MATHEMATICAL KNOWLEDGE AND COMBINATORIAL POSSIBILITY: A NEW STRATEGY FOR SOLVING THE ACCESS PROBLEM

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0. Introduction

Claims like 'there are numbers between 5 and 10' seem to assert the existence of abstract mathematical objects like numbers and sets. But if we suppose that mathematics is really about such causally inert abstract objects, it can seem miraculous that we have knowledge of mathematics. Thus realists about mathematical objects face a prima facie problem: how to account for substantial human accuracy about mathematics without positing a miracle[1]. This classic concern is typically called the access problem.

We should note that what's at issue in the access problem is not skepticism about mathematics. All parties agree that mathematicians are (in some sense) largely right, and their claims are largely justified. The bone of contention is what we should say about the meaning of mathematical claims. What is it that mathematicians have such admirable and systematic knowledge about: abstract objects, what's true in a given fiction, second-order logical consequence or one of the many other options proposed in the literature?

To say that a theory which holds that mathematical claims describe some subject matter S faces an access problem is to argue roughly as follows.

[1]See (1) for an influential early formulation of the problem.

(1) No plausible mechanism could connect human beliefs to the facts about subject matter S.

(2) Given (1) it would be miraculous if human beings had largely true beliefs about S.

(3) Human beings do have largely true beliefs about whatever the true subject matter of mathematics is.

(Conclusion) We have a good reason to think the subject matter of mathematics is not S.

Thus the access problem for the realist challenges her to account for the accuracy of our mathematical beliefs in light of our lack of any causal contact with mathematical objects. Importantly, we need not justify those mathematical beliefs to respond to the access problem[2]. Instead, what the realist needs is some plausible story connecting human beliefs with facts about mathematical objects in such a way as to make our (purported) ability to correctly describe these objects un-mysterious[3].

In this paper I will show how we can tell such a story. First, I will show how we can characterize what it takes for mathematical objects like numbers

[2]Indeed, many contemporary reformulations of the access problem don't even mention terms like justification or knowledge. (5) says the Platonist's access problem is to explain the truth of the following claim, "Reliably, if mathematicians believe that ϕ then ϕ." And (11) says the Platonist must answer the following: Why is it the case that reliably: if "2+2=4" hadn't expressed a truth, we wouldn't have accepted that "2+2=4"?

[3]Note that the realist about mathematical objects does not need to endorse the relevant account of human accuracy with respect to mathematical objects, the account just needs to be plausible enough to defeat the impression that no such story is possible.

and sets to exist in terms of a primitive modal notion which I call combinatorial possibility[4]. This reduces the problem of accounting for human access to facts about mathematical objects to a problem of accounting for human accuracy in our general methods of reasoning about combinatorial possibility[5].

Second I will show that there's no philosophical problem about our possession of accurate general methods of reasoning about combinatorial possibility. If mathematical objects seemed worryingly unable to 'kick back' and correct our beliefs about them, facts about combinatorial possibility turn out to exert kickback on our reasoning about combinatorial possibility in two ways: via inference from what's actual to what's combinatorially possible and via the role of claims about combinatorial impossibility in our best predictions about what's actual. Furthermore, these two mechanisms of correction have sufficient power to account for the kind of fallible but wide-ranging knowledge of facts about combinatorial possibility which we actually have and can be implemented in ways that fit well with the observed phenomenology of mathematical discovery.

In §1 of this paper I will pin down the relevant notion of good principles for reasoning about combinatorial possibility. In §2, as an example of the power of combinatorial possibility to ground mathematical facts, I will present a characterization of what it takes for there to be a number in terms of combinatorial possibility. (For reasons of space, I will focus on showing

[4]This proposal expands on work by (12), Putnam(14) and Hellman(8) who have each explored various ways of giving intuitively correct truth conditions for claims about mathematical objects in terms of modal facts about what patterns of relationships are 'coherent' or mathematically possible.

[5]My story will be agnostic with respect to what particular methods of reasoning about logical possibility claims people use. This is a matter for empirical psychology, not solitary armchair investigation. What I propose to show is that, given any plausible hypothesis about what these methods of reasoning are, there's a straightforward story about how experience could correct them.

how to account for human knowledge of facts about the numbers in this paper, but see Appendix C for discussion of how the same story can be extended to account for knowledge of the sets.) In §3 I will discuss some mechanisms by which dealings with concrete objects could have given us good general methods of reasoning about combinatorial possibility. In §4 I will address worries about whether the above mechanisms could plausibly have given us intuitions about combinatorial possibility with sufficient power to reconstruct standard mathematics.

1. Combinatorial Possibility

1.1. **Introducing the notion of combinatorial possibility.** Let us begin with the notion of combinatorial possibility[6]. To motivate this notion, consider the sentence below:

> **SNOBS:** There are finitely many people at the party, but for each partygoer there is some other partygoer that owns more books on Proust than them.

One can know that SNOBS is false without doing any psychology or investigating the metaphysics of owning or the nature of being a book. SNOBS simultaneously demands that both that there are only finitely many partygoers, and that for each such partygoer A there is some other partygoer B such that B bears the 'owns' relation to more items in the extension of 'book on Proust' than A[7]. This demand seems to violate very general constraints on how *any* objects can be related by *any* relations.

[6]In the literature the labels 'mathematical possibility' (13) and 'coherence' (15) have been used to describe what I think is an essentially a similar idea. So as not make any controversial interpretive assumption I will use the distinct label 'combinatorial possibility' for my notion.

[7]To avoid undesirable complexity we don't provide the full translation of the concepts finitely and more in purely combinatorial vocabulary at this point but note that such a translation does exist.

I take examples like this to suggest that we have a grip on some sense in which the mere (broadly) logical structure of what a sentence demands can ensure that the sentence is false. We have some notion of how it would be 'in principle possible' for any n-place relations to apply to any objects, and we are able to discern that none of these in principle possible choices for how relations like 'owns' and 'is a book on Proust' could apply would suffice to make SNOBS true. Thus, even without considering anything particular about the relation of owning or the property of being a book on Proust, we can see that SNOBS must be false.

In the remainder of this section I want to flesh out the idea that there are primitive modal facts about combinatorial possibility in two ways. First, I will show how the notion of combinatorial possibility constitutes a natural extension of the more familiar notion of logical possibility in propositional logic and in so doing help pin down exactly what I mean by combinatorial possibility. Second, I will explain one way in which a concept can be primitive and argue that combinatorial possibility is primitive in that sense.

1.2. **How combinatorial possibility extends notions from propositional logic.** One can view combinatorial possibility as a kind of generalization of logical possibility in propositional logic. Think about what happens when we say that a sentence like, 'If it is not raining then it is not both raining and snowing' expresses a propositional tautology. First, we think of this sentence as being built up out of atomic propositions and logical connectives in some particular way. Thus, for example we think of the claim mentioned above as having the form $\neg r \supset \neg(r \wedge s)$.

Once we choose a particular formalization, we consider all ways it would be possible to assign either truth or falsehood to each of the different atomic propositions which figure in our formalization, for instance by writing a truth table. We then say the statement is logically possible in propositional logic if on one of those assignments the propositional formula evaluates to true. Note that when doing this we consider the same full range of 'combinatorially' possible assignments of truth-values to atomic sentences when considering any two atomic propositions r and s. We don't worry about whether it would be metaphysically or physically possible for 'it is raining' to be true while 'it is snowing' is false before writing down a line on our truth table corresponding to this possibility.

Now, the modal notion which I am calling combinatorial possibility arises from generalizing this idea to the case of predicate logic in the following way. As in the case of propositional logical possibility, combinatorially possibility or impossibility only applies to a proposition to the extent that we can thinking of it as having a certain logical form. Now, however, the required logical form is much richer. Instead of interpreting the sentence in terms of atomic propositions that we relate via boolean connectives we now interpret the sentence in terms of atomic relations and are allowed the full power of first order quantification to relate them. So, for example, if we consider the sentence 'John likes every person' we might decide to use the 'atomic' relations $J(x)$ to capture the property of being John, $P(x)$ to capture the property that x is a person and $L(x, y)$ to capture the relation x likes y. We could then formalize that sentence as $(\exists x)\,(J(x) \wedge (\forall y)\,[P(y) \supset L(x, y)])$.

In propositional logic we considered all possible choices of values for the propositional variables and declared the sentence to be propositionally possible if one of those choices rendered the propositional formula true. By analogy, to decide if our example is combinatorially possible we consider the space of all in principle possible choices of a domain of objects and which of these objects fall in the extension of the various relations. In this case, since there is an in principle possible choice of domain and extension of our predicates consisting of just one object a satisfying $J(a)$, $P(a)$ and $L(a, a)$, we could conclude that would deem the claim 'John likes every person' to be combinatorially possible. Using the notation introduced above we would write the formal version of this as:

$$\Diamond(\exists x)\,(J(x) \wedge (\forall y)\,[P(y) \supset L(x, y)])$$

Now what do I mean by the space of all 'in principle possible' choices of domain and extension? In the case of propositional logic above we ignored all physical and metaphysical relations that might apply to the propositions represented by the atomic propositions and simply considered all ways to assign truth values to those propositions. I think the natural analog of this idea to the richer logical structure we use to analyze combinatorial possibility is to consider the full range of ways it would be possible to choose a domain and relations on that domain, ignoring

- any limits on the size of the domains
- any particular metaphysical or physical facts about the nature of the relations involved which constrain how they apply (e.g., if we formalize a sentence in such a way as to make raven(x) and vegetable(x) separate predicates we will consider models that assign some objects to the extension of both predicates, even though it is presumably metaphysically impossible for any one object to be both.)

We should note that the resulting notion of combinatorial possibility differs from more familiar 'reductive' conceptions of logical truth in some important ways. Re-interpretation based conceptions of logical truth, following Bolzano, say that the logical truths are those sentences which are 'content neutral' in the sense that they remain true under all ways of reinterpreting the sentence by replacing one n-place relation with another and (perhaps) restricting the domain of the quantifiers [8]. My view differs from these insofar as it doesn't require that witnessing models be made by choosing some objects from the actual world as the domain of the model. This has the attractive consequence that intuitively contingent facts about the size of the universe have no effect on facts about what's combinatorially possible.

Unlike the notion of propositional logical possibility the notion of combinatorial possibility admits a natural extension to sentences already making claims about combinatorial possibility. To do this we must introduce the notion of a sentence being combinatorially possible holding fixed certain relations. So, for example, we might say things like, "Given what kittens and baskets that there are, it is combinatorially impossible that each kitten to have slept in a distinct basket" which will be true if and only if there are at least as many baskets as kittens in the actual world. See A for more details.

1.3. **Primitiveness.** Now let me turn to the sense in which I want to suggest that facts about combinatorial possibility are primitive. The notion of combinatorial possibility I have indicated above is closely related to standard definitions of first order consistency. Indeed, were we in a position to take facts about the sets for granted, we could define $\Diamond$ in terms of the existence of set theoretic models[9]. However, I want to suggest that facts combinatorial possibility require no grounding in the existence of objects of any kind, be they physical objects or abstract set theoretic models. In this regard, the view I am suggesting is analogous to a standard position on metaphysical possibility: taking metaphysical possibility to be primitive, as

[8] (3)

[9] Note that this would only capture the correct truth conditions in the intended model of set theory.

opposed to analyzing it in terms of the actual existence of Lewissian possible worlds (10). Just as a realist about possible worlds can give accurate truth conditions for claims about metaphysical possibility in terms of possible worlds, a realist about mathematical objects can give accurate truth conditions for the claims about combinatorial possibility in terms of sets. But (presumably) we need not think about the metaphysical possibility of there being, say, a pink elephant as being made true by the actual existence of a Lewissian possible world or any other object. Similarly, I want to suggest that there are primitive, fundamental modal facts about what patterns of relationships between objects are combinatorially possible.

To motivate this idea, recall our comparison between facts about combinatorial possibility and facts about propositional logical possibility. We determine whether or not a sentence is a propositional logical possibility by drawing up a truth table but presumably the fact that a sentence is a propositional logical possibility does not require grounding in the existence of a truth table. If you buy the analogy between combinatorial possibility and propositional logical possibility suggested above, then it seems natural to suppose that facts about combinatorial possibility require no more grounding in the existence of some witnessing mathematical objects than facts about propositional logical possibility require grounding in the existence of truth tables.

I think another substantial motivation for taking facts about combinatorial possibility to be free standing in the way I have suggested is that it allows for an attractive solution to the access problem. But there, of course, the proof is in the pudding; I invite you to read the rest of this paper and make up your own mind on how attractive my solution really is.

2. Mathematical Objects and Combinatorial Possibility

Now let us turn to the topic of mathematical objects.

Before we can ground the existence of mathematical objects in terms of combinatorial possibility we must first explain how the existence of any object could be grounded in some kind of fact. That is how can we non-misleadingly claim to be realists about mathematical objects and yet take the view that numbers exist in virtue of certain facts about combinatorial possibility obtaining.

2.1. **Existence statements and metaontology.** At least two broad pictures of how existential statements work are possible. On one picture, existential statements are made true by latching on to pieces of the world fairly directly. The world already comes divided up into different objects, and the only thing that can vary between different languages (or idiolects, or contexts) is what quantifier restrictions are in play, i.e., what sub-collection of those given objects do we take the quantifiers to range over. If we think of the truth or falsity of whole sentences being determined via Tarskian rules, then we should think of the domain of objects which the Tarskian rules apply to as being supplied directly by the world and then restricted by any quantifier restriction that may be relevant to the context.

On the other picture, existential statements are made true by the world more indirectly. Associated with different kinds of objects is a kind of criterion for what it would take from the world for there to be an object of that kind. The criterion[10] for what it takes from the world for there to be a hole

[10]Importantly one should not think of a criterion as being limited to some definition in terms of already recognized categories of objects. Such a view would amount to merely layering on 'virtual' objects on top of the collection of true objects. Rather the world is not seen as intrinsically cut up into objects at all and the existence of objects of all kinds merely amounts to certain facts being true of the world.

will be very different from what it takes for there to be a ball, a company or a wave. Thus we can think of the truth or falsity of a sentence at a given possible world as being determined by a two-step process. First we use the use the individuation criteria associated with different kind terms in the language (e.g., 'hole', 'wave', 'animal') to populate the domain of a model corresponding to that possible world. That is, we go from the possible world to a specification of what objects of various kinds would exist if the world were that way. Then we apply the usual recursive Tarskian rules to determine the truth conditions for individual statements involving $\exists$ or $\forall$ to the model which we have generated. Different languages (and perhaps the uses of the same language in different contexts of utterance) can differ from one another with regard to the first step: how do we associate each possible world with some domain of objects and extensions for predicates in the language?

To compare these views, consider the question of whether there can be stipulative definitions with existential import. Almost everyone will allow that *some* stipulative definitions are possible and acceptable. Consider the classic example "By 'Julius' I will mean the inventor of the zipper"[11]. Someone who makes this stipulation is presumably now in a position to know the sentence they would express by,"Julius invented the zip if any one person did". The truth of this sentences does not commit us to the truth of any positive existential claim. However, it is a far more controversial question whether any stipulation could shift one's idiolect in such a way as to make new existentially committal statements, like those characterizing the intended structure of the numbers or the sets, come out true.

[11](4)

According to the first picture of how quantified statements are made true by the world, the answer to this question is essentially no. The world contains a fixed stock of objects, which all expressions that behave remotely like the existential and universal quantifier pick up on. At most, stipulative definition could perhaps remove quantifier restrictions that had previously been in force in some language or context. But once all such quantifier restrictions have been lifted, no stipulations with existential import are possible. One can introduce new predicates or names to one's language, but the domain over which one's quantifiers range is fixed.

In contrast, according to the second picture, the answer to this question is yes. Stipulative definitions can shift you into a language where positive existential claims that would have expressed falsehoods (or been meaningless) in your old language now express truths. For remember that different languages will differ in the kinds of objects that they acknowledge. It is possible for a stipulation to change the behavior the quantifiers in a language by introducing a new kind of object: stipulating a criterion for what it takes for there to be objects of that kind i.e., a way of determining, for each possible world, how many objects of that kind would count as existing if the world were that way. This stipulation fixes 'what it takes for there to be' a hole or a wave or a company in the sense that I have been using that phrase above. If the world satisfies this criterion then the domain that is relevant to determining the truth of sentences in your language will be larger, and this can make new existential claims true[12]

[12]This is not so say, however, that anything goes with regard to stipulative definitions. Firstly, some proposed stipulative definitions can be internally inconsistent. Secondly, some stipulative definitions which are consistent on their own would be inconsistent if combined. (Boolos gives a simple example of this kind of incompatibility between internally coherent stipulations with his implicit definitions for numbers and parities (2) pg. 215. It has been argued that we face a similar situation with regard to sets and mereological fusions (18) and sets and categories.). Indeed, a classic challenge to stipulative

In this work we take the second view for granted (see (Berry) for a detailed defense of this position). On this account it will be true that holes literally exist, but they do so in virtue of certain facts about the arrangement of matter in the world which can be equally well be described in terms of claims about the positions and locations of physical objects. Along these lines, I propose that what it takes for there to be a *mathematical object* is for certain things to be combinatorially possible. I will now show how to characterize 'what it takes for there to be a number' in terms of combinatorial possibility, in a way that determines right answers to all questions in the language of number theory. Importantly no trickery or special sense of the word exist is relied on to justify realism about mathematical objects. Just like for every other kind of object there is some criterion that must obtain of that world for those objects to exist. It simply happens that the conditions that must obtain for mathematical objects to exist are necessary truths.

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definition approaches to mathematical objects concerns how to draw the line between acceptable and unacceptable stipulations and how to draw the line between good and bad stipulations.
However, we are now in a position to give a simple answer to this question, by appealing to the primitive notion of combinatorial possibility mentioned in the previous section as follows.

> **Harmony Condition on Acceptable Stipulations:** A stipulative definition S of a new type of object can be safely added to a language L iff it would be combinatorially possible to supplement the domain of objects generated from each possible world by the kind terms in L with new objects in such a way as to satisfy S without making any change to the extension assigned to any kind terms which are mentioned in S but not intended to be stipulatively redefined by S.

This harmony condition explains why we e.g., can't generally come to know that *physical* objects exist by way of stipulative definition. A proposed stipulative definition which implied that there is at least one Loch Ness monster would have to require something like a) there is (at the actual world) a monster in the Loch Ness and b) (at all possible worlds) wherever there is a Loch Ness monster there is an very large animal that swims etc. Now given that there is no large animal in the Loch Ness it won't be combinatorially possible to supplement (at each possible world) the objects we currently acknowledge with Loch Ness monsters in such a way as to make both a) and b) true without changing the extension assigned to kind terms like 'animal' at some possible world.
See my paper (Berry), for much more detail on this subject.

2.2. **Existence of the Numbers.** The criterion for the numbers to exist turns out to be a fairly lengthy and technical claim about combinatorial possibility. I provide these details in appendix B. Here we merely give an intuitive sketch of what must be combinatorially possible for the numbers to exist, and explain why you should accept this statement as capturing our informal notion about what it means for something like the numbers to exist.

We can implicitly define the intended structure of the numbers by saying that there are natural numbers corresponding to every way that it would be combinatorially possible to have finitely many things well ordered by some relation e.g. finitely many people well ordered by the 'admires' relation. That is,

H:

(1) The numbers are well-ordered by the '$\leq$' relation.

(2) There are *enough* numbers that it would be combinatorially impossible for a finite well-ordering of objects not to be isomorpic to some initial segment of the numbers.

(3) The numbers are *as few as can be* given that they satisfy (1) and (2)[13].

We can then define expressions like '0' and 'successor' in terms of $\leq$ and number in the natural way: x is the successor of y iff $y \leq x \& \forall z[z \leq x \supset (z \leq y \vee z = x)]$, and 0 is the unique number such that it is not the successor of any other number.

[13]See Appendix B for an demonstration of how all these notions can be unpacked purely in terms of combinatorial possibility

An important advantage to this proposal for grounding existence facts about the numbers in facts about combinatorial possibility is that it allows us to maintain the intuitive idea that all questions in the language of number theory have definite right answers. If we want to think about an implicit definition like H as showing how facts about number existence are grounded in facts about combinatorial possibility, it should turn out that stipulation H (when combined with all relevant facts about combinatorial possibility) suffices to determine the truth of falsehood of all such claims . And, indeed, once H is fully spelled out as per the appendix we can show that H combinatorially necessitates either the truth or the falsity of all statements ϕ in the language of number theory. That is, for every ϕ in the language of number theory either $\Box(H \supset \phi)$ or $\Box(H \supset \neg\phi)$.

Note that using combinatorial possibility we can give a description of the numbers H which has all truths in the language of number theory as combinatorially necessary consequences[14]. This is possible because unlike first-order logical consequence, the notion of combinatorially necessary consequence is not characterized by any exhaustive algorithm which derives all consequences of the relevant kind. It thereby gives us a grip on a notion of 'consequence' which can outrun formal derivability in the way that the Incompleteness Theorem [15] tells us that any notion of consequence must, if definite right answers to all questions in the language of number theory are

[14] I will say that ϕ is a combinatorially necessary consequence of ψ iff $\Box(\psi \supset \phi)$)

[15] (7)

to be consequences of a finite or recursively enumerable stipulation about the numbers[16].

If the above story is right, and something like H characterizes 'what it takes for there to be a number', then problem of knowledge of the numbers reduces to a problem of knowledge about combinatorial possibility. Once someone knows a claim like H, they are in a position to learn any further facts about the numbers ϕ just by working out whether it is a combinatorially necessary consequence of H that ϕ[17]. Thus we get an answer to Hartry Field's question, 'why do mathematicians reliably believe truths?' and a solution to the access problem - if only we can account for our ability to reason correctly about combinatorial possibility.

3. Knowledge of Combinatorial Possibility

It now remains to show that there is no access problem for general reasoning about combinatorial possibility. You might well worry that facts about combinatorial possibility are just as abstract as facts about abstract objects, so reducing the mystery of access to facts about sets to a mystery about access to facts about combinatorial possibility is not making much progress.

But, this is not so. There seemed to be no possible way for facts about mathematical objects to 'kick back' and correct our methods of reasoning

[16]Indeed combining the incompleteness theorem with the argument above that H does have each truth in the language of arithmetic as a combinatorially necessary consequence, shows that there can't be a complete axiomatization of reasoning about combinatorial possibility. For any set of principles we explicitly state, there will be some facts about the combinatorially necessary consequences of a given sentence which this set of principles does not let us derive. The same goes for any algorithm which our brain realizes when thinking about combinatorial possibility. For any textbook stating a 'logic of combinatorial possibility' which lists what inferences about claims involving combinatorial possibility are safe to make, there will be some claims which that textbook leaves out.

[17]Of course, insofar as we do not have, and cannot have, exhaustive principles which axiomatize all truth-preserving reasoning about combinatorially possibility, this does not mean that human beings are in a position to learn all these further facts

about these objects. However, once we turn to the case of combinatorial possibility, the situation looks different. In this section I will outline two ways for facts about combinatorial possibility to 'kick back' and correct our general methods of reasoning about combinatorial possibility[18]. I will then show how these two mechanisms of correction can be deployed to account for human accuracy about combinatorial possibility, and thus complete our solution to the access problem.

3.1. **Two ways for knowledge of concrete objects to correct beliefs about mathematical possibility.**

3.1.1. *Correction by $\phi \supset \Diamond\phi$.* First, and most obviously, there's the inference from ϕ to $\Diamond\phi$. If you are erroneously inclined to think that a certain state of affairs is combinatorially impossible, this inclination can be corrected by learning that that state of affairs is actual. This provides a way for principles which say that too few things are combinatorially possible to be corrected. Imagine, for example, that you aren't sure whether the state of affairs described by some mathematical hypothesis involving relations P, Q, and R is combinatorially possible. If I then point out that the relations of friendship, nephew-hood and having been in military service together apply in just this way to the royal family of Sweden, this will get you to accept that the scenario in question is, indeed, combinatorially possible.

[18]I stress that the mechanisms for empirical correction that I will be listing below are intended to apply to general methods of reasoning about combinatorial possibility (rather than merely to yield particular true beliefs about combinatorial possibility) for the following reason. Our knowledge of combinatorial possibility doesn't just involve accepting the combinatorial possibility of some finite list of cases. Rather it must involve something more general. I can immediately judge that it's combinatorially possible that there are 3002 cats, but it would be implausible to suppose that this is as a result of my ever seeing and counting 3002 of anything. Hence, if we are to appeal to correction by experience to account for the kind of positive knowledge of combinatorial possibility which humans actually have, we need to posit knowledge of particular claims about combinatorial possibility together with some kind process of generalizing from particular experiences, and then correcting these generalizations.

More generally, knowledge that some scenario is physically or metaphysically possible will also allow for the inference that it is combinatorially possible, and hence yield $\Diamond\phi$ beliefs in much the same way. For, no physically or metaphysically possible scenario can be ruled out by the most general principles of how any objects can be related by any relations. Hence, we can reliably infer that what's physically or metaphysically possible is also combinatorially possible. For example, if you think it would be physically possible to have 1000 different chess boards set with chess pieces, each of which boards differ from one another with regard to the occupant of at least one square, then this state of affairs must be combinatorially possible as well. Thus the same pressures which cause us to have accurate beliefs about physical possibility help give us accurate beliefs about combinatorial possibility.

3.1.2. *Explanatory pressure promotes some generalizations $\neg\phi$ to $\neg\Diamond\phi$.* Second, perhaps more surprisingly, the need to explain certain regularities in what's actual can push us toward the conclusion that certain states of affairs are combinatorially impossible.

For example, suppose someone thought it was combinatorially possible to choose 9 distinct subsets of 3 items[19] This person would have to explain the striking and apparently law-like regularity that, even when we consider the most malleable of objects and most variable of relations, we never wind up observing more than 8 such objects. They would have to change their physics to explain why apparently random processes of flipping three coins never generated the forbidden 9th possible outcome. Furthermore, whatever physical explanation they chose would have to apply at every physical scale we can observe, from relationships between the tiniest particles to the fact

[19]Spelled out formally in terms of 9 objects satisfying $A(x)$, 3 objects satisfying $B(x)$ and a relation $R(x, y)$ holding between them.

that when 9 people go to a Sundae bar with three toppings two of them always wind up ordering the same thing. Also, they would have to explain why the same regularity held, in apparently exactly the same way, with regard to much less concrete subject matter like poems. Try as you may, you will never manage to think up a poem with 9 different stanzas, each of which differs from the all the others in regard to which of three poetic themes it mentions.

Of course, in theorizing about the world we can explain patterns in what actually occurs in terms of *some combination of*: a) general facts about what is combinatorially possibility b) specific metaphysical or analytic facts about the properties and relations in question and c)contingent scientific laws. In some cases considerations of theoretical elegance will push us to favor an explanation by appeal to facts about combinatorial possibility.

It might seem like there could be nothing to choose between these three different ways of accounting for the regularities I just mentioned. However, this is an illusion. Firstly, most laws don't apply to all kinds of objects, so they can't be combinatorial necessities. For example, '$\forall x \forall y \forall z$ if taller(x,y) and taller(y,z) then taller (x,z)' is an exceptionless law, but not a candidate for being true merely in virtue of its object-and-relations structure since '$\forall x \forall y \forall z$ if admires(x,y) and admires(y,z) then admires(x,z)' has the same structure but is false. Secondly, when a principle *does* apply to all kinds of objects and fits elegantly into our framework of reasoning about combinatorial possibility that elegance is a prima facia advantage.

3.2. **Three Just-So-Stories.** Now that we have these ways for facts about combinatorial possibility to 'kick back' and correct our beliefs about combinatorial possibility, let us turn to the task at hand: explaining how creatures like us could have gotten correct methods of reasoning about combinatorial possibility.

Recall that the access problem is at heart an explanatory demand. Philosophers who take mathematics to describe mind independent abstract objects seem thereby committed to positing some kind of supernatural assistance shaping our mathematical intuitions[20] or a profound coincidence whereby we just randomly happened to get intuitions that correctly match the facts about an independent realm of abstract mathematical objects. I will now attempt to defeat this impression that no non-supernatural account of human accuracy about objective mathematical objects is conceivable in the most direct possible way: by giving three examples of how perfectly ordinary processes could account for the (general) accuracy of our armchair reasoning about combinatorial possibility - and thus for our access to facts about mathematical objects via the story above.

My first and simplest just-so story appeals to conscious suggestion and correction by experience within a person's lifetime. This story relies only on humans' ability to observe objects and arrive at elegant rather than gerrymandered theories. Each person faces the task of predicting and explaining the observed behavior of the various physical objects around them. To do this they arrive, consciously or unconsciously, at various elegant generalizations and systematic ways of forming new expectations about the world around them.

At this point the useful idea of looking for combinatorially necessary principles - general laws governing what patterns of relationships between objects are in principle possible - may occur to them. If so, they tentatively promote

[20]e.g., something along the lines of platonic recollection or descartes benevolent deity shaping our intuitions

some of these generalizations and methods of reasoning to combinatorially necessary principles which are expected to apply generally for all ways of substituting in one n-place relationship for another. The general methods of reasoning which they promote to combinatorial necessities will be pushed not to be too restrictive by the need to not rule out what's actual (as per our first mechanism of correction), and pushed not to be too expansive by the need to account for regularities in what's actual (as per our second mechanism of correction).

For a concrete example of what this kind of pressure towards correct reasoning about combinatorial possibility could look like, consider the predicament of creatures with observational and linguistic abilities largely like our own, faced with the task of predicting what symbol inscriptions of various kinds they will and won't find. They need to predict what patterns of letter inscriptions they will ever see written in gold, written in ink, carved in cork, stored in patterns of electricity in a computer etc. And, like us they can write down and observe many different finite strings of letter inscriptions in different fonts.

Now there turn out out to be many generalizations which apply to all these physically very different kinds of objects (gold leaf on top of paper, ink soaked into paper, scratches in cork, resistors in a computer in various states). For example, we won't ever see finitely many gold letters related in such a way as to satisfy the syntactic properties associated with being a proof of $0 = 1$ in Peano Arithmetic, nor will we see finitely many ink marks..., nor finitely many scratches in cork etc.

In light of these shared laws, it will often be both more theoretically elegant and more practically efficient to explain patterns in what letter inscriptions we will see in gold, ink, etc. by a combination of general laws about what patterns of relationships between objects are 'in principle possible' with different specific restrictions arising from physical and economic facts about each different medium, rather than by positing (and storing) separate and unrelated laws about what's physically possible by way of inscriptions in each different medium as you encounter it. Thus there is pressure to admit certain general laws about what patterns of relationships are in principle impossible.

Note that this kind of search for explanatory generalizations can also lead one to relatively strong positive principles about what's combinatorially possible. For example, even if all the string inscriptions these creatures ever encounter are be relatively short, there's pressure for them to recognize the combinatorial possibility of string inscriptions of arbitrary size arising from closure principles. For, many closure principles which smoothly predict many physically witnessed facts about what patterns of relationships between arbitrary strings are in principle possible will require this. Take, for example, the principle that if it is combinatorially possible to have a string of letter inscriptions with a certain property, then it is mathematically possible to have a 'doubled' string, consisting of two copies of a string that has the relevant property. The alternative would be to hold the much more complex theory where each such closure condition has an exception clause for sufficiently large instances.

It may seem strange to imagine broadly scientific generalizations from experience extending and correcting our mathematical reasoning as above. Wouldn't such correction have to leave traces in the phenomenology or justification of our reasoning about combinatorial possibility? But actually we have independent reason to believe that experience can suggest and correct mathematical beliefs without leaving any such traces in our mathematical practice.

Think about the development of the kinds of hunches that guide mathematical research. Mathematicians don't choose which proof strategies to try by tossing coins, but rather on the basis of less than fully confident hunches about how certain things should turn out. They have some ideas of what proof strategies are likely to work out or not, antecedent to actually trying them. These ideas develop and improve over time. So it seems highly plausible that past research experiences are causally involved in leading them to have the right hunches. Nonetheless when they say that this strategy looks promising or that one looks unpromising, they don't consciously summon up cases of similar strategies being tried or otherwise appeal to claims about their history and past experiences for justification. If experience can correct without showing up in the justification of the relatively unconfident hunches that guide research, why shouldn't the same be true in with regard to the more confident beliefs and inferences which figure in mathematical proofs? [21]

[21] Or consider the following, more interesting example of correction by experience leading people to accept propositions without then being disposed to cite experience in justifying those propositions:
In the Monty Hall game show, there's a car behind one door, and goats behind two others. You are asked to pick a door, and then Monty opens another door to reveal a goat. Now you have the opportunity to switch doors or to stay with the one you originally chose. When presented with the Monty Hall problem, many people initially find both of the following two analyses attractive: a) switching doors doesn't help because your only relevant knowledge is that a car is behind one of the doors, so you should assign each door probability 1/2 of having the car b) switching doors does help because there's a 1/3rd chance that the door you first chose had the car, hence a 2/3rds chance that the car is now behind the other door.
Then, the mere experience of playing around with a computer simulation, or looking at statistical descriptions of actual cases convinces them that the first analysis is the fallacious one. A recent NY Times article on cognitive illusions about the Monty Hall problem, linked to a computer simulation of the contestant's dilemma, which kept statistics for the results of switching vs. not switching in order to convince readers that its arguments for switching were correct (17)
Note that in these cases experience doesn't prompt the subjects to find new arguments. Rather, it changes their evaluation of the arguments they already have. Rather it leads

A second just so story takes our development of good general methods of reasoning about combinatorial possibility to have happened more slowly, over the course of the whole of human history rather than a single individuals lifetime. Here the idea would be that each individual person gets their principles for reasoning about combinatorial possibility by picking them up from the people around them. Individuals only very rarely promote new principles as holding as a matter of combinatorial necessity, or revise the methods of reasoning about combinatorial possibility which are generally accepted by the people around them. Development and correction of our general methods of reasoning about combinatorial possibility happens, rather, over the course of the history of ideas. Theories and computational practices are adopted when they seem to elegantly predict and explain regularities in what's actual and rejected when they ever fail to do so.

Finally a third possible just-so story takes the methods of correction mentioned above function on an evolutionary level. If reasoning about combinatorial possibility were 'hardwired' in some sense that precluded correction by experience, it could be hardwired in a number of different ways. There might be a system that directly reasons about combinatorial possibility. Or there might be an general undifferentiated system that nudges us towards beliefs that certain things are physically possible, and then a conscious process of inference to the best explanation which leads us to distinguish some of these methods of reasoning as combinatorially necessary principles as opposed to principles that are merely reliable as a matter of physical law or some other kind of metaphysical necessity[22]. Though evolution may not

them to believe the intuitively true claim that one of the arguments they already had ('There is a 1/3 chance the car was behind the first door I picked, hence a 2/3rds chance that it is not, hence I should switch') provided sufficient justification for switching doors, by getting them to give up the other conflicting analysis which had previously seemed attractive.

[22]See Spelke's experiments with infants e.g., (16) for an example of the kind of data which might suggest that some reasoning about what patterns of relationships between objects

care about elegance and theoretical beauty in quite the sense that we do mental resources are expensive and those methods of reasoning that could be encoded in the simplest manner and handle the most general situations would be favored.

Ultimately it's an empirical question if one or some combination of these stories is right. All the realist about mathematical objects needs to do to defeat the access problem is to provide one coherent story explaining our beliefs that's compatible with what we know now. I have attempted to provide a range of different just-so stories so as to show that my solution doesn't depend on any particular plausibility judgement about the importance of nature, nurture or individual experience in determining these kinds of beliefs. I also wanted to show that the key idea of this paper - the world corrects our general methods of reasoning about combinatorial possibility and deploying these methods of reasoning leads us to true beliefs about mathematical objects - can be realized by a number of different scientific stories about the development of human cognition.

3.3. **Two classic challenges for modal epistemology.** Before concluding this section it may be helpful to go into a bit more detail about how the stories above address two traditional challenges from the literature on modal epistemology.

One challenge for philosophers who would account for our knowledge of *metaphysical* possibility has been how to account for our ability to correctly distinguish metaphysical necessities from mere physical laws. We have a

are combinatorially possible are relatively innate. Further results along these lines might suggest that children have good methods of reasoning about combinatorial possibility before they are in a position to do much personal experimentation with concrete objects, or hear good methods of reasoning advocated in the classroom leg up on this problem when we turn our attention from metaphysical possibility to combinatorial possibility. For, as we just saw, combinatorially impossible claims are supposed to demand a pattern of relationships which it would be in principle impossible for any relationships between objects to satisfy. So these claims must remain false when you substitute one predicate or relation symbol in for another. Thus many candidate physical laws couldn't possibly be combinatorial necessities. This gives us a substantial, if not exhaustive, source of information which can guide our reasoning about which exceptionless laws are combinatorially necessary. However, as the supposed metaphysical impossibility of an object being both red and green all over indicates, the same substitutability principle doesn't apply in the metaphysical case. A piece of iron taken directly from the forge could obviously be both red and hot all over. Thus this powerful tool for distinguishing mere physical impossibility from combinatorial impossibility is of no use in distinguishing physical and metaphysical impossibility.

Another challenge concerns the role of conceivability judgements in modal reasoning. For example, if you want to know whether it is possible that ϕ, you might imagine some pattern of dots connected by arrows in such a way as to satisfy ϕ. One might worry that substantial access to facts about mathematics and combinatorial possibility is already built into our dispositions to find certain situations a priori conceivable and inconceivable and that the workings of this faculty of conceivability are immune to correction by experience, and perhaps immune to any of the kinds of correction outlined above.

I have two responses to this objection. First, even if the kinds of mental pictures we can entertain are relatively fixed and innate, experience can still correct our conceivability judgments by correcting the way we use these mental pictures: what we take ourselves to be conceiving of via entertaining a certain ghostly visual image, and generally how we read modal conclusions off particular pictures. Second, whatever aspects of our use of sensory imagination and though experiments **are** innate and immune to correction by experience can nonetheless be corrected by natural selection as discussed in our final just-so-story. See Appendix *D* for details.

4. Validity of Generalization

I will now consider a family of objections to the view just outlined. In order for an account of human access to facts about combinatorial possibility along the lines sketched above to work, we need to suppose the following: the kinds of general principles for reasoning about combinatorial possibility which seem to best predict and explain the facts about what's combinatorially possible with regard to small (finite and countable) collections of concrete objects will frequently also be ones which are also correct in their consequences for larger collections as well. But is it really plausible to suppose that this generalization from cases is reliable in its application to mathematics?

Note this is a subtly different worry than that addressed above by appeals to simplicity and theoretical elegance. Our argument there established that we would settle on simple, seemingly universal principles of reasoning about combinatorial possibility rather than coming up with case specific principles of reasoning about particular types or objects or collections of a certain size. This worry turns that question on it's head. Given that we will universalize the simple elegant principles that best explain our experience with relatively small concrete collections and use them to reason about combinatorial possibility on arbitrary collections should we be confident they are giving us the right answers?

This worry can be developed in a few different ways.

4.1. **'Generalization from cases is completely unreliable with regard to mathematics'.** Firstly, one might simply think that generalization from cases is completely unreliable with regard to mathematics[23]. However, if you take this position it raises a serious problem about how to make sense of a phenomenon which we have already noticed. Working mathematicians frequently use hunches developed from past experience and judgements of general plausibility or theoretical attractiveness to guide their research. They don't pick whether to try to prove ϕ or $\neg\phi$ by tossing a coin, but rather on the basis of their feelings of theoretical plausibility and past mathematical experience. Indeed, professional mathematicians will often cite computational searches that verify a great many cases as evidence that the claim holds in generality[24]. In the section above, we considered this phenomenon as evidence that experience can correct judgements without being cited as evidence for these judgements. However, if we want to make sense of this aspect of mathematical practice we must presumably also admit that experience can correct our judgements *in a reliable way.* Presumably mathematicians who let themselves be guided by these experience-honed hunches aren't completely irrational, or unreliable, in doing so.

Thus it seems we must allow that mathematicians' experience lets them build up reliable intuitions about what proof strategies are promising. However,

[23] Frege seems to have thought this about arithmetic (6) pg. 16

[24] They of course do not do this naively. If they already know that counterexamples would have to be huge they wouldn't change their judgements because no small counterexamples were found.

if we accept this appearance then correction by experience can't be totally unreliable in mathematics. It can't be the case that something about the complexity or variety of mathematical objects makes the kind of elegant generalization from cases we find in the sciences utterly unreliable when applied to the mathematical realm.

4.2. **'Generalization from dealing with small collections can't account for the degree of mathematical knowledge we have'.** Secondly one might worry that the base of knowledge of concreta which we have is too small to support the *degree* of combinatorial (and hence mathematical) knowledge we have. One might advance the following analogy: generalizing from knowledge of finite and countable collections yields principles which accurately describe the larger collections considered in pure mathematics is like saying that inference to the best explanation plus observations of birds in New Mexico allows us to learn about birds in Canada as well. Presumably in the ornithological case we need to go gather more data in order to get true beliefs about birds in Canada. But in the mathematical case, if my story is right, we can't gather more data. Thus facts about combinatorial possibility with respect to larger collections, appear to remain permanently a mystery.

I want to respond to this worry by accepting the analogy and claiming that it actually fits the current state of human knowledge with regard to facts about the higher infinite rather well. Even in the case of birds we can know some things about birds in Canada just by inference to the best explanation from the facts about the birds in New Mexico. If we discovered tomorrow that some new island had never yet been visited by explorers, I think we would reasonably expect many facts to carry over: that any birds on that island would have DNA, that they would have hollow bones etc. Our expectations about the new island would just be very **sparse** and relatively **unconfident** relative to our beliefs about birds in locations that we have got a chance to observe.

And this is just what happens with regard to our knowledge of what's combinatorially necessary with regard to large collections[25]. Our knowledge about what is combinatorially necessary for large collections is very **sparse**: for example, the continuum hypothesis concerns the relationship between the natural numbers, and the sets of natural numbers. Already, we lack any principles which let us decide how these two small and sharply defined infinite collections would have to relate to one another. For example, the Continuum Hypothesis is a fairly simple question involving sets of (relatively) very small size, yet it is known that both the truth and the falsity of CH are compatible with all the first order combinatorial facts about the sets embodied in the standard Zermelo-Fraenkel axioms for set theory[26].) Our

[25]Here it may, again, it may be helpful to contrast my theory with Quine's. Although both theories take correction by experience to play an important role in accounting for mathematical knowledge, my theory and Quine's have very different consequences for what one should say about cases where mathematical facts have little or no bearing on ones expectations for experiences with concrete objects.
On Quine's view we are justified in believing in whatever mathematical objects we quantify over in our best physical theories. So *absence* of relevant physical experience motivates negative claims about the height of the hierarchy of sets. Thus, insofar as our dealings with the physical world don't require us to consider higher regions of the hierarchy of sets, the Quinean picture suggests that Occam's razor should push us to reject the existence of sets at these higher levels. Thus, on this picture lack of empirical pushback on reasoning about the sets motivates definite negative conclusions about them.
In contrast, my proposal says that large sets of various kind exist if and only if this is a combinatorially necessary consequence of a certain ontologically exuberant hypothesis H describing the intended behavior of the hierarchy of sets. (Recall that the hierarchy of sets is supposed to contain, at each level, sets corresponding to every possible way or choosing from the sets below, and to contain levels which extend - in some sense - as far as possible.) But there is no intuitive pressure, analogous to Occam's razor, to say that fewer rather than more objects would be needed to satisfy the hypothesis H. Thus, on my view the divorce between certain claims about the higher infinite and experience motivates pessimism about how much we will be able to learn about the behavior of large sets, whereas on Quine's it motivates active denial that there are any such sets.
[26]The continuum hypothesis states that there are no sets whose cardinality is intermediate between the cardinality of the real numbers and that of the natural numbers. See (9) pg 176-186 for the proof that CH is independent of the Zermelo-Fraenkel axioms

knowledge about what collections of objects are combinatorially possible is also relatively **unconfident**. Sociologically, mathematicians are frequently much more confident in their claims about numbers, sets of numbers and sets of sets of numbers than in the distinctive claims of set theory about what much larger patterns of mathematical objects would have to be like.

Thus I think this last worry points to something that that is a good feature rather than a flaw of the account at hand: it explains why we have so relatively little knowledge of what's combinatorially possible with respect to large collections, and hence the facts about higher set theory.

5. Conclusion

In this paper I have proposed a two-part strategy for solving the access problem. First we can characterize what it takes for mathematical objects like numbers and sets to exist in terms of combinatorial possibility. This reduces the problem of accounting for human access to facts about mathematical objects to a problem of accounting for human accuracy in our general methods of reasoning about combinatorial possibility.

Second, we can solve the resulting 'accuracy problem' by considering the role of reasoning about combinatorial possibility in our attempts to predict and explain the behavior of concrete objects in the world around us. I have suggested a number of ways in which the push to predict and explain the common behavior of all concrete objects by appeal to general principles about combinatorial possibility can be leveraged to explain the fact that we employ correct general principles of combinatorial possibility when reasoning about pure mathematics.

Appendix A. Formalism for statements of combinatorial possibility

To give a very concrete idea of what I take the truth conditions for claims which describe what is actual in terms of what is combinatorially possible, let me introduce a little formalism.

I will use the $\diamond$ operator to express claims about what is combinatorially possible. I will use subscripts like $\diamond_{F,G}$ to express claims about what is combinatorially possible *given* the facts about how some predicates F, G actually apply. So, for example the claim, 'given the kittens and baskets there are, it's combinatorially impossible that each kitten slept in a distinct basket' gets written as follows:

BASKET SHORTAGE $\neg\diamond_{kitten,basket}$ (∀x)(kitten(x)⊃(∃y)[basket(y) & slept(x,y) & (∀z)(kitten(z) & slept(z,y) ⊃ z=x)])

Now what does it take for such a claim to be true? We can mimic the my intended truth conditions for claims of this kind by thinking about a web of mathematical models ('worlds'). A starting world W_i consisting of a choice of domain and extensions the match those of the actual world. However, each starting world can share objects in its domain with a vast number other worlds W_j. These other worlds W_j have domains and extensions for predicates corresponding to each distinct combinatorially possible way of carrying out the following two tasks. First, form a domain for W_j by choosing some number, possibly 0, of objects x_k (which may be from the domain of W_i). Second, give each n-place predicate in the language an extension by choosing some collection of n-tupples from within this domain from within the domain.

Now ordinary sentences that don't contain the $\diamond$ will be true or false at world W_i as determined by the usual Tarskian rules applied to the domain and extension of W_i. In order to characterize the truth conditions for sentences involving the diamond at W_i we must look further out to other worlds W_j. A sentence of the form $\diamond\phi$ will be true iff there is some W_j that makes ϕ true.

A sentence of the form $\diamond_{F,G...}\phi$ is more demanding. Intuitively the idea is that $\diamond_{F,G}\phi$ iff there is no way of adding or removing objects from the extensions of predicates other than F and G so as to make ϕ true. More concretely a sentence of the form $\diamond_{F,G...}\phi$ is true at a world W if and only if there is some other world W' making ϕ true *while preserving the facts about how the subscripted predicates F,G... apply in W*, in the following sense:

- The domain of W' includes all the objects that are in the extension of F, G or any of the other subscripted predicates at W.
- The extension of F in W' includes all and only the objects in the extension of F in W; the extension of G in W' etc.
- ϕ is true at W'.

Accordingly we can imagine the truth value of sentences like BASKET SHORTAGE as follows.

> BASKET SHORTAGE $\neg\diamond_{kitten,basket}$ (∀x)(kitten(x)⊃(∃y)[
> basket(y) & slept(x,y) & (∀z)(kitten(z) & slept(z,y) ⊃ z=x)])

First look at the actual world, and find a mathematical model which reflects it by containing a domain with as many objects as there are items in the world, and assigning one object to the extension of 'kitten' for each kitten there is, one object to the extension of 'basket' for each basket there is etc. Now think about all the other models which 'preserve' the extension of 'kitten' and 'basket' i.e., those worlds which have all the same objects in the extension of 'kitten' and 'basket' as the model corresponding to the actual world does. These worlds can differ from the actual world in many ways, including assigning different things to the extension of the 'slept in' relation. The claim that $\neg\Diamond_{kitten,basket}$ ∀x (kitten(x)⊃[∃y basket(y) & slept(x,y) & (∀z kitten(z) & slept(z,y) ⊃ z=x)]) will be true iff it is not the case that one of these worlds is one in which the $\Diamond$ free sentence ∀x (kitten(x)⊃[∃y basket(y) & slept(x,y) & (∀z kitten(z) & slept(z,y) ⊃ z=x)]) comes out true.

To see how this machinery allows us to make sense of nested claims, consider the following sentence.

> POSSIBLE BASKET SHORTAGE: $\Diamond_{basket}\neg\Diamond_{kitten,basket}$ ∀x (kitten(x)⊃[∃y basket(y) & slept(x,y) & (∀z kitten(z) & slept(z,y) ⊃ z=x)]).

This says that given the number of baskets there actually are, it would be be combinatorially possible for there to be so many kittens that, not only do some kittens have to sleep in the same basket but, it would be combinatorially impossible for each kitten to sleep in a different basket.

To see whether this sentence is true, we can again start by finding a mathematical model 'world' W_a which reflects the objects that exist and the extensions given to predicates. Now we see if we can find a world $W_{a,i}$ which preserves the extension of 'basket' in this W_a while having the following property: there is no third world $W_{a,i,j}$ which both

- preserves the extensions of both 'basket' and 'kitten' in $W_{a,i}$ (and hence preserves the extension of 'basket' in W_a)
- satisfies ∀x(kitten(x)⊃[∃y basket(y) & slept(x,y) & (∀z kitten(z) & slept(z,y) ⊃ z=x)]).

Appendix B. Characterizing the Numbers in terms of combinatorial possibility

Informally we can characterize the conditions for the existence of numbers (analogous to the conditions for the existence of holes) by saying, in essence, that the numbers are well ordered by less than, extend far enough that it would be combinatorially impossible to have finitely many objects without these objects being pair-up-able with some initial segment of the numbers, and that the numbers are as few as they can be given that they satisfy this condition. I will now show that we can state this characterization purely in the language of combinatorial possibility.

I will then show that this understanding of what it takes for there to be a set is sufficient to ground the truth of all true statements of number theory by showing that it has for any statement ϕ in the language of number theory either ϕ or $\neg\phi$ is a combinatorially necessary consequence of ϕ.

B.1. Useful Notions.

So, let begin by showing how to understand the notions of well ordering, and isomorphism and finiteness in terms of combinatorial possibility.

We can express the idea that a two-place relation R **well-orders** the Fs as follows:

- R is reflexive

  $\forall$x R(x,x)

- R is antisymmetric

  $\forall$x$\forall$y R(x,y)$\wedge$R(y,x)$\supset$x=y

- R is transitive

$\forall$x$\forall$y$\forall$z R(x,y)&R(y,z)$\supset$R(x,z)

- R relates the Fs so as to satisfy trichotomy

  $\forall$x$\forall$y F(x)&F(y) $\supset$ R(x,y)$\vee$R(y,x)

- it would be combinatorially impossible (fixing the facts about what actually Fs and Rs) for some of Fs to have some property G without there being some R-least F that has that property G

  $\neg\Diamond_{F,R}$$\exists$x F(x)&G(x)&$\neg\exists$y[F(y)&G(y) &$\forall$z (F(z)&G(z)$\supset$R(z,y))]

If we have one place predicates F and G and two place predicates R and R' we can express the idea that **the Fs (under R) are isomorphic to some initial segment of the Gs (under R')** by talking about how it would be combinatorially possible for some new relation, say 'envies', to relate the Fs and the Gs, as follows.

The Fs (under R) are isomorphic to some initial segment of the Gs (under R') iff it is combinatorially possible that 'envies' relates the Fs and the Gs in a way that satisfies the following constraints:

- Each F envies some G

  $\forall$xF(x)$\supset$ $\exists$y envies(x,y)

- If x and y are Fs that both envy the same G then x=y i.e, the envy relation pairs up Fs and Gs in a 1-1 fashion.

  $\forall$x$\forall$y (F(x)&F(y)&$\exists$a [G(a)&envies(x,a)&envies(y,a)])$\supset$x=y

- If x envies a and y envies b (and x and y are both Fs while a and b are both Gs) then x bears R to y if and only if a bears R to b.

  $\forall$x$\forall$y$\forall$a$\forall$b (F(x)&F(y)&G(a)&G(b)&envies(x,a)&envies(y,b)
  $\supset$ [R(x,y) iff R'(a,b)])

- If G(a) and a is envied by some F, and b is some G which bears R' to a, then this b is envied by some F as well i.e., 'envies' maps to some initial segment of the Gs.

  $\exists$a G(a)$\wedge\exists$x F(x)&envies(x,a)$\supset$ $\forall$b G(b)&R'(b,a)$\supset$ $\exists$y F(y)& envies(y,g)

To express the claim that the Fs are **finite**, we can say that it would be combinatorially impossible for a relation R to relate the Fs to one another Hilbert's-hotel-wise – that is, to send each thing that actually Fs to a distinct other thing that actually Fs in such a way that no two Fs R the same item, and yet there is one F left over that nothing Rs.

Thus we can think of 'there are finitely many Fs' as abbreviating the claim that $\neg\Diamond_F$

- Each F envies some other F.

  $\forall$x F(x)$\supset$ $\exists$y envies(x,y)

- If some Fs x and y both envy the same G then x=y.

  $\forall$x$\forall$y (F(x)&F(y)&$\exists$a G(a)&envies(x,a)&envies(x,a))$\supset$x=y

- Some F is not envied by any other F.

  $\exists$x F(x)&$\forall$y $\neg$(F(y)$\wedge$envies(y,x))

B.2. **Characterization of the numbers.** Now given this vocabulary we can now cash out the informal characterization of the numbers as follows:

(WO) The numbers are well-ordered by the '$\leq$' relation. This can be characterized directly using the account of well ordering above.

(S) There are *enough* numbers that every finite well-ordering of objects is isomorphic to some initial segment of the numbers. We can express this in terms of combinatorial possibility by saying that it would be combinatorially impossible for there to be finitely many Gs well ordered by some relation R without the Gs being 1-1 pair-up-able with the numbers in a way that respects the $\leq$ relation. That is:

> $\neg\Diamond_{Number,\leq}$(R' well-orders the Gs & the Gs are finite and it is not the case that the Gs (under R') are isomorphic to some initial segment of the numbers under $\leq$.

(Minimality) The numbers are *as few as can be* given that they satisfy WO and S. We can express the idea that the numbers are as few as can be by saying that, fixing the facts about what numbers there are, it would be combinatorially impossible for some objects, the Fs, to satisfy WO and S without it being combinatorially possible to pair up the numbers with some initial segment of the Fs in an order preserving way.

By similar arguments as have been used to show the categoricity of second order logic we can show that this suffices to pin down the truth conditions for ϕ. [27]

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[27]To outline how the proof works in a legible fashion, I will introduce the shorthand ϕ[F,G], where this abbreviates the formula you would get by starting with an expression ϕ which contains the expressions 'number' and 'less than' and then replacing all instances of 'number' with some one-place predicate F and all instances of 'less than' with some two-place predicate G. So, for example, a sentence of the form ψ[foxes, admires] says that the foxes are related by admires in the same way that the ψ claims the numbers are related by $\leq$.
Suppose for contradiction that there is some sentence ϕ such that H does not combinatorially necessitate either that ϕ or $\neg\phi$. That is, suppose it is combinatorially possible that $H \wedge \phi$, but also that $H \wedge \neg\phi$. Then it would be combinatorially possible for $H \wedge \phi$[foxes,admires] to be true as well as for $H \wedge \neg\phi$ [hounds, likes] to be true. Furthermore it would be combinatorially possible for both to be true at the same time i.e., it is combinatorially possible that $(H \wedge \phi)$[foxes,admires]$\wedge(H \wedge \neg\phi)$[hounds, likes].
However, if when we consider what is required by this supposedly combinatorially possible state of affairs, an incoherence emerges. From the fact H[foxes,admires] and H[hounds,likes] we can deduce by informal reasoning about combinatorially possibility that it would be combinatorially possible for the foxes and the hounds to be paired up 1-1 in a way that respects the behavior of 'admires' and 'likes'. This is ensured by the fact that 'admires' is supposed to well-order the hounds and 'likes' to well order the foxes, together with the minimality condition (3) above. That is, for all foxes a and b, a admires b if and only if a' likes b', where a' and b' are the hounds which a and b are (respectively)

Appendix C. Characterizing the Sets in terms of Combinatorial Possibility

Now what about characterizing the sets? Can we say "what it takes for there to be a set" in a way that allows us to make sense of the intuitive truth conditions for claims about set theory? In this appendix I will give what I take to be correct truth conditions for first order statements about the sets in terms of claims about combinatorial possibility. The account given below constitutes a fairly minor modification of ideas from (14) (**?**) about how understand the truth conditions for set theoretic claims in terms of a modal notion like my notion of combinatorial possibility.

A traditional, informal characterization of the hierarchy of sets goes roughly as follows. The hierarchy of sets starts from the empty set, and continues up by stages: each level contains the powerset of all the sets previously generated and the hierarchy proceeds by continuing to take levels 'all the way up'.

This description of the intended structure of the sets includes two aspects:

- width (each level contains the full powerset of the level below) and
- height (the sets go all the way up).

The standard ZFC axioms fail to capture even the intended width of the hierarchy of sets.[28]

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paired with. Now, the possibility of this kind of structure-preserving paring implies that for each sentence ψ, ψ[foxes, admires] will be true if and only if ψ[hounds,likes]. But this contradicts $(H \wedge \phi)$[foxes,admires]$\wedge(H \wedge \neg\phi)$[hounds, likes]. Thus there can be no such sentence ϕ.

[28]The axiom schema of restricted comprehension does indeed ensure that, for every set S, we will always have subsets corresponding to every subset R or S that is *definable in the language of set theory.* But, as we can see from the fact that there are uncountably many subsets of the integers yet ZFC has countable models, satisfying the axiom schema of separation does not require having sets corresponding to 'all possible' subsets.

In contrast, if we allow ourselves to appeal to a primitive notion of combinatorial possibility, we *can* capture the intended truth conditions for statements about the sets[29].

C.1. **Width.** To characterize the intended width of the hierarchy of sets we say, in effect, that it would be combinatorially impossible for some an object x to R some objects below level α, without there already being a set at level α which has *exactly those objects* as elements.

$\neg\Diamond_{Ord,Lessthan,Physical,Set,Elt}$ (∃a∃z Ord(a) and z only bears R to things that are either physical objects or sets at levels lower than a & z differs from each set that occurs by level a in respect to which things it Rs.)

- 'z only bears R to things that are either physical objects or sets at levels lower than a' is ∀y R(z,y)⊃[Physical(y)∨ ∃b (Ord(b)&Lessthan(b,a)&Set(y,b))].
- 'z differs from each set that occurs by level a in respect to which things it Rs' is ∀x Set(x,a)⊃ ∃w(Elt(w,x) iff ¬ R(w,z)).

Thus we can characterize models of ZFC which are at least standard with regard to their width in terms of combinatorial possibility.

C.2. **Height.** But now what about the height of the hierarchy of sets? The above description takes for granted a notion of the levels or 'stages' through which we keep applying the powerset operation. But we haven't said anything about the structure of these stages. I propose to capture the idea that these stages go 'all the way up' in a somewhat roundabout fashion. First we will characterize the intended structure of initial segments of the hierarchy of sets. Then we will recursively determine truth conditions for

[29]The resulting theory may not give us an acceptable stipulation for 'what it takes for there to be a set' to which one can apply standard Tarskian semantics, as we did in the case of the numbers. But it does give us a systematic way of capturing the intended truth conditions for claims about the sets

claims about what sets there are, in terms of claims about how it would be combinatorially to extend such initial segments.

We have already seen that nested claims about combinatorial possibility give us the power to express the notions of well ordering and full powerset. Thus we can state the claim that some objects F form the kind of collection that can be built up by applying the full powerset opporation through some well-ordered series of stages. That is we, can make the claim that some things, the Fs, having the structure we expect from any *initial segment* of the hierarchy of sets. Call this "being a V_β" We can also express the claim that the Gs' relationship under P extends the Fs' relationship under R in a way that continues to be compatible the structure we expect of any initial segment of the hierarchy of the sets.

Now how does this let us give intuitive truth conditions to claims about the sets? Let me introduce the following notion.

- I will use the variables P(x) P(x_1, x_2) P(x_1, x_2, x_3)... to represent formulae in the language of set theory that have no quantifiers.
- Let F_1, F_2... and R_1, R_2... be a countably infinite stock of one place and two place predicates, respectively.[30]
- Let P(x)[F_i,R_i] be the formula you get by substituting F_i for every instance of 'is a set' and R_i for every instance of '...is an element of' in P(x).

Now we can give recursive truth conditions for claims about the sets as follows:

[30]We could get such a stock of predicates in various ways. We could use appeal to adjectives: is red, is slightly red, is slightly slightly red etc. Or we could use relations to the numbers to do the job, since - as we have seen - we can characterize what it takes for there to be a number in terms of combinatorial possibility. So for example you can read F_1(x) as shorthand for likes(x,1) and F_2(x) as shorthand for for likes(x,2) etc.

First consider sentences with only one quantifier. So, let P(x) be a formula in the language of set theory with no quantifiers, such as x=x or ¬xϵx. Intuitively, ∃x P(x) should be true iff repeatedly taking the powerset operation could ever generate an x that Ps. But since P contains no quantifiers, an object *o* will satisfy P iff *o* satisfies P relative to any initial segment of the hierarchy of sets that contains *o*. Thus ∃x P(x) should be true iff it would be combinatorially possible for some objects to have the intended structure of an initial segment of the hierarchy of sets V_β while making the analog of '∃x P(x)' true.

> An ∃x P(x) claim is true iff $\diamond$ the F_1s are a V_β under R_1 and ∃ x P(x)[F_1,R_1].

Now consider sentences with nested quantifiers, like ∃x∀y P(x,y). Since the hierarchy of sets goes all the way up, this statement should be true iff an x is generated at some stage which bears P to all the y that ever get generated after that stage. That is, ∃x∀y P(x,y) is true iff it would be combinatorially possible for some V_β to contain an x, such that it would then be *combinatorially impossible* for that V_β to be extended to contain a y such that ¬ P(x,y). Thus:

> An ∃x∀y P(xy) claim is true iff $\diamond$ the F_1s are a V_β and ∃ x P(x) $\neg\diamond_{F_1,R_1}$ the F_2s are a V_β and the F_2s relationship under R_2 extends the F_1s relationship under R_1 and ∃y ¬P(x,y)[F_2,R_2].

We can continue on up the hierarchy in this fashion, using nested claims about combinatorial possibility with respect to $F_1..F_n$ to express cash out the truth conditions of set theoretic statements with n quantifiers. For example, an ∃x∀y∃z P(x,y,z) claim will be true iff it it would be combinatorially possible for an initial segment to contain an x that '∀y∃z P(x,y,z)' in the sense that it would be combinatorially impossible to extend that segment to contain a y, in such a way that you couldn't ultimately extend that extension to contain a z such that P(x,y,z). That is:

> An ∃x∀y∃z P(xyz) claim is true iff $\Diamond$ the F_1s are a V_β and ∃ x P(x) $\neg\Diamond_{F_1,R_1}$ the F_2s are a V_β and the F_2s relationship under R_2 extends the F_1s relationship under R_1 and ∃y $\neg\Diamond_{F_1,R_1,F_2,R_2}$ the F_3s are a V_β and the F_3s' relationship under R_3 extends the F_2s relationship under R_2 such that P(x,y,z)[F_3,R_3].

Appendix D. On the Role of Visual Imagination and Conceivability in Reasoning about Combinatorial Possibility

The role of visual imagination in reasoning about combinatorial possibility can seem to raise problems for my attempt to dissolve access worries about human knowledge of combinatorial possibility. Sometimes we arrive at judgements about what states of of affairs are combinatorially possible by way of using mental pictures, rather than by immediately finding some claim obvious or deducing it from another claim. For example, you might convince yourself that it would be combinatorially possible for a statement ϕ to be true, by imagining a pattern of dots connected by arrows in such a way as to satisfy ϕ. One might worry that these ways of using visual imagination to form conceivability judgements are immune to correction by experience, and perhaps immune to all the methods of correction outlined above. Thus, one might think that an unsolved access problem remains about how we manage to come to largely correct conclusions *when we use visual imagination* (as opposed to immediate judgement, or verbal deduction) to form beliefs about combinatorial possibility.

In response to this worry, I will argue that our methods of using visual imagination in conceivability judgements involve a substantial theoretical component: we have definite expectations about which ways of using a mental picture to imagine a scenario in which ϕ count as conceiving as opposed to mere 'picture thinking'. This is important because it means that, even if our ability to form mental pictures is fixed and innate, experience can still correct our visual-imagination-based judgements about combinatorial possibility by correcting our doctrines about what ways of *using* mental pictures are reliable guides to combinatorial possibility. Experience can and does cull out unreliable ways of using mental pictures to form conceivability judgements. Furthermore I will argue that it is not at all surprising that we have *some* ways of using mental pictures to form conceivability judgements which are reliable and hence survive this culling.

To see what I mean by the claim that our use of visual imagination to judge facts about combinatorial possibility involves a substantial theoretical component, consider how you might use visual imagination to convince yourself that rotating certain match-sticks can transform one figure into another. Now contrast this with the very different ways that visual imagination can figure in your thinking about how it would be possible for Apple stocks to behave in the future. These two uses of visual imagination seem to involve very different ways of associating a picture with a state of affairs which that picture is taken to represent. What is, in some sense, the very same mental image as of red lines swerving on a white background could be used to conceive of either the possibility that a stock undergoes certain changes in value over time (via the second way of using pictures) or the possibility of piece of a paper being decorated with red and black inkmarks in a certain way (via the first). I call these different ways of using mental pictures 'methods of projection' after Wittgenstein, since they are ways of going from a physical or mental picture to some claims about the world which this picture is supposed to represent[31].

These methods of projection provide a way for experience to correct our conceivability judgements, even if the range of mental pictures we can form is relatively fixed and innate. Experience can, and does work, reclassify certain methods of projection as either reliable ways to establish the metaphysical (and hence combinatorial) possibility of some scenario or mere picture-thinking. Think, for example, about the kinds of visual imagery which we are willing to use when thinking about alternative geometries for space - different ways a distance function might relate a set of spatial points. I take it these very same ways of using mental pictures to reason about what patterns of relationships in space are possible would have been counted as mere picture thinking - not an appropriate way to read possibility facts off of mental pictures - until relatively recently.

To see a concrete example of how correctable methods of picturing effect our judgements about what's conceivable (and hence what's combinatorially possible), think of the kinds of visual short-hands which we are willing to allow when attempting to visually imagine a pattern of relationships between objects which would make a certain claim true. When using mental pictures to reason about combinatorial possibility as above, you don't have

[31](19)

to imagine 100 or 1000 dots. In some cases a picture can just label that a single configuration is supposed to be repeated 100 or 1000 times, and this will still suffice to convince you that the depicted pattern of relationships between objects is in principle possible. Thus we seem to accept certain doctrines about what methods of picturing are 'safe' in the sense that any configuration of objects thus pictured is combinatorially possible. And these doctrines can be open to correction by experience even if the range of mental pictures which we can form is not.

Of course, this sort of culling by experience can only explain the accuracy of our visual imagination based judgements about combinatorial possibility if we have some reliable ways of using mental pictures which would survive the culling. However, I think it is anything but miraculous that we have *some* methods of picturing that are reliable guarantees of combinatorial possibility. In fact, the generality of facts about combinatorial possibility ensures that it's relatively easy for simple methods of representation be reliable guides to combinatorial possibility in the sense that they don't allow depiction of combinatorial impossibilities. All that is required is for some aspect of the neural, psychological or linguistic state that corresponds to visually imagining a scenario in which ϕ (via the relevant method of picturing) to involve some items which are themselves related as per ϕ. To give the crudest possible example: it may be impossible to visually imagine a state of affairs in which billiard balls are in combinatorially impossible relationship ϕ via certain direct methods of picturing, because doing this would involve having neurons that are in combinatorially impossible relationship ϕ

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