Four letters by Georg Kreisel

In October 1982 I visited the Mathematics Dept. of ETH in Zürich for about three weeks, upon a kind invitation by Erwin Engeler. Georg Kreisel was spending a few months in the same department and I had the occasion to discuss with him about a notion of Gödel numbering and parametrization I had that could yield a simple approach to computability in higher types. We continued our discussion by correspondence (on paper, of course). It is well-known that Kreisel loved to discuss by writing letters and to express very critical (and stimulating) appreciations about new work. As a matter of fact, his letters ask for more motivations and applications, suggest further links to existing work …. Unfortunately, I have lost my answers. I could find though (and upload) the ETH preliminary note (typewritten by a secretary in one day) Kreisel is referring to and that served as a basis for our discussions. This short text contained just the notion and the conjectures I was exploring and that interested Kreisel. This notion and the conjectures were later developed (and proved) in joint work with my students Simone Martini and Eugenio Moggi, in different directions (type-free and typed systems). The papers with Eugenio, by his crucial mathematical contribution, allowed to prove some non obvious theorems and answer part of the questions raised in this exchange of letters. For example, as for Kreisel's remark that something *natural* geometrically may be *artificial* computationally (and viceversa, I dare to add), we could show the computational pertinence of the approach as a simpler way to construct classical higher type recursive functionals, while the diagrammatic and "universality property" it is based on, hints to its categorical-geometric naturality.

Two of these papers may be found in the last section of my downloadable papers ("1980's: Some of the Good Old Papers that Resist"), jointly to a reference to a very interesting recent article:

> J.R. Cockett, P.J. Hofstra, *Introduction to Turing categories*, **Annals of Pure and Applied Logic** 12; 156(2-3):183-209, 2008,

where "a convenient setting for the categorical study of abstract notions of computability" is presented, while acknowledging that "Longo and Moggi, in [30,29], made significant contributions to this programme. While their main motivation seems to have been the development of categorical settings for the study of computability at higher types (as opposed to elementary recursion theory), they formulated the appropriate categorical concepts corresponding to Gödel numberings and parametrization."[1]

Paris, December 15, 2016

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1 A methodological reflection on the "tools from continua" used in the proofs is presented in:
- G. Longo. "Continuous structures and analytic methods in computer science". *Invited Lecture.* In Courcelle, editor, **Ninth Colloquium on Trees in Algebra and Programming** (CAAP 84), pp. 1-22. Cambridge University Press, 1984.

The exchange with Kreisel had a role also in suggesting the theme of this 1984 lecture.
This interplay of "continuous vs discrete" structures, as for symmetries for example, is relevant also in Biological theorizing; e.g. in
- G. Longo, M. Montévil, **Perspectives on Organisms: Biological time, symmetries and singularities,** Springer**,** 2014;
- G. Longo "Information and Causality: Mathematical Reflections on Cancer Biology", *in print*.

Dear Prof. Longo, 20.XII.82

Thank you very much for your kind letter of 29.XI. with the good news that you and your students Moggi and Martini have proved the conjectures on p.4, resp. p.7 of the preprint we discussed at the ETH at Zürich. I hope that the consequences which you expected to derive from proofs of these conjectures have materialized.

On rereading the preprint I noticed that, for quite trivial reasons, Kleene's countable functionals, that is, the functionals meant by Kleene, cannot possibly be contained in the class you consider. For, in the notation of my paper in [3], p.116, Def. 4.13, Kleene's countable functionals CORRESPOND to the arbitrary continuous functionals (there are uncountably many; cf. the remark on p.4 of your preprint). But even if you consider the recursive countable functionals, all the $F_1, \ldots, F_n$ are unrestricted, only F_{n+1} is required to be recursive.* In your case, we have HEREDITARY (partial) recursiveness (hereditary). This corresponds to the effective operations (Def. 4.2 on p.117), except* that yours are PARTIAL, in other words, a generalization of Myhill-Shepherdson.
CONJECTURE. 4.21 on p.117 holds, mutatis mutandis, for your operations. (In other words, though you operate on Gödel numbers of partial recursive functions, the functionals of higher type so defined ~~by you~~ can also be described in terms of (partial) recursive representing - or, better, neighborhood - functions in the style of Def. 4.13.)

Why do I write 'CORRESPOND' in block letters? For Kleene, each countable functional of type n+1 is defined on ALL functionals of type n (of the full set-theoretic hierarchy), not only on the countable functionals of type n. This, in turn, was necessary to prove closure under S1-S9, because S8 was so set up that the argument has to be defined on the full hierarchy. I believe that nowadays specialists, like Normann or Gandy, have modified Kleene's original convention. I agree with the modification. But it is worth remembering that, once Kleene had decided on the convention in the case of S1-S9, the (odd) definition of 'countable' was forced on him.

* Your type(n+1) is: $(n) \to (n)$, while, for Kleene and myself in [3], type (n+1) was: $(n) \to (0)$.

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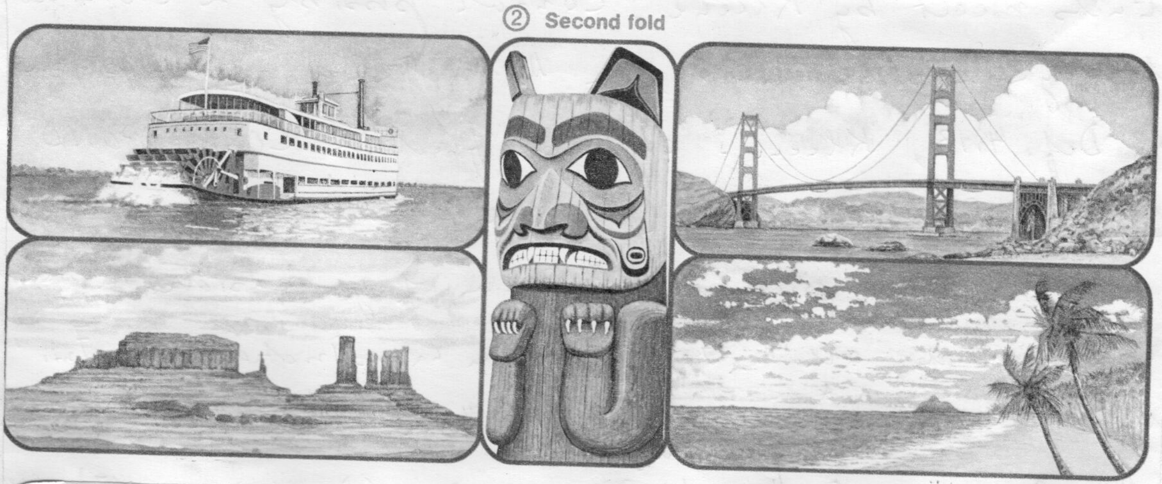

Additional message area

Reminder. There is a delicate question (which we discussed at Zurich) about the exact meaning of a class of hereditarily TOTAL functions being contained in a — or, here: your — class of hereditarily PARTIAL functions. A clarification of this meaning would be of interest not only for the specific problem at the bottom of p. 3 of your preprint, but also, e.g., for Ershov's work.

Interest of functionals of all finite types. (I am afraid I don't know what you mean by 'powerful' or even 'smooth' without any indication of, at least, the GENERAL AREA OF PROBLEMS to be attacked.) I agree that the interest for traditional foundations is clear, e.g. compared to functionals of lowest type; cf. 5.32 on p. 122 of [3]. But this leaves open the separate question to what extent the aims of traditional (logical) foundations are well chosen.

It is too late to wish you a merry Xmas. So: a Happy New Year

As ever GK.

G. KREISEL

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8. V. 83

Dear Prof. Longo,

Thank you for your letter of 20.II. and the MS which I found here on my arrival about one week ago. First, your question.

I do not know an easy way of going from HEO to the recursive countable functionals, but I know of course an easy way of treating them simultaneously (as I did in the Constructivity volume; Hyland gives a slicker treatment). One just needs some simple closure condition on – what I called there 'representing' functions; better – the class of neighbourhood functions used. – You might like to paraphrase this in terms of Ershov's set up.

You have not been boring me at all. (The advantage of written material is that one gives it attention when one feels like doing it; so the chance of boredom is slight.) But you'll give me real pleasure when you and/or your student Moggi decide what kind of recursive embedding, if any, injects the hereditarily total into (Ershov's) partial functionals.

At the risk of my boring you, let me repeat my main QUESTION: In which area of information processing – be it the unwinding of sophisticated classical proofs, manipulating space shuttles or preparing bank statements – are the countable functionals or HEO really RELEVANT? – Obviously, it is a matter of temperament if one wants (i) to prove general theorems about some mathematical object first, and see later for which mathematical (or other) phenomena this object helps us understand and master, or (ii) look at the object briefly first, spot phenomena to which it is relevant as soon as possible, and let these phenomena suggest further questions about that object. – What troubles me is that – after more than 25 years – we do not seem to have discovered much about the PROPER DOMAIN OF APPLICATION of these objects of higher type. – I am not competent to judge the algorithmic needs of space shuttles or banking arrangements. But I have examined recently some OVERSIGHTS 25 years ago in (my) expectations for higher type operations.

Examples. (a) Higher types *versus* lowest type of functional as used in the so-called n(o)-c(ounter example)-i(nterpretation); cf. p. 229 of the Brouwer Centenary volume (NH, 1982), especially Reminder (iii), and p. 231, (b).
(b) Use of (even) the lowest type of functional as in the n.c.i.; cf. pp. 51-52 (sundry observations, (iii), on p. 51), and especially the open question on p. 52, l. 4-8, all in the Herbrand volume (NH, 1981).

I know it is often said that 'systematic' theory is needed for orientation: to help you find the relevance of the mathematical object under study. This overlooks the problem of discovering WHAT (features) to study systematically, to avoid systematic errors (of judgment). Frankly, it seems to me easier to do such systematic studies than to KNOW WHEN TO STOP. But I'd love to be proved wrong in the particular case of HEO and/or countable functionals. I hope you do not find this letter less encouraging than my earlier efforts.

Assez G Kreisel

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13.VI.83

Dear Prof. Longo,

I was very glad to learn from your letter of 26.V. that denotational semantics is considered useful for such things as type checking (which are above my head, and apparently neglected by the computer scientists at Stanford). In my ignorance I thought that experience with complex function theory or differential topology was more useful for computing than experience with f-spaces; for example, because of SMALE's success with analysing the efficiency of Newton's method or the simplex method; cf. Bull. A.M.S. (1981) p.1, resp. p. 285 of Notices A.M.S. (April 1983). – I don't mean 'in my ignorance' ironically, I am not at all familiar with, nor particularly attracted by, the problems of general purpose programming languages. Naturally, I realize their practical interest. (I am less convinced that a 'theory' is needed, with the resulting risk of drawing attention away from 'isolated' bright ideas.)

I seem to have missed Ershov's paper in Algebra i Logic 15, pp. 400 – 408 (and he did not mention it when I raised the problem in question): IN WHICH SENSE IS THE <u>MAPPING</u> OF HEO OR OF THE RECURSIVELY COUNTABLE FUNCTIONALS INTO ERSHOV'S SPACES <u>RECURSIVE</u>? Put more concretely: What do we want to know about HEO and the rec. countable functionals that we can read off from (i) the 'natural' embedding you mention and (ii) existing knowledge of Ershov's spaces?[2] - As you see, I am constantly forced to return to the truly basic question of: What do we want to know about HEO anyway (and don't know already)?!?

NB. A map that is 'natural' geometrically, is usually artificial computationally.

As ever

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18. VI. 84

Dear Prof. Longo,

Thank you very much for your letter of 31. V., and various reprints. – To avoid misunderstanding: the reprints I sent you have little to do with automated deduction (in the usual sense, of automatically proving or refuting a given formula), but with automated MANIPULATION OF PROOFS, for example, to synthesize a program from a proof. NB. The essence of the whole enterprise is that one does not need fully formalized proofs as input, nor need they be intuitionistic.

During the last term I ran a seminar on parallel computation, and, in particular, the use of – manipulating – infinite proof trees to synthesize programs suitable for parallel computation. The essence here is that the manipulations be continuous (in the sense of my article with Mints and Simpson in SLN in Math. 453), that is, on operations on finite stumps of trees, albeit with many branchings). It appears that there is an advantage in operating on the kinds of trees used in Girard's Π^1_2 logic which Jervell calls homogeneous. NB. This is a much stronger condition than merely being balanced.

I have to confess that for parallel computation anything any of us knew of – denotational semantics was of little use.

At least so far (in the seminar) the central dogma of 20th century mathematics seems to apply to programming techniques: what is true in general, is liable to ~~app~~ be trivial in most particular cases. NB At least towards the end of the 19th century and in the first decades of our century the opposite view was held (and practiced): so the dogma quoted above is not a mere prejudice.

I feel that current work, both in mathematics and computer science, is VERY RELEVANT TO FOUNDATIONS, namely, for discrediting foundational IDEALS (for example, of a certain style of exposition: from the general to the particular, as opposed to the INTERPLAY of a certain relatively large stock of 'basic' elements). 'Discrediting' not by showing that the ideals are not realizable, for example, because of incomputability, but by showing that they can be realized, and the realizations do not live up to expectation.

As ever

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I shall be travelling quite a lot. Late June c/o ENGELER in Zürich, early July c/o SCHWICHTENBERG in Munich, early September c/o M. BAAZ, A-1080 Wien (Vienna), LOIDOLDG. 4/10 (Tel. 4277974*)
Late Sept 84 - March 85 c/o STANFORD PROGRAM IN VIENNA, A-1010 WIEN, KARL-LUEGER-PLATZ 2.
(The people at Salzburg whose address I sent you together with the Appendix to Logical Foundations: a lingering malaise will store my papers).

* I give you the number in case to stop in Vienna on your way to or from Prague