Kurt Gödel, *On Formally Undecidable Propositions of Principia Mathematica and Related Systems.* Translated by B. Meltzer, with an introduction by R. B. Braithwaite, New York, Dover Publications, 1992; viii + 72 pp.; ISBN 0-486-66980-7; $4.95.

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There is no way around calling [Gödel *1931*] "Über formal unentscheidbare Sätze der *Principia Mathematica* und verwandter Systeme I", a classic — not only of logic alone, but of the entire intellectual enterprise of mankind. This paper revolutionized logic in the thirties, so that, to give just one nonstandard example, the smartest mathematical mind of those days, John von Neumann, lost the courage to continue his foundational research — Gödel eliminated him from the field (*cf.* [Köhler *1995*, ch. 4.3]). It inspired logical research for decades: "It is appropriate to remark that Gödel's paper was exceptionally rich in new ideas and that only now, after more than 30 years, the wealth of problems stemming directly from it begins to show signs of exhaustions" ([Mostowski *1966*, 23]); and still today it has an effect on logic as well as on theoretical computer science. One need think only of provability logic (*cf.* [Boolos *1993*] or [Smoryński *1985*]) or length-of-proof considerations (*cf.* [Leitsch *1995*]). A mathematical proof can be great for two reasons: because of the result, or because of the proof-technique(s) involved — Gödel's paper surely meets both demands beyond measure. In addition, its two main theorems, the incompleteness, and the 'unprovability of consistency' theorems, entered (not only analytic) philosophy via the most successful philosophical undertaking of this century, logical empiricism, and they are now reaching a common cultural sediment (*cf.*, e.g., [Hofstadter *1979*] and his successors), especially of the learned and/or the computerized world of Western-technological calibre. The paradox is that the technical apparatus of [Gödel *1931*] has helped to promote a cultural paradigm underlying the whole computer stuff current dreams and societies (information superhighway!) are made of, even as its very results call the same into question. The disillusionment following upon this extravagant praise is that the booklet to be reviewed here seems to

* Orrin Summerell was so kind as to improve the English of a preliminary version.

be the only existing self contained edition of [Gödel *1931*] available. It is an unabridged and unaltered reprint of the edition published in 1962 by Basic Books of New York. It consists of a preface by Meltzer (2 pp.), an introduction by Braithwaite (32 pp.), a list of abbreviations (2 pp.) and a translation of [Gödel *1931*] (35 pp.). Of interest here are only the translation and introduction.

The Translation by Meltzer. What this reviewer refrained from doing was cross-checking the whole translation by Meltzer ('$\mathfrak{M}$' for short) with the original German text or with the translation by van Heijenoort ('$\mathfrak{H}$' for short) as presented in the *Collected Works* edition (*cf.* [Gödel *1986*, 144–195). What he did do was to read $\mathfrak{M}$ once and to compare it only when something in $\mathfrak{M}$ took him aback.

On the whole $\mathfrak{M}$ was found to be a very readable translation; sometimes $\mathfrak{M}$ seems even superior to H, as far as legibility is concerned. Nevertheless, the reviewer would like to mention at least six partly flagrant mistakes or misprints he would like to see corrected (where "p.*n*,*m* b(t)" means "line *m*, counted from the bottom (top) on page n"):

- p. 40, 9*b*: „n“ should be „q“ (this flaw is in the original text),
- *p*. 44, 8*t*: read „from either the two,“
- *p*. 53, 1*t*: „O“ (capital ‚O‘) should be „0“ (zero),
- *p*. 54, 13*t*: „x“ should be „z,“
- *p*. 59, 5*t*: the antecedent „xB_C (17 Gen *r*)“ should occur negated,
- *p*. 70, 6*b*: the quantifier „ (*x*)“should not be negated.

For a more fault-finding listing, *cf.* [Bauer-Mengelberg *1965*] (not containing probably the most serious flaw on p. 59!).

Besides this, $\mathfrak{M}$'s terminology exhibits some pecularities worth mentioning, when it obviously does not arrive at the best solution possible (only additional material to [Bauer-Mengelberg *1965*] is mentioned):

– "Beweisfigur" becomes "proof-scheme", thus using a technical term, i.e., scheme, that to a large extent has already a fixed and different meaning in logical contexts — $\mathfrak{H}$ gives: 'proof-array' (the reviewer would prefer 'proof-figure,' the term chosen by Szabo when translating Gentzen's papers);

— "bestimmt"appears uniformly as "determinate," where one is well-advised to educe the nuances: 'specific', 'certain', 'peculiar' (as done in $\mathfrak{H}$);

— "Satzformel" is translated as "propositional formula," which seems misleading, since neither propositional calculus etc. nor interpreted expressions or the like should be touched on, for the word is simply an awkward way of denoting — back in times of even more diverse

terminologies — what today goes under the name "sentence," i.e., an expression without free variables — 𝔖: 'sentence formula';
— "inhaltlich" deserves special attention, a term prominent in the Hilbert school, for it is translated throughout as "interpreted as to content"; the best solution for preserving the specific technical meaning "inhaltlich"has in German, the writings of the Hilbert school resp., would have been to create a neologism — [Bauer-Mengelberg *1966*. 89] proposed "contentual"[1] — but Gödel himself was opposed to it (*cf.* [van Heijenoort *1967a*, 595]), and 'contentual' regrettably has not become a standard technical term like Kleene's neologism "finitary" for Hilbert's "finit," *cf.* [Kleene *1952*, 63]).

All this carping should blind no one to the fact that there is nothing in M that prevents the serious student from grasping Gödel's arguments or from esteeming this outstanding intellectual achievement. So in view of the booklet's price, its pocket size and its seeming robustness to bear some use, it forms — perhaps together with the corrections given above — a usable student edition of a classic. On the other hand, this can't be a recommendation. First, 𝔐 has, as indicated above, its manifest weaknesses, second, it isn't the one approved by Gödel himself like 𝔖.[2] third, it lacks the additions Gödel made later (*cf.* 𝔖: footnotes 13, 15, 43, and further on pp. 175, 179, 189, 194, 196 (original pagination given)), and finally, it does not give the addendum at the end of the text prepared for the reprint in [van Heijenoort *1967a*] (*cf.* [Gödel *1986*, 195]; see also the addendum for [Davis *1965*, 369–371]). Because we join people like Shepherdson [*1964*, 185], whose opinion was that any student of logic should study this most remarkable feat in the original, saying it "*ist doch die ursprüngliche Arbeit ein Muster von Klarheit und Strenge, das kaum zu überbieten ist und von jedem Studenten der Logik selbst gelesen werden sollte*," we urge the editorial team of Gödel's *Collected Works* to supply the learned (and unlearned but interested) world as soon as possible with an inexpensive pocket edition of the bilingual version of 𝔖 as presented in [Gödel *1986*] (especially since the only existing alternative is to buy a whole collection containing 𝔖, [Gödel *1931*] resp., like [Gödel *1986*] or [Shanker *1988*]).

The Introduction by Braithwaite. As apparent from its eight subtitles (metamathematics, Gödel's P, arithmetization, recursiveness, the unprovability theorem, consistency, the unprovability of consistency, syntactical character) the introduction focuses on section 2 (the incompleteness argument) and section 4 (the unprovability of consistency) of

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[1] Thanks to Orrin Summerell for informing me that "contentual" is not a neologism as Bauer-Mengelberg erroneously thought; *cf.* *The Oxford English Dictionary*, vol. 2, 1991, p. 818.

[2] Even worse, 𝔐 was prepared without even contacting Gödel, thus producing a correspondence filling no less than 3 containers in Gödel's estate.

the original paper, thus skipping Gödel's own introductory section 1 and leaving aside also section 3 (more on both later).

To explain beforehand: What Braithwaite B., for short) presents is in the reviewer's opinion the best medium-length introduction to Gödel's original argument available in English[3] (this holds especially in comparison to [Nagel & Newman *1959*], which has led many relying on this account to go astray). This is due to B.'s way of proceeding: He clings as close as he can square it with his didactical conscience to Gödel's original argument, thus trying to make it understandable instead of giving dubious simplifications or of giving one of the more elegant treatments worked out since then (*cf.* the various accounts given by Smoryński on various occasions) but which leave the reader alone again while studying Gödel's own argumentation. Now what is peculiar about Gödel's original presentation is his fear of meeting disapproval in a time when nearly everyone — except perhaps the Polish School (and of course the 'Brouwerians,' who knew it all before[4] — was laid low with syntactical, Hilbertian finitary resp., fever. This forced Gödel to shape at least his public reasoning syntactically as far as possible. So the especially crucial steps in sections 2 and 4 are taken solely in the realm of natural numbers and hence his formulation of incompleteness appears in these terms: the gödelnumber of "$x(\varphi x)$" nor the one of "$\neg\forall x(\varphi x)$", φx a certain Δ^0_0-expression with just x free, is an element of PR_K, i.e., the set of natural numbers being the gödelnumbers of all expressions provable in some ω-consistent recursive extension $\mathcal{K}$ of Peano Arithmetic. B. depicts Gödel's approach by distinguishing sharply instead, using a pleasant "inverted comma-notation" (*cf.* p. 6 for explanation), between a meaningful deductive system and a calculus void of any meaning (but of course capable of being interpreted) and develops Gödel's reasoning along this dichotomy.

Notwithstanding the praise, it is obvious that the introduction is obsolete whenever it makes one move more than depicting Gödel's original argument. There are several minor inaccuracies (perhaps pardonable in 1962, but no longer tolerable), partly historical, partly systematical. (Exercise: Find out what the reviewer was puzzled by on pp: 1, 11, 12, 25, 26, 29, 30.) Besides these minor points, there are three which deserve to be amplified, since here one finds misconceptions about the Gödel Theorems nearly ubiquitous outside the specialized literature.

(1) B. writes p. 19: "But, and here is the crux of the argument, **v** Gen **r**(**v**) *is the same as* **p** G(**p**)." This is false as stated, because **v** is according to B.'s conventions (p. 17) the expression with gödelnumber v; but only the values of terms involved are the same, making the corresponding

[3] Apparently Church held the same opinion; *cf.* [Church *1965*, 358]: "Addressed to 'philosophical logicians,' it is excellent for its purpose . . . ."

[4] *Cf.* for example [Wang *1987*, 88]: "Among other things he [Brouwer] said that he did not think G's incompleteness results are . . . important . . . , because to him G's results are obvious (obviously true)."

expressions not more than equivalent.[5] This muddling of equality between terms with equivalence between expressions happens frequently — not only

[5] To see this, one looks at what Gödel did (appropriately in current symbolism). Define outside the formal system a substitution function $sub(x, y, i)$: $\mathbb{N} \mapsto \mathbb{N}$, which calculates the gödelnumber for a formula φ, with $gn(\varphi) = x$ and *the* i^{th} variable $\mathbf{v_i}$ occurring free, in which all occurrences of the free variable $\mathbf{v_i}$ were substituted by the numeral $\overline{\mathbf{n}}$, for $n = y$; if x is no such gödelnumber, the input x remains unchanged:

$$sub(x, y, i) := \begin{cases} gn\left([\varphi(v_i)]\frac{\overline{n}}{v_i}\right) & \text{, if } x = gn(\varphi(v_i)),\ v_i \in free(\varphi), \text{ and } y = n \mapsto \overline{n} \\ x & \text{, otherwise} \end{cases}$$

Then

$$(*) \qquad sub(gn(\varphi(v_i), n, i) = gn(\varphi(\overline{\mathbf{n}}))$$

holds.

This function, since (primitive-) recursive, is representable, say, in Peano Arithmetic $\mathcal{PA}$, i.e., there is a formula "**sub(x, y, z)**" such that for all natural numbers $n, m, i, k,$ we have:

$$(**) \qquad \mathcal{PA} \vdash \mathsf{sub}(\overline{\mathsf{n}}, \overline{\mathsf{m}}, \overline{\mathsf{i}}) = \overline{\mathsf{k}} \text{ iff "}sub(x, y, z) = k\text{" is true.}$$

Let $\neg\mathsf{Pr}(\mathsf{v_i})$ be the negated provability predicate with $\mathsf{v_i}$ free. Now look at:

$$\text{(i)} \qquad \neg\mathsf{Pr}(\mathsf{sub}(\mathsf{v_i}, \mathsf{v_i}, \overline{\mathsf{i}})),$$

the formula, where every occurrence of $\mathsf{v_i}$ in $\mathsf{Pr}(\mathsf{sub}(\mathsf{v_i})$ is substituted by $(\mathsf{sub}(\mathsf{v_i}, \mathsf{v_i}, \overline{\mathsf{i}}))$. Let its gödelnumber be p:

$$\text{(ii)} \qquad p := gn\,(\neg\mathsf{Pr}(\mathsf{sub}(\mathsf{v_i}, \mathsf{v_i}, \overline{\mathsf{i}})).$$

Now we obtain the desired fixpoint δ for $\neg\mathsf{Pr}(\mathsf{sub}(\mathsf{v_i})$ already by substituting in (i) the numeral $\overline{\mathsf{p}}$ for $\mathsf{v_i}$:

$$\text{(iii)} \qquad \delta :\equiv \neg\mathsf{Pr}(\mathsf{sub}(\overline{\mathsf{p}}, \overline{\mathsf{p}}, \overline{\mathsf{i}})).$$

To see this, first calculate (conveniently outside the formal system):

$$\begin{aligned} & sub(p, p, i) = sub(gn(\neg\mathsf{Pr}(\mathsf{sub}(\mathsf{v_i}, \mathsf{v_i}, \overline{\mathsf{i}})), p, i) && \text{; by definition of } p, (11) \\ \text{(iv)} \qquad & = gn(\neg\mathsf{Pr}(\mathsf{sub}(\overline{\mathsf{p}}, \overline{\mathsf{p}}, \overline{\mathsf{i}}))) && \text{; by definition of } sub, (*) \\ & = gn(\delta) && \text{; by definition of } \delta, \text{(iii).} \end{aligned}$$

in the 'popular' literature — and to set these things straight is important not only to get the proof right, but also to avoid widespread misunderstandings that are linked with an insufficient understanding of the fixpoint construction given.

(2) The second point is that many people seem to believe that to formally establish (*) "$\mathbf{CON} \rightarrow \delta$" as a formal theorem in order to conclude Gödel's second theorem G2: $\nvdash$ **CON**, from (one half of) the first G1: $\nvdash \delta$, means to give a formal derivation for this implication (*) from the axioms by logic alone, or, even worse, to formalize the whole G1-proof (*cf.* pp. 24–25: ". . . this is a lengthy and complicated business"). No one has ever done this! (Not even, contrary to another widespread myth, Bernays in [Hilbert & Bernays *1939*].) What one does instead is to establish a lemma from which (*) follows, so the whole G2-proof takes not more than approximately two pages.[6]

Hence, we get by (**):

(v) $$\mathcal{PA} \vdash \mathrm{sub}(\overline{\mathrm{p}}, \overline{\mathrm{p}}, \overline{\mathrm{i}}) = \ulcorner \delta \urcorner$$

(where $\ulcorner \delta \urcorner$ denotes the numeral inside the system corresponding to $gn(\delta)$ outside the system). And therefore:

(vi)
$$\begin{array}{lll} \mathcal{PA} \vdash \neg\mathrm{Pr}(\mathrm{v_i}) \leftrightarrow \neg\mathrm{Pr}(\mathrm{v_i}) & & \text{; by logic} \\ \vdash \neg\mathrm{Pr}(\mathrm{sub}(\overline{\mathrm{p}}, \overline{\mathrm{p}}, \overline{\mathrm{i}})) \leftrightarrow \neg\mathrm{Pr}(\ulcorner \delta \urcorner) & & \text{; by line (v)} \\ \vdash \delta \leftrightarrow \neg\mathrm{Pr}(\ulcorner \delta \urcorner) & & \text{; by definition of } \delta \text{, (iii)} \end{array}$$

The final equivalence "$\delta \leftrightarrow \neg\mathrm{Pr}(\ulcorner \delta \urcorner)$"is what B. refers to as "**v** Gen **r**(**v**) *is the same as* **p** G(**p**)." The lesson is what one really has is equality between terms, *cf.* (iv) and (v) resp., resulting in expressions equivalent only, see (vi). (**NB**: If one abbreviates $\neg\mathrm{Pr}(\mathrm{sub}(\overline{\mathrm{p}}, \overline{\mathrm{p}}, \overline{\mathrm{i}}))$ to χ, the fixpoint χ becomes $\chi(\ulcorner \chi \urcorner)$, which makes more evident the name 'diagonalization' for the construction given.

[6]The most convenient way known to the reviewer is to prove derivable Σ^0_1-completeness as the crucial lemma, i.e., that for any arithmetical sentence φ with at most one existential quantifier in prenex normal form holds:

(i) $$\vdash \varphi \leftrightarrow \mathrm{Pr}(\ulcorner \varphi \urcorner).$$

A proof sketch of (i) — as ever with qualifications like this: a well-versed reader in logical techniques assumed — takes not more than one-and-a-half pages in a concise textbook, *cf.*, e.g., [Shoenfield *1967*, 212*f*]. Then one shows, by a slight modification of the fixpoint construction given above, that there is a (Jeroslov-) fixpoint:

$$\vdash \delta \leftrightarrow \neg\mathrm{Pr}(\ulcorner \neg\delta \urcorner);$$

(3) What Gödel's second theorem establishes is the underivability of one special arithmetical sentence **CON**, which can be regarded as saying in the standard model of arithmetic that the formal system in question is consistent. But to conclude from this, as is usually done, that all propositions from which consistency would follow are underivable in the formal system itself, and thus no consistency proof can take place within the system itself, or that no such finitary proof exists for systems including Peano Arithmetic resp., is a highly delicate matter and controversial even now. True, the community of (mathematical) logicians has talked itself into

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and further, by routine arguments, that this δ, too, is formally undecidable, i.e., $\nvdash \delta$ and $\nvdash \neg\delta$. This takes another half page. Then conclude:

$\vdash \delta \rightarrow \neg\mathrm{Pr}(\ulcorner \neg\delta \urcorner)$	; by (ii)
$\vdash \delta \rightarrow \neg\mathrm{Pr}(\ulcorner \delta \urcorner)$	; by (i), since $\delta \in \Sigma_1^0$ by construction
$\vdash \delta \rightarrow \neg\mathrm{Pr}(\ulcorner \delta \urcorner) \wedge \mathrm{Pr}(\ulcorner \neg\delta \urcorner)$	; by $\wedge$-introduction
$\vdash \neg(\mathrm{Pr}(\ulcorner \delta \urcorner) \wedge \mathrm{Pr}(\ulcorner \neg\delta \urcorner)) \rightarrow \neg\delta$	; by contraposition
$\vdash$ **CON** $\rightarrow \neg\delta$	; by defintion of **CON**
$\nvdash$ **CON**	; because $\nvdash \neg\delta$.

The next-to-last line is the counterpart to B.'s "**w** imp **p** G(**p**)," thus provable in a little more than two pages. If one takes the fixpoint theorem and derivable Σ_1^0-completeness as belonging to a logical all-round education, which should be true at least for mathematical logicians, the whole proof of Gödel's second theorem consists in the last six easy deduction steps — G2 as a mate in six! (**NB**: The snag is that the proof for derivable Σ_1^0-completeness expands easily to 20–50 (or even more) pages, depending on how detailed it should be, as soon as one really starts filling in all the details necessary. Since this is comparable to the efforts connected to introducing arithmetization and representation — the essentials for G1–G2 is not more, but also not less difficult to prove than G1. But even though the proofs of G1 and G2 are of comparable difficulty — and both demand a great deal of anybody trying to do a thorough job — the myth of the greater difficulty of the G2-proof might result from the fact, that, whereas there are quite a few treatises dealing with arithmetization and representation fairly detailed, one doesn't find an equally detailed treatment for derivable Σ_1^0-completeness in print, unless one reads either German or Italian and wants to see it done for Hilbert/Bernays' 'fossils', $\mathfrak{Z}$, $\mathfrak{Z}_\mu$ resp. (*cf.* [Hilbert/Bernays *1939*, 283–324; 2nd ed., *1970*, 293–337] or [Galvin *1983*, 581–613]). A partial proof, skipping the most cumbersome derivations, especially the one for the so-called second derivability condition, written in English and done within Peano Arithmetic $\mathfrak{PA}$, is now included in [Boolos *1993*, 46–49], and a fully detailed proof, done by this reviewer, will be available soon (for a preliminary account see [Buldt *1995*]).

accepting the above mentioned standard view — for which there is of course some evidence beyond the mere second theorem alone — but maybe one is willing to learn from the 'master' himself, who was much more cautious in deciding such fundamental questions fraught with consequences.[7] Anyway, in spite of this situation B. closes the corresponding section with statements like (p. 26): ". . . and it is now certain that, within any formal system . . . capable of expressing arithmetic, it is impossible to establish its own 'consistency'," thus creating a wrong impression by asserting a definiteness that does not exist.

Finally we owe it to the reader to turn to sections 1 and 3 of [Gödel *1931*], which B. inappropriately ignores in his introduction. First, section 1, Gödel's own introduction, has caused many misunderstandings — prominent for instance is the exchange with Zermelo (in essence) on this matter (*cf.* [Dawson *1985*] and the literature cited there) — an introduction should guide the unprepared reader through the shoals this section doubtless contains ([van Heijenoort *1967b*] seems to be an example for how to do such things well). Second, section 3, containing, among others, reflections on the decision problem and arriving nearly at Church's theorem on its unsolvability, deserves mention and explanation. Third, there are really dark passages in Gödel's paper — notorious in this context is footnote 48a — that should be tackled in an ambitious introduction. (But, all introductions with which the reviewer is familiar avoid these points.)

Conclusion. Since every serious student (not only) of logic will be eager to study Gödel's stroke of genius at least once, this is the edition to carry in one's pocket for half a year or however long it may take to get acquainted with all the details. An excellent first tour guide will be B.'s introduction, but the student will soon learn never to trust too much the introduction or the translation.

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[7]Gödel himself waited over twenty years before stating in print that Hilbert's original program(me) is no longer tenable. But even then, in his usual cautious manner æ see [Feferman *1984*] —he pinned the blame on Bernays for this conclusion from his second theorem, *cf.* [Gödel *1958*, 240]. (Contrarily, in lectures, Gödel noted quite early the failure of Hilbert's original program(me), *cf.* [Gödel *1933*, 52] and [Gödel *1938*, 88–89, 122–123].) Church too, in his [*1965*, 359], blames B. for following ". . . a very widespread opinion without giving a sufficient basis for this." In this context [Detlefsen *1986*] may be regarded as playing a similar rôle as Berkeley did concerning the foundations of the calculus: They rightly criticize(d) conceptual defects of a practise that is only 'pragmatically' justified. (See also [Buldt *1993*, esp. 221–226].))

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