

Panel discussion

The tenability of Russell's early philosophy

by A. J. Ayer (moderator), I. Grattan-Guinness, Nicholas Griffin, Robert Tully, W.V.O. Quine.

Other voices: Yvon Gauthier, Danny Goldstick, John Passmore, Harry Ruja, John G. Slater, and Ian Winchester.*

WINCHESTER: I'm hoping this will be a very informal session. Our panelists will, of course, feel free to say anything that they feel like, that they want to get off their chests, and then the audience will take it upon themselves to make comments.

AYER: I think I'd like the audience to interrupt if they feel like it—if we say anything outrageous or platitudinous, or both. I'll start off by saying a few words. I noticed that early on we had a very well-known, typical Russell quotation, namely that, where possible, logical constructions are to be substituted for inferred entities. But an interesting one that passed unquoted occurs in *The Problems of Philosophy*, namely that every proposition we can understand must be composed of constituents with which we are acquainted. It seems to me—and this is something I would like my companions to discuss—that this ties Russell very closely to phenomenalism, because he argued as early as *The Problems of Philosophy* that the only particulars we are acquainted with in addition to our Selves are sense-data; and he excludes Selves by the time he gets to *The Analysis of Mind*. Otherwise, he allows us to be acquainted with universals. Now if you interpret the theory of definite descriptions in the way that Quine does (and I agree with Quine), Russell should be interpreted as permitting—not only permitting but encouraging—the elimination of singular terms. This means that all the stuffing, as it were, in your statements gets into the predicates, and there's nothing left to be a value of the existentially quantified variable except something that requires no connotation, namely the object of a demonstrative. If that is so, and if the object of demonstratives for Russell can be only sense-data—something he maintained throughout his career right up to *Inquiry into Meaning and Truth*—it means that you are only referring to sense-data and to what properties they can have. This leaves you no other alternative but phenomenalism.

* In passing two series of proofs of the discussion, the panelists and "other voices" not infrequently revised the wording ascribed to them. The result offered here, while not a verbatim transcript of the discussion that took place on 24 June 1984, is what each speaker wishes printed.

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Now Russell, I think, cheats in *The Problems of Philosophy* because he tries to combine this view with a causal account of perception, putting physical objects beyond the veil of perception in a space of their own. But he wants to combine this view with a Humean theory of causation, which, of course, is an inconsistency since, according to Hume, causal statements can only be generalizations from observed entities and therefore can only consist of extrapolations from sense-data. So I think that Russell is, in fact, inconsistent; I think that he did embrace phenomenalism but of course forsook it by adopting, after *The Analysis of Matter*, and also in *The Problems of Philosophy*, a realistic idea of science. I think that Bob Tully's attempt to make him a neutral monist, for example, won't work at all.

TULLY: My effort was to make a different sort of neutral monist of Russell than the prevailing one in the literature, which includes your own account of his neutral monism. I feel that Russell quite self-consciously proclaimed a doctrine in which there were both the immediate data of experience, to which he gave various names, as well as inferred entities of which we can have knowledge and to which we refer in public space, which space itself is inferential. I think that account is characteristic, not simply of Russell's work after *The Analysis of Matter* but in *The Analysis of Matter*, where he explicitly calls himself a neutral monist. I think it's also to be found, as you've just been indicating, in the thread of some of his concerns from *The Problems of Philosophy*, through *Our Knowledge of the External World*, as well as in another 1914 writing, "The Relation of Sense-Data to Physics". So, while Russell says that all of our knowledge must ultimately be tied to what we are immediately aware of, the nature of such knowledge, conceived in such a way as to allow for our conception of the physical world, requires that we have some degree of certainty through inference to entities of the kind that can only be described. His neutral monism was, as a matter of fact, that particular doctrine. It was not an attempt to construct the entirety of the world of physics and common sense out of a neutral stuff. Rather, it was an attempt to find within that picture of the world given by science, a role, an important role, for the things which he called sensible qualities. These sensible qualities are, in the common-sense view, characterized falsely as the properties of material objects such as tables and chairs. They are the items in *Our Knowledge of the External World* which are called "perspectives", out of which we construct these objects of common sense. Progressively and scientifically, psychology tells us what we're doing when we make those constructions; but there is also another role for these same qualities in the other, physical account of the world which gives a fuller causal account of the conditions whereby those various experiences transpire that are described in psychology. So I think our differences are perhaps in our interpretations of Russell, but certainly at the nominal level as to what it means to be a neutral monist; and I feel that Russell meant something different by neutral monism than phenomenalistic construction.

AYER: But he has two separate accounts of space, doesn't he? In *Our Knowledge of the External World* he tries to construct space as a space of six dimensions in terms of perspectives and so on. And in the later books, *The Analysis of Matter* and *Human Knowledge: Its Scope and Limits*, he puts space as something beyond the veil, as it were.

TULLY: It's beyond the veil in part.

AYER: It is structural correspondence with one's perceptions within the space of physics.

TULLY: There are two important ways of corresponding, and Russell at one point in *Human Knowledge* makes a point of saying that in order to avoid confusions in our account of physical space, we have to discuss the place in physical space of perceptions in two different ways. There is one way which serves practical purposes to a great extent. Referring to a table, he says the place of the table in my perceptual space can be roughly correlated with the place in physical space in which are to be found the causal conditions which result in the perception of the table. But, more precisely, in the physical account of the world, we cannot say that the place in physical space corresponds to the place where I'd ordinarily say the properties of the table are. Rather, it is to be found, from the physical point of view, in the brain of the percipient himself. The percept belongs in that particular place. So, in fact, there are two different accounts of space. They are made to be correlated together, certainly, but where we locate qualities is different when speaking from a physical point of view or from the psychological point of view. It's only by thinking that somehow the physical point of view is totally prevailing, and is somehow meant to interpret across or give us the meaning of common-sense statements, that it becomes, to the ordinary-language philosopher, unintelligible. The statement that our percepts are inside our head, for example, is a statement he made about the brain. But there the brain is not being construed as something that could be seen under the skull. It's rather something which is a structure of events as described in physiological science.

WINCHESTER: Let's suppose that Russell is some kind of neutral monist. In his development, this kind of view certainly recurs. But is his theory any damn good? That seems to me to be the question that we would want you to address.

AYER: I think it just doesn't work. In fact, the only person who has really tried to make it work was Carnap. James talks a lot about it and refers very vaguely to places around the Harvard campus which he analyzes in terms of future experiences and so on, but he doesn't analyze them in detail. The only serious attempt to make neutral monism work was done by Carnap in the *Aufbau*. But Goodman in *The Structure of Appearance* showed that Carnap's attempt didn't work. Now, Goodman himself doesn't try to apply his "erlebs" of appearance beyond sensory qualities. I mean, he makes no attempt to get physical objects, although he doesn't say that you can't. He says that you can't get a model of common sense in any way. But then he leaves it an open question whether you can get a solution on that basis.

QUINE: I'd like to go back to this earlier point of Freddie [Ayer]'s on the use of descriptions and demonstratives to eliminate the singular terms altogether, except for variables. That's the theory of descriptions that I like; but as for demonstratives, I don't think that they retained any distinctive status in this respect. They would go, too. The demonstrative general terms would remain ("here" and "now"), but the demonstrative singular terms ("this", "that", "this thing", "that thing") would go the way of descriptions. These would be, again, singular descriptions, but singular descriptions in which the predicate happens to be some indicator word like "here" or "now" rather than an eternal sort of predicate. So I don't see even from Russell's point of view an argument from this to an ontology specifically of phenomena. Rather, the ontological side as I see it becomes just this, that the singular terms are gone. We have, as you say, the predicates. The things, then, are all the things that these predicates are true of. But any characterization of them comes through the predicates, by direct reference to them; the variables are characterless.

AYER: Yes, things in the end, of course, do become collections of qualities related by the relation of compresence, as in *Inquiry into Meaning and Truth*. I have kept demonstratives only because in the last resort they're the only possible values for the variables. But if you care not to go deeper and leave your existentially quantified variable as the subject of the sentence, as it were, then you don't need to. But because they have no connotation, they just provide pointers. It's like a great insect, and all the information swells the body of the insect. You get a residual head which just consists of a pointer.

QUINE: Yes, but my point is that even the pointing can be done by the predicate, the predicate being, however, an indicator word like "here" or "now". The indicator subject, that is to say the demonstrative, would have the same status as other singular terms.

AYER: You could do it that way, too, I agree, except for practical purposes. Though if you want, as it were, to orient your interlocutor, demonstratives would be helpful.

QUINE: How in the here and now do we do it?

AYER: By the predicates.

QUINE: The predicates, yes.

TULLY: I don't think the predicates as Russell construed them would be entirely qualitative. In his later works he allowed for a range of predicates which were thought to be descriptive, but in an abstract way, of collections of events. They were given, or defined, by science. He tended to reserve the term "particular" for these, claiming that this was not a term with any metaphysical significance since its content would be given by science as science progressed. Once a new physical version of the world of, say, subatomic particles came to be, this would be what would be meant by "particular" in his account. It's not simply a qualitative set of descriptions that would be given, and by means of which we succeed, in his view, in making reference to regions of space-time which are beyond perception. I'm really just reporting what I take to be his view from the early 1920s on. It's different from Carnap's phenomenalistic account, which I think goes back a little bit earlier toward Russell's doctrines of 1914.

AYER: I'd like to change the subject and have Grattan-Guinness in on why he thinks pursuing this matter is a waste of time. (Laughter.)

GRATTAN-GUINNESS: The issue is that of the tenability of Russell's logicism. In my view, Russell does not define, even in cursory form, what mathematics actually is in the first place. Now, in general terms, if you have a body of knowledge *B* grounded in another body of knowledge *F* acting as foundations, everything you want to ground has got to be in *F*. Otherwise it is not a reduction. In my view, it is not in the slightest bit obvious that mathematics in any ordinary sense of the term at all can be derived from what is provided in *Principia*. It is just not enough to derive things: you have got to derive them by *logicistically legitimate* means only—otherwise it does not count.

Here is one example of the type of problem we have. Even with pieces of mathematics that Russell does include, he has to use the multiplicative axiom. But the status of this axiom in logicism is *very* dubious, since it cannot be expressed in finitary form. If you were to go off into other parts of mathematics you would meet that axiom often, and probably all sorts of other difficulties.

The problem is not even posed anywhere that I know of. The programme that was outlined in the *Principles* is nothing like what was achieved, even in sketch

form, in *Principia*. And it is not only a question of there being no Volume IV—that would not solve the problem, since the areas of geometry did not encompass the realm of geometries that lay within mathematics of the time. One cannot see what Russell means by the word “mathematics”. It would not correspond to any use that I know—or that anybody else would have—for the mathematics of his time.

I mention another example. A big area of mathematics in those days was abstract algebras. How do you even start to develop them within logicism? You either cannot do these theories or you artificially exclude them.

There is also the problem that Russell and Whitehead had *pure* mathematics grounded in logic. For Russell’s definition of pure mathematics in terms of p implies q , is neither necessary nor sufficient, as you have got to think of the substance and not just the form. There is a better form of logicism, namely the categorial kind of reduction. There is a thing over here, a category called “logic”, which would lead to a category called “mathematics”, and this latter category is still pure, somehow. AYER: Do you think he was influenced by Kronecker at all? You know, “God made the natural numbers, everything else is man’s work”, and so if you can obtain the natural numbers out of logic then the rest can be left for the mathematicians.

GRATTAN-GUINNESS: But it would not have been a strong influence because Kronecker’s position—and Russell was not known at the time as a Kroneckerean—was an anti-Weierstrass stance. There were rows in Berlin over whether there were such things as irrational numbers. The “mathematics” that Russell covers certainly does include the Weierstrass–Cantor type. So there would not be much family kinship between Russell and Kronecker.

AYER: Would your criticism extend to Frege?

GRATTAN-GUINNESS: The position of Frege is quite different because Frege was not trying to show that mathematics reduced to logic. Geometry, for example, was not in his programme. Even then it is really not clear how much analysis he wanted. There is a frequent claim in the literature that Frege and Russell wanted to show that mathematics reduces to logic. This is completely untrue for Frege. He says quite explicitly that geometry is in a different position. That is a difference between Russell and Frege.

AYER: Frege does implicitly try to get rid of geometry.

QUINE: But, Grattan-Guinness, you’re beating a dead horse, aren’t you? I don’t know anybody, any more, who would say that mathematics is reduced to logic. One reason is, of course, the question of how we delimit mathematics. Maybe we’re specifically interested in reducing analysis and number theory, and we could enumerate a few more. I think abstract algebra presents no problems. It could be embedded in the theory of relations, the higher theory of which is the set theory of relations. However, there’s also the question of what is logic, and many think that the significant place to draw the boundary is between logic and set theory, not to include set theory under logic. It’s a question of the ontology of what you’re quantifying over. In logic proper, there is no indication of what things as opposed to other things are quantified over. It’s just left open, all those variables. But when you come to set theory and other parts of mathematics, there’s specific subject-matter, specific objects (sets, for instance), and I think that the reasonable place to draw the line is there. This was partly obscure in *Principia Mathematica*, although not in Frege, because of confusion of use and mention. The ontological status of class just wasn’t clear to Russell, and he had a feeling that he was somehow reducing

it in a nominalistic vein. So what does the thesis become? That these various important classical areas of mathematics—notably number theory, analysis, theory of functions—are capable of being embedded in set theory. This doesn’t give us, I think, any epistemological insight, particularly because it’s not a matter of founding these various parts of mathematics on something more intuitively sound. Indeed, the metaphor of foundation is misleading because this is a case rather where you have a foundation suspended from the superstructure. What really is clear and firm, comparatively, is number theory and theory of functions; whereas set theory, of course, was riddled with paradox until it more or less artificially got fixed up. There’s no general agreement as to what sets to include. We still have a mathematically interesting and important theorem of embeddedness or reduction, where reduction is taken in an unpolarized way. You are reducing the familiar to the unfamiliar; reducing these various parts of mathematics to a neat little economical set of just two quantifiers, two truth-functions and a two-place predicate. That is very striking, but it doesn’t tell us that mathematics is logic, doesn’t tell us that mathematics is analytic. It doesn’t tell us anything about those points.

AYER: Are you happy with that?

GRATTAN-GUINNESS: No. Because my original question asked the question *historically*. Of course one can do all sorts of things *now*: Quine’s work is a distinguished example. I mean in terms of Russell in 1910 putting the *Principia* into press; what did Russell think *then* was being achieved? What was this word “mathematics” actually referring to? I actually looked through the volumes as a whole for the bits that are in and the bits that are out. It is almost random.

But there are deeper questions that are involved, of which the multiplicative axiom poses only one example. The third point I wanted to make about the incoherence of logicism is this. Let us suppose we know what he means by “mathematics”. What does he mean by “logic”? Now, I think there is a serious danger for Russell, actually of a vicious circle kind, namely that you have mathematics grounded in logic, but you define logic as whatever you need to found the mathematics. So the whole thing is going round and round. That is a problem for Russell; and because of the inclusive way he conceives of logic, the questions like “What is logic?” cannot be posed. It is interesting that as late as 1912 he started really trying to get a definition of “logic” independent of logicism and to break this potential circle. People like Wittgenstein and Ramsey also wanted to characterize logic independently of logicism. If you do not do that, this incoherence remains.

GRIFFIN: I think you’re being a bit unfair in attacking Russell for not explicitly expressing what he meant by mathematics. Perhaps he should have done so. But part of his expectation was that the logicist programme itself would help reveal what mathematics was. This is just going back to your circle of: What is logic? Well, it’s whatever you need to ground mathematics. What is mathematics? It’s whatever is grounded by logic. God forbid that I should press the continuance of neo-Hegelianism. (Laughter.) But it’s beginning to look like a dialectical circle of the sort that he was much enamoured with ten years earlier.

GRATTAN-GUINNESS: Yes, I agree. I find it very difficult to locate people today who think this is a serious problem, but I still do so.

AYER: Well, there was the problem of ontological reduction, too, wasn’t there? I mean, he did really want to say there was nothing in the world except classes and certain types of individuals, but we are not quite clear what types of individuals

they were, when really, on Quine's terms, nothing exists but classes?

QUINE: Then he thought he was getting rid of classes, which he thought of at the time as properties. Because of the use he made of them, quantifying over them as he did, they have to be regarded rather as universals, as properties. I don't know if Professor Grattan-Guinness feels alone, but I would like to say that I am in complete agreement with him. And as to the second point: I would like to say that the set-theory part, at least for French mathematicians, is not part of mathematics.

AYER: Do you mean today?

GAUTHIER: Today, yes.

AYER: But is that more than a conventional decision simply not to call set theory part of mathematics? Is there some argument behind it? It seems to me that in the advanced stages it was fairly arbitrary—the way you said logic began, the way it stopped. You found it more convenient to say it stopped short of set theory, but it wasn't more than a conventional logical decision. Is it more than that in the case of your mathematicians?

GAUTHIER: Yes, it is. Dieudonné or René Thom would say that set theory is not serious enough to be part of mathematics. (Laughter.)

AYER: I don't accept that. You mean set theory is too riddled with antinomies. You want it out, is that it?

GAUTHIER: Yes, the antinomies are part of "paradoxology". It has to do with numerology and things like that. Paradoxology is not part of logic either.

QUINE: Dieudonné does not like set theory and does not like logic. We can say that much.

GRATTAN-GUINNESS: That is more a comment on Dieudonné as a mathematician. We see here the culture gap which I mentioned yesterday: the mathematicians and the logicians not really communicating.

The status of the individual is very difficult in *Principia*. In the system that Russell adopts, because of the structure of the type theory, the infinity axiom has to be an infinity of individuals to form a base type when needed—not just an infinity of any old things—and what these individuals actually are is a very tricky business. And you find conflicting texts in Russell. One of them says that they are the fundamental constituents of matter—they are really the bits of matter that the Bohrs and the Einsteins were looking for (because, of course, no abstract objects are allowed); Russell did not want to postulate them as structureless intellectual entities. However, in other places it seems that he treated them as a *relative* base level; the objects at that level are called "individuals" for convenience, and then he makes classes, and classes of classes, and so on out of them.

You have the question, "Can you have infinite analysis or not?" If so, it is a bit incoherent; but if not what are you going to stop with? And it is, I think, a non-trivial question for Russell of which he was quite aware in places, e.g. when he talks in the *Introduction to Mathematical Philosophy* about these empirical individuals. When they are empirical (this is one of his phrases), "This is a defect in logical purity", which is a nice way of saying, "My system does not work."

AYER: There's always the danger of going wrong by there being nothing else, isn't there? And you wouldn't be able to construct the universals, because there would be a shortage of individuals to form your classes.

GRATTAN-GUINNESS: On this view the infinity axiom, he says, quite explicitly, is an empirical matter, and could be false.

AYER: In this case, though, you'd have a finite number system.

GRATTAN-GUINNESS: Yes, and even if true, it still gives mathematics an *à posteriori* aura, at least with empirical components in it—which seems very strange, given his construal of logic.

AYER: Would you mind that, Van?

QUINE: Yes, in fact that's the trouble that Russell recognized, as was pointed out yesterday—I forget by which of you. For me it's been a strong reason for doing things otherwise than through the theory of types. For those set theorists who keep something that is nearly equivalent to the theory of types in a way, namely Zermelo's approach, it has been a reason to construe numbers in another way than the Frege–Russell way. They adopt rather the von Neumann construction: each natural number is the set of all earlier natural numbers, so that zero is the null set, one is the singleton of the null set, and so on up. And there you get the whole apparatus without worrying at all what individuals there are. In fact, it's a widespread custom among set theorists to banish individuals altogether in favour of what they call "pure set theory". There's a certain structural economy that's gained that way. There's an objection, though, in my view—namely, you don't have application; you can't make set theory part of a general language for science. But there's another way of getting the same structural benefit without banishing individuals, namely the trick of identifying individuals with their singletons. Then it works out nicely, and there still isn't any problem of having to assume infinitely many. In fact you could still let that number be as small as you like, even zero, and set theory itself doesn't suffer.

AYER: This is metaphysically very far away from Russell, isn't it? I think Russell did really want to take the mystery out of mathematics. He thought that numbers were mysterious entities, connected with Plato and so on, and something you had to get rid of. I think that Grattan-Guinness is right here, that Russell really wanted to reduce mathematics because he thought that numbers were mysterious and that in some way his logical entities were less mysterious.

GRATTAN-GUINNESS: Yes, I agree with you. I personally agree with Professor Quine that foundations are things you dig down to: you have got a superstructure, you are in a swamp, and you see what happens. Russell did not seem to take that view, in various places in his writings, both for logic and for philosophy in general. We have got to get the foundations secure. Thus they do not hang from superstructures, but build up from a substructure. This was his view of knowledge in general, not just of mathematical knowledge.

QUINE: I wonder if Russell would have liked von Neumann's numbers if he had thought of them?

AYER: I think not.

GRATTAN-GUINNESS: Jourdain once thought of a definitional system rather like von Neumann's, and Russell was not interested in the slightest. That may have been because of the crudity of the form in which Jourdain presented them. But I do not think he basically would have responded, because type theory is not obeyed.

AYER: And he always stuck to analyticity, didn't he, in regard to mathematics? Which again wouldn't suit you?

QUINE: No.

GRATTAN-GUINNESS: And why empirical individuals? Presumably a defect in logical purity.

AYER: Yes, of some scale! (Laughter.)

WINCHESTER: One had the impression that one possible account of why he doesn't, at the beginning of *Principia Mathematica*, talk about what mathematics is, is that he talks about it in *Principles*, and he has laid down a programme there that in fact includes most of mathematics of the kind that was well known at the time. So, thinking of it as the second volume in some sense—of course, it wasn't exactly the volume projected—he may have thought the introductory part didn't have to be done all over again.

GRATTAN-GUINNESS: But let me re-pose my reservations. We know he became tired of logic and all the rest of it. But it is not in the slightest bit obvious as to how you produce mathematics of *his day* within his logicist system, *but not even that is said*. That is my problem.

AYER: There's a big gulf between the *Principles* and *Principia* in that the *Principles* is very non-reductive; I mean, he allows proliferation of entities. I think there's a big departure from the *Principles* in that sense, and he's supposed to show that the theory of descriptions comes in between. That's the important thing. This was meant to show that you could dispense with many of the realities that he brought into the first book. I think that the *Principia* was written very much under the influence of the theory of descriptions.

GAUTHIER: From what I've heard, it seems to me that the Russell paradox is more or less a guillotine rather than a mathematical theme of any importance. So I would like to discuss the idea that paradox has no place in mathematics. Kreisel is a mathematician who doesn't think that paradox has that kind of role to play in logic or in mathematics. It seems to me that Russell's interest in paradoxology originated in his Hegelianism rather than in his knowledge of mathematics.

AYER: Would you like to answer that?

GRIFFIN: Yes, I would agree at least to some extent with that. Nonetheless it's striking how different his attitude to the paradoxes is, once he comes on the class-paradox. Because he had had a lot of earlier ones, and he was used to handling them, to tossing them around and hoping eventually that they would go away, but not being really that disturbed by them. It's only when he comes across the Russell paradox that he thinks that he's got something that he really has to solve before he can do anything else. Now I would read his letter to Frege in rather a different way to the way that Greg Moore read it in his paper. I think that it was a letter of a young, rather diffident man writing to a great man in the field and not wanting to say "I found this devastating objection to your theory" in case Frege would write back in a postcard with a simple resolution of the contradiction. So he writes a bit nonchalantly, "This problem is the only thing that worries me", you know. And then after that Greg [Moore]'s story and mine would agree: that he gets Frege's reply, "Arithmetic's tottering", and his reaction to this is "Yes, this is fundamental and I'll work on it." I think his interest in paradoxes certainly has neo-Hegelian roots. But nonetheless somewhere between the neo-Hegelian period and post-class-paradox period, something happened to change his attitude to the paradoxes, to make them loom even larger, as really serious problems for his system.

PASSMORE: In his earlier views Russell thinks you've got a paradox within a particular theory, but you can always resolve the paradox by rising to a higher level. But now you've got paradoxes at the heart of logic, and there's no higher level. A paradox comes to have a quite different significance at this point in his thinking,

once he drops the doctrine of the Absolute as that in which the paradox will finally be resolved.

GRIFFIN: It's actually a switch, I think, to a more foundational manner of thinking, of securing things on the foundations rather than the sort of Hegelian arch where the different sciences would link with one another.

RUJA: My impression is that Russell didn't relish discovering a paradox, but considered it a misfortune. It was an aspect of his dialectical procedure. By that I mean that he tried to anticipate the difficulties that a particular formulation of his would encounter and tried to deal with those difficulties even before the critics began to formulate them. The paradoxes that showed up were just a product of this self-critical approach of his. From what the gentleman speaking for the French mathematicians said, one might get the impression that Russell deliberately sought out paradoxes because he relished them, he wallowed in them. I don't believe that that's fair.

AYER: I agree with you entirely on that. I'm inclined to think, although perhaps someone in the audience will refute me, that both Frege and Russell thought that unless they could solve Russell's class paradox, arithmetic had been shown to be false.

PASSMORE: This is true. The point I'm making is that—well, there're two points. One is that his earlier work, in which he's constantly going in search of paradoxes in order to show that at no level except the very top level of the Absolute are you going to be free from paradoxes, accustomed him to looking for paradoxes. And so if he notices the paradox it's partly because he's been so used to thinking in this way. But the second point is that, since he'd dropped the notion that they could all be resolved at some higher level because Hegel had gone, he doesn't like this paradox because it threatens his completely new approach.

GRIFFIN: Remember also his remarks at the beginning of "On Denoting" about method, that it's a good idea, in tackling a philosophical problem, to look out for problems, and, as you know, he sets himself three in "On Denoting". As far as I know, he was the only logician of this time who actually provided a sort of encyclopedia of the contradictions, in *Principia*. He lists them—one after the other. He has actually scoured the literature for them. I don't know of anyone else who did that.

GRATTAN-GUINNESS: It is already in the 1905 paper—about twelve of them are there.

GRIFFIN: I mean that he was certainly out to collect them, and wanted a theory of logic which would resolve the lot.

GRATTAN-GUINNESS: This seems to me a continuation of his neo-Hegelian method. The context in which it is done is different, but the method can still be used, I think. I would disagree with Professor Ruja. I think it wrong to say that Russell was unfortunate to find the paradox. It would have been a much greater misfortune if he had not found the paradox—then somebody else would have done it for him! (Note the contrast with unlucky Frege—one big volume out and another in press when the paradox arrived from Russell.) To know the problem is there—this is the way in which his Hegelian habits, let us call them, were still useful to him.

AYER: They do lead him to recognize it as a problem, at least.

GRATTAN-GUINNESS: That was lucky, yes. Well, I think that is lucky. Since it is

undoubtedly there in the system, good that he got it fairly early on in his studies, and congratulations that he got it, too.

QUINE: You could be lucky that you've located the enemy, but you'd be even luckier not to have an enemy! (Laughter.)

TULLY: We have learned that the early Russell was finding antinomies by the score. He was doing so for a good ten or fifteen years before the Russell paradox. In fact, in Greg Moore's description there was more of an exploitation than a mere observing of a new contradiction because he saw at once its importance—that is, the effect it would have on a great deal of the work that he had planned. Whether or not Russell delighted in paradoxes, he certainly fought with them. But later, in his epistemological work, that old Hegelian habit of mind does show itself in the neutral monistic doctrine that he was developing in the 1920s. Although he doesn't present any paradoxes, although he doesn't describe any in terms of the language of antinomies, we have sharp contrasts constantly being developed—the space of physics *vs.* the space of psychology, the objects of physics *vs.* the objects of immediate experience. The neutral monistic doctrine, in some way, is an attempt, if not exactly to rise above, at least to remove the antinomy that is posed by the fact that if we work entirely in physics, we're left without any conception of the way in which we have those contrasting concepts in the first place whereby inference is possible; whereas if we work in psychology, we're left with the problem of solipsism. In an attempt to reconcile these Russell identified a class of things—the neutral things—which aren't the stuff out of which the entire world is constructed but in terms of which you can see the way in which physics relates to psychology.

PASSMORE: I think it's important that when Russell talks about his own life, he always thinks of himself as having suffered an immediate conversion. You know, roughly on Monday he was a Hegelian, and on Tuesday he wasn't a Hegelian. On Monday he was in love with Alys, on Tuesday he wasn't in love with Alys. But no one's ever like this; you always carry over something of the attitudes, habits and what have you from your older doctrine. You see this with anybody who gets suddenly converted from one system of ideas to another. The old system is never quite broken with. I think that these habits of mind that he had formed in his Hegelian years still had a big influence on him, although he dropped the notion that he didn't really have to worry about the antinomies because the Absolute was going to take care of them.

AYER: I should have thought—sorry, John, this is different—that it's most surprising how little he's worried about solipsism. When he writes in *Our Knowledge of the External World* and in "The Relation of Sense-Data to Physics" about constructing the physical world out of sense-data, he helps himself to everybody's sense-data without justification. He ignores any sort of problem about the sense-data of persons other than himself; and not only that, but he makes use of sensibilia which are unsensed sense-data. Now that's really a very much stranger thing than what people laugh at.

TULLY: I think the worry about solipsism is actually contained as much as possible by Russell in a form of cold intellectual respect for its potentiality. He recognizes that it is possible to develop, from a sceptical position, a solipsistic argument. There is no fundamentally successful counter-argument against it. Argument will be displaced in the presentation that he makes by the recognized assumptions that while

it cannot be disproved, it cannot be proved either, and that his system will allow an inferred status to certain entities such as other observers.

AYER: Yes, he wants to be naughty. He says things like, "Strictly speaking, I know only that I see a blue patch", or "Strictly speaking, I can't prove the world began more than five seconds ago", and so on. But then, having been naughty, he goes on quite happily to assume that he knows lots and lots of things which he's not entitled to say that he knows. Even with the theory of descriptions I don't think he can give a satisfactory account of our knowledge of other people.

RUJA: Was he in that frame of mind, do you think, when he said there's nothing but prejudice and habit for the view that there's an external world at all?

AYER: Yes. I think this was part of his naughtiness. He did eventually, of course, come to a position in fact very like a theory of Broad's, in thinking that the external world was something entirely beyond our reach. But it is extremely extraordinary to say that everything I'm now seeing is in my head—you, for example, you're *not* in my head. And yet, according to Russell, what I'm looking at is in my head and there is in a space not perceived by me something structurally corresponding to a percept, which is in fact your body. And this seems to me to be a conclusion which he has no warrant for whatsoever. And in fact optics demands that you should be roughly where you are, if I have to see you, not in a space which is entirely inaccessible to my sight. I think that Russell's much odder than people allow for, and because he was a friend of Moore's, they've assimilated him to Moore's defence of common sense, whereas the last thing Russell wanted to do was defend common sense. He thought common sense was a tissue of absurdity, or rather a hopeless mixture of percepts and physics.

QUINE: A metaphysics of savages! (Laughter.)

AYER: Yes, metaphysics of savages, that's right. And I think he was quite serious.

PASSMORE: Isn't there a problem that runs through a lot of empirical thinking, arising out of a great respect for science linked with a theory of perception which seems to make science just about impossible? I mean, because percepts or sense-data or whatever they are, are all personal. If you are prepared to take the view that science is a set of myths and fictions and all the rest of it, then you could reconcile the two. But if you take science seriously, you can't. And this is a recurrent problem.

AYER: I think that's right. As a battered phenomenalist, I have to agree with you! (Laughter.)

GRATTAN-GUINNESS: Is Russell caught on this view that "foundations must be secure" ?

AYER: I think so. The physical world, to him, is even, right down to the area of human knowledge, entirely conjectural. He thinks it to be a genuine possibility that there's no physical world at all. But then he doesn't go so far as to say, "Well, there's a genuine possibility that there's nothing but me and my sense-data." He lets other people in even though there's no physical world, which is (unless he's going to be a Cartesian) an absurd position.

TULLY: The point is that it's logically possible that there is no external world, but this is not accepted as a truth by Russell in the procedure that he describes. It's not so much a conjecture that there's an external world as something which we will count as rational human beings, which we can infer on the basis of our experience. One other point is that Russell was not suggesting a linguistic reformulation whereby we could suddenly address people in the street and say, "Did you know

that you're inside my head?" The language in which it becomes true to say this is the language of physics. But the language of physics is not the language that Russell will use in order to interpret the qualities of experience. That's why he distinguishes between the two different places: the one that works for practical purposes, the place in physical space roughly where the table is, but from the point of view of physics this is not the supervening point of view and comprehensive one—it's not inside the head.

AYER: You can't say the place in physical space is roughly where the table is—not where the perceived table is, because that's here.

TULLY: What he means is that from the same viewpoint of physics, the causal conditions of that which we identify as the table originate in a causal line from the region of physical space where we would, in common-sense terms, say a table is, even though that would be from the physical point of view an incorrect description. It's physics which proves common-sense to be wrong, but it's psychology which reconstructs a picture of the world which in turn is partly, not completely, interpreted and never, certainly, reduced to physics.

AYER: There is his famous remark that physics, if true, shows that naive realism is false; therefore naive realism, if-true, is false; therefore it's false.

TULLY: That's right. And by naive realism, I think he included the common-sense view of the world.

AYER: Yes, he did.

SLATER: I would like to go back to your very first remarks about *The Problems of Philosophy*. I have just been through virtually all of the letters to Lady Ottoline, and there's an interesting fact about that book that's not generally known. Russell undertook to write it under certain very rigid constraints. It was going to be part of a series, and certain problems couldn't be handled in it at all. He took this very seriously, and it was only a year later when he was reading it for revisions that he said, "I find to my great surprise that my 'shilling shocker' contains a lot of my own philosophy", as though he had not intended to put it in at all. It may be that the kind of inconsistencies that you're pointing to in that book come from this sort of motivation in writing it, namely that he was trying to write an introduction that wasn't his own, but in the course of it he couldn't keep himself out of it. And as a result he espouses, within the same book, an inconsistent position. I think that that is possibly part of it. I'd like your comment. But I'd just like to add one other quotation from the Ottoline letters. About 1917 or 1918 he wrote her and said, "I have come to the conclusion that the only things real are propositional functions." So he had explicitly come to this conclusion. Everything else was gone, including her! (Laughter.)

AYER: Wasn't she constructible out of propositional functions? (Laughter.)

GRIFFIN: He was trying to, very hard!

TULLY: Constructible but not retainable!

SLATER: I would like to know what you think of *The Problems of Philosophy* as exhibiting his philosophy, or whether it was just put together to instruct students in philosophy and happened also to contain some of his own views.

AYER: No, I think that in 1912 it was what he thought. A lot of your quotations from letters are put in to please her. I think that one of the troubles was that Ottoline reproached him for being a dry-as-dust philosopher, and he wanted to show that he wasn't, that he wasn't lacking in emotion. Hence the frequent references to passion.

They had been put in rather more to satisfy what Boswell would have called his "amorous propensities" (laughter) than to represent his actual view of the content of *The Problems of Philosophy*. But I think that he did believe, and consistently, that you can get physical objects in via descriptions. He was acquainted with sense-data. Moore defined a physical object as something that bore a unique relation to a sense-datum. Russell took this relation to be causal. It is interesting that in order to get that view, Russell has to give up what he otherwise adheres to as late as the *Mysticism and Logic* book, the Humean theory of causation; because if you take a Humean theory of causation, it isn't going to carry you outside the possibilities of experience. This is what Russell's theory has to do if he's going to get his physical objects in, in the way they're presented in the 1912 book and again in *The Analysis of Matter*. And this is something we know he never spots. He thinks he can combine the Humean theory of causation with the use of causation to bring in unobservables, but he can't, for it's an unresolved contradiction. But I should have thought, and you have much more knowledge of the contemporary evidence than I have, that *The Problems of Philosophy* did represent his epistemological views at that time.

SLATER: Well, there are just these letters that say he was surprised on rereading it that it includes his own views.

AYER: I think you're being a bit too harsh on him—as if he would have written a book which he thought false for general consumption just to please her.

SLATER: No, I don't think that. I think it's because he had instructions from Gilbert Murray to summarize the positions of all of the prominent philosophers, and in this letter where he talks about his own philosophy he's just had a three-hour session with McTaggart on whether his interpretation of Berkeley is right, and he says, "I spent yesterday in the library getting my sources up, McTaggart came with all his; we had three hours of argument—I persuaded him that my interpretation of Berkeley was at least right." He goes on to say, "In rereading the book I notice that I've worked a lot of my own views in." So you could say that he thought of it as summarizing historical positions (of course, through his own pen), and then he finds that he's put his own views in.

TULLY: I think you're right that the burden of reference to objects in physical space was carried, he thought, by the definite descriptions in that form. As far as I know, there is that same point of view expressed towards the end of "On Denoting" quite a number of years earlier: that by means of descriptions we succeed in managing to refer to things which do not fall within our acquaintance, such as the centre of mass of the sun—well, that would be mathematical, but to other things as well, such as physical objects.

GOLDSTICK: I don't agree with saying that by use of the theory of causation you can't infer unobservables from observables rather than saying merely that by use of a Humean theory of causal inference, you can't make such an inference legitimately. Why unobservables can't be constantly conjoined with observables isn't clear.

AYER: Because the Humean theory is that you're generalizing from observed constant conjunction. You experience causality in terms of the habit of passing from an impression toward an associated idea in virtue of your own observations of their past association. This would not be feasible if you never had observed one of the terms. Both of the terms have to be at least observable; in fact, in the context of Hume, actually as well.

GOLDSTICK: Do you mean the theory of causal inference? But the Humean theory of what causation is doesn't require such a limitation.

AYER: Oh yes it does. This comes into the analysis of necessary connection, which is what Hume thinks causality consists in. Spatial contiguity necessitates no connection, unless that connection is explained in terms of the determination of the mind to pass from an instance of one observed type to an instance of another.

GOLDSTICK: But Russell wouldn't have interpreted the situation that way.

AYER: He wouldn't have used the words "necessarily connected" in that way, no, because he would say that causality contained no necessity at all. In fact, he analyzed it in terms of differential equations, surprisingly, in physics. But basically his attitude is Humean. You see, your interpretation would violate the principle that I started out with: that every proposition we can understand must be composed of constituents with which we're acquainted. Or *ex hypothesi*, we're not acquainted with tables, chairs, and so on.

GOLDSTICK: We could have the universal conception of cause and be acquainted with that and then infer causes of the sense-data from that.

AYER: Yes, so that the physical objects will be defined simply as the causes, whatever they may be, of my sense-data.

GOLDSTICK: That sounds like *The Problems of Philosophy* to me.

AYER: I agree. But it doesn't let in much contemporary physics—you don't get atoms and quarks and neutrinos and so on. That would be a way out, I agree.

QUINE: You get the *ding-an-sich*. That's about the size of it.

AYER: The *ding-an-sich*, exactly—although he had, very curiously, a great hostility to Kant.

GRATTAN-GUINNESS: Although he would not have liked you to put it that way.

AYER: I don't know why. For some reason he regards Kant as a great disaster in philosophy—"a term that should have been reserved for Hegel", said Broad. (Laughter.)

GRATTAN-GUINNESS: Well, that may be the answer. It is guilt by association with the view that he had abandoned.

AYER: I think he chiefly disliked Kant because of Kant's synthetic à priori view of mathematics. The analyticity of mathematics was very important for Russell.

WINCHESTER: Time dictates that we must call this discussion to an end. Thank you very kindly, panel and everyone else. (Applause.)