 1\,,$$

with easily expressible coefficients a. Hence

$$\begin{pmatrix} y_1^2 \\ y_2^2 \end{pmatrix} = \begin{pmatrix} a_1^{(3)} & 0 \\ 0 & a_2^{(3)} \end{pmatrix} \begin{pmatrix} x_1^3 \\ x_2^3 \end{pmatrix} + \begin{pmatrix} a_1^{(2)} & 0 \\ 0 & a_2^{(2)} \end{pmatrix} \begin{pmatrix} x_1^2 \\ x_2^2 \end{pmatrix} +$$

$$+ \begin{pmatrix} a_{11} & a_{12} \\ a_{21} & a_{22} \end{pmatrix} \begin{pmatrix} x_1 \\ x_2 \end{pmatrix} + \begin{pmatrix} 1 \\ 1 \end{pmatrix}.$$

This is the fundamental matrix equation.

Here, obviously, $k(x_1, x_2) = k(u)$, whereas in the general (not hyperelliptic) case I suppose $k(x_1, \ldots, x_g) = K$ already.

I really believe this matrix generation of K to be more suitable than what we formerly tried in the hyperelliptic case. I should be glad if you would push on, perhaps with $g = 2$ as a model first. My own time, and also energy, is rather consumed now, so that I must give up doing anything more for the time being. I have not told Witt, nor anybody else here, about this new "Ansatz" of mine, because I thought you would be glad to have something really promising to spend your time and energy on. But still you must be quick, for I suppose some people here will attack R. H. for $g > 1$ as soon as they see my new papers.

I will let you have all my manuscripts about the elliptic case (three more, besides that you have already got) as soon as they are typewritten.

Good luck, and best wishes, also to Donald,

Yours,

Helmut

1.115 Postcard Hasse to Davenport, no Date

(no Date)[1]

Addition to last letter.

Dear Harold,

In addition to my last letter I have seen that the equation

$$\mathfrak{y}^2 \;=\; A_0\mathfrak{x}^3 + A_1\mathfrak{x}\mathfrak{y} + A_2\mathfrak{x}^2 + A_3\mathfrak{y} + A_4\mathfrak{x} + \mathfrak{a}$$

is such that

A_0 is a regular diagonal matrix

$A_1,\ A_2$ *are diagonal matrices.*

Hence, when $p \neq 2,\ 3$, one can achieve $A_1,\ A_2 = 0$ by a linear substitution.

This equation is closely connected with the field of the Abelian functions belonging to the original field. I think it is the most promising starting point for all further research on this whole subject.

Heil !
Helmut

[1]The archive registration number of this postcard is the direct successor of the archive registration number of the letter from Hasse to Davenport of November 27, 1935

1.116 22.12.1935, Postcard Hasse to Davenport

22. 12. 35

Christmas greetings. Schilling reading Italian papers on singular correspndences. Italian letters seem really deep. H. got an invitation to Manchester.

My dear Harold,

I wish you an extremely Merry X–mas V of our acquaintanceship (X not indeterminate, V an integer). I hope you will have enjoyed Jaeger's nuptial celebration and regret his leaving C. for good. We put on Schneeketten to–day because there is a rather thick layer of snow all over the town. Even with them driving is rather risky. I hope we shall get to Kassel alright to–morrow. During last week I was bombarded with letters from Schilling, at present at Princeton. He seems to be reading the great host of Italian papers on singular correspondences (i. e., complex multiplications) in algebraic varieties (i. e., algebraic function fields), and he obviously tries making most of them for the study of such varieties over a field of characteristic p. I guess he has got the letter of me there. This Italian papers seem to be really deep, from what he writes about them.

I got an invitation to Manchester for the end of Lent term, 3 lectures, 5 guineas. I still wonder whether I am going to comply.

My heartiest wishes to all of you.

Affectionately yours,

Helmut.

1.117 14.01.1936, Davenport to Hasse

D. has worked with Heilbronn on Waring's problem. Got $G(4) \leq 17$. Work with Heilbronn on zeta fctn. H. plans to come over in Feb-March. Teichmüller.

Will write to Clärle tomorrow; was unfortunately prevented from finishing letter today.

14 Jan 1936

My dear Helmut,

I am sorry I have not written earlier. I am ashamed to say I have completely lost interest in meromorphisms for the time being; I have been working hard with Heilbronn on Waring's problem + we have got

$$G(4) \leqq 17 \qquad \text{(best previous 19)}.$$

The calculations are pretty complicated. We have one (perhaps $1\frac{1}{2}$) new idea.

I am glad to hear you are seriously considering coming over in Feb-March and look forward to your visit. Before then my interest in $y^2 = f(x)$ will certainly have revived.

Heilbronn + I have sent our paper to press containing the proof that $\zeta(s,a) = \sum\limits_0^\infty (n+a)^{-s}$ has an infinity of zeros for rational $a \neq \frac{1}{2}, 1$, + similar result for $\zeta(s,\mathfrak{K})$, $\mathfrak{K}$ an ideal-class in a quad. field, $h(\mathfrak{K}) > 1$.

I was amused by the beginning of Teigmüller's paper on Wachs-Raum. Don't you think it shows a little conceit?

Very best wishes,

Yours,

Harold.

Term starts today, unfortunately.

1.118 03.02.1936, Hasse to Davenport

On $G(4) \leq 17$. H. is working on valued fields with Teichmüller. He is a queer chap. H. will go to Manchester. Could D. put him up at Cambridge for a week before that.

Göttingen, den
3. 2. 36

My dear Harold,

I hope you will not think I have entirely forgotten you. Thanks very much for your kind letter of Jan. 14^{th} .

Your result $G(4) \leq 17$ seems *some* achievement even to an hard–boiled algebraicist like me.

My interest in $f(x, y) = 0$ has also faded at present. I am working on "bewertet" fields in general and cyclic fields (of numbers or functions mod. p) of degree p^n in particular, together with Tei*ch*müller (not Teigmüller). His paper on the Wacks–Raum is as queer as the whole chap.

I heard that I shall get the official permission for lecturing in Manchester shortly. I suggested March 5^{th} and perhaps also March 12^{th} to Mordell for the lectures. He wrote me he asked you to come up there too. That would be very nice indeed.

Could you possibly put me up in College for a week or so before that . I think I can manage to leave here about Febr. 26^{th} ? I am sorry to say that Clärle is not going to come with me. It has been no use trying to persuade her. She hopes to join a whole company of young people for a course in skiing somewhere in the Alps, and seems dead set upon this.

I am very much looking forward to seeing you and talking to you.

With all best wishes,

Yours,
Helmut

1.119 06.02.1936, Davenport to Hasse

D. is looking forward to seeing H. who will visit him in Cambridge. H. will also visit Mordell in Manchester. G(4) was quite trivial. Estermann. Vinogradov.

6.2.36.

My dear Helmut,

Many thanks for your letter.

I am very glad to hear that you are definitely coming, and look forward very much to seeing you. Of course I shall get a room for you in College – unless by some accident a room actually in College should prove unobtainable. Let me know later which way you think of coming. [If via Harwich, I could meet you conveniently on a Mon. Wed. or Fri. morning, or a Tu. Th. Sat. evening, or a Sunday.].

I don't know whether I shall be able to go to Manchester (except for a day or two just to take you); [...] before the end of term (March 14) at any rate.

The result for $G(4)$ was quite trivial. That it was "in the air" is shown by the fact that it was obtained simultaneously by us + Estermann. I have just heard that Vinogradov has proved some startling results on the ζ-fn. + prime-number theorem.

Very best wishes,

Yours,

Harold.

1.120 10.02.1936, Hasse to Davenport

H. announcing his visit.

Göttingen, den 10. Februar 1936.

My dear Harold!

I am very glad you are able to put me up in Cambridge. Unless you hear other I am looking forward to seeing you on Tuesday Febr. 25$^{\text{th}}$ (Shrove Tuesday!) evening at the Harwich boat if you will really take the trouble of meeting me there.

May I ask you whether there will possibly be any feast during my stay so that I had better bring my frock-coat or dinner-jacket with me, and which then?

Much in hurry,

Y o u r s

1.121 14.02.1936(?), Davenport to Hasse

D. will meet H. at Harwich.

Friday 14 Feb.

My dear Helmut,

Yes, unless I hear from you to the contrary, or something unexpected turns up before then, I will meet you on arrival at Harwich on the evening of the 25 th.

If there is any chance of your being in Cambridge on March 15 there is the Commemoration Feast on that day. For that I advise full evening dress (i.e. tails + white waistcoat).

Will write again soon.

Very best wishes

Yours

Harold.

1.122 27.03.1936(?), Davenport to Hasse

D. can improve O-results on exponential sums.

27.3 Wednesday.

My dear Helmut!

Very glad indeed to hear that you are alright again. Look forward to hearing from you.

Think I can improve the O-results for exponential sums, though these are perhaps of little interest.

Have just had a note from Mrs Mordell + Kathleen to say they are in Cambridge, but have not seen them yet.

Very best wishes. Sorry your visit came to an end so soon.

Yours

Harold

1.123 27.03.1936, Davenport to Hasse

Proofs of D.'s paper on meromorphisms. D. will make effort with functional equation paper. ϑ-series (Witt?).

27 March 1936.

My dear Helmut and Clärle,

Very many thanks to Helmut for his letter, which I much appreciate, though it is nonsense to speak of any indebtedness to my hospitality. I am very glad you are quite well again, and hope your cold was not a consequence of the journey.

I have nothing particular to relate. Mrs Mordell + Kathleen were in Cambridge for a short time; I only saw them once, yesterday evening, when they spent an hour or two with Heilbronn + me. Mordell has gone on the cruise he mentioned from London to Glasgow via Rotterdam + the north of Scotland.

It is a pity you could not stay longer, as the weather has been very summery since you left. Heilbronn and I have played bowls almost every afternoon.

I enclose the proofs of my paper on meromorphisms, and would be very grateful if you could find time to read it and note any points which are incorrect or obscure. As regards §4, all I ask is that you agree that it is plausible (I might have made a mistake in my rigorous proof; it is some time ago since I wrote it).

I will make an effort with the functional equation paper. ϑ-series are a sound idea, of course, though I do not regard the ϑ-series proof for the ordinary ζ-fn. as being the "natural" proof.

I expect you will be pretty busy, from now onwards.

Is Clärle going to Marburg to assist the Fuchses in their removal, as she once intended? If so, perhaps she will give my kindest regards to both of them.

Many thanks to the Oma for her friendly card.

I am just going to play *the* game with Besicovitch.

Love + best wishes to both of you,

from

Harold

1.124 30.03.1936, Hasse to Davenport

H. will go to Oslo. Theta-functions in Witt's proof of functional equation. Can D. recommend a young mathematician who would come to Gö.?

30 th of March 36

My dear Harold,

Thanks very much for your letters of Wednesday and Friday. I am returning your proofs with a few odd remarks in pencil. Otherwise I found everything correct, in particular §3 and §4 so far.

I have sent in the postcard to the Office of the Oslo congress. I have asked for a single room in a Hotel of category IV and also have asked for being put up in the same Hotel as you. I have written that in case you wished category III, I should also be prepared to pay as much.

The Θ-functions in Witt's proof of the functional equation are only formally analogous to the analytic Θ-functions. They are finite series involving a character. Witt's proof, apart from this formal apparatus, may be described as generalizing the proof for Riemann-Roch's theorem to Strahlklassen instead of ordinary Divisorenklassen.

The Fuchses removed to Heidelberg a week ago, we did not help them.

Clärle has got the flu from me. She has been in bed since Saturday last and is still not quite well to-day. I hope she will recover soon.

Do you know of any young Cambridge mathematician, who would like to come to Göttingen in exchange for Behrbohm? Prof. Sieverts told me, he could manage the technical side of the exchange possibly even to the extent of bridging the gap between the two rates of exchange. He only wanted to be given the applicants' names beforehand.

Do you remember the name of the road in Withington Gribbin's parsonage is in?

Please let me know you improvement on the O-results for exponential sums.

Much love and best wishes

yours

1.125 01.04.1936, Davenport to Hasse

D. will also go to Oslo. New results on exponential fctns. do not give new O-results.

1 April 1936.

My dear Helmut,

Many thanks for your letter, returning so promptly the proof-sheet, and for your card, which has just arrived.

The point you query – why the Homomorphiesatz shows that x_μ and y_μ/y depend only on x – was intended as follows. If $x_\mu = A + By$, $y_\mu = C + Dy$ (where A etc. are rat. fns. of x), then the Hom. Satz shows that $-\mu = \mu(-1)$ and so

$$\begin{aligned} A + By &= A - By \\ C + Dy &= -(C - Dy) \end{aligned}$$

ie. $B = C = 0$.

I am also sending the card to Oslo, taking III class hotel, and repeating your instruction.

I am sorry to say that my new results on exp. sums do not, as I supposed, give new O-results. They shew, for example, that the sum of the first two or three largest abscissae of the roots of the L-fns. is less than one previously knew it to be. e.g. for $f(x) =$ sextic there are five roots, say $\rho_1, \ldots, \rho_5$ where $\Re\rho_1 \geqq \Re\rho_2 \geqq \ldots \geqq \Re\rho_5$. What we knew before was $\Re\rho_1 \leqq 5/6$, + what I prove now is $\frac{1}{2}(\Re\rho_1 + \Re\rho_2) \leqq \frac{3}{4}$. Not very exciting!

I can't at the moment think of any Cambridge student to exchange for Behrbohm. But I will make enquiries. I suppose it would be for a period of a year?

I can't remember the Gribbin's road. Do you remember the name of the church? That would suffice. Was it St John's? Since writing the above sentence I have looked Gribbin up in the clerical directory. Address is

Rev. T.M. Gribbin
St. Chad's Rectory
Withington, Manchester.

I am going to Harrow on Friday morning, + leaving there for Cornwall on Monday.

Much love from

Harold.

1.126 05.04.1936, Nancy Davenport to Hasse

April 5/36

Dear Prof Hasse

I was so pleased to hear that you had quite recovered from your illness, but sorry that Mrs Hasse also had had a bad attack, and hope her recovery is well maintained.

Grace + I are both favourably impressed by your suggestion. It would be particularly agreeable to us to have someone who would be a helpful companion to either of us in the temporary absence of the other.

If Miss Paul would care to come we should be glad to receive her as a member of the family. Perhaps you would not mind hinting to her that we live a quiet life here, but apart from that there is no reason why she should not have a pleasant stay.

If Miss Paul is still agreeable would she care to come about the end of April for a few weeks (as you suggested)?

I should be pleased to offer her ten shillings per week pocket money and pay her fare back to Gottingen. (as this might present difficulties to her in view of the German financial regulations).

Harold came home last Friday and on Saturday we all went to see the Boat Race. It was rather exciting, for a change, because Oxford won the toss + actually led for about a third of the way, but by the time they came within our view it was obvious that the Cambridge crew were much superior.

Harold is setting out for Cornwall tomorrow.

With kindest regards from all of us to Mrs Hasse + yourself

I remain
very sincerely,

Nancy Davenport.

1.127 30.04.1936, Hasse to Davenport

Witt's proof for the L-functional equation. Manuscript (No address.)

Göttingen, den
30. 4. 36

I.) The main source for Riemann–Roch's theorem and generalisations for character classes is, according to Witt, the following theorem:

Let k be an arbitrary field, and K the field of all power series $\sum_{\nu=\nu_0}^{\infty} a_\nu t^\nu$ with a_ν in k, t an indeterminate; furthermore

R_1 the ring of all polynomials in $\frac{1}{t}$ over k,

R_2 the ring of all power series $\sum_{\nu=0}^{\infty} a_\nu t^\nu$ over k (integral power series),

both sub–rings of K.

Let M be a matrix with determinant $\neq 0$, consisting of elements in K. Then there are a matrix A_1 in R_1, and a matrix A_2 in R_2, both with determinant an unit (element $\neq 0$ in k) so, that

$$A_1 M A_2 = \begin{pmatrix} t^{\nu_1} & & 0 \\ & \ddots & \\ 0 & & t^{\nu_n} \end{pmatrix}.$$

II.) Let k now be finite field of q elements, and K, R_1, R_2 as above. For an element

$$\alpha = \sum_{\nu=\nu_0}^{\infty} a_\nu t^\nu \qquad \text{with} \quad a_{\nu_0} \neq 0$$

we put

$$|\alpha| = q^{-\nu_0}.$$

We consider n–termed vectors $(\alpha) = (\alpha_1, \ldots, \alpha_n)$ of elements in K and put:

$$g_1^{(\alpha_1,\ldots,\alpha_n)} = \begin{cases} 1, & \text{if all } \alpha_\nu \text{ in } R_1\text{, i. e. polynomials in } \frac{1}{t}, \\ 0, & \text{otherwise.} \end{cases}$$

$$g_2^{(\alpha_1,\ldots,\alpha_n)} \;=\; \begin{cases} 1\,, & \text{if all } \alpha_\nu \text{ in } t\,R_2\,,\ \text{i. e. } |\alpha_\nu| \le q^{-1} \\ 0\,, & \text{otherwise.} \end{cases}$$

Let moreover $\gamma(\alpha)$ be a (fixed) function in K with

$$\gamma(\alpha+\beta) = \gamma(\alpha)\cdot\gamma(\beta)\,, \quad \gamma(\sum_{\nu=\nu_0}^{\infty} a_\nu t^\nu) = \gamma(a_1 t)\,,$$

$$\gamma(at) \neq 1 \text{ for at least one } a \text{ in } k\,,$$

i. e. $\gamma(\alpha)$ is a character (non–principal), depending only on the linear term $a_1 t$ of α,

$$\gamma(\alpha) \;=\; e^{\frac{2\pi i}{p}\mathrm{Sp}(a_1)}, \quad \text{say.}$$

Now let M be as above and $(\varrho) = (\varrho_1,\ldots,\varrho_n)$ a fixed vector. Then

$$\sum_{(\alpha)} g_1^{(\alpha)} g_2^{(\alpha+\varrho)M} \;=\; \Big||M|\Big|^{-1} \sum_{(\beta)} g_1^{(\beta)} g_2^{M(\beta)} \gamma((\varrho)(\beta))$$

where the sums are extended over all vectors $(\alpha) = (\alpha_1,\ldots,\alpha_n)$ and $(\beta) = (\beta_1,\ldots,\beta_n)$ in K.

$$\left.\begin{array}{llllll} (\alpha+\varrho)M & \text{is} & \text{the} & \text{vector} & \text{with} & \text{components} \\ & & & & & \alpha'_\nu = \sum_k (\alpha_k+\varrho_k)m_{k\nu} \\ M(\beta) & '' & '' & '' & '' & \quad '' \\ & & & & & \beta'_\nu = \sum_k m_{\nu k}\beta_k \end{array}\right\}\begin{array}{l}\text{, when} \\ M = (m_{\mu\nu})\end{array}$$

$$(\varrho)(\beta) = \sum_\nu \varrho_\nu \beta_\nu\,.$$

Remark. This identity becomes Hecke's Θ–transformation, when K is the rational number field $|\alpha|$ the ordinary absolute value, and

$$g_1^{(\alpha_1,\ldots,\alpha_n)} \;=\; \begin{cases} 1\,, & \text{if all } \alpha_\nu \text{ are integers} \\ 0\,, & \text{otherwise} \end{cases}$$

$$g_2^{(\alpha_1,\ldots,\alpha_n)} \;=\; e^{-\pi\sum_\nu \alpha_\nu^2}$$

(Artin's "measure" of "Ganzheit" at the "infinite prime p_∞" of K, i. e. for real numbers)

$$\gamma(\alpha) \;=\; e^{2\pi i\alpha}.$$

III.) Let K be an algebraic function field with a finite field k of q elements as constant field.

Let χ be any character of the group of the divisors of K, which is a congruence character, i. e.,

$$\chi(\mathfrak{a}) \;=\; 1 \qquad \text{for} \quad \mathfrak{a} \sim 1 \text{ mod.} \, \mathfrak{f}$$

with a suitable $\mathfrak{f}$, and let $\mathfrak{f}$ be the exact Führer, i. e., the least divisor with this property, also $\mathfrak{f} \neq 1$ (the case $\mathfrak{f} = 1$ is trivial).

($\mathfrak{a} \sim 1$ mod. $\mathfrak{f}$ means, that $\mathfrak{a}$ is a principal divisor, i. e., corresponding to an element α of K, and that

$$\alpha \equiv c \text{ mod.} \, \mathfrak{f} \quad \text{with a constant } c \neq 0\,.)$$

Now let C be any divisor class (in the ordinary sense) of K, and

$$\Theta(\chi,\, C) \;=\; \sum_{\substack{\mathfrak{c} \text{ in } C \\ \mathfrak{c} \text{ ganz}}} \chi(\mathfrak{c})\,,$$

the sum extended over all integral divisors $\mathfrak{c}$ in the class C. Let m be the degree of all divisors of C, f the degree of $\mathfrak{f}$ and g the genus of K, hence $2g-2$ the degree of the differential class W. *Then*

$$q^{2g-2+f-m}\Theta(\chi,\, C) \;=\; \Theta(\chi,\, W\mathfrak{f}) \cdot \Theta(\overline{\chi},\, \frac{W\mathfrak{f}}{C})\,,$$

where W$\mathfrak{f}$ *denotes the class generated by multiplying the differential divisors with* $\mathfrak{f}$.

This functional equation follows essentially from II.) by normalising M according to I.) It gives the functional equation for $L(s,\, \chi)$ by the usual argument.

1.128 08.05.1936, Davenport to Hasse

Thanks for account of Witt's method. D. plans to go by car to Oslo.

8 May 1936.

My dear Helmut,

Very many thanks for your letter, and account of Witt's method. I have not read this yet, as I prefer to write my MS first. Nothing prevents me from doing this except infinite laziness and total lack of interest for this kind of 'formal' mathematics, where one knows there can be nothing more amusing behind things than trivial identities.

I went to Manchester on Wednesday last week, and returned on Sunday. With me was Stein, a South African mathematician (and a good one too) who was an undergraduate with Mordell, + is spending 6 months in England. There were no signs at all of any friction between the Mordells. I heard nothing of the Gribbins. I was secretly amused on the drive back to Cambridge when Stein said it was quite obvious that the Mordells had a very happy married life.

The Mordells think of spending two or three months in Norway.

I shall probably take the car to Oslo, + thence to Germany, + shall be very pleased if we can do a little sightseeing in Norway. But one must probably allow three days between Oslo + Göttingen, and that does not allow much time, if you wish to be back on the 26th.

I have renewed the subscription to the New Statesman. Do you still do the Caliban problems?

Pension Frogner[1] is O.K.

A new Wodehouse appeared a few weeks ago, but I did not send it you as I thought it was not as good as usual. It consists of several short stories.

Very best wishes + much love from

Harold

[1] undeutlich

1.129 28.06.1936, Hasse to Davenport

Pläne für den bevorstehenden ICM in Oslo. Deuring hat bemerkenswerten Fortschritt in Richtung auf R.H. Karamata.

Göttingen,
Calsowstr. 57
28. 6. 1936

My dear Harold,[1]

I have not heard from you for a long time. Claerle told me about your plans, though, and about a possibility of your getting a £1000 position at Belfast. I really do not know whether I should wish you to get it, left alone the personal reason for me that you will be removed several hundred miles farther from places within our reach. I think mathematical life at Belfast will be somewhere about absolute zero, and the chances to take a decent part in contemporarian mathematical life are very "remote". Nevertheless I am very glad for you that you have got the chance. For I suppose this means some official body acknowledges your ability for a position of this order of magnitude. I am anxious to hear about the further development.

Claerle is rather disappointed that you did not write her for so long. She somewhat resents it that you do not seem too eager to let her take part of what is going on in your life. I, too, should like to hear from you soon. As Claerle already wrote you, there are several reasons for me for being back here earlier than I originally intended. In point of fact, I am afraid I shall have to be in Göttingen on July 22^{nd}, or 23^{rd} at the utmost. So, if you still intend doing some sight–seeing in Norway after the Congress, please let yourself on no account be influenced by me. It will be a rather unique opportunity for you to see some of the beauties of a remote country, and I should not like to stand in your way for making all you can of it. On the other hand, I suppose that now you have decided on going to Norway a fortnight before

[1] Added by Clärle on the margins of this page: "Lieber Harold — es scheint mir so, als ob Du gar keine Lust hast zu uns zu kommen. Denke nicht, dass ich Dir darum zürne, ich möchte Dir nur nochmal offen sagen, dass Du ohne Rücksicht auf uns das tun sollst, was Dir lieb ist. Wahrscheinlich geht in diesen Wochen innerlich mehr mit Dir vor, als Du Dir selbst zugestehst. Von Herzen wünsche ich Dir glückliche Zeit — Dein Clärle"

the Congress, you will perhaps do some sight–seeing now, and be free for leaving with me very soon after the Congress. Let me know, at any rate, as soon as possible, how matters stand. For what with currency difficulties, Congress deduction on railway fares, following suit in an official delegation etc, I should like to order my tickets as soon as possible, and I must know for this purpose, whether I must order single or return.

Did I write you that Deuring made a remarkable progress towards R. H. for $g > 1$? I shall tell about it in my Oslo lecture.

We have a regular Balkan invasion here during the last week of this term, organised by Blaschke. Representatives from several Balkan countries are visiting several mathematical condensation points, such as Hamburg, Göttingen, Berlin, in Germany, on their way up to Oslo.

I usually let them deliver a rather disappointing lecture in broken German and then give them a spin in my car. To–morrow we expect the star amongst them, Karamata from Belgrad.

I wish you a weather more than merely clement for your stay in Norway. Please let me hear soon.

Much love,

yours,
Helmut.

1.130 11.09.1936, Davenport to Hasse

Bilharz. Heilbronn. Erdös and Turan. Prime number theorem for polynomials. Distribution of primitve roots mod p.

11.9.36.

My dear Helmut,[1]

Many thanks for your letter. I enclose Caliban's letter, + various things (MS, + paper by Erdös + Turan) connected with the Billharz. You will see from my MS that the B. is (a) true and (b) not trivial.

You may remember that what happened was that once when I was in Göttingen you mentioned the question to me + I said that the inequality in question was true + could be proved by induction on the number of primes, by an argument due to Heilbronn (unpublished). This informn. you passed on to Billharz, who has reproduced it without rediscovering the argument, which is not surprising, as it is a little tricky. The enclosed MS is 3/4 Heilbronn + $\frac{1}{4}$ Davenport.

I should like to have the MS back sometime (no immediate hurry) to file it in case the same thing should turn up again sometime somewhere. If you would like H. to publish it, to facilitate B.'s reference to it, I daresay I can persuade H. to do so.

You needn't return the paper by Erdös + Turan (which I send in case it is unknown to you) as I have another copy.

I have done a little work on the prime number theorem for polynomials since I got back, + also thought a lot (together with H.), on the distribution of primitive roots mod p. It is very easy to prove that there are 2 consecutive primitive roots mod p provided $p > c_1$. Our old friend

$$\pi(\chi, \psi) = \sum_x \chi(x)\psi(1-x)$$

turns up of course. But we have so far been unable to improve on $O\left(p^{\frac{1}{2}+\varepsilon}\right)$ for the least primitive root (see Landau vol. 2)

[1] Seit dem letzten Brief war Davenport zu einem Besuch bei Hasses gewesen. Siehe Brief von Hasse an Hensel vom 30.7.1936.

Naturally it is a problem which is not likely to lead anywhere, but it can exercise a certain fascination.

I failed to do last weeks Caliban (the 15 numbers). Did you succeed?

I am going to Harrow tomorrow for a week (please tell Clärle). Though Cambridge itself is ideal now.

Love to Clärle + very best wishes from

Harold.

1.131 26.09.1936, Davenport to Hasse

Dirichlet density vs. Abel density. Heilbronn.

26/9/36.

My dear Helmut,

Enclosed are some notes on your conjectured identity. It is altogether too much to expect the natural density to be correct. What you call the Dirichlet density ought better to be called Abel density since it arises from a series $\sum a_n p^{-ns}$, which is a power series, not a D. series. If you look at it from the power series point of view, the identity suggested (as a 'Tauberian' result) is the one I actually prove in these notes.

I will persuade Heilbronn to publish the inequality. I disagree that the case "all $x = 1$" is not interesting – it has a genuine, inherent interest, whereas the case $x_i = \frac{1}{q_i} + f(m) =$ exponent has only an accidental interest.

Have been to London today returned with car, which was found yesterday. All luggage gone + not much hope of recovery – also Clärle's Häs'chen.

Love to the real Häs'chen

Yours in much haste

Harold.

1.132 31.10.1936, Davenport to Hasse

Work with Heilbronn. Hardy.

31.10.36.

My dear Helmut,

Many thanks for your previous letter and your card on my birthday. I am sorry I have not written for some time. I have actually been working quite hard, with Heilbronn. We have succeeded, after some difficulty + many moments of utter despair, in proving that almost all numbers are representable as $x^3 + y^3 + z^2$. (Almost all in the usual Waring sense – number of exceptions $\leqq N$ being $o(N)$ as $N \to \infty$). The difficulty lies in dealing with the 'singular series' of the problem, which turns out to be a partial sum of a combination of L-series of algebraic fields at $s = 1$. I think the L-series are those of $P\left((4n)^{\frac{1}{3}}, \sqrt{-n}\right)/P(\sqrt{\texttt{[...]}})$ and $P\left((4n)^{\frac{1}{3}}/\sqrt{-3}\right)$ or something like that (n being the number we are trying to represent). But actually this way of tackling the S.S. turns out to be hopeless unless one assumes some sort of Riemann hypothesis, + we have found an elementary way of 'getting round' the L-series.

How you do like Hardy's magnificent sentence – "if all the scientific ambitions of my life were realised etc." It would not do for you to say that in Germany today though. Do not talk nonsense about paying for this little booklet. I found it by chance at a bookseller's, by the way. By all means pay my DMV subscription, but remind me to settle with you.

All this work with Heilbronn has kept me happy + fascinated, but it has prevented me from doing anything about congruence ζ fns. etc.

Congratulations on your crossword successes. Somehow I never have the interest to do them when I'm here.

Many thanks for your good wishes.

Yours

Harold.

1.133 13.11.1936, Hasse to Davenport

Hardy?

13.11.36

My dear Harold,

Many thanks for your kind letter. I am very glad to hear that you have found your way, in spite of many moments of utter despair, to a remarkable arithmetical result. I regret though that you had to get round the interesting L-series.

I do not know what you mean by Hardy's magnificent sentence about scientific ambitions. In what connexion did he utter it, and to whom?

It turned out that the very strict financial regulations forbid me to settle the account with the Deutsche Mathematikervereinigung, at any rate without getting an official permission which will take considerable trouble. So I must ask you after all to settle the bill by yourself. You owe RM 5.- for 1936.

Otherwise nothing of interest.

Kindest regards and best wishes

Yours

1.134 11.12.1936, Davenport to Hasse

Abdication of King Edward. More work with Heilbronn.

Friday. 11. Dec. 1936.

My dear Helmut,

I am indeed very sorry that I have been so long in writing to you directly, though I suppose Clärle has told you anything of interest in my letters to her. The fact is that I am getting more and more lazy (time for a New Year Resolution, in fact), and for a few days last month I felt pretty rotten in health. But these excuses are so feeble that I had better abandon the point.

Well, our little "crisis" has come and gone, and Edward VIII became Mr Edward Windsor at 1.50 p.m. today, when Parliament finished passing the Act which gives effect to his Declaration of Abdication. On the whole it has all gone off very well. The position up to yesterday remained much the same as that I outlined in my letter to you a week ago. Actually the King's final decision to abdicate was probably taken on Sunday or even before, but a few days were allowed to elapse before it was announced in order that there should be no suspicion that he was hurried into it by the Government, + in order that all preparations should be made for it to be done properly. (There has been no voluntary abdication in English history + some of the legal points involved were rather ticklish). Of course every day that elapsed made everyone more reconciled to abdication. There has been a gradual recognition that the late King had intended for months to raise the question + therefore must have considered his personal feelings as of more importance than all the trouble he would cause the country. It also became known that he was strongly influenced by a clique of Anglo-American Society people who were only out for a good time + had no sense of responsibility. Also the King's tour of the distressed areas a few weeks ago assumed a different aspect when one saw that he was probably only trying to acquire extra popularity for the coming struggle.

I sent you the Sunday Times of Sunday, the Times of Tuesday, part of the Cambridge Daily News of yesterday, and the Telegraph + Morning Post of today. I hope it has not all bored you. After what the Times wrote on Tuesday (middle column of p. 16) I felt that abdication was absolutely certain. I made a bet on it and won it.

What support the King had was only from sentimental women + isolated politicians who wanted to make capital out of it. Of course the issue cut entirely across all party divisions.

Sat. morning

Sorry I could not finish this letter last night. I had to play bridge + listen to King Edward's broadcast. Did you hear this by any chance? I thought he spoke extremely well, and I found it very dramatic and moving, although I am not particularly in sympathy with him.

The phrase I quoted from Hardy when I wrote to you once was from his Oxford lecture which I sent you.

I am now busy marking Entrance Scholarship examination papers, which have to be done today and tomorrow. One question I set was the following discovered by Pappus :

"A, B, C are three points in a straight line. Semicircles are drawn on AB, BC, AC as diameters, all on the same side of the line ABC, and a circle is drawn to touch the three semicircles. Prove that the diameter of this circle is equal to the perpendicular distance of its centre from ABC."

There is a solution in 3 lines !!

I hope to come over sometime in January, just for a few days, if it will not put you to too much inconvenience.

I will write again to Clärle in a day or two.

Love to both + all the very best wishes from

Harold

P.T.O.

P.S. Heilbronn + I also proved a few weeks ago that almost all numbers are representable as $p + x^k$ where k is a given fixed positive integer, and p is a prime, x a pos. integer. This is easier than $x^3 + y^3 + z^2$.

1.135 22.12.1936, Davenport to Hasse

Agatha Christie. D.'s Enzykl. article. Waring'sproblem. Monopoly.

Tuesday 22 Dec 36.

My dear Helmut,

My very best wishes for a Merry Christmas and a Happy New Year.

I hope you enjoy the Agatha Christie. The calendar is a poor substitute for the Times Calendar, which is out of print and unobtainable.

The only event of interest I have taken part in recently was the presentation from the College of a Silver Cup to the Master (Sir J.J. Thomson) on his 80 th birthday. He is still quite active, though his speeches get more + more rambling.

I discovered with horror the other day that the date for my Enzykl. Article was 31.12.36 and not, as I had supposed, 31.1.37. I have written to Dr Heisig for permission to delay to this date, + will "get down to it" the day after Christmas Day. It would be a great help to me if I could come over + consult you about some of the paragraphs sometime in January. I see now that I have spent too much time this term on Waring's problem and things connected with it.

There is a new game out called Monopoly, which we have played with great pleasure at Cambridge. If I come over I will bring one with me.

I hope you will have an enjoyable time at Christmas + New Year, and enjoy a release from work.

Kindest regards from all, + especially from

Yours

Harold.

1.136 29.12.1936, Davenport to Hasse

Possible visit to Gö? Algebraic functions by Bliss? Hardy returned from U.S.

Very best New Year wishes to all in Calsowstr 57.

29 Dec. 1936.

My dear Helmut,

Very many thanks for your kind letter, which I received when I got here on Sunday (together with my mother, who is staying for the week at the Blue Boar). I greatly appreciate your very kind remarks, and reciprocate your feelings.

I had not seen the Eddington problem in the N.St., but will have a look at it. No doubt he was led to it by his recent researches, which have been in that direction.

As you say, the N.St. is very biassed. But it is one of the few journals written by people who really are intelligent. I agree that it is not always reliable, though frequently it is the first to point out facts unpalatable to the British + French Governments.

The presence of between 5000 and 15000 Arbeitsdienst + S.S. men in Spain is now an undisputed fact. So far they have not come into the fighting, and are apparently enjoying themselves in Seville. But Franco is so unpopular in Spain that he may not be able to do anything without a much larger number. If these are sent from Germany (Italy now being lukewarm) the possibility of a European war in the near future becomes a strong probability. The Russians have sent very few men to Spain, though a fair amount of material. I merely mention these as facts, not with the object of annoying you !

30 Dec 1936.

I am glad you found the English lending library useful; it did not impress me at the time, but perhaps I did not see all the books. As regards early Wodehouse, the Ukridge stories are about the best.

Many thanks for your advice about my possible visit to Göttingen. I think I shall either come for Jan 9 – Jan 16 or else come later in Jan. or Feb. I have had a letter from Heisig implying that there is no hurry about the Enzykl. Article. On the other hand I have to lecture this term on alg. functions of which I still know nothing. I had hoped to postpone this until the summer term, but find this difficult to do without giving trouble to various people. So what I now think is, that if I can prepare a fair amount of my course on alg. fns. in the next 10 days then I will come over for Jan 9–16. The term is effectively (i.e. as regards my lectures) from Jan 16 to March 12. I could come away in the middle of the term by getting Heilbronn to lecture for me.

By the way, if you have a moment to spare any time while in the Institute, please have a look at "Algebraic Functions" by G.A. Bliss, and let me know what you think of it. Is it fairly good? (taking the constant field to be complex numbers, of course).

Cambridge is very quiet just now, but nevertheless pleasant. Heilbronn has gone to Holland, and Hardy, who arrived back from U.S.A. a week ago has just gone to Brighton.

My very best wishes for a happy + successful 1937

Yours, Harold.

Kindest regards from my mother.

1.137 20.01.1937, Davenport to Hasse

It appears that D. had visited H. in Göttingen for a week, as he had envisaged in the foregoing letter. D. has started given lectures on algebr. functions according to the lecture notes which H. had given to him. D. finds the subject unsatisfactory.

20 Jan 1937.

My dear Helmut,

I hope you received safely the Times of Monday, also the Times and Truth of today. In the first of these there was an important article by a correspondent in Spanish Morocco; in the second was Mr Eden's speech and also a short but pregnant summary of a speech by an American observer in Spain on p. 16; in the third there were some interesting paragraphs on new German fortifications. I do not think Truth, which is an old-established weekly of high repute, is likely to have published statements on unsufficient evidence.

I do not, of course, send these periodicals in the hope that you will change your opinion, but merely to draw your attention to facts, or probable facts, which you would not otherwise hear of.

Incidentally, would it not be better for you not to receive the N.St.? I have heard of people getting into serious trouble with less innocent (i.e. innocent from the Nazi point of view) periodicals. I say this only in your own interest.

If you like I will divert your copy to myself + cut out Caliban + the crossword + send you them.

So far I have given two lectures on alg. fns., following your notes + dealing with rat. fns. I find the subject difficult to explain clearly, even when I completely understand the point at issue. It took me a little time to realise that the "residue of ξ at a place" where ξ is an element of $K = K(x)$ ought to be called the "residue of ξ with respect to x" at the place.

Soon I shall depart from your notes + revert to Tung + Bliss + Hensel.

I find the subject unsatisfactory in that the ideal of the theory namely "invariance with respect to the choice of generating element of the field" applies only to enunciations of theorems + hardly ever to proofs.

My lecture time is, unfortunately, 9 a.m., so that on Tuesdays, Thursdays + Saturdays I get up at 8.45 and on other days about 11. At present I play a good deal of Monopoly and of the Russian card game.

I hope that both Clärle + you are well + send you both my very best wishes.

Yours,

Harold

1.138 28.01.1937, Hasse to Davenport

H. sends a few more notes on algebraic functions. H. replies to D.'s remark that he finds H.'s viewpoint unsatisfactory. H. argues in favor of stressing the invariance of the theorems with respect to generators of the function field. H. is looking forward to seeing D. in Gö. before very long.

28.I.37

My dear Harold,

Thanks very much for your extraordinary letter and also für the ordinary one. We had better discuss the former when you are here. As to the latter, I thank you for Times and Truth. I read the paragraphs you indicated with interest. I don't think it is necessary to adopt your proposal about the N.St.

I send you a few more notes on algebraic functions. Please put them together with the other lecture notes I gave you here. From your remark about the residue of an element z at a place $\mathfrak{p}$ I see that you have not understood this notion. In point of fact, this residue is independent of which element x takes the role of a generating element. The place $\mathfrak{p}$ is primarily defined as a correspondence from the function field K to the constant field k, so that to every element z of K corresponds an element c of k, which can also be ∞, and so that addition, ..., for the elements z corresponds to addition, ..., for the elements c. Only the enumeration of all places $\mathfrak{p}$ involves a preference for a generating element x. For, in order to enumerate all $\mathfrak{p}$, one considers the residues of a given generating element x:

$$x \equiv a(\mathfrak{p}),$$

and if K is algebraic of higher degree over $k(x)$, also the residues of a generating element y for $K \mid k(x)$:

$$y \equiv b(\mathfrak{p}).$$

Then with a finite number of exceptions, the places $\mathfrak{p}$ correspond unitely to the pairs of residues a, b satisfying the fundamental equation $f(x, y) = 0$ between x and y.

You complain about the invariance with respect to the choice of x and y applying only to enuntiations of theorems and hardly ever to proofs. That is so of course. But you can't reasonably expect more. The point is rather, that in each such proof the choice of x as an element which is transcendental over k is arbitrary, and also the choice of y as an element which generates $K \mid k(x)$. From my point of view one ought to bear this in mind continually during the whole course and one can't respect it often enough.

Therefore I rather welcome every opportunity of emphasizing this invariance throughout my whole course of lectures.

I am looking forward to seeing you here before long.

Sincerely

Yours

1.139 06.02.1937, Davenport to Hasse

Misunderstanding about notion of residue of diffeential. Thanks for the manuscript on Riemann-Roch. Last paper by F.K.Schmidt too highbrow.

Sat. 6 Feb. 1937.

My dear Helmut,

Very many thanks for your two letters. Your first one (about algebraic functions) was based on a misunderstanding of what I meant when I said that the residue of an element of $K(x)$ or $K(x, y)$ at a place was not invariant. Here I meant 'residue' in the meaning 'Residuum' not 'Rest'. My point was that I had then realised the necessity for considering differentials and not functions only.

I am very glad there is no hurry about the Encycl. Article.

Larwood is a cricketer, and in fact the cricketer around whom the body-line bowling controversy centred a few years ago. The point of the new form of bowling he introduced was that it was a 'fast' bowling directed more at the batsman than at his bat. Hence: hard and fast rule.

Wilfred Shadbolt + Jack Point[1] are the jailor of the Tower of London, and the jester respectively in the Gilbert + Sullivan Opera 'The Yeomen of the Guard'. In the course of the Opera they sing a duet – "Tell me a tale of a cock and a bull".

I hope the enclosed booklet will amuse you.

English opinion regarded Hitler's speech as intended simply for home consumption, as it did not answer any of Eden's points, and gave a false impression of the position e.g. the statement that Hitler's peace suggestion of March 1936 was rejected is quite false – the fact is that the English Government immediately sent a polite but objective Note asking for details, which Note was simply ignored and never answered in any form – an unusual diplomatic procedure.

I do not suppose you have heard that Herr von Ribbentrop has managed to make himself rather unpopular in England, partly by neglecting his business (he has spent more than half his time in Germany), partly by intriguing

[1] undeutlich

against Eden with English politicians who are out of office. His last faux pas was a few days ago, when on being presented with the other diplomats to King George VI, to hand his credentials to the new King, instead of making the customary slight bow, he clicked his heels and made the Nazi salute.

I only mention these things as facts.

Very many thanks for the Riemann-Roch MS. The proof in Schmidt's last paper is too highbrow for me or my class, though no doubt it is a good piece of work.

I have explained to Clärle what the position is about my visiting you, and very much hope we can make it mutually convenient. I look forward with great pleasure to seeing you again.

All the very best wishes,

Yours

Harold.

1.140 15.06.1937, Davenport to Hasse

On Vinogradov's triumph: All odd numbers representable as a sum of three primes. D. has given a talk on it in Hardy's class.

Tuesday. 15.6.37.

My dear Helmut,

Have you heard of Vinogradov's latest triumph? All large odd numbers are representable as sum of 3 primes. Method: Hardy-Littlewood method (without any hypothesis) + Siegel's class-number theorem + sieve of Eratosthenes + a new lemma. I gave a talk on it at Hardy's class last Tuesday. Vinogradov has already received a prize for it.

Did you try the tennis problems in Caliban?

All the very best wishes

Yours

Harold.

1.141 07.10.1937, Davenport to Hasse

D. thanks for the "happy weeks which I have just spent with you." D. wishes that all goes well with H.'s book. D. is "not enthralled" with his new job (assistant lecturer at the University of Manchester). D. criticises H. for accepting Rohrbach's paper vol.177. D. points out that the conjecture was his, and the result had been proved by Heilbronn.

82 Derby Road
Heaton Moor
Stockport.

Thursday 7. Oct. 1937.

My dear Helmut,

I should like to say how grateful I am for the happy weeks I have just spent with you. I am only sorry that I was not able to be of more help to you with your great task (the book, of course I mean).

So far I have spent 24 hours here, and cannot say I am enthralled by the prospect of staying here for years. But then the main thing that repels me is work, and that exists everywhere.

Now I have something to say which constitutes a criticism of you. I hope you will take it in good part, + correct me if I am wrong. I found here awaiting me for review in the Zbl. a paper by Rohrbach (Crelle vol 177, pp 193-6). I am really surprised that you should have let it appear in this form. As far as I remember, the result was never a *conjecture of yours* but was a *theorem proved by Heilbronn in 1933* (conjectured by me, I think) which I told you about in 1935 (or perhaps 1936) because you asked me how to prove the thing which Billharz wanted. Rohrbach's paper does not contain even the word Heilbronn, and being as it happens dated earlier than Heilbronn's paper in the Proc. Camb. Phil. Soc. will create the impression of being the earliest proof. I am surprised that you, as editor of Crelle, should have let it appear in this misleading form.

As I say, I only found it yesterday, + so have not spoken with Heilbronn about it. No doubt he will take it very lightly. But I feel myself to blame in the sense that, as a result of my relating the thing to you, it now appears as X's proof of a conjecture of Y ($X, Y \neq$ Heilbronn).

I hope you do not mind my speaking quite frankly.

My journey was quite uneventful. Heilbronn met me with the car at Harwich, which was rather fortunate, as the train to London which I would otherwise have travelled on had an accident – though nobody was seriously hurt.

I hope all is going well with the book + that you are not overworking too much. I enjoyed very much my stay.

Hope are you getting on with Martin Chuzzlewit?

All the very best wishes

Yours,

Harold

1.142 12.10.1937, Davenport to Hasse

D. is getting to work in his new job. Mordell just has exciting news from Siegel who solved the inhomogeneous Minkowski problem. D. had not yet the time to do the joint paper of H. with H. L. Schmid (on the exceptioal classes in the hyperelliptic case). It appears that H. had sent him a reprint.

82 Derby Road
Heaton Moor
Stockport.

12 Oct. 1937.

My dear Helmut,

I do hope that you were not offended by my last letter : it was written rather on the spur of the moment.

I am getting down to work here, + not as yet finding the lecturing and example-correcting too bad. One of my classes consists of 42 engineers + physicists who have to learn the calculus. With them the difficulty is not what to say, but to know whether to repeat everything merely three times, or whether thirty times is necessary.

Mordell has just had the exciting news from Siegel that he has solved the inhomogeneous Minkowski problem – though as yet he has not got the exact constant (presumably 2^{-n}) but expects to do so.

I have not yet had time to do any of the joint paper with Schmid, but hope to be able to get on with it soon. The last part of Mordell's 1932 paper on exp. sums is not yet quite clear to me, but clearer than it was.

Kindest regards to all, + the very best wishes,

Yours, Harold.

1.143 17.10.1937, Davenport to Hasse

On Rohrbach's paper. Heilbronn. Bilharz. Siegel. Mordell. Erdös. Ko. Mahler. On the whole, D. finds Manchester very tolerable.

82 Derby Road
Heaton Moor
Stockport.

Sunday. 17.10.37.

My dear Helmut,

Many thanks for your letter. Of course I never had the least doubt about Rohrbach's proof being his own; but I confess I am a little surprised that you have entirely forgotten how the question came into Billharz's dissertation. It came in by you asking me once (certainly before Sept. 1936, but perhaps in August 1936) whether I could see any way of proving the inequality

$$\sum_m \frac{\mu(m)}{mf(m)} \geqq c > 0.$$

I replied that I was sure it could be proved by a modification of the method by which Heilbronn had proved his inequality (which he had shown me in 1933, but not published because it did not seem to have any application then.)

You say that Billharz referred to me when the question of a proof turned up later, and that is why I think you might have mentioned Rohrbach's paper to me sometime.

Let us imagine the case the other way round. Suppose you had discovered something + told me the result, but not the proof. If I then came home and found a proof, it not would be right of me to publish the result and proof without either giving a reference to you or waiting until your paper had appeared.

As it is, anyone who looks at both papers is bound to give Rohrbach priority and even to suspect Heilbronn of suppressing a reference to Rohrbach!

As I expected, Heilbronn regarded the matter as utterly trivial. But I think it would be a good gesture on your part if you wrote him a few lines to say that you regret having overlooked that the result came from him + had already been proved.

I propose to review Rohrbach's paper simply by saying "Rohrbach gives an independent proof of a result due to Heilbronn (ref. ...)" and a few words about the proof. I hope you agree to this.

(Monday).

I was wrong in saying that Siegel's proof might lead to the true value of the constant in Minkowski's problem. I have found a simplified version of Siegel's proof, but the constant is very large.

The amount of work I have to do here is not in itself excessive. I lecture 6 hours a week, and reckon roughly that 1 hours lecture requires $1\frac{1}{2}$ hours extra[1] in preparation and correcting solutions to exercises. Tuesday and Saturday I have entirely free. Mordell lectures two hours a week on what he calls "Algebras", but this seems to be simply an introduction to modern algebra as a whole.

Erdös, Ko, and Mahler are all here. Naturally I miss Heilbronn rather. On the whole, I find Manchester very tolerable – but I expect a revulsion[2] of feeling will come in a few weeks. I cheer myself with the reflection that periodical visits to you will be oases in the desert.

I am glad to hear you continue to enjoy Chuzzlewit.

The "rascal" clue is absolutely unintelligible to me.

Regards to Ziegenbein and to Schmid, and all the very best wishes,

Yours,

Harold.

Donald (Sadler) has gone on a Mediterranean cruise.

[1] undeutlich
[2] undeutlich

1.144 26.10.1937, Davenport to Hasse

H. has written to Heilbronn. Explanation of the result of Siegel on Minkowski's conjecture. H. has invited D. again.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

26 October 1937.

My dear Helmut,

Many thanks for your letter. I am glad you wrote a few words to Heilbronn. Of course I now understand how the thing happened, but you will also understand that I had to point out to you what the history had been. I reviewed Rohrbach's paper in the Zbl. simply by :

"The author gives an independent proof of a result due to Heilbronn (see this Zbl. **16**, 290).",

and I wrote to Neugebauer a few words of explanation. I did not write any more in the review, because (1) the review of Heilbronn's paper, done by Khintchine, was very brief, and (2) because I was not greatly impressed by Rohrbach's paper, on reading it. His Hilfsatz contains an irrelevant hypothesis (namely $t \mid P_n$), and can be proved in one line instead of twenty – the proof does not need a single formula.

The result proved by Siegel is:

Let $L_1, \ldots, L_n$ be real homogeneous linear forms in $x_1, \ldots, x_n$ with determinant 1, and $c_1, \ldots, c_n$ be n real numbers. Then there exist integers $x_1, \ldots, x_n$ not all zero such that

$$\prod_{i=1}^{n} |L_i + c_i| < \gamma_n,$$

where γ_n depends only on n.

Minkowski's conjecture was that this is true with $\gamma_n = 2^{-n}$, and this has been proved for $n = 2, 3$. The simplified version of Siegel's proof which I have made gives an explicit value of γ_n for which the result holds, but a very large one, roughly $n^{\frac{1}{2}n^3}$. But previously no result at all was known, though several people had tried to get one.

Thanks for your invitation to the oasis. Although I had not intended the simile to be interpreted geographically, it suits very well, for it implies that the rest of the axis is a desert! I also had a P.C. from Donald, + had the same recollections as you. I very much hope that we shall travel there together again sometime.

I am glad you like Anstey's book. The stories were all written about 40 or 50 years ago. Vice Versa + the short ones are the best : the others are too much based on one idea. Of course there was nothing great about Anstey, but he was amusing and a competent writer.

I frequently do the Telegraph crosswords, which are slightly easier than the Times. All the very best wishes, + looking forward to seeing you again,

Yours,

Harold

P.S. Mordell has given Mahler an assistant lectureship, to his great joy. There is suddenly a shortage of young English pure mathematicians without jobs.

1.145 30.10.1937, Davenport to Hasse

Thanks for H.'s birthday wishes. D. has simplified Siegel's proof and got a somewhat better constant. D. will wait with publication until Siegel has published his proof. Referring to H.'s trouble with H. L. Schmid.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

30 October 1937.

My dear Helmut,

Very many thanks for your letter, with its kind and ingeniously-phrased good wishes. I am afraid I cannot reply in as witty a strain[1]; my epistolatory style as a whole is rather a dull one!

We have not yet got the right constant in the Minkowski problem – or indeed any real improvement on the one I got at the beginning. Mordell has not yet written anything about my simplification of Siegel's proof to Siegel out of the fear that Siegel might not publish his. But as soon as he does so I will send you a copy, as the idea is very simple + may amuse you.

Am I right in inferring that by part 2 of your book you mean vol. 2? If so you are making excellent progress, indeed. The appearance of vol 1 of your book is looked forward to with great interest by all English mathematical circles.

I am very sorry indeed to hear of this trouble with H.L. Schmid. I will wait until I hear from him before doing anything positive or negative about my joint paper with him: I am too busy at present anyhow.

I do hope that no indiscretion of mine has had anything to do with it. I was rather cautious in conversation with him this time, but no doubt I have not always been. I mean, of course about my own views on politics etc. Naturally, I have never said anything to him concerned you which was not

[1] undeutlich

both true + entirely to your credit. [This is rather badly expressed – I do not mean that these two things are different!]

I am alone in the house for a few days; my mother + Grace having gone to Harrogate last night for the weekend, as Grace has a few days free now from her school teaching, which otherwise keeps her very busy. So I have had to wash up + cook meals. Every meal I have had at home today has consisted of bacon + eggs, so it is rather like the Mad Hatter's teaparty in "Alice", with the table always laid for breakfast instead of tea. Also, the rule of moving on one place does not lead to the same inconvenience as it did there!

I am fairly busy here : there are always things turning up apart from my actual lecturing work.

Again many thanks for your congratulations (though really condolences are more appropriate to a birthday)

Yours gratefully (etc!)

Harold

1.146 01.11.1937, Hasse to Davenport

H. asks for D.'s opinion on a new paper of Salié.

1.11.37

My dear Harold,

may I trouble you for your opinion on a new paper of Salié's that was offered me for publication in Crelles Journal. I know you will be able to see in a few minutes whether it is worth while publishing it.

With all the best wishes

Yours

1.147 13.11.1937, Davenport to Hasse

D. sends a copy of his modification of Siegel's result.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

Saturday. 13.11.37.

My dear Helmut,

I hope your cold passed away and left you quite well again. They are most unpleasant things. I felt I had one coming on badly this week, but I took a lot of drugs + seem to have killed it.

I have no particular news, but I enclose a copy of my modification of Siegel's result on Minkowski's problem. I also enclose a small problem for you – decode the message in the Personal column of the Daily Telegraph enclosed. This took me about an hour. I shall be interested to hear what you make of it – I mean, whether you get it out. Of course, I have an advantage over you in knowing a name which occurs (Although it turns out that the message is a local ie. a Manchester one, this was a hindrance rather than a help, as I was not expecting it in the Daily Telegraph. I had no "inside information").

We have experienced some horrible fogs this week, but always in the morning as a prelude to a bright day. All the very best wishes,

Yours,

Harold.

1.148 21.11.1937, Davenport to Hasse

Subfactorial function is classical in English math. education. Zeta function of function fields does not vanish on the line R(s)=1 also for higher genus. H. is still hard at work on the book. D. asks for returning the ms. on Siegel since Erdös wants to have it.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

Sunday. 21. Nov 1937.

My dear Helmut,

Many thanks for your letter. You did the code just as I did – the telephone exchange is Rusholme, suggested by the fact that M/C is the common abbreviation for Manchester. The code letters M,N both correspond to R, and the code letters U,V both correspond to H.

I saw that Caliban printed your sol'n. to the subfactorial problem. Actually the function subfactorial $n = n!\left(1 - \frac{1}{1!} + \ldots + \frac{(-1)^n}{n!}\right)$ is a classical one in English math. education : it is defined in just that way + evaluated in Chrystal's Algebra. I learnt it at school, + was rather surprised that no reference was made to the textbooks.

As regards $\zeta(s) \neq 0$ for $\Re s = 1$, of course this was given for $g = 1$ in your Hamb. Abh. paper as a Zusatz. It does not seem to be so trivial for $g > 1$, I will spend a few more minutes on it tomorrow + write if + when (horrible phrase!) I see how to do it.

Sorry to hear you are still hard at work on the book. It must be a troublesome addition to the routine term's work.

Could you send back my MS on Siegel? Erdös wants to send a copy to a Hungarian friend.

I played two quite exciting rubbers of bridge with the Mordells last night. After Mrs Mordell + I had game + 90 + our opponents nothing, they sacrified 700 to prevent us making rubber + then ended by making it themselves.

All the very best wishes

Yours

Harold.

1.149 27.11.1937, Davenport to Hasse

D. works on the minimum problem for cubic forms. K and Erdös. Mordell.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

27 Nov. 1937.

My dear Helmut,

I hope you found the M.S. on $\zeta(s) \neq 0$ for $\sigma = 1$ intelligible + correct. I am sorry I could not get it into as simple a form in the general case as for $g = 1$.

I have been doing quite a lot of work recently. If ξ, η, ζ are 3 real homogeneous linear forms in x, y, z then the best known result for

$$\min_{\substack{x,y,z \text{ integers} \\ \neq 0,0,0}} |\xi\eta\zeta|$$

was $\frac{4}{19}$ (due to Minkowski). I have improved this to $\frac{15}{16} \cdot \frac{4}{19}$[1] + expect to improve it further + generalise to n variables. (Of course if one form has rational coeffts. then the minm. is 0). By considering the integers of a particular real cubic field one sees that the minimum need not be 0. This work keeps me pretty busy as it involves complicated elementary maximum + minimum problems. Mordell is pleased with it.

I *do* hope you won't overwork yourself with the book.

Manchester is a queer place. Some days one actually sees the sun, and on the other days it is pitch dark at noon. There is one general principle applicable to this season, however, + that is that if there is no wind there is fog.

[1] undeutlich

Ko + Erdös have done some good work on quad. forms in n variables with det. 1. Apparently the class-number $\to \infty$ with terrific rapidity as $n \to \infty$.

All the very best wishes,

Yours,

Harold.

1.150 08.12.1937, Davenport to Hasse

H. had sent a manuscript but D. had not yet time to read through it since he is working on cubic forms. H.L.Schmid.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

8 Dec. 1937.

My dear Helmut,

Many thanks for your letter. I have not had time yet to read your MS. or to think over your remarks on congruence ζ-functions. I have been very busy with the "$\frac{4}{19}$ problem". The true result is no doubt $\frac{1}{7}$, arising from the forms

$$\theta_1 x + \theta_2 y + \theta_3 z$$

and its conjugates, where $\theta_1, \theta_2, \theta_3$ are any basis for $K(\alpha)$, $\alpha = e^{\frac{2\pi i}{7}} + e^{-\frac{2\pi i}{7}}$. I have got $\frac{1}{6 \cdot 3}$ instead of $\frac{1}{7}$, + am in the tantalising position of having a method in which this cubic field turns up naturally of its own accord, + yet I cannot get the $\frac{1}{7}$.

I hope your holiday plans which Clärle mentions materialise, and give you a much-needed chance for rest + recuperation from your labours.

I have not replied to H.L.S., + will not do so until I have seen you.

Next Thursday the Manchester math. dept. is monopolising a meeting of the London Math. Soc.

I cannot think of any more news. All the very best wishes,

Yours, Harold.

1.151 18.01.1938, Davenport to Hasse

D. had visited Gö. and thanks H. for the "most enjoyable time". Siegel's coming to Gö. has aroused great interest in England. Why? D. has settled the "outstanding point" about the three linear forms. Mordell had been in Switzerland. He will lecture on Siegel's work on quadratic forms. D. hopes that H. will finish his book in a reasonable time.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

18 Jan. 1938.

My dear Helmut,

Just a line to say that I am home again, with my nose to the grindstone, and to thank you for the most enjoyable time I had in Göttingen.

Siegel's coming to Göttingen has aroused great interest in English mathematical circles. One question I have been frequently asked was, what caused him to become dissatisfied with Frankfurt, but to that I was unable to give any answer.

I think I have now settled the outstanding point about the three linear forms: if $M = \frac{1}{7}$ then the forms are equivalent to the "critical forms".

Mordell claims that his visit to Switzerland gave him a new zest in life, but then he stretches out his arms as usual and adds "until I got back here". He will probably lecture this term on Siegel's work on quadratic forms.

I do hope you will slay the Jabberwock, i.e. finish vol 1 of your book in a reasonable time, and become a freeman again. Several people have remarked that you are undertaking a monumental task.

All the very best wishes,

Yours ever,

Harold.

1.152 24.01.1938, Hasse to Davenport

Siegel's reasons for his desire to leave Frankfurt for Göttingen.

24.I.38

My dear Harold,

in my capacity as a member of the Committee of the Deutsche Mathematikervereinigung I received the enclosed letter. I am completely baffled, and I don't know what I have to think about this request. How on earth can I help this man by sending him a list of our members? Do you know anything about him, and what do you think I should reply him?

I am glad to see from your last letter that you settled the outstanding point about the three linear forms.

Siegel gives the following reason for his desire to leave Frankfurt for Göttingen: He wanted to escape from the solitude of a big town into the active life of a small town. That is of course only a jesting way of putting it. So far as I know, he suffered from the unusual effects of having spent all too long a time in the same town. He had grown weary of continually meeting the same people. So he was silently pledged to go for a walk with Hellinger every Saturday afternoon. He told me that, although Hellinger was quite a good friend of his, he took no inspiration whatever from the endless conversations with him. He also told me that when a man has reached the age of 40, he cannot afford to remain a 'Einzelgänger' for the rest of his life, because the bad effects of such a 'Einzelgängertum' on the character would no longer be outbalanced by a natural elasticity of the mind as they would with a young man. I hope these remarks will help to make you and the Manchester people understand Siegel's decision.

Kindest regards and the very best wishes

Yours

1.153 29.01.1938, Davenport to Hasse

Mordell lecturing on Siegel's work on quadratic forms - hard going. Siegel's attitude very interesting. Schneider's work. Behnke. Stein. Behnke will take over the active editorship of the Annalen.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

Saturday. 29 Jan. 1938.

My dear Helmut,

Many thanks for your letter. As regards the letter from the mysterious Mr Terry (neither I nor Mordell knows anything about him), the simplest reply is that the list of members is published, (reference to which part or vol. of Jahresbericht) + is obtainable from Messrs B.G. Teubner.

I take it that what he intends to [...] is to circularise the members + invite opinions favourable to a duodecimal arithmetic for practical use, which he would then quote in his book.

Anyhow, that is the reply I should make.

Of course duodecimal arithmetics obviously is preferable to decimal arithmetic, in theory, but it is naturally impossible to change over.

I have been working on various Minkowski-type problems, but so far without any important result. Mordell is lecturing on Siegel's work on quadratic forms – we find it hard going.

Your remarks about Siegel's attitude are very interesting.

Mahler gave an account of his simplification of Schneider's work at the last two meetings of our Seminar.

I can't think of any news at the moment. We still have not been able to get a servant.

Behnke wrote to me a week or more ago about an assistant of his called Stein who whishes to come to England for a year. Behnke said he might come himself for a short visit to get into contact with people about the Annalen, which he seems to be taking over the active editorship of.

Now I have to go to the Mordells (together with the other bridge friends of the Math. Dept.) for an evening bridge. I will write a few lines to Clärle tomorrow.

All the best wishes,

Yours,

Harold.

1.154 06.02.1938, Davenport to Hasse

Behnke. D. works on the non-homogeneous Minkowski problem. No particular news.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

Sunday 6 Feb. 38.

My dear Helmut,

Many thanks for your note with Clärle's letter. The exact sentence which Behnke wrote is:

> "Ich bin, ohne dass jemand anders ausgetreten ist, in die Annalenredaktion aufgenommen und soll vor allen Dingen die Verpflichtung der Geschäftsführung übernehmen".

I have been hard at work since I wrote last, not on the same problem as before, but this time on the non-homogeneous Minkowski problem. Here I have made a considerable advance, but am not yet near to Minkowski's conjecture. I must write to Siegel about it in a few days.

There is no particular news. Ko is working a lot on quadratic forms of det 1 in 12 and more variables, and thinks that a formula given by Magnus based on work by you is wrong, but he is not yet dogmatic on the point.

I continue to divide my time between work and bridge. I am gradually improving in the technique of slam-bidding. It is surprising how often slam bids are justifiably made when playing with good players (Ko and Zelinskas are both better players than I am)

Kind regards + all the very best wishes

Yours

Harold

1.155 06.02.1938, Hasse to Davenport

About the Magnus mistake.(?) H. is labouring hard for his book. Urgent work. H. has postponed visit to Paris. Definition of meromorphisms and their norm – by means of abelian function fields and Deuring's theory.

Göttingen, den
6.2. 38

My dear Harold,

Thanks very much for your kind letter. I very much hope that the brutal calculations for $L\overline{L}L'$ will boil down to a reasonable proof, and also that Heaven may drop you one of his spare ideas for getting from $\frac{C}{\sqrt{n}}$ to 2^{-n}.

Do not trouble about the Magnus mistake any further. I am quite satisfied with what you wrote. I must have this out with Magnus when I see him next.

I am again labouring hard to store my sweet stuff well into the chapters and paragraphs of my book. You will not mind my telling you quite frankly that a visit of you just now would have been to great a distraction for me from my really extremely urgent work. However much I always enjoy your presence, and however much I should have liked a pleasant holiday with you, free from my dreadful burden, I must be hard with myself this time. I am looking forward all the more to your visit in summer.

I postponed a visit to Paris for the same reason. The Faculté des Sciences invited me to give 3 lectures in May.

I think I already told you my definition of the meromorphisms and their norm for $g > 1$. Let K be the function field over an algebraically closed constant field k. Let $\mathfrak{o}$ be a fixed prime divisor of K. For the sake of simplicity suppose, that the normalised generation for $\mathfrak{o}$:

$$K \;=\; k(x,\, y) \qquad \text{with} \quad f(x,\, y) = 0$$

has only y (instead of a basis $y_1, \ldots, y_n$) in it. This generation is defined as follows:

x an element with denominator $\mathfrak{o}^n$, n minimum ≥ 1, and the assumption is that there is an element y so that $1,\, y, \ldots, y^{n-1}$ form a basis for the elements with denominator only a power of $\mathfrak{o}$ (coefficients polynomials in x). Now let A be the field of the symmetrical functions of g algebraically independent solutions $x_i, y_i \quad (i = 1, \ldots, g)$ of $f(x, y) = 0$, i. e.,

$$A = k\{x_1, y_1; \ldots; x_g, y_g\},$$

the $\{\ \}$ indicating the formation of the *symmetrical* functions. A may be generated as

$$A = k(X_1, \ldots, X_g; Y),$$

where $X_1, \ldots, X_g$ are the fundamental symm. fcts of the x_i and Y a suitably chosen element of A. For A is algebraic over $k(X_1, \ldots, X_g)$. Here Y satisfies an algebraic equation (irreducible)

$$F(X_1, \ldots, X_g; Y) = 0.$$

Now let $X'_1, \ldots, X'_g; Y'$ be a solution of this equation consisting of rational functions of $X_1, \ldots, X_g; Y$, i. e., within the field A. I call this a *meromorphism* of K, or rather of A. The degree of A over $A' = k(X'_1, \ldots, X'_g; Y')$ is the norm of the meromorphism. I believe, this degree is already given by omitting the Y, Y', as for $g = 1$. I call the meromorphism *normalized*, when the x'_i, y'_i corresponding to $X'_1, \ldots, X'_g, Y'$ have $\mathfrak{o}$ in their denominator, i. e., become infinite when the x_i, y_i become infinite. I have proved that the norm. mer. form a ring without proper divisors of zero, and of characteristic 0, if k arises from a *G.F.* by algebraic closure.

In point of fact I defined the norm. mer. quite differently, so as to be able to define their addition easily. But my definitions, based on Deuring, boil down to the above, when one tries to express them in terms of rat. fcts.

Many kind wishes,

Yours,
Helmut.

1.156 27.02.1938, Davenport to Hasse

D. thanks for H.'s note. H. seems so make progress with the R.H. D. can solve the problem of minimum of the product of three linear forms, two of which are conjugate. But brutal calculations. Magnus has admitted his mistake in a letter to Mahler. Landau's death. Heilbronn.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

Sunday. 27.2.38.

My dear Helmut,

Many thanks for your note. You seem to be making great progress with the R.H. I should be interested to know what is your def. of a meromorphism and its norm.

I have not made any new progress since my result on the inhomogeneous linear forms conjecture of Minkowski some weeks ago. I think I already told you that I proved the conjecture with a different function of n instead of 2^{-n} on the right – a function which is like $\frac{c}{\sqrt{n}}$ for large n.

The problem of the minimum of the product $L_1\overline{L_1}L_2$ (3 homog. linear forms, 2 of which are conj. complex) which involves $\sqrt{23}$ I can solve, but the calculations will be brutal.

Magnus has admitted his mistake in a letter to Mahler some time ago. I haven't got the details in my head but can let you know if you are interested, – or Magnus will be able to tell you.

I heard about Landau's death last Monday, through Heilbronn.

I am saving the new Wodehouse until the vacation. Lectures end in a fortnight at Manchester.

Any variation on bridge seems preposterous – the game is ideal as it is! We play a good deal of it, + my technique of slam-bidding is improving.

The very best wishes,

Yours,

Harold.

1.157 10.03.1938, Davenport to Hasse

Since H. wants to get on with his book, D. is looking forward to seeing him in summer when H. will have more leisure. Surprising developments on the inhomog. Minkowski problem: Tschebotarev. D. had a very nice note from Siegel. Erdös.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

10 March 1938.

My dear Helmut,

Many thanks for your letter. Of course I quite see that you want to get on with your book, and I look forward to seeing you in summer when you will have more leisure.

There have been surprising developments in the inhomog. Minkowski problem. My method of attack turns out to be not new: Tschebotareff has used it and got $\frac{1}{2^{n/2}}$. His paper, though written in 1934 has only just appeared (printed at Kasan) and no one had heard of it until a few days ago.

But the most surprising thing is a letter from Tschebotareff to Mordell in which he says his friend Delaunay tells him he has found a "trivial" proof for $\frac{1}{2^{n-\varepsilon}}$. Although Delaunay is very good, I cannot help feeling a shade of scepticism, especially about the form of the result. Perhaps he means $O\left(\frac{1}{2^n}\right)$ as $n \to \infty$. Even that would be magnificent.

I had a very nice note from Siegel in reply to a letter I wrote him. Many thanks for writing out the def. of meromorphism in rational form. I am afraid I shall have to think a great deal to "get my bearings".

Term is almost over. I am glad I soon finish lecturing, and gladder still that Mordell soon finishes. His lectures on Siegel's quadr. formen have been difficult + I lost the thread a little while ago.

At the moment I am working on an elementary combinatorial problem of Erdös which looks trivial but probably isn't.

Best wishes for your vacation + the labour it brings.

Yours,

Harold

1.158 13.03.1938, Hasse to Davenport

Tschebotareff. Delaunay. D.'s bearings on the higher meromorphisms? Overwhelming impressions of yesterday's events in Austria.

13. 3. 38

My dear Harold,

Many thanks for your kind letter. You have all my sympathy for the bad luck you have had with the inhomog. Mink. probl. As you have not been in Zürich you will not know Tschebotareff. He is extremely clever and already famous for his essential discovery that led up to Artin's law of reciprocity.

I wonder how this remark of his about Delaunay's alleged proof clears up. Please let me know in due course.

I am sorry I have not even managed to get you your bearings on the higher meromorphisms.

We are still under the overwhelming impression of yesterday's great events in Austria. A political dream has been realised that has moved for over 120 years the best of our nation. We listened on the wireless to the enthusiastic welcome given to Hitler in Linz, and I was strongly reminded of my own experience in 1924, when Innsbruck's inhabitants greeted the German "Naturforscher und Ärzte" with endless shoutings of "Anschluss" and "Zurück zum Reich". You will readily imagine the great admiration that everybody here has for Hitler's wise policy which made this possible in spite of France and others. He has achieved within 5 years, what the Burschenschaft of 1820, the Revolutionaries of 1848, what Bismarck, and Wilhelm II, and the Republic could not bring about.

The very best wishes for your holidays.

Yours,
Helmut.

1.159 07.09.1938, Hasse to Davenport

D. seems to have been in Gö. He left Zbl.-material in H.'s office. H. will be leaving for Baden-Baden, together with Kaluza, Eichler and Kochendörffer. Heilbronn.

Göttingen, 7. 9. 38

My dear Harold,

Thanks very much for your kind letter. You left the Zbl. material in my study where I found it on the day of your depart.

I hope you found the separata I sent you before you left England among the pile accumulated in Manchester during your absence. I also hope the pile of bills you found on your arrival will not prove to be beyond your means, and the pile of circulars will not spoil the amount of leisurely and idle hours you seem to consider necessary for making mathematical progress.

David Copperfield reached me yesterday for the first time. I do not understand, how it can possibly have been here another time. There was only a letter from Bowes and Bowes a few days ago, which I had forwarded to you, and then a second letter from them, with much the same outward appearance, that came together with the book. I had also this second letter forwarded to you. It was very nice of you sending me this precious novel. I shall keep it unread until your next stay with us. I hope we shall read it then together.

I finished the last of the four CrimeClub novels the day before yesterday. It was that where a mutilated corpse was plastered into a niche in a deserted studio. I think detective novels have surpassed their climax. They are much too artificial nowadays. This particularly holds for the one where a man was made to climb a tree. You found that best of the lot you left here, I found it hardly conceivable that a man — even with most extraordinary mental capacities — could hit at that solution from the rather scanty evidence. I am not in favour of a certain deification of Scotland Yard Inspectors. Their average standard ought not to touch on witchcraft. It even ought to be considerably lower than a mathematician's. And the plots ought to be based on real facts, not on most unprobable logical constructions.

I shall be leaving for Baden–Baden on Sunday. Kaluza, Eichler, and Kochendörffer will be of the party.

The very best wishes for your tour through the Lake District. Please remember me to Heilbronn. Kindest regards also to your mother and Grace.

Sincerely yours,

Helmut

1.160 15.09.1938, Davenport to Hasse

D. asks for separata. D. has finished the character sums paper. H.'s plan to visit Finland.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

Thursday. 15 Sept. 1938.

My dear Helmut,

Very many thanks for your letter. I have not found the separata, but later (when you return to Göttingen) I will send a list of what papers of yours I have, + perhaps your secretary will then send me any others you can spare.

I returned yesterday from the Lake District, and Heilbronn caught a train to Cambridge. The weather, after the first few perfect days) turned very misty, so that it was impossible to go on the hills with safety, and we decided there was no point in staying longer.

I wrote two postcards to you from the Lake District, but I don't know whether you would get the second – I lost it after I had stamped it.

I expect to go to Cambridge on Sunday or Monday for two weeks; then I shall have to return here to put my nose to the grindstone. The Mordells are still away, it seems (in France, I think).

Today I bought a radio apparatus. I have just listened to a broadcast (through an English station) of children's play songs from Marburg. This was announced as arranged by Herr Rudolf Stein; – I don't know whether you know him.

I have finished the character sums paper finally, but have not got down to any new work.

I agree to a certain extent with what you say about detective stories. But one must bear in mind that these have been in vogue now for 15 years, and so they get more and more ingenious. I read recently one called "Proof Counter Proof" by a man called Punshon which I am sure you would like – the characterization is good.

I hope you have an agreable journey and a pleasant time in the north. All the very best wishes

Yours

Harold.

Kindest regards from my mother and Grace.

P.S. If I can be of any assistance while you are in Finland, please let me know.

1.161 10.10.1938, Davenport to Hasse

H. has started a trip to Finland. D. succeeded in getting a simple proof of Minkowski's theorem on the product of the n minima associated with a convex body and a lattice. "It has taken a long time."

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

10 Oct. 1938.

My dear Helmut,

I am glad you have been able to make your journey to the Frozen North, and I hope you have had a very good time.

I am just settling down to the routine work of term here. Two members of the bridge four (Erdös and Ko) are at opposite ends of the earth, and the third, Zilinskas, has not yet turned up. The only new arrival is Gunnar Billing, the son of a Swedish bishop, who, though extremely intelligent and agreable in every other respect, doesn't play bridge. So I am reduced to playing patience.

I succeeded last week in getting a simple proof of Minkowski's theorem on the product of the n minima associated with a convex body and a lattice. It has taken a long time.

Mahler is here, but not as a lecturer, and his place has been taken by Young, who was a Fellow of Trinity before me. You will deduce that the market for mathematicians is not a very good one – a very regrettable state of affairs.

There is a new Wodehouse, which I read with great pleasure yesterday.

With all my very best wishes,

Yours,

Harold

1.162 23.10.1938, Davenport to Hasse

D. is reading Minkowski's Gesammelte Abhandlungen. Remak. Mahler. Billing.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

Sunday. 23 Oct. 1938.

My dear Helmut,

I take the opportunity of this 'day of rest' to send you a few lines. I suppose you will have been back some days now, and will be able to work or play as you like until your term starts. I have been very busy in the last two weeks – I have more routine work than last year, and have been working on various "Minkowski" problems in my spare time. I have succeeded in finding a very simple proof of what Remak proves in an extremely difficult and complicated way (Math. Zeitschrift vols 17 and 18).

I am reading Minkowski's Gesammelte Abhandlungen ; they contain many interesting things not in his books. Hilbert's "Gedächtnisrede" gives one a most interesting picture of pre-War Göttingen.

There is a new Wodehouse out – "The Code of the Woosters". It is very good : much better than the other recent ones. It contains this sublime sentence – Although you could not say he was disgruntled, yet it was plain that he was far from being gruntled.

By a sort of self-denying Ordinance, we have given up last year's practice of playing bridge in the afternoons at the University, and instead we play on about two evenings a week. Unfortunately, Mahler remains an extremely bad player.

Billing is an extremely intelligent and well read man. We only discovered after some time that he has a wife and two children in Sweden.

Young is here as an assistant lecturer, so that this year there are three ex-Fellows of Trinity in that capacity.

With all the very best wishes,

Yours,

Harold.

1.163 05.11.1938, Hasse to Davenport

D.'s simple proof of Minkowski's theorem. Simplification of Remak's proof. H. has worked on his book, finishing touches. Yesterday posted it to Springer. Seminar with Siegel. Hope for purely algebraic proofs of A.Weil's and C.Siegel's theorem. For g=1 H. has found such proofs in last term's seminar. Mordell.

5 Nov. 1938

My dear Harold,

I have to thank you for two letters written in October. I was very much interested to hear that you eventually succeeded in getting the desired simple proof of Minkowski's theorem on the product of the n minima associated with a convex body and a lattice. If you have a little spare time, please write it down for me. Congratulations also on your simplification of Remak's proof for the Minkowski hypothesis for $n = 3$. If you think I shall understand your proof, please let me know about it, too.

Thanks also for equipping me with your scientific output of the last two years. If you go on like this, I shall soon have to provide a special case for you in my shelving system. I should be very glad indeed, if you could see your way to send me one of your future papers for Crelle. Surely, with so ample a production, it will not really matter, if one of them does not appear in England.

I have worked on my book all the rest of the vacation giving it the finishing touches. Yesterday I posted the Ms to Springer. I feel extremely relieved having this night mare off my mind. Browsing in the Oxford Dictionary for other reasons, I came across a rather funny definition of a 4–lettered word. [It] inspired me to set the enclosed Crossword for you, containing this definition as clue 21. I reckon you will have no difficulty in solving the whole thing.

I have just started my lectures on Coordinate Geometry, a most dreadful subject. The only bright point in this term is my Seminar with Siegel on Diophantine Equations of genus > 1. We hope we shall master the two outstanding objects by purely algebraical proofs for A. Weil's and C. Siegel's theorem. For $g = 1$ I found such proofs, as I told you, in last term's Seminar.

I read a review of the new Wodehouse in the Times. It was most promising.

Please remember me to the Mordells, and to everybody else I know in Manchester.

Kindest regards, also from Clärle,

Yours,
Helmut

1.164 10.11.1938, Davenport to Hasse

On the non-homogeneous Minkowski conjecture. D. is working on Diophantine Approximation.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

10 November 1938.

My dear Helmut,

I have tried hard to solve the puzzle, but seem to have come to a dead end after getting about 12 words. Indicentally, Poirot's Christian name is Hercule, not Henry – I wonder whether you are making some subtle point. I hope you have not overlooked the fact that he was a *Belgian*.

Your joint Seminar on Diophantine Equations should be extremely interesting and fruitful. Billing gave an account at our Seminar last week on his upper bound for the basis-number for the point of a given algebraic field on an elliptic curve.

I don't think the Remak proof would interest you very much, but if it does, Siegel has an account of it which I wrote to him. Although it is a great simplification of Remak, it is not the "right" proof (of course, perhaps there isn't one). I find the non-homogeneous Minkowski conjecture extremely interesting, and in one sense, the problem is a natural and rather fundamental one. It can be expressed in the form: the domain $|x_1 x_2 \dots x_n| \leqq \frac{1}{2^n}$ contains a fundamental domain for *every* lattice of determinant 1 in n-dimensional space. (A fundamental domain is a set of points such that every point of space is congruent to exactly one of them, modulo the lattice).

I am still working on Diophantine Approximation, but have got nothing definite out, lately.

The Gilbert and Sullivan people have been here for 3 weeks, and I went to "Jolanthe" the other day. It is good, but is not the best.

Kindest regards,

Yours,

Harold.

1.165 15.11.1938, Hasse to Davenport

D. has sent his proof of Minkowski's sharper theorem. About the seminar with Siegel, and the course on coordinate geometry.

Göttingen, den
15. 11. 1938

My dear Harold,

Thanks very much for your kind letter, and also for sending me a copy of your proof of Minkowski's sharper theorem. As to the latter, I am glad to say I understand it throughout and find it very nice indeed. Do you want your copy back ?

I am sorry my crossword offered you unexpected difficulties. I rather thought you would solve it within an hour or so. There was no point in my inadvertently writing Henry instead of Hercule when copying the thing. I quite realised that Poirot was a Belgian, and not a Frenchman.

There is another slight obscurity in one of the clues which I should like to put right: In "6 down" add "meshwise"; I am afraid this will not be an English word; it is clear, however, what it means in connexion with a lattice. If you are interested in the puzzle, let me know what you have got, and I will try to give you a hint or two for carrying on. Otherwise I shall send you the complete solution within a week.

Siegel and myself have started with our Seminar. We begun with an account of the analytical foundation of the introduction of Riemann's theta–funktion, which plays a dominating role in A. Weil's and Siegel's work. We have quite a few interested people in our audience, amongst them a Baltic, a Swiss, a South–African, and a Finlandian (or Finn ?). In my co–ordinate geometry there are ~ 30 students, quite a progress compared with two years ago, when I had only 10.

Kindest regards, also from Clärle,

Yours
Helmut

P. S. I am writing on a new "Silenta" typewriter, hence the bad typing. One must learn to handle it first.

1.166 03.12.1938, Davenport to Hasse

D. has been working at hight pressure. About Waring's problem. Sums of 4 cubes; sums of 16 fourth powers; sums of 14 fourth powers. D. hopes to get a purely arithmetic version of the H.-L. method.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

3 Dec 1938.

Dear Helmut,

Thanks for your letter of 15 November. My delay in replying is due principally to the fact that I had a new idea about 11 days ago – an idea connected with Waring's problem, which I had been looking for for years. Since then I have been working at high pressure, considering its consequences, and writing out detailed calculations. The following are some of the results it has yielded:

(1) almost all numbers are sums of 4 cubes (H.-L. proved 5. 4 is incidentally best possible. Perhaps all large numbers are sums of 4 cubes, but this is hopeless).

(2) all large numbers are sums of 16 4-th powers. (Davenport + Heilbronn proved 17. 16 is best possible, but this is "accidental" owing to the behaviour of 4-th powers mod 2^k).

(3) all large numbers $\neq 16^a(16b + 15)$ are sums of 14 4-th powers.

I have a slight hope that I shall get a purely arithmetical version of the H.-L. method. 'Arithmetical' means without real or complex variables – of course the proofs will still be 99.9% inequalities.

I have not had time to try the crossword again seriously. Sometime I will set you one, using some of the same clues with different solutions.

It is impossible for anyone not to be moved by the recent antisemitic actions in Germany. A distinguished mathematician, whose work I personally have reason to admire, is (among thousands of others) in a concentration camp. We hope to get permission for him to emigrate to England, but of course he will be plundered first.

I don't think it is necessary for me to say much to counteract the anti-British press campaign in Germany. It may suffice to point out that, of the killed Arabs to whom great publicity has been given, *more than half* were killed by the rebels (for opposing them). This fact alone proves that the rebels are instigated (and provided with arms and money) by foreign powers.

Kindest regards,

Yours,

Harold.

1.167 25.01.1939, Hasse to Davenport

Abschiedsbrief wegen D.'s Haltung beim Boykott des Zentralblatts.

Göttingen, 25. 1. 39

Lieber Harold,

Wie ich im November erfuhr, hast Du Deine Mitarbeit am Zentralblatt niedergelegt. Durch diesen Schritt hast Du uns tief betrübt und gekränkt, denn Du hast Dich damit in aller Form in eine Front gestellt, die gegen ein deutsches wissenschaftliches Unternehmen gerichtet ist. Dies, und die uns immer wieder vor Augen kommende Erkenntnis, dass Du auch sonst in einer Front stehst, die dem nationalsozialistischen Deutschland aus ideologischen Gründen übelwill, ist der Grund, weswegen Clärle und mir die Freude an der Fortführung unseres bisherigen freundschaftlichen Gedankenaustausches genommen ist. Und deshalb bitte ich Dich auch, das Abonnement auf den New Statesman für mich nicht zu erneuern. Ich danke Dir, dass Du ihn mir so lange hast schicken lassen, und auch für den Times–Kalender, den ich zu Weihnachten wieder von Dir bekam.

Clärle und ich empfinden mit Wehmut, dass die vielen Wochen und Monate, die Du in Deutschland verlebt hast, zu keinem anderen Ergebnis als Deiner jetzigen durchaus negativen Haltung geführt haben.

Dein Helmut

1.168 05.02.1939, Davenport to Hasse

Reply.

Acton,
82 Derby Road,
Heaton Moor,
Stockport.

5 February 1939.

My dear Helmut,

I was sorry to receive your letter, and to see how your prejudices [...] your sense of proportion.

By the actions of its publishers, the Zentralblatt completely lost its former international character. These actions, and the way they were undertaken, made it impossible for anyone outside Germany (or Italy) to continue their connection with the Zbl. The resignation of Hardy and Veblen (whom you praised for their impartial attitude) shows that this is a plain fact. No question of any "front" arises; what happened was simply that I resigned from an organization which suddenly adopted new principles and practices which I don't approve of.

I have never expected *you* to make any sacrifice of your principles, and my feeling of friendship for you was unaltered even when you joined so enthusiastically the Nazi Party. At your request, I withdrew my resignation from the D.M.V. after that Society had changed its character, and there was no longer (from my point of view) any reason why I should belong to it.

You seem to have got into a state of mind now in which you feel every political difference between us as a personal insult. It seems to me to be a silly attitude to take up, – quite unworthy of a man of your qualities of mind. But until you change it, I suppose my assurances of friendship will fall on deaf ears.

I shall always look back with great pleasure on the happy years before your life became dominated by politics.

My very kindest regards to you both,

Yours,

Harold.

1.169 22.11.1946, Hasse to Davenport

Prof. Dr. Helmut Hasse
(20b) Göttingen/
Hannover
Münchhausenstr. 17
Brit. Zone,
Deutschland

22. November 1946

Lieber Harold,

Wir haben die beiden in Deinem Auftrag gesandten Pakete erhalten — eines aus Dänemark mit Butter, Speck, Wurst und Käse, das andere aus der Schweiz mit Zigaretten — und möchten Dir sehr herzlich danken, dass Du jetzt, wo die Möglichkeit zu solchen Sendungen gegeben ist, in dieser Weise an uns gedacht hast.

Wenn ich mich im vorigen Sommer, unmittelbar nach meiner Heimkehr aus Lazarettgefangenschaft, unter Zurückstellung meiner eigenen Hemmungen und gegen den ausdrücklichen Willen Clärles doch dazu entschloss, Dir durch Todd neben meinen Grüssen die Bitte zu übermitteln, uns über kurz oder lang durch solche Sendungen zu helfen, so geschah das unter dem unmittelbaren Eindruck der Not, in die wir durch den Zusammenbruch unseres Landes gestürzt waren, und in Voraussicht der noch viel schwereren Notlage, die da nach allen vorhandenen Anzeichen für unser Land und uns persönlich kommen musste, wie sie ja dann auch wirklich eingetreten ist. Leicht ist mir dieses Bitte gewiss nicht gefallen, das kannst Du mir glauben, und es war in erster Linie der Gedanke an Clärle, über deren Gesundheitszustand ich Dir nachher noch einiges schreiben werde, der schliesslich den Ausschlag gab.

Du darfst überzeugt sein, dass ich bei einem für uns glücklichen Ausgang des Krieges nicht gezögert hätte, bei der ersten sich bietenden Gelegenheit von mir aus, in Erinnerung an unsere langjährige schöne Freundschaft, die Hand zu einer Aussöhnung mit Dir auszustrecken. Ich hatte gehofft, nun wo es anders gekommen ist, auf Deiner Seite eine solche Bereitwilligkeit zu finden. Umso trauriger war ich, als ich kürzlich über Magnus durch Todd erfuhr, dass du “cross” seist.

Das was uns getrennt hat, war ein kleiner, in unseren beruflichen Lebenskreis eingreifender Ausschnitt dessen, was im Grossen zu dem tiefen Zerwürfnis zwischen unseren beiden Ländern und damit zu dem so unheilvollen Krieg geführt hat oder jedenfalls entscheidenden Anteil an dessen Ausbruch hatte. Ich will auf die Einzelheiten nicht zurückkommen, die ja durch den Ausgang des Krieges ihre Bedeutung verloren haben. Vielleicht war es damals nicht richtig von mir, dass ich Deinen Schritt zum Anlass des Abbruchs einer Freundschaft nahm, welche durch die seit jeher bestehenden tiefen Grundeinstellungs- und Meinungsverschiedenheiten niemals ernstlich beeinträchtigt sondern höchstens belebt worden war. Ich will das heute gerne zugeben. Damals konnte ich nicht anders handeln, genau wie Du mir schriebst, dass Du vor Deinem Gewissen nicht anders handeln konntest. Verstehe das bitte auf rein menschlicher Grundlage, sowie auch unter Berücksichtigung der damaligen sehr zugespitzten Situation in der ganzen Welt und der immer mehr um sich greifenden Vergiftung der Atmosphäre auch gerade auf unserem wissenschaftlichen Sektor, die zu einer Erhitzung der Gemüter auf beiden Seiten führte. Der Schmerz, den ich mir selber durch die Aufsagung einer Freundschaft antun musste, an der ich lange Jahre mit meinem ganzen Herzen gehangen hatte, war gewiss nicht geringer als der, den ich Dir bereitet habe.

Du müsstest mich für charakterlos halten, wenn Du dächtest, dass ich heute — veranlasst durch die bedauerlichen, verhängnisvollen Fehler unserer Führung, durch den Sieg der Waffen Deiner Seite und die daraus hier bei uns entsprungene Welle politischen Druckes — wesentliche Teile meiner Grundeinstellung aufgegeben oder geändert hätte. Diese wurzelt ja in den Erfahrungen und Erkenntnissen meines ganzen bisherigen Lebens, vor allem seit 1918, und lässt sich nicht einfach abstreifen wie eine Haut, bloss weil eine andere heute billiger und nutzbringender ist. Denke Dich bitte in die umgekehrte Situation, dann wirst Du das verstehen. Niemand wird sich mehr freuen als ich, wenn es heute auch für Dich einen Weg gibt, unsere früheren freundschaftlichen Beziehungen auf der Grundlage der gegenseitigen Achtung unserer beiden so völlig verschiedenen Einstellungen fortzusetzen und das zwischen uns Getretene ebenso zu vergessen wie die Leiden, die sich unsere Länder gegenseitig im Krieg zugefügt haben.

Ueber mein persönliches Schicksal bist Du wohl durch Todd und Hardy unterrichtet. Es bleibt noch immer abzuwarten, welchen Erfolg das Eintreten von Hardy und Hall für meine Rehabilitierung im Amt haben wird. Ich darf bei dieser Gelegenheit anmerken, dass Hardys Bitte an Dich, sich auch von Dir aus beim Control Office für mich einzusetzen, nicht mit meinen Wissen und Wollen erfolgt ist. Nachdem ich jetzt über ein Jahr ohne jedes Einkommen und unter rigoroser Beschränkung der Abhebungen von meinen

Bankersparnissen auf 300.- Rm monatlich gelebt habe, werden nun einige Anstrengungen unternommen, mir jedenfalls durch Uebertragung eines Forschungsauftrages seitens der Gesellschaft der Wissenschaften eine bescheidene Existenz zu ermöglichen. Ich kann dies jedoch nur als eine Interimslösung ansehen und werde Göttingen, wo ich nicht nur 1934/35 sondern auch jetzt wieder so viele bittere Erfahrungen machen musste, den Rücken kehren, sowie ich anderswo eine Möglichkeit der wissenschaftlichen Existenz finde.

Clärle musste sich 1942 einer schweren Nierenoperation unterziehen und hat die Folgen davon bis heute nicht überwunden. Ihr allgemeiner Gesundheits– und Kräftezustand macht mir wirklich sehr ernste Sorgen. Sie ist körperlich wie seelisch völlig am Ende ihrer Kraft, und es ist hohe Zeit, dass durch eine grundlegende Wandlung in unserer jetzigen Lebensweise einmal alle die schweren Sorgen und Lasten von ihr genommen werden und sie eine gründliche Erholung und Kräftigung bekommt. Besonders schwer leidet sie, wie wir alle darunter, dass uns unser schönes Heim in der Calsowstr. 57, das der Krieg intakt gelassen hatte, Anfang dieses Jahres unter widerwärtigen Umständen genommen und anschliessend zerschlagen wurde. Selbst wenn wir eines Tages die Möglichkeit bekommen sollten, wieder dort einzuziehen, würden wir von den Dingen, die Clärle mit so viel Mühe und Liebe in jahrelanger Aufbauarbeit zusammengetragen hatte, nicht mehr viel vorfinden. Wir leben jetzt in zwei kleinen Zimmern und haben endlich mit unsagbarer Mühe und Kraftaufwand wenigstens die zum Leben notwendigsten Sachen uns erneut beschafft. Was Clärle doch noch immer wieder die Kraft gibt, sich aufzuraffen und dem Leben die Stirn zu bieten, ist unser kleiner 1943 geborener Rüdiger, der so ganz unberührt und unbeschwert von den Nöten der Zeit heranwächst. Jutta ist jetzt 19 Jahre und steht vor dem Abschluss ihrer Schulbildung. Ihre kräftige, gesunde Natur hat sich bisher ganz gut durchgesetzt, trotz der vielen schweren Entbehrungen, die wir alle seit den Kriegsjahren und vor allem jetzt in noch immer steigendem Masse tragen müssen. Von meinem eigenen körperlichen und seelischen Zustand will ich schweigen. Bis vor einiger Zeit konnte ich mich immer noch ganz gut in die wissenschaftliche Arbeit flüchten. Allmählich versiegen aber durch den mangelnden Kontakt nicht nur mit der wissenschaftlichen Aussenwelt sondern auch mit dem Unterrichts– und Forschungsbetrieb am Institut die Quellen des Produktionsstroms, und vor allem hat auch meine Arbeitskraft in erschreckendem Masse nachgelassen. Trotz alledem halten wir den Kopf hoch, in der Hoffnung, dass auch für uns noch einmal wieder glücklichere

Zeiten kommen werden.

Wie es bei Clärle für ihren körperlichen Zustand vor allem der Fettmangel ist, unter dem sie empfindlich leidet, und wie ihr zur Belebung bei ihrem so häufigen gänzlichen Abklappen der anregende Tee ganz besonders fehlt, so ist es bei mir neben dem Fettmangel, den auch ich immer stärker zu fühlen habe, vor allem das Fehlen der zur Gewohnheit gewordenen Anregung durch Tabak und Kaffee, was zum Absinken meiner Arbeitskraft in hohem Masse beiträgt. Wir bekommen auf Zuteilung nur 200 gr Fett im ganzen Monat, und knapp eine (völlig gehaltlose) Zigarette pro Tag. Du kannst danach ermessen, wie willkommen uns Deine beiden Sendungen waren und wie dankbar wir Dir dafür sind.

Deine wissenschaftlichen Veröffentlichungen aus den ersten Kriegsjahren habe ich an Hand der Zentralblatt–Referate mit Interesse verfolgt; was Du in der letzten Zeit gemacht hast, ist noch nicht bis hierher gedrungen. Wir haben uns sehr gefreut zu erfahren, dass Du durch Deine Berufung auf eine Londoner Professur die lang verdiente Anerkennung für Deine wissenschaftlichen Leistungen gefunden hast. Sehr überrascht hat uns die Nachricht, dass Du inzwischen geheiratet hast; lass Dich nachträglich dazu beglückwünschen. Wir hoffen, Deine Mutter und Schwester haben den Krieg gut überstanden und befinden sich wohl.

Es würde uns freuen, bald von Dir zu hören. Mit den besten Grüssen auch von Clärle und Jutta,

Dein

Helmut

1.170 24.01.1947, Postcard Hasse to Davenport

Postcard Göttingen,
24. Januar 1947

Lieber Harold,

Ich hoffe, Du hast meinen ausführlichen Brief vom November vorigen Jahres erhalten, auf den ich bisher ohne Antwort blieb. Heute möchte ich Dir nur kurz mitteilen, dass ich vor einigen Tagen eine weitere aus der Schweiz kommende „ Liebesgabensendung" mit 200 Zigaretten erhielt, die keine Absenderangabe trug. Ich darf annehmen, dass diese Sendung wie die vorige in Deinem Auftrag erfolgte, und möchte Dir sehr herzlich dafür danken. In der Hoffnung, bald von Dir zu hören, bin ich mit herzlichen Grüssen, auch von Clärle,

Dein Helmut

1.171 13.09.1949, Davenport to Hasse

University College
London
13 September 1949.

Dear Helmut,

Thank you very much indeed for the copy of your Zahlentheorie. I think it will have a great effect in popularising the algebraic approach, which, although it has been carried so deep, is nowhere available in a form which is easy to study. I look forward very much to learning a great deal of which I am ignorant, and of which I am ashamed to be ignorant.

I hope you are well, and have good opportunities for research. My own time is taken up largely by looking after students and sitting on committees, and there are many interesting questions for research, for which I lack the time. But I am happy to know that we have many clever young men here, who will do good work in the future.

With sincere thanks, and all good wishes,

Yours,

Harold.

1.172 11.12.1949, Hasse to Davenport

BERLIN–ZEHLENDORF,
den 11. 12. 1949

Lieber Harold,

Über Deinen freundlichen Brief vom September habe ich mich sehr gefreut und danke Dir recht herzlich dafür, vor allem auch für Deine anerkennenden Worte über mein Zahlentheoriebuch. Ich lese augenblicklich die Korrekturen eines weiteren Buches "Vorlesungen über Zahlentheorie" mehr elementaren Charakters, das in der "Gelben Sammlung" erscheinen wird. Darin werden u. a. Dinge behandelt, die Deiner Geschmacksrichtung näher stehen. Ich hoffe, Dir Mitte nächsten Jahres ein Exemplar in die Hände geben zu können.

Mir scheint, dass Deine wissenschaftliche Produktion trotz der unterrichtlichen Verpflichtungen und Kommissionssitzungen recht erheblich ist. Ich habe mit grossem Interesse Deine neueren Arbeiten verfolgt, von denen ich ja einen Teil für das Zentralblatt zu referieren hatte. Besonders interessant fand ich Deine Entdeckung über die Folgen von besonderen Zahlentypen in quadratischen und kubischen Körpern, die bei den Minimumsproblemen über das Produkt inhomogener Linearformen auftreten. Es wäre schön, wenn es gelänge, diese zunächst als Einzelresultate dastehenden Erscheinungen einem allgemeinen systematischen Rahmen einzuordnen. Vielleicht finde ich unter den zahlreichen, hochbegabten jungen Leuten, die sich hier um mich gruppiert haben, mal einen, den diese Aufgabe lockt. Vorläufig sind diese zum grössten Teil mit Arbeiten befasst, deren gemeinsames letztes Ziel das Zerlegungsgesetz in allgemeinen galoisschen Zahlkörpern ist.

Ich habe seit langer Zeit den Wunsch nach einer persönlichen Aussprache mit Dir. Leider sind ja die Verhältnisse für uns Deutsche immer noch so, dass Auslandsreisen nur in besonderen Fällen und mit grossen Schwierigkeiten ausführbar sind. Aber vielleicht ist es doch möglich, dass ich in absehbarer Zeit einmal nach London kommen kann. Ob ich an dem Kongress im nächsten Sommer in USA teilnehmen kann, ist sehr fraglich.

Clärle und unser nun 6–jähriger Rüdiger leben noch immer in unserem Behelfsquartier in Göttingen, da unser Haus dort nach wie vor in den Händen der Besatzungsmacht ist. Bei den turbulenten und unerfreulichen Verhältnissen hier in Berlin konnten wir uns bisher nicht entschliessen, unseren Familienwohnsitz nach hier zu verlegen. So reise ich in jeden Ferien nach Göttingen, wobei jedesmal das Problem der Durchdringung des eisernen Vorhangs entsteht und meist nur unter aufregenden Nebenerscheinungen gelöst werden kann. Jutta arbeitet seit 2 Jahren hier in Berlin in der Redaktion des Zentralblatts unter H. L. Schmid. Wir wohnen nicht zusammen, aber doch nur 3 Minuten voneinander entfernt. Im selben Hause wie sie wohnt auch einer meiner besten Schüler, der Sohn von H. Kneser, der mit Jutta ungefähr gleichaltrig ist. Ich denke, er wird bald von sich hören machen.

In der Hoffnung, wieder einmal von Dir zu hören, mit herzlichen Grüssen

Dein
Helmut

1.173 02.12.1952, Hasse to Davenport

Hamburg 13,
den 2. 12. 1952

Lieber Harold,

Lass Dir sehr herzlich für die Zusendung Deines Zahlentheoriebuches danken. Wenn ich das erst heute tue, so deshalb, weil ich es erst ganz gründlich durchlesen wollte; denn ich weiss, wie ein Autor empfindet, wenn man ihm gleich nach Erhalt für das „hochinteressante Werk" dankt, auf „dessen Lektüre im einzelnen man sich sehr freut".

Ich bin von diesem Buch wirklich aufs Höchste entzückt. Du hast Dir eine riesige Mühe gegeben, die Sache klar, anziehend, lebendig und suggestiv darzustellen, in sehr wohltuendem Gegensatz gegen den bei der mathematischen Jugend — jedenfalls hier — immer mehr um sich greifenden Bourbaki–Stil. Darüber hinaus hast Du gerade bei den zentralen Dingen, wie Primzahlzerlegung, euklidischer Algorithmus u. a. originelle, neue Wege beschritten und dadurch das alte, etwas ausgefahrene Geleise der klassischen Zahlentheorie durch reizvolles Neuland, und auch zu solchem hin, geführt. Ich habe von der Lektüre dieses Buches, neben reinster Freude an der Schönheit und Geschlossenheit der Darstellung, auch sachlich grossen Gewinn für meine Lehraufgabe mitgenommen. Nochmals mein Kompliment, meine Bewunderung, und meinen aufrichtigen Dank !

Nun möchte ich noch ein paar persönliche Worte hinzufügen. Lieber Harold, ich denke, es ist wirklich an der Zeit, dass wir uns erneut die Hand reichen. Du kennst meine Bereitschaft dazu bereits seit einigen Jahren, und ich weiss auch durch Frau Taussky–Todd, dass Du Dir im Grunde dasselbe wünschest. Wenn bei Dir aus der Vergangenheit etwas zurückgeblieben ist, über das Du nicht hinwegkommen kannst, so schreibe mir bitte offen und unverblümt, was das ist, und ich will versuchen, ob ich Dir durch nähere Erklärungen helfen kann, ein solches Hindernis aus dem Weg zu räumen. Es ist gewiss meine Schuld, dass es zu der Entfremdung zwischen uns gekommen ist. Sage mir, was ich tun kann, um sie zu beenden.

Clärle geht es leider in der letzten Zeit gar nicht gut. Sie hat laufende, schmerzhafte Nierenbeschwerden; die Sache ist recht ernst und sorgenvoll, aber sie trägt sie mutig und ohne viel zu klagen.

Mit herzlichen Grüssen,

Dein Helmut

1.174 19.10.1961, Davenport to Hasse

Trinity College,
Cambridge.

19 October 1961.

Dear Helmut,

I am sorry that I missed you during your visit to the British Mathematical Colloquium at Liverpool. I had made other plans for early September before the date of the meeting was known.

I was very sorry to hear from Cassels a few days ago that Clärle is seriously ill. Please give her my very best wishes.

With kind regards,

Yours sincerely

Harold

1.175 24.10.1961, Hasse to Davenport

Hamburg, den 24. Oktober 1961

Lieber Harold,

Auch mir hat es leid getan, Dich nicht in Liverpool zu treffen. Nun, ich hoffe bestimmt, dass sich bald eine andere Gelegenheit findet, wo wir uns nach so langer Zeit wiedersehen, sei es in England, sei es in Deutschland.

Clärle ist nach schweren 6 Wochen erst □□□ jetzt so weit, dass sie wieder alles auffassen kann, was man ihr sagt, und dann auch ein wenig Interesse an der Aussenwelt zeigt. Ich habe ihr heute von Deinem Brief erzählt, und sie hat sich ganz ersichtlich sehr darüber gefreut. Ich danke Dir von Herzen, dass Du ihr diese Freude gemacht hast, wo doch ihre Tage bei der steigenden Erkenntnis ihres schwer geschädigten Zustandes so voll von trüben Gedanken sind.

Mit herzlichen Grüssen

Dein Helmut

1.176 26.04.1963, Postcard Hasse to Davenport

Postcard 26. 4. 63

Lieber Harold,

Auch ich habe es sehr bedauert, dass wir uns anlässlich Deines Besuches in Oberwolfach nicht getroffen haben. Wir danken Dir für Deine Grüsse aus uns wohlbekannter Umgebung. Leider haben wir zu spät daran gedacht, dass wir doch wenigstens mit Dir hätten telefonieren können. Es war eigentlich nicht Clärles Krankheit, weswegen ich nicht kommen konnte, sondern wir hatten Unruhe mit unserem Rüdiger (*1943), und ich wollte Clärle nicht alleine lassen. Vielleicht treffen wir uns im August in Boulder (Colorado). Herzlichst

Dein Helmut

Sei bedankt Harold für all deine guten Wünsche. Ja es war damals sehr dunkel um mich, aber nun sind wir so dankbar, dass ich's geschafft habe — herzlichst Clärle

1.177 24.08.1963, Davenport to Hasse

1860 Athens St,
Boulder.
24 August 1963.

Dear Helmut,

I enclose two books which have occasionally given me pleasure, in the hope that they may give you pleasure too.

With all good wishes for your birthday + for the future,

Yours

Harold.

1.178 24.10.1963, Davenport to Hasse

Trinity College,
Cambridge.
24 October 1963.

Dear Helmut,[1]

I am sorry to say that our picture-making recently has not been very successful, but I enclose two snapshots of the 4 of us. The black-and-white one was made in our garden here, and the coloured one was made in September in the garden of the Lewises in Ann Arbor.

We came home by boat from New York about the middle of September, and had quite a pleasant trip. Now I am busy mainly with research students and their manuscripts, and with the preparation of Hardy's Collected Papers. Bombieri, from Milan, is here for the year; he seems to be both highly intelligent and well informed.

I send my very best wishes for the health of both Clärle + yourself

Love from

Harold

[1] "*beantw. auf Weihnachtskarte 1963*"

1.179 14.07.1967, Davenport to Hasse, Circular

Trinity College,
Cambridge.

14 July 1967.

Dear Colleague,

Professor Mordell will be 80 on 28 January 1968, and the Council of the London Mathematical Society has agreed that one of the 4 parts of the *Journal* to appear in 1968 shall contain papers dedicated to him on his 80th birthday.

If you would like to contribute a paper dedicated to Mordell, please send the manuscript to the Editor, Mr. J.E. Reeve, as soon as possible. The closing date for the receipt of manuscripts is

25 October 1967;

this will enable the issue in question to be that for April 1968. It would be of assistance in planning the issue if you could inform either the Editor or me of your intention to contribute (or not to contribute) at an early date.

The Editor's address is:

until 30 September 1967: King's College, Strand, London W.C.2.

from 1 October 1967: University of East Anglia, Norwich, NOR77H.

Yours sincerely,

H. Davenport

"Postk. 17.7.67 – *Ev. kurzer Beitr. wenn Fertigstellung bis 25.10. gelingt*"

1.180 30.10.1967, Hasse to Davenport

30.10.67

Davenport, 8 Cranmer Road, Cambridge, England

Herzliches Gedenken

Helmut

Chapter 2

Verschiedene Manuskripte von Davenport

2.1 Der Körper der Abelschen Funktionen. (Die Jacobische Mannigfaltigkeit)

Es sei K ein algebraischer Funktionenkörper vom Geschlecht $g > 0$ über dem komplexen Zahlkörper k.

Für jeden Primdivisor $\mathfrak{o}$ von K gibt es eine *Normalerzeugung* von K, die wir durch

$$K \;=\; k(x) \qquad \text{mit} \quad M(x) \;=\; 0 \qquad \text{andeuten.}$$

Dabei soll sein:

1) $x \;=\; (x^{(0)}; x^{(1)}, \ldots, x^{(n-1)})$, $x^{(0)}$ ein Element aus K mit minimalem Nenner $\mathfrak{o}^n$ $(n > 0)$ (d. h. $n =$ kleinstes ν mit $\dim \mathfrak{o}^\nu > 1$; $x^{(0)} \cong \frac{\mathfrak{g}}{\mathfrak{o}^n}$, $\mathfrak{g}$ ganz, $\mathfrak{g} \sim \mathfrak{o}^n$, $\mathfrak{g} \neq \mathfrak{o}^n$)
$x^{(\nu)}$ ein Element aus K mit minimalem Nenner $\mathfrak{o}^{\nu + k_\nu n}$ ($k_\nu > 0$; $\nu = 1, \ldots, n-1$)
Dann ist

$$[K : k(x^{(0)})] \;=\; n$$

und $x^{(1)}, \ldots, x^{(n-1)}$, $x^{(n)} = 1$ eine Basis für $K \,|\, k(x^{(0)})$, sogar eine Minimalbasis für $K_{x^{(0)}} \,|\, k[x^{(0)}]$.

2) $M(x) = 0$ eine Zusammenfassung des Multiplikationsschemas

$$\text{(M)} \qquad x^{(i)} x^{(j)} \;=\; \textstyle\sum_{\nu=1}^{n} g_{ij\nu} x^{(\nu)} \quad \text{mit} \quad g_{ij\nu} \;=\; g_{ij\nu}(x^{(0)}).$$

Die Primdivisoren $\mathfrak{p} \neq \mathfrak{o}$ von K entsprechen dann umkehrbar eindeutig den konstanten Lösungen $a = (a^{(0)}; a^{(1)}, \ldots, a^{(n-1)})$ der Gleichungen $M(a) = 0$, den Kongruenzen

$$x \equiv a \text{ mod. } \mathfrak{p} \qquad \text{dh.} \quad x^{(\nu)} \equiv a^{(\nu)} \text{ mod. } \mathfrak{p} \quad (\nu = 0, \ldots, n-1)$$

entsprechend. Für $\mathfrak{p} = \mathfrak{o}$ ist dabei formal $a = \infty$ zu setzen.

Zu K gehört eine Riemannsche Fläche F vom topologischen Geschlecht g, dh. F besitzt $2g$ topologisch unabhängige geschlossene Wege. Die Punkte von F entsprechen umkehrbar eindeutig den Primdivisoren $\mathfrak{p}$ von K. Baut man die Fläche F über der $x^{(0)}$–Kugel auf, so besitzt sie n Blätter und dem Unendlichen dieser Kugel entspricht genau ein Punkt $\mathfrak{o}$ von F, und zwar ein n–facher Verzweigungspunkt. Die Beschreibung der Primdivisoren $\mathfrak{p}$ durch die "Koordinaten" a bedeutet die Beschreibung der Punkte von F durch die Werte der n Funktionen $x^{(0)}$; $x^{(1)}, \ldots, x^{(n-1)}$ in ihnen. Der Wert $a^{(0)}$ von $x^{(0)}$ legt den Punkt nur bis auf die Auswahl unter den n Blättern fest; diese wird durch die zusätzliche Angabe der Werte $a^{(1)}, \ldots, a^{(n-1)}$ von $x^{(1)}, \ldots, x^{(n-1)}$ getroffen; für diese bestehen jeweils höchstens n verschiedene Möglichkeiten, weil $M(x) = 0$ das Multiplikationsschema eines Körpers n–ten Grades über $K(x^{(0)})$ ist, also höchstens n verschiedene konjugierte Realisierungen besitzt.

Die Elemente y von K lassen sich als Punktfunktionen $y(\mathfrak{p})$ auf F auffassen, Funktionswerte sind dabei die konstanten Reste b mit

$$y(\mathfrak{p}) \equiv b \text{ mod. } \mathfrak{p}\,,$$

unter Einschluß von gegebenenfalls $b = \infty$. Sie sind genau die Gesamtheit der auf F überall analytischen (dh. bis auf Pole regulären) Funktionen, die im Großen eindeutig sind (dh. bei analytischer Fortsetzung auf den $2g$ geschlossenen unabhängigen Wegen in sich zurückkehren). Eine allgemeinere zu F gehörige Funktionsklasse sind die auf F überall analytischen, aber im Großen nicht notwendig eindeutigen Funktionen, deren Werte über derselben Stelle von F sich nur um Konstante unterscheiden. Jede solche Funktion v hat $2g$ Periodizitätsmoduln $\alpha_1, \ldots, \alpha_{2g}$, entsprechend den $2g$ voneinander unabhängigen geschlossenen Wegen auf der Fläche F (dh. bei Fortsetzung auf dem ν–ten dieser Wege oder einem dazu topologisch äquivalenten kommt man mit einem um die Konstante α_ν von dem Ausgangswert verschiedenen Wert zurück. Die Funktion v liegt somit bis auf ein beliebiges lineares Kompositum $m_1\alpha_1 + \ldots + m_{2g}\alpha_{2g}$ mit ganzzahligen $m_1, \ldots, m_{2g}$ fest. Ihre Ableitung $\frac{\mathrm{d}v}{\mathrm{d}x^{(0)}} = y$ ist somit im Großen eindeutig, also eine Funktion des Körpers K, es ist also

$$v(\mathfrak{p}) - v(\mathfrak{o}) \equiv \int_{\mathfrak{o}}^{\mathfrak{p}} y\,\mathrm{d}x^{(0)} \text{ mod. } (\alpha_1, \ldots, \alpha_{2g})\,;$$

dabei ist die Unbestimmtheit in der Wahl des Integrationsweges gleichbedeutend mit der Unbestimmtheit um Perioden $m_1\alpha_1 + \ldots + m_{2g}\alpha_{2g}$. Jede solche Integralfunktion liefert umgekehrt eine auf F überall analytische Funktion.

Besonders wichtig sind diejenigen der Funktionen v, die auf F überall *regulär* sind (Integralfunktionen erster Gattung). Nach dem Riemann–Rochschen Satz gibt es genau g linear unabhängige solche, $u_1, \ldots, u_g$, so daß jede

Integralfunktion erster Gattung u von der Form ist:

$$u \ = \ c_0 + c_1 u_1 + \ldots + c_g u_g$$

mit beliebigen komplexen Konstanten $c_0, \ldots, c_g$. Jede der Funktionen u_i besitzt $2g$ Periodizitätsmoduln

$$\omega_{i1}, \ldots, \omega_{i,2g}\,, \qquad (i = 1, \ldots, g)\,.$$

Dabei ist ω_{ij} das Integral des u_i entsprechenden ganzen Differentials $y_i \, \mathrm{d}x^{(0)}$ (Differential erster Gattung) über den j–ten der $2g$ unabhängigen Wege auf F. Wir fassen die u_i zu einer Spalte $\mathfrak{u}$ zusammen, die Differentiale $y_i \, \mathrm{d}x^{(0)}$ zu der Spalte $\mathrm{d}\mathfrak{u}$ und dementsprechend die zugehörigen Periodizitätsmodulzeilen zu einer $(g,\, 2g)$–Matrix Ω. Demgemäß schreiben wir

$$\mathfrak{u}(\mathfrak{p}) \equiv \int_{\mathfrak{o}}^{\mathfrak{p}} \mathrm{d}\mathfrak{u} \ \mathrm{mod.}\ \Omega\,,$$

legen also die Integrationskonstanten so fest, daß $\mathfrak{u}(\mathfrak{o}) \equiv 0$ mod. Ω ist. Ausführlich geschrieben heißt diese Relation

$$u_i(\mathfrak{p}) \ = \ \int_{\mathfrak{o}}^{\mathfrak{p}} y_i \, \mathrm{d}x^{(0)} + m_1 \omega_{i1} + \ldots + m_{2g} \omega_{i,2g} \quad (i = 1, \ldots, g\,)\,.$$

Dabei denken wir uns für alle g Gleichungen denselben Integrationsweg genommen, so daß eine Änderung um einen geschlossenen Weg jeweils in allen g Gleichungen dieselben Vielfachheitskoeffizienten $m_1, \ldots, m_{2g}$ nach sich zieht.

Bisher sind die Funktionen (der Funktionsvektor) $\mathfrak{u}(\mathfrak{p})$ nur für Primdivisoren $\mathfrak{p}$ erklärt. Wir erweitern die Definition formal auf ganze Divisoren durch die Festsetzung: $\mathfrak{u}(\mathfrak{p}_1 \ldots \mathfrak{p}_r) \equiv \mathfrak{u}(\mathfrak{p}_1) + \ldots + \mathfrak{u}(\mathfrak{p}_r)$ mod. Ω,

$$\text{d. h.} \qquad \mathfrak{u}(\mathfrak{p}_1 \ldots \mathfrak{p}_r) \equiv \sum_{j=1}^{r} \int_{\mathfrak{o}}^{\mathfrak{p}_j} \mathrm{d}\mathfrak{u} \equiv \int_{\mathfrak{o}^r}^{\mathfrak{p}_1 \ldots \mathfrak{p}_r} \mathrm{d}\mathfrak{u} \ \mathrm{mod.}\ \Omega\,.$$

Man fasse das als Integral des Vektors $\mathrm{d}\mathfrak{u}$ über eine r–gliedrige Punktgruppe auf, die auf einem Wegsystem von $\mathfrak{o}^r$ nach $\mathfrak{p}_1 \ldots \mathfrak{p}_r$ läuft. Damit ist $\mathfrak{u}(\mathfrak{g})$ für beliebige ganze Divisoren $\mathfrak{g}$ erklärt. Schließlich setze man

$$\mathfrak{u}(\mathfrak{a}) \equiv \mathfrak{u}(\mathfrak{g}_1) - \mathfrak{u}(\mathfrak{g}_2) \ \mathrm{mod.}\ \Omega \quad \text{für} \quad \mathfrak{a} = \frac{\mathfrak{g}_1}{\mathfrak{g}_2}\,.$$

Dann ist $\mathfrak{u}(\mathfrak{a})$ für beliebige Divisoren $\mathfrak{a}$ erklärt, und es gilt

$$\mathfrak{u}(\mathfrak{a}_1\mathfrak{a}_2) \equiv \mathfrak{u}(\mathfrak{a}_1) + \mathfrak{u}(\mathfrak{a}_2) \text{ mod. } \Omega\,.$$

Damit haben wir eine homomorphe Abbildung der Divisorengruppe von K auf Punkte des Periodenparallelotops mod. Ω. ((Veranschaulichung: Jeder g–gliedrigen Spalte komplexer Zahlen entspricht ein Punkt eines $2g$–dimensionalen Raumes, den $2g$ Spalten von Ω entsprechen $2g$ Vektoren dieses Raumes, die, ausgehend von demselben Punkt, ein solches Parallelotop aufspannen.))

Wir untersuchen die Struktur dieser Homomorphie genauer.

Die Divisorengruppe von K besitzt die direkte Zerlegung

$$\mathfrak{a} \;=\; \mathfrak{a}_0\mathfrak{o}^{f(\mathfrak{a})} \quad \text{mit} \left\{ \begin{array}{l} \mathfrak{a}_0 \text{vom Grad } 0\,, \\ f(\mathfrak{a}) \;=\; \text{Grad von } \mathfrak{a}\,. \end{array} \right.$$

Weil $\mathfrak{u}(\mathfrak{o}) \equiv 0$ mod. Ω ist, folgt daraus

$$\mathfrak{u}(\mathfrak{a}) \equiv \mathfrak{u}(\mathfrak{a}_0) \text{ mod. } \Omega\,.$$

Es genügt also, die Gruppe der Divisoren nullten Grades zu betrachten. Man schreibt dementsprechend nunmehr

$$\mathfrak{u}\left(\frac{\mathfrak{p}_1\ldots\mathfrak{p}_r}{\mathfrak{o}^r}\right) \equiv \int_{\mathfrak{o}^r}^{\mathfrak{p}_1\ldots\mathfrak{p}_r} d\mathfrak{u} \text{ mod. } \Omega$$

und allgemeiner

$$\mathfrak{u}\left(\frac{\mathfrak{p}_1\ldots\mathfrak{p}_r}{\mathfrak{q}_1\ldots\mathfrak{q}_r}\right) \equiv \int_{\mathfrak{q}_1\ldots\mathfrak{q}_r}^{\mathfrak{p}_1\ldots\mathfrak{p}_r} d\mathfrak{u} \text{ mod. } \Omega\,.$$

Dabei ist die Zuordnung der Punktgruppen $\mathfrak{p}_1\ldots\mathfrak{p}_r$ und $\mathfrak{q}_1\ldots\mathfrak{q}_r$ durch verbindende Wege gleichgültig, da sich verschiedene solche Wegsysteme nur um geschlossene Wege unterscheiden.

Die genaue Struktur der homomorphen Abbildung gibt jetzt das

Abelsche Theorem: *Die Abbildung der Gruppe der Divisoren nullten Grades von K in Punkte eines Periodenparallelotops ist ein Isomorphismus für die Gruppe der Divisorenklassen nullten Grades. Dh. äquivalente Divisoren nullten Grades werden in denselben Punkt abgebildet und umgekehrt.*

zusammen mit dem

Jacobischen Theorem: *Die Gruppe der Divisorenklassen nullten Grades wird auf das volle Periodenparallelotop abgebildet, dh. es gibt zu jeder Spalte von g komplexen Zahlen $\mathfrak{v}$ mod. Ω einen Divisor nullten Grades $\mathfrak{a}_0$ mit*

$$\mathfrak{u}(\mathfrak{a}_0) \equiv \mathfrak{v} \text{ mod. } \Omega\,.$$

Die Klasse von $\mathfrak{a}_0$ ist dabei nach dem Abelschen Theorem eindeutig bestimmt.

Durch die bisherigen Feststellungen ist die Struktur der Divisorenklassengruppe von K aufgedeckt. Im folgenden wird die Struktur der einzelnen Divisorenklassen näher untersucht.

Die Divisorenklassen nullten Grades C lassen sich in der Form darstellen

(C) $\quad \frac{\mathfrak{g}}{\mathfrak{o}^g} = \frac{\mathfrak{p}_1 \ldots \mathfrak{p}_g}{\mathfrak{o}^g}\,.$

Beweis: Ist C eine Klasse nullten Grades, so ist $C(\mathfrak{o}^g)$ eine Klasse g–ten Grades. Nach dem Riemann–Rochschen Satz ist

$$\begin{aligned}\dim C(\mathfrak{o}^g) &= g-(g-1)+\dim\frac{W}{C(\mathfrak{o}^g)}\\ &= 1+\dim\frac{W}{C(\mathfrak{o}^g)} \geq 1\,.\end{aligned}$$

Es gibt also in $C(\mathfrak{o}^g)$ einen ganzen Divisor g–ten Grades $\mathfrak{g} = \mathfrak{p}_1 \ldots \mathfrak{p}_g$. Ist

$$\dim\frac{W}{C(\mathfrak{o}^g)} = 0\,,$$

so ist $\mathfrak{g}$ sogar eindeutig bestimmt; dann heißt C eine *bezüglich* $\mathfrak{o}$ *reguläre Klasse* und $C(\mathfrak{o}^g)$ eine *reguläre Klasse* schlechthin. Bedingung dafür ist ersichtlich, daß in $\frac{W}{C}$ kein ganzes Multiplum von $\mathfrak{o}^g$ vorkommt, oder auch, daß in W selbst kein ganzes Multiplum von $\mathfrak{p}_1 \ldots \mathfrak{p}_g$, dh. kein ganzes durch

$\mathfrak{p}_1 \dots \mathfrak{p}_g$ teilbares Differential vorkommt. Nun ist

$$\begin{array}{rcll}
\dim W & = & g & \\
\dim \dfrac{W}{(\mathfrak{p}_1)} & = & g-1 & \text{für alle } \mathfrak{p}_1 \\
\dim \dfrac{W}{(\mathfrak{p}_1\mathfrak{p}_2)} & = & g-2 & \text{zu gegebenem } \mathfrak{p}_1 \text{ für alle } \mathfrak{p}_2 \text{ mit Ausnahme von endlich vielen, dh. für fast alle } \mathfrak{p}_2 \\
\dim \dfrac{W}{(\mathfrak{p}_1\mathfrak{p}_2\mathfrak{p}_3)} & = & g-3 & \text{für fast alle } \mathfrak{p}_3 \text{ zu gegebenen } \mathfrak{p}_1\mathfrak{p}_2 \\
\cdots\cdots & \cdots & \cdots\cdots\cdots & \\
\dim \dfrac{W}{(\mathfrak{p}_1 \dots \mathfrak{p}_g)} & = & 0 & \text{für fast alle } \mathfrak{p}_g \text{ zu gegebenen } \mathfrak{p}_1 \dots \mathfrak{p}_{g-1}\,.
\end{array}$$

In diesem Sinne ist “im allgemeinen” eine Divisorenklasse nullten Grades C bezüglich $\mathfrak{o}$ regulär.

Um das noch genauer zu verfolgen, denken wir uns die Punkte $\mathfrak{p}_1 \dots \mathfrak{p}_g$ auf F unabhängig voneinander variabel. Dann sind diese g–gliedrigen Punktgruppen auf F entweder *regulär* oder *irregulär*. Will man eine irreguläre Punktgruppe erhalten, so kann man nur $\mathfrak{p}_1 \dots \mathfrak{p}_{g-1}$ willkürlich vorschreiben, denn $\mathfrak{p}_g$ ist dann bereits endlich vieldeutig bestimmt. In der Mannigfaltigkeit aller g–gliedrigen Punktgruppen, die g Freiheitsgrade besitzt (Freiheitsgrad = komplexe Dimension = zweimal reelle Dimension), bilden also die irregulären eine Untermannigfaltigkeit von nur $g-1$ Freiheitsgraden.

Die *regulären* Punktgruppen $\mathfrak{p}_1 \dots \mathfrak{p}_g$ sind den zugehörigen Klassen g–ten Grades — und damit auch den zugehörigen Klassen nullten Grades $(\frac{\mathfrak{p}_1 \dots \mathfrak{p}_g}{\mathfrak{o}^g})$ — umkehrbar eindeutig zugeordnet. Hier stellt also die Formel

$$\mathfrak{u}(\frac{\mathfrak{p}_1 \dots \mathfrak{p}_g}{\mathfrak{o}^g}) \equiv \int_{\mathfrak{o}^g}^{\mathfrak{p}_1 \dots \mathfrak{p}_g} d\mathfrak{u} \equiv \mathfrak{v} \text{ mod. } \Omega$$

eine eindeutige Beziehung zwischen den Vektoren $\mathfrak{v}$ mod. Ω und den (regulären) Punktgruppen $\mathfrak{p}_1 \dots \mathfrak{p}_g$ auf F dar und es kann somit $\mathfrak{p}_1 \dots \mathfrak{p}_g$ als eine eindeutige “Funktion” im Periodenparallelotop aufgefaßt werden, dh. als eine “$2g$–periodische” “Funktion” der Spalte $\mathfrak{v}$ (aus g komplexen Zahlen), deren $2g$ “Perioden” $2g$ Spalten aus je g komplexen Zahlen sind, und deren “Funktionswerte” durch g–gliedrige Punktgruppen auf der Fläche F dargestellt werden.

Diese Funktion ist nur im Bereich derjenigen $\mathfrak{v}$ mod. Ω eindeutig definiert, die den regulären Punktgruppen entsprechen. Der Zusammenhang der irregulären Punktgruppen mit ihren Klassen ist nicht mehr umkehrbar eindeutig. Für eine bezüglich $\mathfrak{o}$ irreguläre Klasse nullten Grades C ist

$$\dim C(\mathfrak{o}^g) \geq 2 .$$

Ist nun $\mathfrak{p}_1 \dots \mathfrak{p}_g$ eine der zugehörigen irregulären Punktgruppen, so werden alle solchen $\mathfrak{p}'_1 \dots \mathfrak{p}'_g$ in der Form

$$\frac{\mathfrak{p}'_1 \dots \mathfrak{p}'_g}{\mathfrak{p}_1 \dots \mathfrak{p}_g} \cong c_0 + c_1 z_1 + \dots + c_r z_r$$

dargestellt, wo 1, $z_1, \dots, z_r$ eine Basis der ganzen Multipla von $\frac{1}{\mathfrak{p}_1 \dots \mathfrak{p}_g}$ und somit $r + 1 = \dim(\mathfrak{p}_1 \dots \mathfrak{p}_g) = \dim C(\mathfrak{o}^g) \geq 2$ gilt; ferner sind dabei $c_0, \dots, c_r$ unabhängige Parameter, deren Verhältnisse $c_0 : c_1 : \dots : c_r$ die Punktgruppen $\mathfrak{p}'_1 \dots \mathfrak{p}'_g$ umkehrbar eindeutig in bezug auf eine feste derselben $\mathfrak{p}_1 \dots \mathfrak{p}_g$ beschreiben. Mit diesem Parameterverhältnis besitzen also die C entsprechenden irregulären Punktgruppen mindestens einen, genau $r \geq 1$ Freiheitsgrade. Andererseits besitzen die irregulären *Punktgruppen* selbst $g - 1$ Freiheitsgrade, von denen mindestens einer bei der Abbildung auf die irregulären *Klassen* verloren geht; diese haben also höchstens $g - 2$ Freiheitsgrade. Aus obigem Prozeß erkennt man genauer, daß “im allgemeinen”, dh. bei willkürlicher Wahl von $\mathfrak{p}_1 \dots \mathfrak{p}_{g-1}$ sogar $\dim \frac{W}{\mathfrak{p}_1 \dots \mathfrak{p}_g} = 2$, also $\dim C(\mathfrak{o}^g) = 2$ gilt, wenn $\mathfrak{p}_g$ zu $\mathfrak{p}_1 \dots \mathfrak{p}_{g-1}$ so gewählt wird, daß eine irreguläre Punktgruppe vorliegt. Die irregulären *Klassen* haben demnach genau $g - 2$ Freiheitsgrade.

Nun ist die Mannigfaltigkeit der irregulären Punktgruppen stetig, denn sie ist durch das Verschwinden gewisser Determinanten (siehe Anmerkung Seite 403) gekennzeichnet. Daher liefert sie als Bild eine stetige Mannigfaltigkeit im Periodenparallelotop. Diese ist nach dem Gezeigten komplex $(g - 2)$–dimensional. Es macht daher für unsere Umkehrfunktion

$$\mathfrak{p}_1 \dots \mathfrak{p}_g \qquad \text{von} \quad \mathfrak{v} \text{ mod. } \Omega$$

nichts aus, daß die eindeutige Definition in dieser Teilmannigfaltigkeit versagt.

Wir betrachten die einfachsten Fälle:

g=1. Hier gibt es überhaupt keine irregulären Punkte. Die Beziehung der Riemannschen Fläche F zum Periodenparallelogramm ist durchweg umkehrbar eindeutig.

g=2. Hier kann $y^2 = f(x)$ ($f(x)$ quadratfreies Polynom vom Grade 5) als Erzeugung genommen werden ($n = 2$). Man weiß, daß die irregulären Punktgruppen $\mathfrak{p}\,\overline{\mathfrak{p}}$ genau die sind, für die $\mathfrak{p}$ und $\overline{\mathfrak{p}}$ auf der zweiblättrigen x–Fläche übereinanderliegen.

$$\begin{aligned} (x,\, y) &\equiv (a,\, b) \ \text{ mod. } \mathfrak{p} \\ (x,\, y) &\equiv (a,\, -b) \ \text{ mod. } \overline{\mathfrak{p}}\,. \end{aligned}$$

Alle diese entsprechen ein und demselben Punkt (0, 0) mod. Ω des Periodenparallelotops.

Sei im folgenden P das g–dimensionale Periodenparallelotop mod. Ω und I die Teilmannigfaltigkeit von $g-2$ Dimensionen, der irreguläre Punktgruppen entsprechen.

Wir haben bisher die Zuordnungen:

$$\mathfrak{p}_1 \ldots \mathfrak{p}_g \ \longrightarrow \ \Big(\frac{\mathfrak{p}_1 \ldots \mathfrak{p}_g}{\mathfrak{o}^g}\Big) \ = \ C \ \longleftrightarrow \ \mathfrak{v} \text{ mod. } \Omega\,,$$

wobei die erstere Zuordnung für nicht in I gelegene $\mathfrak{v}$ umkehrbar eindeutig ist. Wir nehmen jetzt noch die Zuordnung

$$x_1, \ldots, x_g \ \longleftrightarrow \ \mathfrak{p}_1 \ldots \mathfrak{p}_g$$

gemäß dem Kongruenzsystem für die Normalbasis von K

$$x \equiv x_i \ \text{ mod. } \mathfrak{p}_i \quad \text{mit} \quad M(x_i) = 0 \ \text{ (siehe S. 393) hinzu.}$$

Dabei ist

$$x_i \ = \ (x_i^{(0)};\, x_i^{(1)}, \ldots, x_i^{(n-1)})\,.$$

Für die Punktgruppe $\mathfrak{p}_1 \ldots \mathfrak{p}_g$ sind auch die symmetrischen Funktionen der Systeme $x_1, \ldots, x_g$ charakteristisch, denn sie bestimmen die $x_1, \ldots, x_g$ selbst eindeutig bis auf die Reihenfolge. Diese symmetrischen Funktionen bilden einen rein algebraisch definierten Körper L, der eine endlich–algebraische Erweiterung des Körpers der symmetrischen Funktionen von $x_1^{(0)}, \ldots, x_g^{(0)}$ ist, also einen algebraischen Funktionenkörper von g Variablen darstellt.

Damit ergibt sich für jede solche symmetrische Funktion $S(x_1, \ldots, x_g)$, dh. jede Funktion von L, eine Darstellung

$$S(x_1, \ldots, x_g) \ = \ \varphi(\mathfrak{v})$$

als eindeutige $2g$–fach periodische Funktion im Parallelotop zu Ω, und zwar für alle die Punkte $\mathfrak{v}$, die nicht zu I gehören. Diese Darstellung wird vermittelt durch die Relationen:

$$x \equiv x_i \text{ mod. } \mathfrak{p}_i\,, \quad \mathfrak{u}\left(\frac{\mathfrak{p}_1 \dots \mathfrak{p}_g}{\mathfrak{o}^g}\right) \equiv \int_{\mathfrak{o}^g}^{\mathfrak{p}_1 \dots \mathfrak{p}_g} d\mathfrak{u} \equiv \mathfrak{v} \text{ mod. } \Omega\,.$$

Der Einfachheit halber wollen wir in I auch noch diejenigen Punkte mit aufnehmen, für die ein Punkt $\mathfrak{p}_i = \mathfrak{o}$, also ein $x_i = \infty$ wird. Dadurch wird I $(g-1)$–dimensional.

Die so definierten Funktionen $\varphi(\mathfrak{v})$ sind, wie sich zeigt, in allen nicht zu I gehörigen Punkten regulär analytisch, dh. in der Umgebung einer Stelle $\mathfrak{v}_0 = (v_{10}, \dots, v_{g0})$ durch Potenzreihen in $\mathfrak{v} - \mathfrak{v}_0 = (v_1 - v_{10}, \dots, v_g - v_{g0})$ gegeben. Die irregulären Stellen beherrscht man dann auf folgende Art:

Es durchlaufe $S_\nu(x_1, \dots, x_g)$ $(\nu = 1, \dots, N)$ ein hinreichend großes Elementsystem von L derart, daß

$$L \;=\; k(S_1, \dots, S_N)$$

ist, also ein zu dem System der elementarsymmetrischen Funktionen äquivalentes System von symmetrischen Funktionen. Wir betrachten die Divisorenäquivalenz

$$\frac{\mathfrak{g}}{\mathfrak{o}^g}\,\frac{\mathfrak{a}}{\mathfrak{o}^g} \sim \frac{\mathfrak{g}'}{\mathfrak{o}^g}\,,$$

wo $\mathfrak{g}$ ein variabler, $\mathfrak{a}$ ein fester ganzer Divisor g–ten Grades ist.

$$\begin{aligned} \mathfrak{g} \;&=\; \mathfrak{p}_1 \dots \mathfrak{p}_g \quad \text{mit} \quad x \equiv x_i \text{ mod. } \mathfrak{p}_i\,, \\ \mathfrak{g}' \;&=\; \mathfrak{p}'_1 \dots \mathfrak{p}'_g \quad \text{mit} \quad x \equiv x'_i \text{ mod. } \mathfrak{p}'_i\,. \end{aligned}$$

Wir denken uns die x_i als Unbestimmte mit den algebraischen Relationen $M(x_i) = 0$ für $i = 1, \dots, g$ zum Körper k der komplexen Zahlen adjungiert; wir erhalten dann den Körper L. Nun betrachten wir die bei dieser Konstantenkörper–Erweiterung der Erweiterung $K \mid k$ entsprechende Erweiterung $KL \mid L$, also

$$KL \;=\; L(x) \qquad \text{mit} \quad M(x) \;=\; 0\,.$$

Man kann dann zeigen, daß

$$\dim(\mathfrak{g}) \;=\; 1\,, \quad \dim(\mathfrak{g}') \;=\; 1$$

ist, sodaß $\mathfrak{g}$, $\mathfrak{g}'$ sich gegenseitig eindeutig bestimmen. Und dabei gilt dann:

$$L' \;=\; k(S'_1, \ldots, S'_N) \;=\; k(S_1, \ldots, S_N) \;=\; L\,,$$

es liegt also ein *Automorphismus* von L vor. Dieser stellt sich in der $\mathfrak{v}$–Uniformisierung als Translation dar. Setzen wir nämlich

$$\int_{\mathfrak{o}^g}^{\mathfrak{g}} d\mathfrak{u} \equiv \mathfrak{v}\,, \qquad \int_{\mathfrak{o}^g}^{\mathfrak{g}'} d\mathfrak{u} \equiv \mathfrak{v}'\,, \qquad \int_{\mathfrak{o}^g}^{\mathfrak{a}} d\mathfrak{u} \equiv \mathfrak{f} \text{ mod. } \Omega\,,$$

so folgt entsprechend

$$\begin{aligned} \int_{\mathfrak{o}^g}^{\mathfrak{g}} d\mathfrak{u} + \int_{\mathfrak{o}^g}^{\mathfrak{a}} d\mathfrak{u} \;&\equiv\; \int_{\mathfrak{o}^g\mathfrak{o}^g}^{\mathfrak{g}\mathfrak{a}} d\mathfrak{u} \equiv \int_{\mathfrak{o}^g}^{\mathfrak{g}'} d\mathfrak{u} \text{ mod. } \Omega \\ \mathfrak{v} + \mathfrak{f} \;&\equiv\; \mathfrak{v}' \text{ mod. } \Omega\,. \end{aligned}$$

Umgekehrt entspricht auch jeder Translation $\mathfrak{f}$ ein solcher Automorphismus von L.

Man hat also

$$S_\nu(x_1, \ldots, x_g) \;=\; R_\nu^{(\mathfrak{a})}(S_\mu(x'_1, \ldots, x'_g))\,,$$

wo $R_\nu^{(\mathfrak{a})}$ ein von $\mathfrak{a}$ abhängiges System symmetrischer rationaler Funktionen ist (und umgekehrt).

Ist nun $\mathfrak{p}_1 \ldots \mathfrak{p}_g$ irregulär, so bestimme man $\mathfrak{a}$ so, daß $\mathfrak{p}'_1 \ldots \mathfrak{p}'_g$ regulär ist; das ist stets möglich. Dann erhält man Darstellungen

$$\begin{aligned} \varphi_\nu(\mathfrak{v}) \;=\; S_\nu(x_1, \ldots, x_g) \;&=\; R_\nu^{(\mathfrak{a})}(S_\mu(x'_1, \ldots, x'_g)) \\ &=\; R_\nu^{(\mathfrak{a})}(\varphi_\mu(\mathfrak{v}')) \\ &=\; R_\nu^{(\mathfrak{a})}(\varphi_\mu(\mathfrak{v} + \mathfrak{f}))\,. \end{aligned}$$

Dabei sind jetzt die $\varphi_\mu(\mathfrak{v} + \mathfrak{f})$ in der Umgebung der betreffenden Stelle eindeutig. Die Mehrdeutigkeit kommt dann nur durch die Bildung der rationalen Funktionen $R_\nu^{(\mathfrak{a})}$ zustande, ist also harmlos.

Nimmt man hier auch $\mathfrak{a}$, $\mathfrak{f}$ variabel, so erhält man das *Additionstheorem* der Abelschen Funktionen in der Gestalt:

Aus $\mathfrak{v} \equiv \mathfrak{v}_1 + \mathfrak{v}_2$ mod. Ω folgt $\varphi_\nu(\mathfrak{v}) = R(\varphi_\nu(\mathfrak{v}_1), \varphi_\nu(\mathfrak{v}_2))$. Man kann schließlich auch umgekehrt zeigen: Jede zu Ω gehörige $2g$–fach periodische Funktion, die überall durch Quotienten von Potenzreihen in $\mathfrak{v} - \mathfrak{v}_0$ gegeben wird, also (in diesem Sinne) analytisch ist, ist ein Element des Körpers L in seiner obigen Uniformisierung, dh. eine rationale Funktion von $S_1, \ldots, S_N$.

Der Körper L erweist sich also als die Gesamtheit der zum Periodenparallelotop zu Ω gehörigen analytischen Funktionen.

Man nennt L als *algebraisches Gebilde* die *Jacobische Mannigfaltigkeit* und in seiner *transzendenten Uniformisierung* den *Körper der Abelschen Funktionen.*

Zu Seite 399: Determinantenbedingung für die Irregularität einer Punktgruppe $\mathfrak{p}_1 \ldots \mathfrak{p}_g$.

Wir betrachten eine feste *reguläre* Punktgruppe $\mathfrak{q}_1 \ldots \mathfrak{q}_g$, dh. $\dim \frac{W}{\mathfrak{q}_1 \cdots \mathfrak{q}_g} = 0$; dann ist erst recht $\dim \frac{W}{\mathfrak{q}_1 \cdots \mathfrak{q}_g \mathfrak{p}_i} = \dim \frac{W}{\mathfrak{q}_1 \cdots \mathfrak{q}_g \mathfrak{p}_1 \cdots \mathfrak{p}_g} = 0$ für $i = 1, \ldots, g$. Nach dem Riemann–Rochschen Satz folgt:

$$\dim \mathfrak{q}_1 \ldots \mathfrak{q}_g \mathfrak{p}_i = 2 \quad (i = 1, \ldots, g), \qquad \dim \mathfrak{q}_1 \ldots \mathfrak{q}_g \mathfrak{p}_1 \ldots \mathfrak{p}_g = g + 1.$$

Es existieren somit nichtkonstante Elemente $w_1, \ldots, w_g$ in K mit

$$w_i \cong \frac{\mathfrak{g}_i}{\mathfrak{q}_1 \ldots \mathfrak{q}_g \mathfrak{p}_i}, \quad \mathfrak{g}_i \text{ ganz}, \quad (i = 1, \ldots, g).$$ Da $\dim \mathfrak{q}_1 \ldots \mathfrak{q}_g = 1$ ist, tritt $\mathfrak{p}_i$ wirklich im Nenner von w_i auf; die Betrachtung eines linearen Kompositums über k an den Stellen $\mathfrak{p}_1 \ldots \mathfrak{p}_g$ zeigt dann, daß

$$1, w_1, \ldots, w_g \quad \text{über } k \text{ linear unabhängige Elemente}$$

von K sind.

Wegen $\dim \mathfrak{q}_1 \ldots \mathfrak{q}_g \mathfrak{p}_1 \ldots \mathfrak{p}_g = g + 1$ bilden sie also eine Basis der ganzen Multipla von $\frac{1}{\mathfrak{q}_1 \cdots \mathfrak{q}_g \mathfrak{p}_1 \cdots \mathfrak{p}_g}$, insbesondere ist also jedes Element $y \cong \frac{\mathfrak{g}}{\mathfrak{p}_1 \cdots \mathfrak{p}_g}$ mit ganzem $\mathfrak{g}$ als lineares Kompositum von $1, w_1, \ldots, w_g$ darstellbar.

Nun seien $\pi_1, \ldots, \pi_g$ Primelemente zu $\mathfrak{q}_1 \ldots \mathfrak{q}_g$ und

$$w_i = \frac{c_{ij}}{\pi_j} + d_{ij} + \ldots, \quad c_{ij}, d_{ij} \text{ aus } k,$$

die entsprechenden Entwicklungen von w_i bei $\mathfrak{q}_j$ $(i, j = 1, \ldots, g)$. Nun gibt es ein nichtkonstantes Element

$$y = c_0 + c_1 w_1 + \ldots + c_g w_g \cong \frac{\mathfrak{g}}{\mathfrak{p}_1 \ldots \mathfrak{p}_g}, \quad \mathfrak{g} \text{ ganz},$$

dann und nur dann, wenn

$$\sum_{\nu=1}^{g} c_\nu c_{\nu j} \;=\; 0\,, \quad j = 1, \ldots, g\,,$$

also wenn die Determinante $|c_{ij}| = 0$ ist. Dann und nur dann ist also $\mathfrak{p}_1 \ldots \mathfrak{p}_g$ irregulär.

2.2 The congruence $y^3 \equiv ax^3+3bx^2+3cx+d \pmod{p}$

We can suppose that $p \equiv 1 \pmod 3$, otherwise the congruence has precisely p solutions. Let D be the discriminant of the polynomial on the right:

$$D \equiv -a^2d^2 + 6abcd - 4ac^3 - 4db^3 + 3b^2c^2. \tag{1}$$

If $D \equiv 0$ the congruence has precisely p solutions, hence we suppose $D \not\equiv 0$; (and of course $a \not\equiv 0$).

Theorem. The congruence has

$$p + \varepsilon + \begin{cases} \left(\frac{-D}{p}\right)(2u - v) & \text{if } D \text{ is a cubic residue} \\ \left(\frac{-D}{p}\right)(-u - v) & \text{if } D \text{ is a cubic nonresidue} \end{cases} \tag{2}$$

solutions, where ε is -2 or $+1$ according as a is or is not a cubic residue, and u, v are determined uniquely by

$$p = u^2 - uv + v^2 \qquad u \equiv -1 \pmod 3, \quad v \equiv 0 \pmod 3. \tag{3}$$

Lemma 1. The congruence $x^3 - 3kx - m \equiv 0$ has

(a) 1 solution if $\left(\frac{m^2-4k^3}{p}\right) = -1$,

(b) 3 distinct solutions if $\left(\frac{m^2-4k^3}{p}\right) = 1$ and $\frac{1}{2}\left(m + \sqrt{m^2 - 4k^3}\right)$ is a cubic residue,

(c) 0 solutions if $\left(\frac{m^2-4k^3}{p}\right) = 1$ and $\frac{1}{2}\left(m + \sqrt{m^2 - 4k^3}\right)$ is a cubic residue

(d) 2 distinct solutions if $m^2 \equiv 4k^3$.

Proof by Cardan's solution of the cubic.

Let $p = e^{\frac{2\pi i}{3}}$. There exist two non-principal cubic characters, χ and $\overline{\chi} = \chi^2$ such that $\chi(x) = 1, \rho$ or ρ^2 for any $x \not\equiv 0$. $\chi(0) = 0$. The number of solutions in x of $x^3 \equiv y$ is $1 + \chi(y) + \overline{\chi}(y)$. $\chi(-1) = 1$.

Lemma 2. u and v are determined uniquely by (3), and with a suitable choice of χ,

$$u + v\rho = \sum_x \chi\big(x(x+1)\big) = \sum_x \chi(x^2 - \frac{1}{4}).$$

Proof: Bachmann, Kreisteilung, pp 138-141.

Lemma 3. Let $S(\lambda) = \sum_x \chi(x^2 + \lambda)$. Then

$$S(\lambda\mu) = \chi(\mu)\binom{\mu}{p} S(\lambda).$$

Proof. Let $\chi_1(x) = \left(\frac{x}{p}\right)$, and for any χ let

$$\tau(\chi) = \sum_x \chi(x)e(x), \qquad \left(e(x) = e^{\frac{2\pi i x}{p}}\right),$$

so that

$$\chi(t) = \frac{1}{\tau(\overline{\chi})} \sum_u \overline{\chi}(u) e(tu).$$

Then

$$\begin{aligned}
S(\lambda) &= \frac{1}{\tau(\overline{\chi})} \sum_x \sum_u \overline{\chi}(u) e(ux^2 + u\lambda) \\
&= \frac{1}{\tau(\overline{\chi})} \sum_u \overline{\chi}(u) e(u\lambda) \sum_y \{1 + \chi_1(y)\}\, e(uy) \\
&= \frac{\tau(\chi_1)}{\tau(\overline{\chi})} \sum_u \overline{\chi}(u)\chi_1(u) e(u\lambda) \\
&= \frac{\tau(\chi_1)\tau(\overline{\chi}\chi_1)}{\tau(\overline{\chi})} \chi(\lambda)\chi_1(\lambda).
\end{aligned}$$

From this lemma 3 follows.

Proof of the theorem. The congruence can clearly be written in the form

$$y^3 \equiv a(x^3 - 3kx + \ell) \tag{4}$$

where

$$a^4(\ell^2 - 4k^3) \equiv -D.$$

The number of solutions of (4) is

$$\begin{aligned} N &= \sum_x \left\{1 + \chi\left(a(x^3 - 3kx + \ell)\right) + \overline{\chi}\left(a(x^3 - 3kx + \ell)\right)\right\} \\ &= p + \chi(a)S + \overline{\chi}(a)\overline{S}, \end{aligned}$$

where

$$S = \sum_x \chi(x^3 - 3kx + \ell).$$

Let $\nu(y)$ denote the number of distinct solutions in x of $x^3 - 3kx \equiv y$. Then

$$S = \sum_y \nu(y)\chi(y + \ell) = \sum_y \{\nu(y) - 1\}\,\chi(y + \ell).$$

Thus by Lemma 1,

$$\begin{aligned} S &= \sum_{\substack{y \\ \left(\frac{y^2-4k^3}{p}\right)=1}} \left\{\chi\left(\frac{1}{2}(y + \sqrt{y^2 - 4k^3})\right) + \overline{\chi}\left(\frac{1}{2}(y + \sqrt{y^2 - 4k^3})\right)\right\}\chi(y + \ell) \\ &+ \sum_{\substack{y \\ y^2 \equiv 4k^3}} \chi(y + \ell), \end{aligned}$$

the latter term only arising if $\left(\frac{k}{p}\right) = 1$.

Now whatever function Φ may be,

$$\sum_{\substack{y \\ \left(\frac{y^2-4k^3}{p}\right)=1}} \Phi(y) = \frac{1}{2} \sum_{\substack{t \\ t \not\equiv 0,\ t^2 \not\equiv k^3}} \Phi\left(t + \frac{k^3}{t}\right).$$

Hence

$$\begin{aligned}
S &= \frac{1}{2}\sum_{\substack{t\\ t\not\equiv 0}} \{\chi(t)+\overline{\chi}(t)\}\,\chi\left(t+\frac{k^3}{t}+\ell\right)\\
&= \frac{1}{2}\sum_{\substack{t\\ t\not\equiv 0}} \chi(t^2+\ell t+k^3)+\frac{1}{2}\sum_{\substack{t\\ t\not\equiv 0}}\chi\left(\frac{k^6}{t^2}+\frac{\ell k^3}{t}+k^3\right)\\
&= \sum_t \chi(t^2+\ell t+k^3)-1\\
&= S(k^3-\frac{1}{4}\ell^2)-1\\
&= S\left(\frac{1}{4}\,\frac{D}{a^4}\right)-1\\
&= \chi\left(\frac{D}{a^4}\right)\left(\frac{-D}{p}\right)S\left(-\frac{1}{4}\right)-1 \qquad \text{by lemma 3}\\
&= \left(-\frac{D}{p}\right)\chi\left(\frac{D}{a}\right)(u+vp)-1 \qquad \text{by lemma 2.}
\end{aligned}$$

Hence

$$N = p-\chi(a)-\chi^2(a)+\left(\frac{-D}{p}\right)\left\{\chi(D)(u+v\rho)+\chi^2(D)(u+v\rho^2)\right\},$$

which is the theorem.

2.3 All normalized meromorphisms are commutative

k any finite field of $q = p^f$ elements. n.mer. = normalized meromorphism. $|\mu|$ = degree of K over K_μ. $|\mu\nu| = |\mu||\nu|$, $|\mu + \nu| \leqq 2|\mu| + 2|\nu|$, $|\pi| = q$, $|n| = n^2$, $n \nmid p$. All meromorphisms commute with natural multiplication and with powers of π.

If μ is a n.mer., we know that $\frac{dx_\mu}{q_\mu} = c\frac{dx}{y}$ with c in k. We call c the coefficient of μ.

Lemma 1. The coefficients of n.meromorphisms form a subfield k_1 of k.

Proof: If c_1 = coefft of μ_1, c_2 = coefficient of μ_2, then $c_1 \pm c_2$ is coeff. of $\mu_1 \pm \mu_2$ and c_1c_2 is coeff. of $\mu_1\mu_2$. Also if $c_2 \neq 0$, $c_1c_2^{-1}$ is coeff. of $\mu_1\mu_2^{q-2}$.

Let θ be a generating element of k_1, so that every element of k_1 is representable as $a_0 + a_1\theta + \ldots + a_{f_1-1}\theta^{f_1-1}$, where p^{f_1} is the number of elements in k_1, and the a's belong to E_p. Let τ be a n.mer. with coefficient θ. Let $t = |\tau|$. Let

$$P = P(p, f, t) = 3f2^{f-2}p^2t^{f-1}.$$

Lemma 2 For any n.mer. μ there exist integers $a_0, \ldots, a_{f_1-1}$ and a n.mer. μ_1 such that $\mu = a_0 + a_1\tau + \ldots + a_{f_1-1}\tau^{f_1-1} + \pi\mu_1$, and such that

$$|\mu_1| < \max(|\mu|, P).$$

Proof. Coeff. μ is expressible as $a_0 + a_1\theta + \ldots + a_{f_1-1}\theta^{f_1-1}$, with $a_{0}, \ldots, a_{f_1-1}$ integers each with absolute value $< \frac{p}{2}$. Hence

$$\text{Coeff.}(\mu - a_0 - a_1\tau - \ldots - a_{f_1-1}\tau^{f_1-1}) = 0,$$

hence[1]

$$\mu = a_0 + a_1\tau + \ldots + a_{f_1-1}\tau^{f_1-1} + \pi\mu_1$$

[1] Randvermerk: *falsch*!

where[2] μ_1 is a mer. which is ipso facto normalised. Also

$$\begin{aligned} q|\mu_1| &\leqq 2|\mu| + 2|a_0 + a_1\tau + \ldots + a_{f_1-1}\tau^{f_1-1}| \\ &\leqq 2|\mu| + 2^{f_1}\left(\frac{p}{2}\right)^2(1 + t + \ldots + t^{f_1-1}) \\ &\leqq 2|\mu| + \frac{1}{4}P. \end{aligned}$$

Hence, either

$$|\mu_1| < P, \quad \text{in the case } |\mu| < P$$

or

$$|\mu_1| < |\mu| \quad \text{in the case } P \leqq |\mu|$$

(because $q \geqq 3$.)

Denote by $A_0(\tau)$ the polynomial in τ: $a_0 + a_1\tau + \cdots + a_{f_1-1}\tau^{f_1-1}$ and by $A_1(\tau), \ldots$ similar polynomials.

Lemma 2 For any n.mer. μ there exists an integer $n > 0$ and polynomials $B_0(\tau), \ldots, B_r(\tau)$ such that

$$(\pi^n - 1)\mu = B_0(\tau) + \ldots + B_r(\tau)\pi^r.$$

Proof. As in the case $k = E_p$.

Theorem If μ, ν are two n.mer., then $\mu\nu = \nu\mu$.

Proof: obvious.

$$(\pi^m - 1)\mu(\pi^n - 1)\nu = (\pi^n - 1)\nu(\pi^m - 1)\mu$$

implies $\mu\nu = \nu\mu$, because the ring of n.mer. has no zero-divisors, and μ, ν commute with powers of π.

$k = E_p$.

n.mer. = normalised meromorphism (perhaps 0). Every n.mer. μ commutes with integers and with $\pi, \overline{\pi}$. $|\mu|$ means the degree of μ, ie. of K over K_μ.

[2] vorangehende Formelzeile offenbar nachträglich abgeändert in:

$$p^{f-1}\mu = p^{f-1}(a_0 + a_1\tau + \ldots + a_{f_1-1}\tau^{f_1-1}) + \pi\mu_1$$

wobei "$\pi\mu_1$" unterstrichen ist.

$|n| = n^2$, $|\mu\nu| = |\mu||\nu|$, $|\mu + \nu| \leqq 2|\mu| + 2|\nu|$. $|\pi| = p$.

Lemma. If μ is any n.mer., then there exists an integer a_0 and a n.mer. μ_1 such that $\mu = a_0 + \pi\mu_1$, with $|\mu_1| < \max(|\mu|, 2p)$.

Proof. There exists an integer c such that $\frac{dx_\mu}{y_\mu} = c\frac{dx}{y}$. Choose $a_0 \equiv c \pmod p$ with $-\frac{p}{2} < a_0 < \frac{p}{2}$. Let $\nu = \mu - a_0$. Then $\frac{dx_\nu}{y_\nu} = 0$. Hence there exists a meromorphism μ_1 such that $\nu = \pi\mu_1$. μ_1 is ipso facto normalised. Thus $\mu = a_0 + \pi\mu_1$, and

$$|\mu_1| = \frac{1}{p}|\mu - a_0| \leqq \frac{2}{p}|\mu| + \frac{2}{p}\left(\frac{p-1}{2}\right)^2 < \begin{cases} |\mu|, \\ 2p, \end{cases}$$

according as $|\mu| > \frac{(p-1)^2}{2(p-2)}$ or not.

Theorem 1. For any n.mer. μ there exist integers $b_0, \ldots, b_m$ and a positive integer n such that

$$(\pi_1^n - 1)\mu + b_0 + b_1\pi + \ldots + b_m\pi^m = 0.$$

Proof. By lemma 1 we have the process:

$$\begin{aligned} \mu &= a_0 + \pi\mu_1 \\ \mu_1 &= a_1 + \pi\mu_2 \\ \mu_2 &= a_2 + \pi\mu_3 \\ \cdots & \quad \cdots \end{aligned}$$

with $|\mu_r| < \max(|\mu_{r-1}|, 2p)$. For $r > |\mu|$ we have $|\mu_r| < 2p$.

Now there are only a finite number of n.mer. λ with $|\lambda| < 2p$, (for the x-component of such a n.mer. λ has at most $4p$ coefficients each with at most p possible values, and similarly for $\frac{y_\lambda}{y}$ which is a rat. function of x of degree $\leqq 4p$).

Hence two of the μ_r are identical, say $\mu_k = \mu_{k+n}$, $n > 0$. Then

$$\mu = a_0 + a_1\pi + \cdots + a_{k-1}\pi^{k-1} + \pi^k\mu_k$$

and

$$\mu_k = a_k + a_{k+1}\pi + \cdots + a_{k+n-1}\pi^{n-1} + \pi^n\mu_k,$$

hence the result.

Theorem 2 π satisfies an algebraic equation $c_n \pi^n + c_{n-1}\pi^{n-1} + \cdots + c_0 = 0$, where $c_n, \ldots, c_0$ are integers not all 0.

Proof. By Theorem 1 with $\mu = \overline{\pi}$,

$$(\pi^n - 1)\overline{\pi} + a_0 + a_1\pi + \cdots + a_m\pi^m = 0.$$

Hence:

$$(\pi^n - 1)p + a_0\pi + a_1\pi^2 + \cdots + a_m\pi^{m+1} = 0,$$

and the coefficient of the constant term is not 0.

If we use the fact that $\pi + \overline{\pi}$ is an integer, it follows from Theorem 1 that every n.mer. μ satisfies an equation of the form $N\mu = a + b\pi$ where N, a, b are integers.

2.4 Note to joint paper

Note 1.

I suggest (as I did before) the omission of the sentence "Dort ... darstellt". The formula given is an immediate identity from the definition of $\pi(\chi, \psi)$. To consider it as a consequence of our relation $(0, 6)$ is rather absurd – like deducing $2 \cdot 2 = 4$ from the Binomial theorem would be. Again, although it may be regarded as an inversion of our relations, this is only in a purely formal sense, for the whole point of our relations is that we deal with $\pi(\chi, \psi)$ where χ, ψ are indep. of one another. The whole sentence is more likely to confuse the reader than to help him, I think. Suppose one had proved after some difficulty that a field was commutative. One would not then add: in particular we deduce $a.a = a.a$.

Note 2.

Re footnote 13). I suggest this be either omitted or written "Erscheint wahrscheinlich in der Oxford Quarterly Journal". I think this would be a good place to publish it. I have not yet written the paper because I still have some hope of getting an elem. proof of $(0, 9)$.

Chapter 3

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