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Paradox, Natural Mathematics, Relativity and Twentieth-Century Ideas

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Introduction

New historical research shows that twentieth-century thought was expressed in terms of the "natural" mathematics developed at the turn of the century in order to cope with the supposed "paradoxes" generated by Cantorian set theory. Economics, physics, biology—apparently no area of inquiry has escaped being made part of the "natural" mathematics project. This mathematics asserts that mathematical formulations are inherently anomalous; the evidence of this is that they generate paradoxes. Therefore, the idea that mathematics is an aspect of human perception, must be made a part of mathematical formulations even though it plays no internally consistent role in any "natural" mathematical formulation.

The role of "natural" mathematics has gone unremarked for the very reason it was influential in the first place. Whether the researcher was the physicist Albert Einstein, the economist Piero Sraffa, the logician Kurt Gödel, the philosopher Ludwig Wittgenstein, or the biologist Motoo Kimura, scientists in non-mathematics disciplines felt they were unable to express their ideas mathematically. This is the chief revelation of the new historical research, and a remarkable and unexpected (given the exalted reputations of these figures) unifying feature of twentieth-century intellectual history. These thinkers had to search for appropriate mathematical terms in the latest mathematics of their day. They were unprepared to cope with the idea that flaws in the mathematics lodged errors in their theories. The current reexamination of the mathematics of the disciplines began with the revelation of the faulty approach taken to set theory by some of the chief proponents of "natural" mathematics.

It should be noted that this unification of twentieth-century ideas on the basis of the "natural" mathematics they share, was not the unification sought by twentieth-century thinkers themselves. It has gone pretty much unremarked that twentieth-century thinkers sought to unify the disciplines on the basis of relativity. It has gone unremarked largely because the project was abandoned when physics developed terms of art so recherché that the data and concepts of other disciplines could not be matched to them in an internally consistent way. he approach was swiftly abandoned, and suppressed out of embarrassment. As we shall see, bringing Einstein's work into alignment with "natural" mathematics—something which has not been possible until now—allows us to begin asking the kinds of questions which will in the end reveal precisely and in detail, the influence of "natural" mathematics with which we still live and in which we still express our scientific ideas.

With the appearance of the general relativity theory, it became increasingly difficult for other disciplines to "map" their own terms of art to those of relativity in an internally consistent fashion. But we know now that it was attempted very high up in the western intellectual hierarchy, as Galbraith[1] has shown in his work on Keynes. Ernst Mayr, at one time the *doyen* of evolutionary studies, claimed during the 1950s that evolution could be seen as a genetic theory of relativity.[2] However, the concept dropped unexplained out of his later writings. Today, of course, we say that it's impossible: there are no quarks in biology, no leptons in economics and certainly no charm in mathematics. You can't get, logically, from any concept in any of those disciplines, to any concept of the Standard Model. We smile at the naiveté of Keynes for even attempting what until very recently we considered quite impossible. And yet it is not altogether fanciful to see internally consistent links between the relativistic world and the biological or economic worlds. After all, light is one of the postulates of relativity, as it is in biology, and humanity is part of biology, and economics the study of one aspect of humanity.

Links like that, however, didn't arouse the competitive instincts of early twentieth-century intellectuals. What did arouse them was the idea that Einstein's special relativistic argument had wound up at the top of the heap of argumentation. His rhetorical strategy is what proved so seductive. We are starting to unpack that now in the twenty-first century, as I shall show and as Andrea Cerroni[3] has shown. However, at the time of its appearance (although Einstein was frustrated at how long it took to gain recognition even after the publication of the 1905 papers), what impressed intellectuals was the special relativistic argument *qua* argument—above all, the relativistic "event," what today we would call a spacetime point.

To them it was a matter simply of ignoring the subject matter—the materials—of the argument, and just looking at the argument as an internally consistent structure. It was gorgeous—it had no flaws. What was even more impressive was that it required Einstein himself to point out the limitations of special relativity. If you could come to terms with his argument, then you could configure the terms of your own discipline so that they mapped to relativity in an internally consistent way. Then you would have a relativity theory of economics, or biology—or even mathematics!

It must be noted that we are still enamored of the explanatory power of the Standard Model, despite its having turned into something like a Christmas tree.[4] For this reason, historians of ideas pay little attention to the idea that the fundamental ideas of relativity are simply shared by the other disciplines. We are still in an early stage of the examination of the influence of "natural" mathematics. The apparently bad experience of earlier attempts to unify the disciplines, along with disciplinary hubris, still makes us leery of revisiting the settled questions of the various disciplines. And there is nothing wrong with respecting the boundaries these disciplines have set up for themselves. In fact, it allows us to take the chief current ideas of different disciplines one by one, examining them on their own terms in light of the latest mathematical historical research. This examination begins to reveal their shared ideas, and the overarching concerns of twentieth-century thinking. In the course of this examination, we shall see that we have begun to free ourselves of many received ideas.

One of the most important goals of the discussion which follows, is to briefly introduce specialists to major monuments outside their disciplines and to provide reasons for specialists to familiarize themselves with these works which, initially, may seem to be

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[1] James K. Galbraith, "Keynes, Einstein and Scientific Revolution," *The American Prospect*, Volume 5, Number 16 (1994), 62-67.
[2] Motoo Kimura, *The Neutral Theory of Molecular Evolution* (Cambridge 1981), 20-21.
[3] "Discovering relativity beliefs: towards a socio-cognitive model for Einstein's Relativity Theory formation," *Mind & Society*, Volume 3, Number 5 (2002), 93-109. See also D. Howard and J. Stachel, eds., *Einstein the Formative Years 1879-1900*, Basel 2000.
[4] See Peter Woit's horribly funny account of truly farcical string theory, which has made physics the laughingstock of the intellectual world: *Not Even Wrong*, New York 2006. The elaborations of this bad joke are a sure sign that we are at the end of an era. And Woit's work doesn't even take into account the incorporation of "natural" mathematics in physics!

remote from their concerns. Why should a chemist read Sraffa, or an economist read Kimura? Hopefully, the linkage of these writers through "natural" mathematics, will provide, above all, the stimulus for specialists to reexamine ideas in their own fields which they take too much for granted.

Piero Sraffa's Economics of "Natural" Mathematics

Production of Commodities By Means of Commodities[5] is still the most advanced work of economics and one of the chief artifacts of the twentieth century. How does this famous work relate to relativity? We know now that Sraffa read books by Whitehead, Einstein and discussions of quantum mechanics.[6] By the time he came to these works, "natural" mathematics was well under way. The "paradoxes" were so well accepted that their origins—the exploration of which is the means by which Alejandro Garciadiego reveals their flaws—had been buried. What we don't know is the extent to which Sraffa went beyond a general understanding of the terms he read and was able to use them in their own context as terms of art. By the time he started working, had he imbibed enough "natural" mathematics through other means that what he read merely confirmed him in his procedures and terms?[7] It appears, by the way, that Wittgenstein never read a word of Einstein—at least I have seen no documentation of it, although there are comments on relativity in his remarks on the foundations of mathematics[8] and other places.

Sraffa was not, I think, sufficiently aware of the polemical program of "natural" mathematics to be on his guard against it, and so he did not set himself the task of looking into its terms. Nevertheless, he may have sensed that something was amiss, and may have simply been trying to express his misgivings using the received terms of art of economics. Examining *Production* as a form of protest may, in the end, make a lesser but more useful figure of Sraffa. That certainly seems the way we are beginning to examine twentieth-century mathematics itself. It is an approach which allows works which, otherwise, are strangers to each other, to "talk" to each other.

Sraffa, of course, tried his hand at unifying economics and physics, without much success. It does not appear that he was particularly on his guard about the whole notion of terms of art, and he tended—at least early on—to pile one vague term on another. Look at the project he set for himself: "I foresee that the ultimate result will be a restatement of Marx, but substituting to his Hegelian metaphysics and terminology our own modern Metaphysics and terminology: by metaphysics here I mean, I suppose, the emotions that are associated with our terminology and frame…[,] that is, what is absolutely necessary to make the theory living…, capable of assimilation and at all intelligible….This would be simply a translation of Marx into English, from the forms of Hegelian metaphysics to the forms of Hume's metaphysics."[9] But "translation" is not really what goes on when the basis of a later theory, is the pointing out of an internal inconsistency in an earlier theory: the earlier theory is not "in a different language"—it is simply wrong; more precisely, it is meaningless. No translation brings meaning to light in something which has no meaning to begin with.

[5] Cambridge 1960.

[6] Heinz D. Kurz and Neri Salvadori, "Representation of the Production and Circulation of Commodities in Material Terms: On Sraffa's Objectivism," http://econ.em.tsinghua.edu.cn/CRPE114701.kurz&salvadori.pdf, 82-85.

[7] He may have had nearly first-hand exposure to "natural" mathematics through the work of G. Vivanti, who reviewed works by Cantor and with whom Cantor corresponded. See H. Kurz and N. Salvadori, "Sraffa and the Mathematicians," in T. Cozzi and R. Marchionatti, eds., *Piero Sraffa's Political Economy* (New York and London 2001), 256; I. Grattan-Guinness, *The Search for Mathematical Roots* 1870-1940 (Princeton 2000), 112, 122.

[8] I take his remarks on experiments to be criticisms of relativity, but perhaps I am assuming too much about his knowledge.

[9] Quoted in Kurz, "Sraffa's Reception of the German Economics Literature: A Few Examples." Previously, but no longer, found on the web. Contact: www.kfunigraz.ac.at/vwlwww/kurz/kurz.html.

Sraffa's idea was naïve even at the time. If "translation" was ever used to link Newtonian to relativistic mechanics, it was an indulgence to a relativistic beginner, it was designed to lead the beginner to an understanding that Newtonian mechanics had no logical content. Neither is it clear what he means by the rest of his terms. What did he mean by "our own modern Metaphysics?" What "Metaphysics?" Certainly relativity was a protest against "metaphysics," if by that Sraffa means arguments which depend on terms not internally consist with all the other terms of the argument. On what basis did he feel that "Hume's metaphysics" were "our" metaphysics? And what did he mean by "Hume's metaphysics?"

Needless to say, all this was going very far afield of economics, but it was not necessarily fatal. However, when some connection is claimed based on "Metaphysics," we certainly have the obligation—and he had the obligation—to inquire into the connection between his conception of "Metaphysics" and the conclusions he reached as the meaning of received terms of art in his discipline. Which means that he should have investigated more closely the nature of the mathematics he used and, for that matter, the natures of the most advanced physical theories of his day: evolution and relativity. Whatever else he meant by "Metaphysics," he seems—when he uses such terms as a "living" theory—to have meant those arguments which seemed to be logical to his contemporaries (although the term seems to be grounded in Romanticism). That certainly seemed to be his complaint about Marshall's work: it claimed to be state of the art, but it was internally inconsistent. I don't notice that he ever posed that query to any of the mathematicians he consulted, or to evolutionary theory, or to relativity theory.

Instead, Sraffa seems to have regarded relativity as standing for the proposition "that for every effect there must be sufficient cause, that the causes are identical with their effects, and that there can be nothing in the effect which was not in the causes: in our case, there can be no product for which there has not been an equivalent cost, and all costs…must be necessary to produce it."[10] These commonplaces were, of course, a serious misreading and later a misapplication of relativity, further compounded by an even later putative rejection of the misreading. Sraffa disliked subjectivism in economic analysis, but according to relativity, terms such as "cause" and "effect" are problematic—they simply are not relativistic terms of art.

At the same time, if Sraffa's use of generalities seems to take him further and further away from relativity, the linkage between Einstein and Sraffa in terms of "natural" mathematics, makes possible an evaluation which previously was not possible. We can formulate a much more precise question in an attempt to carry out Sraffa's project to "translate" economic terms of art into "our own modern Metaphysics." For example, is the following statement by Sraffa, nothing other than the restatement of a spacetime point?

> [F]or circulating capital, at the same moment that its value passes into the product, in most cases, also the material substance which is the bearer of that value, either passes into the product (raw material) or anyway passes out of the process of production (e.g. fuel). On the other hand, for fixed capital, the transfer of value from, e.g., the machine to the product, appears as a purely abstract process, which takes place without any corresponding transfer of material substance: that value is passed is undoubted, for the machine decreases in value while the product increases, but the machine remains complete in all its parts, with its efficiency unimpaired for the time being, and ready to resume operation in the next year. In order to see how this abstract process takes place an abstract point of view is inevitable.[11]

[10] Kurz and Salvadori, 88.

[11] Heinz D. Kurz and Neri Salvadori, "Removing an 'Insuperable Obstacle' in the Way of an Objectivist Analysis: Sraffa's Attempts at Fixed Capital," http://www.kfunigraz.ac.at/heinz.kurz/pdf/SraffaOnFixedCapital.pdf, 24.

It seems now that every term Sraffa used—production, commodity, and so on—are terms of art, with no lay meaning, which means that everything he wrote has to be reinterpreted in terms of "natural" mathematics and the problems with that mathematics. What is the geometrical expression of Sraffa's statement above? By way of contrast to his previous statement, this statement introduces a term of art, the term "abstract," by means of which it seems that all the other terms in the statement become terms of art as well. Consider, for example, that we cannot understand the word "capital" as used here, as having any of the meanings we previously associated with it, but instead, only the one Sraffa gives it in his argument. Since this opens up the possibility that that argument is the "natural" mathematical argument, we can in turn subject it to questions relating it to relativity as another expression of "natural" mathematics:

1. What are Sraffa's assumptions here about light? about biological theory (considering *Production* deals with agricultural production)?
2. What is the economic "event" here, regarding that as a spacetime point?
3. Does the approach here reflect the "natural" mathematics as of the 1942, when it was written, or the developments of physics of the same period? We think of the "developments" of "natural" mathematics as ridiculous, rather like the "development" of phrenology. However, its practitioners were—and are—busily scribbling away. Did Sraffa "keep up" with this nonsense and "incorporate" it?
4. What are Sraffa's mathematical assumptions in this statement? Are they entirely Euclidean, or Euclidean at all? Remember that Einstein adopts strict Euclidean ideas as the assumptions of special relativity, along with the constancy of the speed of light.
5. Does the train experiment in *Relativity* map logically to the *Production* "event"?
6. Above all, is the Standard Commodity an artifact of "natural" mathematics? It would seem so.

We shall have occasion to give Einstein's formulation of a spacetime point as this same train experiment, and open up the possibility of setting Einstein's and Sraffa's statements side by side as expressions of one idea, or different aspects of one question. In this latter statement of Sraffa, what "paradox" is he trying to express, what "paradox" is he trying to avoid?

Perhaps a good place to begin understanding Sraffa's relation to the set-theoretic "paradoxes," is Karl Marx's own concern with paradox. Marx's intellectual career began with the study of logic, and his mathematical writings reflect his emphasis on "the distinction between the real contradictions characteristic of reality and the contradictions of the sophist type," the latter giving rise to the "paradox."[12] Given Sraffa's own interest in finding a mathematical expression for his ideas, perhaps he sought to mediate between the two notions, and so found attractive a mathematics derived from Cantor's own putative ability to deduce rules from reality. However, he would also have had to grapple with Cantor's ill-defined notion of "intuition" as differentiating the rule from the reality. Is this what Sraffa meant by a "translation of Marx?" We need to know much more about the written comments Sraffa made on his readings of Marx.

Kurt Gödel's Insufficient Examination of "Natural" Mathematics

It is clear now that Alejandro Garciadiego's book[13] on the set-theoretical "paradoxes" is a dagger pointed straight at the heart of Gödel's theorem. Above all, this devastating book shows that the various paradoxes which so entranced Bertrand Russell and his contemporaries, weren't paradoxes at all—they weren't anything at all, they were

[12] V.I. Przhesmitsky, "On the Operational Logical Apparatus Imperative in Karl Marx's 'Capital' and 'Mathematical Manuscripts,'" in Karl Marx, *Mathematical Manuscripts*, ed., by V. Parisad (Calcutta 1994), 428.
[13] Alejandro Garciadiego, *Bertrand Russell and the Origins of the Set-Theoretic 'Paradoxes'*, Basel 1992.

nonsense, letters pulled out of a bag. For example, he shows that the famous "paradox" of Cesare Burali-Forti simply does not exist. In the context of an attempt to prove the Trichotomy Law, Burali-Forti tried "to prove by *reductio ad absurdum* that the hypothesis [involved in his own argument] was false and this method required supposing the hypothesis true and arriving at a contradiction. The employment of the hypothesis, as an initial premise, generated the inconsistency. But once the hypothesis is seen to imply a contradiction it is thereby proved to be false."[14]

It is doubly disconcerting to note that Gödel approvingly cites Richard's paradox in his 1931 paper. Gödel accepted the false but widely held tradition that Richard argued that truth in number theory cannot be defined in number theory. It turns out that what is undefined in Richard's argument (as he himself pointed out) is the number crucial to making the argument. As Garciadiego notes, Richard called his argument a "contradiction," not a paradox, and, specifically referring to his formulation (his paradox—and all notable ones—are available online) said that "the collection G had meaning only if the set E was defined in totality; this could not be done except with infinitely many words." However, nothing daunted, Gödel added to Richard's argument the idea that provability in number theory can be defined in number theory, and came up with mistaken result that if the provable formulae are all true, then there must be some true but unprovable formulae. Gödel depends, for an internally consistent distinction between truth and provability, on the idea that there is some logical content to Richard's "paradox." Because that "paradox" has no logical content, we are left not with an argument, but instead with a question not previously: what is Gödel's argument? Is he actually making an argument? This change in attitude toward Gödel's theorems, is one of the first revolutions wrought by the historical inquiry into "natural" mathematics—but it is not the last. Above all, as we shall see it allows us to link Gödel's ideas in an internally consistent way, to those of other twentieth-century thinkers, the goal of our present inquiry.

And special relativity? In fact, we know very little about Gödel's study of relativity through the years, apart from his rather uninteresting later relativistic studies, and Solomon Feferman in his editorial notes to Gödel's *Works* is quite dismissive of some of Gödel's restatements of relativistic ideas—in fact, he is rather dismissive of some of Gödel's restatements of Gödel's *own* ideas. When did Gödel first read the 1905 papers, or did he ever read them? We just don't know.

This leads us to ask the same sorts of questions about Gödel's paper as we do about Sraffa's book. Is there an assumption about light in that paper? This seems a very odd question, even an inappropriate one, to ask about a mathematical argument. However, Gödel provokes it with this remarkable statement in his paper: "Numbers cannot in fact be put into a spatial order"—this is the infamous footnote 8. What does he mean by a fact? by space? What are the Euclidean assumptions, if any, of the paper? What, in special relativistic terms, is a Gödelian event? Is Gödel's theorem an argument at all, and if so, is it, not a metamathematical argument or even a piece of formal logic, but in fact a straightforward physical theory? Is the paper nothing more than a retelling of Einstein's train experiment?

Motoo Kimura's Search for a "Natural" Mathematics

It may well turn out, based on an improved understanding of "natural" mathematics, that it was not Einstein who developed the special relativity theory, but instead, Mendel and Darwin, because the rhetoric of geometry—the "natural" geometry—in both Mendel's paper and Darwin's *Origin* is what we now recognize as demonstrably similar to the geometry Einstein sets forward in the train experiment in

[14] Garciadiego, 24.

Relativity. Only an understanding of "natural" mathematics makes this linkage possible. Just as Einstein sets it forward to articulate the physical event, so Mendel and Darwin use it to articulate the biological event. It is in biology, of course, that we are most justified in asking for an internally consistent discussion of light. Do Darwin and Mendel, and later Motoo Kimura, have light as an assumption in their arguments, and what is that assumption? Are their assumptions Euclidean? Or better yet, if Einstein were to posit a relativistic biological event, how would he express it? Or is he expressing it? Is selection the relativistic event?

These are not questions necessarily restricted to special relativity. This is because Kimura is a statistician. His increasingly sophisticated use of statistical concepts led him to a mathematical apparatus which, in *The Neutral Theory of Molecular Evolution*, looks remarkably similar to the mathematical apparatus of, say, Richard Feynman's *QED*.[15] The modern discipline of statistics grows out of "natural" mathematics. Are the similarities internally consistent? Is Kimura's random drift—responsible, in his view, for most mutation, rather than selection pressure—an exception to selection, or is it an exception to relativity? What is his biological event: substitution? mutation? selection? something else? Is the neutral theory a biological theory, or a physical theory? This latter question arises in considering a comment drawn from Kimura by a critic. In response, Kimura says: "Just as synonyms are not 'noise' in language, it is not proper to regard the substitution of neutral alleles simply as noise or loss of genetic information….It seems to me to be more appropriate to say that strictly neutral alleles are absolutely noiseless."[16] These metaphors are physical ideas. Of what?

The basis for unfolding the context of the terms of art of these different disciplines, is the understanding that they emerge from a shared "natural" mathematics. The latest expression of this point of view is self-confessedly *ad hominem*: "humans are so constructed as to conceptualize the world in terms of some simple fundamental categories (*e.g*., as comprised of individual objects standing in various relations); that the world, to a large extent, is properly described as so constructed (up to the point of quantum mechanics, at least); and that a rudimentary logic is implicit in these shared structures…."[17] Neither Kimura nor Sraffa came to his discipline from mathematics, and they felt they needed a mathematical expression for their ideas. Kimura learned French rather late just so he could read Gustave Malécot—who pioneered the use of "natural" mathematics in biology—and Sraffa went, like Diogenes, through mathematician after mathematician searching for the mathematical expression of his ideas. We still need to clarify the doctrinal influence on Sraffa of two "natural" mathematicians—Frank Ramsey and Abram Besicovitch—as opposed to the technical assistance they gave him. At any rate, Ramsey spent much of his brief career exploiting a quixotic and quite baseless assumption of difference between types of "paradoxes"—which were not paradoxes. Did he put Sraffa in the picture on the problems with the set-theoretic "paradoxes?" Almost certainly, no. Was Sraffa in a position to ask about them? No. Did Ramsey himself bother to find out about them? No.

Historical research is revealing the difficulties in the chief ideas of "natural" mathematics. For example, L.E.J. Brouwer promulgated what he called an "infinite ordinal number." Supposedly this notion had been ratified by Georg Cantor's well-ordering of the ordinal numbers. But it turns out that Cantor never did so, never claimed he had done so, and never used the term "infinite ordinal number." As Garciadiego says: "[G. G.] Berry maintained that Cantor had virtually proved the existence of the well-ordering of the ordinal numbers by showing that ordinals of the second class are well-

[15] Princeton 1985.
[16] Kimura, 50.
[17] Penelope Maddy, "Three Forms of Naturalism," in Stewart Shapiro, ed., *The Oxford Handbook of Philosophy and Mathematics* (New York 2005), 450. See also her *Naturalism in Mathematics*, New York 2005.

ordered….but Cantor simply indicated that 'we shall show that the transfinite cardinal numbers can be arranged according to their magnitude, and, in this order, [they] form, like the finite numbers, a 'well-ordered aggregate' in an extended sense of the words.'"[18] Nevertheless, Brouwer's term worked its way into the discourses of Émile Borel (the mentor of Malécot), Andrei Kolmogorov, Haskell Curry and John von Neumann, and is, regrettably, at the heart of contemporary probability and computational theory; computer science is replete with "natural" mathematics—what false results is it thereby giving us?

The project of "avoiding" or "solving" the "paradoxes," comes almost immediately to dominate twentieth-century mathematics itself, with all the problems inherent in addressing issues which do not exist. It is worth noting that neither Frank Ramsey nor Alonzo Church nor Alan Turing—nor other figures such as Kurt Gödel, Rudolf Carnap or Alfred Tarski—ever considered whether the "paradoxes" might be simply meaningless. They all believed that these arguments had at least some logical content, and that that content had implications with which they had to deal. From this initial error, many other errors followed. As Garciadiego makes abundantly clear, the "problems" of the "paradoxes" proceeded in no way from logic, but instead, from Russell's megalomania. Alonzo Church had problems with definitions: "A function is a rule of correspondence by which when anything is given (as argument) another thing (the value of the function for that argument) may be obtained." The problem is the word "thing," which is never defined. Church subscribes to the "definition of simple order in terms of the relation precedes," which he attributes to Cantor. However, this attribution is in the context of Cantor's formulation of the notion of a set, a notion, as Garciadiego says, comprising "properties…so unsound that the theory seems to be the product of a charlatan."[19] Indeed, one of the most important revelations of the new mathematical historical research is, Cantor as natural mathematician: "Cantor tried to develop organicism with all the conceptual and methodical rigor of mathematics: he scored 'dialectical logic' and tried to penetrate into the matrix notion of the continuum by studying point-sets and the mathematical infinite. He attempted to become a Newton of the organic world, developing the needed mathematical tools and applying them to natural phenomenon."[20] There was not paradox, or anything else, to be found in such a project.

It is likely that we can put most twentieth-century disciplines in the form of Richard's "paradox," see how they partook of "natural" mathematics, and reveal their flaws. Now that we are more familiar with the idea that the project of the twentieth century—regardless of discipline—is "natural" mathematics, it is probably best to approach any idea in a twentieth-century discipline with two questions: what "paradox" is it trying to avoid? what "paradox" is it trying to express?

It should not be surprising if biology turns out to be a branch of physics. Most of Gregor Mendel's published papers are in meteorology. Charles Darwin began as a physicist seeking to describe reality and that concern is recurrent. He first sought to do so in the context of cosmology and geology and only later turned to biology, as we see when he presents his physical ideas in a book no one reads anymore, *The Structure and Distribution of Coral Reefs* (1842).[21] For Darwin, the identity of physics and biology is due to the progressivism of reality. Nature—encompassing all the disciplines—is the

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[18] Garciadiego, 134 and note 3.

[19] See A. and S. Feferman, *Alfred Tarski: Life and Logic*, (New York 2004); Jérôme Dokic and Pascal Engel, *Frank Ramsey: Truth and Success* (Cambridge 2002); Alonzo Church, *The Calculus of Lambda-Conversion* (Princeton 1951), 1-2; Alonzo Church, *Introduction to Mathematical Logic* (Princeton 1958), notes 541, 550; Garciadiego, 9.

[20] José Ferreirós, "The Motives behind Cantor's Set Theory—Physical, Biological, and Philosophical Questions," 17 *Science in Context* 1/2 (2004), 67.

[21] Darwin's physical ideas should be looked for, in part, among his theological ideas. See Dov Ospovat, *The Development of Darwin's Theory: Natural History, Natural Theology, and Natural Selection, 1838-1859*, New York 1981.

continuum of that progressivism; paradox supposedly flowed from the tension between perfection as an assumption and progressivism as a conclusion. Both Mendel and Darwin seem to have turned to biology because it offered more, and more internally continuous, physical data than cosmology or geology. Of all twentieth-century researchers, it appears to be Kimura who took his discipline closest to relativity. Is that true? Both Darwin and Kimura set their work in the context of physics. Darwin says "that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being evolved." Kimura's gloss on this passage is to remind us that although mutational "random processes are slow and insignificant for our ephemeral existence, in the span of geological times, they become colossal."[22] Indeed, perhaps a clue to understanding Sraffa's use of "natural" mathematics, can be found in this comment on Marx by David Riazanoff, the editor of his notebooks: "If in 1881-82 [Marx] lost his ability for intensive, independent intellectual creation, he nevertheless never lost the ability for research. Sometimes, in reconsidering these Notebooks, the question arises: Why did he…expend so much labor as he spent as late as the year 1881, on one basic book on geology, summarizing it chapter by chapter."[23] What was Gödel's or Sraffa's theory of geology? We in turn hunt among concepts such as "fixed law," "gravity," "random" and "geological times" for the necessary internal links between geology, physics and biology…but perhaps these words have fallen apart and we cannot use them anymore. It appears, in any event, that if physics was the monarch of twentieth-century science, during the nineteenth century, the resort was to geology to test all theories. Perhaps we don't understand twentieth-century thinkers very well because they're not twentieth-century thinkers: they're nineteenth-century thinkers. And as for nineteenth-century thinkers (and before), we don't understand them very well, either, because we don't understand the prejudices we share with them. The neutral theory of molecular evolution is said to remove many facts from selection. Much more important is the idea that Darwin and Kimura use "natural" mathematics. This is a charge laid against both of them.

Another idea is also beginning to take shape: there are no "paradoxes," at least as far as we know. Researchers, it seems to me, have resisted looking into the set-theoretical paradoxes because it leads us further and further back in time and so implicates more and more important ideas. If the set-theoretic "paradoxes" are not paradoxes, are the earlier paradoxes (for example, the liar paradox) really paradoxes? And more importantly, to what extent are the earlier mathematical expressions in the various disciplines, simply projects to "avoid" or "solve" these paradoxes, which in turn may not be paradoxes at all? To what extent is the history of objective discourse, a falsely based "natural" mathematics having no logical object? To what extent can we say to everything we currently consider to be internally consistent: what is your argument?[24]

And relativity? In taking even a retrospective glance at the works of only three twentieth-century figures in relation to relativity, we are free to put ourselves very far in the future, at a time when an internal inconsistency has been found in relativity itself and

[22] Kimura, 327.

[23] Kevin Anderson, "Uncovering Marx's Yet Unpublished Writings," in Scott Meikle, ed., *Marx*, Abingdon (U.K.) 2002.

[24] It is, of course, Artistotle who tells us early on that searching for a way to "solve" or "avoid" paradoxes is a task we must undertake: "Aristotle does not want to expose,but to kill off the paradoxes. This stance is exemplified in the basic axiom shoring up both his metaphysics and his logic, the *Principium Contradictionis* or contradiction principle…: it is not admissible that something is and is not in any sense at the same place at the same time….For Aristotle paradoxes are a problem most urgently in need for a solution." But look at Zeno's paradox: "if they are many [things], they by necessity are as many as they are, not more nor less. But if they are as many as they are, they will be finite [bounded, peperasmena]. But if they are many, they will be infinite [unbounded, apeiron]. For there will always [aei] be others [hetera] in between [metaxu] of the beings, and there again others in between." The problem is that Zeno does not define "they," failing which there is no argument. Quoted in Karin Verelst, "Zeno's Paradoxes," http://arxiv.org/PS_cache/math/pdf/0604/0604639v1.pdf, 34, 3.

that theory is an historical artifact. Then the three look to be, not attempting to map their work to relativity, but rather, using the inherited concepts of their respective disciplines to critique relativity, looking for an internal inconsistency which actually lies in the "natural" mathematics Einstein shares with them. Consider this passage from Lawson's accurate translation of Einstein's *Relativity*:

Are two events (*e.g.* the two strokes of lightning A and B) which are simultaneous with reference to the railway embankment also simultaneous relatively to the train? We shall show directly that the answer must be in the negative. When we say that the lightning strokes A and B are simultaneous with respect to be embankment, we mean: the rays of light emitted at the places A and B, where the lightning occurs, meet each other at the mid-point M of the length AB of the embankment. But the events A and B also correspond to positions A and B on the train. Let M1 be the mid-point of the distance AB on the traveling train. Just when the flashes (as judged from the embankment) of lightning occur, this point M1 naturally coincides with the point M but it moves…with the velocity…of the train.[25]

This passage is by now so familiar that we think there can be nothing new to be seen in it. But there is: it is the term, "naturally coincides." This term ("fällt zwar…zusammen" in the German) leaps out at us because we are looking at it with twenty-first century eyes, not twentieth-century eyes; indeed, perhaps the most difficult cultural task now before us is simply to realize that we are not living in the twentieth century.

"Natural" coincidence is otherwise known as a spacetime point. Einstein has already spent twenty-odd pages of this very brief book laying out the assumptions which underlie the train experiment. He is very careful about being consistent with them, and he is a devoted and very strict Euclidean. But Einstein was not, it appears, quite careful enough. We know that he is assuming, along with Euclid, that the definition of the coincidence of two points is a point. However, we have never gotten (and never get, in any of Einstein's writings) a definition of a "natural" coincidence of two points. This alone prevents us from going on and this argument, which defined the twentieth century, abruptly ends. We also have a problem if we try to resolve the issue ourselves. If we simply drop the term "naturally" we run into a situation in which Einstein has told us to assume two Cartesian coordinate systems, but now leaves us with one, since, following from the definition of the coincidence of two points, if two parallel coordinate systems coincide at one point, they coincide at all points and are one coordinate system, not two. We have been led to a contradiction. A comment by Einstein illustrates his unprompted conflation of "natural" mathematics and geometry:

It is clear that the system of concepts of axiomatic geometry alone cannot make any assertions as to the relations of real objects of this kind, which we will call practically-rigid bodies. To be able to make such assertions, geometry must be stripped of its merely logical-formal character by the co-ordination of real objects of experience with the empty conceptual frame-work of axiomatic geometry. To accomplish this, we need only add the proposition:--solid bodies are related, with respect to their possible dispositions, as are bodies in Euclidean geometry of three dimensions. Then the propositions of Euclid contain affirmations as to the relations of practically-rigid bodies.[26]

It is important to note that these statements play no logical role at any stage of the relativity theory—in particular, they are not among the assumptions of the relativity of simultaneity. However, they play a vastly important cultural role—the examination of which is beginning with the new mathematical historical research—in the theory of relativity and in an increasingly large number of other important ideas. It hardly needs pointing out that not only is there no "stripping" in the train experiment; there is no "adding," either. Einstein shows in this passage that he has imbibed "natural" mathematics; the train experiment itself, however, merely shows an internal inconsistency. Perhaps Einstein would not have made this mistake had he inquired into

[25] New York (Fifth edition, 1952), 19-20.
[26] *Sidelights on Relativity* (New York, 1983), 31-32.

the background of Poincaré—having a good opinion of Einstein, we like to think that he would have realized it was rubbish. On the other hand, he never did make this inquiry, and he lived for *fifty* years after the 1905 papers. That's not so excusable—it's laziness, and self-satisfaction. Today, Einstein's statement looks to be an astonishingly inept basis for a world-renowned argument. As a point of view, it is nonsense. We like to think that can only ever have been accepted as a basis for relativity because it seemed to create no problem for relativity. And yet it is not surprising that Einstein's acceptance of Poincaré's point of view should have led to a logical error in relativity. Poincaré may have had other virtues, but logic was not his forte. As the historian of set theory Ivor Grattan-Guinness points out, Poincaré had a "contempt for logic (and also ignorance of it)…." Poincaré understood mathematical logic "not very deeply…."[27] And yet it needs to be clearly understood that those who find internal consistency in relativity have one opponent with whom to contend: Einstein. We see now that he never intended relativity to be internally consistent, and he made sure that it was not internally consistent. The question is, why anyone ever thought it was internally consistent?

How can the manifestly insupportable protocol of inserting a statement arbitrarily in an argument, have gained acceptance? First, it becomes ever clearer that it has been accepted in mathematics itself as far back as recorded history takes us: precedent sanctioned it. Also, it gave acceptable results, both within mathematics and in disciplines which used mathematics, and the procedure itself was not identified. It was sanctioned by highly regarded thinkers. There was not sufficient strength of mind to resist it. Finally, there was ignorance and misunderstanding of the history of all the disciplines—above all, of mathematics on the part of specialists in disciplines which employed mathematics.

A spacetime point is no longer a physical fact, it is an outmoded doctrine—a twentieth-century expression of Newton's ether. This is the first occasion we have to note a logical mistake in Einstein's fundamental ideas. As it happens, we know how he came to make it. As pointed out recently, Einstein was enormously impressed by Poincaré's *Science and Hypothesis* (1902), making a "careful reading" of it.[28] Alarmingly, we have very recently been told that Sraffa "studied intensively" this same book.[29] That's not a good sign; indeed, it makes us wonder if Sraffa's idea of the "abstract" is the same as Einstein's view of the "natural." What they were totally unprepared for was the "natural" mathematical point of view Poincaré was trying so hard to sell them. As Garciadiego points out, Poincaré used the book to set out "numerous inconsistencies arising from set theory....Poincaré was hunting for 'paradoxes' because he was trying to discredit both Cantor's theory of sets and Russell's logicism."[30] But there were no paradoxes.

The young Einstein faced both a well-developed mathematical debate and a polemic. He had no idea of this. Note that at no time did Einstein ever question the status of the set theory, or other paradoxes, or the historical approach developed to deal with them (neither did Kimura or Sraffa). Instead, he felt comfortable expressing the relativity of simultaneity through "natural" mathematics without ever examining it, with disturbing consequences for his theory. In Poincaré he read and accepted the idea that "the mind has a direct intuition of this power ["proof by recurrence" or "mathematical induction"], and experiment can only be for [the mind] an opportunity of using it, and thereby of becoming conscious of it." In geometry "we are brought to [the concept of space] solely by studying the laws by which…[muscular] sensations succeed one another."[31] These ideas were developed in order to deal with paradoxes which did not exist. Thus, they had

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[27] Grattan-Guinness, 129, 356.
[28] Howard and Stachel, *Einstein*, 6.
[29] Kurz and Salvadori, "Representation of the Production," 82.
[30] Garciadiego, 140.
[31] Poincaré, *Science and Hypothesis* (New York, 1952 edition), 13, 58.

no object—they related to absolutely nothing. Poincaré is such an unreliable guide that we have to look very skeptically at the work of anyone who was influenced by him. This idea of "succession" was vital if the "standstill" to which the "paradoxes" had brought mathematics, was to be overcome. As we shall see, this logically empty notion was applied with damaging results.

We now understand, however, why we never find "natural" coincidence among Einstein's postulates or definitions or among his conclusions: those are not its job. Its job is to float free of all context—depending on shared prejudices or simple uninquisitive ignorance in order to stay afloat—serving as a facilitator of arguments which cannot be carried out logically. Thus, we see exactly *why* the term occurs *where* it does in the relativity of simultaneity: it "allows" one point to "succeed" another, in conformity with the demands of "natural" mathematics. For the first time, we see Einstein—not as our contemporary—but rather, as a figure out of the past. He is hobbled by that by which we distinguish all figures out of the past: by the infirmity of his intellectual appartus. Where is "natural" coincidence in Gödel? in Sraffa? in Kumura?

Einstein also used "natural" mathematics in his earlier comments on Brownian motion, with disturbing effect: "Einstein begins with an assumption whose status is still problematic and troubled his contemporaries: that there exists 'a time interval τ, which shall be very small compared with observable time intervals but still so large that all motions performed by a particle during two consecutive time intervals τ may be considered as mutually independent events…." As the author of this passage notes, "[t]his is essentially a very strong Markov postulate. Einstein makes no attempt to justify it….[W]here mathematics ends and physics begins is far from clear…."[32]

We also see emerging in Einstein's thought a tenet of "natural" mathematics not usually associated with him: he believed that reality is progressive. That, of course, is not an acceptable stand today in any scientific argument; it was dismissed during the twentieth century, which is the Age of the Term of Art. It required perspective on "natural" mathematics in order to realize that it nevertheless is part of Einstein's thought.

One of the tenets of "natural" mathematics is that it either is one of the natural sciences, or is intimately related to them. Cantor expressed his devotion to "natural" mathematics in his belief that chemistry and mathematics are the same thing. Of all his formulae, the most important is: chemical valence=cardinal number. Einstein himself seems to regard the shift to Darwinian biology—effected, in his view, by a substitution of assumptions—as paradigmatic of the shift, again in his view, from Newtonian to relativistic mechanics: regarding the paradigm, "[a]s an example, a case of general interest is available in the province of biology, in the Darwinian theory of the development of species by selection in the struggle for existence, and in the [earlier] theory of development which is based on the hypothesis of the hereditary transmission of acquired characteristics."[33] Is relativity a biological theory? Sraffa's is another example of this tendency to regard theory as some sort of shift or shuffling about, as a substitution or a "translation." These are deceptively *ad hoc*, informal pronouncements. They not only make us wonder whether these thinkers had any basis at all for relating one term to another, but also, they make us wonder about the role of such pronouncements in any relationships they claim to have found? For example, it is fair to ask if Sraffa believe that reality is progressive? Is his system quantum electrodynamics? Both Sraffa and Einstein are here being the dutiful students of Poincaré: their notions of substitution or translation are his "succession." It is important to note that, for Einstein, "natural" coincidence is the

[32] Sahotra Sarkar, "Physical Approximations and Stochastic Processes in Einstein's 1905 Paper on Brownian Motion," in Howard and Stachel, 211, 220-221. Alexander Markov's important role in "natural" mathematics is discussed in Haskell Curry,, *Foundations of Mathematical Logic*, New York 1977.
[33] Einstein, 142.

shift "from Newtonian to relativistic mechanics"—we are supposed to be able to see it with our own eyes. What is the term in Sraffa which is the "translation?"

The basis of supposed need for substitutions, "translations" or shifts of any kind in twentieth-century thought, is "paradoxes" which are not paradoxes. These shifts turn out to be unprompted strategies. Perhaps if Sraffa and Einstein had made some inquiry into Poincaré's project, they would have developed a meaningful strategy. But I think they were simply too ambitious to make the necessary inquiries. As it is, their arguments are identical—the identification wished for in the early twentieth century—but pointing this out is a rather melancholy achievement.

Interestingly, and not surprisingly given his general inquisitiveness, Feynman visited "natural" coincidence when, apparently apropos of renormalization, he remarked: "perhaps the idea that two points can be infinitely close together is wrong—the assumption that we can use geometry down to the last notch is false. If we make the minimum possible distance…the smallest distance involved in any experiment today…, the infinites disappear, all right—but other inconsistencies arise…."[34] But who does he think is trying to do that? No one I know of. In any event, today we would have to say that that's rather good, and very nearly hits the mark. But not quite.

In the event there is nothing to be done about the contradiction in which, due to this "natural" coincidence of points, we are led from an assumption of two Cartesian coordinate systems to a conclusion of one such system. What "paradox" is the relativity of simultaneity designed to avoid, and what is the "paradox" it tries to express? It may well turn out that natural selection=natural coincidence, thus unifying biology and physics on the basis of an error. These new questions and formulation indicate a change in the direction of science.

Einstein's whole proceeding—avoiding a definition of "natural" coincidence by way of providing a method for obtaining the "natural" coincidence, thereby conjuring up "natural points" of coincidence—looks suspiciously like Cantor's avoidance of a definition for a cardinal number while giving a procedure by which a cardinal number can be obtained. Then Einstein's use of "zusammen" resembles the "zusammenfassung" which is Cantor's notion of a set; Einstein even uses "m," which is what Cantor calls the elements of his set.[35]

Is relativity simply a gloss on set theory, a rejiggering of the terms which nonetheless runs afoul of other rules? Is Sraffa doing the same thing by turning the labor theory of value into the value theory of labor, and renormalizing again and again?[36] There is comfort in the idea that there are tangible boundaries which will always force logic through our arguments. However, if that turns out not to be the issue (even though Cantor appears to have believed it), are we then entitled to pose to Einstein and Sraffa the fundamental question which is always posed to set theory: in what sense are the elements considered together in the first place?

Cantor engaged in what was felt to be unrestrained renormalization. The "natural" mathematicians seem to feel that if that unrestrained renormalization is what generated paradox—above and beyond the renormalization and consequent "paradox" to which the disciplines have always been addicted—then any renormalization is sanctioned in order to restrain set theory. There must be limits, so feel free to intervene where you please as you please. That this misconceives the entire project, is beside the point—or

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[34] Feynman, 129.
[35] Garciadiego, 3; Grattan-Guinness, 112.
[36] Kurz and Salvadori, "Representation of the Production," 80; Giancarlo de Vivo, "Sraffa's Path to Production of Commodities By Means of Commodities," 22 *Contributions to Political Economy* (2003), 1-25.

rather, is unknown—to "natural" mathematicians. Their response has always been: if Einstein can do it, they can. At the same time, it may be instructive to consider the attitude of Feynman to renormalization. He called it a "shell game,"[37] and he should know—he played it all his life, won a Nobel Prize for playing it and, in the opinion of his peers, should have won it again for playing it again. It had a devastating effect on physics, and Poincaré made sure to enumerate a series of laws for enforcing it, laws which he says are summed up as a "group."[38] Would Sraffa, Gödel or Kimura ever have been brave enough to regard their endeavors as a shell game? Apparently as long as there was nothing wrong with Einstein, there could be nothing wrong with renormalization, especially in such a good cause! Well, as long as….But no longer than.

Like Kimura and Sraffa, Einstein suffered from an Achilles' heel: mathematics. Like them, he needed—or felt he needed—a mathematical expression for his ideas, and was seduced by the same intuitionist-style mathematics. What did these researchers know of the mathematical politics which lay at the foundation of their ideas? Absolutely nothing. And for all their curiosity, they made no effort to find out. Instead, they were entranced by the apparent speed and finality of the results. This certainly hampered the later collaboration between Gödel and Einstein: they couldn't discuss the one thing they had in common—"natural" mathematics. Actually, all these thinkers had in common something even more important to them than "natural" mathematics: ambition. But when ambition outstrips inquiry, there are bound to be problems with the results.

The idea of a "natural" mathematics as a part of perception, reflects doubt that geometry or other forms of math express propositions, and a belief that perception and expression are one. We find it not only in economics, biology, mathematics and physics—and more evidence of it in chemistry than simply Cantor's yearning. The foundation of contemporary chemical theory is the "natural" mathematics of Condillac which found its way to chemistry, through Lavoisier, where it expresses Condillac's idea of "'analysis' as originating in simple sensory experiences, followed by the process of 'synthesis' in which the ideas were reconstructed in such a way that the relations between them were clearly revealed."[39] What "paradox" is Condillac attempting to avoid as well as to express, by this formulation?

Whatever the independent validity of the notion of "natural" mathematics, it is not logically incorporated in any of the arguments it seeks to express. It may well be that Richard's set E—that infinitely definable set which betrayed his apparently discrete "paradox"—comes alive in the theories of the thinkers whose works we have mentioned, and becomes the unifying feature of these works. This understanding would mark the clearest break with twentieth-century civilization. At the very least, the exposure of "natural" mathematics has begun a revolution in chemistry, physics, economics and biology. As for mathematics, the Pythagorean theorem is itself probably an attempt to avoid as well as to express a "paradox." Which one? Why is it relevant in the present discussion, to inquire as to the status of the Pythagorean theorem?

Conclusion

Einstein said that he hoped his work would provide a few hours' diversion. Perhaps we should have taken him at his word. Perhaps individuals we marginalized—and ideas we thought had been synthesized out of the argument—are now waiting to contribute something relevant. If "noise" matters, perhaps we should bring Bartók, Schoenberg and Webern into the discussion (but how?). Perhaps we can finally bring

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[37] Feynman, 128.
[38] David Pickering, *Constructing Quarks: A Sociological History of Particle Theory*, Chicago 1984. Poincaré, 64.
[39] Grattan-Guinness, 15.

into alignment two concepts which rattle around in the twentieth century like two peas: chance and infinity. Einstein famously said that God does not play dice with the universe. What does he mean by chance (assuming he thinks dice is an example of chance) and God?

In short, we need a much more dynamic approach to what we consider the principal monuments of the twentieth century. Every educated person, during the nineteenth century, was presumed to read widely and be up to date in the research of all areas of inquiry, including art. With the advent of specialization—that is, with the development of terms of art within the disciplines—intellectual life lost that character because, to the extent there was internal consistency within any two given disciplines, it became increasingly difficult to build logical bridges between concepts in the two disciplines. There aren't twenty people in the world who have read both *Production of Commodities* and *The Neutral Theory*. Have you? And yet no highly educated person in the latter eighteenth century could have claimed to be so without having read both Newton and Smith. During the twentieth century, we "couldn't" or "shouldn't" read both *Production* and *The Neutral Theory.* Who had time? And anyway, it would have been like professing two religions. Perhaps this essay will make possible an ecumenical approach.

Today, advances in understanding the rhetoric of the twentieth century have led us to be much more cautious about the caution of twentieth-century thinkers, and hopefully much more direct and demanding than our own twentieth-century selves. Those selves are no longer with us, we left them at the door of this century. We understand more of the prejudices which went into the thinking of people in the twentieth century, and that is part and parcel of the endless process of building up and tearing down ideas. We also freely grant influence within certain groups such as the Vienna Circle or through such well-connected figures as Frank Ramsey, whose ideas found expression in works as apparently diverse as those of Gödel, Wittgenstein and Sraffa. And then there is the ubiquitous Poincaré. We will go much further in this direction, and much faster, if we try to understand how—regardless of the barriers which specialists felt surrounded their disciplines—they nevertheless communicated in internally consistent ways across those barriers: and built bridges over them!