If $n \not\equiv 0 \pmod 8$ then $W - W^{(3)}$ is a relative but not absolute invariant for $\mathscr{G}$. In this situation there is a particular polynomial f (depending on χ) such that $W^{(1)} - W^{(3)}$ can be written uniquely as f times an absolute invariant for $\mathscr{G}$ (see for example [55], [56]). To find the degree of f we compute the Molien series

$$\Phi_\chi(\lambda) = \frac{1}{|\mathscr{G}|} \sum_{A \in \mathscr{G}} \frac{\overline{\chi(A)}}{\det(I - \lambda A)} \tag{26}$$

where the bar denotes complex conjugation. This is easily computed if we observe that $\mathscr{G}$ has a subgroup $\mathscr{H}$ of (

Scan

J. Leech

& NJAS

Correspondence

June-July 1975

5 sides total

add to A6088

where

Then

where $C_j = T^j$, C_{j+4} ... s in (26) we find that $\Phi_\chi(\lambda)$ is

$$\frac{\lambda^{30}}{(1-\lambda^8)(1-\lambda^{24})}, \quad \text{if} \quad n \equiv 6 \pmod 8.$$

On the other hand it is easy to verify that the polynomials f_{18}, f_{12}, $f_{18}f_{12}$ (see f91 ✓

Computing Science : Professor J Leech

6088

UNIVERSITY OF STIRLING STIRLING SCOTLAND | TELEPHONE: STIRLING (0786) 3171

1975 June 16

Dear Sloane:

Many thanks for the offprints, which you have recently sent me; they are much more convenient in this form than your xeroxed typescripts!

I have recently acquired Berlekamp's collection of papers in coding theory, which are similarly in a highly convenient form. I note however that his account of the Leech lattice is somewhat garbled, and that the additional references promised for the new codes in your paper do not seem to have materialized.

I enclose a copy of a review I have just been writing for the "Computer Journal". The reference to you not recomputing every entry is partly a dig at a correspondent in the "Computer Bulletin" (March 1975) who was interested in stamp folding and wrote of your sequence 576 that at least three terms are incorrect, as you have

1, 2, 5, 14, <u>39</u>, 120, <u>358</u>, <u>1176</u>, ...

instead of 1, 2, 5, 14, <u>38</u>, 120, <u>353</u>, <u>1148</u>, ...

and goes on: "Finding major errors at one's first attempt does not inspire confidence in the rest of the book; material of this nature should be checked and double checked before it can be regarded as worth while collecting it in book form."

The editor of this section, who is a bit gormless and wisely hides behind the pseudonym "Aleph Null", adds that he checked the series against other sources and finds the wrong values in J. Comb. Th. for September 1968 (possible source of the error?) [I said he was a bit gormless — this is the reference you give!]; Martin Gardner gave correct values in Scientific American for August 1963. [I have checked by hand that 38 is the correct value, not 39.]

6088

I would appreciate receiving your promised supplements when these are available. Here's another sequence for you: $\prod_{i=1}^{n}(2^i+2)$

1, 4, 24, 240, 4320, 146880, 9694080, ...,
4 4.6 4.6.10 4.6.10.18 4.6.10.18.34 ·66

which I'm sure you will readily identify. (It took me some time to identify originally and even longer to prove!) I could suggest others, but they would be much more artificial and unlikely to occur to anyone else.

I hope all goes well with you.

Yours,

John Leech

fqy

6088

June 30, 1975

Professor John Leech
Department of Computing Science
University of Stirling
Stirling
SCOTLAND

Dear Leech:

It was a pleasure to get your letter of June 16. I had written to Berlekamp about the numerous errors and omissions in his anthology, including the ones you mentioned. But of course it is too late for him to do anything about them; or at least I am confident that nothing will be done.

Did you see that Koehler, S. J.!, who wrote the J. Comb. Theory paper on stamp-folding, actually references Martin Gardner's column with the correct values for sequence 576? But obviously he had not bothered to check his results against Gardner's. The three errors are all caused by faulty subtractions on the very last page of Koehler's article. I tried to telephone Koehler in Seattle about this, but was told that he had taken a year's leave of absence, is doing parish work somewhere in Canada, and can't be reached. No wonder. I have also tried to follow Koehler's "solution" to the problem, but it is awful and I quickly gave up. (Perhaps a thesis problem for one of your students?)

Actually someone had shown me the very snide letter in the "Computer Bulletin" and Aleph Null's ridiculous remarks. I am relieved to hear that you have a low opinion of him. And your review for the Computer Journal is very generous indeed.

The sequence 1,4,14,194... is in Supplement I, which is enclosed. So far this is the only one issued, although another is long overdue. Quite a large number of letters have arrived, many with new sequences. Victor Meally of Dublin has been one of the most enthusiastic correspondents. He has even forced me to move a sequence. Seq. 951 has become 953.1: 1,3,5,6,11,12,..., correcting an error of Motzkin's.

It took about an hour, but $\prod_{i=0}^{n}(2^i+2)$ seems to fit your new sequence. Actually numbers rather like this arise in enumerating codes (or subspaces) - see page 155 of "Good Self-Dual Codes Exist" by MacWilliams, Thompson and myself. Is there a reference for your sequence?

It was good to hear from you.

With best regards,

MH-1216-NJAS-mv

N. J. A. Sloane

Enc.
As above

f91

3060
6088

July 7
1975

Many thanks for your letter of June 30, just arrived. Please excuse this hasty scribble: we are going away for a fortnight beginning tomorrow. Many thanks also for the supplement to your handbook. No, I hadn't pursued Koehler's work on stamp folding.

You should have recognised 4, 24, 240, 4320, ... as the maximum contact numbers in the Reed-Muller sphere packings! References such as Leech 1964 — but I didn't explicitly list the terms (though this goes for many of your arithmetically elementary sequences also). I've forgotten how long it took me to recognise the formula — except that it took much longer to devise the proof in Leech 1964! Notice that the terms 4, 24, 240 give the numbers of unit Gaussian integers, unit quaternions, and unit Cayley numbers — but this definition doesn't generalize!

Re your new sequence 1139·5,

noted

$9^2 = 81 = 1010001_2$ is not a palindrome.

Re sequence 1157·5, the next term is $(10^{19}-1)/9$

3060 $= 111\ldots1$ (nineteen ones in decimal) which is known to be prime. So (If I remember correctly) is $(10^{23}-1)/9$. (Yes - see your sequence 1273·5.)

No more for now. All the best,

Yours,

John Leech