1. INTRODUCTION

Our understanding of mathematics arguably increases with an examination of its growth, that is with a study of how mathematical theories are articulated and developed in time. This study, however, cannot proceed by considering particular mathematical statements in isolation, but should examine them in a broader context. As is well known, the outcome of the debates in the philosophy of science in the last few decades is that the development of science cannot be properly understood if we focus on isolated theories (let alone isolated statements). On the contrary, we ought to consider broader epistemic units, which may include paradigms (Kuhn 1962), research programmes (Lakatos 1978a), or research traditions (Laudan 1977). Similarly, the first step to be taken by any adequate account of mathematical change is to spell out what is the appropriate epistemic unit in terms of which the evaluation of scientific change is to be made. If we can draw on the considerations that led philosophers of science to expand the epistemic unities they use, and adopt a similar approach in the philosophy of mathematics, we shall also conclude that mathematical change is evaluated in terms of a 'broader' epistemic unit than the one that is often used, such as, statements or theories.

But a second consideration emerges at this point. It may be argued that mathematical change does not provide us with any insight into the nature of mathematics or of mathematical knowledge, since this change is often the result of inconsistent theories, of 'contradictory' views about a particular mathematical domain. For instance, if we consider the development of set theory, it becomes clear that by the time of its formulation, there were quite different views about the concept of function (in particular, with regard to arbitrary functions), different understandings of Cantor’s diagonal proof, and conflicting proposals about the distinction between membership and inclusion. Therefore, so goes the argument, given the ubiquity of these inconsistencies, nothing can be learnt about mathematics by focusing on how a mathematical theory has evolved in time (that is, by considering mathematical change).

* Many thanks to Newton da Costa, Steven French, Joke Meheus, and Sarah Kattau for helpful discussions on the issues examined here.

In my view, there are at least two different reactions to this position. The first is that once appropriate patterns of mathematical change are spelled out, all the parent inconsistencies among mathematical theories are dissolved—one should, I think, find consistent formulations of these theories. Thus, once these patterns are found, we are entitled to learn from the study of the dynamics of mathematics. Of course, this reaction adopts a fairly conservative view towards inconsistencies, and basically assumes that all reliable information should be, at least, consistent.

The second reaction will challenge this assumption. The existence of inconsistencies within bodies of mathematical information does not necessarily preclude us om learning from such bodies. In particular, even if proposed patterns of mathematical change involve inconsistencies, we cannot conclude from this that the pattern is hopelessly unreliable as a way of providing information about the nature of mathematics. Notice that we have here, in fact, two different levels of consistency. The first is concerned with inconsistencies at the theoretical level, involving particular mathematical theories; the second concerns inconsistencies at the metatheoretical level, and this involves our representations of mathematical change. What the second reaction counsels is a unified strategy of accommodating inconsistencies—at face value—at both levels.

The constraint of taking inconsistencies at face value is important. The idea is that inconsistency is not to be taken at face value, or to be ignored. This need not imply that the second reaction turns the second reaction into a variation of the first. As opposed to this, the second reaction requires one to find an appropriate epistemic role for inconsistencies. Once we have an appropriate framework in terms of which mathematical change can be modelled, the second reaction will also have to provide a clear strategy for how to accommodate inconsistencies in a more ‘positive’ way, i.e., that assigns a positive role for inconsistencies within mathematics and mathematical change. The existence of important mathematical theories that are consistent (such as the calculus and naive set theory) is enough to demand such a way.

In this paper, I am concerned with articulating and defending this second reaction. In order to do so, I shall first put forward an account of what sort of ontic unit should be taken as basic. As we shall see in Sections 2 and 3, I take it that two independent proposals found in Kitcher 1984 and Lauden 1984, if suitably combined, provide the first step towards an appropriate framework to do this move, however, is not sufficient as a defence of the second reaction, since resulting framework cannot accommodate inconsistencies. In order to pursue this k—-which is the chief aim of Section 3—I propose that we adopt the conceptual resources provided by da Costa and French’s partial structures approach (see, for instance, da Costa and French 1989, 1990, 1993, and 1995). As we shall see in Section 3.1, three important features are brought by this view: a broader notion of structure (partial structure), a weaker notion of truth (quasi-truth), and a consistent setting (in terms of the underlying logic of quasi-truth: a convenient view of Jaśkowski’s logic; see da Costa, Bueno and French 1998). In this way, or I shall argue in Section 3.2, we can put forward an account of inconsistencies involved in mathematical change, without triviality. Finally, in section 3.3, I shall provide a case-study, showing how the framework introduced helps us to understand some aspects of the development of set theory, including a consideration of the role played by some recent quasi-consistent versions of this theory in spelling out the nature of the research on set-theoretical structures.

2. MODELLING MATHEMATICAL CHANGE: A CONSISTENCY-PRESERVING PATTERN

As is well known, there is an old tradition of interpretation of science that adopts the strategy of putting forward a view about the ways in which science evolves in order to draw general conclusions concerning the nature of scientific knowledge. This tradition takes the notion of scientific change as basic for the understanding of the nature of science and the knowledge it supplies. Of course, this is not to say that problems unrelated to the dynamics of scientific change are not to be considered within such a view; they are. But the analysis proposed is (to be) articulated in terms of an account of scientific change. Indeed, it is at this level that the ‘explanatory power’ of this tradition can be found.

Kitcher’s account of the nature of mathematical knowledge (as presented in Kitcher 1984) certainly belongs to this tradition. The role that Kitcher assigns to scientific change—in particular, to mathematical change—parallels in importance that found within, for instance, either Popper’s or Lakatos’s proposals: the task of taking into account scientific (or mathematical) knowledge is to be achieved mainly by the formulation of a theory of scientific (or mathematical) change.

However, Kitcher’s view of mathematical change is embedded in a general epistemological framework, and much of the motivation for his proposals rests on his critical appraisal of previous epistemological accounts of mathematics. The relevance of mathematical change is presented in the context of a critique of two interrelated doctrines that have characterised such previous accounts, namely, mathematical apriorism (the claim that mathematical knowledge is a priori), and mathematical ‘individualism’ (a lack of consideration of the role of mathematical community in the development of mathematics). As opposed to the former, Kitcher puts forward his mathematical empiricism, rejecting earlier empiricist interpretations of mathematics, such as Lakatos’s, Putnam’s and Quin’s (see Kitcher 1984, 4). As opposed to the latter, he advances a theory of mathematical practice, motivated by Kuhn’s view, but stripped of his general framework, in order to claim that the development of mathematical knowledge occurs in terms of a ‘rational modification

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1 Actually, one of Kitcher’s main points consists in spelling out the similarities (without disregarding the differences) between scientific and mathematical changes, in order to motivate his account of the methodology of mathematics (see Kitcher 1984, 150, and the rest of Chapter 7). Indeed, one of his claims is that the growth of mathematical knowledge is far more similar to the growth of scientific knowledge than is usually appreciated (Kitcher 1984, 8). Of course, such a claim clearly echoes Lakatos’s view about the connection between scientific and mathematical knowledge (for details, see Lakatos 1976 and Lakatos 1978b).

2 In particular, the Kuhnian notion of paradigm is not accepted (see Kitcher 1984, 163-164).
of mathematical practices’ (Kitcher 1984, 165). These practices, which are the basic epistemic unit of mathematical change, have five components: a language, certain accepted (mathematical) statements, certain accepted types of reasoning (an underlying logic), certain questions considered as important and certain metamathematical standards with regard to proofs, definitions and so on (Kitcher 1984, 163). The introduction of this notion of mathematical practice is Kitcher’s way of assigning a function to the mathematical community within his proposal, even though in a rather idealised way.

As a result of the shift to mathematical empiricism and the emphasis on mathematical practice, Kitcher’s account of mathematics acquires a historicist outlook: mathematical knowledge is obtained by extending the knowledge produced by previous generations (see Kitcher 1984, 4-5). At this point, it is natural to ask, working backwards in the chain of generations, how the knowledge of the first generations was produced at all? To this question, Kitcher formulates a surprising answer: he traces back this knowledge to ‘ordinary perception’ (1984, 5). In order to explain the possibility of obtaining mathematical knowledge from perception, Kitcher devises a theory of mathematical reality, of ‘what mathematics is about’ (1984, 101-148). In order then to show how from such perceptual origins mathematics can be developed into such an impressive body of theories, techniques and results that one finds today, he puts forward his account of the growth of mathematical knowledge (1984, 149-228). These two doctrines constitute the core of Kitcher’s mathematical empiricism.

Thus, Kitcher’s account of mathematical change has two important features. Firstly, it represents mathematical change in terms of broad epistemic units, mathematical practices (in Kitcher’s sense). Secondly, because of this feature, it allows one to formally accommodate the role of mathematical activity and practice in the changes of mathematical theories. As a result, so Kitcher argues, a more faithful account of the dynamics of mathematics, vis-à-vis the historical record, is put forward.

In this picture, each particular historical moment is characterised by a given mathematical practice, involving a community of mathematicians who adopt the same language to investigate a given mathematical domain, who share a common core of accepted mathematical statements and open questions, and who, in trying to settle the latter, adopt the same metamathematical standards and the same logic. Mathematical change results from the interplay of each of these five components, as well as changes in them. For instance, new, more demanding patterns of rigour (a change in metamathematical standards) may lead to the rejection of a previously accepted proof and to the demand for a new proof of a given result. Similarly, the rejection of a previously accepted mathematical statement S will bring an epistemic change in all results which depend on S, requiring new proofs of them independent of S. Moreover, by considering some open questions as no longer important, changes are introduced in the way mathematics is practised. This may result from the acceptance of new mathematical statements, which change the relative importance of the open questions, since they change the direction of the research in particular mathematical domain. Furthermore, by changing the underlying logic of given mathematical practice, new inferential patterns that have been previously rejected can be accommodated—and this may increase the set of accepted statements. However, depending on the logic in question (suppose classical logic is replaced by intuitionist logic), previously accepted inferential patterns may be rejected (for instance, the law of excluded middle would not be valid anymore). This may reduce the accepted statements, given that we are no longer able to derive, on the basis of the underlying logic, any statement depending on the excluded middle. Finally, changes in the language used by a given mathematical community also play a role in mathematical changes.

Two questions immediately arise at this point. The first concerns the ‘completeness’ of Kitcher’s account. How can one guarantee that all different kinds of mathematical change will be accommodated in terms of this pattern? If there is a case that cannot be so explained, Kitcher’s proposal will be clearly ‘incomplete’. The point, however, is not so decisive, given that Kitcher may legitimately relinquish the provision of a ‘complete’ account of mathematical change: he may be only concerned with spelling out some factors involved in this change, without aiming at characterising all of them. Although each factor he presents is clearly sufficient to characterise a circumstance in which a change has occurred, it does not seem to be necessary for it. From the fact that, in Kitcher’s proposal, there are several components in a mathematical practice, we can already suppose that each of them is not taken to be necessary. For example, Kitcher will certainly accept that there might be changes in mathematics without any change in metamathematical patterns, but only, say, in the underlying logic. But provided one is able to acknowledge this point (that if taken in isolation, the components of a mathematical practice are not necessary for explaining mathematical change), this first remark may not be problematic. What one would have abandoned is the idea that the framework is capable of accommodating any kind of mathematical change. After all, if none of the components is necessary for the explanation, there are no grounds to claim that all possible types of change will be accommodated. So, the claim of ‘completeness’ is given up. But it should be acknowledged that any such claim is certainly too strong. In fact, it is most unlikely that an account of mathematical change will be able to accommodate not only every single change in past mathematical theories, but also in those theories which are yet to be constructed. What we should strive for is an account as complete as possible, able to save the phenomena as comprehensively as we can.

The second point, which is far more important, concerns two requirements involved in an account of theory change in mathematics. Patterns of mathematical change should provide: (i) an aim in terms of which the various changes can be evaluated, and (ii) an overall framework to formally represent the changes in question. Such a framework should accommodate three further issues: (a) epistemic changes associated with changes of mathematical theories, (b) formal interaction between these different theories, and in particular (c) the existence of inconsistencies in theory change in mathematics.

Two questions raise themselves at this point: (1) What is the importance of each of these requirements? Why should an account of mathematical change deal with these issues? (2) Can Kitcher’s account accommodate them? As for the first question, the requirements are introduced because, in providing an account of
mathematical change, we should not only describe particular cases in which
mathematical theories have changed, but also explain such changes. By putting
forward an aim of mathematics, we should be able to trace back the description
of mathematical change to a particular interpretation of mathematics; in other
words, we should be able to spell out in what respects such a change contributed to our
understanding of mathematics and, in particular, to mathematical knowledge. The
main thought is that, just as in the case of science, we can learn about the nature of
mathematics by studying how mathematical theories are formulated and developed
through time. This is done by relating the changes in question to a given aim of
mathematics, and depending on how such an aim is satisfied, we will be able to
explain the changes that have occurred.

But in order to do that, we will need a proper formal framework to represent
three distinct levels of change, namely changes about the epistemic status of
different mathematical theories (epistemic changes), changes about the formal
interactions between these theories (formal changes), and in particular, such formal
interactions should be articulated in such a way that inconsistencies can be
accommodated (inconsistency-tolerating view). The importance of each of these
three features is that they allow one to represent important factors of mathematical
change. To say the least, inconsistencies play a heuristic role in the development of
new mathematical theories—although we should arguably be able to assign a
stronger, more positive, role to them. Epistemic changes are an outcome of changes
in mathematical theories: theories which have previously received strong epistemic
support may later not be taken so favourably, and vice-versa. This is a result of the
interplay of each of the five components of a mathematical practice. But there is
something that such a practice, in Kitcher’s sense, can hardly accommodate: the
formal interaction between distinct mathematical theories. Of course, I am not
concerned with particular relationships between such theories at the level of
mathematical activity (this is something to be dealt with by particular mathematici-
ans). What I am concerned with is the formal representation of mathematical
change itself: a formal pattern that represents the interplay between distinct
mathematical theories in time.

None of these features seems to be found in Kitcher’s account. There is no aim
of mathematics, in order to allow an evaluation of mathematical changes, and no
formal framework is presented in terms of which the three kinds of change just
presented can be accommodated. What is required then is a new approach that is
able to handle these issues. However, such an approach should preserve the most
important feature of Kitcher’s view, namely the five components he has identified in
mathematical change. To sketch this alternative is our task in the next section.

3. MODELLING MATHEMATICAL CHANGE: AN INCONSISTENCY-
ACCOMMODATING PATTERN

In order to advance a pattern of mathematical change that allows one to
accommodate inconsistencies in mathematics, we should first have a proper formal
framework. In my view, due to its rich expressive resources and its ‘formal
openness’, da Costa and French’s partial structures approach provides a suitable
setting. Let us start then by introducing its main features.

3.1 Partial Structures and Quasi-truth

The partial structures approach relies on three main notions: partial relation, partial
structure and quasi-truth. One of the main motivations for introducing this proposal
comes from the need for supplying a formal framework in which the ‘openness’ and
‘incompleteness’ of scientific practice and knowledge can be accommodated in a
unified way (see da Costa and French 2001). This is accomplished by extending, on
the one hand, the usual notion of structure, in order to model the partialness of
information we have about a certain domain (introducing then the notion of a partial
structure), and on the other hand, by generalising the Tarskian characterisation of the
concept of truth for such ‘partial’ contexts (advancing the corresponding concept of
quasi-truth).

The first step then, in order to introduce a partial structure, is to formulate an
appropriate notion of partial relation. When investigating a certain domain of
knowledge A, we formulate a conceptual framework that helps us in systematising
and organising the information we obtain about A. This domain is then tentatively
represented by a set D of objects, and is studied by the examination of the relations
holding among D’s elements. However, we often face the situation in which, given a
certain relation R defined over D, we do not know whether all the objects of D (or
n-tuples thereof) are related by R. This is part and parcel of the ‘incompleteness’ of
our information about A, and is formally accommodated by the concept of partial
relation. The latter can be characterised as follows:

Definition 1. Partial relation:
Let D be a non-empty set. An n-place partial relation R over D is a triple (R1,
R2, R3), where R1, R2, and R3 are mutually disjoint sets, with R1∪R2∪R3 = Dn,
and such that: R1 is the set of n-tuples that belong to R, R2 is the set of n-tuples
that do not belong to R, and R3 is the set of n-tuples for which it is not defined
whether they belong or not to R.

Remark 1:
Notice that if R3 is empty, R is a usual n-place relation which can be identified
with R1.

However, in order to represent appropriately the information about the domain
under consideration, we need of course a notion of structure. The following
characterisation, spelled out in terms of partial relations and based on the standard

3 This approach was first presented in Mikenberg, da Costa and Chuaqui 1986, and in da Costa
1986a. Since then it has been extended and developed in several different ways; see da Costa
partial relations defined over $D$, and $P$ is a set of accepted sentences. The idea, as we shall see, is that $P$ introduces constraints on the ways that a partial structure can be extended.

Our problem now is, given a pragmatic structure $A$, what are the necessary and sufficient conditions for the existence of $A$-normal structures? We can now spell out one of these conditions (see Mikenberg, da Costa and Chuaqui 1986). Let $A = (D, R, P)_{tel}$ be a pragmatic structure. For each partial relation $R$, we construct a set $M_i$ of atomic sentences and negations of atomic sentences, such that the former correspond to the $n$-tuples which satisfy $R$, and the latter to those $n$-tuples which do not satisfy $R$. Let $M$ be $\bigcup_{i=1}^{n} M_i$. Therefore, a pragmatic structure $A$ admits an $A$-normal structure if, and only if, the set $M \cup P$ is consistent.

As this condition makes it clear, the notion of consistency plays a crucial role in the partial structures approach. In fact, as we shall now see, the very concept of quasi-truth, since it depends on the existence of $A$-normal structures, supposes the consistency of $M$ and $P$. Having said that, let us see how this concept can be formulated.

**Definition 4. Quasi-truth:**
A sentence $\alpha$ is quasi-true in $A$ according to $B$ if (i) $A = (D, R, P)_{tel}$ is a pragmatic structure, (ii) $B = (D', R')_{tel}$ is an $A$-normal structure, and (iii) $\alpha$ is true in $B$ (in the Tarskian sense). If $\alpha$ is not quasi-true in $A$ according to $B$, we say that $\alpha$ is quasi-false (in $A$ according to $B$). Moreover, we say that a sentence $\alpha$ is quasi-true if there is a pragmatic structure $A$ and a corresponding $A$-normal structure $B$ such that $\alpha$ is true in $B$ (according to Tarski’s account). Otherwise, $\alpha$ is quasi-false.

The idea, intuitively speaking, is that a quasi-true sentence $\alpha$ does not necessarily describe, in an appropriate way, the whole domain to which it refers, but only an aspect of it—the one modelled by the relevant partial structure $A$. After all, there are several different ways in which $A$ can be extended to a full structure, and in some of these extensions $\alpha$ may not be true. As a result, the notion of quasi-truth is strictly weaker than truth: although every true sentence is (trivially) quasi-true, a quasi-true sentence is not necessarily true (since it may be false in certain extensions of $A$). This is an important feature of this notion.

It is time now to return to the issue of mathematical change. As we shall see, it can be addressed in a new way if we explore the resources provided by the above framework.

**3.2 A Simple Pragmatic Pattern**

The pattern of mathematical change to be provided in this section has two main features. The first is to advance a clear aim of mathematics, in terms of which mathematical change can be evaluated. The second is the possibility of accommodating inconsistencies in mathematical theories, both at the theoretical and

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*For a different formulation of quasi-truth, independent of the notion of an $A$-normal structure and in terms of quasi-satisfaction, see Bueno and de Souza 1996.*
the metatheoretical levels, that is, not only involving particular theories but also formal representations of mathematical change.

I will suggest here a pattern of mathematical development, whose inspiration derives from Kitcher’s (and, to some extent, Lakatos’s) work, but which is articulated in terms of the partial structures framework. The idea is that those episodes in the development of mathematics that involve inconsistencies, ambiguous notions, or overlap of different concepts, can be straightforwardly accommodated in terms of partial structures. As we shall see, the information about these concepts was only partially grasped by the structures in question. Moreover, the resulting dynamics is articulated, or reconstructed, in terms of the search for (increasingly more) quasi-true theories—the latter are not, of course, necessarily true. (I shall define the notion of degree of quasi-truth in a moment.)

But first how is the quasi-truth of a mathematical theory to be evaluated? Given the definition of quasi-truth, this is always a contextual matter: we shall determine it in terms of a given partial structure and a given set of accepted sentences (the two main components of a pragmatic structure or, as we may call it, since it incorporates crucial information about a given domain of inquiry, a partial mathematical practice). As we saw, we say that a mathematical theory $T$ is quasi-true (in a partial structure $A$) if there is an $A$-normal structure $B$ in which $T$ is true. Now this, in any sense, an appropriate aim of mathematics? I think it is. First, although this may not seem to be the case at first sight, quasi-truth is not too weak. Notice that for a theory to be quasi-true is not equivalent for it to be consistent: what is required is a model of the extended partial relations of a given partial structure and the set $P$ of accepted sentences. So, pragmatic structures play a crucial role in the evaluation of quasi-truth, and to this extent, the latter is not reducible to sheer consistency. Therefore, I am not countenancing consistency as an aim of mathematics. This would be inadequate on two grounds: (i) consistency is certainly too weak to accommodate the several uses of mathematics to be taken into account by an interpretation of mathematics. For instance, in order to explain the applicability of mathematics to non-mathematical domains, we need more than the mere consistency of mathematics (although we certainly need less than truth). What is required, at least according to Field, is the consistency of the relevant mathematical theory with all internally consistent non-mathematical theories; in other words, we need the conservativeness of mathematics, which is more than its consistency (see Field 1980 and 1989). Moreover, (ii) it is question-begging to assume consistency as an aim of an activity that is so prone to entanglement with inconsistencies. And there are good arguments to the effect that inconsistencies may have an epistemic role not only in the understanding of mathematics, but also in its development (the well-known examples of the calculus and naive set theory are enough to establish this point; see da Costa 1982, 1986b, and 1989, da Costa, Béziau and Bueno 1998, Priest 1987, and Mortensen 1995). This provides a second reason for the appropriateness of quasi-truth as an aim of mathematics. Given that the underlying logic of quasi-truth is paraconsistent (see da Costa, Bueno and French 1998), the resulting conceptual setting provides an adequate framework to accommodate inconsistencies in mathematical theories.

In the present account, mathematical change results from the interplay of the five components of a mathematical practice (identified by Kitcher), but in such a way that either quasi-truth is preserved, or its degrees are extended. Before elaborating on the notion of degree of quasi-truth, let me point out that the five components do not belong to the same category; it is important to clearly distinguish the levels to which they pertain. They can be organised in three distinct levels: (i) a given axiology of mathematics (which involves aims of mathematical research), (ii) the adoption of a particular methodology (bringing the methods to accomplish the desired aims), and (iii) the construction of a particular theoretical core (which are mathematical theories taken to realise the aims in accordance with the adopted methodological principles). Each of these three levels receives further refinements. An axiology is constituted not only by an aim of mathematics, but also by tasks and values. Tasks are ‘intermediary aims’, which have to be assumed in order to achieve the basic, chief aim; values are constraints introduced in mathematical research: necessary conditions for the adequacy of the tasks entertained. A methodology involves two further elements: a properly methodological part (which is concerned with rules and methods introduced in order to establish mathematical results), and a metamethodological part (which involves rules to choose between different mathematical practices). Among these components we find metamathematical patterns and the use of a given logic. Finally, the theoretical core is constituted by particular mathematical theories, which are formulated in a given language, and are constructed in order to settle open questions, satisfying a set of accepted sentences.

Following Laudan’s 1984 proposal, these three categories (aims, methods and theories) are not assumed to change in a unique, hierarchical way. Theories may require changes in aims (for instance, by showing that the latter cannot be ultimately realised), and also in methods (by requiring new patterns of theory construction). Furthermore, methods constrain aims (besides providing means to realise them), and aims, in turn, should be fulfilled by theories. Hence, there is a number of complex interrelationships between these three categories (in particular, they are always ‘two-way’ relations), and the development of mathematics can be accommodated in terms of the resulting interplay. The main idea is that what constitutes a mathematical practice, at a given moment, is the instantiation of each of these three components, and that mathematical change is a piecemeal process in which parts of these components are successively changed, but not the whole mathematical practice at once. In the present view, the aim is, of course, taken to be quasi-truth (and in some contexts, if it is not the aim, at least it is a value to be met by the theories constructed within the practice). As we saw, at the methodological level we find

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6 For a reformulation of Field’s approach in terms of partial structures and quasi-truth, see Bueno 1999a.

7 I am using and blending together here two distinct methodological frameworks: one derived from Laudan 1984, the other from Chiappin 1989. I owe to Laudan the idea of organising scientific debates in terms of the interplay between aims, methods and theories. To Chiappin I am indebted for the refinement of aims via tasks and values, and in particular, to the distinction between methodological and metamethodological debates. None of them, however, has applied the resulting framework to the philosophy of mathematics.

8 For the articulation of this distinction in the context of the philosophy of science, see Chiappin 1989.
More information about the latter is often obtained by establishing new relations about it, or by extending those that were not entirely defined.

In particular, there are two important processes in order to accomplish this quest for more information: by providing detailed proof analyses (which often bring new concepts), and by making definitions of given concepts (more) precise.¹⁰ When a mathematician is trying to establish a result (by providing a proof to it), but ends up finding a counter-example, there are two basic strategies that she can take: (a) to deny that the counter-example is actually such (this often involves redefining some of the concepts used up to that moment, in order to get rid of the putative counter-example), (b) to accept the counter-example as it is, and to examine whether it brings problems to (i) the whole theorem, or (ii) only to the tentative proof. In the latter case (i), the mathematician starts an analysis of the proof, trying to spot a false lemma, shown to be false by the counter-example, trying then to resume her proof. In the case (i), the mathematician shall refine some of the concepts used up to that moment, in order to come up with a new version of the theorem.¹¹

Thus far, the notion of degree of quasi-truth has been advanced, assuming that we have a clear grasp of what a domain of inquiry is. But how should we characterise this notion of domain in mathematics? One possibility is to say that each domain is determined by the kind of structures studied in it. In real analysis, for instance, one is concerned with real numbers structure; certain algebraic structures are the subject matter of algebra; topology examines topological structures, and so on. To some extent, Bourbaki’s conception of mathematics as the interplay of three basic kinds of structures (algebraic, topological and of order) is an important instance of this view (see Bourbaki 1950 and 1968). Despite stressing the role played by the interaction between these kinds of structures, rather than by taking them in isolation, it is crucial for Bourbaki’s view to understand the domain of mathematical theories in terms of their structural (set-theoretical) properties. In this picture, mathematics is conceived as the study of structures, and it leads to an ‘open-

¹⁰ An illuminating discussion of these two processes can be found in Lakatos 1976. In what follows, I shall be considering some of his views.

¹¹ Without developing the details, I wish to point out that the partial structures approach supplies fruitful tools to consider the heuristic features involved in the process of construction of proofs, i.e. the process of trying to establish particular conjectures. The stage in which one is unsure whether a certain conjecture holds or not—the stage in which, according to Lakatos 1976, the mathematician tries both to prove and to refute the conjecture in question—can be modelled very naturally in terms of partial structures. In fact, we can think of the various possible outcomes of such an inquiry as represented in a problem-space, a very general kind of mathematical structure that generalises the notion of state-space employed in van Fraassen’s version of the semantic approach (see van Fraassen 1970, 1980 and 1989). The inquiry itself can be represented as a trajectory in such a problem-space. The trajectory, in its turn, is represented in terms of the relations that hold between the objects of the problem-space. Now, if such relations are partial (in which case, we will have a partial-problem-space), they are not defined for every n-tuple of objects of the domain. Which means that the mathematician is still uncertain about the actual truth-value of the conjecture that depends upon such relations. The latter may either belong to the relevant domain (in which case, let us say, the conjecture would be true), or not belong to the domain (in which case, the conjecture would be false). Thus, Lakatos’s heuristic suggestion with regard to trying both to prove and to refute an open conjecture, being connected to the actual ‘openness’ of the epistemic situation of the mathematician, seems natural enough within such a context.

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The idea is that, as we move from a pragmatic structure A to one of its partial extensions A', we provide more information about A, increasing, in this sense, the degree of quasi-truth of the theory about A. More formally, we say that a mathematical theory T' is more quasi-true than a mathematical theory T if (i) T' is quasi-true (in a partial structure A'), (ii) T is quasi-true (in a partial structure A), and (iii) A' is a partial extension of A. This formulation accommodates the point that the more information a mathematical theory is able to encompass, the more quasi-true it is.⁹ In this sense, by adopting quasi-truth as an aim of mathematics, we are able to represent mathematical changes as processes in which more quasi-true theories are put forward. Notice that, since we are considering here mathematical theories (as opposed to theories about the physical world), the increase in the degree of quasi-truth depends only on the ‘structural’ properties of the domain under consideration, that is, on the relations holding among the (mathematical) objects of this domain.

⁹ Since, as we saw, quasi-truth is weaker than truth, from the fact that a theory is more quasi-true than another, it does not follow that it is true. This supplies one of the reasons why quasi-truth can be taken as an aim of mathematics appropriate for an empiricist (or an anti-realist) view (see Bueno 1999a).
ended' view about the development of mathematics (although Bourbaki never puts the issue in these terms). After all, the characterisation of mathematical structures in terms of three basic kinds of structures is, of course, at best tentative: it may well be that, in the future, new types of structures will be formulated which cannot be reduced to any of the extant structures, nor to the interplay between them. In this sense, the characterisation provided by Bourbaki is tentative, historically determined, and inductive, because it is based on, and generalises from, our current mathematical knowledge to its future evolution. These are all important features, especially for the 'openness' they allow in our representation of mathematics.

My claim is that, by taking quasi-truth as the aim of mathematics and by countenancing partial structures as the corresponding formal setting, such 'openness' can be straightforwardly accommodated. Moreover, if we then use the methodological categories previously discussed, we have a pattern of mathematical change that, as opposed to the standard accounts, make room for inconsistencies as an important feature of mathematics. More importantly, given the use of a convenient paraconsistent logic, the resulting inconsistencies do not lead to triviality (see, for instance, da Costa 1974). In order to illustrate some of these points, I will sketch an application of this pattern, considering the early formulation of set theory.

3.3 The Pattern at Work: Set Theory and Paraconsistency

Two main themes can be put together in order to spell out (an aspect of) the development of set theory: (i) Cantor’s celebrated diagonal proof, and the related distinction between inclusion and membership; and (ii) Russell’s paradox, which emerged from Russell’s own concern with this distinction, and the role played by this paradox in the subsequent development of set theory. According to Kanamori (1997), the important point is that we are dealing here mostly with mathematical moves, and that despite the ‘pronounced metaphysical attitudes and exaggerated foundational claims that have been held on [set theory’s] behalf’, the ‘progression of mathematical moves’ has been with set theory since its beginnings (1997, 281). We may wonder, however, in what sense these moves can be called a progression. If we want to assign to this progression any epistemic import, we need a normative framework in terms of which we can evaluate judgements about mathematical change; otherwise, we end up only describing (rather than explaining) a succession of moves that have been taken in the past. One of the points of this section is to spell out how the framework provided above can be used to fulfil this role.

As is well known, in 1891, Cantor provided his celebrated diagonal proof, establishing that the collection of functions from a set $X$ into a set of two elements, say, $\{0, 1\}$, has a higher cardinality than $X$. Today, with set theory far more articulated, this result is considered as proving how the power set operation leads to higher cardinalities. However, as Kanamori correctly notices, ‘it would be an exaggeration to assert that Cantor himself used power sets’ (1997, 282; see also Lavine 1994, 245). He was, in fact, extending the 19th Century concept of function, by countenancing arbitrary functions. At this stage, Cantor had already taken an important step that would culminate in the concept of function we adopt today. From his inquiries in trigonometric series during the 1870’s, and in trying to extend the theory of these series to more general classes of functions, he became interested in arbitrary sets of real numbers. From this point, it was natural to consider any association from the real numbers to the real numbers as a function, no matter how arbitrary it was (see Lavine 1994, 3). This would then pave the way for Cantor to construct the diagonalising function. He could consider all functions with a given domain $L$ and range $\{0, 1\}$, and take these functions to be enumerated by a superfunction $f(x, z)$, with $z$ being the enumerating variable. The diagonalising function is then $g(x) = 1 - f(x, x)$. We have already here a pattern of abstraction that would be typical of, and crucial for, the subsequent development of set theory.

However, Cantor’s diagonal argument could only run in terms of sets once the distinction between inclusion ($\subseteq$) and membership ($\in$) was clearly made (for a discussion, see Kanamori 1997, 283-284). This distinction is, of course, crucial for Frege’s Begriffsschrift, and it was presented—not without some blunders—by Peano, who explicitly admonished against confusing the sign ‘$\in$’ with the sign ‘$\subseteq$’ (see Peano 1889, 86). It was in the hands of Russell that the distinction played a crucial role. Of course, set theory, as an abstract study of the set formation ({} operation, could only be articulated if we had such a distinction clearly established (Kanamori 1997, 284). In September of 1900, after adopting Peano’s distinct signs for membership and inclusion, Russell extended Peano’s approach to the logic of relations, defined cardinal number, and recasted some of Cantor’s work. With these results, by the rest of the year, he worked out and concluded the final draft of The Principles of Mathematics. However, the book itself was to be published only a few years later (see Russell 1903). The reason for this is well-known: in May 1901, in the course of this development, Russell realised that Cantor’s diagonal proof could be changed into what we now call Russell’s paradox—spotting thus a tension between Cantor’s one-to-one correspondences and Peano’s inclusion / membership distinction (see Kanamori 1997, 286).

Russell’s first reaction was to claim that he had found an error in Cantor’s proof. But he soon realised that there was no error there, but actually a paradox. In

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12 Even if Bourbaki has not presented his own view in this fallibilistic tone, following Newton da Costa, I think this is probably the most sensible and charitable way of understanding Bourbaki’s programme. (For a discussion of Bourbaki’s view in the context of the philosophy of science, see da Costa and Chuaqui 1988.)

13 These themes have been presented and articulated in great detail by Kanamori, and are actually only part of a comprehensive perspective on the development of set theory (for a fuller picture, see Kanamori 1996 and 1997). In particular, in his 1997 paper, Kanamori also examines Zermelo’s study of functions from the power set of a set $X$ into $X$ (i.e. $f: \mathcal{P}(X) \to X$), Zermelo’s proofs of the well-ordering theorem, and the contribution of the latter to his own articulation of set theory. In what follows, although I will not consider Zermelo’s case, I shall heavily draw on Kanamori’s account, trying to provide a general conceptual setting for the changes it describes.

14 As Kanamori notices (1997, 284), after clearly distinguishing between membership and inclusion, Peano made the former follow from the latter, thus undermining the very distinction he wanted to stress (see Peano 1889, 90, formula 56).

15 In a letter to Louis Couturat (from 8th of December 1900), Russell notices: ‘I have discovered an error in Cantor, who maintains that there is no largest cardinal number. But the number of classes is the largest number. The best of Cantor’s proofs to the contrary […] amounts to showing that if $x$ is a class
and made \( x \in x \) a meaningless proposition. In this way, the contradiction is avoided (Russell 1903, 517).\(^{17}\)

This move, however, seems arbitrary. Propositions of the form \( x \in x \) are certainly meaningful, and to make them meaningless just to circumvent the paradox looks like a desperate move. With hindsight, we can understand Russell’s despair: if the very foundations of mathematics were inconsistent, the whole mathematical building would collapse under the weight of triviality.

From Cantor to Russell, we have seen a conceptual move in which the investigation of a particular domain (the infinite) has been studied, and delicate distinctions had to be introduced in order to investigate it. Starting with the distinction between inclusion and membership, we have a long process in which this very distinction had to be abandoned (at least with regard to some objects) in order to proceed the investigation. This particular change can be represented as a process in which more information about the structures under consideration was obtained, by extending relations which were initially only partially defined, and by producing a theory which (even if not true) was at least more quasi-true than the existent alternative. Roughly speaking, Russell provided a partial extension of some of Cantor’s framework, and in doing so he could determine the appropriate extension of a relation (between membership and inclusion) that was crucial for the inquiry thus far—at least from the viewpoint of the pragmatic structure that Russell was considering. In order to overcome the resulting difficulties, the theory of types was formulated. However, given Russell’s own tentative pronouncements about this theory,\(^{18}\) it is not (epistemically) adequate to say that he took it to be true. The best evaluation is to assert something weaker: its quasi-truth (with regard to the structures under consideration). Thus, we have a framework to evaluate some of the moves involved in mathematical change and in mathematical practice, in the context of the early years of set theory.

Russell’s proposal was clearly devised in order to avoid inconsistency. And in this respect, the alternative set-theoretical strategies to avoid Russell’s paradox are not different. Let us say that Cantor put forward a naïve theory about the infinite.\(^{19}\) It was based largely on two fundamental principles. The postulate of extensionality (namely, if the sets \( x \) and \( y \) have the same elements, then they are equal), and the postulate of separation (i.e., every property determines a set, composed of the objects that have this property). The latter postulate, in the standard language of set theory, becomes the following formula (or scheme of formulas):

\begin{align*}
\forall x \exists y \forall z (z \in y \leftrightarrow (z \in x \land \neg \psi(z)))
\end{align*}

\[ \psi(z) \]

whose number is \( \alpha \), the number of classes included in \( u \) (which is \( 2^\alpha \)) is larger than \( \alpha \). The proof presupposes that there are classes included in \( u \) which are not individuals that is, members of \( w \); but if \( u = \text{Class} \) [that is, the class of all classes], that is false: every class of classes is a class’. (This passage, whose original is in French, is quoted in Kanamori 1997, 286; for a discussion, see also Moore 1995, 321.)

\(^{16}\) As Kanamori acknowledges (1997, 288, note 21), the formal transition from \{\( x : x \notin f(x) \)\} to \{\( x : x \notin x \)\} was stressed in Cresswell 1973.

\(^{17}\) For the articulation of the theory of types, see Whitehead and Russell 1910-13. A historical analysis of the development of Russell’s type theory to Gödel’s constructible universe can be found in Drebend and Kanamori 1997.

\(^{18}\) As Russell stresses: ‘It appears that the special contradiction of Chapter X Russell’s paradox’ is solved by the doctrine of types, but there is at least one closely analogous which is probably not soluble by this doctrine. The totality of all logical objects, or of all propositions, involves, it would seem, a fundamental logical difficulty. What the complete solution of the difficulty may be, I have not succeeded in discovering; but as it affects the very foundations of reasoning, I earnestly commend the study of it to the attention of all students of logic’ (Russell 1903, 258).

\(^{19}\) This point has been challenged by Lavine (1994), who claims that naïve set theory is a creation of Russell, and it is incorrectly attributed to Cantor. But let us stick, for the sake of the argument, to the received view about the development of set theory, in which naïve theory plays a crucial role.
Of course, if we replace the formula $F(x)$, in (a), for $x \notin x$, we immediately derive Russell’s paradox. In other words, the principle of separation (a) is inconsistent. As a result, if (a) is added to first-order logic (which is often taken as the logic of the language of set theory), the resultant theory is trivial.

We can distinguish the various classical set theories in terms of the restrictions they impose on (a), in order to avoid the paradox. The problem, however, is that the resulting theory might become too weak. In order to overcome this difficulty, further axioms have to be added, besides extensionality and separation (with appropriate restrictions). Which axioms to introduce will depend on the particular case under consideration. For example, in Zermelo-Fraenkel set theory (ZF), separation is formulated thus:

$$\forall z \exists y \forall x (x \in y \leftrightarrow (F(x) \land x \in z))$$

where the variables are subject to the usual conditions. We can say that, in ZF, $F(x)$ determines the subset of the elements of the set $z$ that have the property $F$ (or satisfy the formula $F(x)$). But in the Kelly-Morse system, separation is formulated differently:

$$\exists y \forall x (x \in y \leftrightarrow (F(x) \land \exists z (x \in z)))$$

Finally, with the notion of stratification, in Quine’s NF the scheme of separation has the form:

$$\exists y \forall x (x \in y \leftrightarrow F(x))$$

where the formula $F(x)$ is taken to be stratifiable (assuming also that the standard conditions with regard to the variables are met).

Given that all these proposals were formulated in order to avoid a given inconsistency, we may wonder whether this problem can be considered from a distinct point of view. Would it be possible to accept the scheme (a) without restrictions—but without triviality? The answer is straightforward: in order to do that, we have to change the underlying logic, so that (a) does not lead to trivialisation. Since the separation scheme (without considerable restrictions) leads to contradictions, this logic has to be paraconsistent. (This was in fact one of the motivations underlying da Costa’s formulation of paraconsistent logic; see da Costa 1974, and, for further discussion, da Costa and Bueno 2001.)

It was slowly verified that there are infinitely many ways to make the classical restrictions to the separation scheme weaker, each of them corresponding to distinct categories of paraconsistent logics. Moreover, extremely feeble logics have been formulated, and in terms of them it is possible to use, without triviality, the scheme (a). Some set theories, in which the forms (b), (c) and (d) of separation are either combined or adopted in isolation, have also been constructed.

### REFERENCES


