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On Alternative Geometries, Arithmetics, and Logics; a Tribute to Łukasiewicz

Abstract. The paper discusses the similarity between geometry, arithmetic, and logic, specifically with respect to the question of whether applied theories of each may be revised. It argues that they can - even when the revised logic is a paraconsistent one, or the revised arithmetic is an inconsistent one. Indeed, in the case of logic, it argues that logic is not only revisable, but, during its history, it has been revised. The paper also discusses Quine's well known argument against the possibility of "logical deviancy".

Keywords: Łukasiewicz, revisability, inconsistent arithmetics, Traditional logic, paraconsistency, Quine.

1. Introduction: the Place of Łukasiewicz in the History of Logic[1]

The last hundred years have produced a number of great philosophical logicians; perhaps more than any other period of the same duration.[2] Russell, Carnap, Quine, Prior, Kripke, are just some of the names that come immediately to mind. The Polish logician Jan Łukasiewicz is also one of this number. And when the history of the period is written, he will surely have a distinctive place in it.

Łukasiewicz contributed to philosophy and logic in many ways, with work on the propositional calculus, with an analysis of syllogistic, and, very importantly, with his scholarship on the history of ancient and medieval logic.

[1] This paper is based on an invited address given to the conference *Łukasiewicz in Dublin*, held at University College Dublin, July 1996, to celebrate the work of Jan Łukasiewicz. For some years it was slated to appear in the proceedings of that conference. It would seem that the production of those proceedings has lapsed, so I thought that it should now find another outlet. I considered rewriting it in a form that did not bespeak its origin, but decided, in the end, to leave it in its original form, as my own tribute to Łukasiewicz. Just before the conference was held I learned of the sad and sudden dealth of another great logician, and my close friend, Richard Sylvan. This written version, as was the spoken version, is dedicated to his memory.

[2] In absolute terms anyway; relative to the size of the human population, 1250-1350 was more prolific.

Presented by **Ryszard Wójcicki**; *Accepted* September 16, 2002

Studia Logica **74**: 441–468, 2003.

However, it is his role in the foundations of non-classical logic for which he will, I think, be primarily remembered.[3]

I think it no exaggeration to say that Łukasiewicz is the foundational figure in modern non-classical logic. Volume I of *Principia Mathematica* was hardly off the press when Łukasiewicz was busy investigating many different ideas. The most fundamental of these was many-valued logic. The idea that there might be systems of modern rigour where sentences may be neither true nor false seems to be entirely his.[4] One of Łukasiewicz' major motivations for this enterprise was a concern with modality. Modern modal logic owes more to C. I. Lewis than to Łukasiewicz. None the less, Łukasiewicz' concerns with modality (as opposed to the conditional) would seem to predate Lewis'. Łukasiewicz also generalised his finitely many-valued logics to infinitely many values, producing what would later become known as fuzzy logic, beloved of a number of modern AI practitioners. Łukasiewicz did not, himself, found paraconsistent logic, but the possibility of this in clear in his critique of Aristotle on contradiction, and it was one of his students, Jaśkowski, who first articulated a paraconsistent logic in detail.

2. Logics, Geometries and Arithmetics

In producing these alternative logics, Łukasiewicz had the model of non-Euclidean geometries very much in mind. For example, in an essay on many-valued logic:[5]

> It would perhaps not be right to call the many-valued systems of propositional logic established by me 'non-Aristotelian' logic, as Aristotle himself was the first to have thought that the law of bivalence could not be true for certain propositions. Our new-found logic might rather be termed 'non-Chrysippean', since Chrysippus appears to have been the first logician to consciously set up and stubbornly defend the theorem that every proposition is either true or false. The Chrysippean theorem has to the present day formed the most basic foundation of our entire logic.
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> It is not easy to foresee what influence the discovery of non-Chrysippean systems of logic will exercise on philosophical speculation. However, it seems to me that the philosophical signif-

[3] For an account of Łukasiewicz' work, see Sobociński (1956), Borkowski and Słupecki (1958), Kotarbiński (1958), and Kotarbiński's introduction to McCall (1967).

[4] Some of Brouwer's work on intuitionism predates Łukasiewicz' work, but intuitionist logic itself was created by Heyting, somewhat later.

[5] Łukasiewicz (1930), quotation taken from p. 63 of the English translation.

> icance of the systems of logic treated here might be at least as great as the significance of non-Euclidean geometry.

The analogy between non-standard logics and non-Euclidean geometries is an important and interesting one, though several people have denied it. For example, the Kneales (1962) say, p. 575:

> Even from a purely formal point of view the ordinary two-valued system has a unique status among deductive systems which can plausibly be called logic, since it contains all the others as fragments of itself. In short, they are not alternatives to classical logic in the sense in which Lobachevski's geometry is an alternative to Euclid's.

Now this is just factually wrong. There are, for example, connexivist systems of logic that contain logical truths that are not classical logical truths, such as $(\alpha \rightarrow \beta) \rightarrow \neg(\alpha \rightarrow \neg\beta)$.[6] But even if it were not, the conclusion would hardly follow. From the fact that a logic is maximal in some sense, it does not follow that another cannot be an alternative. The maximal logic might entail *too much.* Some have insisted that classical logic has some kind of priority over other systems, since metatheoretic reasoning must be classical.[7] Even if this were to entail some disanalogy, the claim is, again, just false. Intuitionist metatheory for intuitionist logic makes perfectly good sense.[8] Even Rescher (1969), p. 219, who has a good deal more sympathy for the claim than many others, denies the analogy, on the ground that the articulation of a logical system requires the employment of a metalogic (normally an informal one, and not necessarily classical), whereas the articulation of a geometric system does not require the employment of a metageometry. Rescher's observation seems correct. But again, it is difficult to see it as having significant import for the question. The formulation of a grammar for a language requires the employment of a metalanguage, and so a metagrammar. But this hardly entails that there cannot be rival grammars for a language, or that the question of which is correct is not *a posteriori.* The same could be true of logic.

The purpose of this paper is to examine the analogy between logic and geometry carefully. I will argue the analogy is, in fact, a very close one. To facilitate the comparison of geometry and logic, we will also look at an important half-way house, arithmetic. Logic, arithmetic and geometry are the three great *a priori* sciences of Kant's *Critique of Pure Reason.* According

[6] See, e.g., Anderson and Belnap (1975), 29.8.

[7] E.g., Linke. See Rescher (1969), p. 229.

[8] It is illustrated in Dummett (1977). See esp. p. 214.

to Kant, the mind has certain cognitive structures which, when imposed on our "raw sensations" produce our experiences. The first two, space and time, are dealt with in the Transcendental Aesthetic. The third, the categories, is dealt with in the Transcendental Analytic. In the case of all three, a certain body of truths holds good in virtue of these *a priori* structures; and these constitute the three corresponding sciences; geometry in the case of space, arithmetic in the case of time, and logic in the case of the categories. As the difference in location in the *Critique* indicates, the sciences are not entirely on a par: geometry and arithmetic are synthetic; logic is analytic. None the less, each, as a science, is certain and, essentially, complete. This gives us Euclidean geometry, (standard) arithmetic, and Aristotelian logic.

3. Non-Euclidean Geometry

There are few now who would agree with the Kantian picture of these three sciences—at least in its entirety. It has disintegrated, not just under the pressure of philosophical criticism, but under the pressure of developments in science itself. This is clearest in the case of geometry.

Until the Nineteenth Century, 'geometry' just meant Euclidean geometry; but in the first part of that century some different geometries were developed. Initially, these were obtained by Lobachevski and Bolyi simply by negating one of the postulates of Euclidean geometry in order to try to find a *reductio* proof of it. But under Riemann, the subject developed into one of great generality and sophistication. In particular, he developed a highly elegant theory concerning the curvature of spaces in various geometries. Whether non-Euclidean geometries were to be called geometries *in stricto sensu* might have been a moot point; after all, they did not describe the structure of physical space. But they were at least theories about objects called 'points', 'lines', etc., whose behaviour bore important analogies to that of the corresponding objects in Euclidean geometries. Moreover, Riemann realised that it might well be an empirical question as to which geometry should be applied in physics.[9]

Within another 50 years, and even more shocking to Kantian sensibilities, Riemann had been vindicated. The General Theory of Relativity postulated a connection between mass and the curvature of space (or space-time) which implied that space may have non-zero curvature, and so be non-Euclidean. Predictions of this theory were borne out by subsequent experimentation, and the Theory is now generally accepted.

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[9] On the history of non-Euclidean geometry, see Gray (1994); and on Riemann in particular, see Bell (1937), vol. 2, ch. 26.

These developments have forced us to draw a crucial distinction. We must distinguish between geometry as a pure mathematical theory and geometry as an applied theory. As pure mathematical theories, there are many geometries. Each is perfectly well-defined by some axiomatic or model-theoretic structure. What holds in it may be *a priori.* By contrast, which to apply to the cosmos as a physical geometry is neither *a priori* nor incorrigible. Each pure geometry, when applied, provides, in effect, a theory about spatial (or spatio-temporal) relations; and which is correct is to be determined by the usual fallible scientific criteria.

This by no means resolves all pertinent philosophical issues. In particular, how to understand the status of applied geometry is still contentious. The simplest interpretation is a realist one.[10] Physical geometry is a theory about how certain things in physical space, i.e., points, lines, etc., behave; and a non-Euclidean geometry gets it right. The alternative to realism is non-realism, of which there are many kinds. One is reductionism: talk of geometric points and lines is to be translated without loss into talk of relationships between physical objects. This is the view traditionally associated with Leibniz, but has found few modern adherents. Another kind of non-realism is instrumentalism.[11] Geometry has no descriptive content, literal or reductive. It is merely auxiliary machinery for the rest of physics. As such, we may choose whatever such machinery makes life easiest elsewhere; and a non-Euclidean geometry does just that. One notable version of instrumentalism is that according to which, once we have chosen a geometry, its claims become true by convention. Such conventionalism is often associated with the name of Poincaré.[12]

4. Non-Standard Arithmetics

Is the situation concerning different logics the same as that concerning different geometries, at least in principle? *Prima facie*, it might well be thought not. A crucial difference would appear to be that logic, unlike geometry, is analytic, not synthetic. Thus, logic is not about the world in any interesting sense; consequently, the question of changing logic to provide a better description of the world, as geometry was changed, does not arise.

Before we consider this issue, it will be illuminating to consider the case of arithmetic, which is a half-way house. Kant, it is true, considered both geometry and arithmetic to be synthetic. The received view in the Twentieth

[10] This is endorsed by Nerlich (1976).

[11] This is endorsed in Hinckfuss (1996).

[12] See, e.g., Poincaré (1952).

Century has been more influenced by Frege, however.[13] Geometry may be synthetic, but arithmetic is analytic.

Let us start by asking if there could be alternative pure arithmetics in the sense that there are alternative pure geometries. Normal arithmetic is the set of sentences of the usual first-order language that are true in the standard model, the natural numbers, $0, 1, 2, \ldots$, as subject to the usual arithmetic operations. We may take an alternative arithmetic simply to be one that is inconsistent with this. In other words, we form an alternative arithmetic by throwing in something false in the standard model. Naturally, if consistency is to be preserved, other things must be thrown out. There are two possibilities here.

The first is that we retain all the axioms of Peano Arithmetic, but add the negation of something independent of Peano Arithmetic but true in the standard model. We then have a theory that has a classical non-standard model.[14] As a rival arithmetic, such theories are a little disappointing, however. For as is well known, any model of such a theory must have an initial section that is isomorphic to the standard model. In a sense, then, such theories are not rivals to standard arithmetic, but extensions.

The second, and more radical, way of obtaining a nonstandard arithmetic is to add something inconsistent with the Peano axioms, and jettison some of these. (This is the analogue of how non-Euclidean geometries were initially produced.) In principle, this could produce many different systems, but I know of only one to be found in the literature. This jettisons the axiom which says that numbers always have a successor, and adds its negation, producing a finite arithmetic.[15] Although there are such systems, then, there is no well worked-out theory of their general structure.

5. Models of the Inconsistent

A more radical way still of producing a non-standard arithmetic, for which there is now a general theory, is to drop the consistency requirement. We may

[13] For example, Wright (1983) shows that standard arithmetic may be derived in second order logic augmented by "Hume's Principle": if X and Y are in one-to-one correspondence then the number of Xs is the same as the number of Ys. This certainly looks as though it could be analytic.

[14] In this model there may even be solutions to diophantine equations that have no solution in the standard model—by the solution to Hilbert's tenth problem—though these solutions will have no name in the standard language of arithmetic.

[15] See Van Bendegem (1987). Goodstein (1965) gives an arithmetic where a number can have more than one successor, though it would be more accurate to describe this as an arithmetic in which there is more than one successor function.

then add the negation of something true in the standard model and jettison nothing. This situation is novel enough to warrant an extended introduction.

A driving force behind the development of mathematics can be seen as the extension of the number system in such a way as to provide solutions to equations that had no solution. Thus, for example, the equation $x + 3 = 2$ has no solution in the natural numbers. Negative numbers began to be used for this purpose around the Fifteenth Century. Or consider the equation $x^2 = -1$, which has no solution in the domain of real numbers. This occasioned the introduction of complex numbers a little later. In each case, the old number system was embedded in a new number system in which hitherto insoluble equations found roots.

Now consider Boolean equations. A Boolean expression is a term constructed from some of an infinite number of variables, p, q, r... by means of the functors $\wedge$, $\vee$ and $-$ (complementation). A Boolean equation is simply an equation between two Boolean expressions. The simplest interpretation for this language is the two-element Boolean algebra, whose Hasse diagram is:

$$\begin{array}{c} \top \\ \updownarrow \\ \bot \end{array}$$

($\wedge$ is interpreted as meet, $\vee$ as join and $-$ as order-inversion). Within this interpretation many Boolean equations have solutions. E.g., the equation $p \vee -p = q$ is solved by $q = \top$ and $p = \top$ (or $\bot$). But many equations have no solutions, e.g., $p = -p$. It is natural, then, to extend the algebra to one in which all equations have solutions. The simplest such one is that whose Hasse diagram is as follows:

$$\begin{array}{c} \top \\ \updownarrow \\ \mu \\ \updownarrow \\ \bot \end{array}$$

(where operations are interpreted in the same way; in particular, μ is a fixed-point for $-$). In this structure the equation $p = -p$ is solved by $p = \mu$. More generally, it is not difficult to check that if every variable is assigned μ, any Boolean expression evaluates to μ. Hence every Boolean equation has a solution.[16]

[16] Technically, what is going on here is that a Boolean algebra is being embedded in a De Morgan lattice with a fixed point for $-$. All Boolean equations will have solutions in such an algebra. It can be shown that all Boolean algebras can be "completed" in this way.

Algebras have many applications. The most obvious in a philosophical context, is to provide a structure of truth-values for a propositional language. In this context, the fact that every Boolean expression can be evaluated to μ means that, provided we take the designated values of our logic to be those $\geq \mu$, every sentence is satisfiable; as, more generally, is every set of sentences. (The interpretation where everything takes the value μ is, of course, a rather uninteresting one. But there will be many others. The point is simply to demonstrate the existence of models.)

In terms of logics, the above algebraic structure is exactly the logic *LP*,[17] and the value μ is normally thought of as *both true and false.* The truth value μ might seem a rather odd one. One might be tempted to call it an imaginary truth value, for the same reason that $\sqrt{-1}$ was called imaginary. But there is nothing really imaginary about imaginary numbers. Mathematically speaking they are just as *bona fide* as real numbers. Indeed, they even have applications in physics. Leave quantum mechanics out of this; even in classical physics magnitudes such as impedance are given by complex numbers. Similarly, mathematically speaking, there is nothing imaginary about μ. And like imaginary numbers, μ may even have important applications. For example, it has been argued that one application for μ is to take it to be the truth value of paradoxical self-referential sentences, α (for which we often appear to have $\alpha \leftrightarrow \neg\alpha$).

Anyway, as we have now seen, every set of sentences, even inconsistent ones, has a model in the logic *LP* (at least the trivial one, where everything takes the value μ, but in general many non-trivial models too).[18] In particular, and to return to the main issue, so do inconsistent arithmetics.

6. Inconsistent Arithmetics

The fact that there are *logical* models of inconsistent arithmetics does not tell us anything about their *arithmetical* structure. In fact, they have an interesting common structure. What is this? I will restrict myself here to describing the situation for the simplest finite models. The general case (even the general finite case) is more complex.[19]

In every such model there is a tail, T, of numbers, $0, \ldots, n-1$, that behave consistently. The numbers from n onwards form a cycle with some

[17] See, e.g., Priest (1987), ch. 5.

[18] Strictly speaking, we have seen this only for sentences of a propositional language. The result extends in a natural way to a full first order langauge, however. The details of this can safely be left as an exercise.

[19] A complete characterisation of the finite case is given in Priest (1997). Details of the general case can be found in Priest (2000).

period, p (> 0), so that for any $i \geq n$, $i + p = i$. (But since this is a model of arithmetic $i + p \neq p$ also.) These models may therefore be characterised by two parameters: the tail, T, and the period, p. The trivial model is the special case in which $T = \phi$ and $p = 1$.

Every structure of the kind described is a non-standard arithmetic, and the numbers in it are analogous to the points and lines in a non-standard geometry. Since each of the non-standard arithmetics extends ordinary arithmetic, every equation that has a solution in standard number-theory has a solution in it. But, as might be expected, there are equations that have no standard solution but which have solutions in non-standard arithmetics. Let a numerical expression be any term formed from variables, x, y,..., and the constant $\mathbf{0}$, using the functors $+$, $\times$ and $'$ (successor).[20] (I will use $\mathbf{m}$ for the numeral of m.) An equation is any identity of the form $s = t$, where s and t are arithmetic expressions. It is not difficult to establish that in the trivial model, every arithmetic equation has a solution (viz., $x = y = ... = 0$.) But many families of equations have general solutions in non-trivial models. For example, any equation of the form $\mathbf{m} = \mathbf{n}$ has a solution in a model where $m, n \notin T$ and $m = n \pmod{p}$. And any equation of the form $s(x) = t(x)$, where x occurs in s and t, has a solution in every model where $p = 1$ (namely the least inconsistent number).[21]

7. Empirical Applications

We have seen that there are alternative pure arithmetics, in the same sense that there are alternative pure geometries: abstract mathematical structures dealing with objects (numbers or points) that behave in a way recognisably similar to the corresponding objects of the standard theories. But, it might be suggested, this is as far as the similarity goes. There is no possibility of applying a non-standard arithmetic to an empirical situation in the same way. There could be no such thing as applied non-standard arithmetic in the same way that there is applied non-Euclidean geometry. Is this so? I take the answer to be 'no'. There are theoretical arguments for this; we will come to these when I turn to logic. For the present I will demonstrate the point by telling a story to show how we might come to replace a standard application of ordinary arithmetic with a different one. There are a few such stories already in the literature, notably, those of Gasking (1940). But I will give

[20] Other functional expressions, e.g, exponentiation, could be added to the list. However, others, such as subtraction, cause problems.

[21] Likewise, families of equations that have no simultaneous solution in standard arithmetic can have one in inconsistent arithmetics. The case of families of linear equations, with an application to control theory, is discussed in Mortensen (1995), ch. 8.

one which might motivate the adoption of one of the inconsistent arithmetics that we met in the last section.[22] I do not want to claim that the situation described is a possibility in any real sense: it is merely a thought experiment. But if it succeeds it will show the required conceptual possibility.[23]

Let us suppose that we come to predict a collision between an enormous star and a huge planet. Using a standard technique, we compute their masses as x_1 and y_1, respectively. Since masses of this kind are, to within experimental error, the sum of the masses of the baryons (protons and neutrons) in them, it will be convenient to take a unit of measurement according to which a baryon has mass 1. In effect, therefore, these figures measure the numbers of baryons in the masses. After the collision, we measure the mass of the resulting (fused) body, and obtain the figure z, where z is much less than $x_1 + y_1$. Naturally, our results are subject to experimental error. But the difference is so large that it cannot possibly be explained by this. We check our instruments, suspecting a fault, but cannot find one; we check our computations for an error, but cannot find one. We have a puzzle. Some days later, we have the chance to record another collision. We record the masses before the collision. This time they are x_2 and y_2. Again, after the collision, the mass appears to be z (the same as before), less than $x_2 + y_2$. The first result was no aberration. We have an anomaly.

We investigate various ways of solving the anomaly. We might revise the theories on which our measuring devices depend, but there is no obvious way of doing this. We could say that some baryons disappeared in the collision; alternatively, we could suppose that under certain conditions the mass of a baryon decreases. But either of these options seems to amount to a rejection of the law of conservation of mass(-energy), which would seem to be a rather unattractive course of action.

Then someone, call them Einquine, fixes on the fact that the resultant masses of the two collisions were the same in both cases, z. This is odd. If mass has gone missing, why should this produce the same result in both cases? An idea occurs to Einquine. Maybe our arithmetic for counting

[22] There are certainly others. For example, even if arithmetic is not the form of the intuition of time, as Kant thought, one might tell a story where circumstances suggested the possibility of calibrating time with a non-standard arithmetic. In virtue of the cycles in the finite inconsistent arithmetics, stories of time-travel would appear to be particularly fruitful here.

[23] It is worth noting that Gasking is criticised by Castañeda (1959), who argues that if we use a different arithmetic and preserve our standard practices of counting, we end up with either a simple change of terminology or inconsistency. I will not discuss Castañeda's argument here. Even if he is right, this is clearly no objection in the present context.

baryons is wrong.[24] Maybe the appropriate arithmetic is one where z is the least inconsistent number, and p (the period of the cycle) $= 1$. For in such an arithmetic $x_1 + y_1 = x_2 + y_2 = z$, and our observations are explained without having to assume that the mass of baryons has changed, or that any are lost in the collisions! Einquine hypothesizes that z is a fundamental constant of the universe, just like the speed of light, or Planck's constant.[25]

While she is thus hypothesising, reports of the collisions start to come in from other parts of the galaxy. (The human race had colonised other planets some centuries before.) These reports all give the masses of the two new objects as the same, but all are different from each other. Some even measure them as greater that the sum of their parts. Einquine is about to give up her hypothesis, when she realises that this is quite compatible with it. Even if the observer measures the mass as z', provided only that $z' > z$ then $z = z'$, and their results are the same!

But this does leave a problem. Why do observers consistently record result that differ from each other? Analysing the data, Einquine sees that values of z (hers included), are related to the distance of the observer from the collision, d, by the (classical) equation $z = z_0 + kd$ (where z_0 and k are constants). In virtue of this, she revises her estimate of the fundamental constant to z_0, and hypothesizes that the effect of an inconsistent mass of baryons on a measuring device is a function of its distance from the mass. Further observational reports bear this hypothesis out; and Einquine starts to consider the mechanism involved in the distance-effect.

We could continue the story indefinitely, but it has gone far enough. For familiar reasons, there are likely to be theories other than Einquine's that could be offered for the data. Some of them might preserve orthodox arithmetic by jettisoning conservation laws, or by keeping these but varying some physical auxiliary hypotheses. Others might modify arithmetic in some other, but consistent, way.[26] And each of these theories might become more

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[24] We already know that different sorts of fundamental particles satisfy different sorts of statistics.

[25] The revision of arithmetic envisaged here is a local one, in that it is only the counting of *baryons* that is changed. It would be interesting to speculate on what might happen which could motivate a global change, i.e., a move to a situation where everything is counted in the new way.

[26] An obvious suggestion here is that we might use, instead, a finite consistent arithmetic, with maximum number z_0, which is its own successor. For all we have seen of the example so far, this would do just as well. However, there might well be reasons that lead us to prefer the inconsistent arithmetic. Notably, this arithmetic gives us the full resources of standard arithmetic, whilst the finite arithmetic does not. For example, in the inconsistent arithmetic it is true that for any prime number there is a greater prime, which is false in

or less plausible in the light of further experimentation, etc. But the point is made: it is quite possible that we might vary our arithmetic for empirical reasons. There can be alternative applied arithmetics, just as there are alternative applied geometries.[27]

An applied arithmetic is, in effect, a theory concerning certain relations of magnitude. As with geometry, there are various ways as to how one might understand such a theory. One can tell a realist story: collections of physical objects have a certain physical property, namely size; and sizes, together with the operations on them, have the same objective structure as the numbers and corresponding operations in the non-standard arithmetic. Hence, facts about the mathematical structure transfer directly to the physical structure, and this is why it works. Or one could tell instrumentalist stories of various obvious kinds. For example, Gasking (1940), runs a conventionalist line on arithmetic that is similar to Poincaré's on geometry. We need not pursue these matters further here, though.

8. Revising Logic: the Case of Syllogistic

We have seen that there are numerous pure geometries and arithmetics. We have also seen that, when applied, a geometry or arithmetic is a corrigible theory of some domain. Let us now turn to the question of whether the same is true of logic. I will argue that it is. Indeed, it may fairly be pointed out that I have already assumed this, since the argument concerning arithmetic was partly based on arithmetics whose underlying logic is non-classical.

First, there are numerous pure logics. This point I take to be relatively uncontentious. There are all the non-classical logics that Łukasiewicz himself invented, not to mention others such as intuitionism, quantum logic and paraconsistent logic (one of which, *LP*, we met in the preceding sections). Possibly, a purist might say that they are not logics since they are not the *real* logic. But that would be like saying that non-Euclidean geometries are not geometries since they are not the *real* geometry. In both cases we have a family of structures (logics or geometries) that are perfectly well-defined mathematical structures; and, as far as that goes, all on a par.

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the finite arithmetic. This extra strength might cause us to prefer it. Alternatively, the difference might even occasion different empirical predictions, which verify the inconsistent arithmetic.

[27] Some of the philosophical issues surrounding inconsistent arithmetics, were aired in Priest (1994). There it is argued that our ordinary arithmetic might be an inconsistent one. A critique of this is to be found in Denyer (1995), with replies in Priest (1996). Denyer's criticisms are not relevant here. For present purposes, I conceed that our ordinary arithmetic is the usual one. My concern is with inconsistent arithmetics as revisionary.

The more contentious question is whether in logic, as in geometry and arithmetic, which one we apply is a corrigible matter. Of course, a pure logic can have many applications (as may pure geometries and arithmetics). For example, standard propositional logic may be used to test inferences or simplify the design of electrical circuits; and for some of these applications (e.g., the latter) the question of which logic is correct may well be a theory-laden and corrigible matter. But when talking of application here, what we are talking about is what one might call the canonical application. The canonical application of geometry is in physical geometry; the canonical application of arithmetic is to counting and measuring; the canonical application of logic is to reasoning.

Now, there is a well known and very famous argument to the effect that all applied theories are revisable in principle, and so corrigible. I refer, of course, to that contained in Quine's celebrated paper 'Two Dogmas of Empiricism' (1951). The argument is to the effect that there is no principled way of drawing a line between the revisable and the unrevisable theories; and hence the sciences traditionally thought of as *a priori*, such as geometry and arithmetic are just as revisable as psychology and physics.[28] Quine also mentions logic explicitly in this context, but does not pursue the matter at great length.[29] Different arguments for similar conclusions were put forward by Putnam in (1962). Moreover, Putnam went on to develop the conclusion for logic. In (1969), drawing on the example of non-Euclidean geometry explicitly, Putnam argued that the situation in Quantum Physics might well occasion the replacement of classical logic by quantum logic—at least in reasoning about the micro-world.

We need not work through the details of Putnam's example here. To show that the received logic may change it is not necessary to argue for the *possibility* of this. One can point to a place where it has *actually* happened.[30] When Kant defended the incorrigibility of logic in the *Critique* he defended Aristotelian logic—by which I mean, here, not Aristotle's logic, as found in the *Organon*, but the logic that developed from this (notably with the work of scholastic logicians).[31] Within 150 years, this had been replaced by the entirely inappropriately called 'classical logic' of Frege and Russell.

[28] Interestingly enough, exactly the same argument was used some 10 years earlier at the end of Gasking (1940).

[29] The consequences for logic are drawn out clearly in Haack (1974), ch. 2.

[30] Further on the following, see Priest (1989).

[31] For an excellent summary of this, see Prior (1967).

That Aristotelian and classical logic are distinct will hardly be denied. But it might well be suggested that the adoption of classical logic did not revise Aristotelian logic in any interesting sense: Aristotelian logic was perfectly correct as far as it went; it was just incomplete. Classical logic simply extended it to a more complete theory. Such a suggestion would be false. It is a well known fact, often ignored by philosophers (though not, perhaps, historians of philosophy) that Aristotelian logic is inconsistent with classical logic in just the same way that non-Euclidean geometries are inconsistent with Euclidean geometry. A central part of Aristotelian logic is syllogistic, and the most natural translation of the syllogistic forms into classical logic is as follows:

AaB	All As are Bs	$\forall x(Ax \supset Bx)$
AeB	No As are Bs	$\neg\exists x(Ax \wedge Bx)$
AiB	Some As are Bs	$\exists x(Ax \wedge Bx)$
AoB	Some As are not Bs	$\exists x(Ax \wedge \neg Bx)$

Given this translation, Aristotelian syllogistic gives verdicts concerning the validity of some syllogisms that are inconsistent with classical logic. Consider the inferences called by the medievals *Darapti* and *Camestros*, which are, respectively:

All Bs are Cs
All Bs are As
Hence some As are Cs

All Cs are Bs
No As are Bs
Hence some As are not Cs

Both of these are valid syllogisms. Both are invalid in classical logic.

The problem is, of course, one of existential import. Some syllogisms seem to presuppose that various categories are instantiated.[32] It is sometimes suggested that the problem can be repaired by adding the import to the translations explicitly. Specifically, we add the clause $\exists xAx$ to each of the a and e forms. (It would be redundant in the other two.) This is, indeed, sufficient to render all the syllogistic forms classically valid, but the problem with this is that it invalidates other central parts of Aristotelian logic, notably, the square of opposition. The square is:

[32] For the record, these are the inferences Darapti and Felapton in the third figure, and all the medieval subaltern moods: Barbari, Celaront, Cesaro and Camestros. A discussion of the problem of existential import can be found in Kneale and Kneale (1962), pp. 54-61.

AaB	AeB
AiB	AoB

where the claims on the top line are contraries; on the bottom line are subcontraries; and on both diagonals are contradictories. Now it is clear that, once the a form is augmented with existential import, a and o are not contradictories: both are false if there are no As. For the same reason, neither are e and i.

Another suggested repair is to add existential import to the a form (but not the e), and take the o form to be its negation $(\exists x(Ax \wedge Bx) \vee \neg\exists xAx)$. This validates all the syllogisms and the square of opposition. The oddity of taking 'Some As are not Bs' to be true if there are no As is clear enough. But more importantly, this repair invalidates another part of the traditional logic: the inferences of obversion. Specifically, obversion permits the inference from 'no As are Bs' to 'all As are non-Bs'; which fails if the e form is not existentially loaded. Obversion is not in Aristotle, but it is a perfectly standard part of traditional logic.

It is sometimes suggested that, rather than adding existential import to the translations explicitly, we should take the instantiation of all the categories involved to be a global presupposition. This is a move of desperation. If it is correct, then we cannot use syllogistic to reason, e.g., in mathematics, where we certainly do not make such presuppositions. I don't think that the traditional logicians who endorsed syllogistic believed this. Moreover, if we were to allow validity to have contingent presuppositions, pretty much *anything* could be made to be valid.[33]

More importantly, the suggestion really will not save syllogistic.[34] All winged horses are horses, and all winged horses have wings. Applying Darapti, we may infer that there are some winged horses. The argument clearly generalises. All As are As.[35] *A fortiori*, all ABs are As; and symmetrically, all ABs are Bs. By *Darapti* it follows that there are some ABs. Thus syllogistic allows us to prove that any two categories intersect. And if it be replied that this is just one of the global presuppositions, take B to be $\overline{A}$, the complement of A (non-A). It can hardly be maintained that Aristotelian logic globally presupposes contradictions. This argument requires the use of

[33] It is worth noting that classical logic has its own contingent presuppositions, notably that something exists, and its own Darapti: $\forall xFx \models \exists xFx$.

[34] The following argument is due to Len Goddard. For a further discussion see his (2000).

[35] One might, of course, deny this. But then one has to reject another standard part of traditional logic: the Law of Identity.

compound terms. Again, these are not in Aristotle, but are an established part of traditional logic.[36]

What we have seen is that however one interprets traditional logic in classical logic, *something* has to be given up. Moreover, this is quite essential. For as the last argument shows, traditional logic is, in fact, inconsistent. At any rate, classical logic is not (just) a more generous framework subsuming traditional logic. Prevarication aside, modern logic has given the thumbs-down to *Darapti* and its ilk.

9. Quine and Meaning-Variance

As Quine insisted, then, there can be rival logics for canonical application, just as there can be rival geometries and arithmetics for their canonical applications. Some years after writing 'Two Dogmas', Quine developed his views on radical translation, with some corollaries for logic. And many commentators have viewed these as a partial reneging on his earlier radical views concerning logic.[37] The views are spelled out first in *Word and Object* (1960), section 13; but they are put most pithily in *Philosophy of Logic*, thus:[38]

> To turn to a popular extravaganza, what if someone were to reject the law of non-contradiction and so accept an occasional sentence and its negation as both true? An answer one hears is that this would vitiate all science. Any conjunction of the form '$p. \sim p$' logically implies every sentence whatever; therefore acceptance of one sentence and its negation as true would commit us to accepting every sentence as true, and thus as forfeiting all distinction between true and false.
>
> In answer to this answer, one hears that such full-width trivialisation could perhaps be staved off by making compensatory adjustments to block this indiscriminate deducibility of all sentences from an inconsistency. Perhaps, it is suggested, we can so rig our new logic that it will isolate its contradictions and contain them.
>
> My view of the dialogue is that neither party knows what he is talking about. They think that they are talking about negation,

[36] They are omitted in Łukasiewicz' (1957) formalisation of syllogistic, which allows it to be consistent.

[37] For example, Haack (1974), ch. 1, Dummett (1978), p. 270.

[38] Quine (1970), p. 81.

> '$\sim$', 'not'; but surely the notion ceased to be recognisable as negation when they took to regarding some conjunctions of the form '$p.\sim p$' as true, and stopped regarding such sentences as implying all others. Here, evidently, is the deviant logician's predicament: when he tries to deny the doctrine he only changes the subject.

Quine has paraconsistent logic in his sights here, but the context makes it plain that the point is meant to be a general one; and the point would seem to be that a change of logic occasions a change of meaning of the connectives concerned (though Quine might not put it this way because of his skepticism about meaning).

The claim has been denied by some,[39] and I think that it would be wrong to suppose that the connectives of different logics *must* have different meanings. However, I think that it must be agreed that the connectives of different pure logics often do have different meanings. Consider, for example, negation in intuitionist and classical logics. Given a possible-world interpretation for a propositional language, the truth conditions of classical and intuitionist negation are, respectively:

$\neg\alpha$ is true at world w iff α is not true at world w

$\neg\alpha$ is true at world w iff for all w' such that $w \leq w'$ α is not true at w'

It is clear that the truth conditions are quite distinct. Since identity of meaning would certainly seem to entail identity of truth conditions, it follows that the two negations mean something different. An intuitionist would say that the first truth conditions do not succeed in defining a meaningful connective at all. I do not think that this is right; but even if it were, it would still follow that the two connectives do not mean the same: one is meaningful; the other is not.

It would be wrong, however, to suppose that Quine takes the meaning-variance of connectives across different logics to be an argument against his earlier view concerning the revisability of logic. A few pages after the passage just quoted, he says (p. 83):

> By the reasoning of a couple of pages back, whoever denies the law of excluded middle changes the subject. This is not to say that he is wrong in so doing. In repudiating 'p or $\sim p$' he is indeed giving up classical negation, or perhaps alternation, or both; and he may have his reasons.

And summing up some of the lessons learned in the book he reiterates explicitly the revisability of logic (p. 100):

[39] For example, Putnam in his (1962) and (1969).

> ...I am committed to urge the empirical nature of logic and mathematics no more than the unempirical character of theoretical physics; it is rather their kinship I am urging, and a doctrine of gradualism... A case in point was seen ... in the proposal to change logic to help quantum mechanics. The merits of the proposal may be dubious, but what is relevant now is that such proposals have been made. Logic is in principle no less open to revision than quantum mechanics or the theory of relativity.

Moreover, Quine is absolutely correct to insist that the views concerning meaning-change do not render rivalry and revision impossible. One way to see this is to recall that a number of influential writers in the philosophy of science, such as Kuhn (1962) and Feyerabend (1975), have argued for a version of meaning-variation for scientific theories. According to them, the meanings of terms in scientific theories are defined by the scientific principles in the theory, and thus, e.g., 'mass' in Newtonian mechanics means something different from 'mass' in relativistic mechanics. They concluded from this that the theories are incommensurable, i.e., that no direct comparison of content is possible between them. But they did not infer that the theories are not rivals. They obviously are: they give different and incompatible accounts of, e.g., motion, to the point of making inconsistent predictions.[40]

So, if Quine is not arguing against the possibility of rivalry in logic, what point is he making? Let us return to that in a moment. It will pay us, first, to look more closely at how rivalry in logic manifests itself.

10. Rivalry

As pure logics, no logic is a rival of any other. They are all perfectly good abstract theories. It is only when we apply them that a question of rivalry occurs. To give a theory canonical application, it is crucial that we forge some link between it and the practices that give the application life.[41] In the case of geometry, we need to relate the objects of the theory to our practices of measuring lines and angles; in the case of arithmetic, we need to relate the objects of the theory to our practices of counting. And in the case of logic, we need to relate the theory to our practices of inferring.

[40] In a passage just before the first one quoted, Quine raises the possibility of someone who uses 'and' as we use 'or', and vice versa, and notes, quite correctly, that there is no real rivalry between the respective views, merely a trivial linguistic one. This is because there is a translation manual between their idiolect and ours, which is precisely what is impossible when we have a case of incommensurability.

[41] The link may, of course, be coeval with the theory itself, as it was in the case of Euclidean geometry and Aristotelian logic.

Inferring is a practice carried out in the in the vernacular; maybe the vernacular augmented with a technical vocabulary (such as that of chess, physics or whatever); maybe the vernacular augmented with mathematical apparatus; but the vernacular none the less.[42] Hence, to apply a logic, we must have some way of identifying structures in the formal language with claims in the vernacular. (Whether these are sentences, propositions, thoughts or wot not, we need not go into here.) Notoriously, in the case of logic, the mode of identification is largely tacit. It is a skill that good logicians acquire, but no one has ever spelled out the details in general. It is simple-minded, for example, to suppose that every sentence with a 'not' in is adequately represented in the language of a logical theory by whatever represents the sentence with 'not' deleted, and prefixed with '$\sim$'. For a start, the negation of 'All cows are black' is not 'All cows are not black'. And English uses 'not' for functions other than negation (as in 'I'm not British; I'm Scottish').[43] Similar points can be made concerning '$\wedge$' and 'and', and '$\vee$' and 'or'.[44]

Rivalry in geometry, arithmetic or logic, occurs when different theories, each relative to the way it is applied, deliver different, incompatible, verdicts. In the case of logic, this happens when incompatible verdicts are given concerning the validity of some inference. For example, consider the inference 'If there is not a greatest prime number then there is a greatest prime number. Hence there is a greatest prime number.' Given the usual identifications, classical logic says that it is correct to draw this inference; intuitionist logic that it is not. Or take the inference '107 both is and is not the greatest prime number. Hence Fermat's Last Theorem is true.' Classical logic says that this is a correct inference to draw; paraconsistent logic that it is not.

11. Quine in Defence of Classical Logic

Let us now return to Quine's observations and ask, assuming them to be correct, what their point is supposed to be. The deviant's logician's 'predicament' is that if they change the logic they change the subject; but if this does not rule out rivalry in logic, why, exactly, is it a predicament? I take the answer to be this. The deviant logician may want to change logics and

[42] This is not to say that portions of the reasoning may not be formalised in the language of some logical theory. But this is already to apply the theory, since it presupposes the adequacy of the formalisation.

[43] For more on this see Priest (1999).

[44] See Strawson (1952), pp. 78-93. My own favourite example is due to John Slaney: 'One move and I shoot'.

meanings—and may be justified in doing so—but what they cannot do is say that the principles of (the received) logic are *false*. If someone denies one of the principles, then either their words mean something different, or what they say is (logically) false.

The observation is clearly made in defense of classical logic against its rivals. It is, after all, supposed to be a predicament for the *deviant* logician, not the classical one.[45] But the first thing to note about the above point is that it is entirely symmetrical. The classical logician is in exactly the same situation with respect to the rival logic: when they deny the principles of that, they, too, mean something different, or what they say is (logically) false.

So why does Quine think that the observation tells against non-classical logic? The answer is that he *assumes* that the logical constructions of the vernacular *are* those of classical logic. This is demonstrated by the passage in the original quotation where he lists negation, '$\sim$' and 'not' as the same thing, without further comment. Even granting that negation and 'not' are the same (which they are not, as I have already observed), '$\sim$' is a sign of a formal language with a certain semantics (classical for Quine), whereas negation is a notion from vernacular reasoning.[46]

But Quine's assumption is exactly what a partisan of a logic other than classical is likely to take issue with. Someone who rejects classical logic, say a paraconsistent logician, need not deny that the (classical) meaning of '$\sim$' is sufficient to guarantee the validity of inference $p.\sim p \vdash q$ in classical logic (the pure abstract theory); what they will deny is that this is the meaning of negation, as it occurs in vernacular reasoning, about, say, the claim 'This sentence is not true'. According to them, the semantics of their pure logic is the correct semantics for vernacular negation. Seen in this way, a dispute between rival logics is, then, exactly a dispute over meanings.[47] This may surface, for example, as a dispute over the truth conditions of vernacular negation. It is therefore entirely open to someone who holds that our theory of logic is revisable to hold that the correct logical principles are analytic, that

[45] There is no doubt who the deviant logician is supposed to be. Introducing the topic, Quine says: '[In previous chapters] we did not consider any inroads on the firm area [logic] itself. This is our next topic: the possible abrogation of the orthodox logic of truth functions and quantifiers in favour of some deviant logic.' Quine (1970), p. 80.

[46] Quine's identification of logic with classical logic is also manifest in the following passage. 'It would seem that the idea of a deviation in logic is absurd on the face of it. If sheer logic is not conclusive, what is? What higher tribunal could abrogate the logic of truth functions or quantification?' Quine (1970), p. 81.

[47] Which is the way that Dummett sees it. See (1978), p. 288.

is, true solely in virtue of the meanings of the logical connectives employed;[48] it is just that which principles are analytic is a corrigible and theory-laden issue.

At any rate, as we see, Quine's defence of classical logic completely begs the question, as does any logician who claims that classical logic is right *by definition.*[49] The question is: which definition? And this is to be settled by investigations of the kind standardly engaged in trans-logical debates.

12. Logic and Translation

As I observed earlier, what underlies Quine's views about the meaning-variance of connectives in different logics are certain of his views concerning radical translation. These are brought to bear on the issue explicitly in the section in *Philosophy of Logic* immediately following the one I have just been discussing. Here, Quine appeals to aspects of radical translation to try to establish that one can not attribute to a speaker any view incompatible with classical logic.

The argument is to the effect that when we translate the utterances of another, it is a constraint on correct translation that we cannot take them to be denying things that we take to be obvious (since they must be equally obvious to them). But logic is obvious. Hence we cannot attribute to them beliefs we take to be logically false. As he sums up the argument ((1970), p. 83):

> ...logical truth is guaranteed under translation. The canon 'Save the obvious' bans any manual of translation that would represent the foreigners as contradicting our logic (apart perhaps from corrigible confusions in complex sentences).

In the present context, several comments on this argument are pertinent. First, even if the argument is right, the situation concerning classical and non-classical logicians is entirely symmetrical. If a classical logician ("we") cannot impute the denial of a classical logical truth to an intuitionist or paraconsistent logician (a "foreigner"), then they cannot impute a denial of what they take to be a logical truth to him. The argument cannot, therefore, be used as an argument for classical logic.[50]

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[48] For example, as in Priest (1979).

[49] E.g., Slater (1995).

[50] One might also observe that even if the argument is correct, it is wrong to say, as Quine does, that logical truth is guaranteed under translation. What is guaranteed is *belief* about logical truths. (The methodology of translation requires us to ascribe to others those of our beliefs that we take to be obvious.) The fact that we believe something to be a logical truth does not make it so, in logic as elsewhere.

Secondly, the argument is, in any case, badly deficient. There are two reasons for this. The first is that the claim that logic is obvious is mind-numbingly false. That certain principles hold in various pure logics may well be obvious. That they are so in the vernacular is not at all true—or what is the debate between rival logics all about? From a modern perspective, it may *seem* obvious that Darapti is invalid. It was not obvious to logicians for the previous 2,000 years that this was so—or if it was, they kept very quiet about it. History has taught us that what seems obvious may well not be so. Indeed, it may be false. Quine can assume that logic is obvious only because he has tacitly identified logic with the received theory[51]—an error I have already commented on.

The second failure of the argument is due to the fact that the rule that the obvious should be built into translation is only a defeasible rule, and may be defeated by other aspects of the context. Someone standing in the pouring rain may yet deny that is raining, since they may believe that they are in a virtual-reality machine. Less fancifully, people in the Fifteenth Century took it to be obvious that the earth is stationary (apart from occasional quakes and tremors). When Galileo said otherwise, they did not translate his beliefs away: they showed him the thumb-screws. Similarly, and closer to home, when writers such as Hegel and Engels explicitly endorse contradictions, we do not automatically say that they cannot mean, or believe, what they say. In the light of the rest of what they say, we may well conclude that this is exactly what they believe.[52]

For all these reasons, appeal to radical translation will not help, one iota, in defending classical logic.

13. Realism vs. Instrumentalism in Logic

We are now finished with Quine. Let us return to the analogy between logic, geometry and arithmetic. An applied geometry is, in effect, a theory of spatial relations; an applied arithmetic is a theory of relations of magnitude; and an applied logic is a theory of logical relations. In particular, then, when we change our logical theory, it does not follow that what principles of logic are valid also changes, any more than when we change our theory of geometry, the geometry of the cosmos changes. The fact that we may use the

[51] '...every logical truth is obvious, actually or potentially. Each, that is to say, is either obvious as it stands or can be reached from obvious truths by a sequence of individually obvious steps. To say this is in effect just to repeat some remarks of Chapter 4: that the logic of quantification and identity admits of complete proof procedures...' Quine (1970), p. 82f.

[52] The point is well made by Dancy (1975), p. 34ff.

word 'logic' to apply both to our theory and the concerns of that theory may encourage this confusion.[53] The word 'geometry' is similarly ambiguous; but in a post-Kantian age only the naive would suffer from the corresponding confusion.

What, though, makes a theory of logic correct? Just as with geometry and arithmetic, one may tell both realist and non-realist stories here. It would take far too long to try to decide between these, and I will not attempt this here. Let me just chart some of the positions in conceptual space.

According to a simple instrumentalist story, logic is a tool that we use for inferring; and what makes a logical theory right is that, combined with our other theories, it produces the right results. 'Right' here is to be understood as meaning that the things that we deduce which are subject to relatively direct testing, test positive. Thus, for example, if I deduce the existence of something satisfying a certain condition (an atomic particle, or a number), and investigation fails to find such a thing, then the prediction would appear to be dubious. Of course, as Quine (of 'Two Dogmas') would have pointed out, we can decide to hold on to any predictions our theory makes if we make suitable changes elsewhere—which we may well determine to do. What settles the most appropriate theory, then, is simply that which is over-all simplest, most adequate to the data, least *ad hoc*, and so on.[54]

As in geometry and arithmetic, this simple instrumentalism may be augmented by some kind of conventionalism. Even though the correct logic is a matter of choice, one may still regard claims of logic as true or false. What makes them so is the convention of our choice. This is conventionalism in logic, of the positivist variety.

The realist story is different. What makes a theory the right theory is that it correctly describes an objective, theory-independent, reality. In the case of logic, these are logical relationships, notably the relationship of validity, that hold between propositions (sentences, statements or whatever one takes truth bearers to be). But what are these logical relationships? Several answers are possible here. Perhaps the simplest is one according to which logical truths are analytic, that is, true solely in virtue of the meanings of the connectives, where these meanings are Fregean and objective. Logical relationships are therefore platonic relationships of a certain kind.

A different, and more common story, which avoids reification of mean-

[53] See, further, Priest (1987), ch. 14.

[54] This is the view of logic endorsed by Haack (1974), ch. 2, and also Rescher (1969), ch. 3.

ings, is Tarski's.[55] Interpretations are set-theoretic structures of a certain kind; truth (or maybe satisfaction) in an interpretation is given a recursive definition; and validity is defined as truth-preservation in all interpretations. The facts of validity are then defined by general facts about what interpretations there are. Depending on one's views, this may or may not be a partly empirical question. (And if it is, logic turns out to be an *a posteriori* subject (in the traditional sense), as well as corrigible.)

If a realist account of the nature of logical relations is given, then, whichever that is, the question arises as to the criteria one should use to determine which theory is correct. The standard answer to this is that one decides on the basis of which theory is over-all simplest, most adequate to the data, least *ad hoc*, and so on. These are, of course, exactly the same criteria as are used by the instrumentalist. Thus, although there are profound ontological differences between instrumentalists and realists, when it comes to the decision as to which logic is correct, all parties may appeal to the similar considerations. This is why the debates between various logical theories may well be, and often are, conducted largely independently of the ontological issue.

14. Localism vs. Globalism

Let me finish by commenting on another issue, that of whether the correct logic is global or local. Is the same logical theory to be applied in all domains, or do different domains require different logics? This question is orthogonal to the realist/instrumentalist issue. Since instrumentalists are guided purely by pragmatic concerns, localism sits well with it; but it is quite possible for an instrumentalist to insist that logic is, by its very nature, domain neutral, and so be a globalist. For example, Ryle (1954), argues that topic-neutrality is a necessary condition for a construction to be the concern of formal logic. Globalism is probably the more natural position for a realist, but it is quite possible for a realist to insist that the nature of the logical relationships between statements about one domain is different from that between statements concerning another. Thus, it is quite possible for a realist to be a localist.

In the end, I doubt that there is a serious issue here. Even if modes of legitimate inference do vary from domain to domain, there must be a common core determined by the syntactic intersection of all these.[56] In

[55] Tarski (1956). Tarski's account has recently been criticised by Etchemendy (1990). For some defence of Tarski, see Priest (1995).

[56] It could be, I suppose, that this intersection is empty, but I have never heard a

virtue of the tradition of logic as being domain-neutral, this has good reason to be called *the* correct logic. But if this claim is rejected, even the localist must recognise the significance of this core. Despite the fact that there are relatively independent domains about which we reason, given any two domains, it is always possible that we may be required to reason *across* domains, for example about the relationship between the macrodomain and the microdomain, or between mathematical objects and physical objects. Now, if α is a statement about one domain, and β about another, the only logical relationships that we can count on, e.g., $\alpha \wedge \beta$ or $\alpha \rightarrow \beta$ satisfying are those that are common to the two domains.

But conversely, even a globalist may admit that when applying logic to certain domains, the generally valid inferences may legitimately be augmented by others. An intuitionist, for example, may agree that it is permissible to use all of classical logic to reason about the finite (or at least, the decidable); and a paraconsistent logician may hold that it is permissible to use all of classical logic when reasoning about the consistent. Thus, there would seem to be no significant disagreement between localists and globalists about the facts; only about how to describe them.

15. Conclusion

We started this discussion of geometry, arithmetic and logic with Kant. For him, these were the three great *a priori* sciences. The appearance of non-Euclidean geometries, and, in particular, the adoption of one of them as a physical geometry early this century, made it impossible to subscribe to the Kantian picture. There are many pure geometries; each, when applied, provides a theory of corrigible status. We have seen that exactly the same is true of arithmetic and logic. There are many pure arithmetics and logics; any one of these may be applied to provide a theory of some appropriate domain; and no theory can claim incorrigibility in this matter. Whether such theories are to be interpreted realistically or non-realistically is always a separate issue. Łukasiewicz' analogy between non-Euclidean geometries and non-Chrysippean logics is therefore a highly apt one. In virtue of his role in the creation of such logics, and his realisation that it may be correct to apply them, it may be appropriate for history to see Łukasiewicz as the Riemann of logic.[57]

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plausible argument to this effect.

[57] Besides being given at the conference *Łukasiewicz in Dublin*, parts of this paper were read at the Universities of Queensland and Western Australia. I am grateful for helpful comments by a number of those present on those occasions, including André Gallois, Bill

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Grey, Gary Malinas, Hartley Slater and Ellen Watson. I am also grateful to Len Goddard, Richard Sylvan and, particularly, Ian Hinckfuss for valuable written comments.

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