SOME SET THEORIES ARE MORE EQUAL

MENACHEM MAGIDOR

Preliminary Draft

1. The shock of of independence

The proof of independence of the Continuum Hypothesis by Paul Cohen in 1963 and the avalanche of additional independence results was the trigger to a very intensive discourse about the future of Set Theory and its foundational role in Mathematics. It will be only fair to say that while the invention of forcing initiated the blooming of Set Theory as a technical mathematical discipline, it had a shocking effect on the perception of Set Theory as the framework in which all of Mathematics (or at least a major part of it) can be formalized.The prevailing tone of these reactions is that Set Theory (at least as formalized by the Zermelo-Fraenkel axioms, denoted by **ZFC**) lost its uniqueness.What made the shock even more dramatic was the fact the proof of the independence of the Continuum Hypothesis showed that **ZFC** gives us very little information about the size of the Continuum and as far as the axioms are concerned there is a huge range of possibilities like

$$2^{\aleph_0} = \aleph_1, 2^{\aleph_0} = \aleph_2 \ldots 2^{\aleph_0} = \aleph_{211086} \ldots 2^{\aleph_0} = \aleph_{\omega+17} 2^{\aleph_0} = \aleph_{\omega_1}$$

. Here are two quotes from many:

> Such results show that axiomatic Set Theory is hopelessly incomplete. . . If there are a multitude of set theories then none of them can claim the central place in Mathematics. (Mostowski-1967 [29])
>
> Beyond classical analysis there is an infinity of different mathematics and for the time being no definitive reason compels us to chose one rather than another. (Dieudonné-1976 [**?**])[1]

This shock is somewhat puzzling because the fact that **ZFC** is not complete and therefore contains some independent statements was well known in view of Gödel's incompleteness theorems. Gödel himself in the early version of [15] is confident that **CH** is independent of **ZFC**.

[1] I do not know what Dieudonné means by Classical Analysis , but any reasonable attempt to delineate "Classical Analysis" will also run into independent statements.

But some how the mathematical community at large believed that "real" Mathematics is immune from independence and the problems mathematicians are really interested in are not of the kind of "esoteric" "artificial" independent statements produced by the proof of Gödel's incompleteness theorem.

A typical reaction of a certain mathematical community when one of its central problems was shown to be independent is to try and ostracize the offending problem by labeling it as "ill defined" "vague " etc. An example of this attitude is the reaction to Shelah's proof of the independence of Whitehead's problem where a problem that seems natural and well defined was suddenly declared to be the wrong problem.[2] I find this kind of reaction somewhat intellectually dishonest.

The central issue of the discussions thus far in this series of talks was pluralism versus uniqueness or at least selectivity in Set Theory. Should we accept multitude of set theories, each one with its own peculiar set of axioms or should there be one or small number of canonical Set Theories which are preferable to others and which provide a reasonable level of completeness by deciding a substantial class of otherwise independent problems? If we opt for the non pluralist option, how do we choose the "right" or the "preferable" Set Theory? (Note that asking for the "right" Set Theory" is different question than asking for the preferable Set Theory.) Is this choice relevant at all to the working mathematician? In this talk I'll try to argue for anti pluralist position . I'll try to claim that the choice of the underlying Set Theory is relevant to the mathematical work and that we can develop insights and criteria which will lead us in the process of finding the right or the preferable assumptions for Set Theory. This process can well lead us to finding the preferable answer to central independent problems like the Continuum Hypothesis. The non pluralist position was argued very convincingly here in this series by Woodin and by Koellner , (in [21],[42]).Our arguments will be somewhat different. Woodin and Koellner were mainly relying on arguments which under the classification initiated by Gödel [15] can labeled as intrinsic arguments. We shall give stronger weight to extrinsic arguments,namely relating to the impact and the connection of Set Theory with other fields of mathematics and the natural sciences. This is natural for us since we are close to the Naturalism of Maddy [23],[24],[25]).

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[2]My favorite statement of Whitehead problem is that "A compact Abelian pathwise connected topological group is a product of copies of the unit circle". One can feel the feeling of surprise, even shock, for the language of the anonymous author of the Wikipedia article on "h"

2. Should we accept multitude of set theories?

An extreme pluralistic position was presented in this series by Hamkins ([17] who argued for the multiverse view of Set Theory.Namely when we talk about sets we are not talking about a fixed universe of sets in which for instance **CH** is either true or false but we envision simultaneously diverse concepts of sets or a multitude of universes which may have some relations like one being a forcing extension of another.Hamkins makes a strong analogy between Set Theory and Geometry: in the same sense that there is a multitude of geometries that are all legitimate objects of study for the geometer who is committed to classifying and finding all the sorts of geometries that are available , so the set theorist should study the different possible set universes that coexists in the multiverse.Geometry is not unique in this sense, the group theorist domain contain many different groups:Abelian and non Abelian, free and non free etc. And there is nothing troubling about the fact that many natural statements in group theory are "independent" in the sense that there are groups that satisfy the given statement and there are groups that violate the given statement. Why should Set Theory be different?

I think that the answer lies in the Mathematical goal of the subject. Studying different possible models of **ZFC** is a great fun and definitely has mathematical interest , not unlike classifying possible groups. But the main motivation of studying Set Theory is still its foundational role: creating a framework in which all of Mathematics (or at least a major part of it) can be included under a uniform system. Of course the Number Theorists does not think about numbers as sets , the algebraist does not think about groups as sets and the analyst does not think on real valued functions as sets, still the fact that the numbers , groups and real valued functions can be construed to be members of the same universe, obeying the same rules is the most important reason d'etre of Set Theory. So in some sense deciding about the universe of Set Theory in which we assume we live is like deciding on the rules of the game for Mathematics. It is no coincidence that Dieudonné in the quote above is bothered by having infinitely many possible mathematics , not by infinitely many possible groups or geometries. The specter of having multitude of set theories , hence a multitude systems of mathematical rules of the game , is as troubling as imagining a city with different set of traffic rules for every street.

Let us accept for the sake of argument that the truth value of different set theoretical statements are all relevant to particular problem of group theory . (We shall argue in the next section that this is a definite possibility). Now our group theorist when she states the theorem she proved she will have to specify, on top of the assumptions about the group , the set theoretical universe in which the action takes place. To a limited extent it is already being done when mathematicians state theorems by using **CH** or Martin's Axiom (**MA**) but if in the course of time (like we believe will happen) more set theoretical methods will be used more and more the exact set theoretical assumptions used will become additional burden on mathematical communication.My guess is that the Mathematical community can tolerate a limited small number of set theories but will find it awkward to choose the appropriate set theoretical assumptions from a long list of possible universes or theories. It is up to the Set Theorists to sort out the different possibilities and to pick the Set Theory or the small number of set theories which will be more fruitful or "truer". If we continue Hamkins' analogy to Geometry: the main goal of the set theorists is not exactly like the goal of the pure geometer whose goal is to study all possible geometries but it closer to the goal of the geometer that together with the physicists try to pick the geometry which is most appropriate for describing the physical space.To some extent the relation between the set theorist and the the rest of the mathematical community is like the relation between the geometer and the physicist.

The reader of this article may have noticed that I avoided any attempt to to argue against pluralism in Set Theory by using ontological arguments, like "realism in ontology" or "realism in truth values" in the sense of Shapiro[36].Like many working Mathematicians I am basically a realist of and I do believe in some kind of objective existence of mathematical concepts, but I also believe that one can follow a rational process of finding the favored Set Theory (which for a realist like me will be the "correct set theory") without making any firm ontological commitments. I am sympathetic the argument of Maddy in [25] that by her terminology, the difference between the thin realist and arealist does not reflect on the the methodology in which they will analyse the practice of Set Theory.

Of course one can argue, like Feferman in his talk in this series and elsewhere [8],[9] that "higher" Set Theory is irrelevant to the Mathematical practice (or the particular choice of axioms) and anyway most of the Mathematics being done today can be done in some weak constructive system. We shall try to deal with this argument in the next section .

3. Does it Matter?

If the main mathematical goal of Set Theory is to provide foundational framework for Mathematics then the claim that all or most of the mathematical practice can be formulated in a much weaker system is a challenge to the relevance of the effort to find extensions of **ZFC** that will decide many of the otherwise undecidable problems. If there is no chance that the working Mathematician will use any thing more that a tiny fragment of **ZFC** then it seems futile to try and decide on the right extension of **ZFC**.My counter argument to this claim is that even if it true that 99% of the mathematics done today can be done within a subsystem of **ZFC** it does not mean that it will be stay like that for ever. There are many cases in Mathematics that methods and tools that were never considered to be relevant to a particular class of problems were suddenly recognized as essential for solving them. There are cases in which it took decades before the realization of the fruitfulness of the "external" tools . (A typical case is the use of group theory in number theory.). There are parts of mathematics which obviously use substantial segment of the power of **ZFC** and these subjects did run into independent problems. (The Whitehead problem in the theory of Abelian groups is a typical case) . But since it is common to label a field of mathematics that faces independent problems as being essentially "set theoretical" we shall concentrate on "down to earth" fields like number theory or analysis.

Formally , in view of the incompleteness theorem , we know that there are very simple problems of number theory or analysis can not be decided in **ZFC**. One can hear the claim that the real problems that are on the agenda of the working mathematicians will be never be of this kind. I find it very hard to accept . In some sense this claim is kin to feeling before the discovery of forcing that natural problem like **CH** is not independent.

Consider for instance the open problems in number theory like the twin numbers problem (which is a Π^0_1 statement) , the Goldbach conjecture and the $P \neq NP$ (both are Π^0_2 statements).There does not seem to be any formal criteria that will differentiate between these problems and the independent problems produced by the proof of the incompleteness theorem. Similarly the statement of Borel determinacy looks very similar to statements studied by analysts. (In fact the study of Borel determinacy was initiated by non set theorists). The claim that the basic problems studied by number theorists or analysts will never turn out to be independent assumes that the practicing mathematicians has some mysterious intuition that saves them from spending their efforts on independent problems . I find it hard to believe, so I believe that sooner ar later the working number theorists will formulate as problems that they will consider to be well defined and natural but are independent of **ZFC**.Feferman in [9] argues that supposing that additional set theoretic assumptions will prove a Π^0_2 statement not provable otherwise then the statement follow from the 1-CON of the extended theory and it is really the assumption made (similarly for more complicated number theoretic statement , if it is proved from a certain theory , containing **PA**, then it follows from the ω consistency of the theory.)[3].This is true but as Steel argues in his section of [9] this is in way dodging the issue because typically the way to justify the number theoretic statement "The theory we consider is 1-CON" is to derive it from the extension of **ZFC** which presumably was not picked arbitrarily but following some justification along the lines that we explore in the next sections.This objection to the potential use of higher set theoretical methods to settle problems of number theory is even less effective when you consider problems of analysis . The problems of the structure of Projective sets like their Lebesgue measurability or Projective Determinacy looks so much like genuine analytic problems , that it is difficult to assume that the working analyst will ,in the course of his studies,avoid running into similar problems that require additional axioms to be settled.

Even if a particular problem can be solved in principle in a weaker system, it is many times the case that the first time the proof is discovered or the more natural and simpler proof is discovered in a stronger system. An illustrative case is the story of Wiles proof of Fermat last theorem. The original proof used Grothendieck's universes , hence formally it assumed the existence of inaccessible cardinals. As everybody expected they can be eliminated but the point is that Wiles constructed his proof it came naturally for him to make the assumption that formally moved him away from **ZFC**. The interesting twist is that when I talked to several number theorists about the project of getting the proof in a weaker system like **ZFC** or **PA** they were not interested! The assumption of the existence of Grothendieck's universes (hence the assumption of the existence of unboundedly many inaccessible cardinals) seems to them such a natural extension of **ZFC** that having a proof of this Π^0_1 statement in this theory looks like good enough ground for believing the truth of the theorem and an attempt to eliminate the

[3]1-CON of a theory is the statement that the set of the Σ^0_1 of the theory is consistent.

use of the stronger axioms looks to them like an unnecessary logicians' finicking.

There are other examples like Martin's proof of Borel Determinacy which initially was inferred from his proof of analytic determinacy using measurable cardinals and the Wadge Borel determinacy , which looks like any genuine statement studied by analysts, which was proved originally from Borel determinacy, hence used **ZFC** in a strong way but was later proved in second order arithmetic.

In summary I think that it is very likely that the working mathematician will run into problems that the only way to settle them will be by expanding the usual axiom system of Set Theory. And the mathematical community will accept such a solution as legitimate if the set theoretical principles used will be accepted as part of a canonical widely accepted system of axioms.

We , as set theorists , face the problem of how to choose among the multitude of possible set theories those that will be mathematically fruitful,and acceptable in the long run. The next sections will chart some of the considerations that can lead us in this process.

4. The search for new axioms

We are going to present several criteria or principles that should serve as guidance in the search for new axioms. These are rather informal principles which leave a large leeway in their interpretation in a particular case.We see the search for new axioms as a ongoing process , not dissimilar to the process in other fields of science , by which a scientific theory is crystalized by a sequence of trials and errors, where at any particular moment there may be several competing options. The criteria of testing a particular axiom or a set of axioms is very likely very different form those used in Science but there are similarities. The famous quote from Gödel [15] is very appropriate introduction to the process we envision.

> . . . Even disregarding the intrinsic necessity of some new axiom. . . a probable decision about its proof is possible also . . . by studying its success. Success here means fruitfulness in consequences. . . There might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole field and yielding such powerful methods for solving problems . . . that, no matter whether or not they are intrinsically necessary, they would have to be accepted at least in the same sense as any well-established physical theory

Feferman in [8] makes the clear distinction between structural axioms whose role is to organize and expose a body of mathematical work and there is a great freedom in their formation and foundational axioms which are very basic to the concepts studied. For the later he quotes as a requirement the definition of the word "axiom" from the Oxford English dictionary

> A self-evident proposition requiring no formal demonstration to prove its truth, but received and assented to as soon as mentioned.

We think that this is too restrictive because the process by which an axiom is accepted as a fundamental and natural maybe a long process of reflection. The axiom can be foundational but still not "received and assented to as soon as mentioned". I guess that any axiomatization of Quantum Mechanics will start from the axiom that the set of states of a physical system is the set of one dimensional subspaces of Hilbert space. This is universally accepted by physicists but it had to go through a long process before being "received and assented". Still we believe that an axiom in order to be adapted has to conform the concept under study. So we put it rather vaguely that

> The new axiom should have intuitive or philosophical appeal. It should conform to some mental image of the basic concepts of Set Theory.

Some semi-serious way of phrasing this principle is that a good axiom needs a good slogan in order to be adapted. Of course the intuitive mental images are not always a reliable guide to fruitfulness or truth but on the other hand we should not underestimate them as important source for insights on the basic concepts.

Independence was the motivating force for introducing new axioms therefore it is natural that we shall expect that

> The new axiom should be strong enough to decide a large class of statements which are undecidable on the basis of the axioms adapted so far.

When the axiom decides a class of problems we would prefer that it gives a coherent structure to these problems.

> The Axiom should produce a coherent elegant theory for some important class of problems.

A well known example of interplay of the last two principles is the the decision between **PD** and $\mathbf{V} = \mathbf{L}$. Both them provides a very rich structure theory of the projective sets of reals but where the structure of the projective sets given by **PD** is much more elegant and coherent than the structure given by $\mathbf{V} = \mathbf{L}$. When **PD** was suggested for the first time I think that it lacked somewhat as far as the first principle : of fitting some intuitive mental image of the universe of sets. But the work of Martin-Steel and Woodin which derived **PD** and stronger determinacy statements from large cardinals increased its intuitive appeal substantially .

The next principle is along the lines of the passage form Gödel quoted above.

> To the extent possible the axiom should have testable verifiable consequences.

Here we have to explain what do we mean by "testable verifiable" consequences. We are not talking about experimental science so the "verification" is a mathematical verification. So for instance if the axiom has some new Π^1_0 consequences for number theory then the fact that so far we did not get a counter example is a verifying fact. (Of course this could change with time but this is not different than any scientific theory.) An evidence for the axiom could be a result that we intuitively believe that it is true and we were not able to derive without the new axiom. I think that it is even possible that axioms could be tested by their impact on fields outside of mathematics like physics. It may sound like an outrageous speculation and admittedly we do not have any concrete example of such a possible impact , but in the next section we shall give an example where the set theory we use may have some relevance to the mathematical environment in which a physical theory is embedded.

The standard way in which we generate different set theories is by forcing . The forcing extensions of a given universe of **ZFC** can have properties which are very different from the properties of the original universe. But some properties are resilient under forcing extensions. (We are talking about set forcing. As shown by Jensen followed by S. Friedman and others that using class forcing we can make dramatic changes to the properties and the structure of the ground universe.) For instance if $0^\#$ exists in the ground model it still exists in any generic extensions. If the ground model has a proper class of one of the standard large cardinals notions then a set forcing extension does not change this fact. A very remarkable generic absoluteness result is the result of Woodin showing that the theory of $L[R]$ is absolute under forcing set forcing extensions, provided the universe contains a proper class of Woodin cardinals. While we agree with Foreman in [13] that the generic absoluteness can not by itself be an argument for adapting a new axiom, we still think that having resilient axioms is a very desirable property , so if we consider different axioms that could be supported by the previous principles, then having generic absoluteness provides additional appeal to the axiom.

> If possible the axiom should be resilient under forcing extensions.

5. Can Set Theory be relevant to Physics?

In the previous section we presented the process of search for new axioms which is not dissimilar to the formation of theories in any branch of science. Can we carry this analogy even further and the decide between different set theories on the basis on their scientific consequences, say in Physics?

As to be expected we do not have any definite case in which different set theories have an impact on physical theories but we believe that the possibility that may happen in the future is not as outrageous as it may sounds . Here are two examples which relates to basic issues in Quantum Mechanics. Two famous arguments against hidden variable interpretation of Quantum Mechanics are the Bell Inequalities ([4]. See also [38]) and the Kochen-Specker Theorem. ([19]).

Bell inequalities imply for instance that for spin $\frac{1}{2}$ particles , the correlation which is observed between the measurements of the spin of two entangled particles along different axis can not realized by a function that assigns to any direction in space a definite value which is the value of the spin along the given direction. (We identify directions in space with points on the the two dimensional unit sphere S^2.) . A natural requirements form the function (which does not exist!) is that if will be measurable with respect to the usual measure on S^2. Pitowsky in a series of papers ([32],[33],[34]) showed that if one allows a larger class of functions then one can have a deterministic model realizing correlations which violates the Bell inequalities . (The class of functions Pitowsky considered were still nice enough so that we can still talk about integration, correlation etc.) But in order to show that a spin function realizing the Quantum Mechanical statistics he assumed **CH** or **MA** (Martin's axiom). The use of additional axioms is necessary in view of the following result:

Theorem 5.1 (Farah,M.[7]). *If the continuum is real valued measurable then Pitowski's kind spin function does not exists. The same holds* *in the model one gets from any universe of* **ZFC** *by adding* $(2^{\aleph_0})^+$ *random reals.* [4]

Similar situation exists with respect to the Kochen-Specker theorem. It deals with spin 1 particle. It follows from Quantum Mechanics that for such a particle and given three mutually orthogonal directions in space $\vec{X}, \vec{Y}, \vec{Z}$, while one can not measure simultaneously the value of the spin along this three axis ,(The corresponding operators do not commute), one can still measure simultaneously the *absolute* value of the spin along the three given axis and then the three values we get are always two 1's and one 0's. The Kochen Specker theorem claims that such behavior can not be realized by a deterministic function. i.e. there is no function F on S^2 which gets the values $0, 1$ which satisfy the conditions:

(1) For all $\vec{X} \in S^2$ $F(-\vec{X}) = F(\vec{X})$
(2) For every triple of mutually orthogonal vectors in S^2:$\vec{X}, \vec{Y}, \vec{Z}$ $F(\vec{X}) + F(\vec{Y}) + F(\vec{Z}) = 2$.

(The theorem is really a finitary statements and hence there are no restriction on the function F).Call such a function A KS function.

Pitowsky in [34] suggested relaxing the requirements above on the function F so that clause (2) is assumed to hold almost always in the following sense : For a fixed vector $\vec{X} \in S^2$ the set of $\vec{Y}$ such that if $\vec{Z}$ the a vector orthogonal to $\vec{X}$ and $\vec{Y}$ then $F(\vec{X}) + F(\vec{Y}) + F(\vec{Z}) \neq 2$ is of measure 0 with respect to the usual measure on the great circle perpendicular to $\vec{X}$. As before Pitowsky shows that under Martin's axiom **MA** *there does exists* a function on S^2 satisfying the modified requirements. Call such a function a PKS function. Similarly to the last theorem we are able to prove

Theorem 5.2 (Farah,M.)**.** *If the continuum is real valued measurable then there is no PKS function on* S^2 *does not exists.The same holds in the model one gets from any universe of* **ZFC** *by adding* $(2^{\aleph_0})^+$ *random reals.*

The functions that Pitowsky constructs above are not measurable and so probably will rejected as having physical meaning in the same sense that the partition of the sphere given by the Banach-Tarski paradox lacks physical meaning , but the Pitowski functions are not as pathological and there is some variation of measure theory in which these functions are rather well behaving. As Pitowsky says in [32]:

[4]Shipman in [39] announced that the same conclusion is consistent with **ZFC**. We were not able to reconstruct his arguments neither directly nor after corresponding with him).

> ...We can conceive of mathematical situations where natural concept of probability emerges which is not captured by the usual axioms of probability theory.What I have in mind is not a radical extension of probability ...but rather a conservative extension...

We can envision a development in physical theories which will encompass a larger class of functions than accepted today as legitimate functions. So it will not be a complete absurdity to ask whether a function satisfying the Pitowsky condition exists. So Physics could be potentially partial to the set theoretic context in which its mathematical modeling is takeing place.

While being a wild speculation it is not impossible that Scientific Theories will prefer one set theory over others because it makes the scientific theory simpler and more elegant. It may even be possible that in order to derive certain experimentally testable results one would have to prefer one set theory over others. I am not claiming that it is likely to have an experimental test that will decide between different Set Theories but that we will be able to compare between different Set Theories according to what type of mathematical hinterland they provide for theoretical Physics. I believe that it is possible.

6. Some paths to follow

In previous section we described several criteria for evaluating potential extensions of **ZFC** , hopefully leading to the preferable set theory that will decide many of the outstanding independent problems. A well known success story that meets our criteria is the series of strong axioms of infinity.They definitely fit an appealing mental image of the universe of set theory going into larger and larger levels. Many of these axioms can be justified as reflection principles. (We mean reflection principles in somewhat different sense than Koellner in [20] , so his negative result there does not apply) .

Definition 6.1. *Given a property of structures in a fixed signature such that the property is invariant under isomorphism of structures. A reflection cardinal for the given property is a cardinal κ such that every structure in the given signature has a substructure of cardinality less than κ having the property.*

For instance if the property is first order then $\aleph_1$ is a reflection cardinal for the property.(This is of course the Löwenheim-Skolem theorem.) The existence of a supercompact cardinal is equivalent to the existence of a reflection cardinal for every property which is expressible in second order logic. The ultimate reflection principle is the assumption that every property of structures has a reflection cardinal and it is equivalent to well known Vopenka's principles which implies the existence of unboundedly many extendible cardinals.[5] My favorite slogan for this principle is that the universe of sets is so large that every proper class of structures of the same signature must contain repetitions in some sense.

The large cardinals axioms are success story because they decided a substantial class of undecided statements, the most noteworthy class is the theory of L[R] which contains the theory of projective sets and it provides a very elegant and coherent theory . In the presence of the large cardinals this theory is resilient under forcing extensions. As far as verifiable consequences I consider the fact that these axioms provides new Π^0_1 sentences which so far were not refuted. In some sense we can consider these Π^0_1 sentences as physical facts about the world that so far are confirmed by experience .Unfortunately they do not decide some central independent problems like the Continuum Hypothesis. In this section we shall study three potential paths of extending **ZFC** and shedding light on the Continuum Problem. In view of the success of strong axioms of infinity we take for granted that any extensions of **ZFC** that is considered, is compatible with the standard axioms of strong infinity.

We shall consider three directions in which **ZFC** can be extended and which shed light on the possible values of the continuum. These directions involve unproved conjectures so the decision about their efficacy will have to wait further development. This is not unusual in any scientific discipline that we have some promising theories, some of them competing and the decision about them is delayed till further evidence is available.

6.1. **The Ultimate L.** The mental image for any L like model ("canonical inner model") is that the the universe of sets is constructed by a sequence of stages where each stage is obtained from the previous stage by an operation which is definable in a very canonical way. "canonical " here is rather vague but for instance if each stage is the power set of the previous stage , like in the definition of the V_α's , then it is not "canonical" enough . We can too easily change the meaning of this operation , for instance by forcing. On the other hand taking the next stage as the definable subsets of the previous stage , like in the definition of the the L_α's is very canonical.The definition that is being used in the definition of the inner models for the large cardinals

[5]This is essentially an unpublished result of Stavi. See also [40]

constructed thus far, where at each stage you throw in some rather canonical objects like the minimal (partial) extender missing on the sequence constructed thus far seems to fit the intuitive picture of being canonical.

The obvious example for such a L like universe is L it self. It has the advantage of deciding most of the interesting independent problems. The disadvantage of L is that the direction it decides the independent problems is usually in the less elegant and coherent direction. In some sense "L is the paradise of counter examples". In view of our decision of adapting any of the reasonable large cardinals axioms , L should be dropped as an option because it rules out rather mild axioms of strong infinity.

The alternative is to adapt some inner models for large cardinals, where the slogan for justifying it is that if while we may want larger and larger cardinals to exist in our universe, we want only sets that are necessary in some sense. (Given all the ordinals.) .The most appealing inner model is the "Ultimate L" suggested by Woodin. ([45]) which is supposed to be a the canonical inner model for supercompact cardinal, but it has the pleasant feature that it catches stronger cardinals , if they happen to exist. The construction of the ultimate L is still a work in progress and if successful then the axiom "V=Ultimate L" maybe a very strong competitor for the set theory that decides **CH** (It decides **CH** in the positive direction) but I have my doubts for several reasons.

It is very likely that the Ultimate L, like the old L, will satisfy many of the combinatorial principles like $\diamondsuit_{\omega_1}$. These principles are usually the reason that "L is the paradise of counter examples". They allow one to construct counter examples to many elegant conjectures . (The Souslin Hypothesis is a famous case). In the next subsection we shall try to suggest as intuitive principle that the universe of sets should be as rich as you can reasonably expect. that this principle obvious conflicts with the axiom "V=Ultimate L".I am going to take a greater risk by stating a conjecture that, if true, will violate the possibility of building the ultimate L, at least along the lines considered now.

The conjecture has to do with the absoluteness of different set theoretical theories under forcing extensions. As we mentioned above, one of the most remarkable features the work done in the late 80's on the connection of **AD** with large cardinals is that in the presence of a proper class of Woodin cardinals , the theory of $L[R]$ is invariant under forcing extension. In particular there is no well ordering of the reals which is definable in $L[R]$. A stronger result is the following:

Theorem 6.2 (Woodin)**.** *Assume that there is a proper class of measurable Woodin cardinals and that* **CH** *holds. Let* Φ *be a* Σ^2_1 *sentence. (Namely* Φ *has the form "There is a set of reals* A *such that* $\Psi(A)$ *holds where all the quantifies in* Ψ *are over reals.) Then* Φ *is true in the ground model.In particular under our assumptions there is no* Σ^2_1 *well ordering of the reals.*

So the theorem claims that if **CH** is true (and we have the appropriate large cardinals) then the Σ^2_1 sentences which are are true is maximal among all forcing extensions of our universe.Results of Abraham and Shelah ([2],[3]) show that this result is the best possible in the sense that for any model of set theory there is a forcing extension not adding any reals (hence preserving **CH**) and introducing a Σ^2_2 well ordering of the reals. Also **CH** is necessary because over any model of set theory one can introduce by forcing a Σ^1_1 well ordering of the reals. Naturally one may ask about possible generalizations of the last theorem to Σ^2_2 sentences. In view of the Abraham-Shelah results one should replace **CH** by a stronger statement. We make the following conjecture:

Conjecture 6.3. *(Under the assumption of the appropriate large cardinals) Assume that the combinatorial* $\diamondsuit_{\omega_1}$ *holds and let* Φ *be a* Σ^2_2 *sentence, then if* Φ *can be made true in a forcing extension that satisfy* **CH** *then* Φ *is true in ground model.In particular in the presence of appropriate large cardinals and* $\diamondsuit_{\omega_1}$ *there is no* Σ^2_2 *well ordering of the reals.*

Namely $\diamondsuit_{\omega_1}$ implies in the presence of strong enough large cardinals that the set of true Σ^2_2 sentences is maximal for forcing extensions satisfying **CH**. A partial results giving some support for this conjectures were obtained in [6]. The problem is that the present attempts for constructing the Ultimate L , if successful, will very likely satisfy $\diamondsuit_{\omega_1}$ and will have a Σ^2_2 well ordering of the reals.So the last conjecture , if true, will kill the possibility of constructing the Ultimate L along the suggested lines.

6.2. **Forcing Axioms.** Forcing axioms like Martin's Axiom (**MA**), the Proper Forcing Axiom (**PFA**), Martin's Maximum (**MM**) and other variations were very successful in settling many independent problems. The intuitive motivation for all of them is that the universe of sets is as rich as possible, or at the slogan level

> A set that its existence is possible and there is no clear
> obstruction to its existence does exists

This "slogan"is obviously very vague and each of the terms used is problematic , but the spirit of this talk is that such vague principles can be a good guidance for sharpening our concepts and getting a better and better axiom systems for Set Theory. In what sense the imagined set is specified? As first approximation let us say that a set is simply specified as any satisfying a certain property. So we should rephrase our slogan as

> If a set satisfying a given property is possible and there is no clear obstruction to the existence of such set then such set exists.

What do we mean by "possible"? I think that a good approximation is "can be forced to exists" So let us try

> If one can force the existence of a set satisfying a given property and there is no clear obstruction to its existence then such a set exists.

Still this principle is problematic. One can introduce by forcing a set which is an enumeration of all the reals of order type ω_1 but also one can introduce by forcing a list of ω_2 different reals. Of course it is inconsistent to have sets satisfying both properties. The statement is even more problematic if in the property one allow parameters say a given set A. Because suppose that our parameter A is uncountable . By using Levy's collapse one can make A countable, so introduce a 1-1 mapping between A and ω. Obviously such a mapping does not exists in the our universe.

I consider forcing axioms as an attempt to try and get a consistent approximation to the above intuitive principle by restricting the properties we talk about and the the forcing extensions we use. The restriction of the forcing notions is usually following the intuition of allowing only forcing notions that do not make a very dramatic change in the universe , like making an uncountable set countable.This is somewhat similar to restricting in the interpretations of the modalities "it is possible that..." the set of possible universes to universes which are not too different from the current universe.So we restrict the forcing which we consider to "mild" forcing extensions.

When we extend the universe by using forcing, what we add to the universe is the generic object with respect to the forcing notion. Since any other set introduced by the forcing notion is defined (over the ground model) from the generic filter, our typical forcing axiom is of the form:

> For a given class of "mild" forcing notions $\mathcal{P}$ and for every forcing notion $P \in \mathcal{P}$ there is a rich family of filters in P which are generic enough

A more rigorous statement will be

> For the given class of "mild" forcings $\mathcal{P}$, for every $P \in \mathcal{P}$ and for every ordinal α such that $P \in V_\alpha$ there is rich family of elementary substructures of $\langle V_\alpha, \epsilon, P\rangle$ M such that there is a M generic object for $M \cap P$.

The different forcing axioms differ only in the choice of the class of the forcing notions and the notion of "rich" collection of elementary substructures. Since for a countable model M we can always find a M generic object for every forcing notion in M, the schemata above is interesting only if we assume that there is a rich family of *u*ncountable M for which we can find a generic. Typically the notion of "rich" means that there the set of M which are elementary substructure of V_α and for which there is a generic is stationary subset of $P_\kappa(V_\alpha)$ for some set of cardinals κ.[6]

For instance the first forcing axiom was Martin's axiom which is

Axiom 6.4 (**MA**). *If P is a forcing notion satisfying the countable chain condition (c.c.c) then for every α such that $P \in V_\alpha$ and for every $\kappa \leq 2^{\aleph_0}$ the set $M \in P_\kappa(V_\alpha)$ for which there is a M generic filter for $P \cap M$ is stationary in $P_\kappa(V_\alpha)$).*

The way we presented the axiom is trivially true under **CH** so if **CH** holds we do not get any new interesting mathematical statement and the same is true for other forcing axioms. So when we state any forcing axiom we shall implicitly assume that $2^{\aleph_0} > \aleph_1$. **MA** which was proved consistent by Martin and Solovay in [26] decides many independent statements but it still leaves a lot of freedom as far as the size of the continuum.

The next step is enlarging the class of forcing notions for which the axiom applies so the next step was the Proper Forcing Axiom:

Axiom 6.5 (**PFA**). *If P is a proper forcing notion (see* [1] *for definition.) then for every α such that $P \in V_\alpha$ and for every $\kappa \leq 2^{\aleph_0}$ the set $M \in P_\kappa(V_\alpha)$ for which there is a M generic filter for $P \cap M$ is stationary in $P_\kappa(V_\alpha)$).*

This axiom was shown to be consistent relative to the consistency of supercompact cardinal by Shelah ([37]). The next step was the maximal possible strengthening of this axioms as far as the class of

[6] $P_\kappa(V_\alpha)$ is the set of all subsets of V_α of cardinality less than κ. A subset of $P_\kappa(V_\alpha), S$ is stationary if for every enrichment of the structure $\langle V_\alpha, \epsilon\rangle$ by countably many new relations and functions there is an elementary substructure of this structure which is in S and whose intersection with κ is an ordinal.

forcing notions for which the axiom applies. It is Martin Maximum (**MM**) introduced by Foreman,Magidor and Shelah and shown to be consistent relative to the existence of supercompact cardinals.([11])

Axiom 6.6 (**MM**). *If P is a forcing notion that does not kill the stationarity of subsets of ω_1 then for every α such that $P \in V_\alpha$ and for every $\kappa \leq 2^{\aleph_0}$ the set $M \in P_\kappa(V_\alpha)$ for which there is a M generic filter for $P \cap M$ is stationary in $P_\kappa(V_\alpha)$).*

In [11] it was shown that now the this forcing axiom, besides deciding many other independent problems, settles the size of the Continuum,namely **MM** implies that the $2^{\aleph_0} = \aleph_2$. In particular in the statement of the theorem the only κ for which the statement is interesting is $\kappa = \aleph_2$. This result was improved by Todorcevic and Velickovic (see [41]) showing that the same conclusion follows from **PFA**. In fact additional results of Moore (in [28]) get the same result from weakening of **PFA**. In some sense even the tiniest strengthening of **MA** fixes the continuum at $\aleph_2$. The fascinating fact is that $\aleph_2$ keeps appearing as a very special cardinal.

Thus I would have loved to suggest **MM** as natural axiom, deciding a large class of problems , including the size of the Continuum, that has intuitive appeal and therefore should be considered to be a natural candidate for adaption. But **MM** has a competitor: Woodin's axiom (*) ([44],see also [21]) has the same intuitive motivation: Namely the universe of sets is rich. (At least H_{ω_2} is rich.) Formally (*) is equivalent to the statement that every π_2 statement that can be forced to hold for H_{ω_2} is already true in H_{ω_2} of the ground model. The remarkable fact ([44]) is that (*) implies many of the consequences of **MM** for H_{ω_2}.In particular it implies $2^{\aleph_0} = \aleph_2$. There is a strong evidence that (*) is the right axiom to assume for the structure $\langle L[P(\omega_1)], \epsilon, NS_{\omega_1}\rangle$ where NS_{ω_1} is the non stationary ideal on ω_1 in the same sense that the Axiom of Determinacy (AD) seems the right axiom for the structure $\langle L[P(\omega)], \epsilon\rangle$. So it also seems a natural axiom to be adapted.[7]

So we have two competing axioms to , motivated by the same intuition, supported by similar slogans. Are they compatible? can we adapt both of them? Till recently it seems that the combination is problematic. Larson in [22] showed , assuming that consistency of supercompact limit of supercompacts , that one can get model in which **MM** holds but (*) fails. In fact what one has in this model that in

[7](*) does not imply the results of **MM** for the larger segment of the universe of Set Theory because it is essentially an axiom about $L[P(\omega_1])$. It is equivalent to the statement that $L[P(\omega_1])$ is a forcing extension of $L[R]$ by a particular nice forcing notion P_{max}.

the structure $\langle H_{\omega_2}, \epsilon, NS_{\omega_1}\rangle$ there is a definable well ordering while (*) implies that such a well ordering does not exists. On the other hand the only known way to get a model of (*) was to force over $L[R]$ and since **MM** is a global axiom one can not get a model of **MM** by forcing over a "small" model like $L[R]$.

But there is a conjecture that ,if true will, change the situation dramatically. It involves a natural extension of **MM** which is denoted by **MM**$^{++}$.Each forcing axiom has its ++ version. Remember the original motivation of the forcing axioms:

> If one can force the existence of a set satisfying a given property and there is no clear obstruction to its existence then such a set exists.

then the ++ version seems like even a better formulation of the intuitive concept than the formulation we adapted. **MM**$^{++}$ is the following axiom:

Axiom 6.7. *Let P be a forcing notion which preserves stationary subsets of ω_1. Let α be an ordinal such that $P \in V_\alpha$. Then the set of $M \in P_{\omega_2}(V_\alpha)$ such that there is a M generic filter $G_M \subseteq P \cap M$ such that if $S \subseteq \omega_1, S \in M[G_M]$ then $M[G_M] \models S \in NS_{\omega_1} \Leftrightarrow V \models S \in NS_{\omega_1}$ is stationary in $P_{\omega_2}(V_\alpha)$.*

Namely the ++ version claims that we not just that we can find enough elementary substructures of V_α which has a generic object in V but also that we can assume that this generic object is correct about subsets of ω_1 being stationary. So the object we get in the ground model is even a better approximation to the object we get by forcing. This is clearly very much in the spirit which let us study the forcing axioms in the first place.So the conjecture is:

Conjecture 6.8. **MM**$^{++}$ *implies Woodin's (*) axiom.*

If this conjecture is true then it will be strong evidence for adapting **MM**$^{++}$. I think that a proof of this conjecture will be a confirmation for both **MM**$^{++}$ (hence for **MM**) and for (*) in the same sense that the fact two separate scientific theories with desirable consequences can be merged into one unified theory can be considered to be confirmation for both of them.

MM or even better **MM**$^{++}$ is a global axiom that has many consequences throughout the universe . (See for instance [5] for the impact of **MM** on variations of the combinatorial principle $\square_\kappa$ for many cardinals κ.) But **MM** impact is mostly on sets of size $\leq \aleph_1$ so a natural research program is to try and find reasonable forcing axioms that apply to sets of size $\aleph_2$ and more. There are some initial steps in this direction due to Neeman ([30]) and and Gitik ([14]).This study is only in its infancy.

6.3. **Definable Version Of CH.** We very briefly describe a third stream of ideas that could motivate anther approach for deciding **CH**.This stream of ideas is presented in Koellner [21] section 2 which follows the analysis in Martin's [27]. The motivating slogan is

> If there is a counter example to **CH** there there should be a definable or "nice" example.

Or even more

> If the continuum is greater or equal than a specified cardinal κ then there should be a "nice" evidence for this fact.

Namely the intuition is that the answer to the problem of **CH** should be the same when we consider the problem in the context of "nice" sets.The analysis of [27] makes it very clear that in the definable context there are actually three different versions of **CH**:

(1) **The interpolation version**: Does there exists a "nice" set of reals whose cardinality is strictly between $\aleph_0$ and $2^{\aleph_0}$?
(2) **The well-ordering version** : Does there exists a "nice" well ordering of a set of real numbers of order type ω_2?
(3) **The surjection version**: Does there exists a "nice" surjection of a set of reals on ω_2?

As pointed by Martin the three versions are different for any reasonable definition of "nice" . In order to discuss it let us fix what do we mean by "nice". If we take it to mean definable by a formula of some fixed complexity , then we have the problem of the resilience of the definition and of the set defined when we change the universe of Set theory. We therefore prefare the more structural definition of "nice" which is requiring that the sets in question being universally Baire.

Definition 6.9 (Feng,M., Woodin ([10])). *A set of Reals ,A is said to be universally Baire if for very topological space Y and every continuous function $F : Y \to R$ the set $f^{-1}(A)$ has in the property of Baire in the space Y. (A similar definition applies to subset of R^k or for that matter any Polish space). A surjection F of a set of reals onto an ordinal λ is Universally Baire if the set of pairs $\{(x, y)|F(x) \leq F(y)\}$ is universally Baire subset of R^2.*

Universally Baire sets have most of the regularity properties (even just assuming ZFC) one would expect from a "nice" set: They are Lebesgue measurable, has the property of Baire, Have the Berenstein property (either the set or its complement contains a perfect subset) etc. If one assumes the existance of even one Woodin cardinal then the Universally Baire sets are either countable or contain a perfect subset. Universally Baire sets also have the important property that when we take forcing extension of our universe then there is a canonical interpretation of the meaning of the set in the extension. Actually even more is true:

Theorem 6.10 (Neeman [31]). *If A is a universally Baire subset of the space ω^ω then the infinite game $G(\omega, A)$ is determined.*

The following theorem connects the property of being Universally Baire and definability.

Theorem 6.11 (Woodin [43]). *If there is a proper class of Woodin cardinals then every set of reals which is in $L[R]$ is Universally Baire.*

So if we agree that "nice"="Universally Baire" then the problems of the "nice" **CH** becomes:

(1) **The interpolation version**: Does there exists a Universally Baire set of reals whose cardinality is strictly between $\aleph_0$ and $2^{\aleph_0}$?
(2) **The well-ordering version** : Does there exists a Universally Baire well ordering of a set of real numbers of order type ω_2?
(3) **The surjection version**: Does there exists a Universally Baire surjection of a set of reals on ω_2?

Even if we assume only ZFC with no additional axioms still there are some open problems concerning the three variations of the Universally Baire **CH**. For instance for the interpolation version : it is not known that there is in any model of Set Theory an uncountable Universally Baire set of cardinality different form $\aleph_0, \aleph_1, 2^{\aleph_0}$. [8] So we conjecture

Conjecture 6.12. *If A is a universally Baire set of reals then either $|A| \leq \aleph_1$ or A contains a perfect subset.*

Similarly for the the well-ordering version : We can have for instance (in L) a well ordering of a set of reals of order type $\aleph_1$. But no well ordering of larger cardinality is known in any model of Set Theory. So we conjecture

Conjecture 6.13. *Any ordinal which is the order type of a Universally Baire well ordering of a set of reals is $< \omega_2$.*

[8] In the models constructed by Harrington [18] in which there are Π^1_2 sets of any given cardinality bellow the continuum,the sets constructed are simply definable but not Universally Baire.

In the presence of large cardinals the picture is much simpler as far as the first two versions: If there is a Woodin cardinal then every Universally Baire set of reals is either countable or contains a perfect subset. The situation is even more dramatic with respect to the well ordering version: Under the existence of Woodin cardinal then every Universally Baire well ordering of a set of reals in countable. So if we assume the existence of a Woodin cardinal then the well ordering version has no bearing on the Continuum problem. Also as Martin and Koellner argue in [27] and [21] the interpolation version does not shed light on the continuum problem.

The third version:the surjection version is more interesting. Here there is a chance even assuming the existence of large cardinals that if $2^{\aleph_0} = \kappa$ then we can have a Universally Baire evidence for that. We feel that there is some intuitive appeal to following principle:

> If $2^{\aleph_0} \geq \kappa$ then there is a Universally Baire Surjection of R onto κ.

This intuition motivated what Koellner in [21] called the Foreman-Magidor program. This was an attempt in [12] to prove that if there is a Universally Baire surjection of R onto an ordinal α then $\alpha < \omega_2$. If this program was successful then we believe that it can be construed as evidence for **CH**. The program as stated was shot down by Woodin

Theorem 6.14 (Woodin [44])**.** *If the NS_{ω_1} is ω_2 saturated and there is a measurable cardinal then there is a Δ^1_3 surjection of R onto ω_2. Since this surjection is in $L[R]$ then if we assume the existence of a proper class of Woodin cardinals then it is a Universally Baire surjection of R onto ω_2.*

If we combine this theorem with a result of Schimmerling ([35]) about **PFA** and **AD** and the fact from [11] that **MM** implies that NS_{ω_1} is ω_2 saturated. we get

Theorem 6.15. **MM** *implies that there is a Universally Baire surjection of R onto ω_2*

So our favored axiom **MM** implies that really there is a "nice" evidence for the size of the continuum is $\aleph_2$. (in the surjection sense.)

What is being called The Foreman-Magidor program is not completely dead because we do not know any model of **ZFC** in which there is a Universally Baire surjecton of R onto ω_3.[9]

[9] We are talking here about model with choice. In a universe that satisfies **AD** the first cardinal such there is no surjection of R onto it is rather large and definitely larger than ω_3.This cardinal is usually denoted by Θ.

So one can still conjecture which makes sense whether one assumes large cardinals or not, even though of proof of it even under the assumption of large cardinals will be very interesting:

Conjecture 6.16. *There is no Universally Baire surjection of R onto ω_3.*

If this conjecture is true then it could be considered as evidence assuming that $2^{\aleph_0} \leq \aleph_2$ is a very natural assumption .

7. CODA

I hope that the previous sections give some good arguments why it is a meaningful question to ask whether one strengthening of **ZFC** is better than another and it is relevant question even if we consider that the main goal of Set Theory is extrinsic, namely to give a foundational support for Mathematics and through it to all of Science. We described some process by which we evaluate different set theories and decide on our favorite axioms.Admittedly this decision can change over time when more mathematical facts become available. This is a process that is not dissimilar to the process by which any branch of science decides on the current theory, where at any given time there may be several candidates but we are never in a situation in which we allow wild pluralism. The process, which is an on going process is based both on the consequences of the theory, or on its coherence with some intuitive feeling of what the right theory should be. This intuition is also an ongoing process and the intuition is refined by additional evidence.I believe that this description of the process is close to what Maddy ([25] was refereing to as a "proper set theoretic method".

As far as the most important open problem: **CH**, we believe that the process we described above leads in directions that will eventually will refine our theory to the extent that we shall have a definite answer for the value of the Continuum as well as answers to many other independent problems. Interesting fact is that the three directions charted in the previous section leads us to only two possible values for the continuum: either $\aleph_1$ or $\aleph_2$. We of course have to remember that the approaches in the previous section are based on unproved conjectures. Another interesting fact is the prominence of the cardinal $\aleph_2$ which keeps appearing over and over again in many seemingly different contexts.It is a historical curiosity that Gödel in his last years believed that the right value of the continuum is $\aleph_2$ and tried to find arguments supporting it though his attempted proof of $2^{\aleph_0} = \aleph_2$ from the axioms that he considered natural was wrong. See the introduction by Solovay to the Gödel unpublished paper in [16], vol 3 pages 405-425.

So not all set theories are equal . In Orwellian language "Some set theories are more equal". The challenge to the set theorists is to make sure that "The set Set Theory will win!".

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Einstein Institute of Mathematics, Hebrew University of Jerusalem, Jerusalem, 91904, Israel

E-mail address: `mensara@savion.huji.ac.il`