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WAS RUSSELL A SECULAR HUMANIST?

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Cover: Bertrand Russell on the terrace at Portmeirion about 1950

# MIND

## BERTRAND RUSSELL CENTENARY SPECIAL ISSUE

*Mind* has celebrated the Centenary of Bertrand Russell's landmark essay 'On Denoting' by devoting our October 2005 issue entirely to this seminal paper.

This special issue includes an introductory editorial by Professor Stephen Neale, and papers by a distinguished group of philosophers, including

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## IN THIS ISSUE

WAS RUSSELL A SECULAR HUMANIST? Many members of the Russell Society seem to think so; others disagree. Francis Mortyn of the American Humanist Association reports that the term 'secular humanism' was first used by the Moral Majority in the 60s and 70s as a form of disparagement and only gained currency among humanists when Paul Kurtz adopted the term in 1979 or 1980 to refer to his own humanist group, which today is the Council for Secular Humanism. Mortyn further claims that the concept of secular humanism "offers no philosophical advance" over that of humanism, that is, there is no theoretical difference between the two terms.<sup>1</sup>

There is some justice to the claim that there is no philosophical difference between humanists and secular humanists. What American humanists have meant by 'humanism' from at least 1933 on can be seen in their 1933 *Humanist Manifesto*. Comparing this document to the description of secular humanism at the website of the Council of Secular Humanism shows that both humanists and secular humanists reject theism (belief in a supernatural being) and accept naturalism (the view that it is only within science itself that reality can be described). Both reject a supernatural creation of the earth, both accept evolution theory as best accounting for the creation of human beings, both seek moral values that will make people's lives better, and both think it is by reason and experience that such values will be found.<sup>2</sup>

The difference between humanists and secular humanists, if any, seems to be an emotional one. Though rejecting theism and accepting naturalism, the humanists of the 1933 manifesto quite astonishingly called themselves 'religious humanists' – the majority of the signatories of the manifesto were in fact Unitarian clergy! Essentially, they were naturalists committed to using reason to improve people's lives *who liked religion* and so wanted to call this view religion too. In contrast, secular humanists are naturalists committed to using reason to improve people's lives who, according to their website, are people who "typically describe themselves as non-religious", that is, they are people who *don't* like religion.

<sup>1</sup> Francis Mortyn, private email to John Ongley, July 16, 2006.

<sup>2</sup> The 1933 Humanist Manifesto and Council of Secular Humanism description of secular humanism can both be found on the web.

So where does Russell belong in all of this? While he was a member of the British Humanist Association (at one time presided over by A.J. Ayer), he tended to call himself either a rationalist or a skeptic. Still, it seems fair to say that he was some sort of humanist. But what kind? He valued what he called "personal religion" by which he seems to have meant ecstatic religious experiences and strong moral intuitions, though he refused to infer from these experiences to belief in a supernatural being, much like the religious humanists, and so would have fit well among them. At the same time, he never passed up an opportunity to kick organized religion and so would have also fit in well among the secular humanists.

Then again, humanists are naturalists, so if Russell was not a naturalist it would not be right to call him a humanist of any kind. Was Russell, then, a naturalist?

IN THIS ISSUE, ANDREW LUGG ARGUES, in 'Russell as a Precursor of Quine', that yes, Russell was indeed a naturalist in his philosophy from at least 1914 on. Lugg argues this point by comparing Russell's views on philosophy with Quine's naturalism, showing that Russell and Quine agree on most fundamental issues, and concludes that the best way of viewing Russell is as a precursor of Quine's naturalism and the best way of viewing Quine is as a follower of Russell. And if Lugg is correct, then given Russell's other likenesses with humanism, perhaps it is most fair to say that on Mondays, Wednesdays and Fridays Russell was a religious humanist and on Tuesdays, Thursdays and Saturdays a secular one. I leave it to the reader to decide what Russell was on Sundays.

The nature of Russell's views on naturalism are not all that are examined in this issue. Alongside these matters, CHAD TRAINER explores THE NOOKS AND CRANNIES OF RUSSELL'S VIEWS ON WORLD GOVERNMENT in his essay 'Solitary, Poor, Nasty, Brutish and Short: Russell's Views of Life without World Government'. Trainer begins by noting that Russell's views on world government were nearly the same as Hobbes's views on national ones and then fleshes this observation out, exploring the details of their similarities and differences, ending with an assessment of the pertinence of Russell's views on world government for today's world.

## SOCIETY NEWS

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TIME TO RENEW. The BRS exists to serve you by providing you with annual and other meetings, various publications and an online community, and the membership dues of the society enable the BRS to fulfill its function, so if you have not already paid your dues, we hope that you will do so now.

IOWA CITY, IOWA. The weekend of June 1-3 marked the occasion of the 33rd Annual Meeting of the BRS, convening for the first time at the University of Iowa in Iowa City, Iowa. The Society extends its thanks to Gregory Landini, convener, for making the event possible and for seeing to it that the weekend went smoothly. This was the first time that the BRS annual meeting has made it to this part of the US, and a general sense of excitement about the venue accompanied the many fine talks. A complete report of the Annual Meeting will be published in the next issue of the BRS *Quarterly*.

### LETTER TO THE EDITOR

Dear Editors:

I was saddened to learn in the August 2005 *Bertrand Russell Society Quarterly* of the death of Whitfield Cobb. He was an extremely warm and engaging man who I met at several BRS annual meetings in the early 1990s. I was new to the Society then, and Cobb was one of the several people who helped me feel at home and convinced me that I was in the right place (despite being perhaps 20 years younger than most of the meeting attendees). He will be missed.

Yours very sincerely,

Peter Stone.

A BRS LIFE MEMBERSHIP COUPLE! The Bertrand Russell Society gained two new life members this past spring *and its first life membership couple* when Eberhard and Yvonne Jonath of Volketswil Switzerland became life members of the Society in March. Yvonne says that it was from reading his *History of Western Philosophy* that she first became impressed by Russell – for his rationalism, courage, humor, and personality. Eberhard says he admires Russell's willingness to give up his scientific opinions when they were no

longer well-grounded. Yvonne suggests a BRS meeting in Europe sometime, something that has been discussed now and then in the BRS but never acted on. We are grateful to the Jonaths for their generous gift and are delighted to have them as life members.

AND YET ANOTHER LIFE MEMBER. Michael Berumen has also become a life member of the Russell Society. Michael is a businessman and philosopher who lives in Southern California (Laguna Niguel) who has written a book on ethics called *Do No Evil: Ethics with Application to Economics Theory and Business*. Michael has wondered about a BRS meeting in California, another prospect that has been discussed now and again in the BRS. As with the Jonaths, we are grateful to Michael for his generous gift and pleased to have him as a life member of the BRS.

NEW SOCIETY HONORARY MEMBER. David Henehan reports that after months of searching for Tariq Ali, David finally found him and notified him of the Society's offer of an honorary membership. Ali has accepted the offer and so is the newest honorary member of the Society. Tariq Ali is a noted author, filmmaker, BBC commentator and historian who regularly contributes to the *Guardian*, *Counterpunch*, and the *London Review of Books*. We feel privileged to have him as an honorary member of the BRS.

GET ON THE MAP! Curious where other BRS members live? Thanks to Bob Riemenschneider, you can now find out! To find the BRS map site, go online to <http://frapper.com/bertrandrussellsociety>. Once there and enrolled, you can add your name—and a photo if you like—to the BRS map along with the 29 members who are already on it. A tiny balloon marks your spot on the map. (It marks your movements too, if you travel.) Clicking on your balloon expands it and displays your name, photo, and message to the BRS community. Clicking on any area of the map allows you to zoom into a location almost to street level. Besides being useful, the website contains such moving tributes to intellectualism and free thought as “Philosophy is mathematics”, “Mathematics is philosophy” and “Warren, move your balloon, you’re crowding me!”

APROPOS OF THE MAP, a pipe-smoking Bertie was spotted in the (Frapp) vicinity of Pembroke Road, East of Ham and Northwest of London. The same Bertie was subsequently reported wandering around Utah in an empty field West of Route 50. According to our sources, Bertie appears to be lost and resorting to strong language.

EASTERN AND CENTRAL DIVISION ANNUAL MEETINGS OF THE APA. The Bertrand Russell Society hosted sessions of talks at both the Eastern and Central APA meetings this past year. An account of the sessions at the Eastern meeting (December 27-30) can be found in the Traveler's Diary in the back of this issue. This year's Central meeting (April 26-29), housed within the gilt walls of the Palmer Hotel in Chicago, included a broad mix of talks, with Stephen Mumford (University of Nottingham) speaking on “Russell's Defense of Idleness”, Nikolay Milkov (Bielefeld University) on “The Joint Program of Russell and Wittgenstein: March-November 1912”, and Eric Wielenberg (DePauw University) speaking on “Bertrand Russell and C. S. Lewis: Two Peas in a Pod”, with Nikolay Milkov responding. Our friends in HEAPS (History of Early Analytic Philosophy Society) sponsored a related session of talks there, with Paul Pojman (Towson University) speaking on “From Mach to Carnap: A Tale of Confusion”, Aaron Preston (Malone College) speaking on “Scientism and the Emergence of Analytic Philosophy”, and Giancarlo Zanet (University of Palermo) speaking on “Pragmatism, the A Priori and Analyticity: C. I. Lewis and Quine”.

THIS ISSUE'S TREASURER'S REPORTS. In order to correct for some past inaccuracies, we are republishing several past BRS treasurer's reports.

## FEATURES

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### RUSSELL AS A PRECURSOR OF QUINE \*

ANDREW LUGG

AT THE END OF HIS CONTRIBUTION to a panel on Bertrand Russell's philosophy at the American Philosophical Association in Philadelphia in 1966, W.V. Quine draws attention to Russell's "increasing naturalism".<sup>1</sup> Unsurprisingly given Quine's belief that "it is within science itself, and not in some prior philosophy, that reality is to be identified and described",<sup>2</sup> he applauds Russell's move to neutral monism and the subsequent "drift" in his thinking towards a more comprehensive naturalism. He only regrets that Russell's "neutral particulars are on the side of sense data" and his later epistemology falls short of "the physicalistic pole, even in *Human Knowledge*". On his interpretation, "Russell had stated the basis for [the naturalistic] attitude already in 1914 [in *Our Knowledge of the External World*, one of Russell's earliest forays into epistemology]".<sup>3</sup> He observes that in *Our Knowledge* Russell says: "There is not any superfine brand of knowledge, obtainable by the philosopher, which can give us a standpoint from which to criticize the whole of knowledge of daily life. The most that can be done is to examine and purify our common knowledge by an internal scrutiny, assuming the canons by which it has been obtained".<sup>4</sup>

Quine's account of Russell's developing philosophical views is not uncontroversial. It is questionable whether Russell advanced a "frankly phenomenistic form" of "logical atomism" and developed his more naturalistic version of neutral monism by "warping" his

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\* The main ideas of this paper were presented at the Annual Meeting of the Bertrand Russell Society held at McMaster University, Hamilton, Ontario, May 2005. Thanks to Lynne Cohen, as always, for her help and to Paul Forster for detailed comments on an earlier draft of the paper and frequent discussion.

<sup>1</sup> 'Russell's Ontological Development', p. 85.

<sup>2</sup> 'Things and Their Place in Theories', p. 21.

<sup>3</sup> 'Russell's Ontological Development', p. 85.

<sup>4</sup> p. 71 in the edition of *Our Knowledge* that Quine quotes, pp. 73-74 in later editions.

atomism.<sup>5</sup> And questionable too whether he came to favour the naturalism Quine attributes to him only in 1928 and whether his philosophy became increasingly naturalistic in the years that followed. Naturalism makes an appearance in the 1914 lecture Quine refers to, and Russell's epistemology of the 1940s is not significantly more naturalistic than his epistemology of the 1920s. Quine is surely right, however, about the naturalistic cast of Russell's thinking. Early and late, Russell rejected the possibility of justifying our knowledge of the external world without assuming anything whatsoever about the world itself and never attempted to seek out "superfine ... knowledge".<sup>6</sup> However much Russell changed his views between the early 1910s and the late 1950s, he took the methods of epistemology to be the methods of science and everyday life. He shunned the idea of a first philosophy and demonstrated, as Quine notes, "a readiness to see philosophy as natural science trained on itself and permitted free use of scientific findings".

In what follows I take up Quine's hint and defend his conception of Russell as a naturalistic epistemologist. I argue that Russell approaches the problem of our knowledge of the external world in much the same way as Quine approaches the problem and the difference between them regarding the relationship of our knowledge to the evidence on which it is based, though important, is one between philosophers in the same naturalist camp. Nobody needs reminding that Russell does not agree with Quine on everything – that, for instance, he has a different view of the nature of necessity and the *a priori* and thinks of analysis as getting at hidden meanings rather than as a clarificatory enterprise. My contention is that Russell advances his epistemological speculations in as scientific a spirit as Quine, occasional appearances to the contrary notwithstanding. He intends his discussion of our knowledge of the external world to be understood as a contribution to science and is far better viewed as a precursor of Quine than as the traditionally-minded philosopher he is usually taken to be – and Quine correspondingly better viewed as a follower of Russell than as a lapsed logical positivist. I start by noting some important similarities between Russell's thinking and Quine's.

<sup>5</sup> 'Russell's Ontological Development', p. 85.

<sup>6</sup> *Ibid.*

IT NEEDS NOTICING RIGHT AWAY that Russell is as antipathetic as Quine to the dream of a foundation for scientific and commonsense knowledge firmer than and prior to science and commonsense. He does not discuss our knowledge from a standpoint beyond what we know but scrutinises it given what we know. It is, he thinks, no part of the philosopher's task to demonstrate once and for all that belief in the existence of the external world is justified, and he devotes his efforts to critically examining and organising our beliefs to reveal their relative strengths and how they are interrelated. Thus in 1927 in *Outline of Philosophy* he writes: "Philosophy involves a criticism of scientific knowledge, not from a point of view ultimately different from science, but from a point of view less concerned with details and more concerned with the harmony of the whole body of special sciences" (p. 2). And still earlier, in 1912, he writes in *Problems of Philosophy*, his first major work in epistemology: "Philosophical knowledge ... does not differ essentially from scientific knowledge; there is no special source of wisdom which is open to philosophy but not to science, and the results obtained by philosophy are not radically different from those obtained from science" (p. 149).<sup>7</sup>

Time and again Russell declares that he takes epistemology to be a science combining logic and psychology. In *Theory of Knowledge*, for instance, he avers that "it is impossible to assign to the theory of knowledge a province distinct from that of logic and psychology" (p. 46) and in *An Inquiry into Meaning and Truth* he underlines that "[e]pistemology involves both logical and psychological elements" (p. 18). On his reckoning, as he explains in *Inquiry*, epistemology is psychological insofar as it concerns "the relation of basic propositions to experiences, the degree of doubt or certainty that we feel in regard to any of them, and the methods of diminishing the former and increasing the latter", logical insofar as it concerns "the inferential relation ... between basic propositions and those we believe because of them; also the logical relations which often subsist between different basic propositions, ... also the logical character of the basic propositions themselves". For all the differ-

<sup>7</sup> Also compare *Problems of Philosophy*, pp. 25-26, *Our Knowledge of the External World*, p. 74, and Quine's quotation from the same work cited earlier.

ences between Russell and Quine regarding experience, basic propositions and the character of the “inferential relation”, their general stances are much the same. Both are concerned with “the central logical structure of empirical evidence” and both persuaded that the “essentials” of “the relation of evidential support ... can be schematized by little more than logical analysis”.<sup>8</sup>

When Russell turns to the specific problem of the external world, moreover, he states without qualification that he is engaged in a scientific endeavour. Thus in ‘Professor Dewey’s “Essays in Experimental Logic”’, a review he wrote for the *Journal of Philosophy* in 1919, he says: “The chief thing that I wish to make clear is that in discussing the world as a logical problem, I am dealing in a scientific spirit with a genuine scientific question, in fact a question of physics” (p. 21). (The reason Russell refers to the problem of the external world as a “logical problem” is that he takes it to concern the question of “[w]hat, apart from argument and inference ... surviv[es] a critical scrutiny [and] what inferences will then be possible?” (pp. 20-21).)<sup>9</sup> Here Russell not only anticipates Quine’s question: “Whence then the strength of our notion that there is a physical world?”, he also anticipates his view of the question as one “for the natural science of the external world, in particular, for the psychology of human animals”.<sup>10</sup> (Incidentally, Russell also agrees with Dewey regarding the nature of the problem. He is not, as followers of Dewey frequently assume, committed to the idea of a first philosophy.)<sup>11</sup>

Russell avails himself of scientific results whenever he can. He would have had no quarrel with Quine’s claim that “it is a finding of natural science itself, however fallible, that our information about the world comes only through impacts on our sensory receptors” or with his conception of “the relation of science to its sensory data

<sup>8</sup> Quine, *Pursuit of Truth*, p. 18 and pp. 1-2.

<sup>9</sup> Also compare *Our Knowledge of the External World*, Lecture III. In this lecture Russell speaks of himself as “apply[ing] the logical-analytic method” to the problem (p. 72).

<sup>10</sup> Quine, ‘The Scope and Language of Science’, p. 230.

<sup>11</sup> In ‘Professor Dewey’s “Essays in Experimental Logic”’, Russell writes: “I suppose [Dewey] would say, what I should agree to in a certain fundamental sense, that knowledge must be accepted as a fact, and cannot be proved from the outside” (p. 17).

[as] a relation open to inquiry as a chapter of the science of [an antecedently acknowledged external] world”.<sup>12</sup> As Russell explicitly notes in his review of Dewey, in observing that “the conception of a ‘datum’ becomes, as it were, a limiting conception of what we may call scientific common-sense”, he is “proceeding along ordinary scientific lines” (p. 21). And as he explicitly says in his 1923 article on ‘Vagueness’ he thinks that “if you are going to allow any inferences from what you directly experience to other entities, then physics supplies the safest form of such inferences” (p. 154).<sup>13</sup>

More striking still, Russell’s picture of those doing the knowing is reminiscent of nothing so much as Quine’s picture of them. Like Quine, he conceives the knowing subject as a physical object acted on by external forces and reacting from time to time by disturbing his or her immediate surroundings. It was Quine who said: “I am a physical object sitting in a physical world. Some of the forces of this physical world impinge on my surface. Light rays strike my retinas; molecules bombard my eardrums and fingertips. I strike back emanating concentric airwaves. These waves take the form of a torrent of discourse about tables, people, molecules, retinas, air waves, prime numbers, infinite classes, joy and sorrow, good and evil”.<sup>14</sup> But it could have been Russell. Certainly Russell had no compunction about writing in *The Analysis of Matter*: “In the last analysis, all our knowledge of matter is derived from perceptions, which are themselves causally dependent on effects on our bodies.... What we hear, and what we read in books, comes to us entirely through a flow of energy across the boundaries of our bodies” (pp. 27, 28).<sup>15</sup>

To picture knowers as surfaces across which energy travels, as Russell – and following him Quine – does, is to opt for the physicist’s view of them and to refrain from describing them in intention-

<sup>12</sup> *Pursuit of Truth*, p. 19.

<sup>13</sup> Also compare *Our Knowledge of the External World*, pp. 75-80.

<sup>14</sup> These are the opening sentences of ‘The Scope and Language of Science’, the paper in which Quine lays out his epistemological project for the first time.

<sup>15</sup> Burton Dreben, perhaps Quine’s closest associate, told me that when he asked Quine about the similarity of his view to Russell’s, Quine assured him – much to Dreben’s surprise – that he had not read *The Analysis of Matter* before writing ‘The Scope and Language of Science’.



al or mentalistic terms. The thought is that each of us comes up with our knowledge of the world from the slenderest of data, specifically physical, sensory data, and it falls to the epistemologist to explain how we can know about the world beyond our surfaces given that we only have (according to natural science) what crosses our surfaces to go on. Though it is hard to imagine Russell declaring in so many words: "All I am or ever can hope to be is due to irritations of my surface, together with such latent tendencies to response as may have been present in my original germ plasm",<sup>16</sup> he is as committed as Quine to regarding the knowing subject as a system governed by the laws of physics. In his 1927 book *The Analysis of Matter*, for instance, he stresses the "physical significance" of his conception and treats the individual knower as "an oval surface, which is liable to continuous motion and change of shape, but persists throughout time", a surface across which energy flows, "sometimes inward, sometimes outward" (pp. 27-28). He even writes in 'Vagueness': "People do not say that a barometer "knows" when it is going to rain; but I doubt that there is any essential difference between the barometer and the meteorologist, who observes it" (p. 154).

HAVING DRAWN ATTENTION to important similarities between Russell and Quine, I turn now to what may be thought to be stumbling blocks to grouping them together, starting with the seemingly awkward fact that Russell avails himself of the method of Cartesian doubt, something Quine never does. It is tempting to object that no naturalistic philosopher would appeal as brazenly as Russell to such doubt, never mind invoke it as he does in *Our Knowledge of the External World* to isolate data "which resist the solvent of critical reflection" (pp. 77-78).<sup>17</sup> This objection, however, labours under the difficulty that Russell does not invoke the method of doubt to determine what is "completely indubitable", only deploys it in the course of his logico-psychological investigations, as he says in *An Inquiry into Meaning and Truth*, to determine a set of propositions "not wholly derived from their logical relations to other propositions" (p. 125). It is no coincidence that he emphasises in *Human Knowledge*

<sup>16</sup> Quine, 'The Scope and Language of Science', p. 228.

<sup>17</sup> Russell also describes his task in this work as one of "discovering what sort of world can be constructed by ... means [of hard data] alone" (p. 79).

that he is "expounding part of Descartes' argument", not the whole of it, and troubles to note that Cartesian doubt has "value as a means of articulating our knowledge and showing what depends on what" (pp. 188, 196).

Russell does, it is true, say in *An Inquiry into Meaning and Truth* that "the whole subject [of epistemology] is a product of Cartesian doubt" (p. 16; also p. 117). But Quine, a naturalistic thinker if ever there was one, believes the same thing, his view being, as he puts it in 'The Nature of Natural Knowledge', that "the theory of knowledge has its origin in doubt" (p. 67). Moreover, as Quine immediately goes on to note, the crucial question is not whether "[d]oubt prompts the theory of knowledge" but whether "knowledge, also, was what prompted the doubt", i.e. whether the doubts are "scientific" rather than independent philosophical doubts (pp. 67, 68). And in any case Russell himself is motivated by doubts that arise within science, not by ones imported from the outside. To his way of thinking, epistemology would be an idle pastime were it not for the fact that, as he says in *An Inquiry into Meaning and Truth*, science undermines "the doctrine that things are what they seem", indeed is "at war with itself" (p. 15). He takes our knowledge to be worth scrutinising, criticising and reorganising for the simple reason that whenever he ponders the external world from within the framework of science he finds himself "full of hitherto unquestioned assumptions, for many of which [he has] as yet no adequate reason".<sup>18</sup> In other words for him the method of doubt is a method of science, one that scientists help themselves to from time to time, for instance when they submit their own scientific beliefs to "internal scrutiny".

Nor is it a problem for the interpretation of Russell's philosophy I am defending that he stands foursquare against the Quinean view that behaviourism is mandatory for a properly scientific epistemology. Russell does, to be sure, criticise behaviourism by arguing that psychologists may be deceived in much the same way as "the animals [they are studying] are deceived by mirrors" and that "[w]hen the behaviourist observes the doings of animals, and decides whether these show knowledge or error, he is not thinking of himself as an animal, but at least as a hypothetically inerrant recorder of what

<sup>18</sup> 'Professor Dewey's "Essays in Experimental Logic"', p. 20.

actually happens".<sup>19</sup> For Russell the behavioural scientist "gives a false sense of objectivity to the results of his observation[s]" because he "omit[s] the fact that *he*—an organism like any other—is observing".<sup>20</sup> None of this, however, shows Russell to have been less than fully consistent in regarding the problem of our knowledge of the external world as a scientific problem. To the contrary, far from repudiating naturalism and opting for first philosophy, he simply draws what he takes to be a consequence of our present-day scientific knowledge. His attack on behaviourism is an attack from within the naturalist's framework, not from outside it.

Russell's central point about behaviourism is, as he put it in *An Inquiry into Meaning and Truth*, that it fails to acknowledge an important item of scientific scripture, "scripture [that], in its most canonical form, is embodied in physics (including physiology)" (p. 15). He holds that a "serpent [has been introduced] into the behaviourist's paradise" once "the fallibility of the observer" is noticed, a serpent that "has no difficulty in quoting scientific scripture [to prompt doubts about the external world]". It is, he would have us appreciate, a consequence of science itself that we must start from sensory data rather than from overt behaviour and consider how we manage to obtain our knowledge of the world from our perceptions. The epistemological problem concerns our knowledge of human behaviour (and human physiology) as well as our knowledge of the physical world, and only by considering the antecedents of behaviour and what goes on in our heads from a scientific standpoint can we hope to clarify how human knowers know anything at all.

In this context it is also important to notice that Russell's remarks about acquaintance in *Problems of Philosophy*, *Theory of Knowledge* and other early epistemological writings do not cause trouble for my line of interpretation. Though his conception of knowledge by direct, unmediated acquaintance is foreign to Quine, his epistemological project, early and late, is not fundamentally different. There is for one thing more than a slight echo of Russell's view that "the meaning we attach to our words must be something with which we are acquainted" in Quine's view that "all inculcation

<sup>19</sup> *An Inquiry into Meaning and Truth*, pp. 14-15. Also compare *An Outline of Philosophy*, p. 105.

<sup>20</sup> *An Inquiry into Meaning and Truth*, p. 15.

of meanings of words must rest ultimately on sensory evidence".<sup>21</sup> And for another the principle of acquaintance – "Every proposition which we can understand must be composed wholly of constituents with which we are acquainted"<sup>22</sup> – is not in and of itself antithetical to naturalism. Nor, contrary to what is often supposed, did Russell himself regard the principle as an independent constraint on analysis, one that precedes scientific investigation. In good Quinean fashion, he took it to stand and fall with his theory of the world and treated the question of what we are (directly) acquainted with, if anything, as a scientific question.<sup>23</sup>

Again it is no objection to the present line of thought that Russell contrasts his brand of "'theory of knowledge' ... or 'epistemology', as it is also called", with "theory of knowledge [that] accept[s] the scientific account of the world ... as the best at present available".<sup>24</sup> To insist, as Russell does, that the first kind of theory of knowledge is "deeper and [of] much greater importance" than the second kind of theory is not to come down on the non-naturalist side of the fence. The distinction in question is a distinction between two types of naturalistic theory of knowledge, the sort Russell aims to develop and the sort a psychologist or sociologist might attempt to provide, i.e. one that recognises that "the world ... contains a phenomenon called 'knowing', and ... consider[s] what sort of phenomenon this is". In fact there is no discernible difference between the sort of "theory of knowledge" Russell favours and the sort Quine envisions in point of depth and importance. Both philosophers are occupied with what in *Pursuit of Truth* Quine characterises as "central to traditional epistemology", namely the job of clar-

<sup>21</sup> Russell, *Problems of Philosophy*, p. 58; Quine, 'Epistemology Naturalized', p. 75.

<sup>22</sup> Russell, *Problems of Philosophy*, p. 58 (Russell's italics).

<sup>23</sup> A full discussion of this point is out of the question here. I shall only say that I believe Russell is best read as revising his views about acquaintance along with his understanding of the deliverances of natural science (in this connection see the references in footnote 7 and the accompanying text). Also I would argue that Russell eventually dispensed with the notion of acquaintance because he came to think it scientifically problematic and superfluous.

<sup>24</sup> *An Inquiry into Meaning and Truth*, pp 15, 14. The following quotations are from p. 14 and pp. 12-13.

ifying the relation of our knowledge as a whole to the sensory information on which it is based (p. 19). And both philosophers take the epistemologist's main task, as Russell says in *An Inquiry into Meaning and Truth*, to be one of arranging "what we think we know in a certain order in which what comes later is known (if it is known) because of what comes earlier" (p. 16).

Finally to allay another possible worry, I should stress that nothing I have been suggesting runs counter to Russell's conception of logic in *Our Knowledge of the External World* as "the essence of philosophy" (Lecture II) or his view in 'On the Scientific Method in Philosophy' that "*philosophy is the science of the possible*" (p. 84, italics in the original). These remarks, as Russell intends them, are perfectly compatible with the naturalistic attitude Quine discerns in his thinking, even required by it. He takes logic to be at the heart of philosophy because, as he says in Lecture II, it "enlarg[es] our abstract imagination" and "provides a method which enables us to obtain results that do not merely embody personal idiosyncrasies" (pp. 68, 69). And he takes philosophy to deal with "the possible" because, as he immediately goes on to note in 'On the Scientific Method in Philosophy', it deals with "the general", the possible and the general being "indistinguishable" (p. 84).<sup>25</sup> Moreover I am persuaded that when considered in context, other seemingly troublesome remarks – e.g. Russell's claim in *Theory of Knowledge* that "[a] *knowledge of physics and physiology must not be assumed in theory of knowledge*" (p. 50; italics in the original) – are no less readily accommodated within the framework of the interpretation I am promoting.

MY ARGUMENT HAS BEEN that however much Russell differs from Quine about the nature of natural knowledge, he agrees with him in taking epistemology to be a branch of natural science and in regarding the problem of our knowledge of the external world as a scientific problem. He is an empiricist in the Quinean mode, one who takes the doctrine that there is nothing in the mind about the

<sup>25</sup> Compare Quine, *Pursuit of Truth*, p. 18. It is, I fancy, hardly accidental that Quine writes: "In the fused phrases of Kant and Russell, [I am concerned with] a question of how our knowledge of the external world is possible."

world not first in the senses to be a finding of science (as opposed to a result of pure inquiry prior to scientific research). His empiricism is integral to his naturalism and he intends his claims about the evidence of the senses and our knowledge of the external world to be understood as hypotheses open to criticism and improvement.<sup>26</sup> Where he disagrees with Quine is over what science tells us regarding the data and how the rest of our knowledge is related to them, his epistemological naturalism being one of sense and sensibilia, Quine's one of neural receptors and their stimulation.<sup>27</sup> One can summarise how Russell differs from Quine, not too misleadingly, as stemming from the fact that whereas Quine takes the epistemologist's task to be one of shedding light on the transition "from stimulus to science", as the title of his last book has it, Russell takes it to be, as he puts it in his final important philosophical work, one of clarifying "the transition from sense to science".<sup>28</sup>

The picture I've been sketching of Russell as a naturalistically-minded epistemologist in the Quinean mould is very different from the usual picture of him. He is not engaged in a none too successful quest for certainty (over and above the certainty provided by science) or trying to answer the sceptic who aims to put the whole of science into question. The object of the exercise, as Russell understands it, is to develop a genuinely scientific account of "hard" and "soft" data and the relationship between them, and nobody should be fooled by the question he raises at the beginning of *Problems of Philosophy*, his most widely read book: "Is there any knowledge in the world which is so certain that no reasonable man could doubt it?" (p. 7). The reasonable people he has in mind are scientifically-informed thinkers, not sceptics, and he does not mean to suggest his conclusions are immune to sceptical doubt. Rather the opposite. He allocates to philosophy "the more modest function" of providing "an orderly systematic organisation of our knowledge" and allows "it is ... *possible* that all our beliefs may be mistaken"

<sup>26</sup> See especially *Our Knowledge of the External World*, p. 94, and *My Philosophical Development*, p. 20.

<sup>27</sup> Compare Quine, *Pursuit of Truth*, p. 19.

<sup>28</sup> *My Philosophical Development*, p. 153. See also *ibid.*, p. 80, and 'The Relation of Sense-Data to Physics', pp. 111-113.

(pp. 26, 25; italics in the original). In fact he thinks "the sceptical philosophy is so short as to be uninteresting".<sup>29</sup>

I hope I have said enough to show that Russell's post-1912 epistemological writings deserve more attention than they are usually accorded and it is a mistake to dismiss them as dull, lacking in substance or without lasting importance. Russell pioneered an important approach to the subject, one that is nowadays commonly endorsed, not least by Quine and philosophers influenced by him. Though perhaps not the first to turn his back on *a priori* philosophical speculation about our knowledge of the external world, Russell is one of only a few philosophers to have attempted, using all the resources of modern logic and modern psychology, to provide a detailed, scientific account of what we know and how we know it. In resisting the lure of *a priori* (non-scientific) conceptual analysis, he can be seen in retrospect at least as attempting to rescue epistemology from what in 'Things and their Place in Theories' Quine calls "the abyss of the transcendental" (p. 23), indeed as paving the way for Quine's own naturalistic epistemology. One can debate whether Russell succeeded in reconstructing epistemology as a branch of natural science and whether he proceeded in a genuinely scientific fashion just as one can debate whether Quine managed to pull off the trick. But there can be no denying his exceptional contribution to naturalistic epistemology as a going concern in the twentieth century.

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<sup>29</sup> 'Vagueness', p. 154. See also *An Analysis of Matter*, p. 28, *An Outline of Philosophy*, p. 234, *Human Knowledge*, pp. 9, 196, and *My Philosophical Development*, p. 78.

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SOLITARY, POOR, NASTY, BRUTISH, AND SHORT  
RUSSELL'S VIEW OF LIFE WITHOUT  
WORLD GOVERNMENT\*

CHAD TRAINER

THE JUDGMENTS PASSED BY BERTRAND RUSSELL in his *History of Western Philosophy* on the philosophy of Thomas Hobbes are primarily negative. However, the type of politics Hobbes advocated that countries adopt domestically Russell advocated that countries adopt internationally. Though others have noted this analogy, including Russell himself, more needs to be made of the Hobbesian that Russell was capable of being when it came to international relations, especially since Russell was the sort of rebellious reformer who probably would not be tolerated by the sovereign of a Hobbesian state.

I.

THE FOUNDATION OF HOBBS' POLITICAL THINKING is that "the natural state of men, before they entered into society, was ... a war of all men against all men", with Hobbes citing native Americans as an example of this principle.<sup>1</sup> Justice does not exist in such circumstances, and "the time...wherein men live without ... security [is] ... solitary, poor, nasty, brutish, and short". Lamenting this "war with every other man" as "the greatest evil that can happen in this life", Hobbes envisioned our refuge in a governed and legal society where we would have a greater chance of achieving our interests than in a state of nature.<sup>2</sup>

Hobbes advocated a complete concentration of power in the sovereign both because the separation of powers is thought to diminish power's efficacy and because, however much power corrupts and is subject to abuse, such corruption and abuse only in-

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<sup>1</sup> *Elements of Law* Pt. I, Ch. 14, § 11; *De Cive* VIII, 3; *Leviathan* 1, 13, at *De Cive* I, 13; *De Cive* VIII, 10.

<sup>2</sup> *Leviathan* 1, 13; 2, 30; *De Cive* I, 2 and *Leviathan* 1, 11.

crease in proportion to the number of parties empowered. Moreover, for Hobbes it is inconceivable that the interests of the sovereign and interests of the subjects diverge.<sup>3</sup> The interests of the people are best served by their having an absolute sovereign.

The sovereign's absolute rights include "the absolute use of the sword in peace and war, the making and abrogating of laws, supreme judicature and decision in all debates judicial and deliberative, the nomination of all magistrates and ministers, with the rights contained in the same", and they ought to re-enforce each other and not be divided. For example, the power of the judiciary is vain without the power of executing the laws. Hobbes thought democracy's supposed superior liberty is really just its proximity to the state of nature and war of all against all. If supporters of democracy would only grasp this, they would abhor the liberty of democracy as "worse than all kinds of civil subjection whatsoever".<sup>4</sup>

In his *History of Western Philosophy*, Russell was persuaded by few of Hobbes' points and took him to task, observing that:

[Hobbes] always considers the national interest as a whole, and assumes tacitly, that the major interests of all citizens are the same. He does not realize the importance of the clash between different classes, which Marx makes the chief cause of social change.... In time of war there is a unification of interests, especially if the war is fierce; but in time of peace the clash may be very great between the interests of one class and those of another. It is not by any means always true that, in such a situation, the best way to avert anarchy is to preach the absolute power of the sovereign. Some concession in the way of sharing power may be the only way to prevent civil war. This should have been obvious to Hobbes from the recent history of England (1945 pp. 556-7).

Russell believed that the gravest danger of the state is that its paramount objective is power for its own sake. Given this priority, he says: "It is of the essence of the State to suppress violence within and facilitate it without", maintaining that "The tyranny of the holders of power is a source of needless suffering and misfortune to very large sections of mankind". Democracy, by preventing the

<sup>3</sup> *Leviathan* 2, 30; *Elements of Law* Pt. II, Ch. 5, §§ 4-8.

<sup>4</sup> *Elements of Law* Pt. II, Ch. 1 § 8-13. *De Cive* X, 8. *Elements of Law* Pt. II, Ch. 1 § 16.

concentration of power in the hands of the few, has "... in addition to stability ... has the merit of making governments pay some attention to the welfare of their subjects – not, perhaps, as much as might be wished, but very much more than is shown by absolute monarchies, oligarchies, or dictatorships." In response to the BBC's Woodrow Wyatt's query about the quality of the West's democratic systems, Russell touted the checks on their power as their primary merit.<sup>5</sup>

Moreover, while he agreed with Hobbes that the earlier sort of anarchic existence is worse than legally governed societies, Russell preferred even anarchy to efficient fascism, arguing that "A state may ... be so bad that temporary anarchy seems preferable to its continuance, as in France in 1789 and Russia in 1917". And the perils that accompany the exercise of power can only be compounded by a minority's incompetent approaches to governing.<sup>6</sup>

Within the realm of Russell's own thinking, though, a sharp contrast can be found between his political thinking on domestic and foreign policy. In domestic matters, Russell expressly preferred erring on the side of anarchy rather than tyranny, but when it came to international politics, Russell believed that "only one thing can make world peace secure, and that is the establishment of a world government with a monopoly of all the more serious weapons of war" (1952, p. 277).

A Utopian vision? Not to Russell. He saw the idea of world government as being no less fantastic than the idea of national governments had been during the Middle Ages. In *Common Sense and Nuclear Warfare*, Russell says:

All this, however utopian it may appear, is only a close parallel to what happened in national States as a result of the invention of gunpowder. In the Middle Ages throughout Western Europe powerful barons in their castles could defy the central Government. It was only when artillery became able to destroy castles that the central Government was able to control feudal barons. What gunpowder did in the late Middle Ages, nuclear weapons have to do in our time. I do not

<sup>5</sup> Russell 1916, pp. 43, 45; Russell 1917b, p. 23; Russell 1938, p. 132; Russell 1960, pp. 81-2.

<sup>6</sup> Russell 1916, p. 34; Russell 1945, p. 556; Russell 1938, p. 71.

mean that they have to be actually employed. Gunpowder does not often have to be employed to enforce the authority of national Governments against internal criminals.... Submission to a Central Authority may be as distasteful as submission to the king was to medieval barons, but it is in the long run equally necessary" (pp. 69, 71).

Nor did he think because a world government was most likely to be a tyranny, at least at first, that this made the idea unacceptable. In *Unpopular Essays*, he wrote:

In the history of social evolution it will be found that almost invariably the establishment of some sort of government has come first and attempts to make government compatible with personal liberty have come later. In international affairs we have not yet reached the first stage, although it is now evident that international government is at least as important to mankind as national government (p. 142).

Russell's political activism ranged from supporting the West's Cold War priorities to campaigning for nuclear disarmament. The fixed stars in this ever-changing constellation of his political stances, though, were his support for world government and his view that "The only legitimate use of force is to diminish the total amount of force exercised in the world" (1917b, p. 70).

During the 1920s, 30s and 40s, Russell believed that "Far the easiest road to international government would be the unquestionable preponderance of some one State. That State would then be so strong that no other would venture to quarrel with it, and it might for its own purposes forbid the others to fight among themselves...." And in a 1945 article for *Cavalcade*, he more specifically remarked: "I would rather see the United States conquer the whole world and rule it by force than see a prolongation of the present multiplicity of independent Great Powers." However, when Russia acquired nuclear weapons, Russell retreated from the idea of establishing a world government by force and began looking more and more to the United Nations to serve the function of a world government with "sole possession of the major weapons of war".<sup>7</sup>

For example, in a July 14, 1960 letter to *The Guardian*, Russell declared: "The road to World Government, if it is to become possible, must be through the United Nations, enlarged and strength-

<sup>7</sup> Russell 1923, p.75; Russell 1983 Vol. 2, p.313; Pigden 2003, p.492.

ened, and not through rival military alliances" (Perkins 2002, pp. 223-5). And in his *Autobiography* he said:

The ultimate goal will be a world in which national armed forces are limited to what is necessary for internal stability and in which the only forces capable of acting outside national limits will be those of a reformed United Nations. The approach to this ultimate solution must be piecemeal and must involve a gradual increase in the authority of the United Nations or, possibly, of some new international body which should have sole possession of the major weapons of war. It is difficult to see any other way in which mankind can survive the invention of weapons of mass extinction" (1969, p. 268.).

But whether advocating world government via the US or the UN, Russell's view was that "Every argument that [Hobbes] adduces in favour of government, in so far as it is valid at all, is valid in favour of international government. So long as national States exist and fight each other, only inefficiency can preserve the human race. To improve the fighting quality of separate States without having any means of preventing war is the road to universal destruction" (1945, p. 557). Hobbes' reasons for replacing the state of nature with the sovereign were Russell's reasons for replacing this planet's individual autonomous states with world government.

Interestingly though, it is at just this level of international relations that Hobbes despaired of a legal society with power concentrated in a sovereign; whereas it is precisely at such a level that Russell seemed particularly sanguine about seeing power so concentrated. True, the whole notion of international law was not as common in Hobbes' time as it is in our own. But Hobbes' despair on this front is more attributable to the darkness of his overall outlook than to the conventional wisdom of his day. Over a century earlier, the University of Salamanca's Francis of Vitoria (1480-1546) had composed his landmark tract defending the native Americans in the light of the *ius gentium*, or law of nations. Francis Suárez (1548-1617) further developed the concept of international law, and Hugo Grotius (1583-1645) remains renowned to this day for his *De Jure Belli ac Pacis*'s contribution to the field.

## II.

WERE RUSSELL ALIVE TODAY, it is interesting to consider how he would respond to charges that, while he may have been duly cynical regarding authority figures when it comes to countries' do-

mestic matters, he was unduly optimistic regarding a world government's authority figures, especially considering that he would have a world government enjoying a monopoly on military power.

I suspect that Russell would have encouraged us to understand the contrast as being not so much between domestic politics and foreign policy as between civil and military power. Russell was in favor of this dissociation of civil and military power on the grounds that "The greater modern States are already too large for most civil purposes, but for military purposes they are not large enough, since they are not world-wide" (1916, pp. 71-2). I think Russell would also have hastened in directing us to understand that for Hobbes the power of the sovereign is absolute,<sup>8</sup> whereas Russell saw himself as preferring the establishment of a world government by consent rather than by force (1948). He further advocated much narrower powers for a world government than anything involved in Hobbes' sovereign. For example, in *Common Sense and Nuclear Warfare*, Russell spoke of how "There will need to be, as in any Federation, a well-defined Constitution, deciding which powers are to be federal. It should be understood that these powers must be only such as are involved in the prevention of war. There must be no interference by the Federal Authority with religion or economic structure or the political system" (1959, p. 68).

At the beginning of World War I, Russell's view was that an international council charged with arbitrating all disputes between nations should rely on moral force alone, for fear that if it tried to enforce its verdicts with armed force, the world was likely to become embroiled in warfare as a result. In taking this view, Russell assumed that such a council would be armed with the forces of its member nations rather than with an international force directly under its control. Such a situation, he thought, would lead to coalitions of belligerents defying the council and neutral states refusing to take part in opposing them so the result was more likely to be a world-wide war than to any other outcome. However, by the following year, he had come to the view that a truly international force assembled under the direct control of the council would be the best way to maintain peace.<sup>9</sup>

<sup>8</sup> *Elements of Law* Pt. II, Ch. I §§ 8-13; *De Cive* VI, 13; *Leviathan* XVIII.

But what assured Russell that a world government with a monopoly on military power, whether it is the United States or the United Nations, would not seek power for its own sake much as national governments do? Russell's faith in a world government's police power seems to contrast quite sharply with his grim assessments of police power within a country. In *Political Ideals*, Russell made the issue seem as simple as "Just as the police are necessary to prevent the use of force by private citizens, so an international police will be necessary to prevent the lawless use of force by separate states" (1917b, p. 71). And yet, in his 1938 work *Power*, Russell made the point that, even in democracies, "individuals and organisations which are intended to have only certain well-defined executive functions are likely, if unchecked, to acquire a very undesirable independent power. This is especially true of the police" (p. 192). So why would this not be equally true of an international police?

In the ninth chapter of *Common Sense and Nuclear Warfare*, Russell addressed this issue when he said:

Whenever an international armed force is suggested, many people at once raise objections which are equally applicable to municipal police forces. They suggest that such an armed force might make a military revolution and establish a tyranny over the civil authorities. In theory this is possible in the case of national armed forces, and in the less settled parts of the world it sometimes occurs. But there are well-established methods, both in Communist and in non-Communist countries, by which, not only in Russia and in the United States, but even in Nazi Germany, the civil authorities have maintained their supremacy. I see no reason to doubt that these methods would be equally effective in the international sphere (1959, p. 70; see also 1961a, p. 264 and 1961b, pp. 86-87).

And in "Ideas That Have Helped Mankind", Russell stated:

I find it often urged that an international government would be oppressive, and I do not deny that this might be the case, at any rate for a time, but national governments were oppressive when they were new and are still oppressive in most countries, and yet hardly anybody would on this ground advocate anarchy within a nation ... as in the course of the past 5,000 years men have climbed gradually from

<sup>9</sup> Russell 1915, 1916; Lippincott 1990.



the despotism of the Pharaohs to the glories of the American Constitution, so perhaps in the next 5,000 they may climb from a bad international government to a good one (1950, pp. 142-3).

In 1945, Russell expressed his preference for "all the chaos and destruction of a war conducted by means of the atomic bomb to the universal domination of a government having the evil characteristics of the Nazis".<sup>10</sup> Yet in the 1950s and 60s, Russell was horrified by claims like those of Eleanor Roosevelt and Sidney Hook that the extinction of the human race would be better than life under Soviet rule.<sup>11</sup>

For example, in the early 1960s Russell inveighed against anti-Communists who invoked Patrick Henry's "Give me liberty, or give me death!" to support their claims that "a world without human beings would be preferable to a Communist world". Russell countered that Patrick Henry's words were "right and proper" in his day because a loss of American lives was an inevitable price to be paid for triumph over British hostility, so "his death might promote liberty". However, Russell contended, "ordered liberty such as Patrick Henry wanted" would never result from a nuclear war.<sup>12</sup>

Significantly, then, if forced to choose between "peace under a tyranny" and "bloodshed under a democracy," Russell would have opted for the former. In "World Government: By Force or Consent?" in the September 4, 1948 edition of *The New Leader*, Russell acknowledged that "I should wish the advocates of world government to realize that its greatest merit, namely the prevention of war, does not depend upon its being established by general consent, but upon its possession of obviously irresistible armed force."<sup>13</sup> And besides, he viewed history's most horrific regimes as having had a sufficiently brief duration so as to make the long-term preservation of the human race worthwhile. For example, he noted that Genghiz Khan and Kublai Khan were only a generation apart.<sup>14</sup>

<sup>10</sup> "Humanity's Last Chance", in *Cavalcade*, October 20 1945, See 1983, p. 312.

<sup>11</sup> Russell 1969, pp. 146-7.

<sup>12</sup> Russell 1961b, pp. 42,43.

<sup>13</sup> I am indebted to Ray Perkins for bringing this source to my attention.

This preference has been aptly characterized by J.C.A. Gaskin as "pure Hobbes".<sup>15</sup> Predictably, Russell would have retorted that "peace under a tyranny" and "bloodshed under a democracy" do not exhaust the options. Rather there is the third option, which he in fact favored, wherein a world body governs countries federally and voluntarily.

The present writer's reservation about Russell's imaginary retort here is that even Hobbes would have been quite fine with voluntary and democratic institutions, provided they acted as a "unitary" sovereign that did not share power with some other governmental unit such as a monarch or other assembly. It is, however, precisely when ideal choices are *not* available that the resulting tough choices provide an index to a person's true politics. In that context, Russell was prepared, with Hobbes, to make the pragmatic choice of tyranny over anarchy.

### III.

SINCE RUSSELL WROTE ON THESE SUBJECTS, many changes have taken place in the world, including some international agreement on limiting nuclear weapons, the most important being the 1968 Nuclear Non-Proliferation Treaty (NNPT). However, the UN has not proven to be the force for world government many had hoped it would, much less has it become the "sole possessor of the major weapons of war" that Russell envisioned. When playing peacekeeper in world trouble spots, it frequently has only enough authority to defend itself, and hardly even has the power to collect dues from its members, most notoriously, from the US. Nor has the US, now that it is once again the dominant world power, fulfilled Russell's early hopes that it would create a world government with its unique position in the world.

As noted above, the US is reluctant even to pay its UN dues, and especially during the Bush administration, has withdrawn from or declined to participate in international treaties at an alarming and unprecedented rate, in particular from treaties that aim to make the world safer from war. Among others, it has withdrawn from the

<sup>14</sup> Russell 1959, p. 74-6, Russell 1961b, p. 43, Russell 1969, p. 59; Russell 1969, pp. 146-7.

<sup>15</sup> Gaskin 1994, p. xlii.

1972 Anti-Ballistic Missile Treaty, refused to ratify the Comprehensive Test Ban Treaty, rejected the Landmine Treaty of 1997, opposed a UN agreement to restrict international trade in small arms (the only UN member to do so), rejected the Kyoto Agreement on Global Warming, and opposed the international criminal court, demanding immunity of all US citizens from prosecution by it. And in threatening to deny Iran (which is an NNPT signatory) civilian nuclear technology while agreeing to provide India (which is not an NNPT signatory) with civilian nuclear technology, both in defiance of the Nuclear Non-Proliferation Treaty, the US threatens to destroy, or at least leave that treaty agreement as well. If anything, the US is kicking to pieces whatever international agreements on limiting war there once were.<sup>16</sup>

At the end of the Cold War, the world seemed suddenly safer than it had at anytime since the end of WWII, but that greater safety is not so apparent today. While the Cold War's strategy of Mutual Assured Destruction (MAD) has receded into the background of international relations, the threat of nuclear, chemical, and biological weapons has not, especially with the chance that North Korea and Iran might soon have such weapons (along with Israel, India, Pakistan, China, Russia, France, Britain, and the US, who already have them). In fact, just before his death, Joseph Rotblat argued that the threat of a new nuclear arms race is more possible now than ever before given the relaxation about arms control after the end of the Cold War, the Bush administration's 2002 Nuclear Posture Review, which declared that nuclear weapons should now be treated like any other weapons in the military arsenal, that is, used whenever militarily appropriate, and the fact that the US is now building new nuclear weapons that will need to be tested. Similarly, the *Bulletin of the Atomic Scientists*, with their famous Doomsday Clock set to just a few minutes before midnight to represent the threat of nuclear holocaust, moved the minute hand back to 10 minutes to midnight in 1990 from 3 minutes to midnight in 1984, and then back again in 1991 to an unprecedented 17 minutes to midnight, in the same sense of safety others felt at the end of the Cold War. Since then, however, citing circumstances similar to those

<sup>16</sup> Coates 2003, p. 42.

cited by Rotblat, they have moved the minute hand steadily forward again until it now stands at 7 minutes to midnight, in the same position at which it began when the clock first appeared on the *Bulletin's* cover in 1947.

Russell's disappointment with the ability of the United States or United Nations to effectively serve as a world government, combined especially with what Joseph Rotblat and the *Bulletin of the Atomic Scientists* see as a recent heightened threat of nuclear war, would probably have prompted Russell, were he alive today, to contend that now, more than ever, even with its attendant risks, efforts should be directed toward power being concentrated in a world government.

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## Book Review

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### A COPIOUS HARVEST: FREGE AND CARNAP AT JENA

JAMES CONNELLY

Review of Erich H. Reck, Steve Awodey, Gottfried Gabriel, and Gottlob Frege, *Frege's Lectures on Logic: Carnap's Student Notes, 1910-1914*, Open Court Publishing, 2004.

PRIOR TO HIS EMERGENCE as one of the most significant figures in analytic philosophy, Rudolf Carnap attended several courses offered between 1910-1914 by an aging Gottlob Frege at the University of Jena, where the latter had been a professor of logic and mathematics since 1874. The recent publication of Carnap's notes from these courses, as *Frege's Lectures on Logic: Carnap's Student Notes, 1910-1914*, is a significant event in Frege scholarship in particular and the history of analytic philosophy in general. In addition to being of intrinsic interest as a documented philosophical interaction between these two seminal thinkers, the notes also provide extensive insight into the evolution of Frege's logical system and the content of his teaching following Russell's 1902 discovery and communication of his eponymous paradox to Frege.

As the editors of *Carnap's Notes* point out, the volume sheds valuable light on those aspects of Frege's thought that he felt could be retained despite the failure of his logicist project, thus indicating what he saw as the harvest of his life's work (p. 4). The volume also illuminates an important source of Frege's influence within the analytic tradition, namely, Carnap's absorption and subsequent dissemination of Frege's ideas as he encountered them in these lectures.

#### I

IN ADDITION TO THE TRANSCRIPTIONS of Carnap's notes from three separate lecture courses, *Begriffsschrift I*, Winter 1910-11, *Begriffsschrift II*, Summer 1913, and *Logic in Mathematics*, Summer 1914, the volume contains two appendices meant to be part of either *Begriffsschrift I* or II (it is not clear in which lecture they belong). The transcriptions are accompanied by two introductory essays, which provide key historical, biographical, logical and philosophical background.

The first introductory essay is written by Gottfried Gabriel, the editor of the original German version of these lecture notes.<sup>1</sup> Gabriel compares the exposition found in the notes with those occurring in Frege's *Begriffsschrift* (1879) and *Grundgesetze der Arithmetik* (*The Basic Laws of Arithmetic*), Vols. I (1893) and II (1903). He finds that while the exposition is by and large congruous with that offered in *Grundgesetze* – right down, for instance, to the employment of identical code numbers for the relevant laws and theorems – there are some important differences.

As in the *Basic Laws*, Frege employs additional rules of inference beyond the *Begriffsschrift*'s single *modus ponens*, and there is a corresponding reduction in the number of the *Begriffsschrift*'s basic laws. In fact, the number of basic laws is reduced even further in the notes than in the *Grundgesetze*; here Basic Laws IV, V and VI are all eliminated. While there is "no obvious reason" (p. 3) to dispense with Basic Law IV, Gabriel notes that the elimination of Basic Laws V and VI corresponds to Frege's eliminating value ranges and the description operator, reflecting his retreat from the more constructionist ambitions of his logicism following Russell's identification of the contradiction inherent in it.

Further evidence of Frege's retreat from the logic of the *Grundgesetze* can be found in his analysis of the notion of ordering in a series, which dispenses with value ranges. Other such instances include his use of the term 'content-stroke', as he had in the *Begriffsschrift*, in lieu of his later phrase, 'the horizontal'. Despite this change in terminology, however, "in substance ... the conception of the *Basic Laws* dominates", the content-stroke being characterized as "a special function of first-level, whose value for the argument 'the true' is the true and for all other arguments is the false" (p. 4).

Other highlights of Gabriel's essay include a discussion of how the notes support but also belie Carnap's later and somewhat controversial insistence that Frege defended the viability of logicism in these lectures. Gabriel notes that though Frege had "quietly drawn

<sup>1</sup> The English version, unlike its German counterpart, contains the material included under the title *Logic in Mathematics*, which is the first publication of this material in any language (p. 1), although it "is related to the *Nachgelassene Schriften* (Frege 1983) item by the same name and should be compared to it" (p. 34).

the consequences" of Russell's paradox by eliminating value-ranges, he is nevertheless silent about the antinomy, a fact which may have "led Carnap to the premature conclusion that it presented no problem for him" (p. 7). His "casting doubt" at the outset of the third lecture course "on the representability of mathematical induction ... in purely logical terms" (p. 6) confirms that though Frege treated the *methods of proof* in mathematics and geometry as logical, he did not in these lectures defend the stronger thesis that arithmetic (or geometry) is reducible to logic. Despite his evident abandonment of logicism, Frege nevertheless seems to have continued to conceive of numbers as non-logical objects and "attributions of number as statements about concepts" (p. 7).

The second introductory essay, by Eric H. Reck and Steve Awodey, explicates key ideas and notation prominent in the logical system developed in the notes and provides sketches of Frege as a person and lecturer by people who came into contact with him while Carnap studied at Jena. Carnap's own reflections are included as well as Wittgenstein's. Frege appears as a somewhat frail and unapproachable older gentleman, possessed of an unquestionable charisma, perhaps as a result of the keen intellect and immense passion for logical and scientific work he continued to display despite his advancing years.

## II

THE FIRST LECTURE COURSE, *Begriffsschrift* I, resembles the sort of introduction to Frege's key logical and semantic contributions one might get in any contemporary North American philosophy department. Frege begins by explaining such rudimentary elements of his notation as the content-, judgment-, conditional- and negation-strokes, showing how these operate as functions from the truth values of the component sentences they take as arguments to the truth values of the compound statements formed from them, and how other truth-functions, such as conjunction and disjunction, may be built up in turn out of these more primitive ones.

Frege presents several key rules of inference, the most basic of which is transportation (or contraposition (p. 160)), in which an upper term negated takes the place of a lower term, and the lower term negated takes the place of the upper term. Other more intricate forms of inference, like 'cut' and 'negation', are also introduced

(pp. 33, 60-63). Frege then analyzes rules of inference involving generality, culminating in the classical square of opposition presented in his own function-theoretic and quantificational notation.

Frege notes that the propositions displayed in the square of opposition are identified only for the purposes of showing the connection between his own system and that of traditional logic and that the distinction between subject and predicate, characteristic of the traditional Aristotelian analysis of these forms of judgment, "does violence to the nature of things" (p. 71). The course concludes with a discussion of such semantic distinctions as that between meaning and sense as it applies in the cases of proper names, sentences, concepts, and indirect discourse, as well as such logical distinctions as that between first and second order functions. Interestingly, Frege insists on treating concept words as names of certain sorts of quasi-objects, i.e., concepts (p. 74), despite continuing to adhere to a rigorous distinction between concepts and objects and despite eliminating concept-extensions.

Following *Begriffsschrift* I are two appendices. In the first, Frege analyzes the ontological proof of the existence of God, noting that existence is a 'feature' (*Beschaffenheit*) rather than a 'characteristic' (*Merkmal*) of a concept; in the second he analyzes statements of number as statements about concepts. The appendices are followed by *Begriffsschrift* II. It begins by recapitulating some of the basic logical and semantic notions covered in *Begriffsschrift* I, building on these notions to present a more systemic and advanced treatment of formal deduction.

Frege first shows how his notation can be used to define two key mathematical notions, namely, the continuity of an analytic function at a particular point and the limit of a function for positive arguments increasing towards infinity (pp. 88, 91). Following a four page gap in the notes, which the editors conjecture is where Frege introduced Axioms I and II, he then introduces Axiom III, using it to derive such properties of identity as Leibniz's law, reflexivity, and symmetry (pp. 37, 93-97). This is followed by two proofs, the first that two numbers are equal if each is greater than the other when increased by an arbitrarily small amount and the second that limits are unique. These examples are provided, Frege says, for the purpose of showing "how one can conduct proofs with our notation" (p. 98).

Frege rounds out *Begriffsschrift* II by stressing the importance of rigour in mathematical proof, along with relevant distinctions between the psychological and the logical, functions and their values, real and apparent variables, as well as signs and what those signs signify. He considers several examples from differential and integral calculus, employing them to show that failure to maintain the requisite philosophical distinctions leads to the result that "one contradicts oneself continually" (p. 133). He then concludes by recommending the various questions considered to the student "for further reflection" (ibid.).

*Logic in Mathematics*, the third lecture course, picks up where *Begriffsschrift* II leaves off, that is, in a more philosophical vein than the earlier material, which consists, by and large, of a technical, if rudimentary, exposition of Frege's logical system. Frege opens the course asking: "Are the inferences in mathematics purely logical? Or are there specifically mathematical inferences that are not governed by general laws of logic?" (p. 135) He then examines a proof of the proposition ' $(a + b) + n = a + (b + n)$ ' via mathematical induction, which he identifies as an inference of the later, specifically mathematical sort (ibid.). After a discussion of this proof, Frege concludes that "every mathematical inference is analyzed into a general mathematical theorem or axiom and a purely logical inference" (p. 134), thus rendering questionable Carnap's claim that at the time of these lectures Frege adhered to the logicist program.

Frege goes on to detail the role played by logical inference within the sort foundational project which he *does* intend to endorse, which involves supplementing purely logical laws with "axioms, postulates, and perhaps definitions" (p. 138). These, he maintains, should be limited to as few a number as possible in the interest of discovering "that kernel out of which all of mathematics can be developed" (p. 137).

Following cursory remarks on postulates and axioms, Frege shifts to a detailed discussion of definitions, which he characterizes as "stipulations that a group of signs can be replaced by simple signs" (p.139), and which he argues are "logically superfluous, but psychologically valuable" (p.140). The discussion leads him to consider some contemporary views of definition and to a critical discussion of various putative definitions of the concept of number reminiscent of that undertaken in the *Foundations of Arithmetic*. In

particular, Weirstrass's definition that "a number is a group of similar things ... (and) a numerical magnitude results from the repeated positing of similar elements," comes up for consideration, leading Frege to remark that "(a)ccording to Weierstrass a railroad train would be a number...(which) now comes racing along from Berlin" (pp. 139-144).

In the remainder of the course, Frege develops some familiar themes in an extended discussion of distinctions between the psychological and the logical, the sense and meaning of proper names and sentences, concepts and objects, as well as first and second order functions. Frege also repeats ideas developed in *Begriffsschrift* II on the importance of distinguishing between a function and its value, particularly when one seeks to identify a complex function as comprised of two simpler component functions, e.g., '(1 + 2x)' from '(1+ 2x)' and 'z' (p. 154).

Some important ideas introduced here include Frege's insistence on the importance of clear and sharp boundaries for concepts, and on the philosophically essential role played by elucidations: "what a function is cannot be defined, it cannot be reduced logically to something more simple; one can only hint at it, elucidate it" (p. 152). The course concludes with reflections on the distinction between direct and indirect proof, with Frege giving examples from geometry which show that false propositions can be employed in constructing sound proofs, provided those propositions are never asserted but are rather explicitly taken throughout the proof as antecedents of conditional statements.

### III

I HAVE TRIED TO GIVE A SENSE of the quality and content of the volume by tracing a path through it, highlighting some of the elements which seem to me most interesting and relevant. Specialists in Frege's logical and mathematical work are likely to discover much of value in the volume which has not been touched on here at all, or else very briefly – for example, Frege's discussion of indirect proof vis-à-vis non-Euclidean geometry in the latter portions of the course on *Logic and Mathematics*. By contrast, specialists in philosophy of language are likely to be intrigued by the various discussions of key themes in Fregean semantics developed throughout the volume, which are worth examining both in connection with their

reception by Carnap and in light of developments in Frege's system after the failure of his logicist program. For example, Carnap's claim in *Meaning and Necessity* that Frege held concept-extensions to be meanings of concepts is contradicted in the notes and examined by the editors. Even a non-specialist will benefit from the editors' and Frege's own presentation of key logical and semantic innovations and from the wealth of historical and biographical information concerning both Carnap and Frege. The volume is a first rate piece of scholarship which I recommend to anyone working on or interested in Frege in particular or the history of analytic philosophy in general regardless of their specific level of expertise.

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## DISCUSSION NOTES

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### APPRECIATING THE VARIETIES OF ANALYSIS A REPLY TO ONGLEY

MICHAEL BEANEY

In 'What is Analysis?' (2005), John Ongley reviews my entry on analysis in the *Stanford Encyclopedia of Philosophy*. On the whole, he gives a fair summary of the survey of conceptions of analysis in the history of philosophy that I offered, and his criticisms raise important issues. However, he fails to do justice to my account in one fundamental respect, and this gives those criticisms an inappropriate edge. As I state explicitly at the beginning of my entry, one of my main aims was to give a sense of the varieties of analysis that can be found in the history of philosophy. It was not my aim to pigeon-hole philosophers into particular categories, which is what many of Ongley's criticisms seem to suggest. Of course, some kind of conceptual framework must be developed to elucidate the various forms of analysis and their interconnections, but it was not my intention to impose a rigid taxonomy. Analytic methodology in the history of philosophy is a dense and tangled forest, and it has too often been assumed that the trees are more or less the same. In recent years there have been fine studies of individual philosophers' conceptions and practices of analysis, but few attempts to see the wood as a whole. Ongley makes pertinent points in relation to individual philosophers, but in offering them up as criticisms of my account, mischaracterizes my project.

In section 1 of his review, Ongley notes correctly that I distinguish three main modes of analysis – decompositional, regressive and interpretive. But he then remarks that "In general, [Beaney] says, *analysis* breaks a concept or proposition down into elements that are used in *synthesis* to justify or explain it" (p. 33). This is not well expressed, and is not what I say; at best, it just reflects the decompositional conception. One of my aims in writing about analysis has been to try to break the stranglehold that the decompositional conception has had on philosophical methodology in the modern period, and in discussions of twentieth-century analytic philosophy, in particular. What I call 'regressive' analysis, understood as the

process of working back to first principles (by means of which something can then be justified or explained in a corresponding process of 'synthesis'), was the dominant conception in the pre-modern period, and is still influential today. (Such a conception is illustrated, for example, in Russell's 1907 paper, 'The Regressive Method of Discovering the Premises of Mathematics'.) Interpretive analysis, too, I argue, is an important mode of analysis, which came to prominence in early analytic philosophy in the emphasis placed on translating propositions into 'correct' logical form, but which also has been implicitly involved in practices of analysis throughout the history of philosophy and science. Although he recognizes these three modes, Ongley fails to appreciate that the assumption that 'analysis' essentially means conceptual decomposition is what most needs to be questioned in understanding the nature of analytic philosophy (in my view). In his final section, he talks of a 'revisionist turn' in the recent history of early analytic philosophy movement, a turn which my work has helped foster. But it is my attack on this assumption that I would want to single out as fundamental in my work.

This is not to say, however, that the decompositional conception is not important, or even central, in many projects of analysis. Rather, when we look at actual practices of analysis, we must recognize that other conceptions may also be involved. Ongley notes this, too, in the first section of his review and in the first paragraph of the second section (p. 34). But he then seems to forget it in the rest of his essay. In the light of his criticisms, I can see now that I should have stressed it more throughout my entry, but as I have said, my main aim was to clarify some of the key forms of analysis and not to do justice to any individual philosopher's conception or practice. For example, in my discussion of Kant (which I admit is far too brief), I was mainly concerned to illustrate the decompositional conception that reached a highpoint in the Leibnizian/Kantian conception of an 'analytic' truth as one in which the predicate is 'contained' in the subject. I had not meant to imply that this was the only conception of analysis in Kant's philosophy. Indeed, on the contrary, I have elsewhere indicated some of the complexities involved in Kant's actual talk of 'analysis' and the 'analytic' method (Beaney 2002). As Ongley quite rightly says (p. 41), Kant also has a regressive conception of analysis. I also agree with Ongley (p. 42)

that sorting out the sense in which the *Critique of Pure Reason* is a 'synthesis' and the *Prolegomena* is an 'analysis' is a key question for Kant scholarship.

Another issue that Ongley raises in his discussion of Kant is that of whether any analytic method is apriori or not. In fact, my failure to address this issue is the main complaint that he makes in his review. In section 2 he writes:

Beaney does not claim that any philosophical method is apriori—in fact, he does not consider whether they are apriori or not. What he does, for the most part, is describe various instances of analysis as 'regressive', 'decompositional', or 'interpretive'. But by simply attaching one of these labels to a method of analysis, we do not learn the details of how the method works, and it is the details that will tell us such things as whether it is empirical or apriori, that is, whether or not empirical propositions must be assumed in order to analyze some concept or proposition. With his own approach to analysis, Beaney cannot answer such questions. This is the major limitation of his approach. (p. 36)

Ongley is right that I do not adequately address the issue of the apriority of analytic methodology, and he has persuaded me that I need to say more about it in my subsequent work. But part of my target in attacking the assumption that analysis is essentially conceptual decomposition is indeed the idea that analysis consists in uncovering the meanings of terms by some apriori method. Ongley comments on the issue at various points in his review, and I found his remarks pertinent and helpful.

Nevertheless, this concession aside, Ongley is bizarrely uncharitable in the passage just cited. For the impression is given that my 'approach' is simply to label different instances of analysis as 'regressive', 'decompositional' or 'interpretive'. This is a caricature of the crudest kind, which is reflected elsewhere in Ongley's review. In opening section 4, for example, he writes (p. 39): "Kant's method of analysis is likewise 'decompositional' according to Beaney. I hope it is becoming apparent how limited the use of these metaphorical labels to describe types of analysis is." Fortunately, however, this caricature is contradicted by Ongley's own summary of my survey, a summary which provides at least some details of specific methods of analysis; and many more details are provided in my survey itself. Ongley makes use of my terminology, too, in pointing out (correctly, as just noted) that Kant has a regressive as

well as a decompositional conception of analysis: expressing it like this neatly encapsulates a feature of Kant's work which has not been sufficiently recognized. Of course, 'regressive', 'decompositional' and 'interpretive' are only terms that represent the first step in going beyond simple talk of 'analysis', and one needs to look at the details of how any given method works to understand it properly. I find it baffling that someone could have read my entry on analysis and thought that all I was doing was offering a tripartite taxonomy, not least because of my emphasis on the way that all three modes are typically implicated in any actual practice of analysis. The conceptions of analysis I distinguish are intended as tools to open up our thinking about analysis, and not as a classificatory device to block further understanding.

In fact, in elaborating my account, I draw all sorts of other distinctions (which can be found in the literature) – between whole-part (decompositional) analysis and function-argument analysis, between 'logical' or 'same-level' analysis and 'metaphysical' or 'new-level' analysis, between 'analysis' and 'quasi-analysis', between reductive analysis and connective analysis, and so on. I also discuss related conceptions such as that of Plato's method of division and Carnap's notion of explication, and issues such as the paradox of analysis and Ryle's idea of a 'category-mistake'. Ongley mentions some of this (pp. 47, 50-1), which makes it even more surprising that he should think that I am essentially engaged in a pigeonholing exercise.

Ongley and I share an interest in the history of early analytic philosophy, and it is here, in particular, that Ongley's assumption that I am essentially pigeonholing distorts his discussion of my account, and motivates some unwarranted complaints. In section 5 of his review, he writes:

Examining the twentieth century, Beaney begins with a general characterization of 20th century philosophical analysis. "What characterizes analytic philosophy as it was founded by Frege and Russell," he says, "is the role played by *logical* analysis, which depended on the development of modern logic. Although other and subsequent forms of analysis, such as linguistic analysis, were less wedded to systems of formal analysis, the central insight motivating logical analysis remained." Beaney admits that this characterization does not fit Moore or one strand of analytic philosophy, but thinks that the



tradition founded by Russell and Frege is analytic philosophy's central strand. (p. 44)

At the level of conversational implicature, this is misleading. For it makes it look as if I am offering a general definition, but then finding myself forced to admit an important exception. What I actually do in beginning my section on conceptions of analysis in analytic philosophy (note the use of the plural here) is criticize the assumption that decompositional analysis is what characterizes analytic philosophy (since decompositional analysis was around long before analytic philosophy emerged). I remark that "This might be true of Moore's early work, and of one strand within analytic philosophy; but it is not generally true". It is *at this point* that I then say what Ongley quotes me as saying. The "as it was founded by Frege and Russell" makes clear that I am just referring to one – albeit central – strand in analytic philosophy, and not to analytic philosophy as a whole. Ongley gets the dialectic of my argument wrong. I am not forced to 'admit' that my characterization does not fit Moore. It was never intended to do so in the first place.

As I said above, one of my targets in writing about analysis is the view that philosophical analysis is essentially conceptual decomposition, and that this is therefore what characterizes 'analytic' philosophy. But this view does no justice at all to the actual methodologies employed by those who are generally regarded as analytic philosophers (understood as including Frege and Russell, as well as later philosophers such as Wittgenstein, Carnap, Ryle, etc.). So in focusing on logical analysis, and the Frege-Russell strand, my aim is to correct this mistaken view. Ongley seems to think that I am merely replacing one crude definition of analysis in analytic philosophy with another, whereas my main concern is to show just what a rich variety of conceptions of analysis there are even within analytic philosophy. In fact, we have only to consider the Frege-Russell strand itself to see that there are important differences here, too. As I point out in my entry, and have argued in more detail elsewhere (2003b, §6), for Frege function-argument analysis is fundamental, whereas for Russell decompositional analysis remains at the core of his thinking. (Cf. also Levine 2002; Hylton 2005b; Griffin forthcoming.) The case of Russell is instructive here. For it shows just how complex a particular philosopher's practice or conception of analysis can be. Russell may engage in logical analysis, in showing,

for example, how definite descriptions can be 'analysed away' when sentences in which they appear are recast into their 'correct' logical form. But decompositional analysis is still assumed to be required in identifying the ultimate constituents of a proposition.

Ongley's failure to appreciate all this leads him to make some quite unjustified criticisms of my account. He writes, for example:

Beaney finds G. E. Moore's notion of analysis to be of a traditional decompositional sort, where complex concepts are analyzed into their constituents. This puzzles Beaney: while he admits that Moore influenced conceptions of analysis among analytic philosophers, Beaney does not address the fact that this means that his theory that 20th c. analysis as Fregean/Russellian logical analysis does not seem to work even for the major analysts. He simply ignores this problem and goes on to Wittgenstein. (p. 50)

This is a travesty of my account. There is much to be puzzled about in Moore's philosophy. (Indeed, Moore would hardly approve if one did not feel puzzlement.) But I am not puzzled that he had a decompositional conception of analysis. I say it is "surprisingly traditional", given his status as one of the founders of 'analytic' philosophy, but that just shows that the use of decompositional analysis cannot be the hallmark of 'analytic' philosophy. More importantly, I do not have a 'theory' that twentieth-century analysis is Fregean/Russellian logical analysis, and so do not feel flummoxed that Moore does not fit this straitjacket. On the contrary, I pointed out from the very start that Moore represents one genuine strand in analytic philosophy. So there is no problem that I ignore and quickly cover up by turning to Wittgenstein.

Am I just being overly sensitive to the rhetorical flourishes of Ongley's exposition? As I said at the beginning of this reply, Ongley gives a fair summary of the main elements of my survey. However, it is to some of his connecting critical patter that I object. The impression is given at numerous points that I am simply pigeonholing philosophers and offering a crude generalization as to what 'analytic' philosophy is, which does not do justice to my aim of showing the variety of conceptions of analysis in the history of philosophy. In concluding his account of my survey of twentieth-century analytic philosophy, Ongley remarks: "it should be obvious even from this brief description of Beaney's survey of the 20th c. that his model of 20th c. analysis as based on logical analysis does

not fair well even on his own terms. In the end, Beaney changes tack and defines analytic philosophy as being a set of interlocking subtraditions unified by a shared repertoire of conceptions of analysis that different philosophers drew on in different ways." (p. 51) I do indeed suggest that analytic philosophy should be seen in this latter way (but not 'defined' like this, which is not how I put it). I am not changing tack, however, since I was never in the game of offering a 'theory' (or 'definition') of analytic philosophy. As I have stressed, I was concerned all along to indicate the richness and complexity of conceptions of analysis throughout the history of philosophy, and not least, within analytic philosophy itself.

Let me end, though, by thanking John Ongley for his detailed review. As he notes at the beginning of his essay, I am currently writing a book on analysis, and my entry in the *Stanford Encyclopedia* was a first report on the work I have been doing. The hypertext format of the *Stanford Encyclopedia*, and the fact that entries can be updated in the light of criticism and further research, made writing such an entry the ideal way to proceed. I could offer an outline of conceptions of analysis in the history of philosophy in the main document, while reserving further details for the linked subsections. I could also make available the extensive bibliography I had been compiling, to help and encourage others to explore the topic of analysis. Of course, even with the subsections, attempting to cover twenty-six centuries of history of philosophy in just one entry is asking for trouble, and as Ongley notes at various points, there are significant gaps (not least concerning conceptions of analysis in the nineteenth century), some of which I am hoping to fill in soon. But I am grateful for the generous remarks Ongley makes in the concluding section of his review. I have concentrated in this reply on the main (and only real) grumble that I have with Ongley's review, but as indicated above, I accept his key criticism, about the need to address the issue of the apriority of analytic methodology. Ongley also makes other, more specific comments in his review, such as those concerning Kant mentioned above. I know that these, too, will be helpful to me both in revising my *Stanford Encyclopedia* entry and in completing my forthcoming book on analysis.

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## PROPERTIES OF ANALYSIS: REPLY TO BEANEY

JOHN ONGLEY

In my review of Michael Beaney's entry on analysis for the online *Stanford Encyclopedia of Philosophy*, I made several statements to which he takes exception. His main criticism is that my review fails to appreciate the rich varieties of analysis that he was trying to show exist in philosophy. Instead, it focuses on only three types discussed by him – regressive, decompositional and interpretive analysis – and ignores the other kinds he mentioned and also ignores the complex interrelations between these various methods.

I am glad that Beaney has taken the opportunity to stress this point in his reply to my review. I agree that it is much more interesting to look at the various periods in the history of philosophy as a rich complex of methods rather than as consisting of a few methods that can be categorized as belonging to one of three types, and it is true that he mentions and describes many more than these three kinds of analysis. Still, it is decompositional, regressive or interpretive analysis that Beaney mainly discusses, and he most often says of some method of analysis, after describing it, that it is like one or more of these three major types, which is why I focused on them.

What other types of analysis does he mention? One that he discusses throughout the text is "reductive" analysis. In it, a concept is said to be "reduced" to others when one can eliminate it in favor of the other concepts, for example, in one's description of the world, in which case one has discovered a metaphysical fact about the world. Beaney principally describes reductive analysis as a kind of interpretive analysis that interprets talk using the concept into talk without it, though I suppose decompositional analyses can be equally reductive.

Beaney discusses reductive analysis at length in the case of Gilbert Ryle and other Oxford analysts, making the point that Ryle shifted from this principal method of analytic philosophy to a kind of non-reductive interpretive analysis that Beaney calls "connective" analysis. Where one cannot eliminate a term without circularity, so that it is in some metaphysical sense irreducible, one can still (circularly) clarify its meaning by interpretating it in terms of other

concepts that can only be defined in terms of it. Such an interpretive analysis is a connective one, one that shows the logical connections between these basic irreducible concepts. Though I did not quite gather what claims are made for such analysis, that is, what its significance is supposed to be (are these the "true meanings" of the analyzed concepts or are they something more arbitrary), Beaney makes the intriguing suggestion that "connective analysis would seem to be particularly appropriate ... in the case of analysis itself" (that is, I think, in analyzing analysis itself).

It needs to be emphasized however that my point in discussing the three kinds of analysis I saw Beaney spending most of his time on was not to say that they are inadequate to describe the varieties of analysis in philosophy and that we need a richer taxonomy than he provides, but that there are certain questions about analysis which Beaney's explication of even these three types does not answer for me. Nor do I take this as an inadequacy of Beaney's discussion, since his purposes are not to answer my questions, but his own.

Beaney refers to some of my concerns when he says that I am right in saying that he needs to address the issue of the apriority of these and other methods of analysis. My point, though, is more general than this. What puzzles me whenever I hear people talk of analysis or see such ideas in print is how exactly the analysis is supposed to work. What are the specific steps that one takes in each kind of analysis, and most important, how does one justify each of these steps? (It would also be interesting to learn how it is thought that we psychologically move from step to step, how we are thought to discover the various steps in an analysis.)

When asking how a step in an analysis is justified, the question of whether the justification is supposed to be a priori or a posteriori does arise, but so do other questions such as what metaphysical assumptions are being made in each case, and since it is usually concepts that are being analyzed, what theories of meaning are being presupposed. (These questions might also arise when trying to say how the various steps of an analysis are arrived at, that is, discovered.) But these are my questions. This being the case, perhaps it is up to me and no one else to answer them. Beaney has done an impressive job of answering his own questions, one that that I think will

inspire others to try to push the subject even further.

To move back to Beaney's criticisms of my review, he points out that right at the start of it I attribute to him a general characterization of analysis as being decompositional when that is not what he says, and I am chagrined to see that I do make this error. His most general characterization of analysis comes in the second sentence of the piece and is that "in its broadest sense, [analysis] might be defined as disclosing or working back to what is more fundamental by means of which something can be explained (which is often then exhibited in a corresponding process of synthesis)...." As he notes, he then goes on to emphasize that decompositional analysis (breaking a concept down into more simple parts) is not the only sort of analysis that philosophers have practiced, and is arguably not the most important one.

Beaney also points out that I suggest that he tries to give a single characterization of analytic philosophy and must then immediately admit Moore as an exception. I did erroneously suggest this and I withdraw the suggestion. Beaney emphasizes that there are several major strands of analysis to be found in analytic philosophy and that Moore's is one of them. (He also points out that the "knowledge is or isn't justified true belief" discussion in analytic philosophy is another instance of decompositional analysis.)

A final quibble with Beaney though. He says in his reply to me that it is not his aim to pigeonhole philosophers into various categories or impose a rigid taxonomy. I hope this is just a matter of emphasis and that he will not wholly dismiss taxonomic tasks in his further work on the subject, and I suspect that he won't, for he also says that "of course, some kind of conceptual framework must be developed to elucidate the various forms of analysis and their interconnections". For my part, I do not think that any historical period can be accurately discussed or even clearly thought about without a good taxonomy and genealogy of its ideas, and the game of taking someone's taxonomy (even one's own) and trying to refine or modify it or elaborate on it is an important and probably essential way of moving the understanding of a period forward.

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### *Traveler's Diary / Conference Report*

The Annual Meeting of the Eastern APA (December 27-30) shares in the emotional angst of the holiday season in which it occurs. Like the extended visits with extended family these holidays involve, the normal person approaches the Eastern with mingled excitement, resentment and dread. This year's conference location, Times Square, being what it is, is unlikely to sooth these feelings. This is especially true if, like me, you're ignorant of the fact that the Square contains two Hilton Hotels. Yes, my unplanned run-walk from Hilton A to Hilton B was a "special" joy, as was my subsequent disheveled Grand Entrance to the APA, gripping tatty plastic bags stuffed with Quarterlies and flyers in each sweaty hand.

Hilton B, though grander than Hilton A, is something of a disappointment: what on earth did they do with the chairs? Are they outlawed in New York along with cigarettes, or are the hoteliers trying to prevent riffraff from settling down for a rest? There were some chairs, to be sure, but they were fiercely guarded, and whole stretches of hallway, vast acres of registration area, and echoing chambers of bookseller space were chair-free zones. I did notice a father and his three children sitting on the floor in front of the ATM (and thereby inconveniently blocking access to it), but none of the gilt-tongued concierges seemed to notice. After locating the room in which the BRS session was to occur, I therefore laid out my Society trifolds and other wares and had a seat. But what a sitting it was!

The first session of the day, hosted by the History of Early Analytic Philosophy Society, and chaired by Stefanie Rocknak (Hartwick College), boasted Sandra Lapointe (Kansas State) speaking on "Husserl and Frege on Formal Meaning", Karen Green (Monash University, Melbourne, Australia) speaking on "Fregean Existence and Non-Existence" with commentary by Kevin Klement (U Mass/Amherst), and Chris Pincock (Purdue University) speaking on "An Overlapping Consensus Model of the Origins of Analytic Philosophy" with commentary by Aaron Preston (Malone College). Sandra Lapointe's paper remains an unknown to me, as I stepped out at for a bit to get money from the ATM (where I found the encampment mentioned above) and to register for the conference. I returned in time to hear Karen Greene deliver a very persuasive

paper that received high praise from Kevin Klement, followed by a debate between Chris Pincock and Aaron Preston, on the topic of whether we analytic philosophers have a topic, to a large and sometimes electrified audience. As convener of these events, it behooves me to count heads: I counted 24.

The BRS group session immediately followed the HEAPS session with three speakers of its own: Gary Cezarz (Southeast Missouri State University), speaking on "McTaggart and Broad on Leibniz's Law", Nikolay Milkov (Bielefeld University), with a paper titled "Lotze's Influence on Russell" and John Ongley (Edinboro University of Pennsylvania), with a paper on "Lotze and Anti-Psychologism". John Symons (The University of Texas at El Paso) served as Chair and as commentator of Gary Cezarz' talk, while David Sullivan (Metropolitan College of Denver) commented on Nikolay Milkov's paper. The lack of a respondent for John Ongley's paper turned out to be fortunate, as each speaker ran so overtime that no commenting would have been possible in any case, and John's talk was written as a follow-up commentary to Milkov's paper anyway. Milkov argued that Russell's turn from idealism and monism actually preceded Moore's, despite Russell's own story to the contrary, and that in turning this way, Russell exhibited the influence of Hermann Lotze, a forgotten but influential philosophical muse of the 19th century. Some historians in the audience argued for a broader view, and Sullivan began to summarize his own objections, which were based on his claim that Russell studied Lotze's *Metaphysics* and not his *Logic* so that the influences from Lotze that Milkov claimed to find in Russell and Moore (from the *Logic*) could not have been from Lotze, but Ongley's analysis of the historical influence of Lotze in at least some ways supported Milkov's general point. Counting heads was interrupted by the sudden appearance of Warren Allen Smith, Peter Stone and other BRS regulars, crashing into our midst with their boys like gangsters before a shootout. Even after the dust settled, I still counted two-dozen heads.

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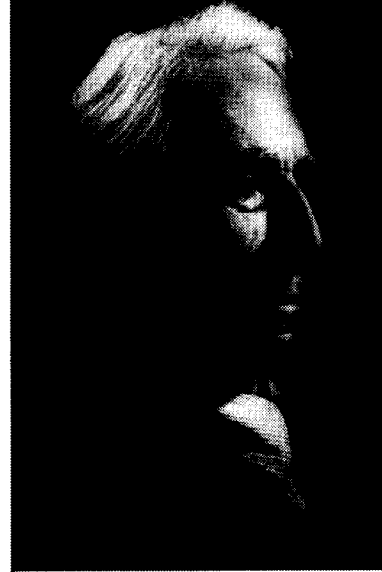
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