WHAT DID GÖDEL BELIEVE AND WHEN DID HE BELIEVE IT?

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Gödel has emphasized the important role that his philosophical views had played in his discoveries. Thus, in a letter to Hao Wang of December 7, 1967, explaining why Skolem and others had not obtained the completeness theorem for predicate calculus, Gödel wrote:

> This blindness (or prejudice, or whatever you may call it) of logicians is indeed surprising. But I think the explanation is not hard to find. It lies in a widespread lack, at that time, of the required epistemological attitude toward metamathematics and toward non-finitary reasoning.
> . . .
> I may add that my objectivist conception of mathematics and metamathematics in general, and of transfinite reasoning in particular, was fundamental also to my other work in logic.
> How indeed could one think of *expressing* metamathematics *in* the mathematical systems themselves, if the latter are considered to consist of meaningless symbols which acquire some substitute of meaning only *through* metamathematics?
> Or how could one give a consistency proof for the continuum hypothesis by means of my transfinite model Δ if consistency proofs have to be finitary?[1]

In a similar vein, Gödel has maintained that the "realist" or "Platonist" position regarding sets and the transfinite with which he is identified was part of his belief system from his student days. This can be seen in Gödel's replies to the detailed questionnaire prepared by Burke Grandjean in 1974. Gödel prepared three tentative mutually consistent replies, but sent none of them. One of the questions was as follows:

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Along with all who study Gödel's thought, I owe an immense debt to Sol Feferman and his collaborators for their great accomplishment, the spectacular five volumes of Gödel's ***Collected Works***. I am also grateful for very useful criticisms of an earlier draft of this article by Akihiro Kanamori.

[1] [6], pp. 397–8

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> Your philosophical leanings have been described by some as 'mathematical realism' whereby mathematical sets and theorems are regarded as describing objects of some kind.
> (a) How accurate is this characterization?
> (b) In particular, how well does it describe your point of view in the 1920's and early 1930's, as compared with your later position?

Gödel replied to (a) saying that this was "correct." To (b) he asserted: "Was my position since 1925."[2] It should not be a surprise that a more nuanced view of Gödel's developing ideas and beliefs reveals a more complex picture. In his momentous investigations on incompleteness and on the continuum hypothesis, he was entering essentially virgin territory, bringing conceptual understanding to bear and developing the technical tools he needed. Surely when he reflected on what he had done and how he had done it, it was inevitable that his philosophical views would be affected. In this article, I will exhibit some of the evidence for such changes, in particular with respect to Gödel's view of Hilbert's Program and of his attitude concerning a realist stance towards sets.

§1. Gödel's changing attitude toward Hilbert's Program. In the little textbook published by Hilbert and his student Ackermann in 1928 [7], the problem of the completeness of predicate calculus was stated as an open problem. The young Gödel, presumably as unaware as the authors of the relevance of Skolem's work, chose this problem as the subject of his doctoral dissertation. In his introductory section, Gödel discusses at length and seeks to justify the non-constructive methods he used in the proof. This discussion concludes as follows:

> Finally, we must also consider that it was not the controversy regarding the foundations of mathematics that caused the problem treated here to surface (as was the case, for example, for the problem of consistency of mathematics); rather, even if it had never been questioned that 'naive' mathematics is correct as to its content, this problem could have been meaningfully posed within this naive mathematics (unlike, for example, the problem of consistency), which is why a restriction on the means of proof does not seem to be more pressing here than for any other mathematical problem.[3]

This clearly suggests that in 1929 Gödel saw Hilbert's program to prove the consistency of formalizations of classical mathematics by finitary means as perfectly reasonable. Further evidence suggests that, indeed, he set out to contribute to that program. In a draft for an unsent letter to Yossef Balas,[4]

[2][5], pp. 446–450.
[3][2], p. 65.
[4][5], pp. 9–11. The draft is a reply to a letter from Balas dated May 27, 1970.

Gödel explains that it was his attempt to supply a relative consistency proof of second order arithmetic (which he called "analysis") in first order arithmetic that led him to his incompleteness theorem.

> For an arithmetic model of analysis is nothing else but an arithmetical $\in$-relation satisfying the comprehension axiom:
>
> $$(\exists n)(x)\left[x \in n \equiv \phi(x)\right]$$
>
> Now if in the latter "$\phi(x)$" is replaced by "$\phi(x)$ is provable", such an $\in$-relation can easily be defined. However (and this is the decisive point) it follows from the correct solution of the semantic paradoxes that the "truth" of the propositions of a language *cannot be expressed* in the same language, while provability (being an arithmetical relation) *can*. Hence true $\not\equiv$ provable.

In the historical context in which Gödel had sought such a relative consistency proof, it was thought that the work of Ackermann and of von Neumann on the consistency of first order arithmetic was well on the road to yielding that conclusion. Thus Gödel could well imagine that had he attained his goal, it would have advanced Hilbert's program considerably.

In conversations with Hao Wang in 1976, Gödel spoke in a similar vein:

> In summer of 1930 I began to study the problem of the consistency of classical analysis. It is mysterious why Hilbert wanted to prove directly the consistency of analysis by finitary methods. I saw two distinguishable problems: to prove the consistency of number theory by finitary number theory and to prove the consistency of analysis by number theory. . . . I began by tackling the second half: to prove the consistency of analysis relative to full number theory.[5]

Of course, contrary to what Gödel had anticipated, the result of his investigation, especially his second incompleteness theorem (the unprovability of consistency) dealt a devastating blow to Hilbert's program. Nevertheless, in his famous paper of 1931 in which the incompleteness theorems were presented to the world, Gödel saw fit to comment:

> It is particularly to be remarked that [the second incompleteness theorem] do[es] not contradict Hilbert's formalistic point of view. For this viewpoint presupposes[6] only the existence of a consistency proof in which nothing but finitary means of proof is used, and it is conceivable that there exist finitary proofs that *cannot* be expressed in the formalism[s] [to which the incompleteness theorems apply].[7]

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[5] [11], p. 82.
[6] the original German was "setzt voraus".
[7] [2], p. 195.

But only two years later in a remarkable address in Cambridge, Massachusetts in 1933 on the state of research into the foundations of mathematics, Gödel spoke quite differently:

> But unfortunately the hope of succeeding [in obtaining the desired finitary consistency proofs] has vanished entirely. ... all the [finitary] proofs ... that have ever been constructed can easily be expressed in the system of classical analysis and even in the system of classical arithmetic, and there are reasons for believing that this will hold true for any proof which one will ever be able to construct.[8]

In January 1938, at the request of Edgar Zilsel, Gödel gave a seminar address in Vienna on various possible ways to extend Hilbert's strict finitary viewpoint so as to obtain the desired consistency proofs. In this address he showed both respect for what Hilbert had been trying to do, and great interest in the endeavors to at least partially overcome the limitations that his own work had uncovered:

> If the original Hilbert program could have been carried out, that would have been without any doubt of enormous epistemological value. ...
>
> As to the proofs by means of the extended finitism ... it seems to me [that their] mathematical significance is extraordinarily great, and I am convinced that the methods applied here will lead to very interesting results in foundational research and also outside it.[9]

At a conference in Zurich in 1938, Paul Bernays spoke about Hilbert's proof theory to a certain extent paralleling Gödel's own remarks at Zilsel's seminar regarding extensions of Hilbert's strict finitism that could lead to appropriate consistency proofs. The proceedings of the conference were published in 1941. In a letter to Bernays sent in January 1942, Gödel reacted to a few lines in the concluding portion of Bernays's talk with evident shock:

> I read your article ... with great interest; only what you say ... is not comprehensible to me. Wouldn't that be tantamount to giving up the formalist standpoint?[10]

The passage in Bernays's presentation to which Gödel referred was as follows:

> ... it is not necessary to understand evidence and certainty in too absolute a manner if one wishes to leave open the possibility

[8] [4], p. 52.

[9] [4], p. 113. It should be remarked that what was available in Gödel's *Nachlass* was his own notes for the lecture in Gabelsberger shorthand. Preparing it in a form accessible to readers was a major undertaking. After Cheryl Dawson had transcribed the shorthand, the notes were meticulously edited by Charles Parsons, Wilfried Sieg, and her. Wilfried Sieg and Charles Parsons also collaborated in an excellent very informative introductory essay.

[10] [5], p. 133.

> of enlarging the methodological limits. Moreover by proceeding thus, one secures the advantage of not being obliged to declare the methods of traditional analysis to be illegitimate or dubious.[11]

Bernays replied to Gödel's question saying that his astonishment "is very understandable" given the brevity of his comment. He explained that what he had in mind was that it was unnecessary to disparage certain methods as dubious as long as one "resolves to distinguish different layers and kinds of evidence".[12] What one can take away from the exchange is the importance that Gödel still attached to Hilbert's "formalist standpoint" in 1941.

In December 1951, having been invited to give the annual Gibbs lecture, Gödel presented an address to the American Mathematical Society, entitled *Some Basic Theorems on the Foundations of Mathematics and their Implications*. It is noteworthy that although the unprovability of consistency plays a key role in the lecture, Hilbert's program is ignored. This contrasts sharply with his discussion of similar matters in 1933.[13] We shall have more to say about this remarkable essay in the next section.

In notes, apparently prepared in 1961 for a lecture Gödel thought to deliver before the American Philosophical Society to which he had recently been elected, Gödel proposed a scheme for classifying possible philosophical world views (*Weltanschauungen*) on a continuum running from "right" (metaphysics, religion) to "left" (skepticism, positivism). Gödel sees Hilbert's program as somehow trying to bridge the "left" and "right" aspects of mathematics and dismisses it as "that strange hybrid that Hilbert's formalism represents".[14] Thus, over the years Gödel moved from an initial position of allying himself with Hilbert's program, to holding out hope that his own work had not destroyed it, to realizing with some regret that hope was gone, to ultimately speaking of the project with something like disdain.

§2. Gödel's Platonism. In Gödel's 1933 address in Cambridge already mentioned, he divided the problem of the foundations of mathematics into two parts: establishing the axioms and then justifying them:

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[11] [1], p. 152. The translation is mine. The original is as follows: " … *il ne faut pas concevoir l'évidence et la sûreté de façon trop absolue, si l'on veut conserver ouverte la possibilité d'élargir le cadre méthodique. D'autre part, en procédent ainsi, on s'assure l'avantage de ne pas être obligé de déclarer illégitimes ou douteuses les méthodes traditionelles de l'analyse*"

[12] [5], p. 139.

[13] [4], pp. 303–323. The closest Gödel comes to referring to Hilbert's program is in his footnote 23, in which he remarks that the "nominalistic" view of mathematics, which the main text is in the process of refuting, is closely related to "the formalistic program".

[14] [4], p. 379. The translation there provided for the phrase "*merkwürdige Zwitterding*" is "curious hermaphroditic thing"; I have ventured to suggest that "strange hybrid" might be closer to Gödel's intention.

> I come now to the second part of our problem, namely, the problem of giving a justification for our axioms and rules of inference, and as to this question it must be said that the situation is extremely unsatisfactory. Our formalism works perfectly well and is perfectly unobjectionable as long as we consider it as a mere game with symbols, but as soon as we come to attach a meaning to our symbols serious difficulties arise. . . .
>
> The result of our previous discussion is that our axioms, if interpreted as meaningful statements, necessarily presuppose a kind of Platonism, which cannot satisfy any critical mind and which does not even produce the conviction that they are consistent.[15]

How can we reconcile this clear disavowal of the kind of Platonism that regards sets as real objects with an objective existence, with Gödel's assertion that he had held precisely that view since 1925? However one wishes to understand the statement about his earlier views, it seems clear that in 1933 Gödel's beliefs were quite different from those of his later years.

Much of the information that casts doubt on the uniformity of Gödel's metaphysical stance only came to light after his death through documents found in his *Nachlass*. However there was one clear signal in his first published announcement of his work on the Continuum Hypothesis. To recapitulate the situation:

Cantor's Continuum Hypothesis is the assertion: *Every infinite set of real numbers is either countable or has the cardinality of the continuum.* Writing $D[S]$ for the collection of all subsets of S that are definable in the language of set theory with parameters from S, Gödel's hierarchy of *constructible sets* is defined as follows:

$$\begin{aligned} L_0 &= \emptyset, \\ L_{\alpha+1} &= D[L_\alpha], \\ L_\lambda &= \bigcup_{\alpha<\lambda} L_\alpha \quad (\lambda \text{ a limit ordinal}). \end{aligned}$$

S is *constructible* if for some α, $S \in L_\alpha$.

Gödel used the letter "A" to stand for the statement:

Every set is constructible.

He was able to prove that A is consistent with the Zermelo-Fraenkel axioms and that it implies both the axiom of choice and the continuum hypothesis[16] so that these are also consistent with those axioms.

In an abstract announcing these results in 1938, Gödel concluded:

[15] [4], pp. 49–50.

[16] In fact, Gödel proved that A even implies that $2^{\aleph_\alpha} = \aleph_{\alpha+1}$ (the so-called "generalized continuum hypothesis").

> The proposition A added as a new axiom seems to give a natural completion of the axioms of set theory, in so far as it determines the vague notion of an arbitrary infinite set in a definite way. In this connection it is important that the consistency proof for A ... seems to be absolute in some sense ... [17]

This passage is clearly at odds with some of Gödel's later utterances. Here he suggests that there is something "vague" about the "notion of an arbitrary infinite set". Nine years later, in an expository article on the continuum problem, he would write:

> This concept of set ... according to which a set is anything obtainable from the integers (or some other well-defined objects) by iterated application of the operation "set of", and not something obtained by dividing the totality of all existing things into two categories, has never led to any antinomy whatsoever; ... [18]

In this same article, instead of suggesting that A might be accepted as an axiom, Gödel strongly suggests that he regards it as false:

> ... there are two quite differently defined classes of objects which both satisfy all the axioms of set theory that have been written down so far. One class consists of [the constructible sets], the other of the sets in the sense of arbitrary multitudes ... Now, before it is settled what objects are to be numbered, ... one can hardly expect to be able to determine their number ... [19]

Finally, noting the prospect that the negation of the continuum hypothesis might well also be shown to be consistent with the axioms of set theory (which indeed it was by Paul Cohen in 1963), Gödel wrote:

> ... even if one should succeed in proving [the independence of the continuum hypothesis], this would ... by no means settle the question definitively. Only someone ... who denies that the concepts and axioms of classical set theory have any meaning (or any well-defined meaning) could be satisfied with such a solution, not someone who believes them to describe some well-determined reality. For in this reality Cantor's conjecture must be either true or false, and its undecidability from the axioms as known today can only mean that these axioms do not contain a complete description of this reality ...
>
> Therefore one may on good reason suspect that the role of the continuum problem in set theory will be this, that it will finally lead

[17] [3], p. 27.
[18] [3], p. 180.
[19] [3], p. 183.

> to the discovery of new axioms which will make it possible to disprove Cantor's conjecture.[20]

We may well contrast this with what Gödel had to say in an address at Göttingen in 1939 not long after his work on the consistency of the continuum hypothesis.

> Finally, the consistency of the proposition A (that every set is constructible) is also of interest in its own right, especially because it is very plausible that with A one is dealing with an absolutely undecidable proposition, on which set theory bifurcates into two different systems, similar to Euclidean and non-Euclidean geometry. ... I am fully convinced that the assumption that nonconstructible sets exist is also consistent. A proof of that would perhaps furnish the key to the proof of the independence of the continuum hypothesis ... That would then yield the definitive result that one must really be content with a proof of the consistency of the continuum hypothesis, because then what would have been shown is exactly that a proof of the proposition itself does not exist.[21]

The occurrence of the term "absolutely undecidable proposition" in this passage resonates with Gödel's use of the word "absolute" in his 1938 abstract. In undated notes for a lecture apparently never given,[22] he formulated Hilbert's belief in the solvability of every problem as:

Given an arbitrary mathematical proposition α,
there exists a proof of α or a proof of $\neg\alpha$.

Gödel noted that if the axioms and rules of inference are made precise, this becomes a proposition capable of proof or disproof. Moreover he explained that if these axioms and rules satisfy some simple requirements, one can even find arithmetic propositions of the form:

$$(\forall x_1, \dots, x_n)(\exists y_1, \dots, y_m)\big[p(x_1, \dots, x_n, y_1, \dots, y_m) = 0\big]$$

where p is a polynomial with integer coefficients that can neither be proved nor disproved from the given setup. He continued:

> But it is clear that this negative answer may have two different meanings:
>
> 1. it may mean that the problem in its original formulation has a negative answer, or
> 2. it may mean that through the transition from evidence to formalism something was lost.

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[20] [3], pp. 181, 186.

[21] [4], p. 155. Gödel's belief at the time that his axiom A is undecidable in some absolute sense is discussed from a somewhat different point of view in [10].

[22] [4], pp. 164–175.

> It is easily seen that the second is actually the case, since the number-theoretic questions which are undecidable are always decidable by evident inferences not expressible in the given formalism.

Gödel concluded:

> So the belief in the decidability of every mathematical question is not shaken by this result. ... However, I would not leave it unmentioned that apparently there do exist questions of a very similar structure which very likely are really undecidable in the sense which I explained first. The difference in the structure of these problems is only that also variables for real numbers appear in this polynomial. Questions connected with Cantor's continuum hypothesis lead to problems of this type. So far I have not been able to prove their undecidability, but there are considerations which make it highly plausible that they really are undecidable.

Although no specific statement is singled out here as a candidate for being an absolutely undecidable proposition expressible in a simple manner in terms of a polynomial equation, Gödel does suggest such a proposition in an address given at Brown University in 1940. It is a weakened form of the proposition A asserting that all sets are constructible conjectured to be absolutely undecidable in the Göttingen address, specifically the statement that *every real number is constructible*, which we'll designate by $\mathring{A}$. Indeed for the proof of the the consistency of the continuum hypothesis, it suffices to use $\mathring{A}$, the full hypothesis that every set is constructible not being needed.[23]

> ... this consistency proof for the continuum hypothesis and for the proposition $\mathring{A}$ is in a sense absolute, i.e., independent of the particular formal system which we choose for mathematics. ... my consistency proof goes through for systems of arbitrarily high type. ... This, so to speak, absolute consistency of $\mathring{A}$ is very interesting from the following point of view. It is to be expected that also $\sim\mathring{A}$ will be consistent with the axioms of mathematics ... So $\mathring{A}$ is very likely a really undecidable proposition ... This conjectured undecidability of $\mathring{A}$ becomes particularly surprising if you investigate the structure of $\mathring{A}$ in more detail. It then turns out that $\mathring{A}$ is equivalent to a proposition of the following form:
>
> $$(P)\big[F(x_1, \ldots, x_k, n_1, \ldots, n_\ell) = 0\big]$$
>
> where F is a polynomial with given integer coefficients and with two kinds of variables x_i, n_i, where the x_i are variables for real numbers

[23] Although it is needed for the proof of the consistency of the generalized continuum hypothesis.

> and the n_i variables for integers, and where P is ... a sequence of quantifiers composed of these variables x_i and n_i.[24]

Once again there is reference to a polynomial equation with two types of variables. However instead of the sharp $\forall\exists$, all that is claimed about the form of $\mathring{A}$ is that the prefix consists of "a sequence of quantifiers". Now, by Shoenfield's well-known absoluteness theorem[25] it is clear that no such $\forall\exists$ representation is possible for $\mathring{A}$. There are two possibilities:

1. Gödel was thinking of some proposition other than $\mathring{A}$ that is also "connected with Cantor's continuum hypothesis" that really did have that $\forall\exists$ form and which he thought could be an example of an absolutely undecidable statement.
2. He did have $\mathring{A}$ in mind all along, but erred in working out the quantificational prefix in its representation in the polynomial form.

There is no way to be sure, but I lean very much to the second alternative. Those handwritten notes for a proposed lecture in which the $\forall\exists$ prefix was claimed were found in a spiral notebook in Gödel's *Nachlass*. As explained in the introductory note preceding the text of the notes, although "the lecture was well thought out ... in some ways it was still a rough draft".[26] In the context, citing the same $\forall\exists$ prefix followed by a polynomial equation for the merely relatively undecidable proposition as well as the statement conjectured to be absolutely undecidable (the pair differing only in the ranges of the variables) would have added an attractive elegance to the lecture. As Yiannis Moschovakis has remarked (in an email message) errors in computing the correct quantifier prefix are easy to make. But the best reason to believe that Gödel had no proposition other than $\mathring{A}$ in mind is that otherwise, in his Brown lecture in which he gave a correct representation of $\mathring{A}$ with no claims about the form of the prefix, he surely would have mentioned it.

We have seen the contrast between Gödel's beliefs in 1938 and in 1947. The newer point of view is already apparent in Gödel's essay on Bertrand Russell's contributions to mathematical logic, published in 1944:

> Classes ... may ... be conceived as real objects ... existing independently of our definitions and constructions. It seems to me that the assumption of such objects is quite as legitimate as the

[24] [4], pp. 184–85. Because it is clear that it is the weaker assumption that Gödel is talking about in this passage, I've replaced his use of "A" by "$\mathring{A}$".

[25] [8]; of course it is very unlikely that Gödel would have known this theorem at the time.

[26] [4], p. 156. This introductory note (which, in fact, I wrote) continues: "For example, there is some ambiguity about whether the 'integers' referred to were to be understood as meaning the positive integers or whether 0 was to be included as well."

> assumption of physical bodies and there is quite as much reason to believe in their existence.[27]

In the 1951 Gibbs lecture already mentioned, Gödel proposed that the incompleteness theorems furnished strong evidence for an idealist philosophical stance. These theorems imply the disjunction:

> Either mathematics is incompletable in [the] sense ... [that] the human mind (even within the realm of pure mathematics) infinitely surpasses the power of any finite machine, or else there exist absolutely undecidable diophantine problems ...

Corresponding to this disjunction Gödel insisted was a philosophical disjunction, either term of which is "decidedly opposed to materialistic philosophy". He explained:

> Namely, if the first alternative holds, this seems to imply that the working of the human mind cannot be reduced to the working of the brain ... On the other hand, the second alternative ... seems to disprove the view that mathematics is only our own creation; ... So [it] seems to imply that mathematical objects ... exist objectively ... that is to say [it seems to imply] some form or other of Platonism or "realism" as to the mathematical objects.[28]

In a letter to Gottard Günther dated June 30, 1954, Gödel is far less tentative:

> When I say that one can (or should) develop a theory of classes as objectively existing entities, I do indeed mean by that existence in the sense of ontological metaphysics, by which, however, I do not want to say that abstract objects are present in nature. They seem rather to form a second plane [*Ebene*] of reality, which confronts us just as objectively and independently of our thinking as nature.[29]

In a final contrast to Gödel's 1933 dismissal of set-theoretic Platonism as unable to "satisfy any critical mind" what he said in 1975 in a letter to Bernays might be noted:

> I'm pleased that ... you advocate a cautiously [*vorsichtig*] Platonistic point of view. To me a Platonism of this kind (also with respect to mathematical concepts) seems to be obvious and its rejection to border on feeble-mindedness [*an Schwachsinn zu grenzen*].[30]

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[27] [3], p. 128.
[28] [4], pp. 310–12.
[29] [5], pp. 503, 505.
[30] [5], p. 309.

§3. Gödelian empiricism — A road not taken. We have had occasion to refer to Gödel's notes of 1961 for a possible lecture before the American Philosophical Society in which he classified philosophical world views on a scale from left to right. In this manuscript he clearly identifies his position as being on the "right" and recommends Husserl's phenomenology as a fruitful direction. In [9] the authors incisively examine Gödel's struggle to accommodate his rationalism with the quite apparent objectivity of mathematics, a struggle which led him eventually to Husserl's transcendental idealism. They quote Husserl as complaining that "German Idealism has always made me want to throw up".[31] Despite this, Husserl came to appreciate and to incorporate aspects of German Idealism in his own philosophy. He relies on the abstract notion of "consciousness" as the foundation of his ontology. For Husserl the sense in which other abstract objects exist is precisely their being conceivable by a real or possible consciousness.

Nowadays, neuroscientists seek to understand human consciousness as a direct manifestation of the human brain. This is a view that Gödel adamantly refused to accept. In his Gibbs address he referred to "the opinions of some of the leading men in brain and nerve physiology, who very decidedly deny the possibility of a purely mechanistic explanation of psychical and nervous processes".[32] In a letter seeking to comfort the dying Abraham Robinson, Gödel wrote: "The assertion that our ego consists of protein molecules seems to me one of the most ridiculous ever made."[33]

Yet Gödel himself had indicated a way to reconcile the objectivity of mathematics with an empiricist outlook — in his typology, a possible view from the left. In his Gibbs lecture, he said:

> If mathematics describes an objective world just like physics, there is no reason why inductive methods should not be applied in mathematics just the same as in physics. The fact is that in mathematics we still have the same attitude today that in former times one had toward all science, namely we try to derive everything by cogent proofs from the definitions (that is, in ontological terminology, from the essences of things). Perhaps this method, if it claims monopoly, is as wrong in mathematics as it was in physics.[34]

One could argue that this is really what has been happening throughout the history of mathematics and that this view is particularly in accord with the practice of contemporary set theorists. But such an argument does not belong in an article devoted to the evolution of Gödel's philosphical thought.

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[31] [9], p. 443.
[32] [4], p. 312.
[33] [6], p. 204.
[34] [4], p. 313.

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