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THE PROSPECTS FOR MATHEMATICAL LOGIC IN THE TWENTY-FIRST CENTURY

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Abstract. The four authors present their speculations about the future developments of mathematical logic in the twenty-first century. The areas of recursion theory, proof theory and logic for computer science, model theory, and set theory are discussed independently.

We can only see a short distance ahead, but we can see plenty there that needs to be done.

A. Turing, 1950.

§1. Introduction. The annual meeting of the Association for Symbolic Logic held in Urbana-Champaign, June 2000, included a panel discussion on "The Prospects for Mathematical Logic in the Twenty-First Century". The panel discussions included independent presentations by the four panel members, followed by approximately one hour of lively discussion with members of the audience.

The main themes of the discussions concerned the directions mathematical logic should or could pursue in the future. Some members of the audience strongly felt that logic needs to find more applications to mathematics; however, there was disagreement as to what kinds of applications were most likely to be possible and important. Many people also felt that applications to computer science will be of great importance. On the other hand, quite a few people, while acknowledging the importance of applications of logic, felt that the most important progress in logic comes from internal developments.

It seems safe to presume that the future of mathematical logic will include a multitude of directions and a blend of these various elements. Indeed, it speaks well for the strength of the field that there are multiple compelling directions for future progress. It is to be hoped that logic will be driven

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both by internal developments and by external applications, and that these different directions will complement and strengthen each other.

The present article consists of reports by the four panel members, at times expanding on their panel presentations. As in the panel discussion, the presentations are divided into four subareas of logic. The topics are ordered as in the panel discussion: R. Shore discusses recursion theory in section 2; S. Buss discusses proof theory and computer science logic in section 3; A. Pillay discusses model theory in section 4; and A. Kechris discusses set theory in section 5.

We, the panel members, wish to thank Carl Jockusch and the rest of the program committee for conceiving of the panel topics and inviting us to participate.

S. Buss

§2. Recursion theory, by Richard A. Shore.

When I was asked to participate in this panel, the first thing that came to my mind was the verse from Amos (7:14): I am neither a prophet nor a prophet's disciple. Nonetheless, with some trepidation, I agreed to participate. When I saw that there was to also be a panel on logic in the Twentieth Century, I thought that one would have been easier – after all it has already happened. In any case, I decided to start, in some sense, with the past. Rather than a prophet for mathematics, I have in my own past been (somewhat like Amos) a tender of problems and a gatherer of theorems. To change my ways, I will start not so much with specific questions and results as with an attempt to point to attitudes, approaches and ideas that have made recursion theory what it is and might continue to prove useful in the future. After all, the true role of the prophet is not to predict the future but to point out the right path and encourage people to follow it.

For recursion theory, as for logic as a whole, there are great theorems in our past but perhaps a major share of our contribution to mathematics is in the view that we take of the (mathematical) world and how it guides us to problems, questions, techniques, answers and theorems. When I teach the basic mathematical logic course, I like to say in my first lecture that what distinguishes logic is its concern for, and study of, the language of mathematics. We study formal languages, their syntax and semantics and the connections between them. Our work is motivated by the idea that not only are these and related topics worthy of study in their own right, as both mathematics and foundations, but that formalizing and analyzing the language of mathematical systems sheds light on the mathematics itself.

Along these lines, the language that recursion theory originally formalized and still studies is that of computability: machines, algorithms, deduction systems, equation calculi, etc. Its first great contribution, of course, was the formalization of the notion of a function being computable by an algorithm and the discovery of many remarkable instances in all branches of mathematics of the dividing line between computability and noncomputability, decidability and nondecidability. I see these results as instances of the overarching concern of recursion theory with notions of complexity at all levels from the space/time and other subrecursive hierarchies of computer science through Turing computability and arithmetic to descriptive set theory and analysis and, finally, to higher recursion theory and set theory. Each way station on this road of exploration has its appropriate notions of reducibility and degree structure and its own hierarchies. Our viewpoint is that an analysis of relative complexity in any of these terms sheds important light on the fundamental notions of computability and definability and can serve to illuminate, distinguish and classify mathematical structures in useful ways.

I would like to mention a few issues and areas where I think these ideas and approaches have been useful and I expect will continue to be so in the future. Of course, there is no expectation of being exhaustive and I admit to concentrating on those areas that have caught my own interest.

Classical recursion theory. In general, I view the important issues in classical recursion theory as analyzing the relations among various notions of complexity and definability and so also investigating the possible automorphisms of the computability structures of interest. At the general level, there are many open problems about the connections between Turing degrees, rates of growth of functions, set theoretic structural properties, definability in arithmetic and the jump classes. For a whole array of specific question in a range of areas, I recommend Cholak et al. [6], the proceedings volume of the 1999 AMS Boulder conference on open problems in recursion theory.

The most important and pressing current problem in this area is, I believe, the clarification of the situation with respect to, first, the definability of the jump operator and the notion of relative recursive enumerability and, second, the existence of automorphisms of the Turing and r.e. degrees. In the past year, Cooper's original definition of the jump in the Turing degrees [9, 10] has been shown to be false – the proposed property does not define the jump (Shore and Slaman [52]). Taking an approach quite different from Cooper's, Shore and Slaman [53] then proved that the jump is definable. Their approach uses results of Slaman and Woodin [59] that strongly employ set theoretic and metamathematical arguments. While this is pleasing in some ways, it is unsatisfactory in others. In particular, it does not supply what I would call a natural order theoretic definition of the jump. For example, the definition explicitly talks about codings of (models of) arithmetic in the degrees. (See Shore [51].)

In various versions of Cooper [11], Cooper has since proposed two other candidates for natural definitions of the jump. The first does not define the jump (Shore and Slaman [52]), and it remains to be seen whether the second, much more complicated one, does. Cooper has also proposed a number of ways of using such a definition of the jump to define relative recursive enumerability. There are fundamental difficulties to be surmounted in any attempt to define the notion of r.e. by these means (Slaman [personal communication]), but establishing a natural definition of the jump still seems to be the most likely route to a definition of recursive enumerability. (See Slaman [57].)

In the area of using classical computability type complexity properties to classify mathematical structures, I would like to point to the exciting developments in current work by Nabutovsky and Weinberger [46] discussed by Soare in his lecture at this meeting (see Soare [60]). This work uses complexity properties not just on the decidable/undecidable border but far beyond. It uses both rates of convergence and r.e. Turing degrees to distinguish interesting classes of spaces and Riemannian metrics by capturing certain types of invariants in terms of computability properties. We certainly hope for more such interactions in the future.

Descriptive set theory. Although I am far from an expert, I am a great fan of descriptive set theory and I view this subject as a major success for what I have called the recursion theoretic point of view. The whole array of issues connected to hierarchies, complexity classes, reducibilities and definability are fundamental to recent work in this area. It well illustrates the ideas of classification and analysis of mathematical structures in such terms and the belief that such analysis supplies important information about the structures. At a deeper level, the ideas of effective descriptive set theory — the direct application of notions of computability for numbers and functions from classical recursion theory to analyze sets of reals via the relativization of light faced results and other methods — permeate many aspects of the area. (See Moschovakis [44] and the forthcoming book of Louveau.)

This topic really belongs to Alekos Kechris and §5, but for myself, I also have hopes for interactions between Borel notions and computability ones in the areas of effective algebra and model theory. The issue here, as I see it, is to classify the complexity of mathematical, algebraic and model theoretic properties in the domain of computable mathematics. This issue arises in many disguises some of a descriptive set theoretic nature and others more concerned with computability and relative computability. (Examples here can be found in Friedman and Stanley [22], Camerlo and Gao [4], Hjorth [27], White [65] and Hirschfeldt, Khoussainov, Shore and Slinko [26].)

Effective and reverse mathematics. Perhaps, with the revival of interest in computational approaches to much of mathematics, we will see more interest in some of the topics of effective mathematics such as determining what input information about structures is needed to compute various properties, functions and so on. The technical notions involved here include versions of intrinsic computability, degree spectra and the like. (For these specific topics see, for example, Khoussainov and Shore [39] or Shore [50] and, for the whole range of issues involving effective mathematics, the handbook Ershov et al. [16].)

These issues in effective mathematics are also related to the foundational concerns of reverse mathematics which, on its face, uses another, proof theoretic, yardstick to measure complexity. The proof theoretic measures used, however, turn out to be intimately connected to ones studied in recursion theory from relative computability and the Turing jump to relative hyperarithmeticity and the hyperjump. The basic reference here is Simpson [56]. The foundational issues addressed by reverse mathematics are important ones and we expect the contributions and approaches of recursion theory in this area to increase and prove important as well. A current snapshot of work in the field is provided by the collection Simpson [55].

Along with the rise of mathematical interest in computational procedures come other views of computability that should be worth investigating. These should be measures that reflect mathematical practice in particular domains rather than just what we traditionally view as computation in our traditional discrete, digital approach. One obvious candidate is the notion of computation introduced by Blum, Shub and Smale [3] (see also Blum et al. [2]). Here the corresponding notions of complexity have been used (for example, in Chong [7, 8]) to distinguish interesting phenomena about Julia sets and the like in dynamics. I would suggest that we should look for other applications and other notions of computability appropriate to various mathematical domains as well.

Set theory. Next, I would like to mention (general) set theory as an area for uses of the recursion theoretic world view. I view the early development by Jensen of the fine structure of L [33] as another outstanding example of the application of the this viewpoint to open up a whole line of analysis and investigation. Its extensions continue to grow remarkably on their own as witnessed by the talks of Steel and Neeman at this meeting and, for example, Löwe and Steel [42]. There is also still room for applications of definability, complexity and effective analysis in the setting of classical set theoretic problems as witnessed, for example, by recent work by Slaman [58] and Groszek and Slaman [24].

Saving the best for last, we come to computer science.

Computer science. The origins of logic were in foundations and philosophy; most of us on this panel, and in the audience, were trained and grew up as mathematicians; the primary future growth opportunities of logic, however, clearly lie in computer science. I would make the analogy that logic is to computer science what mathematics is to physics, and vice versa. Logic serves as the language and foundation for computer science as mathematics does for physics. Conversely, computer science supplies major problems and new arenas for logical analysis as physics does for mathematics. This relationship has affected, and will affect, not just recursion theory but all of logic. There are far too many instances to even mention but I point to a symposium last year at the American Association for the Advancement of Science titled "On the unusual effectiveness of logic in computer science" reported on in Halpern et al. [25] as one multifaceted indicator of the view from computer science.

Of course, Sam Buss has much more to say about logic and computer science in §3, but for now I'll put in a word from my own sponsor, recursion theory. The notions of reducibility, complexity measures, and hierarchies are fundamental to theoretical computer science. Both below and above the level of polynomial time computability we have analogs of the classical reducibilities including one-one, many-one, (bounded) truth-table, weak truth table and Turing that reflect various views of boundedness of different resources. We also see analogs of notions from higher recursion theory such as fixed points, inductive definability and admissible ordinals. Interestingly, new notions have been developed at low complexity levels that present alternate notions of computability involving nondeterminism and probabilistic procedures among others. The relationships among these various restricted (and extended) notions of computation form the core questions of complexity theory in computer science. What are the relationships among the classes P, NP, PP, BPP and so on. We hope that the methods and insights of recursion theory will play a role in the solution of these fundamental problems of computability.

These concerns will continue to illuminate the investigations of computation from both practical and theoretical vantage points. More radical innovations are needed, however, to better reflect the finiteness of all types of resources. This is a challenge for computer science, computability theory and logic as a whole. Linear logic and finite model theory represent some attempts at addressing these issues, but others are needed that incorporate finiteness and boundedness of resources in other ways. Nonetheless, our view from infinity will continue to lend perspective (as in the role of uniformity) and so play a role in analyzing the finite as well.

In a different direction, we return to the original language of computation. Here the beginnings of recursion theory have already played an important role, e.g. the Turing machine model as a basic one for computation and the λ-calculus as one for programming languages both abstract and actual. And so we come back to the beginnings of the study of the formal languages of computation. Along these lines, I would like to close with three, certainly not original and probably pie-in-the-sky, problems.

1. "Prove" the Church-Turing thesis by finding intuitively obvious or at least clearly acceptable properties of computation that suffice to guarantee that any function so computed is recursive. Turing [63] argues for the thesis that any function that can be calculated by an abstract human being using

various mechanical aids can be computed by a Turing machine (and so is recursive). Gandy [23] argues that any function that can be calculated by a machine is also Turing computable. Deutsch [14] approaches this issue from a more quantum mechanical perspective. Martin Davis has pointed out (personal communication) that one can easily prove that computations as given by deductions in first order logic relations from a finite set of sentences about numerals and the function being defined are necessarily recursive. An analysis based on the view that what is to be captured is human mechanical computability is given in Sieg [54].

Perhaps the question is whether we can be sufficiently precise about what we mean by computation without reference to the method of carrying out the computation so as to give a more general or more convincing argument independent of the physical or logical implementation. For example, do we reject the nonrecursive solutions to certain differential equations as counterexamples on the basis of our understanding of physics or of computability? Along these lines, we also suggest two related questions.

2. What does physics have to say about computability (and provability or logic)? Do physical restrictions on the one hand, or quantum computing on the other, mean that we should modify our understanding of computability or at least study other notions? Consider Deutsch's [14] Church-Turing principle and arguments that all physically possible computations can be done by a quantum computer analog of the universal Turing machine. He argues, in addition, that the functions computable (in a probabilistic sense) by a quantum Turing machine are the same as the ones computable by an ordinary Turing machine, but that there is, in principle, an exponential speed-up in the computations. How do these considerations affect our notions of both computability and provability? For some of the issues here see Deutsch et al. [15].

3. Find, and argue conclusively for, a formal definition of algorithm and the appropriate analog of the Church-Turing thesis. Here we want to capture the intuitive notion that, for example, two particular programs in perhaps different languages express the same algorithm, while other ones that compute the same function represent different algorithms for the function. Thus we want a definition that will up to some precise equivalence relation capture the notion that two algorithms are the same as opposed to just computing the same function. Moschovakis [45] is an interesting approach to this problem from the viewpoint that recursion, and an appropriate formal language for it, should be taken as basic to this endeavor.

§3. Proof theory and logic for computer science, by SAM BUSS.

I discuss in this section prospects both for proof theory and for computer science logic. I will first present a broad overview of the presently active areas in proof theory and computer science logic and my opinions on which

Three-fold View of Proof Theory

Stong Systems	Weak Systems	Applied Proof Theory
Topics: - Central foundations of proof theory. - Ordinal analysis. - Fragments of type theory and set theory.	*Topics:* - Expressive but weak systems, including bounded arithmetic. - Complexity (esp., low-level). - Proof complexity. - Propositional logic.	*Topics:* - Very diverse - Theorem proving. - Logic programming. - Language design. - Includes logics which are inherently not first-order.
Goals/directions: - Constructive analysis of second-order logic and stronger theories. - Methods and tools used extensively in the other areas.	*Goals/directions:* - Central problem is P vs. NP and related questions.	*Goals/directions:* - Again: very diverse. - Central problem is the "AI" problem of developing "true" artificial intelligence.

Table 1. The areas of proof theory, organized by goals.

areas are likely to be important in the future. I then make some specific predictions about future developments in these areas. The section concludes with an exhortation for mathematical logicians to pay more attention to applications of logic in computer science.

I am charged with the task of discussing both proof theory and computer science logic in this section. It thus has happened that proof theory is somewhat shortchanged. The reader who wishes to seek more comments on proof theory can find opinions by a large group of proof theorists in the compendium prepared by S. Feferman [17].

Proof theory. I first present a very quick overview of the present goals of proof theory. Table 1 gives a "three-fold" view of proof theory, in which proof theory is split into three broad categories based on the goals of the work in proof theory.

The first column represents the traditional, classic approaches to mathematical proof theory: in this area the goal has been to understand stronger and stronger systems, from second-order logic up through higher set theories, and especially to give constructive analyses of the proof-theoretic strengths of strong systems. This part of proof theory has of course seen outstanding progress in the past century, but in recent times has had more limited success. The work on constructive analyses of strong systems has become stymied by technical difficulties, and progress has tended to be incremental. Further significant progress will require a substantial breakthrough in applicability, say to all of higher-order logic, as well as a breakthrough in technical simplicity. Lest the assessment of this area of proof theory seem too harsh, I hasten to add that the methods and results of this area are fundamental to the other areas of proof theory.

The second column represents the smallest area of proof theory. The presence of this category is justified by the fact I am presenting the areas of proof theory categorized by their goals. The essential goal of this area is the resolution of the important questions in computational complexity such as the P versus NP question. This question turns out to be very closely linked to corresponding questions about provability, about proof complexity, and about proof search in very weak proof systems, including systems as weak as propositional logic. It is an amazing fact that very fundamental and simple questions about propositional logic are still open! I will make predictions about the future of these questions below.

The third column represents the broadest branch of proof theory; it is also the oldest in that it predates the modern mathematical development of proof theory, with its essential goals stated already by Leibniz. One goal of this area is to provide logical systems strong enough to encompass more and more of human reasoning. A second goal is the development of true AI, or "artificial intelligence". Of course, this area of proof theory is extremely diverse, and it includes the aspects of proof theory that deal with reasoning in restricted domains and aspects of proof theory that are applicable to programming languages, etc.

I wish to avoid philosophical issues about consciousness, self-awareness and what it means to have a soul, etc., and instead seek a purely operational approach to artificial intelligence. Thus, I define artificial intelligence as being constructed systems which can reason and interact both syntactically and semantically. To stress the last word in the last sentence, I mean that a true artificial intelligence system should be able to take the meaning of statements into account, or at least act as if it takes the meaning into account. There is some debate about whether logic is really a possible foundation for artificial intelligence. The idea that logic should be the foundation for AI has fallen out of favor; indeed, much of the work of artificial intelligence today is done with non-discrete systems such as neural nets, which would not count as part of proof theory. To the best of my knowledge, there is only one large-scale present-day attempt to build an AI system based on logic, namely the Cyc system, and this so far has not reported significant success in spite of a massive effort. Nonetheless, it is my opinion that purely analog systems such as neural nets will not provide a complete solution of the AI problem; but rather, that discrete processing, including proof theoretic aspects, will be needed for constructing AI systems.

Of course, most present day work in applied proof theory is not aimed directly at the AI problem. There is a large amount of work being done to extend logic beyond the domain of first-order logic. This includes, for instance, non-monotonic logics, modal and dynamic logics, database logics, fuzzy logic, etc. What these rather disparate areas have in common is that they all wish to extend logic well beyond the boundaries of the kind of first-order logic that has been successful in the foundations of mathematics.

I make some specific predictions about the prospects for artificial intelligence in a later section.

Logic for computer science. A skeletal overview of the present state of affairs for applications of logic to computer science is presented in Table 2, which is titled the "octopus of logic for computer science." I have not attempted to make a definitive summary of the applications of logic to computer science in Table 2, as they are far too numerous and varied for me to make such an attempt. The main point of the table is to illustrate how diverse and extensive the applications of logic for computer science have become.

Computer science has strong interactions with most of the traditional areas of logic, with the sole exception of set theory. First, the early developments of both the theory and practice of computer science were very closely linked to the development of recursion theory, beginning with the emergence of the stored program paradigm arising from Turing's model for universal computers. In more recent times, recursion theory has been less closely linked to computer science; however, developments in complexity theory have often been inspired by constructions in recursion theory. Proof theory has many connections to computer science; indeed, the bulk of the work in the third category of 'applied proof theory' in Table 1 is oriented towards applications in computer science. In addition, the second category of proof theory has been found to have many connections to complexity theory. There are likewise many applications of model theory in computer science as well: in Table 2 these include finite model theory, database theory, and model-checking. More generally, whenever one deals with the semantics of a language, one is implicitly doing model theory.

Future directions and some predictions. The above overviews of proof theory and of logic for computer science indicate the directions that I feel will be their most important areas for future development. To be even more explicit about my expectations for the future, I will now make a series of quite specific predictions about when we may solve the important problems in these areas.

P versus NP and related questions. Although progress in actually solving the P versus NP problem has been slow, there has been a vast amount of work related to P, NP and other complexity classes. I do not believe that there should be any inherent reason why a solution to the P versus NP problem

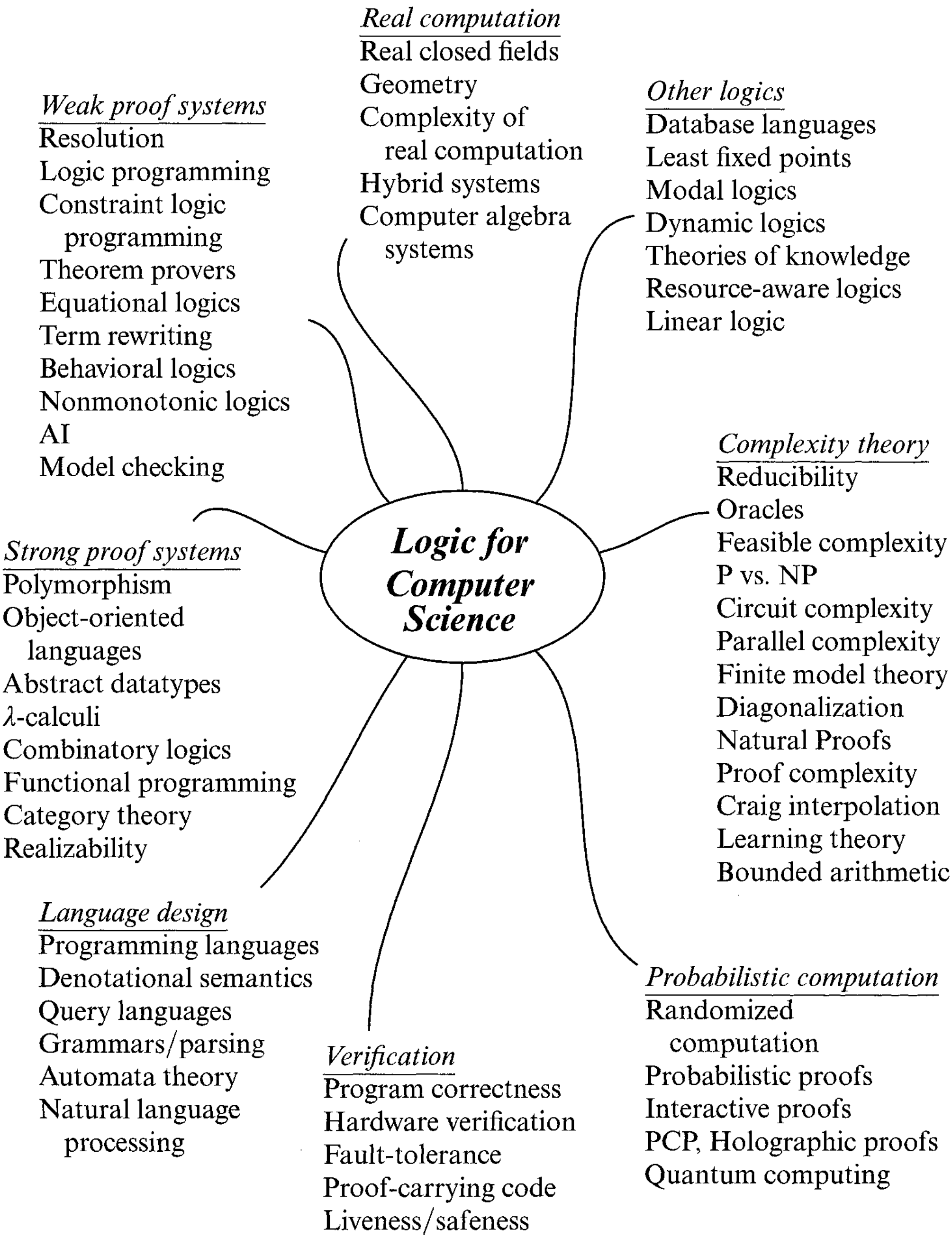

Table 2. The octopus of logic for computer science.

should be difficult, but rather think we just need to find the right idea. Thus I make the following prediction.

PREDICTION 1. *The P versus NP problem (and many related questions in complexity theory) will be solved by the following date:*[1]

$$2010 \quad \pm 10 \textit{ years.}$$

I further predict that P is distinct from NP; however, I am agnostic about the truth of many of the commonly conjectured cryptographic conjectures.

I *hope* that the solution to the P versus NP problem will be some kind of extension of the diagonalization method, that is to say, that there will be some logical reasoning extending ideas of self-reference, which will be able to resolve the P/NP problem. The alternative to a logical solution of this type would be a combinatorial proof more in the lines of the so-far obtained circuit lower bounds of Yao, Hastad, Razborov, Smolensky, and others. To my mind, a combinatorial proof would be a bit disappointing and a logical proof would be far preferable. Obviously, a logical proof would be a tremendous boost to the prestige and importance of logic.

Two promising recent approaches to solving the P versus NP problem include recent work on diagonalization (by Fortnow and others) and on natural proofs and Craig interpolation (beginning with the work of Razborov and Rudich). More broadly, much work in weak first-order theories and on propositional proof complexity is motivated by the desire to find a logical proof that $P \neq NP$.

Future problems in proof complexity. Probably the most important problem in proof complexity is to better understand the structure of propositional and first-order proofs with cuts.

Another very important problem is to either find, or prove impossible, proof search procedures which both (a) are more efficient than human mathematicians, and (b) yield humanly intelligible proofs. However, I think this problem is extremely difficult and can be accomplished only with the solution of the next problem.

Artificial intelligence. As discussed above, true AI will involve semantic reasoning based on machine "understanding." I do not expect that artificial intelligence will be an all-or-nothing event of the kind frequently envisioned in popular literature where we one day suddenly discover that machines have become intelligent. I also think that some of the currently expressed fears about the dangers of artificial intelligence are way over-blown. Rather, I predict that progress in artificial intelligence will be a long, slow process of incremental gains. Nonetheless, I make the following prediction.

[1]These predictions were formulated in June 2000.

PREDICTION 2. *There will be limited but significant success in artificial intelligence by*

$$2050 \quad \pm 30 \text{ years.}$$

As discussed earlier, I predict that success in artificial intelligence will require logic-based reasoning. By "limited, but significant success", I envision that artificial intelligence may be successful in some relatively broad domain of knowledge which is generally acknowledged as involving operational understanding of semantic concepts.

One good possibility for a first knowledge domain for the initial artificial intelligence systems is the area of mathematical reasoning. There are several advantages to mathematical reasoning as a knowledge domain. Firstly, a computer can interact more-or-less on an equal footing with a human since no physical interaction is required. Secondly, the domain is precisely describable with fixed rules. Thirdly, reasoning in mathematics requires both creativity and a significant semantic understanding of the subject matter, and thus represents a significant challenge for an AI system.

This leads to the next prediction.

PREDICTION 3. *Computer databases of mathematical knowledge will contain, organize, and retrieve most of the known mathematical literature, by*

$$2030 \quad \pm 10 \text{ years.}$$

The first step in fulfilling this prediction is to design a formal language which can faithfully represent mathematical objects and constructions in a flexible, extensible way. Perhaps an object-oriented language would be a good choice for this; however, present-day object-oriented languages are not adequate for representing mathematical objects.

One of the original stated goals of mathematical logic was to provide a foundational understanding of mathematics. Quite possibly, the next major step forward in the foundations of mathematics will occur in conjunction with the development of systems fulfilling Prediction 3 or perhaps even with AI systems for mathematical reasoning.

The relation of logic and computer science. As illustrated in the "octopus", the area of logic for computer science is a very active, vital and diverse discipline. Indeed, it is likely that there are more people working on logic within computer science than outside of computer science.

In addition, many of the recent developments in computer science call into question the fundamental concepts of mathematical logic. For instance, the introduction of probabilistic proofs and interactive proofs and the possibility of quantum computing, threaten the correctness of two of the most fundamental notions in logic, namely the notions of "proof" and "computability".[2]

[2]In his talk at the Annual ASL Meeting in June 2000, A. Widgerson gave an illuminating survey of some of these new notions for proof and computability that have arisen from the

However, the so-called core areas of logic have historically slighted or ignored developments in computer science. Of course, this is not universally true and there are numerous examples of cross-over research; furthermore, in recent years, the use of logic in computer science has reached a critical mass and it is no longer really possible for core areas of logic to ignore applications of logic for computer science. Nonetheless, I think most people would agree that there is a significant cultural separation between the traditional areas of logic and the use of logic for computer science.

This separation started before my time, so it is difficult for me to say with any certainty why it occurred. But my impression is that the separation arose in part because, as the field of theoretical computer science began, the work lacked focus, seemed somewhat ad hoc and overly concrete, and sometimes lacked depth. (Of course, this is not unexpected in a field which was still in its formative stages.) By comparison, logic in the 1960's was embarked on a grand project of building coherent and deep theories about large-scale concepts, such as large cardinals, higher notions of computability, stronger constructive theories, etc. The work in computer science took the opposite direction of looking at low-level complexity, expressibility and provability in weak languages, etc. After about fifty years of work, theoretical computer science has reached the point where on one hand it is a mature field with deep and far-reaching results, but, on the other hand, still has extremely basic open questions: questions such as whether P is equal to NP or whether mathematically secure cryptography is possible.

Stronger ties between mathematical logic and computer science certainly need to be encouraged. The field of theoretical computer science has grown extremely large, but is still very much in a formative stage, with many key theorems still to be proved and very many advances still needed. Theoretical computer science offers many new applications of logic, and challenges or extends many of the fundamental notions of mathematical logic.

Acknowledgement. I wish to thank Jeff Remmel for comments on an earlier draft of this section.

§4. Model theory, by ANAND PILLAY.

My aim is to describe some trends and perspectives in model theory. This article is an expanded version of my talk in the "Panel of the Future" at the ASL meeting in Urbana. I will also incorporate some points which came out during the subsequent discussion, and so this article may have a somewhat polemical flavour.

Wilfrid Hodges' book *Model Theory* [29] is a basic text for the subject and its comprehensive bibliography can be used as a reference for much

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study of the mathematical foundations of cryptography using notions of probabilistic proof and interactive proofs and based on complexity conjectures.

of the work cited in the present article, in particular for everything in the introduction.

As in Kechris' article it is useful to distinguish between internal and external aspects of research in model theory. One could also call these aspects inward and outward-looking. Of course this distinction is not clear-cut, and in fact I want to describe a remarkable unification that has been in process for a few years. In any case, this inward versus outward dichotomy in no way corresponds to logic (or foundations) versus mathematics (or applications).

By inward-looking I mean the development and study of concepts, problems, etc., proper to model theory itself. Included here are the compactness theorem for first order logic (Gödel, Malcev, Tarski), the theory of quantifier-elimination and model completeness (Tarski, Robinson), homogeneous-universal and saturated models (Morley-Vaught), countable models of complete theories (Vaught), omitting types, products (Feferman-Vaught), generalized quantifiers, infinitary logics, ... Shelah's work on classification theory, following Morley's work on uncountably categorical theories, possibly represented the first fully-fledged *program* within model theory proper. The nature of the problem, as well as various theorems of Shelah himself, allowed him to restrict his attention to a rather small class of first order theories, the *stable* ones, for which a deep theory was developed.

By outward-looking I mean the use of model-theoretic methods in the study of specific structures or theories from mathematics and even from logic itself. Early such work was Malcev's use of the compactness theorem to prove local theorems in group theory. One should include completeness, model-completeness, and quantifier-elimination results for abelian groups (Szmielew) and various classes of fields such as real-closed (Tarski), algebraically closed (Robinson, Tarski), p-adically closed (Ax-Kochen, Ershov, Macintyre), differentially closed (Robinson) etc., and resulting applications. One should also include here nonstandard analysis as well as the use of model-theoretic methods (such as nonstandard models) in set theory and in the study of Peano arithmetic and its fragments. Among the past successes of "outward-looking" model theory are the Ax-Kochen-Ershov analysis of Henselian valued fields and the resulting asymptotic solution to a conjecture of E. Artin.

The current situation. The 70's and 80's saw something of a separation between (i) those interested primarily in model theory as a tool for doing mathematics (or logic) and (ii) those, often working in and around stability theory, for whom model theory was also an end in itself. This separation is again only an approximation to the truth: there were people on both sides (and also on neither side), and already results of Zilber, Cherlin, Harrington and Lachlan, and Macintyre, had connected the pure theory with some basic structures of mathematics. In any case, the "separation" referred to above gave rise to a necessary and important internal development of the subject.

Even though there were good relations and mutual admiration between the different "camps", some people in group (i) were somewhat suspicious of what they saw as overtly set-theoretic preoccupations in Shelah's program and theory.

The last ten or fifteen years have seen a remarkable unification or even re-unification of these differing trends and emphases. One aspect is that the machinery and conceptual framework of stability theory has been brought to bear on the analysis of concrete structures in new ways. Related to this is that various notions/dichotomies in stability theory turn out to have meaning, not only for the general theory, but for the (outside) mathematical world. (Actually I am here talking not about stability theory per se but what one might call "generalized stability theory", the development of the machinery of "independence", dimension theory, orthogonality, in model-theoretic contexts both broader than and outside stable theories, such as simple theories and o-minimal theories.) Going the other way, a kind of sensitivity to the mathematical world, especially what one may call a "geometric sensibility" (complementing the usual "set-theoretic sensibility" characteristic of logicians) has influenced the pure theory.

As a result of these and earlier developments, model theory has assumed a rather new role, complementing the classical "foundations of mathematics". This is reflected in Hrushovski's description of model theory as "the geography of tame mathematics". I give no definition of "tame mathematics" or "tame structures". The real, p-adic, and complex fields are tame. The ring of integers (and field of rationals) are very nontame (or wild) as is any structure which interprets them. Making sense of the tame/wild borderline becomes a mathematical issue. Generalized stability theory tends to rule out the interpretability of wild structures.

Let me give a couple of examples of the unification referred to above. The first is the amazing journey from finite fields to the "Independence Theorem" for simple theories. James Ax established the decidability of the theory of finite fields in the 60's, using among other things the Lang-Weil estimates for the number of points on varieties over finite fields. In spite of much work on pseudofinite fields and their generalizations, pseudo-algebraically closed fields, the connection with abstract model-theoretic notions remained obscure. Shelah [49] introduced simple theories in the late 70's as theories without the "tree property" (a certain combinatorial property of formulas) generalizing stable theories (theories without the "order property"). His idea was that the machinery of stability theory (such as forking) might generalize to simple theories. Although Shelah made several crucial insights, the situation remained problematic, and the subject was not developed further until the mid 90's. In the early 90's, Chatzidakis, van den Dries and Macintyre, continuing Ax's work, gave a description of definable sets in finite fields (and thus in the limit, pseudofinite fields), associating to definable sets both dimensions and measures, and asking several questions (such as the status of "imaginaries" in pseudofinite fields). I remember receiving the preprint and leafing through it with wonder late one afternoon in Notre Dame. Hrushovski [30] went further than I did. He answered the questions, in a more general context, theories of finite S_1-rank, and proved the "Independence Theorem" for these theories, a result concerning the amalgamation of free extensions of types. In the meantime Kim [40] had shown that the basic theory of forking does indeed go through for Shelah's simple theories. Motivated by Hrushovski's work (as well as Shelah's earlier work), this Independence Theorem was proved for arbitrary simple theories, and was moreover observed to be a *characteristic* property of simple theories [41].

Another example is o-minimality. The notion of o-minimality was developed both as an abstraction of the properties of semialgebraic sets over the reals, and as an analogue of strong minimality in the presence of a total ordering (see [64]). In any case, if one allows the notion of a total ordering as belonging to logic, the classification of o-minimal structures is an issue also of pure logic. A theorem of Peterzil and Starchenko [47] recovers (expansions of) real closed fields from o-minimality. This should be considered as a foundational result in the new sense: from a notion of pure logic one recovers model-theoretically (expansions of) real algebraic geometry. Hrushovski and Zilber in an earlier paper [32] had already proved a similar result for "Zariski geometries", recovering algebraic geometry.

Major results of the 90's were Wilkie's proof of model-completeness (and o-minimality) of the real field equipped with the exponential function [66], and Hrushovski's proof of the Mordell-Lang conjecture for function fields in all characteristics [31]. Wilkie's ingenious proof made use of the general theory of o-minimality. There is continuing work on finding richer o-minimal expansions of the real field. Hrushovski's work was informed by almost all the accumulated results in stability theory, geometric stability theory and stability-theoretic algebra (differentially closed fields and separably closed fields). From this work and ongoing work by Hrushovski and others (such as Scanlon) one sees that the model-theoretic/stability-theoretic distinction between linear (or modular) and nonlinear (nonmodular) behaviour of definable sets has meaning in the world of geometry and number theory.

The terms "applied model theory" and "applied model-theorists" have been recently bandied around by various people, to describe in a blanket fashion much of the current work in model theory and its practitioners. I hope that the above discussion and examples show that this is just wrong and moreover completely misses the point. Although individuals may choose to view themselves as "applied", what is specific to current developments is *not* a shifting of attention to the external mathematical world, but the mutual interaction between external and internal points of view, and the corresponding enrichment of both.

There is now a reasonably coherent sense of what it means to understand a structure: it means understanding the category of definable sets (including quotients by definable equivalence relations). Generalized stability theory gives a host of concepts and tools which inform this analysis: dimension theory (the assignment of meaningful ordinal-valued dimensions to definable sets, invariant under definable bijection), orthogonality, geometries, definable groups and homogeneous spaces. As mentioned earlier, the contexts in which such tools are applicable tend to rule out Gödel undecidability phenomena. Interpretability is a key (even characteristic) notion, and in a tautological sense the business of "pure" model theory becomes the classification of first order theories up to bi-interpretability.

It is worth pointing out what some may consider paradoxical in foundations, model theory and the wild/tame distinction. From a classical foundational point of view the objects of mathematics which can be most immediately grasped are the "accessible domains" referred to in Sieg's talk (such as the set of natural numbers equipped with all its arithmetic operations). On top of these are built the set-theoretically more complicated objects of mathematics. In fact it is some of these *latter* objects (such as locally compact fields), which, once their set-theoretic genesis is forgotten, we have direct access to, via quantifier-elimination and decidability theorems. The accessible domains, such as number fields and their absolute Galois groups, although among the central objects in mathematics, remain mysterious in many ways. It is typical in mathematics to approach problems about these objects via tame objects (such as via the Hasse principle and its obstructions).

There are many important current areas of research in and around model theory which are not directly included in the above discussion. The model theory of modules has been a particularly active area. The Ziegler spectrum of a ring, originating from model-theoretic considerations (positive-primitive formulas) is now a key notion and tool in the representation theory of rings. In this case too, the stability-theoretic perspective has been important. Work on generalized quantifiers and infinitary logic continues, especially in the context of "nonstructure theorems" by Shelah and his collaborators. The subject "finite model theory", the study of definable classes of finite structures and definability in finite structures has also been rather active, with connections to computer science and complexity. In this context first order definability is often the wrong notion to consider and either fragments (such as first order logic with finitely many variables) or other logics are more appropriate. Even in the context of first order definability on infinite structures, nonfirst order considerations naturally arise, for example when one wants to consider type-definable sets and even their quotients by type-definable equivalence relations as structures in their own right. Although nonstandard analysis has long ago become a separate subject, model theory has been enriched by the development (by Keisler [38], Henson and others) of appropriate logics and tools for dealing with metric spaces, Banach spaces and the like.

The future. I will not try to predict developments but will limit myself to discussing a few "themes" (and problems) which are mostly related to the current developments discussed above. This is of course both limited and influenced by my own knowledge and preoccupations.

Foundations of model theory. What is the right language and level of generality for model theory? The traditional framework of one-sorted structures and their point-sets has long been recognized as being rather restrictive. The actual practice of model-theorists is somewhat more in line with points of view from categorical logic. Moreover a degree of flexibility is required to deal with various natural elaborations of and variants of first order definability.

Classification of uncountably categorical and related structures, up to bi-interpretability. This is a rather strong formulation of Zilber's program. In this form it will probably never be accomplished, but it subsumes an enormous amount of work in model theory: the geometry of strongly minimal sets, the mathematics around Hrushovski's amalgamation/fusion techniques, the structure of simple noncommutative groups of finite Morley rank (Cherlin's conjecture) and the theory of covers. Included in "related structures" are the structures of finite SU-rank, say, where much of the geometric theory has still to be developed.

Interpreted more loosely we could include here ongoing work in stability, its generalizations (such as simple theories), and the classification of first order theories.

Model theory and analysis/geometry. It is hoped that the second part of Hilbert's 16th problem (uniform bounds on the number of limit cycles of polynomial planar vector fields) can be approached by finding suitably rich o-minimal expansions of the real field.

The understanding of complex exponentiation is a major challenge, in particular Zilber's conjecture that the complex field equipped with the exponential function is "tame" modulo countable definable sets.

Bimeromorphic geometry is concerned with the classification of compact complex manifolds up to bimeromorphic equivalence. There is a hope that geometric stability-theoretic methods would yield nontrivial results here, although maybe it is too early to tell. There are intriguing connections with "o-minimal complex analysis".

I also include here further development of model theoretic techniques and notions appropriate for metric spaces, Banach spaces and function spaces, as well as applications.

Model theory and number theory. The kind of model-theoretic methods discussed in this article have not yet penetrated the central problems concerning rational points (namely over number fields) of varieties. This is a major challenge, and any progress would have to incorporate arithmetic features such as heights into associated model-theoretic structures.

On the other hand, various theorems about rational points (such as Mordell-Lang over number fields) have *equivalent* model-theoretic statements (although not as yet model-theoretic proofs). In fact we have a new twist on the notion "fragments of arithmetic": Fix a variety V defined over $\mathbf{Q}$ and let M_V be the structure $(\mathbf{C}, +, \cdot, V(\mathbf{Q}))$ (so we adjoin a predicate for the rational points of V to the complex field). Is it the case that M_V is either stable or undecidable? What are the possible Turing degrees of such structures? Are these questions settled by the Lang conjectures on varieties of general type?

Model theory and differential equations. I mean here the algebraic theory of differential equations and the structure of solution sets. Concerning ordinary differential equations, the fine structure of definable sets of finite Morley rank in differentially closed fields is relevant. A challenge is to extend finiteness theorems for equations of order 1 to the higher order case. For partial differential equations, infinite-dimensional sets (i.e. definable sets of infinite Morley rank in the appropriate structures) come into the picture, and are hardly understood at all model-theoretically.

Another important problem is to identify and axiomatize the universal domains appropriate for the kind of "asymptotic differential algebra" embodied in Hardy fields.

Finally, one would hope for model-theoretic methods (especially those discussed in this article) to be relevant to Grothendiek's conjecture in the arithmetic of linear differential equations.

Finite and pseudofinite structures. I am referring here to the (first order) model-theoretic study of infinite limits (in various senses) of finite structures, and the light this sheds on uniformities in families of finite structures. (So this is not exactly the same as so-called finite model theory.) The work on smoothly approximable structures (Lachlan, Cherlin, Hrushovski [5], Kantor, Liebeck, Macpherson and others) as well as work on pseudofinite groups and fields falls under this rubric. The content and implications of *pseudofiniteness* (being an ultraproduct of finite structures) is an important issue.

Hilbert's 10th problem over $\mathbf{Q}$. This is very much related to the number theory discussion above. The problem is whether there is an effective way of deciding, given a finite system of polynomials in several variables with rational coefficients, whether or not this system has a solution all of whose coordinates are rational numbers. Formulated logically it is the problem of the decidability of the existential theory of $(\mathbf{Q}, +, \cdot)$. (The full theory is undecidable.) Formulated geometrically it is the problem of deciding the existence of rational points on varieties defined over $\mathbf{Q}$. A negative solution would follow from being able to existentially define the ring $\mathbf{Z}$ in the field $\mathbf{Q}$. (A possible obstruction to this is a certain conjecture of Barry Mazur on the *topology* of rational points of varieties.) It is rather interesting that number theorists appear to favour a positive solution to the main problem. In fact in the case of curves (1-dimensional varieties), it has been conjectured that one can even *compute* the set of rational points.

Vaught's conjecture. Vaught's conjecture for first order countable theories remains open: a first order countable theory has either at most ω or exactly 2^{ω} countable models. One hopes for a renewal of the "approach from below" started by Shelah for the ω-stable case and continued by Buechler and Newelski for the superstable of finite rank case. It would be nice to see also an approach from above. In the more general context of $L_{\omega_1,\omega}$ theories, it is a special case of the Topological Vaught conjecture from descriptive set theory.

Logic and mathematics. One theme in the discussion following the panel presentation was: how can logic increase its prestige within mathematics and how does one go about making a "splash" which mathematicians will take notice of? My feeling is that this is the wrong sort of question. If one wants some kind of meaningful interaction with other parts of mathematics, it is the conviction that this is a worthwhile intellectual enterprise, rather than the desire to make a "splash", which is crucial. This conviction amounts essentially to a belief in the unity of mathematics. There has been much discussion of this "unity of mathematics" in recent times, often in connection with deep conjectures relating arithmetic, geometry, analysis, representation theory etc. One feels moreover that logicians, especially in the light of their foundational concerns, should have some level of engagement with these issues and conjectures. There is another sociological aspect. In so far as logicians live and operate within mathematics departments there is a need to talk to and interact with the people around them. So the issue is that of a sensitivity to mathematics and educating our graduate students accordingly. I believe that with such a sensitivity, interactions and "splashes" will take care of themselves, and our subject, or rather its various branches, may end up being transformed in the process.

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§5. Set theory, by ALEXANDER KECHRIS.

A) I will present here some speculations on future directions in set theory. Modern set theory is a vast and very diverse subject, so it is obvious that in a short time I cannot possibly cover all important aspects of research in this field. I will also concentrate on discussing, in fairly broad terms, general programs and trends, as opposed to specific problems, with some obvious exceptions.

B) For the purposes of this presentation, it will be convenient for me to distinguish two aspects of research in set theory:

The first, which I will call *internal* or *foundational*, is concerned with the understanding and clarification of the basic concepts of set theory itself, and aims at providing a foundation for a comprehensive and satisfactory theory of sets. Since the time of Cantor, set theory has been continuously evolving towards that goal and this trend will undoubtedly continue in the future.

The second aspect, which I will call *external* or *interactive*, deals with the connections of set theory with other areas of mathematics. This includes the use of set theoretic concepts, methods, and results in establishing the foundations or helping the development of other mathematical disciplines as well as the application of set theoretic techniques in the solution of specific problems in such areas.

Of course, these two aspects, internal and external, are often closely interrelated.

C) Also, following a well-established tradition going back to Cantor, it will be useful to subdivide the theory of sets into (i) The *theory of the continuum* or *theory of pointsets*, i.e., the study of sets and functions on the reals, complex numbers, Euclidean spaces or, more generally, Polish (complete separable metric) spaces, and (ii) the *general set theory* of arbitrary sets and cardinals.

An important further distinction in the theory of the continuum was introduced in the early 20th Century by the French, Russian, and Polish analysts, who laid the foundations of *descriptive set theory* or *definability theory of the continuum*, which is the study of definable (e.g., Borel, projective, etc.) sets and functions on Polish spaces. So we can subdivide the theory of the continuum into *descriptive set theory* and the *theory of arbitrary pointsets*. For example, a question such as the measurability of the projective sets belongs to the first part but the Continuum Hypothesis (CH) or the study of cardinal characteristics of the continuum belongs to the second.

Again all these aspects of set theory are closely interrelated. With these classifications in mind, I will now discuss some prospects for research in set theory.

Descriptive set theory.

A) Work in the last 30 years or so has resulted in a resolution of the foundational (internal) issues facing descriptive set theory. There is now a very satisfactory and comprehensive foundation for the theory of definable sets and functions on Polish spaces, based on the principle of *Definable Determinacy* (see Kechris [36], Moschovakis [44]). This theory also fits beautifully within the framework of global set theory as currently developed through the theory of large cardinals. The determinacy principle is in fact "equivalent", in an appropriate sense, to the existence of certain types of large cardinals (see Martin-Steel [43], Woodin [67]). Moreover the structure theory of definable sets in Polish spaces, that determinacy unveils, has a very close and deep relationship with the unfolding inner model theory of large cardinals, an example of which was so vividly illustrated in Itay Neeman's talk in this conference.

B) Thus the foundational aspects of descriptive set theory are by and large settled now. Research in this area is now increasingly preoccupied with external issues. These consist of applying the ideas, methodology, and results of descriptive set theory, both in its classical and modern manifestations, to other areas of mathematics, while at the same time developing new directions in the theory itself which are motivated by such interactions. Interestingly, this also leads to the use of sophisticated methods and results from other areas of mathematics in the solution of purely set theoretic problems in descriptive set theory.

Intriguing such connections have been discovered during the last 15 years or so in areas such as classical real analysis, harmonic analysis, Banach space theory, and ergodic theory (see, for example, Foreman et al. [19], Kahane-Salem [34], Kechris-Louveau [37]). More recently, a very promising new area, that is now very actively investigated, deals with the development of a *theory of complexity of classification problems in mathematics*, a classification problem being the question of cataloging a class of mathematical objects up to some notion of equivalence by invariants, and the closely related theory of *descriptive dynamics*, i.e., the theory of definable actions of Polish groups on Polish spaces (see Becker-Kechris [1], Hjorth [28], Kechris [35]). This work brings descriptive set theory into contact with current developments in various areas of mathematics such as dynamical systems, including ergodic theory and topological dynamics, the theory of topological groups and their representations, operator algebras, abelian and combinatorial group theory, etc. Moreover, it provides new insights in the traditional relationships of descriptive set theory with other areas of mathematical logic, as, for example, with recursion theory, concerning the global structure of Turing degrees (see P. Cholak et al. [6]), or with model theory, through the Topological Vaught Conjecture and the general study of the isomorphism relation on countable structures.

Moving beyond descriptive set theory, I will concentrate on two other major aspects: the *theory of large cardinals* and the *theory of small cardinals*.

The theory of large cardinals.

A) The goal of the theory of large cardinals is to understand the higher reaches of infinity and their effect on its lower levels. Another important aspect here is the use of large cardinal principles as a global scale for calibrating the consistency strength of extensions of classical ZFC set theory.

Most of the effort in this area today is going towards the internal or foundational aspects of this theory, where a vigorous and far reaching program is actively pursued, dealing with the development of canonical inner models for large cardinals and the detailed analysis of their structure, as well as their relationship with descriptive set theory (see Löwe-Steel [42], Steel [61]). This program is by no means complete yet, and it will undoubtedly be one of the main topics of set theoretic research in the foreseeable future. It is also closely interrelated to many other important directions of research in set theory, including infinite combinatorics and the development of forcing techniques (see the forthcoming Foreman et al. [18]).

B) Simultaneously with the pursuit of the foundational goals, there have been several interesting external developments here as well. There is of course a long tradition of application of set theoretic techniques, involving for example, forcing, infinite combinatorics as well as large cardinals, to many areas of abstract algebra, functional analysis, measure theory and general topology, for instance in obtaining significant independence and consistency results, as for example in the Whitehead Problem (Shelah; see Shelah [48]), the Kaplansky Conjecture (Dales, Esterle, Solovay, Woodin; see Dales-Woodin [12]), or the S- and L- space problems (see, for example, Todorcevic [62]), and this will of course continue in the future. More recently, large cardinal theory is finding its way into more concrete situations. H. Friedman (see, for example, Friedman [21]) applies combinatorics of large cardinals to obtain new combinatorial principles for finite sets. Moreover he shows that these principles require, in an appropriate sense, these large cardinal hypotheses. Another interesting direction relates the structure of elementary embeddings associated with large cardinals to that of self-distributive algebras and braid groups, through work of Laver, Dehornoy, and others (see Dehornoy [13]).

The theory of small cardinals.

A) In this context, I include both the study of arbitrary pointsets, in particular problems such as the CH, as well as the theory of the "small" alephs $\aleph_1, \aleph_2, \ldots$

Here the foundational situation is far from clear. The theory of large cardinals has many important implications here, in particular in terms of consistency and independence results (see, for instance, Foreman-Magidor-Shelah [20]). However, it is well-known that in its present form, which is largely immune to forcing constructions, it does not resolve key issues such as the CH. It is clear that a satisfactory and comprehensive theory of small cardinals needs to be developed, within which we can hope to achieve the resolution of this basic set theoretic problem and related questions.

B) A promising new approach along these lines has been recently initiated by Woodin (see Woodin [68]), which aims at developing a theory that leads to a "complete" understanding of the definability structure of the power set of ω_1, $P(\omega_1)$, which will parallel the "complete" understanding of the definability structure of the power set of ω, $P(\omega)$, based on the principle of definable determinacy. Towards developing such a theory, Woodin proposes a new principle, concerning the definability structure of (an enriched form of) $P(\omega_1)$, which implies the failure of the CH, in fact it gives the answer $2^{\aleph_0} = \aleph_2$ for the value of the cardinality of the continuum. It is of course too early yet to know the full effect of this theory, and whether it will be eventually viewed as the "right" theory for the definability structure of $P(\omega_1)$, finally leading to a satisfactory resolution of the CH. This will require a much more detailed development of the theory than is presently available, and should be the focus of extensive research in the future. Even if this turns out to be successful, further questions concerning the theory of arbitrary pointsets and the structure of small cardinals would need the development of a theory of $P(\omega_2), P(\omega_3)\dots$, for which no hints are available at this stage.

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