

The British Society for the Philosophy of Science

Towards a Theory of Mathematical Research Programmes (I)
Author(s): Michael Hallett
Source: *The British Journal for the Philosophy of Science*, Vol. 30, No. 1 (Mar., 1979), pp. 1-25
Published by: Oxford University Press on behalf of The British Society for the Philosophy of Science
Stable URL: https://www.jstor.org/stable/687414
Accessed: 16-07-2019 12:38 UTC

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at https://about.jstor.org/terms



Oxford University Press, The British Society for the Philosophy of Science are collaborating with JSTOR to digitize, preserve and extend access to *The British Journal for the Philosophy of Science*

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

Brit. J. Phil. Sci. **30** (1979), 1–25 *Printed in Great Britain*

Towards a Theory of Mathematical Research Programmes (I)*

by MICHAEL HALLETT

Introduction

1 *The Development of Mathematical Theories*
(*a*) *Progress in Informal Mathematics*
(*b*) *A Criterion of Mathematical Progress*

2 *Why Did Cantor's Theory of Ordinal Numbers Constitute Mathematical Progress?*
(*a*) *Cantor's Postulation of Transfinite Ordinal Numbers*
(*b*) *Mittag–Leffler's Theorem*
(*c*) *Borel's 1895 Proof of the Heine–Borel Theorem*

3 *Conceptual Innovation and the Role of Proof in Mathematics*

4 *Does Hilbert's Criterion Reflect Mathematicians' Reactions to Theory Change?*

5 *Heuristics and Mathematical Programmes*

Conclusion

INTRODUCTION

The main aim of the methodology of scientific research programmes (henceforth *MSRP*) is to set out clear, general desiderata for distinguishing between progressive and degenerating science which are in accord (more or less) with the informed, intuitive assessments scientists make in particular cases.[1] In this paper I want to consider whether a modified form of that methodology can perform the same function with respect to mathematics. Mathematicians generally have an intuitive feeling for what constitutes

* Previous versions of this paper were read and criticised by John Bell, John Mayberry, Jerry Ravetz, John Worrall and Elie Zahar. I would like to thank them all, particularly John Worrall, for their valuable suggestions, though naturally none of them will agree with everything I say in this present version.

[1] For historical case studies, see the excellent contributions to Howson (*ed.*) [1976]. For the methodology of research programmes itself, see Lakatos [1970].

Received 21 *March* 1977

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

good mathematics (see 1 below) but find it difficult to give an adequate characterisation of the general principles underlying that feeling. Can a methodology of mathematical research programmes supply this general characterisation?

Obviously not, unless mathematical theories develop through different 'versions' in the same way as do scientific theories according to Lakatos. But this is, I believe, a historical fact. (For instance, Lakatos's 'Proofs and Refutations' traces the development of the theory of polyhedra through various versions). Hence the answer to the above question revolves around two separate issues. (1) Given a series of different versions of a developing mathematical theory, is it possible to specify conditions of acceptability (or progress or non-*adhoc*ness) of one version with respect to its predecessor similar to those given by *MSRP* for theoretical shifts in physical theories? (2) Do developing mathematical theories exhibit the kind of heuristic unity or heuristic evolution which according to *MSRP* is exhibited by programmes in physics?

(2) is, I think, less problematic than (1), and it is not too difficult to argue for a positive answer to it. Consequently, I will deal with (2) only at the end of the paper (in 5) and then only rather briefly.

The much more problematic and contentious question is (1). It might be argued that because mathematics and physical science are radically different the answer to (1) must be negative. For example, proofs play a highly important role in mathematics, but a much less significant role in physics.[1] On the other hand physics can rely on a growing fund of unproblematic statements of fact (observation reports of meter readings, viewings through telescopes *etc.*) for which there is no analogue in mathematics. According to *MSRP*, significant progress is attained only when a programme makes some novel predictions which agree with the facts. If there are no analogous mathematical facts, how can there be a methodology of *mathematical* research programmes?

Certainly these differences between mathematics and empirical science are highly important. I do not want to claim, as some philosophers have, that mathematics and science are really part of one enterprise. But after all, I am only looking for a criterion of mathematical progress *analogous* to *MSRP*'s criterion of scientific progress. These considerations only show, as one might expect, that the two criteria cannot be identical.

[1] Physics does occasionally make use of impossibility proofs (*e.g.* Rosenthal's proof of the impossibility of an ergodic gas system) and, of course, it continually makes use of deductions from theories and initial conditions to predictions. But the impossibility results are usually lifted directly from mathematics as special cases of quite general theorems (for the example of Rosenthal's proof, which was pointed out to me by Peter Clark, see Brush [1967], pp. 168–83). As for strict deduction this is only a small part of the role of mathematical proof: see below, 3.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

1 THE DEVELOPMENT OF MATHEMATICAL THEORIES

(*a*) *Progress in Informal Mathematics*

Mathematicians, historians and philosophers frequently characterise good mathematics as mathematics which is important, significant relevant or deep, and bad mathematics as that which is none of these. Now while in specific contexts it may be intuitively clear what is meant by 'relevance', 'significance', *etc.*, the methodologist's task, as I construe it, is to try to make these intuitive notions more precise, and less open to personal variation.

It is not difficult to put bounds on the notions by giving examples of, say, theories whose results are clearly relevant or important, or new theories which clearly have nothing new or interesting to say. Thus, no-one would deny that if a theory finds applications in a progressing *physical* programme then this is a very strong demonstration of its 'relevance' or 'importance'. On the other hand, it is not hard to find historical examples of theories going through periods of obvious stagnation. The theory of algebraic forms in the period leading up to 1890 is one such example.[1] Lakatos gives another in his history of the Euler conjecture. He describes a period where the search was on for a 'complete all-embracing' formula covering all know polyhedra. Complicated formulae were constructed to deal with the latest counterexample only to be refuted by a new counter-example and replaced by a new formula. But each time the new formula was constructed from the old by simply adding terms (parameters) and filling in the result from the latest counterexample. Each new and more complicated version of the conjecture does nothing more than 'save the phenomena'. As Lakatos says, such generalisations are 'cheap'; they are *ad hoc* in a way which the philosopher of science would clearly recognise.[2]

It is tempting to take some very clear-cut cases and turn them into definitions. Von Neumann apparently succumbs to this temptation in his [1947]. Weyl had earlier suggested that classical mathematics can only be given a sense when fused with physics. Comparing Hilbert's programme with Brouwer's in his [1925], Weyl asserted that the former is only a serious rival to the latter if the concepts of classical mathematics which it seeks to defend are given physical interpretation. For, he claims, even a consistent mathematics is no good unless it has some meaning.[3] Von Neumann, perhaps frustrated with the failure of Hilbert's programme to prove classical mathematics consistent, seems to follow Weyl's suggestion.

[1] See Bell [1945], pp. 421–2 and Fisher [1966–7], section II.
[2] See Lakatos [1976], pp. 79–81 (especially n. 1, p. 80) and pp. 97–8; see also p. 7 *below*.
[3] See *op. cit.* p. 541.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

Worried that much purely formal mathematics is apparently just *l'art pour l'art*, von Neumann suggests that the degeneration of a theory sets in when the theory is 'at a great distance from its empirical source' ([1947], p. 196), and that stagnation or degeneration can be halted or reversed with 'the re-injection of more or less directly empirical ideas' (*ibid.*).[1] Now while the idea of 'empirical relevance' is in some ways very attractive, it is by no means as definite as it seems, and, if insisted upon as a necessary condition for good mathematics, is far too restrictive.[2]

In the first place, it is not clear where the line is to be drawn. If a theory A is directly interpretable *via* a physical theory P, then certainly according to Weyl and von Neumann it is significant. Now assume that a theory B is used to solve problems in the 'significant' theory A. Does this make B significant, or must it find direct physical interpretation as well? Secondly, it is too narrow as a piece of advice to working mathematicians. Indeed the dictum 'work only on those theories which are physically interpretable' would actually be counterproductive: it is impossible to say in advance whether or not a theory will be physically interpretable. Who would have said at the time of their proposal that differential geometry or the theory of Hilbert space would turn out to have applications in the theory of gravitation and quantum mechanics? Surely such theories were judged *not* according to their physical potential, but according to their qualities as *mathematical* theories. Thirdly, following on from this, the proposal is too narrow because it does not capture the actual history of mathematics. For example, the theory of algebraic forms which had made little headway up to 1890 was revived by Hilbert's solution of a fundamental problem *via* a purely existential proof which very quickly led to a much more general theory of algebraic invariants.[3] And again, the theory of polyhedra was revived by Poincaré's abstract proof which led to the modern theory of combinatorial topology.[4] Neither of these historically clear examples of revival and progress were the result of 'empirical injections'.

Similar problems arise with the constructivist thesis. Constructivism may be more definite than the 'empirical relevance' thesis, since (at least according to some versions of the doctrine) it is possible to give in advance specific indications as to what is constructive and what is not.[5] But it

[1] *Cf.* Weyl [1949], p. 235.

[2] It should be pointed out that von Neumann probably did not intend 'empirical relevance' to be a rigid acceptability criterion, but rather a condition to be applied when theories become too 'baroque', or fall under the influence of mathematicians who do not have 'an exceptionally well-developed taste'.

[3] See Fisher [1966–7], sections II and III.

[4] In Lakatos's list of formulae (1)–(7) on pp. 80–1 of his [1976], it is (5) which became the standard 'Euler formula'; see the discussion in *ibid.* pp. 97–8. It was (5) which was proved by Poincaré (though for arbitrary dimensions). See also Bell [1945], pp. 459–60.

[5] See for example the Preface and chapter 1 of Bishop [1967].

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

would appear that constructive mathematics is already too weak for some parts of physics (certainly if physics makes extensive use of topology, for example in cosmology[1]). And certainly it is not sufficient for explaining why large parts of classical mathematics were accepted, even those parts which can be given a constructivist interpretation.

Unfortunately, then, neither of these attempts to characterise relevance as 'empirical relevance' or 'numerical relevance' (*i.e.* constructivism: see the Preface to Bishop [1967]) is adequate. What is required is a more general characterisation of mathematical significance which perhaps includes these as special cases, but which also takes in such examples as Poincaré's proof of the generalised Euler conjecture (or rather *one* of the generalisations of Euler's conjecture), and at the same time excludes the *ad hoc* theoretical modifications which preceded it. But just what is it about Poincaré's theorem and its proof which makes it 'good' mathematics; why does it stand out from the previous 'cheap generalisations'? The answer, I suggest, is connected with the fact that Poincaré's result does much more than just solve the problem it was originally constructed to solve. It turned out that Poincaré's proof and the framework (definitions *etc.*) that it employs can be applied not just to geometric objects (polyhedra), but to analytic objects (manifolds) as well. Combinatorial topology began with the imposition of geometric (polyhedral) form on analytic manifolds. Once this is done, the proof of the Euler formula becomes a direct combinatorial method of calculating the Betti numbers of the manifold, and thus of classifying it.[2] This is a far cry from the previous 'cheap generalisations'. Not only did Poincaré's result solve a problem in 'solid geometry' but it solved a great many more problems besides.[3] The same is true of the Hilbert result which saved the theory of algebraic forms from degeneration. According to Fisher the Hilbert theorem was originally intended as a solution to a specific problem in the theory of algebraic forms, but turned out to be applicable to a great many more algebraic problems. So much so that the theorem eventually became one of the cornerstones of modern algebra.[4] Similar remarks apply to the Heine–Borel theorem, to take an example from analysis. The Heine–Borel property (the existence of a finite sub-covering) was originally established for countable coverings of a closed interval on the real line.[5] But the property subsequently turned

[1] According to Bishop: 'Very little is left of general topology after that vehicle of classical mathematics has been taken apart and reassembled constructively. With some regret, plus a large measure of relief, we see this flamboyant engine collapse to constructive size' (*op. cit.*, p. 63).

[2] For further discussion see section **5**.

[3] For a brief indication of how Poincaré's theory was applied by Schönflies and Brouwer, see Wilder [1953], pp. 433–4, or Alexandroff [1932], pp. 11–12, 30–1.

[4] See Fisher [1966–7], sections II and III.

[5] See pp. 20–5 *below*.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

out to be satisfied in so many other cases, that eventually the whole class of compact spaces was isolated and studied as a separate category.

In all these cases, the new results were applied in unexpected ways, to solve problems in circumstances they were not originally designed to cover. It is *relatively* easy to construct theories or design theorems for narrowly prescribed purposes; it is much more difficult to construct theories or theorems which subsequently turn out to have mathematical applications elsewhere. These cases deserve special merit. For it is through these that the various back connections and similarities of mathematics are discovered.[1]

This idea is by no means new. One can find hints of it in Gödel [1964], in Hadamard [1949], in Weyl [1951], in Wilder [1953] and more recently in Dieudonné [1975]. But it was most strikingly put by Hilbert:

> The final test of every new mathematical theory is its success in answering pre-existent questions that the theory was not designed to answer. By their fruits ye shall know them—that applies also to theories.[2]

I take this remark of Hilbert's as my starting point in what follows. I will try to show how with some elaborations this comment can be turned into a criterion of progress for mathematical theories very like *MSRP*'s criterion of progress for empirical programmes.

(*b*) *A Criterion of Mathematical Progress*

As I have pointed out, there is no clear analogue of empirical data in mathematics. The first important feature of Hilbert's comment is that it suggests a quite natural way around this difficulty. Hilbert suggests that a mathematical theory gains its support *via* the problems it solves, and thus that problem solutions constitute an analogue for correct predictions of empirical data. Empirical theories are set up to capture truths about the world, *i.e.* facts; hence appraisals of empirical theories are largely based on comparing their relationship to facts. Mathematical theories are set up to solve, through their consequences, mathematical problems, so it is quite natural as Hilbert suggests that their merit be judged on the basis of their problem-solving ability. If it is possible to give reasonably precise systems of appraisal based on this substitute for empirical data, then the absence of mathematical 'facts' poses no serious problem for the methodologist of mathematics. Let us then see if it *is* possible.

Methodologies which accept that it is the facts if anything which lend a scientific theory support invariably stress that real support cannot be

[1] For the importance of establishing such 'back connections' see Wilder [1953], especially his discussion of the evolution of the concept of curve, and the related diagram on p. 431.

[2] Hilbert [1926], p. 384.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

achieved too easily. Hilbert's conditions likewise demand that not every problem solved by a theory should count in its favour. To this end Hilbert imposes two conditions. He demands that to demonstrate its worth a theory must solve a problem that pre-dated it, and that the theory must not have been designed to do this. As I see it, Hilbert's first condition is intended as a minimal guarantee that the theory solves an 'important' problem, while the second condition is intended as a guarantee that the theory does not solve this problem in an *ad hoc* way. I will come back to the question of 'importance' below; for the moment I want to consider the question of *ad hoc*-ness.

Hilbert's formulation of his second condition is rather ambiguous. Reference to a theory's 'design' might be taken as a reference to the intentions of its creator. If this is the case then it seems to me that Hilbert's condition misses its mark. For example, Cantor certainly designed his theory of transfinite numbers with the *intention* of solving the continuum problem.[1] Now in fact the theory did not solve this problem. But I fail to see why if it *had* Cantor's intention ought to exclude the solution of this problem from being a 'fruit' of the new theory. However, if one interprets Hilbert's intention as simply to guard against a theory having built in success (and this will be my interpretation in what follows) then it is not difficult to give Hilbert's condition a precise non-psychologistic interpretation.

Problems are solved by specific statements entailed by theories. If one knows in advance that a particular statement or set of statements will solve a given problem then there is the possibility in constructing a new theory of building the statements into the theory, thus guaranteeing its 'success' in solving the problem. In other words the set of problem-solving statements (or the problem solution for short) has been used in the construction of the theory. Now one doesn't want to say that such a move is by itself bad; but clearly as far as the solution of the problem in question is concerned the new theory has taught us nothing. Consequently, this particular solution shouldn't count as a genuine success for the theory. Its genuine successes, if any, must be among the other problems it solves.

Note that what makes many of the revised polyhedral theories discussed by Lakatos *ad hoc* is that their only 'success' is built-in success. The method of exception barring discussed on pages 24–30 of Lakatos's [1976] creates rather extreme examples of theories of this kind. For instance, in Lakatos's dialogue pupil Alpha proposes (p. 15) a twin tetrahedron (two tetrahedra joined at one vertex, or alternatively along one edge, for which $V-E+F=3$) as a counterexample to Euler's conjecture that $V-E+F=2$

[1] See **2**(*a*) *below*.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

for all polyhedra, V, E and F being the number of the polyhedron's vertices, edges and faces respectively. Applying Lhuilier's and Gergonne's method of exception barring (Lakatos [1976], p. 26, n. 2; the method is advocated in the dialogue by pupil Beta) to resolve this conflict yields the new conjecture: $V-E+F = 2$ for all polyhedra *except* twin tetrahedra. If challenged to devise a theory which also covers twin tetrahedra, the exception barrier would presumably reply with: $V-E+F = 2$ for all polyhedra except twin tetrahedra, for which $V-E+F = 3$. Both these conjectures solve the *given* problem by building in the 'facts' as face-saving clauses. But this clearly exhausts their problem solving capacity: for polyhedra other than twin-tetrahedra they refer back to Euler's conjecture. Such exception-barring conjectures teach us nothing new: they simply *rearrange* existing knowledge so as to avoid refutations.[1]

Once the nature of this *ad hoc*ness is recognised it is not difficult to specify a condition which guards against it. Indeed, this problem has been met with in the methodology of empirical science and tackled by Zahar and Worrall.[2] Thus if a theory solves a problem and the statement solving the problem was not used in the construction of the theory, then the solution is not *ad hoc*. To refer to Cantor's continuum problem again, although Cantor conjectured and firmly believed the statement 'only two infinite powers are represented in the linear continuum' this statement was not used in the construction of his theory of transfinite numbers. Thus, if the theory had solved the continuum problem (as he intended!) this solution would have been non-*ad hoc*, and should certainly have been counted as a genuine success.

Hilbert's first condition also has its difficulties, but these are harder to overcome. As I said, I think Hilbert's condition is intended as a minimal condition on the importance of a theory. But importance is a tricky issue. Clearly any new theory since it will introduce new objects or new concepts will create a host of new problems concerning itself. It may happen that the new theory is powerful enough to solve some of these problems. But although this might demonstrate the ingenuity of the creator of the theory or of its devotees, this does not show by itself that the theory is a significant contribution to mathematics. Thus, Hilbert demands that whatever else

[1] Other examples are mentioned on p. 3 *above*, text to n. 2. These examples are by no means as trivial as the exception barring examples. For, although the new conjectures are apparently created by adding and adjusting parameters to deal with specific refutations, they are nonetheless quite general. Consequently, they have explanatory and problem-solving capacity beyond that trivially built into them. However, only in the case of conjecture (5) was this capacity used (see *above*, p. 4, footnote 4, and p. 5). The rest solved only those problems whose solutions were built into them.

[2] See Zahar [1973], pp. 101–4 or, for a fuller account, Worrall [1978]. See also pp. 9–10 *below*.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

it does a good theory must solve a *pre-existent* problem. For this guarantees that the theory 'goes beyond' its own subject matter. The condition in effect simply makes explicit the general recognition that one of the characteristic features of mathematical progress is the emergence of theories which establish fundamental connections between apparently diverse fields.[1] Good theories are thus theories which help to establish such connections.

But while the intention of Hilbert's condition is clear, its formulation is, I think, too rigid. For, in the first place what if a theory turns out to solve an important problem not of its own creation which *post*-dates it? Surely the theory will still have demonstrated its importance even though it doesn't satisfy Hilbert's condition.[2] However, this difficulty is avoided by dropping any reference to temporality and simply demanding that the theory solve some problem not of its own creation, regardless of whether this problem pre-dates the theory or is discovered later.

Secondly, on a slightly different tack, Hilbert's formulation implies that a theory *via* its consequences must *completely* solve a problem, that is, it must supply all the elements which go to make it up. But this again is too strong. Surely, if a theory only supplies *part* of the solution to a problem, the other parts coming from elsewhere, then it still deserves credit for its contribution (providing it was not achieved in an *ad hoc* way). This difficulty is also easily avoided by slightly relaxing Hilbert's condition. Instead of demanding that a theory solves a problem, we need only demand that it is *used* in the solution of a problem.

However, the modified condition is still only a minimal condition, a necessary but not sufficient condition on a theory's importance, since a theory might solve a quite trivial problem outside its own domain.

But I do not regard this situation as necessarily distressing. In the first place, if a theory satisfies the Hilbert conditions (as I have modified them) then this at least *does* show that the theory is potentially a valuable tool of mathematical discovery. Moreover, in historical investigations of theories it may be possible to discover *historical* evidence for the importance of a problem, particularly if mathematical theories are indeed arranged as programmes with long term research aims.[3]

The two modified Hilbert conditions form the basis of a criterion of progress for series of theories which is now (partly by design) quite similar to *MSRP*'s. I propose that the setting up of a new mathematical theory

[1] See p. 6 *above*, and also Wilder [1953], particularly p. 444.

[2] I discuss an example of this *below*, namely where the theory of transfinite numbers was used in solving a problem of Borel's which post-dated it. See section **2**(*c*).

[3] I give an example of a clearly identifiable important problem below, namely the problem of finding analytic expressions to represent analytic functions: see **2**(*b*).

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

T'_m constitutes progress with respect to its predecessor T_m if T'_m is used in the solution of at least one problem P which T_m did not solve,[1] provided that P is not of T'_m's own making and that the statement solving P was not used in the construction of T'_m. (In what follows I will call this criterion 'Hilbert's criterion' for ease of reference.) The similarity with *MSRP* is quite clear. *MSRP* states that a consequence of T' provides support for T' providing it is true and was not used in the construction of T'. (This last condition is that of Zahar and Worrall: see the references in n. 2, p. 8 *above*.) Hilbert's criterion states that a consequence of T' provides support for T' if it is used to solve an 'important' problem and likewise was not used in the construction of T'. Thus both criteria state similar desiderata for progress once theoretical novelty has been achieved.

Can Hilbert's criterion be applied to the actual history of mathematics? By presenting a detailed case-study I want to show that it can. I apologise in advance for the length of the study and for burdening the reader with so much detail. In defence, I might say that since the methodology of mathematics is still in its infancy detailed case studies are certainly needed. Moreover, the case history I use (Cantor's theory of transfinite numbers) raises various difficulties concerning Hilbert's criterion which will give me the opportunity to make the criterion clearer.

2 WHY DID CANTOR'S THEORY OF ORDINAL NUMBERS CONSTITUTE MATHEMATICAL PROGRESS?

(*a*) *Cantor's Postulation of Transfinite Ordinal Numbers*

Cantor first introduced the transfinite ordinals in his [1880], the second in a series of six papers with the title 'On infinite linear point-manifolds'.[2] Their introduction marked the beginning of a two-fold attack on the general *problem of powers*, and in particular *the problem of the power of the continuum*.

The problem of powers in its general form was that of finding a calculus of power or absolute size adequate for discussing and describing the sizes of arbitrary infinite sets of points. By 1878 Cantor had clearly adopted the principle of using one-one correspondences to measure the *relative* size of sets (a principle which Bolzano and others had earlier rejected); and he had already implicitly adopted the idea that infinite sets have an *absolute size* which he called its power.[3] But three questions now arise. How can

[1] That is, did not solve P up to the time of T'_m's introduction. It may be shown *subsequently* that T_m can solve P; but see *below*, **2**(*c*) and particularly **3**.

[2] The six papers are his [1879], [1880], [1882], [1883*a*], [1883*b*] and [1884].

[3] The term power was originally used in the *relative* sense. Thus Cantor said that 'A has the same power as B', or 'A has smaller power than B' if there is a one-one correspondence between A and B, or between A and a subset of B respectively. (See Cantor [1878], p. 119.) But then he quickly slips into using the term in an *absolute* sense, thus hypostatizing it. He talks, for instance, of 'the smallest amongst infinite powers' (*op. cit.*, p. 120).

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

these infinite powers be represented? Is it possible to set up a scale of size and an effective arithmetic for infinite power, just as we have a scale and an arithmetic for finite powers? (Cantor had already identified the powers of finite sets with the natural numbers: see his [1878], p. 119.) And how many powers are there?

Cantor's papers [1874] and [1878] provided partial answers to the last of these questions. In his [1878] Cantor showed that *all* Euclidean intervals of whatever dimension (even of countable dimensions) have the same power as any linear interval. This was a surprising result,[1] but it nevertheless simplified the search for different powers since it showed that all powers of Euclidean point sets of whatever dimension must be already represented in the real line. Cantor, of course, knew that there were at least two powers amongst infinite linear sets, since he had shown in his [1874] that a linear interval has a 'greater power' than the sequence of integers. Indeed he also showed that the algebraic numbers, and thus the rational numbers, have the same power as the sequence of integers. This possibly suggested to Cantor that there are very 'few' infinite powers in Euclidean space. In any case, in his [1878] he put forward the strongest possible version of this thesis, for he conjectured that there are just *two*. He conjectured that any infinite linear set must either have the power of the natural numbers or the power of the whole line. This was the first version of Cantor's *continuum hypothesis*.

The desire to prove this conjecture was the main creative spur to Cantor's work from this time on, and the main reason for tackling the general problem of powers.[2] Nevertheless, Cantor admitted the possibility that it might be false. Despite his *belief* in its truth, his aim as a mathematician was to *decide* the issue, first by looking for a proof, and then if a proof was not forthcoming, by looking for a disproof. From Cantor's viewpoint in 1878 there were two possible ways of approaching the continuum problem. One was to try to prove (or disprove) directly that any infinite linear point set is either countable or has the power of the continuum. The second was *via* the general problem of powers, by defining an arithmetical scale of infinite

[1] See the correspondence with Dedekind for the period May 1877 to January 1879 in Noether and Cavaillès (*eds.*) [1937], pp. 21–50.

[2] In his famous report on set theory of 1899 Schönflies remarked of the continuum problem that

> . . . we have this to thank for a large part of his [*i.e.* Cantor's] set-theoretic investigations. (Schönflies [1899], p. 49).

Certainly, as Schönflies later remarked, Cantor struggled with the problem on and off throughout his life, and put his 'highest ability' into trying to solve it: see Schönflies [1927], p. 16. The effects of this struggle are dramatically presented in the excerpts from Cantor's letters to Mittag–Leffler, quoted in Schönflies [1927], and Schönflies's comments on them.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

powers, and showing at what place in the scale the power of the continuum is represented. If it is represented by the power in second place in the scale the continuum hypothesis must be correct; if not, then it must be incorrect. Cantor tried both lines of attack, and both involved the transfinite ordinals, though in different ways. The second line of attack led to the modern theory of cardinal numbers in which the ordinal numbers are used to construct the scale of alephs. But it is the first line of attack which concerns us more here since it was for this that the ordinal numbers were first introduced.

Cantor's direct approach to solving the continuum problem was to set up a further method of classifying point-sets alongside the classification by one-one correspondence and to try to obtain information about the latter *via* the former. His further method of classification was by studying certain decomposition properties using the idea of *point-set derivation*. In his remarkable [1872][1] Cantor introduces the idea of the derived set of a point set P denoted by $P^{(1)}$ and containing exactly the 'limit points' (*i.e.* accumulation points) of P. If P is bounded and infinite then according to Weierstrass's theorem $P^{(1)}$ will be non-empty. Correspondingly, if $P^{(1)}$ is bounded and infinite it too will have a non-empty derived set $(P^{(1)})^{(1)}$, which Cantor denoted by $P^{(2)}$. Cantor extended this to $P^{(n)}$ for any finite n. This is as far as Cantor went at this stage: the central theorem of his [1872] uses only sets P for which $P^{(n)} = \varnothing$ for some n.[2] However, Cantor recognised even this early that there is a much more general heuristic method here, namely: to discover properties of P by investigating the sequence of derived sets.[3] There are clearly point sets for which $P^{(n)} \neq \varnothing$ for any n, for example an interval, or the rationals in an interval. Thus, in order to sub-classify these 'second-species' sets ('first species' sets being those with $P^{(n)} = \varnothing$ for some n)[4] Cantor required some way of analysing

[1] The central topic of this paper is the problem of the uniqueness of trigonometric series expansions of functions. (The history of this problem is described in both Hawkins [1970] and Dauben [1971].) But it was in this paper that Cantor first began to use the idea of arbitrary infinite *sets* of points, and where he gives his classical definition of real numbers as Cauchy sequences of rationals.

[2] The theorem is:

> If $f(x)$ is a function which is represented by a trigonometric series at all points of the set $[0, 2\pi] - P$, where $P \subseteq [0, 2\pi]$ such that $P^{(n)} = \varnothing$ for some n, then the representation is unique.

See Cantor [1872], pp. 99–101, or Hawkins [1970], pp. 21–28 or Dauben [1971]

[3] In his [1872] Cantor wrote:

> If a point-set is given in a finite interval, then in general a second point-set is given with it, and with this second a third, and so on [Cantor means $P^{(1)}$, $P^{(2)}$, *etc.*]. *These point-sets are essential in order to be able to understand the nature of the first point-set* (p. 97; my italics).

[4] Cantor introduced the terms 'first species' and 'second species' in his [1879], p. 140.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

the derivatives 'beyond' the $P^{(n)}$. To this end he introduced in his [1880] new 'symbols of infinity', which were *contextually* defined *via* the derivation process. Thus, $P^{(\infty)} = \bigcap_{n=1}^{\infty} P^{(n)}$, $P^{(\infty+1)} = (P^{(\infty)})^{(1)}$ and so on.[1]

Cantor's hope was that this more general idea of derived set would provide an exhaustive classification of point-sets discriminating enough to give information about the power of any set by reducing it to components whose power is known or easily calculable. The introduction and use of the symbols in his [1880] and [1883*a*] constituted the first stage in the creation of Cantor's new theory. But for both logical and mathematical reasons Cantor was forced to shift to a second stage in which the 'symbols' were reintroduced as transfinite ordinal *numbers*.

In the first place, Cantor had already begun to use an arithmetic of symbols, combining them both with themselves and with the natural numbers. To take an example from his [1880], pp. 357–8, he there introduces $P^{(\infty^2)}$, $P^{(\infty^\infty)}$, and more generally $P^{(n_0\infty^\nu + n_1\infty^{\nu-1} + \ldots + n_\nu)}$ for natural numbers $\nu, n_0, \ldots n_\nu$. This presupposes that the 'symbols' and the natural numbers are objects of the same kind, subject to the same arithmetical operations and obeying generalised arithmetical rules. But this meant that the 'symbols' had to be presented as *numbers* and the generalised arithmetic spelt out. Secondly, some classification of the symbols themselves was required. The attempt to establish decomposition theorems for arbitrary sets *via* derivation presupposed an investigation of such questions as how many new symbols are required to cover all cases. This meant that Cantor could no longer rely on *ad hoc* introduction of 'symbols', but required general principles for their introduction and classification. Thus in his [1883*b*] Cantor re-introduced the symbols in a much more general setting as transfinite ordinal numbers postulated to exist in their own right away from the context of point set derivation.[2] These numbers extend the natural number sequence and the extended sequence of ordinal numbers then satisfies a general ordinal arithmetic. Cantor now had a much more versatile point-set theory with its apparatus of recursive definitions, transfinite induction and so on, with which to tackle the continuum problem, and in particular the problem of decomposing point sets by derivation.[3]

[1] See Cantor [1880], pp. 357–8.

[2] Cantor in effect postulated the existence of the transfinite ordinal numbers *via* his two so-called generating principles ([1883*b*], p. 547 and pp. 576–8). Although the problem was to worry later set theorists a good deal, Cantor at this stage made no attempt to define the numbers in terms of the background set theory.

[3] Although, as I've explained, Cantor introduced the ordinals in two stages, when referring to 'Cantor's new theory' I shall not distinguish between them.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

Cantor's new theory is now almost universally regarded as good mathematics, and a substantial improvement on its predecessor, point-set theory. I argue that Hilbert's criterion can explain this judgment because the theory was used to solve 'important' problems in a non-*ad hoc* way. I will describe two successful applications of Cantor's new theory in solving important problems: the first application is in the proof of Mittag–Leffler's theorem; the second is Borel's first proof of the Heine–Borel theorem. Later I will indicate that these two applications were of some importance in changing mathematicians' opinions about Cantor's theory. This will suggest that whether they do so consciously or unconsciously mathematicians themselves apply something like Hilbert's criterion in appraising new theories.

(*b*) *Mittag–Leffler's Theorem*

In 1884 Mittag–Leffler made substantial use of Cantor's new theory in extending certain fundamental theorems due to himself and Weierstrass concerning the representation of analytic functions.

The representation problem grew out of Weierstrass's definition of analytic functions in terms of power-series. Each power-series is taken to represent an analytic function inside its radius of convergence and Weierstrass turned this into a general definition of analytic functions using the notion of analytic continuation. In fact, he defined an analytic function as a 'monogenous system of power-series', meaning by this a class or set of power-series, the *elements*, such that between any two points a_1 and a_2 inside circles of convergence of any two elements it is possible to get from a_1 to a_2 by analytic continuation. The function so defined is then said to be *regular* in the interior of the union of the discs of convergence of the elements.

But this notion of a function is rather unwieldy. In the first place a system may well contain uncountably many elements.[1] But more importantly it does not give a global picture of the behaviour of the function. In particular, for any given system of power-series it will not generally be clear how the function defined by this system behaves in the neighbourhood of singularities, or even where these singularities are.[2] Thus, Weierstrass in 1876 raised the following general problem: given a single-valued analytic function, is it always possible to find a single *analytic*

[1] Though in his [1888] Poincaré showed that a Weierstrassian analytic function can always be defined by a countable system of power-series simply by considering a countable dense set of regular points and taking the power-series based on them.

[2] For an excellent critical discussion of Weierstrass's conception of analytic functions, see Borel [1898], chapter 4.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

expression (which in general will be an infinite series[1]) which represents the function in its domain of regularity and which actually exhibits the singularities of the function by virtue of its form?

As Weierstrass points out (Weierstrass [1876], pp. 81–3) the problem is just that of finding a common form for functions regular in a given domain. The problem had been solved in certain simple cases. For example, if the point c is the only singularity in the finite plane, then all functions with this as the only finite singularity will be representable by some rational or transcendental function of $1/(z-c)$ (depending on whether c is a *pole*, or an *essential singularity*), plus an *entire function* (*i.e.* a function regular in the whole finite plane). More generally, finite sums and products of such functions can be constructed to cover the case where there are a finite number of singularities in the finite plane.[2] Weierstrass suggested taking these known cases as a pointer and proposed solving the general problem step by step, using similar methods applied to more and more complicated sets of singularities.[3]

Weierstrass believed he had solved the problem where the set of singularities is made up of a finite number of essential singularities, and arbitrarily many, even infinitely many poles.[4] But in fact, as he later acknowledged,[5] he had really only dealt with the case where there are finitely many poles, in other words when *all* the singularities are *isolated* (*i.e.* can be surrounded by a regular disc). The first step in dealing with the *infinite* case was taken by Mittag–Leffler in 1877.

Mittag–Leffler succeeded first in solving the problem in the case where the singularities form an infinite sequence of poles tending to a limit at ∞, which is then a non-isolated essential singularity. He showed:

(1) Let $a_1, \ldots, a_n, \ldots$ be a sequence of distinct complex points, with

$$\lim_{n\to\infty} a_n = \infty$$

and let $g_n\left(\frac{1}{z-a_n}\right)$ be a corresponding sequence of rational functions, each with a single pole at a_n, and no other singularities. Then it is possible to construct a single-valued analytic function $F(z)$ with poles

[1] An analytic expression will be formed by putting together an infinite series of expressions, each formed from the variable z by a finite number of instances of the addition, subtraction, multiplication and quotient operations.

[2] See Weierstrass [1876], pp. 79–84. The problem of taking the product of finitely many transcendental functions (*i.e.* infinite series) led Weierstrass to his famous *factor theorem*, *i.e.* forming expressions for functions with infinitely many prescribed *zeros*. (See *op. cit.*, pp. 85–6, or *e.g.* Hille [1959], pp. 226–9). This is necessary because one or more of the transcendental functions might have infinitely many zeros.

[3] See Weierstrass [1876], p. 81.

[4] *Ibid.*, p. 85.

[5] See Weierstrass [1880], p. 199.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

$a_1, \ldots, a_n \ldots$, and an essential singularity at ∞, such that $F(z)$ has the same principal parts as g_n at a_n. The simplest such F is given by the expression

$$F(z) = \sum_{n=1}^{\infty} \left[g_n\left(\frac{1}{z-a_n}\right) - \gamma_n(z) \right]$$

where the γ_n are suitably chosen polynomials in z and are actually constructed from the g_n. There are more general functions than F, having the required properties, but any F' having these properties will be of the form

$$F'(z) = F(z) + G(z)$$

where G is an entire function.[1]

This solves Weierstrass's problem for this case, and moreover by a fairly simple and direct extension of the methods used in the *finite* case. The complications creep in only in choosing the $\gamma_n(z)$ in such a way that the series

$$\sum_{n=1}^{\infty} [g_n - \gamma_n]$$

converges uniformly in every regular region.[2]

But now, as Weierstrass showed in his [1880], it is possible to generalise the results of his [1876] to the case where there are *finitely* many essential singularities, and *infinitely* many poles. What Weierstrass had neglected in his [1876] was the possibility that one of the essential singularities is the limit of some sequence of the poles. In other words, he explicitly assumed that the essential singularities (indeed *all* the singularities) are *isolated.* Mittag–Leffler's theorem shows how to get round this problem. First, Weierstrass showed that we do not need to assume that the limit of the $a_1, \ldots, a_n, \ldots$ is ∞: the result will hold where $\lim_{n\to\infty} a_n = c$ for any c (which is now an essential singularity) in the extended plane. (Weierstrass [1880], pp. 195–6.) Secondly, Weierstrass extended the result to cover a finite set of essential singularities $c_1, \ldots, c_n$ where any or all of the c_i's can be the limit of a sequence of poles (*ibid.*, pp. 196–9).

Thus all functions whose singularities are given by the points $c_1, \ldots, c_n$, and the points a_{ij} as i ranges over 1, ..., n and j ranges over 1, 2, ..., m, ..., and $\lim_{j\to\infty} a_{ij} = c_i$ for each i, will take the form:

$$F'(z) = F(z) + G(z).$$

[1] See Osgood [1928], pp. 565–6, or almost any general textbook on function theory. (1) is usually referred to in textbooks as 'Mittag–Leffler's theorem'; the generalisations mentioned below are only given in the more advanced textbooks. The proof standardly given for (1) is Weierstrass's from his [1880]. For a very clear rendering, see Hurwitz and Courant [1929].

[2] See for instance Hille's remarks on pp. 218–19 of his [1959].

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

$F(z)$ is the function constructed by Mittag–Leffler's method out of the g_{ij} where each g_{ij} has the sole singularity a_{ij}; and $G(z)$ is any function whose only singularities are among $c_1, \ldots, c_n$. The problem of representing a particular $F'(z)$ with these given singularities then reduces to finding a particular $G(z)$ with $F'(z) = F(z)+G(z)$.

So far, Weierstrass and Mittag–Leffler had not required any special techniques of point-set theory. Their results were achieved by the ingenious use of analytic methods. But it is clear that if Weierstrass's programme was to be carried any further some detailed analysis of the structure of infinite point-sets would be required. What was needed especially, as Weierstrass's difficulty of 1876 shows, was an analysis of the behaviour, distribution and concentration of the limit points of infinite sets. This was just what Cantor's theory of 1872 began to develop, and what his new theory of 1880 and 1883 sought to extend. Indeed, the theory of point-set derivation is precisely the theory of the distribution or concentration of point-sets.[1] Hence it is not surprising that after 1880 having exhausted the possibilities of purely analytic techniques Mittag–Leffler began to combine these techniques with Cantor's theory in his attempts to extend the representation theorem to cover more general sets of singularities. Indeed Mittag–Leffler's progress with the problem from 1880 on mirrors almost exactly the various successive stages of generalisation which Cantor's theory passed through.

Mittag–Leffler's first step (in his [1882]) was to use Cantor's 1872 theory of first-species sets to generalise Weierstrass's 1880 result. This was a natural step to take since Weierstrass's result concerned a particular kind of first-species set, namely those P for which $P^{(1)}$ is finite. Mittag–Leffler[2] wanted to extend this to any P for which $P^{(n)}$ is finite for some

[1] In his [1903] Russell remarks:

> Popularly speaking, the first derivative consists of all points in whose neighbourhood an infinite number of terms of the collection are heaped up; and subsequent derivatives give, as it were, different degrees of concentration in any neighbourhood (p. 324).

He goes on:

> Thus, it is easy to see why derivatives are relevant to continuity; to be continuous, a collection must be as concentrated as possible in every neighbourhood containing any terms of the collection (*ibid.*).

This gives some idea of why Cantor was using derivatives to study continua.

[2] In this respect Mittag–Leffler was following the path marked out by Cantor in 1872 in his work on trigonometric series, and later followed by Dini in his work on integration. See Hawkins [1970], chapters 2 and 3. The extension is quite natural. For in many respects infinite sets with only *finitely* many limit points behave like finite sets, particularly with regard to their concentration in intervals. Likewise, infinite sets which have only a finite number of nth order limit points are also good generalisations of finite sets, for, like finite sets, they are very thinly distributed (*e.g.* they are nowhere dense).

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

natural n.[1] But as Mittag–Leffler acknowledged (*loc. cit.*, p. 941) this was surely not the most general theorem possible. Cantor had claimed in his [1880] that there are many *second-species* sets which are countable. So presumably if one could understand more about the way the derivatives of such sets behave then surely it must be possible to solve the representation problem for some of these. This, however, depended on Cantor's theory of classification of second species sets and this was not yet very far advanced, at least in its published state. The breakthrough came in Cantor's [1883*a*] and [1883*b*] with the new theory of transfinite numbers and transfinite derivation. It was this theory which Mittag–Leffler took up and used in his [1884*a*].

Mittag–Leffler's first fundamental result in his [1884*a*] was the theorem:

(2) Let Q be an isolated point-set (*i.e.* $Q \cap Q^{(1)} = \varnothing$). Then it is possible to construct an analytic expression $F(z)$ such that the singularities of F are just the points of $Q \cup Q^{(1)}$.[2]

The proof of (2) depended crucially on Cantor's [1883*a*] result that *every isolated set is countable* (*loc. cit.*, p. 158). (The proof uses Cantor's [1882] theorem that any infinite set of n-dimensional spheres or intervals which have at most boundary points in common must be countable.) Given this, Mittag–Leffler could assume that $Q = \{a_1, \ldots, a_n, \ldots\}$ and that there is a countable sequence g_n of rational or transcendental functions each having just the singularity a_n. (This follows because, by definition, each point of Q can be surrounded by a disc of regularity.) This done, the analytic expression $F(z)$ is constructed in a way similar to the construction of $F(z)$ in (1).[3] Then, since $F(z)$ has singularities at $a_1, \ldots, a_n, \ldots$ and any limit of singularities is itself a singularity, then $F(z)$ has singularities at all points of $Q \cup Q^{(1)}$.

But now, as Cantor had observed, for any P, $P^{(1)}-P^{(2)}$, $P^{(2)}-P^{(3)}$ and so on must be isolated; indeed $P^{(\alpha)}-P^{(\alpha+1)}$ must be isolated for any number or 'symbol of infinity' α. Moreover, if $P^{(1)} \subseteq P$, as must be the case if P is the set of singularities of a function, then $P-P^{(1)}$ is isolated as well.[4] This combined with (2) opened the way for Mittag–Leffler's second fundamental result.

[1] See Mittag–Leffler [1882], p. 938–40.

[2] See *loc. cit.* pp. 8 and 22–3. The definition of an isolated set as one which does not intersect its first derivative is given in Cantor [1883*a*].

[3] See, *e.g.* Osgood [1928], pp. 569–74. (2) is sometimes referred to as the 'Generalised Mittag–Leffler Theorem'. It is interesting to note that not only does the proof depend on Cantor's *theorem* that *every isolated set is countable*, but also on the *proof* of the latter.

[4] These results of Cantor not only enabled Mittag–Leffler to prove and extend (2), but they also served to *explain* his previous theorems. For example, in his [1882] in dealing with first-species sets Mittag–Leffler had *implicitly assumed* that each of $P-P^{(1)}$, $P^{(1)}-P^{(2)}, \ldots, P^{(n-1)}-P^{(n)}$ (P being of the nth kind) is countable (see *loc. cit.*, pp. 939–40). Cantor's [1883*a*] justifies this assumption.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

(2′) Let F' be a single-valued analytic function whose singularities form a set P. Assume that $Q = P - P^{(1)} \neq \varnothing$, then we can form an analytic expression F such that

$$F'(z) = F(z) + G_1(z)$$

where the singularities of F are just the points of $Q \cup Q^{(1)}$ and G_1 is a single-valued analytic function whose singularities form a set P_1 with $P_1 \subseteq P^{(1)}$. (*i.e.* G_1 is regular in Q).[1]

F is constructed as in the proof of (2), since Q must be an isolated set.

The importance of (2′) is that it shows that the representation problem for $F'(z)$ in (2′) can be reduced to that for $G_1(z)$ which has a simpler set of singularities, namely $P_1 \subseteq P^{(1)}$. But given that $P^{(1)}$ can be broken down into $(P^{(1)} - P^{(2)}) \cup P^{(2)}$ where $P^{(1)} - P^{(2)}$ is an isolated set there is the possibility of applying theorem (2′) to G_1. Thus, if $P^{(1)} - P^{(2)} \neq \varnothing$, $F'(z)$ can be represented by $F(z)$, some $F_1(z)$ constructed for $G_1(z)$ and some new $G(z)$ whose singularities are made up from the much simpler set $P^{(2)}$. *But this process can be carried on indefinitely.* Indeed it can be iterated up to any ordinal number from Cantor's first or second number-classes (*i.e.* any finite or countable ordinal), *giving a countable sequence of analytic expressions* $F_\beta(z)$.[2]

Thus Mittag–Leffler proved:

(3) Let $F'(z)$ be a single-valued analytic function whose singularities form the set P. Then if α is a finite or countable ordinal, there is an analytic expression $F(z)$, and a single-valued analytic function $G(z)$ whose singularities all lie in $P^{(\alpha)}$, such that

$$F'(z) = F(z) + G(z).$$[3]

The $F(z)$ in this case will be constructed from the *countably* many $F_\beta(z)$ constructed at each stage of the process.

This result (3) combined with Cantor's fundamental decomposition theorems showed that Weierstrass's problem can be completely solved in a large class of cases. For, it follows from Cantor's results using his new theory that

(4) If P is a *closed set* (*i.e.* if $P^{(1)} \subseteq P$) then there is always a number α of the first or second number-class such that $P^{(\alpha)} = P^{(\alpha+1)}$.

(4) breaks down into the following two cases:

(5) If P is closed and countable then there is a number α from the first or second number-classes such that $P^{(\alpha)} = P^{(\alpha+1)} = \varnothing$;

[1] See Mittag–Leffler [1884*a*], p. 29.
[2] See Mittag–Leffler [1884*a*], p. 57.
[3] See *ibid.*, pp. 57–72.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

and

(6) If P is closed and uncountable, then there is a number α from the first or second number-classes such that $P^{(\alpha)} = P^{(\alpha+1)} \neq \varnothing$, *i.e.* such that $P^{(\alpha)}$ is *perfect*.[1]

(3) and (5) combined completely solve the representation problem where only countable sets of singularities are involved, no matter how the limit points of this countable set are distributed. And (3) and (6) combined reduce the problem where uncountable sets of singularities are involved to that involving only perfect sets of singularities.

The development of Mittag–Leffler's work is a beautiful example of one theory feeding off the results of another. And the novel consequences of Cantor's 1880–3 theory in particular enabled Mittag–Leffler to take a substantial step towards a complete resolution of the representation problem. The 'importance' of this problem is not in any doubt; it was clearly not a problem created by Cantor's new theory, and Weierstrass himself had marked it out as a fundamental problem for his programme in 1876. Lastly, Mittag–Leffler's theorem played no part in the construction of Cantor's theory; rather (5), the crucial contribution from Cantor's theory, was a developed consequence of the new theory, not 'built into' it.

Thus according to Hilbert's criterion, Cantor's introduction of transfinite numbers into point-set theory clearly represents progress over Cantor's pre-1880 point set theory. Thus, we have one example of how Hilbert's criterion can be applied in appraising theoretical change.

I now want to turn to another application, at first sight rather a curious one, of Cantor's theory of transfinite numbers, namely Borel's 1895 proof of the Heine–Borel theorem. This is particularly interesting because it raises an important question about the use of problem solutions in appraising mathematical theories.

(*c*) *Borel's 1895 Proof of the Heine–Borel Theorem*

In his [1895], Borel uses the following lemma:

. . . if an infinity of given partial intervals on a line is such that its total sum is

[1] See Cantor [1884]. (5) follows from the fact that any closed P can be represented as

$$P = (P - P^{(1)}) \cup \bigcup_{\beta<\Omega} (P^{(\beta)} - P^{(\beta+1)}) \cup P^{(\Omega)}$$

(where Ω is the first ordinal of the *third* number-class) and the fact, proved in Cantor [1883*b*], that the second-number class is uncountable. Hence, if P is countable, there must be a first ordinal $\beta < \Omega$ such that $P^{(\beta)} = \varnothing$, otherwise there would be uncountably many non-empty, disjoint sets on the right-hand side, thus making P uncountable.

(6) follows from Cantor's proof ([1884], pp. 465–6) that $P^{(\Omega)}$ and hence some $P^{(\alpha)}$ for $\alpha < \Omega$ must be perfect (*ibid.*, p. 467).

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

less than that of a given total interval, then there is at least one point of the interval not belonging to any of the partial intervals.[1]

It is clear from the context that 'infinity', 'partial interval', and 'total interval' are to be understood as 'countable infinity', 'open interval' and 'closed interval' respectively. Thus stated contrapositively the lemma reads:

(7) If a countably infinite collection of open intervals completely covers a closed interval, then its total sum must be at least as great as the length of the closed interval.

The problem in proving (7) is to assign some value to the open cover, or at least to put a lower bound on it. This is precisely what the Heine–Borel theorem would do. Borel deals with this in a note at the end of his paper. He claims that although the lemma is 'almost obvious', it is nevertheless worth giving a proof based on a theorem 'interesting in itself'. This he states as:

Given an infinity of partial intervals on a line segment such that each point of the line lies in the interior of at least one of the intervals, then one can effectively determine a *limited number* of intervals chosen among the given intervals and having the same property (every point of the line is interior to at least one of them).[2]

This formulation is not quite that of the Heine–Borel theorem. But what Borel actually sets out to prove is:

(8) Any countable cover of a closed interval by open intervals can be reduced to a *finite* cover.

This is now just the Heine–Borel theorem for countable covers, and (7) follows easily from it.[3]

The proof which Borel gives for (8) uses Cantor's transfinite ordinals (actually only the countable ordinals) as follows.

Let $[a, b]$ be the interval in question. Let (a_1, b_1) be an interval which covers a, (a_2, b_2) an interval which covers b_1, (a_3, b_3) an interval covering b_2 and so on. If b is reached after a finite number of steps, the theorem is proved. If not, we eventually reach (a_ω, b_ω) which may or may not cover b. In any case b must eventually be covered by some (a_α, b_α) where α is a countable ordinal since otherwise an uncountable number of intervals would be required to cover $[a, b]$, which is contrary to the statement of the theorem.

Thus, $[a, b]$ is covered by $\{(a_\xi, b_\xi) : \xi \leqslant \alpha+1\}$. Once this is done the proof then shows, by induction on α, that this collection of intervals can

[1] The particular problem Borel was investigating is described in Hawkins [1970], pp. 97–102. Although Hawkins mentions the Heine–Borel theorem, he doesn't discuss Borel's proof.

[2] Borel [1895], p. 281.

[3] (8) was christened the 'Heine–Borel theorem' by Schönflies in his [1899], p. 51.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

be reduced to a finite collection. This is done in the following way. Assume as hypothesis that $[a, b_\xi]$ can be finitely covered for all $\xi < \beta \leqslant \alpha$ then it is shown that $[a, b_\beta]$ can also be finitely covered. If β is a successor ordinal the result is immediate. If on the other hand β is a limit ordinal, then since β is countable there must, by definition, be a countable sequence of increasing countable ordinals $\xi_1, \ldots, \xi_n, \ldots$ which has β as a limit. Hence, $b_{\xi_1}, b_{\xi_2}, \ldots, b_{\xi_n}, \ldots$ must be a sequence of points in $[a, b_\beta]$ tending to b_β from the left. Hence, since $(a_{\beta+1}, b_{\beta+1})$ includes b_β in its interior, it must also include all but a finite number of the b_{ξ_m}. Let $b_{\xi_{m_0}}$ be the sequence point with least index in $(a_{\beta+1}, b_{\beta+1})$. By hypothesis $[a, b_{\xi_{m_0}}]$ can be finitely covered, since $\xi_{m_0} < \beta$. Hence $[a, b_\beta]$ can be finitely covered simply by taking the finite cover of $[a, b_{\xi_{m_0}}]$ and $(a_{\beta+1}, b_{\beta+1})$. This completes the proof.[1]

It seems clear that this use of Cantor's theory again demonstrates its merit according to the conditions of Hilbert's criterion. But the interesting point here is the following. The Heine–Borel theorem was proved very soon after 1895 in point-set analysis alone without any use of Cantor's theory of transfinite numbers. It might then be wondered whether Borel's use of the transfinite numbers was not just something of an oddity, and thus whether it is correct to regard this use as an indication of the merit of Cantor's theory. This raises a more general question concerning the use of problem solutions in appraising theories. If a theory T is used to solve problems in a theory T^* and it is later shown that the problems can be solved in T^* alone, can we regard the original solutions using T as an indication of the merit of T?

It seems to me that we can and should. Any general theory T^* has potentially infinitely many consequences. But these consequences have to be *discovered*. Thus, if a theory T plays a part in the discovery of consequences of T^* then this should surely be allowed to count in T's favour even if T is subsequently shown to be strictly unnecessary for deriving the statement in question from T^*. After all, in looking at the history of mathematics we are not concerned with whether a theory is in the long run logically indispensable, but rather with the impact it had on the state of mathematical knowledge in the period after its proposal. Theories which are heuristically important may turn out to be logically dispensable. But the historian and methodologist of mathematics must be prepared to give a theory credit for the heuristic role it played.[2]

[1] See Borel [1895], pp. 280–2, and Schönflies [1899], pp. 51–2. Borel actually gives very few of the details and even Schönflies doesn't give them all. For example, he only proves the induction step from finite numbers to ω, and not in complete generality.

[2] As John Worrall pointed out to me the situation here is analogous to that regarding physical theories. If a theory T predicts a novel fact e then according to *MSRP* this fact supports T, and the support is not destroyed by the subsequent derivation of e from a

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

It is often not difficult to see just what the heuristic role played by the discovering theory is. In many cases, for example, the theory T might suggest how to look for a proof in T^* alone. This is just what happened in the case of the Heine–Borel theorem.

The standard proofs of the Heine–Borel theorem rely on the connection between either the *Bolzano–Weierstrass property* and *compactness*, or *sequential compactness* and *compactness*. (Indeed it is now known that for metric spaces these three properties are equivalent.) But it was the original transfinite numbers proof which first established that connection. What the original proof shows, in effect, is that the Bolzano–Weierstrass theorem employed a transfinite (but countable) number of times entails the Heine–Borel property.

Take as an example the first stage of the proof. Define the intervals (a_n, b_n) as above (p. 21), and assume that $\{(a_j, b_j) : j = 1, \ldots m\}$ fails to cover $[a, b]$ for each m. Then $b_1, b_2, \ldots, b_n, \ldots$ will be a sequence of points of the space $[a, b]$, and thus (by the Bolzano–Weierstrass property of sequential compactness) will have a limit point p in $[a, b]$. This is *one* application of the Bolzano–Weierstrass theorem. By the hypothesis of the theorem there is an (a_ω, b_ω) which includes p; and since p is the limit of the b_n, (a_ω, b_ω) will include all the b_n from some n_0 on. From this it is easy to show that $[a, b_\omega]$ can be finitely covered.

The construction of the set $\{(a_\xi, b_\xi) : \xi \leqslant \alpha+1\}$ (*above*, p. 21) and the fact that it can be reduced to a finite sub-cover now follows from countably many repetitions of the same argument, *i.e.*, in particular, countably many repetitions of the Bolzano–Weierstrass theorem.

Once a proof has established a connection between two statements or properties, one might then wish to proceed to investigate the connection, by seeking to discover, for example, whether the connection can be made more simply, or perhaps by a different route altogether. In the case of the Heine–Borel theorem, Borel and others (particularly Lebesgue) investigated the established connection between the Bolzano–Weierstrass property and compactness. The two most 'obvious' paths to follow are first to investigate whether the Heine–Borel property follows from fewer applications of the Bolzano–Weierstrass theorem; or second, whether even the proof of this theorem can be adapted to establish the Heine–Borel property. Borel took the second path, and Lebesgue the first. Thus Borel in his [1898] succeeded in showing that the standard proof of the Bolzano–Weierstrass theorem

---

different theory T^*. This applies even if T^* is T's predecessor, and e is also novel with respect to T^*. The deduction of e from T^* will certainly show that T is logically redundant as regards e. But the methodology nonetheless insists that the introduction of T constitutes progress. Intuitively, this is correct since T has led to an increase in our knowledge. Its logical dispensability is irrelevant.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

can be adapted so as to prove the Heine–Borel theorem directly. His proof runs as follows.

Let $[a, b]$ be given, and assume we are dealing with a cover of $[a, b]$ by open intervals which *cannot* be reduced to a finite subcover. (Borel still assumes the cover is countable, but this is not necessary.) Divide $[a, b]$ into $[a, (b+a)/2]$ and $[(b+a)/2, b]$. Clearly for at least one of these intervals the cover cannot be reduced to a finite sub-cover. Denote this interval (and if the same applies to both take the left hand one) by A_1. Now similarly divide A_1 into two closed intervals, and so on. In this way we get a descending sequence of closed intervals $A_1 \supseteq A_2 \supseteq A_1 \ldots \supseteq A_m \supseteq \ldots$ whose diameters tend to zero. Up to this point the proof is just a repetition of the standard Bolzano–Weierstrass argument. Continuing that argument, there will be a point p lying inside *all* the A_m which will be the limit of a convergent sub-sequence of end-points of the A_m. Now lastly, let (c, d) be an open interval from the cover cf $[a, b]$ which includes p in its interior. Since, the diameter of the A_m decrease to zero there will be an n_0 such that $A_m \subseteq (c, d)$ for all $m \geqslant n_0$. Thus, all these A_m are finitely coverable which gives us a contradiction. Hence, every cover is reducible to a finite subcover.[1]

The proof is almost an exact replica of the Bolzano–Weierstrass proof. Lebesgue's procedure was even simpler. In his [1904] he argued as follows.

Let X be the set of all x in $[a, b]$ such that $[a, x]$ has a finite sub-covering. Assume $b \notin X$. Then there is either a *first* point *not* in X, or a *last* point *in* X. In either case let x_0 be this point. In fact x_0 will be the supremum of X. Now there is an interval (c, d) from the cover which includes x_0, since $a < x_0 < b$. Form (c, x_0) and (x_0, d) and let $x_1 \in (c, x_0)$ and $x_2 \in (x_0, d)$. Then $a \leqslant x_1 < x_0 < x_2 \leqslant b$ and so $x_1 \in X$. Thus, there are finitely many intervals covering $[a, x_1]$. Add (c, d) to these and we have finitely many intervals covering $[a, x_2]$, which is a contradiction since $x_0 < x_2$. Thus, $b \in X$, and $[a, b]$ can be finitely covered.[2]

In this case the theorem is derived from just *one* application of the Bolzano–Weierstrass theorem or the equivalent sequential compactness property.[3]

The *heuristic* importance of the original transfinite numbers proof is now clear; their use in the proof of the Heine–Borel theorem is *not* merely an oddity. Thus, while Borel for example might have been led to find a

[1] See Borel [1898], pp. 43-4.

[2] See Lebesgue [1904], pp. 104–5. Proofs very similar to this are given in Kelley [1955], pp. 144–5, and Hocking and Young [1961], p. 18.

[3] This is required to prove the existence of x_0, assuming that we are dealing with real numbers given by Weierstrass's or Cantor's definitions. See Courant [1937], pp. 61–3. If we use Dedekind's theory, then the Dedekind cut theorem has to be used.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms

new proof because of his *philosophical* objections to the transfinite numbers and the actual infinite, it is certain that heuristically he was very much indebted to them. Indeed Borel admitted as much himself in some general remarks he made in his [1899]. Pointing out that he does not make a great deal of direct use of Cantor's theories in his paper he says:

> I hope that this exposition, which is set out in such a way that Cantor's name is hardly mentioned, will not prevent anyone from realising how great is the importance of his works and the influence that the reading of them has had on the development of the ideas which I try to set out here.[1]

Then in a footnote he goes on:

> It is all the more important for me to say this since the fact that I did not point it out in a recent book of mine, together with the fact that I did not use Cantor's methods there, has led certain readers to believe that I do not place a high value on his work. *The merit of the discoverer subsists even if, for one reason or another, the method which he followed to achieve his aim is abandoned.*[2]

(*To be continued.*)

London School of Economics

[1] Borel [1899], p. 136.
[2] *Ibid.*, n. 1; my italics. *Cf.* Schönflies [1908], p. 76, n. 1.

This content downloaded from 62.3.71.42 on Tue, 16 Jul 2019 12:38:18 UTC
All use subject to https://about.jstor.org/terms