MATTHIAS BAAZ, CHRISTOS H. PAPADIMITRIOU, HILARY W. PUTNAM, DANA S. SCOTT, CHARLES L. HARPER, JR., eds. ***Kurt Gödel and the Foundations of Mathematics: Horizons of Truth***. New York: Cambridge University Press, 2011. ISBN 978-0-521-76144-4. Pp. xxiii + 515.

Reviewed by PETER SMITH

Blockbuster centenary conferences are usually very unsatisfactory affairs. Too many of the great and the good will have nothing very new to say. And despite a notional common focus, speakers interpret their brief in wildly different ways. Unless editors take a stern line, the resulting conference volumes are equally unsatisfactory. This book, comprising 21 papers arising from the 2006 Vienna 'Horizons of Truth' symposium marking Gödel's anniversary, is even more of a mixed bag than usual. Which papers are worth looking at, five years after the event?

Some pieces can be rapidly passed over by most readers of this journal. Ivor Grattan-Guinness writes with his usual care about 'The reception of Gödel's 1931 incompletability theorems by mathematicians, and some logicians, to the early 1960s'. There are one or two interesting anecdotes (e.g. Saunders Mac Lane studied under Bernays in Hilbert's Göttingen in 1931 to 1933, but writes that that he was not made aware of Gödel's result). But the general theme that logicians got to know about incompleteness early (with some surprising little delays), while the word spread among the wider mathematical community much more slowly, is no news. Karl Sigmund discusses 'Gödel's troubled relationship with the University of Vienna based on material from the archives as well as on private letters'. This is also a nicely readable paper about the facts of the case, but there's nothing here that sheds new light on Gödel's intellectual development. By contrast, Georg Kreisel's contribution isn't readable at all, but is like a cruel parody of late Kreiseleriana, rambling and allusive. The paper's inclusion does no kindness at all to the author – though the endnotes, more than twice as long as the paper itself, have moments of interest. I was struck by one where Kreisel reports that he at one point 'saw a good deal of Bernays, who liked to remember Hilbert .... According to Bernays ... Hilbert was asked (before his stroke) if his claims for the ideal of consistency should be taken literally. In his (then) usual style, he laughed and quipped that the claims served only to attract the attention of mathematicians to the potential of proof theory'. And Kreisel goes on to say something about Hilbert wanting to use consistency proofs to bypass 'then popular (dramatized) foundational problems and get on with the job of doing mathematics'. This chimes with the'naturalistic' reading of Hilbert, most recently defended by Curtis Franks.

Piergiorgio Odifreddi contributes less than five thin pages on how 'certain philosophers of Gödel's favourite philosophers can be seen as an inspiration for ... his mathematical work'. Christos H. Papadimitriou writes briefly on 'Computation and intractability'. He touches on Gödel's 1956 letter to von Neumann and his prefiguring of something like the question whether $P = NP$. But this has been extensively discussed elsewhere, and there is nothing new here. Papadimitriou also adverts to a result about the intractability of finding Nash equilibria which is proved by a method of arithmetization inspired by Gödel: but you won't learn how or why from this paper.

Karl Svocil's paper on 'Physical Unknowables' is included in a section on Gödelian cosmology, but doesn't even mention Gödel's model universe. Svocil instead rambles about indeterminism, 'intrinsic self-referential observers', unpredictability, busy beavers, deterministic chaos, quantum issues, complementarity, and lots more in a hopelessly unfocused way. Like Svozil, John Barrow ranges widely over notions of incompleteness in mathematics and science in his 'Gödel and physics'. At least he writes with his usual admirable clarity. But it is all too slapdash (from irritating little things like trying to define syntactic consistency using the notion of truth to bigger things like quite mis-stating how a Turing machine thrown at a space-time singularity can in principle be used to decide classically undecidable questions in Mark Hogarth's now famous construction). The theologian Denys Turner contributes a paper on 'Gödel, Thomas

Aquinas, and the unknowability of God'. But even he thinks that any analogies between Gödel and the tradition of 'negative theology' are pretty tenuous, and he says 'I simply do not know whether the superficial parallel is genuinely illuminating'. Trust me: it isn't.

Finally, you can also miss Roger Penrose's latest effort on 'Gödel, the mind, and the laws of physics'. Stewart Shapiro's wonderfully lucid, careful, and generous treatments of the Lucas/Penrose arguments in his 'Incompleteness, mechanism and optimism' (1998) and 'Mechanism, truth, and Penroses new argument' (2003) have become the necessary starting point for any subsequent discussion here. But Penrose's contribution here is written as if Shapiro had never made the effort to try to sort things out (there's the briefest of footnote references, but he simply doesn't address the raft of technical issues that Shapiro raises).

Of the remaining dozen papers, four basically have the form of overviews/tutorials. I have always hugely admired Wolfgang Rindler's exemplary textbooks on relativity. Here he contributes a paper aiming to explain Gödel's contribution to a wider audience. He doesn't disappoint. If you want to know what Gödel's cosmological model looks like, and have a smidgin of knowledge about relativity theory, then this paper is a great place to start. True, there is no philosophical discussion about worries concerning the very idea of closed time loops: but that's no complaint – the paper does beautifully what it does set out to do. Avi Wigderson also writes well on computational complexity. Like Rindler's piece, this paper will no doubt will be tougher for many than the author really intends: but if you already know just a bit about P vs NP, it should be accessible and will show you just how prescient Gödel's insights were in his letter to von Neumann. Petr Hájek offers a bit more than a tutorial in his review of 'Gödel's ontological proof and its variants'. Enthusiasts for the game of exploring that proof will enjoy this paper, for there is some nice systematising work going on (or so it seems to this non-enthusiast). By contrast, Ulrich Kohlenbach, writing on Gödel's functional interpretation, misses the mark. This is a pity, as philosophers in particular might welcome another insightful tutorial. But Kohlenbach rachets up the technical level radically, and his paper will be pretty inaccessible to most readers. The author has done significant work in this area: but as an effort towards making this available and/or explaining its importance to a slightly wider readership than researchers in one corner of proof theory, this over-brisk paper fails.

So what of the eight papers left? Taking them in the order in which they appear in the collection, we first have Angus Macintyre on 'The impact of Gödel's incompleteness theorems on mathematics'. His title is much the same as that of a short and rather more readable piece by Feferman in the *Notices of the AMS*, and his conclusion is also much the same: the impact is small. There's a lot of reference to mathematics which is likely to be beyond the horizon for most readers of this journal ('Étale cohomology of schemes can be used to prove the basic facts of the coefficients of zeta functions of abelian varieties over finite fields' anyone?). But Macintyre's general conclusion looks right: '[A]s far as incompleteness is concerned, its remote presence has little effect on current mathematics.' The novelty, though, isn't here but in the substantial appendix which aims to give an outline justification for the view that we have 'good reasons for believing that the current proof(s) of FLT [Fermat's Last Theorem] can be modified, without abandoning the grand lines of such proofs, to proofs in PA'. Enthusiasts or sceptics about Macintyre's view will certainly want to consult the paper for the details, which indeed look rather impressive to me.

Next, Juliette Kennedy reflects on the brief philosophical remarks at the outset of Gödel's dissertation (which were omitted when he published his completeness proof). She is developing what Dreben and van Heijenoort say in §2 of their introduction to the dissertation in the *Collected Works*, who note that Gödel's remarks are somewhat misleading but also prefigure the incompleteness theorem. Kennedy's discussion is less than ideally clear. But she wants to stress the importance of Carnap in shaping Gödel's ideas, and that at least is surely right.

The following paper is a typically lucid and persuasive piece by Solomon Feferman on the Bernays/Gödel correspondence, on the theme of 'Gödel on finitism, constructivity, and Hilbert's Program. As he writes,

> There are two main questions, both difficult: first, were Gödel's views on the nature of finitism stable over time, or did they evolve or vacillate in some way? Second, how do Gödel's concerns with the finitist and constructive consistency programs cohere with the Platonistic philosophy of mathematics that he supposedly held from his student days?

On the first question, Feferman wrote in his introduction to the correspondence in the *Collected Works* of Gödel's 'unsettled' views on the upper bound of finitary reasoning. Tait has dissented, and Feferman returns to defend his earlier description. On the second question, he 'venture[s] a psychological explanation', following Takeuti in seeing Gödel's whole academic career as shaped by the goal of surpassing Hilbert.

B. Jack Copeland's offers one of the longer papers in the volume. Most of it is a story about the development of computing devices from Babbage to the Ferranti Mark I (complete with photos): mildly interesting if you like that kind of thing, but utterly misplaced in this volume, and eminently skippable. But randomly tacked on the end is a final section which *is* germane. The issue here is Gödel's 1970 note which attributes the view that 'mental procedures cannot go beyond mechanical procedures' to Turing. Copeland responds not by worrying about Gödel's anti-mechanism but with evidence that Turing in fact shared it. He cites passages where Turing criticises what he calls an 'extreme Hilbertian' view and writes of mathematical intuition delivering judgements that go beyond this or that particular formal system. In fact,

> Turing's view . . . appears to have been that mathematicians achieve progressive approximations to truth via a nonmechanical process involving intuition. This picture, in which minds devise and adopt successive, increasingly powerful mechanical formalisms in their quest for truth, is consonant with Gödel's view that 'mind, in its use, is not static, but constantly developing.' These two great founders of the study of computability were perhaps not quite as philosophically distant on the mind-machine issue as Gödel supposed.

Or indeed, as casual commentators might suppose: but Copeland's evidence seems rather convincing.

Back in 1967, Benacerraf famously gives a nice argument, going via Gödel's Second Theorem, that proves that either my mathematical knowledge can't be simulated by some computing machine (there is no particular Turing machine which enumerates what I know), or if it can be then I don't know which machine does the trick. But of course, how interesting you think this result is will depend on just how seriously you can take the notion that there might such a determinate body of truths as 'my mathematical knowledge'. And indeed, worries about idealizing mathematicians and about the vague open-endedness of the informal notion of proof will always beset attempts to get sharp anti-computationalist conclusions about the mind from Gödelian considerations. In his brief paper, 'The Gödel Theorem and human nature', Hilary Putnam brings such worries to bear against Penrose in particular. Rather than pick holes again in the details of Penrose's arguments, he now stresses that the whole enterprise is misguided. 'The very notion of an ideal mathematician is too problematic' to enable us to set up a contrast between what a suitably idealized version of us can do and what a 'mechanism' can do. The complaint is a familiar one, though none the worse for that: still, it hardly makes for an original contribution at this stage in the game. If there is novelty here, it comes when Putnam turns to a different line of argument – developing a theme from his 'Reflexive reflections' (1985). The target is a Chomskian hypothesis to the effect that we have a 'scientific faculty' such that this faculty – in idealized form – can be simulated by some particular Turing machine T. In other words, (C) T enumerates (a coded version of) every true sentence of the form 'we are justified in accepting p on evidence e'. Putnam offers an argument that either (C) isn't true, or if it is we are not justified in believing it (I can't have a justified belief about which machine does the simulation trick). Oddly, however, Putnam does not mention the analogous Benacerraf argument at all, so readers need to do their own 'compare and constrast' exercise. But the Chomskian hypothesis strikes this reader as so implausible as to the make the game not worth the effort.

We turn next to Harvey Friedman, who aims to discuss a 'sample of research projects that are suggested by some of Gödel's most famous contributions' a prospectus which should immediately alert the reader to the likelihood that the paper will cover too much too fast. The piece has the remarkably self-regarding title 'My forty years on his shoulders' and ends with the usual Friedmanesque announcements of results about the equivalence of the provability-in-various-arithmetics of certain combinatorial claims with the consistency of certain set theories with large cardinals. The style and content will be very familiar to subscribers to the FOM list, and probably pretty baffling to others.

One place where Friedman's paper goes a bit slower is in discussing the Second Incompleteness Theorem, and there are intimations by the author that he has found a neater, more insightful way of developing the result than usual. But with his customary academic incivility, he doesn't bother to explain this in accordance with the normal standards of exchange between colleagues, but refers to online unpublications . . . where things remain equally unexplained. This is, to put it mildly, just irritating.

The late Paul Cohen provides the text of a short talk 'My interaction with Kurt Gödel: the man and his work'. The title is full of promise, but there seems relatively little new here. For Cohen had previously written with great lucidity a quite fascinating paper 'The discovery of forcing' and he already talks there of his interactions with Gödel. Still he does make some remarks now which will be of particular interest. For example,

> I visited Princeton . . . and had many meetings with Gödel. I brought up the question of whether, as rumor had it, he had proved the independence of the axiom of choice. He replied that he had, evidently by a method related to my own, but he gave me no precise idea or explanation of why his method evidently failed to succeed with the CH. His main interest seemed to lie in discussing the truth or falsity of these questions, not merely their undecidability. He struck me as having an almost unshakable belief in this realist position that I found difficult to share. His ideas were grounded in a deep philosophical belief as to what the human mind could achieve.

And Cohen suggests that in seeking independence proofs, Gödel was still 'looking for a syntactical analysis that was in the spirit of his definition of constructibility'. Indeed,

> In our discussions, the word *model* almost never occurred. . . . His total lack of interest in a model-theoretic approach quite astounded me. Thus, when I mentioned to him my discovery of the minimal model also found by John Shepherdson, he indicated that this was clear and, indirectly, that he knew of it. However, he did not mention the implication that no purely inner model could be found. Given that I also believe he was strongly wedded to the syntactical approach, this would have been of great interest.

This hints at an interesting diagnosis of Gödel's failure to prove the independence results he wanted.

Last, but quite certainly not least, we come to a paper by Hugh Woodin on 'The transfinite universe'. This is a further contribution to his campaign against 'the Skeptic' who says that the continuum hypothesis is neither true nor false, For those who can't already tell their Reinhardt cardinals from their supercompacts, this will be far too breathless a tour at too stratospheric a level to be at all accessible. But for experts, it is another exploration of the evidence that should be a way of constructing an inner model, Ultimate L, for almost all known large cardinals. This model – to make the connection with the theme of the volume – would have properties similar to Gödel's constructible universe L (and the axiom V = Ultimate L would settle the Continuum Hypothesis). Set-theory enthusiasts will of course want to read this paper.

A mixed bag indeed, then. On balance, your university library should have a copy. But if it doesn't, then you will find a significant number of the more substantial papers online.