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FREGE AND RUSSELL: DOES SCIENCE TALK SENSE?

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ABSTRACT

Over the course of the nineteenth century mathematicians became vividly aware that great advances in intuitive "understanding" could be obtained if novel definitions were devised for old notions such as "conic section", for one thereby often gained a deeper appreciation for why old theorems in the subject had to be true (the new definitions were said to have proved "more fruitful" in these regards). From a naïve philosophical standpoint, such definitional alterations look as if they must properly displace the "propositional contents" of the very theorems they seek to illuminate. Haven't our reformers merely "changed the subject", rather than truly provided the conceptual enlightenment they claim? Many practitioners of the time claimed that "Science" enjoys a special prerogative to ignore "surface content" in its search for truth, a sentiment with which Frege often concurs, at least in his early writings. Yet it is hard to render these opinions consistent with his official views on sense and reference, as this essay details. It also surveys Russell's views on such topics, although he was generally less aware than Frege of the revolutionary mathematical work pursued within the "search for fruitful definitions" program.

Key words: definition, 19th century definitional practice, objective content, sense, meaning, reference, Frege, Russell

This essay will highlight a vital, yet seldom discussed, fashion in which orthodox scientific methodology can induce conflicts with our intuitive expectations as to "propositional content." The early Frege seems quite alive to these difficulties whereas Russell writes as if he were largely innocent of such concerns. However, the former's exact opinions on these issues remain a bit mysterious and other considerations eventually force Frege's own conception of "thought content" to approach the notion of a Russellian "proposition" more closely than is generally recognized. Or so it can easily seem. A brisk survey of the problematic may help bring the thought of both men into sharper focus.

The issues I have in mind stem from the *great liberality* with which both physics and pure mathematics began to approach issues of *definitional practice* over the course of the nineteenth century. Rather than presuming that a notion such as 'conic section' enjoys a single conventional explication upon which lexicographers

dutifully report in their dictionaries, it became common to assume that key advances in scientific understanding often transpire when "deeper definitions" for old terms are uncovered (I'll supply a few illustrations below). Educated at Göttingen, from which many of these nineteenth century definitional recastings radiated, Frege seems fully aware of the philosophical strains and opportunities that such liberal policies engender. In contrast, Russell's more circumscribed Cambridge mathematical education left him with an inferior sensitivity to such issues. Here we might observe that the quest for "deeper diagnoses of concepts" in the Göttingen spirit remains a vital aspect of working scientific practice even today, although many philosophers still approach issues of "conceptual content" in veins akin to Russell's.

By way of background, let us recall the family of considerations that claim to show that all true sentences must designate the truth-value True, if sentences can be said to *designate* anything at all. Many of these arguments proceed by toggling between sentences that utilize predicates or entire sentences and replacement claims that employ singular terms in their stead yet seem mathematically equivalent to the first. At each stage it is assumed that substituting terms with the same denotation will not change the overall designation of the sentences in which these replacements are made. That assumption being made, certain substitution patterns appear to carry us from the "designation" of any starting truth we select to that of any unrelated second truth we might desire. For example, let us move from "Archie has freckles" to "Jughead wears a beanie" by these allegedly "designation preserving" devices.

Archie has freckles.

Archie has freckles & Betty = Betty.

{Betty} = {x| x = Betty & Archie has freckles}.

{x|x = Betty & Archie has freckles} = {x|x = Betty & Jughead wears a beanie}.

{Betty} = {x| x = Betty & Jughead wears a beanie}.

Jughead wears a beanie & Betty = Betty.

Jughead wears a beanie.

Conclusion: all true sentences must therefore designate the same thing, if they designate at all. Patterns like this were dubbed "slingshot arguments" by Perry and Barwise.[1] To the best of my knowledge, Frege never argues in any comparable manner, but reaches the same conclusion about sentential denotation simply by observing that we can entertain a complete thought without considering its truth-value, in allegedly the same manner as we can entertain a descriptive phrase without considering what it denotes. Undoubtedly Frege is much impressed by the fact, already noted by the Booleans, that

[1] Perry 1996.

if you regard truth-functions as functions, they will behave as if they represent maps over the two-valued domain {T, F}.

If, like Frege, we maintain that "thoughts" or "propositions" represent non-denotative "senses" or "mode of presentations" that merely *aim* at their truth-value designations, we will not expect that such "thoughts" themselves will be preserved under the suggested slingshot replacements (Russell can cite his theory of descriptions to the same effect[2]). However, the liberal definitional practices cited previously can potentially affect the qualities of "thoughts" themselves in a manner allied to the slingshot considerations, albeit with somewhat less drastic effects. And this is the topic I want to survey here.

The problems stem from the fact that scientific practice apparently embraces a wide variety of definitions as equally acceptable from a methodological point of view (of these, a particular textbook author may select the one she views as "deepest" for her specific purposes, yet recognize that other authors with other ends may legitimately favor some alternative). As Frege writes,

> For the mathematician, it is no more right and no more wrong to define a conic section as the line of intersection of a plane with the surface of a circular cone than to define it as a plane curve with an equation of the second degree in Cartesian coordinates. Which definition he chooses—one of the two, or some other again—depends entirely upon convenience. (Frege 1984, p.200)

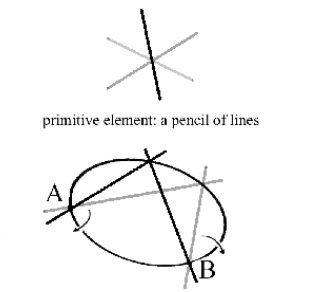
That is, if one looks in different geometry textbooks, one will find different choices of primitive terms and different definitional policies in play. At first blush, this observation seems entirely innocuous yet its tacit ramifications for the notion of 'propositional content' are more radical than we might anticipate, given the range of definitional latitude that became accepted by the middle of the nineteenth century. This tolerance, for example, presents modern fans of Kripke-style metaphysical necessity with greater difficulties than they often appreciate. Typically they presume that the "property identities" of science reflect the necessities they seek. But where does one find such "identities" expressed within a real life science text? Presumably within its array of permitted definitions, *inter alia*. But different books exploit Frege's freedom of definitional choice in different ways and if we accept them all as equally admissible metaphysical truths, we can often recover virtually the full content of the underlying theory merely by utilizing a suitable array of textbook definitions (I have carried out this exercise for classical particle physics[3]). Granted, any adequate textbook will select *just one definition* to introduce a term and the results will then supply a conservative extension of its starting postulates. But the results of *combining definitions* drawn from many different

[2] Kurt Gödel makes this very point in one of the seminal presentations of the slingshot argument (Gödel 1941).

[3] Wilson 1983.

textbooks can be highly creative and sometimes prove equipollent to the full background theory involved. It would prove disastrous for "metaphysical thinking" if, *e.g.*, the complete laws of quantum mechanics turned out to be "metaphysically necessary," for most common possibilities get ruled out as "impossible" by such strictures (one sometimes runs across philosophers who presume otherwise but I doubt they appreciate how destructive equating metaphysical necessity with law-like behavior would be). But I've never run across a convincing diagnosis, from the metaphysician's point of view, of why science should be allowed such great liberality in tolerating "definitions" that, collectively, do not behave as they anticipate.[4]

Insofar as I am aware, few philosophers of an analytic metaphysics persuasion seem adequately aware of the strains that standard definitional latitude in science place upon their conceptions of 'property identity.' Frege, in contrast, seemed quite alive to the fact and even gloried in it, as Jamie Tappenden has stressed in his work on "fruitful concepts."[5] Indeed, the revelatory merits of recalibrated derivational arrangements involving fresh choices of alternative primitive elements represented a mathematical commonplace within Frege's time (and remain so to this day, within mathematical circles). Such methodological themes trace back to Gauss and were nicely exemplified within Steiner's celebrated reorganization of geometry wherein a conic section became redefined somewhat oddly as the intersection of two projectively corresponding pencils of lines (where a 'pencil' of lines running through a point became selected as a primal element within Steiner's reorganization of Euclidean geometry). The mathematician Robert Walker explains the purpose of these reorganizational maneuvers:



primitive element: a pencil of lines

conic: intersection of two corresponding pencils

> An aspect of the interplay between analysis and synthesis in the mathematical investigation of possible kinds of object is the attempt to decompose the objects under investigation into simpler ones which do not decompose further, investigating the indecomposable objects first and then building up all the others. This going back to simplest objects is a reduction process, and hence one calls the indecomposable objects "irreducible". (Walker 1978, p. 204)

Quite commonly, the reorganized basis will also suggest important *extensions* of old ideas that would not have proved otherwise apparent—Laguerre's strange definition of 'angle' in the same setting provides a standard example of these advantages—but a recalibrated set of postulates and definitions can still prove fruitful even without this

[4] Many modern authors criticize Frege and Russell's assumption that names carry uniquely associated descriptive contents, without acknowledging that such a demand is absolutely mandatory in foundational work, lest unwanted creativity creep in through the definitions.

[5] Tappenden 1995.

feature. Indeed, it was common in this historical period to employ the phrase "making X into a *science*" (as opposed to "practicing X as an *art*") to indicate the *unexpected orderliness* that often emerges when a particularly effective set of primitive elements is discovered (we have somewhat lost sight of the employment of "science" in this manner; I own a Victorian pamphlet that advertizes itself as "the only scientific approach to banjo playing"). The famous engineer Franz Reuleaux discusses the need to turn the established art of mechanical design "into an exact science" in such terms:

> In the development of every exact science, its substance having grown sufficiently to make generalization possible, there is a time when a series of changes brings it into clearness. This time has most certainly arrived for the science of Kinematics. The number of mechanisms has grown almost out of measure, and the number of ways in which they are applied no less. It has become absolutely impossible still to hold to the thread which can lead in any way through this labyrinth by the existing methods. (Reuleaux 1963, p.23)[6]

I believe that when Frege writes of logic as providing "the most general science of Truth" that he has in mind both aspects of these methodological gambits: (1) codifying the inference schemes that allow for the creation of satellite "logical objects" (such as sets, complex lines and ideal factors) supplementary to an original domain and useful for articulating exact generalizations and (2) the greater organizational simplicity and inferential control achievable when an original subject matter can be rejiggered Steiner-style through a sagacious choice of primitives and definitional extensions. Endorsing Lange's attempts to reorganize Newtonian mechanics in an allied fashion, Frege remarks:

> I should like to subscribe to [Lange's] statement "that elementary concepts are not the original data of a science", or as I should like to express it, that they must be first discovered by logical analysis. Similarly, the chemical elements are not the original data of chemistry, but their discovery indicates an advanced stage of the development of the science. (Frege 1984b, pp. 135-136)

Interestingly, he here alludes to the then popular view that chemical atoms might prove empirically non-isolatable in the manner of quarks: they can never be "seen" in practice except as combined within larger groups (the mathematician Kummer defended his famous "ideal factors" in algebraic number theory using the same metaphor). In fact, I believe that in his early thinking Frege hoped to extract the natural numbers from the logical woodwork using an algebraic scheme akin to Kummer's, rather than working with sets (I'll supply a sketch below). Such policies suggest that science is free to

[6] Incidentally, I do not think that the deep linkage in late nineteenth century thought between "generality required in a proper science" and "the right to postulate additional elements" has been properly explored, especially with respect to the inclination to address most modal issues in terms of disguised generality.

take great liberties within pre-established propositional content. I might also observe that Frege's celebrated yet cryptic remarks about the mission of logic probably embody claims like (1) and (2) at least as often as they express neo-Kantian themes with respect to the regulative normativity of inquiry.

In any case, the radical definitional allowances encouraged by these scientific goals place our intuitive expectations with respect to "propositional content" under considerable strain because key features of a given sentential claim can alter greatly when one moves from one permissible framework scheme to another. It is quite common, for example, to find that a previously difficult theorem (Desargues' theorem, say, or the conservation of energy) become quite easy to prove when it is placed within a different framework of axioms and primitives. After all, part of science's motive in searching for "more fruitful settings" lies exactly in the expectation that "the hard can become easy" in the process (of course, some governing principle of the Conservation of Difficulty predicts that various previously easy claims may turn into hard lemmas, but these relocated difficulties may prove less central to our basic enterprise). However, on one natural measure, many of us are inclined to answer the question, "Does the sentence S express the same proposition when S appears in the different organizational formats **A** and **B**?" *negatively* if S manages to appear as a near-tautology within one scheme but as a very difficult theorem within another. "No," we complain, "in **A** S reports a triviality, whereas in **B** it expresses a very deep fact." And our usual intuitions about "same subject matter" seem threatened as well: when we discuss 'conic sections' first in Euclid's manner as a "section of a cone" and then in Steiner's peculiar fashion, are we still "talking about the same traits"? Orthodox definitional practice advises us, "yes," but it can certainly seem as if quite different properties of a figure are being discussed.

Clearly, some limitations need to be placed upon science's capacity to overlook intuitive "content." The mere preservation of syntax alone between an original set of doctrines and their purported replacements cannot guarantee that "thought contents" are the same, even by science's relaxed standards, not only because of the duality relationships between theorems within geometry but also the "mechanical analogies" between electric circuits and mechanical elements that were popularly discussed in the physics of the day (Frege is aware of both forms of isomorphism). And these concerns raise the question: *Exactly what sorts of "content" must science be concerned to preserve when it wreaks its reorganizational transformations upon a familiar subject matter?* This strikes me as a methodological puzzle rather analogous to the slingshot arguments but one that instead arises at the thought/sense level. Furthermore, its details turn upon vital features of real life scientific practice, rather than relying upon the more contrived forms of transformation highlighted in standard "slingshot" routines.

In his early writings, Frege frequently evokes a rather unspecified notion of "objective content" as a vehicle to express the invariants of subject matter to which science must

attend in its truth-seeking ministrations (in contrast to those of us concerned with its stylistic or poetic trappings). Consider the famous passage:

> The concept of direction is only discovered at all as a result of a process of intellectual activity which takes its start from the intuition... [I]f it were false that "straight lines parallel to the same straight line are parallel to one another", then we could not transform a//b into an identity. (Frege 1959, pp. 74-75)

It would seem, in this talk of "transforming a//b into an identity," that some radical standard of "preserving objective content insofar as science cares about it" is implicitly presumed. Unfortunately, most of the time Frege runs these concerns together with the humbler thesis that claims articulated in terms of private concepts ("looks red to me") cannot be communicated "objectively" to others. Perhaps so, but science's definitional practices suggest that some second and deeper layer of "objective content" must be apprized from the wider body of material that we can capably communicate to others (after all, normal "poetic connotation" proves fully "objective" in the sense employed here). The dramatic changes affected in Steiner's transmogrifications of geometry and Frege's own a//b transformation seem to indicate that our normal "non-private thoughts" contain a rich "husk" (Frege's term) of unwanted "connotations" that science can freely ignore in its reorganizational ministrations. Some deeper and more radical account of "scientific objectivity" seems wanted, but the early Frege never clarifies these issues adequately.

Let me interject my own view of these interpretational issues. I believe that, up to the moment when he uncovered a procedural glitch in the course of writing his *Foundations*, Frege intended to introduce the natural numbers in a manner that relies upon a rather radical procedure for manipulating intuitive "content." Specifically, he believed that the interior contents of "a is parallel to b" could be shifted to its two ends, rather as the contents of toothpaste tube can be squeezed into its extremities. No overall "content" is lost (insofar as science, in its progressiveness, cares about it), yet "a//b"'s new dumbbell shaping might permit the completion and consolidation of vital patterns of mathematical thought in a surprising manner (suppose that we have been laboring over a jigsaw puzzle of a rainbow when we suddenly realize, as we set our reshaped "a//b" piece into place, that a doubled bow is actually portrayed). Indeed, this "rainbow puzzle" metaphor can be neatly aligned with the algebraic "line coordinate" work of Julius Plücker and Otto Stolz, who were important figures at Göttingen in and around the time that Frege was a student there.[7] I believe that he originally intended to imitate their "content reshaping" techniques in his *Foundations*. For various technical reasons,

[7] Indeed, Stolz precisely employs the "a// b" to "the direction of a = the direction of b" conversion to introduce the "points at infinity" needed to complete the pretty pattern of point/line duality that is suggested within orthodox Euclidean geometry yet not fully provided for without a program of additional supplementation. See Wilson 2005.

however, he was forced to abandon this policy and to employ "extensions" (= sets) instead, even though the initial stretch of his manuscript had been written. Because sets must be introduced via some straightforward existence postulate such as Frege's later "Axiom V," any need to manipulate internal sentential content in a "toothpaste squeezing" manner evaporated from Frege's own projects.

Readers of the early Frege differ widely in how they parse his enticing, yet obscure, "context principle." Along this front suggestions range from the utterly banal to the methodologically radical (such as I have just sketched). The fact that Frege supplies no clear and substantive illustrations of how the "context principle" affects real life mathematical or logical practice prolongs this hermeneutic uncertainty substantially. As such, the texts Frege has left us are too skimpy and guarded to permit any ready resolution to such issues.

Do matters become any clearer after he introduces his sense/reference distinction? Not entirely, insofar as I can see. In "On Sense and Meaning," he writes:

> One might also say that judgements are distinctions of parts within truth-values. Such distinction occurs by return to the thought. To every thought attaching to a truth-value would correspond its own manner of analysis. (Frege 1984c, p. 165)

Here he seems to picture "the contents of a thought" as a *structured map* from a selected list of properties and allied elements to truth-values, regardless of how those elements happen to be defined within the enveloping derivational setting. If so, he can answer our "what contents must science preserve?" question by demanding that sentences must be available within any new formalization that can capture exactly the same structured maps as were available within the old formalization.

There are two problems with this answer. (i) It plainly fails to ratify the liberalized "scientific content" preserving moves of which his earlier writings seem to approve. (ii) The mapping point of view actually suggests that *many* distinct "thoughts-conceived-as-a-mapping" will naturally correspond to each sentence S appearing within a scientific formalism, because, under each different selection of definitional primitives, different collections of properties will serve as the domain of the maps.

As to (i), perhaps this observation only serves as a rebuff to those of us who have believed that the context principle was originally intended as a radical methodological thesis. As he grew older, Frege became increasingly fond of utilizing only trite illustrations for his philosophical morals (including most of the alleged errors of "psychologism" he cites). Insofar as I can see, Frege seemed to believe that one should beat up on ones opponents with the bluntest sledgehammer one can find, rather than relying upon any instrument that might reveal salient differences more subtly. By favoring this blunderbuss argumentative mode, it is often difficult to fathom what his exact attitude to important currents within the mathematics of his time might have been, given that

most of these views can be easily purged of the surface faults of which Frege complains (these remarks apply, in my opinion, to not only Hilbert's views but to those of Hermann Schubert as well). What would Frege think of their *concrete mathematical methods* once their irrelevant "psychologistic" trappings have been discarded? Frequently, one doesn't really know. I happen to believe that, even here, Frege was generally concerned with issues of greater methodological substance than meet the eye, but a fair amount of charity is required to make this case.[8]

Leaving these issues to the side, let's now consider (ii), the fact that standard methodological practice associates a variety of different thoughts-construed-as-mappings to a given syntactic form. Why? First assume that S expresses the proposition in formalism *A* as a map from primitive properties P, Q, R to True. But now consider the same S as it appears within the context of some variant formalism *B*. Form S* from S simply by replacing some of S's defined terms by their *B* definitions in terms of T, U, V. *Prima facie*, this S* should contain the same "scientific content" as our original S. However, S* can obviously be viewed as also expressing a distinct map to truth-values as well, *e.g.*, the map defined over *B*'s newly inserted primitive properties T, U, V. Which of these two thoughts-conceived-as-mappings should be regarded as capturing S*'s (and S's) "proper sense"? Each map, after all, seems to possess equal bragging rights to qualify as a proper rendering of the "thought" shared within the two formalisms *A* and *B*. To consider a specific example, which sentence—that found in Euclid or in Steiner?—best exemplifies the *mapping contents* that an acceptable rendering of a syntactically given theorem on conic sections must capture? Accordingly, a clash arises between the assumption that "thoughts" represent "modes of presentation" conceived as mappings and the notion that science preserves these same "thought contents" when it seeks more fruitful patterns of definitional organization.

I am not aware of any passage where Frege directly addresses this apparent disharmony between the mapping view (apparently) endorsed in "On Sense and Meaning" and his earlier opinions on reorganizational "fruitfulness." For myself, I do not discern any ready resolution of this dilemma that will not shift Frege's picture of "sense" and "thought" substantially in the direction of Russell's approach to propositions. Although Russell evinces little evidence of appreciating the value of mathematical investigations like Steiner's adequately,[9] he did entertain an essentially Lockean view of *physics*' purposes throughout his life. As is well known, he applied his celebrated distinction between entities known by *direct acquaintance* and those that known only by *description*

[8] For speculations about a deeper range of concerns that may lie behind his squabbles with Hilbert, see Wilson 2007. After reading this paper, Penelope Maddy asked whether I favor the view that there are "two Freges." There are glancing passages in the Grundgesetze that suggest that Frege maintained a radical conception of "content" even after "On Sense and Meaning" but he makes little of them.

[9] In his *Essay on the Foundations of Geometry* (Russell 1897), Russell evinces little appreciation of the value of the extension elements in geometry. More generally, his assumption that he is replacing "fictions" by logical constructions usually makes his exact views of former "propositional content" hard to ascertain in the context of mathematics.

to "universals" (= properties) as well. In the case of terms like 'temperature' or 'force,' their scientific investigation usually begins with some distanced description of their capacities, *e.g.*, "the unknown quantity responsible for the heat exchange between two unmoving adjacent bodies." If we are very lucky, we may be able to improve this account until one day we manage to grasp a suitable universal *directly*, in the vein of those philosophers who fancy that 'temperature' directly denotes *mean kinetic energy*. However, this "direct acquaintance" eventuality is by no means assured; we may be forever precluded from directly grasping the quality truly responsible for thermal behavior, just as none of us alive today retain any capacity to become truly "acquainted" with Bismarck. Indeed, the British empiricists commonly doubted that we would ever understand the property responsible for "force" directly: sense experience simply hadn't provided us with an inventory of concepts equal to the task (God will be directly acquainted with the well springs of *force*, but He has not provided us conceptual access of that same intimacy).

From this point of view, a milder form of the "science is free to ignore content" thesis is obtainable. In writing a textbook of physics, it is not essential that we enter therein absolutely everything we know about 'force.' Instead, we can gradually replace the term's older descriptive associations with updated ones that cut nearer to the bone, as it were, insofar as we can approximate direct acquaintance with the quality in question through a tightened schedule of descriptions. In short, not all descriptions are created equal; science should always highlight the nimblest associations that seem most central to its purposes. Applying this same "improving descriptions" methodology to mathematics, we can say: yes, the propositions articulated in a modern geometry text are not those of our father's Euclid, but they represent updated descriptive refinements of them.

Returning to Frege, he can potentially resolve his "many mappings" problem by evoking similar distinctions: among all of the descriptive thoughts that hover around a traditional Euclidean theorem considered syntactically, there will be some that represent "more fruitful" maps from base objects to the True. As with Russell, these favored "thoughts" will not be any that Euclid himself entertained, but represent their more ennobled replacements. In other words, the "thoughts" that science hopes to codify are not exactly the concrete thoughts of its present-day practitioners, but mappings that can be viewed as superior replacements once they have become articulated.

If Frege does indeed adopt this Russellian rationalization of science's definitional practices within his later thinking, his "thoughts" become considerably more ineffable creatures than the phrase "mode of presentation" suggests, at least insofar as he wishes to retain any vestige of the claim that "science organizes thoughts into more fruitful patterns" (strictly speaking, I will have "organized my garage" if I simply throw out all of its contents and buy new stuff, but that's not usually what 'organizes' connotes). But great interpretational problems huddle around this particular nexus of issues, for Frege offers very little that is genuinely revealing on these topics in his later writings. This silence

baffles me, since his earlier views on definitional practice seem far more rewarding. In an allied vein, Juliet Floyd reports,

> Wittgenstein wrote to Frege that he considered Frege's 1918 essay "Der Gedanke" ... to be an inferior work, flawed because it attacks Idealism on its weak side.[10] (Floyd 2005, p.29)

I don't know Wittgenstein's own rationale for this complaint, but many of the mathematicians whom Frege criticized as "psychologistic" often evoked neo-Hegelian Idealism as an engine to pull scientific development forward through ever-deepening stages of conceptual articulation, precisely because they recognized the progressive importance of improved "definitional settings." To complain that such views make numbers "private mental objects" scarcely acknowledges the surprising facts about real life methodological practice that motivate such opinions. To argue against such views as Frege does is truly to "attack (mathematical) Idealism on its weakest side."

At a minimum, the radical methodological practices that Frege highlights within his early years suggest that more layers of "propositional content" are required to rationalize standard scientific methodology than the deceptively innocent prose of "On Sense and Meaning" suggests.

REFERENCES

Beaney M. & E. Reck eds. (2005), *Gottlob Frege: Critical Assessments*, London: Routledge

Ben-Menahem Y. (2005), *Hilary Putnam*, Cambridge: Cambridge University Press

Clark A., Ezquerro J. & J. Larrazabal eds. (1996), *Philosophy and Cognitive Science: Categories, Consciousness, and Reasoning*, Dordrecht: Kluwer Academic Publishers

Floyd J. (2005), "Putnam's 'The Meaning of Meaning': Externalism in Historical Context", in Ben-Menahem 2005, pp. 17-52

Frege G. (1959), *The Foundations of Arithmetic*, trans. by J. L. Austin, Oxford: Basil Blackwood

Frege G. (1984a), "Review of E.G. Husserl", trans. by H. Kraal in McGuiness 1984 pp. 195-204

Frege G. (1984b), "On the Law of Inertia", trans. by H. Kraal in McGuiness 1984 pp. 123-136

Frege G. (1984c), "On Sense and Meaning", trans. by M. Black in McGuiness 1984 pp. 157-177

Gödel K. (1944), "Russell's Mathematical Logic", in Schilpp 1951, pp. 123-153

McGuiness B. ed. (1984), *Collected Papers on Mathematics, Logic, and Philosophy*, Oxford: Blackwell

Perry J. (1996), "Evading the Slingshot", in Clark, Ezquerro & Larrazabal 1996, pp. 95-110

Reuleaux F. (1963), *The Kinematics of Machinery*, trans. by A. Kennedy, New York: Dover

Russell B. (1897), *An Essay on the Foundations of Geometry*, Cambridge: Cambridge University Press

Schilpp P. ed. (1951), *The Philosophy of Bertrand Russell*, New York: Tudor

[10] Floyd correctly observes that Russell's attitudes about scientific investigation reveal many attitudes quite close to (some readings of) Putnam's own "externalism," a point that Lionel Shapiro also advances with respect to Locke (Shapiro 1999).

Shapiro L. (1999), "Towards 'Perfect Collections of Properties'", *Canadian Journal of Philosophy*, 29, pp. 551-593

Tappenden J. (1995), "Extending Knowledge and 'Fruitful Concepts': Fregean Themes in the Foundations of Mathematics", *Noûs*, 29, pp. 427-467

Walker R. (1978), *Algebraic Plane Curves*, Berlin: Springer-Verlag

Wilson M. (1983), "Why Contingent Identity is Necessary", *Philosophical Studies*, 43, pp. 301-327

Wilson M. (2005), "Ghost World", in Beaney & Reck 2005 pp. 157-175

Wilson M. (2007), "Frege's Mathematical Context", *PhilSci Archive*

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