24.711: Topics in Philosophical Logic

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These are lecture notes for the Spring 2017 offering of MIT 24.711: Topics in Philosophical Logic with Vann McGee. Pardon any mistakes or typos.

Contents

1 February 15, 2017: Zeno's Paradox of the Arrow

1.1 An object is at rest during an interval iff it occupies only a single position during that interval (not during a moment of time). It's stationary during an instant only if the instant is contained in an interval during which it's at rest.

1.2 Ancient Greeks could only answer the question qualitatively because they couldn't measure time (although they could measure distances). Use a compass to make circles and form multiples of a unit length on a line.

1.3 Can use a straight edge and compass to get an estimate of length to any precision up to a rational number, but no further.

1.4 Aristotelian physics gives no good way of measuring a length of time. Earth, air, wind, fire all have natural motion (up, down), but because of acceleration you can't figure out good lengths of time - you need something that moves at a uniform speed. The natural motion of things in the heavens is circular, so you can use star movement to measure lengths of time (not as simple as one would think because Aristotle's system was geocentric where the planets orbited in a circle about the earth).

1.5 Need to be able to measure time and distance in order to measure speed and so to get close to a solution for the core question of what it means to be stationary.

1.6 Galileo look at pendulum movement - he saw a smooth continuous movement that happens at intervals (PE being converted into KE). The Aristotelian saw chaotic movement in the pendulum (you are displacing the bob and it's swinging is it trying to go downward but being contained by the string). Aristotle didn't have the principle of balancing of forces on the bob. Aristotle's answer to the question of why the bob doesn't stop is that there is some nonsense with a vacuum of air being generated behind the bob and pushing it forward. We can measure time by dividing the length of the pendulum rod and noting the period is inversely related to the square of the length of the rod.

1.7 Speed during an interval is the quotient of change of distance over change and time. Zeno gave us the idea of 0 speed over an interval, now we have a quantitative idea of non-zero speed during an interval. But we still have to account for the idea that the ball is at rest only at its apogee but at no other point in its path.

1.8 We can solve the ball problem by solving $x'(t) = 0$, where x' is the limit of the difference quotient of $(x(t+h) - x(t))/h$. Aristotle thought that bodies were the only things that were real and that bodies aren't made up of points or surfaces. So we can't just think of the ball but also need to consider the position in spacetime of the ball - we need to think of the trajectory (potential positions occupied by the ball) as existent, even when the ball isn't there. But Aristotle argued that a line isn't made up of points so this doesn't play out well under Aristotelian physics - i.e. we can talk about velocities but not of instantaneous velocities.

1.9 Why does Aristotle preclude the solution of taking the derivative? Do we not just take the limit over smaller and smaller intervals about the instant? The Aristotelian post-Newton understood the limit as the continual dividing of the line into smaller pieces.

1.10 Things change when we switch to dynamics - throw a ball up at some initial speed and the downward force of gravity will decelerate the ball until it reaches 0. But since acceleration is dv/dt there's no good way to define instantaneous acceleration out of velocity, which is also a function.

1.11 You know there are times when the ball is going up and times when the ball is going down. We want to say there is a time when the ball reaches its highest point when its velocity was 0, how do we know there was such a time? We could solve this with the IVT but we want to do this constructively by constructing the cut between positive velocities and negative velocities. Can we get the LUB principle from numbers just from the movement of the ball? If the LUB principle didn't hold for the numbers then there would points in the ball's flight where it didn't have any associated number for its height.

1.12 Most of what we can use real numbers for (measurements of things) we can get away with just rational numbers. Dedekind had originally the bad idea that we could get away with just rational numbers, but that was thrown out with $\sqrt{2}$ and the Ancients. Also need cube roots to deal with the roots of cubic polynomials. So the real numbers fall out of solving polynomials with rational coefficients - but this doesn't solve the ball problem because you have no reason to believe that the place where the ball is stationary is the root of an equation.

1.13 The Ancients don't allow for the idea that there is a point on a line that is defined by the all the points to the right of it or to the left of it. For the ball, there needs to be a top point to define the switch of signs.

1.14 Is it true that there is a supremum of all the points achieved by the ball? Is the ball at rest at this point? What if the ball only achieves heights of rational numbers (so it skips between points) or if the ball has a hole at the maximum of its height curve?

1.15 From Mathematica, you have a tire on a hubcap that each go through the same number of rotations - how does the hubcap not move much less distance than the tire? Galileo: the tire is just a polygon, the hubcap goes through places where there is no corner of the polygon instantaneously which is just a wacky idea.

1.16 Conclusion: you can't explain what is going on if you just think in terms of bodies and don't think about space and lines as being made up of points.

1.17 An alternative view says that only bodies are real, and the bodies can be in different places but the places aren't real, just the bodies are (and the distance relations they have with each other). Kant disputed this view - if God made a single marble hand

before anything else, then He could've made either a right hand or a left hand (it has to be one or the other), but if all we have is bodies with spatial relations then there is nothing different between a right hand and a left hand; but there is a difference between right hand and left hand so there has to be something to account for the difference - you need position.

2 February 21, 2017: Dedekind's Construction of $\mathbb{R}$

2.1 Jill starts at the bottom of a hill when Jack starts and the top and the reach the top/bottom at the same time. Is there a point where they are at the same elevation? Yes - take the set of all times when the difference in height was positive and then take the least upper bound. Most people say that it comes from their motion being continuous.

2.2 Really early on people learned how to use numbers to measure things - at least from having to re-establish property lines after the flooding of the Nile. This entailed measurement by yardstick which always yield results in fractional form. But can we get these same numerical properties without actually deferring to the use of numbers?

2.3 The rationals as fractions seem very well understood - the gaps between them were less well understood. We know that all the rationals along the path will be reached by the ball - how can we justify whether there are non-rational points that are hit by the ball.

2.4 Pythagoreans found that simple ratios of where you put your finger on the lyre string correspond to harmonic sounds, but then one of them discovered that there are lengths that aren't rationals. He didn't find it out by measurement but rather by reductio ad absurdum - finding the length of the diagonal of the square with side length 1. He showed that it can't be rational (and was thrown overboard for his discovery). So you have to have square roots if you want to do geometry like the Greeks (because of conic sections).

2.5 There's a problem which is for any square find another square that has twice the area - it was known by Plato and was in the Meno - solve it by taking the diagonal of the first as the side length of the second. Can you do this with a cube? You can't do this with a compass and straight edge. The medieval Arabics pushed forward by going passed compass/straightedge and finding the roots of a cubic equation without requiring that the length we're talking about can be one you can actually create with a compass and straightedge. That produced a whole new family of lengths never talked about by the Greeks. It was widely believed that you could create new numbers by constructing equations with irrational roots with only rational coefficients. For example $x^3 = 2$ you would introduce $\sqrt[3]{2}$. It was widely thought that all the real numbers were got by solving such equations, but for example π isn't.

2.6 Cantor showed that there are many more real numbers that aren't solutions to equations than ones there are. The question at the end of the 19th century was how to get a picture of what the real line was like - how to fill in all the holes. Dedekind's answer was to introduce the cuts. To fill in all the gaps, we need a cut everywhere. He gave a criteria for what it would mean for $\mathbb{R}$ to be complete, and second to show that there really was such a thing as that complete system of real numbers. The algebraic properties of the real numbers are relatively easy to satisfy, and you need added properties in order to fill in all the gaps. The principle Dedekind proposed was the least-upper bound principle. He also wanted to convince people that this was final - that you couldn't go any further.

2.7 Dedekind started by seeing if a geometric approach would suffice - he thought this was obvious - why? You also need a lot of (controversial) set theory to get Dedekind cuts, but the Dedekind cuts do reduce the reals to the rationals to the integers. Around this time, everyone considered the laws of geometry to be synthetic a priori (Kant), where we could only make sense of our sensory experience if we organized them into three-dimensional space. It seems dubious that the way we organize space should be the foundation of analysis because we have tons of applications of the real numbers that have nothing to do with space. Also, there's non-Euclidean geometry.

2.8 Dedekind started by enumerating the rules needed for $\mathbb{R}$: field axioms, ordering, etc. But that doesn't distinguish from the rationals. You need something more: the completeness principle (LUB). Dedekind gives some axioms that the thought could answer the question of what the real numbers are, or at least he says the real numbers satisfy those structural features. Why is identifying the structural features enough to say that you've identified the real numbers? There are going to be lots of other algebraic systems that satisfy those axioms. You have to also give the intended model for those axioms. The physical line satisfies those axioms, but we have no reason to believe that any particular one is not the real number system. Also any complete model of those axioms is going to be isomorphic to any other. Why is giving the structure not enough?

2.9 At the same time, Frege was investigating the foundations of arithmetic. He thought that the only principle he needed was Hume's Principle (two collections have the same elements iff there's a one-to-one correspondence between the elements). He showed you could get PA axioms from Hume's Principle, but he still asked: how do I know whether Julius Ceasar is a number? We expect a numeral to pick out a definite thing, but we can never do this with only axioms. Dedekind introduced the idea that the numerals don't have to stand for definite objects - that mathematics is rather about identifying structural properties, and that identifying the structure is all you can and need to do.

3 February 22, 2017: Dedekind's Construction of $\mathbb{R}$

3.1 Dedekind asked: what further properties than the normal ordering and field properties are necessary to specify the real numbers? Completeness. His proposal is to create cuts of the rationals into two non-empty classes so that everything in the lower portion is less than everything in the upper portion. Completeness means that anytime you have such a cut there is a real number defining it. This principle amounts to the least-upper bound principle. He showed that this condition characterizes the real number system (up to isomorphism). Historically, people had made systems that purported to be the real numbers but it turned out that they left some numbers out. But Dedekind showed that you can't add anything to his system and get anything that's properly larger.

3.2 The isomorphism theorem says that if you have any two models of his axioms a and b, you first define the positive integers of a to be those that contain $0 \in a$ and are closed under succession (and define the positive integers of b the same way), and then define a map between the two sets of natural numbers. This map is injective - if it weren't, then the set of integers that had mappings would have a LUB b', and somewhere between b' and $b' - 1$ you would have an integer that gets mapped. This lets you map the rationals of a to the rationals of b, and so then we get an isomorphism of Dedekind cuts and so of the real numbers in a and b.

3.3 So we've completely characterized the structure we're looking for - how do we know that there is something that exhibits that structure? Dedekind's response is to define the set of all cuts to be the real numbers and then show that this structure satisfies the axioms. So he takes the construction to be a sufficient answer to the question of what the real numbers are. But it feels like he's just given us something that is isomorphic to the real numbers.

3.4 Frege equivalently gives structural axioms for the natural numbers, but wasn't satisfied because he couldn't answer the question of whether Julius Caesar was a natural number. Hume's Principle is a structural property that only characterizes the natural numbers up to isomorphism.

3.5 All we need to construct the natural numbers is an infinite set from which we can generate a simply infinite set, but that's non-trivial because, starting from Zeno's Paradox, there is a long line of people saying that we can't have an infinite set, just a potentially infinite set. They say that all we can do is give methods for taking a finite sequence and extending it, but the idea that we can treat an infinite set as a whole was long treated as contradictory. Dedekind thinks he can build an infinite set by taking the totality of objects of his thought and take a member of that totality S and form the thought "S is an object of one's thought", and then recursively do

that. That's a one-to-one function from the objects of one's thought to the objects of one's though, but it isn't onto because one's ego isn't of the form "X is the object of my thought". This skirts the question of potentially vs. actually infinite because the totality of objects of thought is something given to you, not something constructed.

3.6 He says that the natural numbers are a "free created of the human mind" which he clarifies as being because they are a result of the process of abstraction specified above. This isn't completely new - Cantor does something similar when he wants to go beyond the natural numbers into the transfinite by looking at the different ways of ordering the collection and then defining an order isomorphism and then defining the order types as got from the orderings by the process of abstraction. You regard two orderings the same if their orderings are isomorphic (there's a bijection from one to the other that preserves the order). Or you can go even more abstract and say that two totalities are the same if there is a bijection between them, irrespective of ordering. That gives you the cardinal numbers. Aristotle also gets the objects of geometry similarly - he thought of them as ordinary solid bodies but we abstract away all the non-geometrical properties. Both Cantor and Dedekind were dipping into that tradition.

3.7 In retrospect, it seems like this is a big transition from thinking of mathematical terms like "natural/real/complex number" as denoting definite things (not things known from sense-experience, but still definite entities) to thinking about them as if we can talk about them as definite things once we've characterized them up to isomorphism. Modern mathematicians are pretty much invariably subscribers to the latter view - you talk about mathematical objects, but mathematical objects are what they are because of their properties and properties only characterize up to isomorphism. There was a change in the way mathematicians talked about mathematical objects that philosophy was really slow to notice.

3.8 Cantor also had a way of creating the real numbers out of the rationals. Cantor himself was interested in Fourier Series and when they converge, for him the idea that the real numbers were complete was cashed out by the idea that series converge whenever it's possible for them to converge. For any convergent sequence, we have $\forall k, \exists N$ such that $n > N \Rightarrow |a_n - a_N| < \frac{1}{k}$ (Cauchy sequence). So one criterion for the completeness of the real numbers other than the LUB principle is the Cauchy criterion implies convergence for any sequence. You can derive one criterion from the other. Cantor then defined the equivalence relation between $\{x_n\}$ and $\{y_n\}$ as for any k there is a sequence term such that y_n and x_n are arbitrarily close passed the sequence term. You can show that Dedekind's and Cantor's principles yield isomorphic copies of the real numbers. But the question never arose as to which of these is the genuine construction of the real numbers. It didn't occur to them to ask: what's a real number - a Dedekind cut or an equivalence class of Cauchy sequences?

3.9 Hilbert's axiomitization of geometry: he gave the set of axioms and wasn't initially concerned with the question of whether the system was complete. We know we get a model of the geometry axioms if you take a point to be an ordered triple of real numbers (identify a point with its coordinate in $\mathbb{R}^3$, which doesn't have anything to do with space and yet is still a perfectly good model). You also get a perfectly good model if you identify a point with an ordered triple of algebraic numbers - so the geometric axioms do not require completeness. Later on, Hilbert started to worry about completeness and so he added an axiom in the 9th edition so that the new axiom system is complete. One of the axioms of the original system was the Archimedean axiom: you can construct a line segment out of a sub-segment so that there is some multiple of the sub-segment that will surpass the end of the larger segment. Traditionally, geometric axioms were supposed to represent three-dimensional space, but Hilbert rather didn't deny that the axioms describe physical space but didn't assume that they were about physical space. He said the axioms were about systems that satisfy those axioms, and nothing more - there's nothing more to being an intended model for the geometric axioms. This is similar to group theory - there's no intended model for group theory other than a system that satisfies those axioms. This is a real departure from the idea that mathematics has a definite subject matter.

3.10 You can have a name without a referent - for example, "Cerberus" or other names that occur within myths. You don't have that with names like "Cherry" - there's a definite referent to the name "Cherry". But we have these numerals in mathematics (e.g. $\sqrt{2}$) that don't have a uniquely determined thing that they refer to. The issue with idea of identifying the square root of 2 with the set of all things that when squared is 2 is problematic because you can't square a set. And you can't identify it with any one object whose square is two for similar reasons. There's a worry that Dedekind gave us a construction of what he called the real numbers and long before him the Egyptians were using numerals that purported to be real numbers, so did the Ancient Egyptians refer to a Dedekind cut when they wrote down one such numeral? Or, possibly, could the real number signs not refer to entities but instead to some action, like the act of taking the successor with the natural numbers? Well, the numerals do correspond in an objective way similarly to how people's names objectively refer to people. If it were purely made up, then there wouldn't be a "correct" answer for question with real-numbered answers.

3.11 Dedekind described what he was doing as the reduction of analysis to arithmetic, which was understood to have secure foundations. But analysis has less secure foundations because people didn't have a clear understanding of continuity and limits and what was required for a sequence to converge - there was just a general lack of clarity. It's more accurate to say that he reduced analysis to set theory or set theory

plus arithmetic because what he needs to form the cuts is a lot of set theory. Looking back, it seems that he really did do something important for our understanding of the foundations of analysis, but it was not reducing analysis to arithmetic but rather constructing analysis within this theory of sets. Next time let's look at the axiomatic theory of sets and consider Dedekind's construction as a construction of the real numbers within set theory.

4 March 1, 2017: Combinatorial Conception of Sets

4.1 Last time we talked about Zermelo's axiom of set theory. One thing Zermelo wasn't addressing but were in the background were the paradoxes, which is why people responded to his proof of the well-ordering theorem with such uncertainty (their understanding of how sets work was shaken by the appearance of paradoxes). Formal set theory was still pretty new and their study only came to the foreground with Dedekind and Cantor and it seemed to fall immediately into contradiction.

4.2 The most prominent of paradoxes was Russell's paradox - some sets are elements of themselves, so consider the set of sets that are not members of themselves. If you ask the question of whether that set is a member of itself, you get a contradiction. The paradox has a direct analogue about language - "dog" isn't a dog so it doesn't apply to itself, but the phrase "does not apply to itself" applies to itself iff it doesn't.

4.3 Another paradox was with Cantor's study of ordinal numbers, he found that the ordinals were well ordered and for any well ordered set there is an ordinal that describes the order type of that set. Any ordinal is the order type of the ordinals less than it. But consider the collection of all ordinals, which is well ordered, and so there is a map from all the ordinals to all the ordinals less than some ordinal. Keep applying the map and then you get an infinite descending sequence which is a contradiction because the ordinals are supposedly well ordered. The semantic analogue is to take all the names of ordinals, there are only countably many names and so there are only countable many names of ordinals, but since there are uncountably many ordinals there are ordinals that aren't named. Since the ordinals are well-ordered there is some least ordinal is not named, but we have just specified a name for it. Also there is a least natural number nameable in fewer than 19 syllables.

4.4 Cantor showed that there are uncountably many real numbers by assuming that there are only countably many real numbers and then disproving the bijection by constructing a new real number not in the enumerated set. You can make a bijection $\mathbb{R} \to \{q_1, q_2, q_3, \ldots \in \mathbb{Q}\}$. He generalized the result to show that for any set there are more subsets of S than the number of elements of S. But then take the set of all sets, the cardinal number of the power set of the set is larger than the cardinal number of the set itself. But the power set of all sets of sets is included in the set

of all sets and so the cardinal number of the power set is less than or equal to the cardinal number of the set of all sets. There is no direct linguistic analogue but it came from the generalization of Cantor's real number uncountability proof. If we look at that theorem linguistically, we see that there are only countably many names for the uncountable $\mathbb{R}$, and then put the names in a list an use Cantor's procedure to construct a real number that is nameable but not on the list of all possible names.

4.5 Cantor just rejected that there was such thing as the set of all sets. He thought there were theological implications. He wrote to the Pope (no response) and said that he was developing a theory of the actual infinite but there may be worries because the church teaches that only God is infinite. He says it isn't blasphemous because he says he has found an intermediate level between the finite and the absolutely infinite (transfinite) where the transfinite goes beyond what we ordinarily think of the finite, but it's not complete limitless. If you had something completely limitless then you can't number it or comprehend it mathematically - only God is at the level of the absolutely infinite which is above not just the finite but also the transfinite. So Cantor's work isn't bringing God down to the level of ordinary mathematicians, but rather he boosted God up to a level farther beyond the level of things that Earthly things are.

4.6 So there's these analogues between set theoretic paradoxes and semantic paradoxes. Zermelo didn't seem interested in the semantic part. Russell had the exact opposite attitude - he thought this showed a deep discontinuity in human thought, one that was so profound because it manifested in a lot of different ways. How important is it to get a unified solution to the set of two paradoxes? It really seems like they are the same phenomenon.

4.7 This week we're going to look at a paradox that wasn't among the list of Russell's paradoxes - developed around 1960. You can't recursively pick sets as elements of other sets - there's no infinite descending chain. Say a set is well-founded if there aren't any infinite descending chains beginning with. Let W be the set of all well-founded sets. If there were an infinite descending chain ending with W then look at the chain the ends at the element which is an element of W and so that chain is infinite and the element is not well-founded. So W has to be well-founded. But then $W \in W$ and so there's an infinite chain ending at W by picking W for every link of the chain. Since no well-founded set is a member of itself, this paradox seems to reduce straight to Russell's Paradox. But it doesn't because you could have $A \in B \in A \in B \in \ldots$. So no loops of any length around whereas Russell's paradox is just a loop of length one. This paradox turned people's attention to sets that are well-founded - but most sets that are useful for math are those that are well-founded. If you want to really think closely about set theory but your interest in set theory is just for finding a theory of sets that is useful for mathematics, then you can just stick with the well-founded sets.

4.8 So there was an axiom proposed that said that all sets are well-founded. What amounts to the same thing is: for any non-empty set S, there is an element $s \in S$ such that $\forall t \in s, t \notin S$. Originally it was just adopted for convenience but it does yield a really pretty picture for what the universe of sets looks like. If we assume that the things that aren't sets form a set, then you get this picture of the theory of types. The crucial axiom for building out this structure is the axiom of ordered pairs - this allows you to put together two elements from different levels. This is the way that Zermelo was thinking about it but not the way that Russell and Whitehead were thinking about it. Cantor's paradox entails forming a set at some particular level, but the set of all sets must be at a level before which the set of all sets is formed and so there is no level where the set of all sets is at. Since a set is always formed at a level after the level at which its elements are formed, so you solve the same set of paradoxes. For the ordinals, each level of the hierarchy introduces a new ordinal. Each level of the hierarchy introduces exactly one new ordinal. So in order to get the collection of all the ordinals it would have to be formed at a stage after which you'd already gotten all the ordinals and so there is no collection of all the ordinals. So there's a response to all the set-theoretic paradoxes that comes just from trying to form a set that violates the foundation axiom.

4.9 So adding another axiom gave us the stronger system because it removes an inconsistency but does not introduce one. If you restrict the axioms of set theory to range only over well-founded sets. You can verify that each formula you get from an axiom of set theory by restricting the quantifier is a theorem of set theory. If you could derive a contradiction from the axioms of set theory including the axiom of foundation then you could get a contradiction in ZFC.

4.10 The tradition, beginning with Cantor, is thinking about collections formed from contained elements and you need collections to be well-founded and so you get paradoxes if you don't restrict to well-founded collections. The opposite view is from Frege where you get sets by starting with concepts. Von Neumann thought that you get contradictions when you start forming sets that are really large ($>>> \mathbb{R}$) like the set of all the ordinals or the set of all the cardinals, etc. So forming sets that are way too big is impossible but formulating sets that are relatively small is okay. The range of a function on a set will be at most the same size as the domain set so then if the things in the domain aren't too many to form a set then the things in the range aren't too many to form a set, either. VN had the replacement principle and this more global principle that you can form sets unless they are too big, "too big" meaning things form a set unless those things are equinumerous with the entire universe. So if you have set that's so big that if it's a set then the entire universe is a set then that set is not actually a set. So there's a different way of thinking about where the paradoxes come from by having sets that are just too big. Axel took this idea and build a whole set theory out of it.

4.11 This is the combinatorial conception of sets as collections. Next time we will think about sets from a logical point of view by starting with concepts and then get to sets by a process of abstraction.

5 March 6, 2017: Logical Conception of Sets

5.1 We've been talking about set theory, of which there are two conceptions. The combinatorial approach says that sets are collections built up from individual. The other idea is the logical conception of sets, where you gets sets by starting with concepts and forming sets by abstraction. This conception starts with Frege. Frege started out by looking at complex names like "the capital of France" and saw that the name was formed by "France" and the function sign "the capital of _". "France" names something, but "the capital of" only becomes complete by begin supplanted by a name. Concepts are unsaturated and are not individuals because it needs another object to be completed.

5.2 We know what an incomplete building - there are still bricks and mortar, but just not enough to yield a whole. But what is it for a concept to be essentially incomplete? At the syntax level, you put a name into a concept-phrase; at the semantic level, you put an object into a concept to get a truth value. Someone might (incorrectly) think that Versailles was the capital of France and say "We're going to Paris and then going to the capital of France". But you can't substitute "Paris" for the latter reference because the person believes that the referent of "the capital of France" is Versailles. Senses deal with things like this (i.e. belief contexts). Thinking about sentences as having the True/the False as their referents turned out to be very fruitful because he could think as predicates as functions. This allowed standard operations with functions from basic algebra to be applied to concepts.

5.3 Frege has individuals, functions, function signs with more than one argument, and second-level functions. Second-level functions: "someone is a wise philosopher" has a similar grammatical structure as "Socrates is a wise philosopher", so it looks like you have "_ is a wise philosopher" as a concept which can take "Socrates" and "someone" as arguments. But in the latter case the individual about which you're talking is only indefinitely specified. Frege saw that this wouldn't work because of things like "Someone is wise" and "Someone is a philosopher" does not yield "Someone is a wise philosopher" and so the logical structure of the two is really quite different. Frege's idea is that "someone" is not a name, but rather a function sign that takes as its argument a concept. So "someone" is the property that a property has if there is at least one person who falls under ther latter. So things like quantifiers or the definite integral sign are second-order.

5.4 Frege wanted to use this logical framework to establish arithmetic on a purely logical

foundation. The prevailing view was that the laws of arithmetic were synthetic a priori, where you become acquainted by arithmetic by experiencing the act of succession. He wanted to reject that view and show that the arithmetic truths are analytic. The principle he uses to establish this is called Hume's Principle, which says that two concepts have the same number just in case there is a one-to-one correspondence between the things that fall under them. He wanted to avoid postulating the existence of the numbers, but he has to go deeper than Hume's Principle in order to avoid this. He wants to be able to think of the laws of number as purely logical laws.

5.5 Something deeper going on was the theory of classes. The motivating idea is that if you say Traveler falls into the class of horses, you aren't saying anything more or less than just that Traveler is a horse. So he wanted to do something like identify 5 with the class of all 5-element sets. But he was able to define the numbers so as to avoid the circularity.

5.6 He had a theory of arithmetic based on counting as derivative from Hume's Principle, and then he set out on similar project with the real numbers. But then he got a letter from Russell, who pointed out Russell's Paradox that comes out of the set of sets that aren't members of themselves. Frege (basically) gave up the project, but Russell took it over. Frege had gotten to this point by saying that for any concept there is a corresponding extension. The idea that you could match concepts and objects in a one-to-one fashion is exactly what Cantor showed you couldn't do with the real numbers. This is a very general argument that there are more concepts than extensions. Russel saw that and thought that the way to get around this is not to treat numbers as objects but rather as second-level concepts. So there is a concept that is true of all concepts under which exactly 5 things fall. The former concept is what Russell is going to treat as the number 5. Frege got into all this trouble by postulating classes, Russell tried to get away without classes. Russell wanted to postulate concepts, and he thought that you could guarantee the existence of concepts corresponding to the predicates of the language on purely logical grounds. Because from "Traveler is a horse" we can infer "$\exists x$ x is a horse" we can also get "$\exists F$ Traveler is F". He thought that would give you on purely logical grounds the existence of concepts.

5.7 But you can still formulate Russell's Paradox by thinking about the concept of not falling under itself. Russell wanted to use concepts in all the places where Frege used classes, and even though you can still get arithmetic from both you can also still formulate the paradox in both. He wanted some principle to ensure that the expressions of the language are meaningful because he knew that you could take expressions that seemed reasonably and get contradictory results. He appealed to Poincare's Vicious Circle Principle, because then you can exclude notions like a concept applying to itself. Russell's idea was that you need to follow Poincare in

order to avoid the paradoxes. He added to Frege's levels of concepts (the **type** of the concept, or the number of arguments) the **order** of the concept which pertains to which types of concepts are necessarily for formulating that concept. Later, he would replace concepts with propositional functions, which are functions that take an object as an argument and gives a proposition as its result.

5.8 The Vicious Circle Principle gets in Russell's way. First, we want to be able to state the LUB principle of the real numbers. In terms of the vicious circle principle, you have this collection S that is bounded above, so you have a collection of upper bounds of S and the least member of that collection is the LUB of S, which violates the vicious circle principle. There's another example of violating the VCP with trying to define the natural numbers. To get $\mathbb{N}$, he needs to postulate the existence of an infinite set of individuals. Zermelo's theory was neutral in that it allowed individuals but didn't require individuals. Russell had to require the existence of infinitely many individuals, which already was a problem because General Relatively says that the universe is finite and QM says that at the level of the very small we can get indivisible pieces, so it was actually a question of whether there are infinite individuals (at least, the postulate is contingent and dubious). If you suppose there is a simply infinite set, then you show this by showing a one-to-one function whose range is a proper set of the domain and then generating a simply infinite set by taking something that is in the range but not in the domain and then following that cycle. Russell tried to use the axiom of infinity to get 0 and the successor operation from this simply infinite set. The natural numbers can't just be a set with 0 and everything accessible with successor – you need to be talking about the *smallest* collection that contains 0 and closed under successor or else you don't get induction. But what is meant by "smallest"? The intersection of all collections with 0 and closed under successor. But then you are defining the natural numbers as the intersection of all collections that contain the natural numbers and your definition is circular.

6 March 8, 2017: Iterating on the Theory of Types

6.1 Last time, we ended with a discussion on how Russell's theory of types is inadequate for doing basic mathematics (you can't get the real or rational numbers).The problem was that the vicious circle principle was too restrictive and issues arose with only being able to define propositional functions on lower-order/type functions. As a patch, Whitehead and Russell proposed that for any propositional function there is a coextensive propositional function with the same type and lowest possible order (without talking about anything that isn't in the extension of the propositional function). This gave them the mathematics they wanted but completely gave up the possibility of mathematics being pure logic. As long as all the propositions you needed could come just from existential instantiation, then it seems that you

could just use pure logic. But existential instantiation does not get you lower-order correlates of the propositional function. Assuming the axiom of reducibility, they really gave up the idea of reducing mathematics to logic (and admitted as much).

6.2 It's pretty similar with the axiom of choice, that if you have a set of non-empty overlapping set there's another set that picks one element from each of them. To Vann, it seems very obvious if you're thinking about classes as collections (you can always form the product space and find a projection). If you're thinking not in terms of classes but in terms of propositional functions and classes as things that we construct by constructing predicates, then there's not really intuitive grounds for axiom of choice. If you have infinitely pairs of socks, then there's no reason you should be able to form a predicate of only one from each pair (??). If you think of sets as being created by some form of mental process, then the axiom of choice seems dubious.

6.3 The Banach-Tarski paradox says that you can cut up a sphere of diameter 1 into pieces and re-assemble the pieces into two spheres of length 1. You need the axiom of choice in order to create the cut. Some people thin kit's a problem and other's don't. Why it seems like a problem is that you get 8x as much matter for free (it seems). Others say that it just seems like a problem because we expect volumes to be preserved, but if you think of how volume is defined (you get volume of a sphere by inscribing polyhedra inside and outside the square and taking the limit of many faces), but you can never really close the gap between the inside/outside and the sphere.

6.4 Russell had two motives for adopting the vicious circle principle: he wanted to respond to the set-theoretic paradoxes and he wanted to respond to the semantic paradoxes. In the introduction to *Principia Mathematica* there is some talk about the semantic paradoxes. Ramsey proposed that we distinguish between the semantic (epistemic) paradoxes from the set-theoretic paradoxes. For the purposes of creating a foundation of mathematics you just focus on the set-theoretic paradoxes. Then you realize that you don't need the vicious circle principle to solve the set-theoretic paradoxes when you have the axiom of reducibility. Ramsey suggested that you scrap order altogether and just arrange the propositional functions so that a function just has to be a higher type than its arguments. Quine took that a step further: Frege wanted to talk about classes, Russell wanted to replace that with talk of propositional functions. Quine noticed that the talk of propositional functions was intended for the logical reduction project, but since that failed we might as well go back to talking about classes. So we had individuals, classes of individuals, relations (binary/ternary/so on) between individuals, binary relations relating classes to classes, individuals to classes, etc. So you end up with this structure that is still kid of complicated but still much simpler than Russell's solution.

6.5 Norbert Weiner saw that even that picture could be simplified by instead of talking about relations, let's identify a relation with a class of ordered pairs/triples/etc. (identify $< a, b, c >$ with $< a, < b, c >>$. So you can just talk about individuals, classes of individuals, classes of classes of individuals, etc. Weiner's idea was that you didn't have to treat ordered pairs as primitive. The law of ordered pairs says that $< a, b >=< c, d >$ iff $a = c$ and $b = d$. Norbert noticed that you could take $\{\{\{a\}, \emptyset\}, \{\{b\}\}\}$, and you can verify that this satisfies the law of ordered pairs. There are more brackets than you'd think because he was working in the theory of types and need to be working with things of the same type. With this simplification, what started as a monstrously complicated system becomes very simple.

6.6 To get mathematics, he needed infinitely many individuals. If you have only finitely many individuals, no matter how high up the hierarchy he went you'd still only have finitely many things. But if the individuals about which you're talking are physical objects than it is actually pretty dubious that are infinitely many. So they added the Axiom of Infinity and they didn't regard it as obviously true.

6.7 There's still one feature of that project that is still kind of anomalous. You want to say that there are 8 people in the room, and how we're going to make sense of that is by saying that the number 8 is the collection of all 8-element classes of individuals. So a number is a class of classes of individuals such that every class of individuals that can be put in one-to-one correspondence with a member of the class is a member of the class. What is still peculiar about this theory of types is that you can't mix types which means you can't use the same numbers to count individuals as to count classes of individuals. So there's a different number system of each type and there are infinitely many systems of natural numbers. It's not fatal, but it's just a little fussy and seems kind of artificial.

6.8 Gödel stepped in a suggested that there is no good reason not to make the classes cumulative instead of only allowing members of type $n-1$ in classes of type n. Originally, we wanted this hierarchy of classes in order to protect us from the paradoxes and the vicious circle principle, but we've given up the vicious circle principle. He noticed that we get the paradoxes when we ask whether a member of the class is at the same type as the class, and so we can lighten the restriction to allowing any type below the class, not just the one type just below it. Originally, we get the type hierarchy from a grammatical restriction on concepts/objects, but once you allow cumulative classes you need to abandon the grammatical distinction - so you say that you won't make type distinctions among the things we're talking about as far as logic is concerned. Some of the things we will talk about are individuals, sets of individuals, and so forth, but we're going to be able to talk about those all at once without distinguishing between them (there's only one kind of variable for the logic). What gets us the hierarchical structure is not a grammatical restriction but rather an axiom that tells us that the sets are well-founded. so in Gödel's system you have

a very simple logic (first-order predicate calculus) but you have a lot of not simple axioms guaranteeing the existence of sets.

6.9 So it looks as if there is no hope for reducing mathematics to logic, it looks like Cantor and Dedekind won. The advantages of a logical conception of set over a combinatorial conception of set disappear once this project doesn't work. And ZF set theory is so much more powerful because it can extend into the transfinite. But this is also possibly bad because it can make it more vulnerable to contradiction. Still there are lot more points that have gone to the combinatorial conception rather than the logical conception. There is still a lot to be desired and it's a big loss to have mathematics be synthetic a priori rather than analytic.

6.10 Gödel's incompleteness theorem throws a wrench in the works by showing that there are arithmetic truths that we can recognize as true that aren't derivable from the axioms, and that even if you add more axioms there are still more truths that aren't provable.

6.11 More recently, the Neologicist program has been trying not to get us all of ZFC but rather to get us mathematics strong enough in order to serve or purposes will staying in an (arguably) logical framework.

6.12 Dedekind proved that arithmetic is categorical, but if you think of arithmetic as a first-order theory the way we do know, then that is not categorical (it is not a second-order theory). You get induction from a quantification over properties and you get categoricity from induction. But when you do things in a first-order framework you have an axiom schema for induction and so you have infinitely many induction axioms instead of a single second-order axiom. But then this new set of axioms isn't categorical and by Gödel's work isn't complete.

6.13 The next part of our story is going to be a revival of second-order logic as something we can do in the context of modern mathematics when we're well beyond the *Principia* stage. That revival is due to George Boolos. Next time, we'll talk about Boolos and Quine and then we'll start talking about Benacerraf. Read Quine and Boolos articles.

7 March 15, 2017: Quine's Nominalism

7.1 Quine really wants to be a thorough-going nominalist. He wants to deny that there are any abstract objects. But he doesn't think he can do it for reasons that we'll spend most of April talking about. Right now, we're going to talk about his more-limited version of nominalism.

7.2 He's willing, albeit reluctantly, to concede that there are such things as classes, but he wants to deny the existence of universals and properties. His reasons for doing that are mostly having to do with the obscurity of the identity conditions for properties.

7.3 You can tell whether two classes are the same, which is in the case where they have all the same elements. It is hard to tell whether two properties are the same. It's interesting that Russell went the other way, trying to get rid of classes and use only properties.

7.4 It doesn't really matter to us that Quine wants to deny that they are properties. What is a more interesting question is how is that even an intelligible thought. We see all the dogs and are able to distinguish all the things that are dogs from those that are not dogs. How can we classify that which is a dog and that which is not?

7.5 The obvious answer is that there are some properties that dogs have in common and we can use those properties for our classification. Quine wants to deny that there are these properties that the dogs have in common.

7.6 One approach is to take the nominalist view that the only thing dogs have in common is the name we give them. But this approach is bad because it isn't arbitrary how we classify the dogs but it is arbitrary how we name them. This is how Russell thought of it. To say, 'X is a dog', 'Y is a dog', and 'Z is not a dog' all logically entail that there is a property that X and Y share but Z lacks.

7.7 There's this position that Quine wants to advocate that there are not any properties, but it is hard to formulate that in a way that a competent English speaker wouldn't regard as obviously false.

7.8 The idea is that there is something that X and Y have that Z lacks. The thought is that there is this property that is shared between the former but not by the later. Question: why can't you not specify what they have in common but instead just say that they are both dogs.

7.9 Or is there a difference between properties and propositional functions. Common doctrine is that 'that' clauses denote propositional functions. If you use 'that' clauses as direct objects of verbs (??) commit us to propositional functions.

7.10 But Quine wants to deny that there are any propositional functions. Didn't we learn anything from going through this pointless complexity of going through the ramified theory of types. We really just need an inclusive view of what objects there are. In particular, we don't need a special logical category for propositional functions. Just put them in the same domain as your quantifiers. It's going to make our life more complicated without adding much by build distinctions (like type distinctions) into the logic. So for type distinctions we just need some way to demarcate the differences within the universe of the domain of the quantification instead of adding logical distinctions.

7.11 Quine's answer to his own question: first, he has a nice slogan. "To be is to be the value of an variable", which tells us a method for answering the question of what the ontological commitments are of a given theory. That seems to be as far as it goes

but really doesn't seem to go very far at all because anybody who doesn't agree that because X, Y, and Z all walk on four legs and share the property of being four-legged. Anyone who wants to deny that is not a competent speaker of English.

7.12 Quine proposes that this criterion is right but in order to employ it you need to enter a process of translating English into a canonical form where you are trying to fully make the commitments of your theory as explicit as possible. In particular, Quine says that if you want to make it explicit what the ontological commitments of your theory are, then reformulate the theory within the first-order predicate calculus. Then, the things that have to serve in the variable place tell you what the ontological commitments of your theory are. He doesn't think this is obligatory but he thinks that if people refuse then it means that they are refusing to make their own logical/ontological commitments explicit.

7.13 It seems like this is demanding too much. The machinations Carnap had to go through in order to talk about disposition terms (??) in the first-order predicate calculus. To talk about what it means for a chemical to be soluble: you know that water dissolves sugar, soy ou can see which particular samples of sugar dissolve and you can predict which won't. You can predict that sawdust cannot dissolve. So a natural formulation is x is soluble iff x dissolves when put into water. But if you use this as your material conditional, you'll get that things that are never put into water are soluble. But you don't want that. Carnap went through this big effort to try and figure out a way to express solubility in the first-order predicate calculus by saying: if a theory of solubility contains two axioms:

7.13 Dissolves in water

7.13 Two identical substances either both dissolves or neither of them does. (??)

But if something is never put into water and nothing else like it is ever put into water than you cannot say that is it not soluble.

7.14 You get something that looks a lot more natural if you're allowed to understand the conditional that x is soluble if it dissolves when put into water - to understand this is a much more strong material conditional (if it were put into water, it would dissolve) - that seems like a use of disposition terms is deeply entrenched in our efforts to describe the world in science and without. And our use of counterfactual conditionals is also a very fruitful usage. It seems necessary restrictive to go with Quine and restrict to the lower-predicate calculus.

7.15 But if you accept Quine's criteria you seem to be getting bogus ontological commitments inasmuch as you want to understand disposition terms in terms of counterfactuals. Well, you can't take counterfactual usage as primitive because you have to ultimately formulate things in terms of the predicate calculus, but you cant take such usage as defined by adopting a possible world semantics. x is soluble if it would

dissolve in the closest possible world. But we wanted to avoid talking about possible worlds because we wanted only to look at what is around us and speak about that.

7.16 Vann wanted to return to the discussion we were having last time about the possible skeptical conclusions from the Löwenheim-Skolem theorem. Beyond our theorizing, there is a use of number talk in counting and measuring that doesn't really seem to get us significance beyond what we get from just the theorems. The fear is that if we accept Quine's mandate to restrict ourselves to first-order logic, we seem to be forced into some sort of skeptical position. Our mathematical theories don't have a unique intended model. That is something that we saw already from Dedekind and that's something we feel like we can cope with. What Dedekind did was to capture the real and rational numbers in a way that was unique up to isomorphism, but didn't go any farther than that to specify which of the isomorphic structures was the real number system. Look at how Dedekind develops the theory: that his terms doesn't have uniquely determined referents doesn't seem to cause any problems for the way he does mathematics, even just for practice; classifying up to isomorphism it seems good enough. Take whatever our mathematical theory is, assuming infinite models, it is going to have wildly dissimilar infinite models that don't resemble each other at all structurally.

7.17 Something Putnam wants to insist on is that we don't appeal to occult powers of the mind to see how our terms latch on to their determinate referents. But we're not we're just using number talk that leave it drastically undetermined what the referents of our terms is. We get a kind of skepticism that is upstream from the kind of skepticism you get from thinking about whether theorems/axioms are really true. This is at a level of skepticism that beings even earlier. Nevermind how you know they're true, how are you going to have mathematical beliefs if supposedly beliefs are about these objects but there is nothing that you do that is even close to picking out those mathematical objects.

7.18 Skolem's paradox was from the early paradox and was easily answered just by saying that you get the elementary submodel by neglecting some functions that we know are really there. So, yeah, if you neglect things you can get a theory that looks just like our theory. That looks like a sleepy little paradox. But then Putnam took it and made it look like a very serious paradox, both inside and outside of mathematics.

7.19 There's a way to get around the paradox which is to do what Dedekind did and say that contemporary mathematics' describe the natural numbers etc. as models of a first-order theory. Dedekind proceeded differently: wrote down his axioms as a second-order theory where he could range quantifiers over properties and was able to prove categoricity (that his axioms actually determined the real or natural numbers uniquely up to isomorphism). But his proof took place within this second-order theory, Can't we reapply the Löwenheim-Skolem argument to the higher theory.

Unless we assume that we have some grasp of what properties there are that can't be explained in how we use property talk. So you just get back to using occult powers of the mind again.

7.20 There's an answer to Skolem's problem that claims to be different than just kicking the problem upstairs to higher-order logic. Though whether or not it really is different is controversial. The alternative answer started with Peter Geach with the project of formulating ordinary English sentences into ordinary language. He was particularly interested in the use of plural noun phrases. Take the sentence: "there are some critics that only admire one another". The sentence "there are some critics who just admire impressionists" can be formulated easily in a first-order idiom. But "there are some critics that only admire one another" can only be expressed in a first-order idiom if we can appeal to the use of classes.

$$(\exists S)((\exists x)x \in S \,.\, (\forall y)(y \in S \Rightarrow \text{Critic}(y))\forall x \forall y(x \in S \,.\, \text{Admire}(x, y) \Rightarrow y \in S))$$

But then:

$$(\exists S)(\exists xSx \,.\, \forall ySy \Rightarrow \text{Critic}(y)\forall x \forall ySx \,.\, \text{Admires}(x, y))$$

But if I say that there are critics who admire each other it doesn't seem like I am telling you anything about classes and you could imagine someone who is a strict nominalist and certainly believe that there are critics, you can think anything you like about their group relations, and you can belief that there are critics who admire only one another, but you can't be a nominalist and only belief this sentence. If you accept the commonly held believes about sentences then you learn that there are the same truth conditions for the first and second sentences but you may belief the things about critics without believing the reformulation.

7.21 You can't formulate the statement about classes just in terms of first-order logic; take the sentence: "there are some critics who only admire one another". That sentence has the same form as "there are some non-zero natural numbers that are adjacent only to one another" but the latter sentence is only going to be true in a non-standard model of arithmetic. If you could successfully formulate in first-order logic that there are some critics who only admire one another then you could successfully formulate that there are some non-zero natural numbers that are adjacent to one another, then you could construct a sentence that is true in the standard models but not in the non-standard models. But then it is not possible to give a categorical description of the natural numbers system.

7.22 Boolos proposed that the correct way to understand that kind of use of plurals we see in the last sentence is by taking a different kind of the use of the quantifiers. We have quantifiers that range over individuals, second-order quantification where they range over properties. Boolos is proposing the notion of plural quantification where the variables range over individuals and the difference is that in singular quantification

the variables range over individuals one at a time while with plural quantification the quantifiers range over individuals many at a time. So there is in the semantics for first-order predicate calculus a variable assignment is a function assigning a value with each of the variables. Then you inductively define what it is for a variable assignment to satisfy an open sentence.

7.23 There are critics who only admire one another with:

$$\exists xx((\forall y)y \in xx \rightarrow (\text{Critic}(y) \,.\, (\forall y \forall z)(y \in xx \,.\, \text{Admire}(y, z)) \Rightarrow z \in xx))$$

Note the $\exists xx$ which is the plural quantifier. This theory doesn't postulate any new things and doesn't add any new attributes or properties to get the range. Both quantifiers range over individuals. The plural variables are as restrictive because you can have multiple individuals instead of just one: you have to associate at least one individual but you can associate more than one. Once we have plural quantification we get our categorical descriptions of the objects of classical mathematics.

7.24 The key to describing the natural numbers if: for any set of natural numbers, there is one among them that is less than all the others. If you want to classify the real numbers, you have to know: for any real numbers, if there is a number that is greater or equal to all of them then there is a least number that is greater or equal to all of them. Zermelo formulated the separation principle in terms of definite properties, people weren't happy with brining the metaphysics of properties into set theory so Z said that we should formulate it instead as a first-order theory, with the difficulty that the first-order schemata admits non-standard models. Now that we have plural quantification, we can say that for any set X, $\forall x \in X$, $\exists Y$ such that only the x such taht $x \in X$ are members of Y. W ecan similarly give a plural quantification version with the replacement axiom schema. Vann will embarrass himself if he tries and says it, but there is a second-order formulation of the replacement axiom, if you insert the replacements into ZF set theory then you do not get a categorical representation but you get instead ZF axioms and their plurally quantified versions and the fact that for any two models of the axioms either they are isomorphic or one of them is isomorphic to an initial segment of the other, which you get by clipping off the taller model. So that means we don't get a categorical generalization of the universe of set theorem.

7.25 The continuum problem comes from Cantor's proof that the number of real numbers is greater than the number of natural numbers. Are there any infinite sets with cardinality between the two? That question is going to have a definite answer, especially if we're using plural quantification. The axioms of set theory do not have a uniquely determined model, but they are just alike to the part of the model that is smaller, and the real numbers and all the functions defined on them are in the part of the universe below the first inaccessible, and so if the continuum hypothesis is true an one model of ZFC reformulated in terms of plural quantification, then it is true in

all the models and so the continuum hypothesis is going to have a definite answer. It would take a bit of patience to verify or disprove the continuum hypothesis because you have to check equinumerosity with all possible subsets of the real numbers, of which there are one or two.

7.26 We have lots of results about things being undecidable in first-order ZFC. The only things we have that are undecidable in second-order ZFC will be if it implies some large-cardinal axiom. If you can prove that this sentence proves the large-cardinal axiom and you think it's consistent (but you can't prove it), then that will tell you that it is independent of plural quantification of ZFC. There also aren't that many second-order models just like with first-order models. It's so hard to construct models that there is hardly anything to say about second-order model theory.

8 March 20, 2017: Benacerraf, "What Numbers Could Not Be"

8.1 Von Neumann and Zermelo had different set-theoretic definitions for the natural numbers: $3 = \{2\}$ for one and $3 = \{0, 1, 2\}$. It seems like they are two ways of representing the same thing; but it also seems like these are two ways of misrepresenting because you don't really have strict identity here.

8.2 They're following a tradition that begins with Dedekind: you get an infinite set if you can it can be put in one-to-one correspondence with a proper subset of itself, and then you get a simply infinite set by taking something that is not in the image of the one-to-one function and then repeatedly applying the one-to-one function to it. Dedekind identified a simply infinite set and then said that we can take the natural numbers to be any simply infinite set we'd like. But if you really do it that way that it seems like each of us can have our own choice for what is the natural numbers; how would we be able to communicate?

8.3 Paul Benacerraf, who gave us this parable, said that each answer works equally well and so there is no reason to prefer one over the other. In fact, any simply infinite progression will be a satisfactory representation of the natural number system (originally with recursiveness, later he realized he doesn't need it).

8.4 Some definitions may serve better use or be more aesthetically pleasing, but this is a question of the referent of a term and about the definitions of the natural numbers.

8.5 Benacerraf specified that the progression needed to be recursive, but here it is a bit unclear what is meant by a set of sets being recursive (rather than a set of integers). Something maybe like the method of counting to obtain numerals needs to be recursive. It is easy to understand the association of numerals with sets of sets and to ask which of those types of sets constructions are recursive (there is ID

between an algorithm and a recursive set of integers). Recall that V^ω is the set of finite sets all of whose members are finite sets all of whose members are finite sets, and so on. From that perspective, it seems that Benacerraf is requires a much stronger condition: that the progression has to consist of elements from V^ω.

8.6 Once you identify the natural numbers with some simply infinite progression, then you can define the operations of addition, multiplication, and less than on that infinite progress and then after that you just treat members of that infinite progression in exactly the same way as everyone learns to treat numbers. The fact that we are talking about sets here doesn't make any different for how we do arithmetic or counting. What is important is that the series is simply infinite, but once you have that then arithmetic doesn't change.

8.7 We want to reduce number theory to set theory, so we're going to say that every numeral denotes a set. The embarrassment is that you need the numerals to name sets in some such way that you never have two numerals denote the same set, but beyond that it looks completely arbitrary which sets you choose. Why that is troublesome is that, if the numerals are names of numbers, then for a name to be part of a nonfiction story it must have a referent out in the world. So we have these numerals that grammatically act like names but have no referents, then are we saying that arithmetic is just a fiction. Benacerraf sees this as pushing us towards a skeptical conclusion. the idea is that, for a long time, people thought that the Iliad and the Odyssey were entirely fictitious and that there was no place as Troy. So if the Iliad is an entirely fictitious story, then there wouldn't have been any such city as Troy. There is (was) such a city, so the Iliad is not entirely fictitious. If there is nothing out there in the world that is referred to by "3", then is it fictitious? This situation is similar to color words ("red") in that you can use "red" both as an adjective and as a noun. When you use "2" as an adjective, you are going to us a plural noun; with color, you can also use with a singular noun (but you can't with "2").

8.8 Benacerraf ends the article with a bewildering sentence: "there are no such things as numbers; which is not to say that there are not at least two prime numbers between 15 and 20". What the heck does that mean?

8.9 Here's a possibility: does this all come about because people decided they wanted to reduce arithmetic to set theory? Zermelo was keen on having set theory as the foundation of mathematics (it makes for an efficient theory with a clean ontology). Do the problems we've been talking about persist even without the set theory?

8.10 Vann asks: could we have an example of an intrinsic problem? Maybe self-identity or being abstract? It really seems right to say that Gauss knew what the numbers were, although he certainly did not have any reduction of number theory to set theory. Maybe the problem is thinking that number theory should be reduced to set theory, maybe the problem is to say that number theory can be ontologically reduced

to set theory (as in, you can identity numbers with sets). Maybe the reduction of number theory to set theory went one step too far: the really useful thing was to see that the natural numbers are isomorphic to the several simply infinite progressions identified by Von Neumann and by Zermelo. That turns out to be really useful because set theory has powerful mathematical methods that aren't going to be available to you if you restrict your attention to numbers (there are features of $\mathbb{N}$ that are only recognizable by the methods of higher mathematics). But as an ontological reduction if you say that the only things there are are sets, then your mathematics doesn't change - you get all the mathematics from just realizing that $\mathbb{N}$ is isomorphic to the simple progressions. The new theorems you could prove after making the identification are things like $7 \in 17$. Vann is wondering if this is all a tempest in a teapot, where you get these difficulties and this controversy because you are insisting that the numbers are actually identical with the sets - but there is no benefit in that. Just say that numbers are their own kind of thing or throw up your hands and not decide whether the numbers are sets or not.

8.11 There were some really good motives that lay underneath the reduction of mathematics to set theory: one is that you need uniform definitions that could be understood across the different branches of mathematics (there is a lot of crossover in the field). You can't cross over if the definition of continuity in complex analysis is at odds with the definition of continuity in another field. In the 19th century, there were lots of quarrels because there weren't good definitions. There is agreement among mathematicians about what constitutes a valid proof, but this doesn't require the identification of the natural numbers with steps. It maybe leaves you with a slightly bloated ontology because now you have both sets and numbers instead of just sets, but the benefits of ontological reduction aren't all that great when the reduction isn't all that efficient. For example, reducing heat to molecular kinetic energy actually explains what heat is, but reducing natural numbers to sets doesn't add any explanatory power.

9 March 22, 2017: Benacerraf and Structuralism

9.1 Last time we talked about Benacerraf and his alarming conclusion that it is not possible to have knowledge about mathematical objects. Two parts to this: first, assuming that there are mathematical objects, we can't have any beliefs about them because to have beliefs about a thing you have to be able to refer to a thing, and to refer to a thing you have to be able to pick it out. We aren't able to pick out a unique referent for any of our mathematical terms because the referents of those terms are at best determined up to isomorphism, and so if there are mathematical objects we aren't able to have beliefs about them. Second, assuming for the sake of argument that we are able to have about them, there isn't the right connection

between our mental states and those things in order for those beliefs to count as knowledge. That's the conclusion he comes to and doesn't think he can escape from it (see the last sentence of the paper).

9.2 There is an analogy to Little Red Riding Hood: she doesn't exist, but within the story there is still a wolf between when she leaves and when she reaches her destination. similarly, there are no numbers but within the story of numbers there are at least two primes between 15 and 20.

9.3 The first part of the argument is that if there are mathematical objects then we can't have beliefs about them. Tarski's theorem requires knowing what the names refer to, but Benacerraf shows that they can't have any reference at all. For number claims to be true, the names have to refer. So truth-conditional semantics for mathematics can never get off the ground. If we want to say that mathematical statements are true, then that commits us to give them truth-conditional semantics. If the truth-conditional semantics for mathematical language has lower requirements than the rest of the language, then at best mathematics gets a lower status than the rest.

9.4 The question that came up at the end of last time was how we can choose a preferable candidate amongst a set of candidates for the referents of the natural numbers. But even if there was just one candidate we showed that a permutation among the elements of that candidate would still be a valid candidate, and so you're stuck with the same problem.

9.5 Benacerraf lays down a pretty demanding condition: that the compositional semantics needs to be uniform for different parts of the language. So if you have a semantics that tells you that for empirical statements that don't involve number you have a referential semantics based on your ability to refer to empirically found things, and then for mathematical semantics you don't use a referential semantics. But we want a homogeneous semantic theory. So Benacerraf would be dissatisfied by the idea that God can just fix the reference of the numerals.

9.6 There's the broadly structuralist idea that mathematical objects are only determined up to isomorphism. What we have are a bunch of intended models of mathematical language which are all isomorphic to one another, and even though none of those is preferred the same sentences are going to wind up being true in every model. For mixed statements, you're going to have a bunch of models that agree in what values they assign to the empirical terms, but different models are going to disagree in what they assign to the mathematical terms, but you know that the referents of the mathematical terms are isomorphic and so you know that the truth-conditions for the mathematical and empirical statements (and the joint statements in counting and measuring) are going to be the same in all the models. It's just going to be questions like whether $3 \in 17$ that is going to have different answers in different models. Benacerraf is going to reject this because it ends up not yielding a homogeneous

semantics. Any two intended models are alike in what they assign to the empirical terms yet differ in what they assign to the arithmetical terms, but the referents they assign to the arithmetical terms in the models are going to be isomorphic to each other. So you get really different truth-conditions for atomic sentences, but once you get the truth-conditions for the atomic sentences the compositional semantics will be the same.

9.7 There's an attitude prevalent in Quine that if you're going to have any abstract objects, then the mathematical ones are the least problematic ones to admit into your ontology. The reason for thinking that is that for the mathematical objects you have definite identity conditions, but for properties and propositions you do not have definite identity conditions. For Quine, the lack of definite identity conditions was a fatal defect ("no entity without identity").

9.8 The only real obligation of a semantic theory is to get the truth-conditions of sentences right. And the Benacerraf arguments (the permutation argument) doesn't really cause any problem with this because it doesn't attack whole sentences, just the referents of individual terms.

9.9 Is this the right thing to say? We have indeterminacy of mathematical referents, but there is nothing special about mathematical referents here - we have indeterminacy of reference everywhere we look (e.g. abstract objects). This almost seems more troublesome.

9.10 Maybe you could bring in a causal constraint in the non-mathematical case rather than the mathematical case. This is definitely what Benacerraf had in mind - that the crucial difference was that you could have a compositional semantics where you have a referential semantics for atomic terms (??) which is just a causal theory of reference. So we're back to Putnam's reply that a causal theory of reference is just more theory. For Skölem, he takes it for granted that we have a meta-theory and in the meta-theory we can describe these different models and see that countable model of set theory is unintended even all the right sentences come out true. Putnam said that if we have a meta-theory that we can regard as fixed and stable, then in the meta-theory if you assume that your terms refer than it isn't very hard to get determinate referents in the object theory.

9.11 A reason why the permutation argument is better than the original Lowenheim-Skölem argument is that the latter is very specific about first-order languages. But we don't do scientific or mathematical inquiries in a first-order language. But the permutation argument is much more versatile and applies to higher-order logic (in which the LS argument completely collapses) or even in model languages (permute the possible individuals in the possible-world semantics; you get all the right truth values for the modal and counterfactual sentences as well as for the vanilla sentences). That particularly matters because what we require of an adequate semantics isn't just

that it gets us the right truth values, it's that it gets us the right truth-conditions.

10 April 5, 2017: The Inscrutability of Reference

10.1 Last time we were caught up in the inscrutability of reference. Consider the sentence "Mr. MacGregor chased Peter Rabbit." There is one theory of reference that tells us in that sentence that the name "Mr. MacGregor" refers to Mr. MacGregor and the name 'Peter Rabbit' refers to Peter Rabbit and 'chased' refers to the action of chasing. Another theory of reference is: the name 'Mr. MacGregor' refers to Mr. MacGregor's unit set, 'Peter Rabbit' refers to Peter Rabbit's unit set, and the word 'chases' refers to the relation one unit set bears to another if the unique element of the first thing chases the unique element of the second thing. So we have two theories of reference and it looks like the two theories of reference do an equally good job of accounting for truth conditions, assertion conditions, etc. of the language.

10.2 How do you refer to actual things in the second theory if you can only refer to unit set? Vann had the idea that you could have the permutation of MacGregor and the unit set with MacGregor. This is really weird if you try to describe the other theory of reference, it looks like for some reason when I want to talk about Mr. MacGregor you wind up only talking about the unit set of Mr. MacGregor.

10.3 The thoughts underlying the inscrutability of reference are that the two theories of reference do an equally good job of accounting for how the speakers of the language use it. There is nothing more to making a semantic theory correct than its ability to account for the way speakers use their language.

10.4 With the second theory, how can you refer to the class of everything if your referent has to be greater than that? The answer is both there is no class of everything and that you're just permuting, not adding anything.

10.5 There is nothing more to linguistic meaning that just what the users of a language mean when they use it, the only way to say that one theory of reference better is to find a place where one theory doesn't capture the way users use the language.

10.6 You just need a one-to-one function π that sends a element to its unit set and vice versa.

10.7 The one constraint for creating these theories of reference is from Quine and Putnam who said that you could permute the entire universe under referent and then keep all the truth values the same. Vann thinks that this isn't sufficient, e.g. your name can refer to a galaxy and the galaxy can refer to you. In this situation eternal sentences will be true. But the things that are wrong here is that all you are getting is truth values rather than truth conditions, and that you want to get the truth conditions for occasion sentences and not just eternal sentences. Vann doesn't know how you're

epistemology is going to work if somebody tries to say that looking at you is a good way of determining facts of the distant galaxy.

10.8 There is this relation IC when somebody sees something, then the unit set of that person ICs the unit set of that thing. Both theories of reference give sense to saying 'rabbit!' when there is a rabbit present and not saying it when there is not.

10.9 For the first theory of reference, the term 'sets' refers appropriately but for the second one the definition is very wacky and the term refers to thing that either aren't sets or aren't sets of singletons. The well seasoned answer is the one Goodman trotted out when people objected to his groom example. They say: 'the way you use "green" makes it look like my use of the word "green" is really weird because there was a time when all the green things stopped being green'. But there are objective facts with 'set' where one is right and one is wrong. Vann thinks: there is a physical antecedence to the color referents (physical similarities in reflection patterns) that have caused the usage to be universal. So in the new theory there are drastic differences in the reflection patterns. Can we say the same thing about the set case? There are ways that certain sets are grouped together that seem similar to the natural differences in the 'green' case.

10.10 This is getting to a really crucial part of the argument: spectral reflections really are a natural property (physics), but the natural distinction between sets and non-sets isn't something that physics tells us about. It looks like 'naturalness' was invented because if we don't have natural properties that serve as reference magnets, then we wouldn't have inscrutability of referents, but we want those and so we admit the natural properties. But these 'natural properties' aren't at all like the actual properties that we find in nature. There is a more basic doctrine underlying it that the constraints on the semantic theory is that it's got to get truth/assertion/acceptance conditions right at the level of whole sentences. At the level of whole sentences, systematic ways of manipulating what the parts refer to that cancel each other out don't make any difference at the level of whole sentences and the only thing that determines the meaning of the word is its contribution to the semantic values of the sentences that contain the word. That's a crucial doctrine that people are relying on for the inscrutability arguments. There is only going to be a fact of the matter at what happens at the sub-sentential letter; the thought is that the beauty of permutations is that anything you do at the sub-sentential letter gets cancelled out - for example what you do with names gets cancelled out by what you do with predicates.

10.11 Peter Rabbit and the set of natural numbers have something in common. For the first theory of reference they have nothing in common, whereas for the second one they both are sets.

10.12 If we have rival semantic theories, part of having rival semantic theories is having different accounts of which properties fall under the extension of 'natural'. Can we just

kick the problem up a level and say that the two classifications are natural/unnatural just according to the semantic theory you are using but it's fine for my semantic theory. It looks like your theory of naturalness is indeterminate just like how all the other theoretical terms are indeterminate.

11 April 10, 2017: Structuralism

11.1 Hilbert talked about making a system of geometry where point, line, and plane are played by ball, beer mug, and car and it would be a perfectly fine geometry. Frege disagreed. This was a point where people stopped looking for a unique model, and is the break between old/new (even though Frege was so ahead of his time). Vann understands structuralism: what distinguishes the assertoric from algebraic system is that the assertoric systems have an isomorphism class of intended models. Algebraic systems also has a bunch of models but with assertoric systems the intended model is unique determined up to isomorphism. According to structuralism to ask what is the intended model for arithmetic is a misguided question. Van has been thinking of structuralism as a semantic thesis that you get by looking at the language of mathematics from the outside, and it's a thesis about the inscrutability of reference- it's saying that the referents of mathematical terms are not uniquely determined. Structuralism just says the referents of mathematical terms are determined only up to isomorphism.

11.2 But after thinking about it, it's not that the referent is undetermined, but rather that mathematical terms refer to patterns or structures. So we have all these isomorphic systems that are copies of the natural number system, Vann has been taking the characteristic doctrine of structuralists to be that there isn't any such thing as the natural number system and that what exists are just various structures that all end up being isomorphic. The structuralist view as Vann now thinks it could be understands it is that there are all these systems and then there is the common structure that they share, or the pattern that they exemplify. So $\sqrt{2}$ refers to a particular position in the pattern.

11.3 So the Gödel sentence is part of the structure of the natural numbers even though it's not derivable, and so the structure is not just what is derivable. Dedekind thought that the axioms determine an isomorphism class of structures, which means that any sentence that is true in one of the models will be true in all of the models. So you've done enough to pin down the truth value of each arithmetic sentence. We can do that because we're using second-order axioms; if we're using first-order axioms then the incompleteness theorem tells us that the Gödel sentence is true but not derivable. If we're using plural quantification or second-order logic (add to PA the LUB axiom), then you've uniquely characterized the natural number system so that

for every sentence either it or its negation will be a consequence of the axioms, but if we use second-order then we don't have the Gödel sentence (?).

11.4 This whole discussion got started by permuting an arbitrary structure and the result is something indistinguishable from the first. But can't we do the same thing with structures/patterns? Just swap the first and second place in the pattern. The answer is something like a permutation of a pattern is the same - or is there something built into the pattern so that positions in the pattern aren't distinguished. The pattern should be the thing that encodes what all the different permutations have in common.

11.5 First, there is the idea of a natural number system. So there are some systems, some of them are structured, and so some of them may instantiate the natural number system. Along with that comes a bunch of relational notions of something occupying the 3 position. But we've gone higher order very quickly. Before we were thinking as numbers as objects of some sort (in the range of individual variables). Now we're thinking of numbers as relations relating members of a system. Being a natural number system is a pattern that is universal, and it's not an ordinary universal that is true of individuals, but rather is a higher-order universal that is true of individuals collectively ("these individuals have the collective property of being countable"). But we seem to have gotten very far up the type hierarchy. The two main structuralists are Shapiro and Resnick and they come on different sides of this. Shapiro is a leading proponent of the user of second-order methods, he thinks that a lot of mathematics has been distorted by people insisting on first-order axiomizations. If we want the foundations of mathematics to be in line with how people actually do mathematics we ought to do as Dedekind did and think of natural numbers in second-order. Resnick is a Quine student and is very skeptical of second-order stuff.

11.6 A reason for not being an unapologetic mathematical realist is a reluctance to accept abstract things. Abstract things don't affect our sense organs, etc. But structuralism doesn't look to be any better because the patterns are abstract entities. For Aristotle, you have concrete things and universals aren't anything over or above concrete things, just looking at concrete things from an abstract POV. The Aristotelian view of universals takes them to be abstract in the sense that you're thinking about only certain properties that are relevant, but they still are concrete things. Platonic universals are completely isolated from experience. Resnick says that we're epistemically better of talking about patterns than Platonic numbers because patterns are things that we can see instantiated.

11.7 Anti-rem structuralism takes that these patterns exist on their own (they exist independently and autonomously of their instances). And the (en?) anti-re structuralism is the analogue to Aristotelian view; the pattern exist because of it's instantiation. There is an epistemic difficulty; the latter can say that knowledge of the patterns comes from knowledge of the particular instances. But there doesn't seem to be as

many instances as patterns; there are particular difficulties with infinite patterns.

11.8 How to create structure without instances? If just concrete objects, then just choose more than 2-to-the-2-to-the-aleph-not because that's the number of spacetime regions. If you allow abstract things, just make one up. Or just say that the natural number pattern exhibits the natural number pattern and so the pattern is an instance of itself.

12 April 12, 2017: Science and the Existence of Numbers

12.1 The law of gravitation tells us that there is a number G such that for any two spacially separated bodies with masses m_1 and m_2 there is a gravitation whose value is $G\frac{m_1 m_2}{r^2}$. All the principles of cosmology are derived from the fact that there is one such number. One might say that the Sun is pulling the Earth towards it, and you'd naturally conclude that we're going to die, but rather the Earth travels in an orbit about the sun. But to learn about orbits you rely on the fact that G is a constant. So the theory of G entails that the re are numbers. For philosophers, that is enough, as the worry of philosophers is that there might not be any numbers, and so if there are any numbers then we can be satisfied.

12.2 But G has units? Isn't it just a relation between two physical quantities? One way to say it is that there is a number G that is the ratio between two terms ($m_1 m_2$ and r^2) and then the physical units just allow it to serve in the ratio.

12.3 Can't we also say that it is a physical law that there are 7 people in this seminar room at this particular time? This is at least as true as the gravitation thing, so you could just say that the number 7 exists by the same form of argument. Is that any different?

12.4 Rephrase $\exists_{\geq 1} xFx$ as $\exists xF$ and rephrase $\exists_{\geq 2} xFx$ as $(\exists x)(Fx . \exists_{\geq 1} yFy . y \neq x)$, rephrase $\exists_{\geq n+1} xFx$ as $(\exists x)(Fx . \exists_{\geq n} y(Fy . y \neq x)$. There are exactly nFs if there are at least n and at most n Fs.

12.5 So there is a law of nature that entails that there is a real number. So if you are going to deny that there are real numbers then you are going to have to deny the law of nature. Wherever you look throughout the sciences, you find mathematical methods being used. The striking difference between modern science and the science of the ancients is that the ancients used numbers to measure lengths and areas and other things whose properties could be understood in terms of measurements. Most of ancient science made use of qualitative descriptions without mathematics. pretty much any scientific claim that you want to commit yourself to is going to have a similar status to the law of gravitation in that th is going to entail the existence of numbers. The argument is that pretty much anywhere you look in science you

are going to find judgments that entail the existence of numbers and so if you deny numbers you are going to have to repudiate those numbers.

12.6 Why is this a semantic thing instead of just a syntactic thing? The existence of G could just be within the formal language used to expressed the law of gravitation. But that g is the same for any two physical bodies means that there actually must be an underlying number.

12.7 We use mathematical objects to describe things, but mathematical objects are not useful for explaining things. The thought is: I want to buy a TV cart. I want to be able to describe in some detail the cart that I want. To describe the cart in detail without a lot of extra effort I am going to use a tape measure and measure the dimensions of the cart and then buy a cart that fits my dimensions. You could also do this relationally by saying that the cart is bigger than the chair but smaller than the couch etc.

12.8 One difference between numbers and physical objects are that numbers are useful for describing what we see, but numbers aren't useful for explaining why something is. But stuff like what molecules of water bounce around each other in certain ways ; before people understood Brownian motion people thought that molecules were really part of reality or they thought the they were like infinitesimals in calculus, but that's not reality which is that it is infinitely indivisible. As late as the end of the 19th century both of those views seemed pretty sensible. Now the view that molecules are genuine things is one out because Einstein showed that Brownian motion depends on the effects of particular molecules. So if you postulate the existence of molecules and their sizes then you can generate Brownian motion, but if you postulate that water is continuous then you cannot explain Brownian motion. A way to undermine mathematical realism is to say that numbers are like what molecules were like before they played an explanatory role.

12.9 But even the explanations without numbers don't seem to be particularly good explanations. For example in theoretical physics we just plug numbers into models and that's our explanation. For example, in Newton's Principia his explanation of why the planets have elliptical orbits (too hard for Vann), but Feynman's explanation of why the planets have elliptical orbits really makes it seem simpler than it really is. Feynman gives an explanation of how the orbits come out of the original equations from Newton etc. So it seems to Vann like Newton's explanation of why planets have elliptical orbits is the best possible explanation you can give, but this is a purely mathematical document. So the idea that mathematics does not explain things seems pretty ridiculous.

12.10 One thing that is going on that is making this more complicated than we'd hoped is that if the law of gravitation is always stated in a particular way, but it's not quite right in that what matters is that the ratio of the product of the masses to the square

of the distance is a universal constant of nature. For any choice of units of measure, there is a number that is G and it'll follow from that that if you choose different units of measure you get a different numerical value for G but all the calculations would go unchanged except for a constant factor. Back when people were engaged in the enterprise of trying to formalize unified science on a uniform basis, they were worried about things like whether they needed to include inpure numbers in the structure of the universe. The conclusion of that discussion was that they didn't need impure numbers. Different people are the same height, so there is something that they share regardless of what numbers you use. The thought at the time was that we don't need impure numbers, instead of asking what is John's height we'll ask what is John's height in inches. So with G you say that it is a physical quantity with a somewhat odd status but we don't have to worry about that because we'll just pick units of measure and relative to the unit system you use you get a value for G.

12.11 we do have pure constants in defining μ_0 in regards to electric polarizability of a vacuum. these are physical constants in maxwell's equations that we would want to say are actual quantities. so maybe even π has to be a physical quantity. you cannot get john's height as a number until you stipulate a system of measurement. on the other hand, π isn't arbitrary; it's going to be the ratio of the circumference of a circle to it's diameter, and it isn't relative to any choice of measure. and π appears in a lot of physical laws.

12.12 Why can we say that circles exist and that numbers don't? Doesn't the same reasoning apply?

12.13 Back to the law of gravitation, the force matters because knowing the force allows you to know how the position is going to vary because of $F = ma$. One thing that is sort of interesting here is that people think of $F = ma$ as a law of nature, but it's only because of a coordinated choice of the units of measurement that you have equality here. If you had chosen different uses of measure you'd have $F = 73ma$ or something and that would express the same law. But the really odd thing is that we're looking at the acceleration of the Earth-for that notion to make sense we have to suppose that the Earth has a trajectory and not only that but that there is a function that will predict its location at any particular point in time. Not only is there such a function but it's differentiable twice, so we have to treat the trajectory as something that is physical real in order for computations that use the law of gravity to determine how the planets are going to move. Philosophers are constantly saying that the only things that are real are going to be bodies, but it is hard to see how you could do basic physics if all you suppose is that all you have are bodies without supposing that the motion of the bodies can be described by ascribing a trajectory to them. Why that matters is that if a body has a trajectory then that trajectory is going to be isomorphic to the real line, so Newtonian physics seems require that bodies have

trajectories and the trajectory is something isomorphic to the real line, so Newtonian physics requires something to be physically real that is isomorphic to the real line. But what we've learned from structuralism is that to say that there is such a thing as the real line is that there just needs to be something that is isomorphic to the real line. So the real line exists!. All Dedekind required was a complete ordered field that satisfies the properties. Well the moral of structuralism is that we complete of every complete ordered field as an isomorphic copy of the real numbers, but there isn't actually one such thing as the real numbers but just the isomorphisms. So what is needed to make real analysis true is that there must be something that satisfies the axioms of real analysis. That's a pretty outlandish confusion-that this physical theory would guarantee the existence of numbers-but what is doing the work here is structuralism.

12.14 For next week let's talk about Hartry Field's idea that we don't really need numbers. This is very similar to Barkley.

13 April 19, 2017: Benacerraf on Mathematical Knowledge

13.1 Benacerraf had several skeptical arguments that were highly influential. One of them was based on a question: everybody agrees that if you know something, then you believe it and that if you know something, then it is true. But everyone also agrees that just having a true belief is enough for you to count yourself as having knowledge. Benacerraf proposes that for your true belief to count as knowledge it has got to be the case that the thing you believe has to have caused your belief. Benacerraf notes that facts about mathematical objects do not cause anything, and that mathematical beliefs cannot have been caused by anything that is going on in the realm of mathematics because nothing in the realm of mathematics has any affect on us at all. So there's a two prong argument: the arguments about the inscrutability of reference tell us that our beliefs purport to be about mathematical objects but our beliefs don't have the right kind of connection with the things about which they are supposed to be about. The second line of defense is, even if you say you have beliefs about mathematical objects for the sake of argument, since they are not caused by those objects those beliefs are not knowledge.

13.2 One form of skeptical argument is to say that there aren't any mathematical objects. Benacerraf is not going for that form of argument, but he's just saying that if there are objects then we don't know about them.

13.3 For example, Fermat's last theorem is an example of a justified belief that was not true for Fermat when he came up with it. He thought that he had a proof, but if he had looked closely at the proof he would've realized that it was not actually a proof.

So it's not clear that his belief really was justified. It often happens that we form a mistake and then look more closely and realize it and correct it.

13.4 Outside mathematics, few people are going to doubt that if you know something then you belief it. What is more vulnerable to doubt is the further contention that if you know something, then you believe it and the belief was caused by the thing you know. It seems to make the most sense about tangible things, but we would need an additional condition for things like patterns or abstract objects. Causation does not necessarily interact well with patterns. If we take true belief and add that it's a belief that is caused in an appropriate way by the thing about the belief is enough, the questions are whether that is enough to make it knowledge and if you have knowledge then do you have to have this causal condition in addition to being a justified belief. Starting with the idea that, at the very least, if you know then it is true and you believe it then is that necessary or sufficient to get you knowledge? Benacerraf is arguing that the extra condition is necessary because what is he trying to show is that we do not have mathematical knowledge, and for that he needs to show that there is a necessary condition that is not fulfilled by our purported mathematical knowledge.

13.5 Her is something that looks like a problem for Benacerraf: knowledge of the future. If you have knowledge about things that are going to happen in the future, but since the future event has not caused your current belief. It seems like knowledge of future events is based on knowledge of current events. "I know I am going to die" is not causally dependent on my death in the future. The Benacerraf condition for your future death is that your death must have caused me to belief I am going to die, and this is false. So the causal condition that Benacerraf lays out seems to be too demanding. But wouldn't he just say that you don't actually know that you're going to die? So just concede that you don't have knowledge about the future. Also disjunctive claims seem to disprove this: "I know that either I am going to die or I am not going to die". But Benacerraf would have to deny this.

13.6 The logical truths also seem like they are not going to be causes of anything. Also tautologies seems to be trivial and not have any propositional content. And we can assimilate mathematical truths with tautologies. This is why the case of logical knowledge is particularly important for us because there close analogies between logical knowledge and mathematical knowledge. It could be that we're able to say that we have logical knowledge without expected causal connection, maybe we have to say about mathematical knowledge. Maybe for synthetic and analytic claims are also such that the latter doesn't need a causal connection. You have to have a connection to Tom to know that he is a bachelor, but you don't have to have a connection to him to know that if he is a bachelor than he is married. It seems to go awfully far down the path of easy disbelief to say that we don't know that each of us will have mass and occupy space assuming we're alive tomorrow. The fact that

I will have mass is something that will happen to tomorrow and so cannot causally interact with me today and yet it still seems to really undersell our capacity to know things.

13.7 Benacerraf is going to have to deny that there is any synthetic a priori knowledge, even though we can get an exception for analytic knowledge. He gets the stuff about causal conditional knowledge mostly from Alvin Goldman who explicitly allows an exception for logical knowledge. But it seems like I can know that Cherry is going to continue to be warm-blooded (assuming she is still alive), or that the conditional "If Cherry is alive tomorrow, then she will be warm blooded".

13.8 One traditional way to try and cash out this idea is to say that we know general laws by induction (e.g. every dog anyone has ever seen has been warm blooded. and so draw the conclusion that all dogs are warm blooded). But inductive conclusions are never certain, because they are conditioned on your past experience. So this generalization may hold up in the future, but you cannot be certain about this. So if you don't require that knowledge does not necessarily need to be certain, in that you can just provide inductive evidence for a claim that have affected you, and you your beliefs for each particular dog being warm blooded fits the pattern. The observation that Cherry is a dog can lead to the conclusion that she will be warm-blooded tomorrow, so maybe what is going on is that Goldman was on the right path but just that he exaggerated because you can allow for uncertainty into knowledge.

13.9 Can we just do what Russell did with "If there are an infinite number of individuals, then P" when he added the Axiom of Infinity. Vann: unless you know that there are infinitely many things, then it doesn't look like you have knowledge. Instead of mathematical knowledge being knowledge of certain things, is mathematical knowledge is just a set of conditionals about possible existing things? Russell as a logicist really thought that mathematical knowledge is logical knowledge, and we've already allowed an exception to the causal theory of knowing for logical knowledge (Cherry is or is not a dog). But does that challenge Benacerraf's objection? Then there is no need for causation in the theory of mathematical knowledge and so Benacceraf's implicit premise that mathematical knowledge is a different kind than logical knowledge can be disputed. His belief to that effect is informed by the logicist program which was pursued by a lot of smart people and ended up not working. So it looks like Benacerraf is going to resist the idea that mathematical knowledge is a species of logical knowledge. He would agree the speculative possibility that if logicism was successful then Benacerraf's objections wouldn't matter.

13.10 But mathematical and logical knowledge are similar in that they are both necessary and neither relies on facts about the world to be true, and so they have the same epistemic properties. Benacceraf is going to deny this: there is a tradition that the truths of logic are analytic in that you can know them from reflecting on their content,

but the truths of mathematics are synthetic a priori. Arithmetic knowledge doesn't come to us by experience (don't touch, taste, feel mathematical objects). Gödel thought that we had some mental faculty for recognizing mathematical truths, and that mathematical truths forced themselves upon us in much the same way that other experiences do. He thought there was something roughly analogous to perception that enabled us to recognize arithmetical truths. If that's right then there is no problem, because we get mathematical truths from experience in the same way we get other truths about the world. They thought that before we were embodied we were able to have direct perception of the world of mathematicals. If that story is right then there is no particular problem about the epistemic status of mathematics. It's pretty hard to convince yourself that there is some kind of sense organ that detects mathematical truths the same way our eyeballs detect light. So you don't have to dismiss the idea because it was Gödel. Kant clearly saw the gap there and quickly proposed that we had knowledge that is synthetic a priori, which people thought was highly credible for a long time, but now most people do not find that view credible mostly because of non-Euclidean geometry.

13.11 The idea that all our synthetic knowledge has to be caused by what it is about does not seem right, even if just because of our inability to have knowledge about the future. A variant of that view that might fair better was proposed by Robert Nozick: if I know something, I believe it and its true, and moreover if the belief hadn't been true I wouldn't have believed it. What it wants to rule out is counting accidentally true beliefs as knowledge, for example beliefs you have because of magic 8 balls that happen to be true. On the other hand, if you think of a belief you get by examining something, then if it wasn't true then you wouldn't have had the belief to being with. What about necessary truths and if I believed that $2 + 2 = 5$? The thought that people have is that if the things your beliefs are about were actually hallucinations. If numbers weren't there, it would make any difference to what my beliefs are. Also if we believed $2 + 2 = 5$ even if numbers do not exist what does that entail about mathematical knowledge? But the standard semantics for counterfactuals does not apply to the material conditionals of mathematics. The standard Stolniker semantics say that for a conditional $a \rightarrow b$ to be true then you look at the closest possible world where a is true and then see if b is true. If there is not such close possible world or if a is not true in any of them then the conditional is still true.

13.12 Vann wanted to cover the Benacerraf argument and then ask whether the indispensability argument counters that and shows that we really do have mathematical knowledge. So we only did one of the things and we'll do the other next time.

14 April 24, 2017: Quine's Confirmational Holism

14.1 If knowledge is synthetic, then you have to be able to point to the cause of that knowledge. We're looking Benacerraf's arguments about mathematical knowledge and criticisms against the causal theory of knowledge. To a belief to count as synthetic knowledge, the belief has to have been caused by whatever it is the knowledge is about. The causal theory of knowledge is too demanding because it doesn't even seem to allow us to have knowledge of the future, but the idea underlying the Benacerraf complaint seems to have a lot of currency. If the knowledge is synthetic then it somehow has got to be grounded in experience, but you have to be able to tell the story about the causal change spawning the belief, and if you can't tell the story then the belief is not connected with the things in the world that make it true. We don't have an appropriate connection with respect to mathematical objects because they are not connected to experience at all.

14.2 Quine has an answer to that objection: it's a doctrine he gets out of Pierre Duhame and it is a doctrine of **confirmational holism**, the idea that we can see a particular phenomenon like for say an experimental result as, say, establishing some result about what the world is right, is an oversimplification. We hypothesize that there are electrons, we want to know whether there are electrons, we perform an experiment that confirms the hypothesis and if we do enough experiments than we can know that there are electrons. Duhame says this is too simple because your experiments validity depends on all kinds of background assumptions that are required to get supposed connection between the experimental results and the hypothesis you're trying to confirm. For example, you get a result in the laboratory, but saying that the result signifies about what the world is right depends on a prior understanding of how your instruments work. You take for granted that the instrument works because earlier on people had to contemplate the instruments, but even if it's in the background the connection between the experimental observation and the hypothesis depends on background assumptions. Benacerraf tells us that the belief being confirmed has to be caused by the fact that your belief is true. But that's to say that an event has a single clause. The application of this argument to the causal theory of knowledge is: no, it won't be the case that the fact that p was the cause of your belief that p because things never have a single cause. This is just a special case of the general idea that you never get a one-one correspondence between experimental results and hypothesis about what the world. This doesn't undermine the particular application to mathematical knowledge, but it does undermine the theory of knowledge. What does undermine it is the idea that you'll be able to say with assurance that this experimental result confirms this hypothesis or that it disproves it because the relationship is mediated by a body of theory. The body of theory might not be in doubt and so you may only be questioning the hypothesis, but this is just part of your psychological state.

Quine took this idea that there are no crucial experiments and that you can never regard an experimental result as indubitably confirming or dis-confirming a scientific hypothesis. Quine took this idea and give it a nice slogan: "our theories face the tribunal of experience as a whole". Your expectations for what you're going to see when you look into the microscope are there because of a large body of theory that leads you to beliefs about what is under the microscope and how microscopes work. If you don't see what you expect to see, then your expectations were thwarted and so there is something that is wrong with the larger theory that is wrong but you can't specifically pick out which part of the theory is to blame. Going the other way, if you look into the microscope and you see what you expect to see, then that gives confirmation to the theory on the basis of which you had the expectations; but we can't say well, it left the old parts of the theory as is but only confirms this one hypothesis. But this is fallacious because you can either verify the entire theory or none of that.

14.3 Quine is going to apply this argument against mathematical knowledge. We have this scientific theory that has mathematics deeply embedded in it as a part, and this theory has a really good track record successfully predicting the things we are going to experience. This means we should think of the theory as well confirmed. But confirmation is something that happens to a theory as a whole, so our whole scientific world view, broadly understood, goes up against reality and comes away victorious and by and large successful. The theory has this body of successes, the successes confirm the theory, and part of the theory is mathematics, which isn't something that you added onto the end. Mathematics is deeply embedded part of the theory, and so the theory of the whole is correct and so the mathematics is, too. Quine's extreme view was that all beliefs are interconnected as a web and he thought that there was nothing in the web that was so secure to be completely immune to doubt. So each of our beliefs are susceptible to doubt, and if we really wanted to cling to one belief we could just make adjustments somewhere else. He did think that there was a difference, though, in that there are some beliefs that are closer to the center of the web, some beliefs that are connected to almost anything else and so it would be very hard to separate them cleanly from the web of beliefs. So they are central beliefs that you are going to be really reluctant to change, peripheral beliefs are ones you can change much more easily. And he thought that logical and mathematical beliefs are part of this web near the middle. sometimes he say even logical beliefs are mutable, and other times he says that people talk about alternative logics but he thinks you can't really change your logic, so it is unclear what he really believes.

14.4 So something that happened several times was that there was an idea that started in Quine and Putnam took it up and, as a general rule, Putnam made it sharper and clearer. So, we ascribe the indispensability argument to Putnam and Quine. In Quine you never see him explicitly stating the argument and arguing for it (it's there,

but tucked in); Putnam was the one who made it clear. Another example of that was the idea that maybe even tautologies are vulnerable to experimental refutation. That seems to be an accurate representation of Quine's views on knowledge, but he sometimes seems to say things that go the other way. The person who clearly embraced the idea that tautologies can be rejected on the basis of scientific experiment was Putnam.

14.5 Putnams' idea was that there is a long history of people thinking they knew things on a priori grounds, but it turned out that they not only did not know them on a priori grounds, but they turned out not to know them at all because they were false. For example, Euclidean geometry was supposedly synthetic a priori knowledge. But it turns out that, from General Relativity theory (and the experiments that confirmed it) is that Euclidean Geometry is not true. Putnam wants to take a step further; consider the double split experiment. What Putnam said was that this shows us that our classical understanding of the world fails so much harder than how we thought it could. But this experimental results shows that laws of logic fail at the quantum level. The fact that what you get if you have two slits and you ask how likely is it for the photon will pass through one of the slits, but $P(A) + P(B) \neq P(A \cup B)$. Putnam thought that the thesis that the photon passes through either slit A or slit B is different from the proposition that it passes through slit A or it passes through slit B and gets through even though the two claims should be tautologically equivalent under the sequential calculus.

14.6 Quine's story is going to be that there is this web of belief and that things in the center of the web means a whole lot of the other things in the web depend on them, and so if you have an experience contrary to what you expected, you're going to have to modify your beliefs but as a general rule you don't want to disrupt your beliefs more than you need to and so given a choice you're going to be more inclined to disrupt the periphery beliefs rather than the core ones. Our mathematical and logical beliefs are near the center of the web of belief because mathematics is used so continually throughout the sciences that if you start messing with those (mathematics) you're going to see massive disruptions across the sciences. Most of the time you're going to want to avoid massive disruptions, most of the time you're going to want to adopt an adaptation that doesn't disrupt your system of beliefs a lot more than it has to. Mathematical beliefs are near the center and so mathematical beliefs are going to be such that you're going to give them up only very reluctantly. He doesn't think that any beliefs are immune to revision, but he thinks that they are near the center and so we're going to revise our mathematical beliefs only very rarely. That is why under the Quinian framework we want to seek empirical results that depend on mathematical results and that we're not going to be able to find the source of them unlike beliefs like "there are squirrels in the attic" because of the mathematical claims they depend on and so you can't reject your mathematical beliefs because they are so

firmly connected to everything else, and you can't seek out a single empirical belief that is the result of the mathematical beliefs. Benacerraf wants to use the fact that we can't point to any experience that depends definitely on our mathematical beliefs, he wants to point to that as a reason for why our mathematical beliefs are not really know. Quine wants to turn that around and say that we're going to have a hard time coming up with any possible arrangement of knew evidence that would lead us to question our mathematical beliefs, but that just shows that our mathematical beliefs are right at the center of our web of beliefs and so are least likely to be the ones we can revise. So because we can't come up with a confirmation experiment for mathematical beliefs is only because of their centrality to our beliefs.

15 April 26, 2017: Quine's Confirmational Holism

15.1 I believe there are spies. I also believe that no two people are exactly the same height. It follows that there is such a person as the shortest spy. What Vann is wondering about is should we also say: I am ontologically committed to the shortest spy. The example is a descendant of an example of Quine: there is a shadowy figure in a trenchcoat who Ralph met on the beach, and Ralph suspected that the shadowy figure is the spy. That means that there is someone Ralph believes to be a spy. So it's true that Ralph believes that someone is a spy. Ralph also believes that there are spies. There is some particular person who Ralph believes is a spy, versus Ralph just believes generally about the world that there are spies. In the first case, we want to say that there is someone who Ralph believes to be a spy. In the second case, we don't want to say that there is someone of whom Ralph believes that person is a spy. The existence of the shadowy character on the beach tells us that $(\exists x)(believes(Ralph, Spy(x)))$ versus $believes(Ralph, \exists x(Spy(x)))$. So what is required for the de re (the first) reading to hold is that you have to be able to pick out someone as a spy in an appropriate way. For Ralph to believe that someone is a spy, it is not enough for Ralph to just believe that there is someone who is a spy. Nor is it enough for Ralph to be able to provide a name of someone he believes to be a spy. Ralph believes he can identify a spy with the phrase "the shortest spy", but this phrase as he uses it does not pick out its bearer in an appropriate way; there is not the right connection between Ralph's belief and the bearer of the name for us to conclude from the fact that Ralph believes the shortest spy is a spy that Ralph has a particular shortest spy in mind. From that particular starting point, thinking about ontological commitments. A central question in the philosophy of mathematics is whether we should be ontologically committed to mathematical objects. Thinking in that context, Vann wants to know if the fact that I believe that there are spies and that there are no people with the exact same height is enough for me to be ontologically committed to the shortest spy.

15.2 One approach to this question is to consider that there are fictional stories that contain characters and names that in terms of the internal dramatic structure of the story play the same role as names of people, but do not play that role totally because that story is a fiction. There are also news stories with contents that we believe are true, and we believe that the names in those news stories do refer to actual people. The interesting case is when you have a story that you do not know to be either truth or fiction, and there are names in that story where you can ask whether the name actually refers. In the mid 19th-century is was the consensus opinion of all the scholars that the Illiad was a completely mythical story. In the philosophy of mathematics, are the numerals supposed to be treated as they are in stories that may or not be fictitious. Is this a good way to think of the ontological question. Mathematics is the story people tell one another; the philosophical question is whether the story is really true, and the way to ask that is to ask whether the number 17 really exists.

15.3 Quine set the stage for how people think about ontological questions. He said that this was the wrong way to think about it. He thought you were supposed to ask whether the name being referred to actually exists, because to ask the question that way is to presume that 17 does exist because 17 is the thing that the name refers and if there is no 17 than the name doesn't refer. A great project of Quine's life was to fight off Meinong. Meinong had this idea that in addition to the things that exist, there are also things that don't exist like the Golden Mountain. So if you imagine a thing of a certain then it still has some kind of existence.

15.4 Quine wants to reject this because he wants to reject the ideas of grades of existence. He also wants to fully reject quantified modal logic, he has this paper "Three grades of modal involvement". The least incriminating grade is when we talk about necessity as an attribute of sentences. The version of necessity Quine regards as harmless. There is a second grade where you have modal operators applied to sentences, where if the sentence expresses a necessary truth then you can express that fact about the status of the sentence by putting a necessarily in front of it. Quine thinks that this is not so bad because in saying "Necessarily bachelors are unmarried" all you are saying is that the sentence "Bachelors are unmarried" is unnecessary, and so there is this confusion/misleading way of talking from Carnap where you take this metalinguistic statement and pull it down into the object language but prefixing a modal operator. So "Necessarily, all bachelors are unmarried" is a fine ascription but it's misleading because of the metalinguistic crossover. The worst kind is where you apply the necessity operator to sentences with free variables, where we want to say that if something is a brown dog then it is necessarily a dog but only contingently brown.

15.5 Vann took a class from Quine at Harvard, he taught Phil 140: introduction to logic and stood up in Emerson Hall and read 3x5 index cards without looking up for the

entire class. It was only much much later that he discovered that Quine was, in fact, Quine and that if you got him out of the lecture hall he is actually witty and entertaining. The reason why he did that is that he worked very hard at getting Methods of Logic so that said exactly the things he wanted to say in precisely the way he wanted. If he gets up and just started talking he would only say something approximately the same, but what was actually going to be beneficial was to actually read the book. Vann also went to one of its graduate seminars where he had Set Theory and its Logic written on 3x5 cards that he read without ever looking up.

15.6 So a connection between modal logic and Meinong is that a reason people were willing to go in for quantified modal logic was that they discovered a plausible semantics for quantified modal logic. The plausible semantics involved talking about individuals that exist only in other possible worlds. The first grade of modal involvement just talks about sentence, but we're going to be committed to sentences so that is okay. You can introduce the modal operator without introducing ontological commitments, but the semantics for the modal sentential calculus require that we can talk about other possible worlds, so we need to allow the existence of worlds other than the actual world. That looks like we've plunged into deep mysticism, but it's not really that bad because we talk about something being possible as it's being true in some other possible world, but that just be a vivid way of talking. You could also say that something is possibly true if there is some complete, consistent theory in which it is true. But the idea of a possible world in the sentential calculus is just an index, it does not have to be anything like what the world is like. In fact, the origin of the semantics for modal sentential calculus came in work of McKinsey, Tarski, and Johnson in the 1940s. In their initial work "possible worlds" were just points in a topological space where necessity took a set of points to its interior and diamond took a set of points to its closure. In, the third grade of modal involvement, we not only talk about possible worlds but we also talk about possible individuals in possible worlds. At that point, we're really drinking the Kool Aid.

15.7 So the thought is that once we adopt the semantics for quantified modal logic then we find ourselves talking about individuals that exist only possibly, so we get these things that don't exist that have something like an outer domain consisting of all possible individuals, and then within it we have a sub-domain comprising those among the possible individuals that happen to actually exist. So if we have a robust sense of reality, then we're going to want to reject this idea that there are things that don't exist. What exists, exists; there aren't these other things that have these shadowy existence. That at least is what Quine wants to tell us. He wants to reject quantified modal logic because he thinks once you go down this path you're going to end up with non-existent things. And what is, is, and what is not, is not. There is a philosophical doctrine for you.

15.8 What would Quine respond to the question of whether we necessarily exist? He

would have to deny that the question is sensical because he believes that there is only type of existence. It seems pretty shocking that he's committed to this view. What he says is that acceptance of quantified modal logic plunges into the nightmare jungle of Aristotelian essentialism.

15.9 How does Quine deal with counterfactuals? Is he going to say that every counterfactual statement is false? If you reject quantified modal logic, then you reject counterfactuals and can't seem to say that much. You can also say stuff like "If the Reimann Hypothesis holds, then this algorithm will terminate within these bounds". Quine can't distinguish between something like a mathematical implication and things like "If we're here on Monday, then we'll be in class". And clearly counterfactuals are going to be a problem for Quine. There is a story that people tell about counterfactuals whose best statement is a paper by Nelson Goodman called "The Problem with Counterfactuals". It's that you regard a counterfactual as true if it the consequent follows by law if it is entailed by the antecedent and some certain set of background assumptions. He thinks that the problem of counterfactuals is a serious problem because you seem to need a theory of counterfactuals to get an understanding of disposition terms. And without some good account of disposition terms you can't really have a good philosophy of science. So you want to account for what it means to say that "something is soluble" or "something is an electrical conductor" even if you never put it in water or apply an electrical voltage to it. You want to say that something is an electrical conductor if when somebody applies a voltage to it, then it conducts the electricity. But things that never have a voltage applied to them do not entail that those things are conductors, so you have to exclude them. So many of the key terms of science are disposition terms, and we don't seem to be able to get a clear acocunt of disposition terms if we can't get a clear account of what is going on with counterfactuals. Goodman had this idea that we can conlude the material conditional (if a voltage is applied to this, current will pass through). So there is a beginning of a story there and Goodman worked very hard at trying to fill in the stroy but kind of gave up, and the study of counterfactually pretty much stalled there until Stalnaker took up the question. So possible worlds seem like they are metaphysically costly because they apparently involve these shadowy entities that are there in some sense even if they don't exist. But as theoretical entities they are able to carry their weight because you need them to understand counterfactuals and it seems like you need them to understand propositional attitude constructions.

15.10 Quine didn't flat out reject counterfactuals, but he did think there was something dubious about them. Example: both of these are things that you could say. "If Caesar had been a commander in Korea, he would've used the atom bomb" and "If Caesar had been a commander in Korea, he would've use catapults." There is some level of context dependence so that what counterfactuals are true does not just depend on your idea of what the world is like, but also on what is important to you. With

the math examples (If the Reimann hyptohesis were false), the standard semantics doesn't take account of them because it says that to say that a counterfactual is true you look at the nearest possible world in which the antecedent is true and ask whether the consequent is true in that world. But for mathematical statements, if a statement is true then it is necessarily true and similarly if its false, so you want to look at the nearest possible world in which the Reimann Hypothesis is false. Quine seems to have to force all conditionals to collapse into material conditionals, and if he wants to avoid talking about possible worlds. Even with Goodman's account he takes a specific possible world and makes it so we talk about it as if it were not a possible world. Quine thinks that if you want a theory that is clear and explicit, the way to do that is to formulate your theory in the first-order predicate calculus. He realizes that we don't talk that way, but his idea is that it's part of the job of philosophy to see how to take both the statements of regular people and scientists and reformulate them in a way that's clear, correct, and precise. That's not on the frontlines of scientific work, it's rather a clean-up after science has made the big discoveries. Part of what philosophy does is to make things clear and explicit that were kind of brushed over and to try and unify and clarify. To unify and clarify a theory, the best option is to formulate it in the first-order predicate calculus. Part of what leads him to think that modal logic is such a mess (he worries about things like the possible fat man in the doorway (is there only one of them? could there be a bunch of them? are there more possible thin men in the doorway then possible fat man in the doorway), but he thinks it is such a waste of time to deal with all those. So part of the reason to go for the predicate calculus is to avoid modality and propositions and things because in order to really understand something you have to have clear identity conditions for it. We don't have clear identity or individuation conditions for propositions or modalities, but we do have these for sets and so if we want a really good theory we need to stop talking about properties and rephrase everything that we would want to be a property in terms of just classes. Properties are tough because we don't have a good grasp on when two properties are the same, but have a great grasp of when two classes are being the same. So talk about the class of brown things instead of the property of being brown, etc. There is also the historical fact that in the first half of the 20th century, mathematicians got into immense tangles because they were trying to work within higher-order logics that classified things into logical types that turned out to be a bad approach, not in terms of any broad philosophical picture but rather in terms of what is successful mathematically because what is successful turned out to be giving up the theory of types and go straight with the first-order predicate calculus. If you want to understand a person's ontological commitments, ask them to formulate the theory they want to tell as a enumerated set of axioms in the first order predicate calculus because then you will know what their ontological commitments are. So to ask whether someone is committed to the shortest spy,

don't bring in these non-existent things from possible world, just ask whether you belief there is a unique spy who is shorter than every other spy. So to make any real progress in ontology, you want to avoid getting bogged down in things like which of these possible things exist or which of these things that subsist actually exist. If we want to make real progress we want to simplify the logic so as to make things as clear as possible, and that requires formulating things in the first-order predicate calculus. This is what Quine recommends. If you want to make real progress, you need to be explicit about what the ontological commitments of your theory are, and to do that you jneed to formulate the theory in the first-order predicate calculus and then look at what existential sentences are true. If your there shows that there exists a so-and-so, then your theory is committed to so-and-so. If you formulate your theory in the first-order predicate calculus, then you have a definite ontological opinion that people can evaluate and disagree or agree with. If you don't do that or refuse to, then Quine thinks that you're refusing to make your own ontological commitments plain.

15.11 Vann thought we were going to finish the indespensability argument, but we will just try that again next week.

16 May 1, 2017: Vann's Softened Indispensability Argument

16.1 Vann wants to begin today presenting a different take on the indispensability argument, and he knows that on sociological grounds that what he is going to say has got to be wrong because the indispensability argument has been discussed by a lot of people, a lot of them were a lot smarter than Vann and didn't come up with that idea.

16.2 Two features of the indispensability argument, one of which we have already talked about (that it depends on a hypothesis of confirmational holism where knowledge is a interdependent corporate body). If you've just been presented with the causal theory of knowledge in the way that Benacerraf presents it, then the confirmational holism seems like a welcome change because the causal theory seems far too demanding. For each of our well-established beliefs that counts as knowledge we can point to something in experience that causes that belief, but that view seems way too strong. It still seems plausible that there is something in between.

16.3 Another feature of the indispensaiblity argument is that the conclusion it draws is something methodological. You look at the recent history of science and convince yourself that a methodology that forbade mathematical reasoning would be inadequate for the purposes of science, and so you've convinced yourself that we need mathematics in order to do science. That's what the indispensability argument

shows, the conclusion that people wnat to draw from it is that there are numbers. In order to get that there are numbers, you need to get from: scientists needs to assume that there are numbers in order to do their work to that there are numbers. So you really need to some form of transcendental argument to make that leap.

16.4 That step seems awfully dubious. It seems a lot like wishful thinking: we're going to get into trouble if there are not any numbers and so there must be numbers is not a particularly strong argument. Well, if you're trying to choose a theory, it looks like other things being equal you're probably going to choose the theory in which there are numbers otherwise. But you don't really have the ability to make the conclusion that there are numbers, you just need to be able to say that you're making certain assumptions in order to carry out certain computations and calculations that require that you reason as if there were numbers. So we better at least be willing to pretend that there are numbers, we need to be able to reason about scientific contexts as if there were such things as numbers. Having a methodology that fundamentally relies on pretext is disreputable and dishonest, wouldn't it be better if we move forward with things as they are without having to engage in some forms of elaborate pretense. That seems like a pretty good argument, but it doesn't seem like a compelling argument. We need to reason as if there were numbers, the easiest story to tell is just to say that there are numbers.

16.5 One concern that people might have is that having the pretense of numbers allows you to explain current phenomena and explain what you observe, it seems to take some of the credibility to make predictions off of that. Vann thinks that this may go the other way: if you think that there is a chance that future experience is going to empirically show mathematics for the fraud it is, well the whole brief against mathematical knowledge is that these mathematical judgments were neither confirmable nor refutable by experience. If you think, well no, there may be some experience that could refute mathematics, then it looks like, okay, there could be experiences like that but we haven't had experiences like that. Well the fact that we haven't had any conflicts seems like a reason that the totality of scientific experience confirms the mathematics. If you think that mathematics cannot be confirmed by our experience, whatever experience is, well if you think that it can be disconfirmed by our experience that it really isn't off in its own world.

16.6 If all you need to do is compute things, then does this change anything? It does seem like applied mathematics that can actually be pretty complicated, in that the geometry people had to develop for the purposes of special relativity was a lot more complicated than the geometer mathematicians working on their own. On the other hand, if we're really worried about philosophers skepticism towards mathematics, it's that the philosopher 's inclination is to say that there aren't any numbers at all, and that mathematics is entirely fictional. There is plenty fo room for saying that there are numbers but that parts of the theory of numbers but that large parts of

analysis. Vann doesn't think that philosophers don't have much to contribute to that conversation.

16.7 Vann's crazy idea is really just taking what Putnam says at face value. Putnam points to an illustrative example of why mathematics is needed for science: the law of gravitation. The law of gravitation tells you that there is a number that marks a certain regularity in nature, that there are these ratios of physical quantities that is always the same (it doesn't change in time or based on the particular bodies it is relating). It is a ratio and this ratio is a number. So the law of gravitation asserts that there is a number that plays such and such a physical role. Therefore, there is a number–that is, the law of gravitation entails the existence of numbers. We don't need methodological holism; the bit of methodology that everyone is committed to is that the logical consequences of well-confirmed theories are well-confirmed. You don't need any fancy methodology for that, just: you have good reason to believe p, and $p \rightarrow q$, then you have good reason to belief q. The fact that you have a good reason gives you sufficient justification by anybody's standard to accept q and so we don't need the hypothesis of methodological holism, all we need is tha the consequences of well-confirmed theories is well-confirmed. So you have good reason to believe in the existence of numbers.

16.8 Moreover, the way Putnam and Quine thought of the argument is that they get this methodological conclusion that mathematics is necessary for science, and then you need some sort of transcendental argument to get to the conclusion that there are numbers, but we don't need to go that route the conclusion that we get from the law of gravitation is that there are numbers. So you don't get that the laws of classical mathematics are true by looking at the law of gravitation. Philosophers are worried skeptically about: not that classical mathematics gets the laws of the calculus wrong. The philosopher's worry is that there are not any numbers at all, because you are not able to encounter numbers physically. To answer that skeptical worry you don't need a big methodological premise, you just need the law of gravitation.

16.9 How is this different than saying that the number of fingers on my hand to my hand is 5:1 so picking the law of gravitation is a good example. There's a level of reasoning that high school students can do that will get you there. Also Newton had a law that if you weigh something you determine what its mass is and so if you know what its mass is you know how it is going to respond to any kind of force that you pout it under (magnetic, weak/strong gravitational, pressure, etc.). This one number tells you all kinds of things about how the body is going to behave in a great variety of circumstances.

16.10 One thing that Vann thinks is going on here is that you get the attitude that the question of whether there are numbers is a question about what metaphysical commitments you take on . You get that attitude from taking a perspective where you

as a philosopher are able to stand outside of theories and select the best of several theories and the one that commit you to numbers seem to latch onto nature very simply, and the ones that deny there are numbers latch onto reality with great difficulty, if at all. That's a reason–the fact that mathematics figures so prominently in science–means that theories that commit themselves to numbers have a much easier time mediating the gap between our thoughts and nature and so that's a reason to go for that theory. Thinking of it that way, in terms of us being able to stand outside the process of scientific inquiry and choose one that we like best, that's an idea that figure really prominently in Carnap and that Quine rejected. Rather Quine said that developing science and developing a methodology of science happened at the same time. There is no way that we can just drop the science itself and just focus on the methodological part. There isn't a process from choosing among scientific theories that is different from just engaging in the process of science. The idea in Carnap that there are these ontological questions or external questions that are only answered by standing apart from science like this was a key idea of Carnap that Quine emphatically rejected. Well, if he had carried that thought through, then he would've seen that this argument doesn't just show something about the methodology of science, but also about the theory of science in that our best theories of science entail the existence of numbers. Surely some fragment of our scientific theories is well validated and so is the existence of numbers.

16.11 So there is a deviation from the tradition that mathematical knowledge has to be a priori. It doesn't mean that the knowledge is a posteriori, also if its a priori that doesn't mean that you can't get at it through experience. That in the old days would've been surprising, but Vann thinks this isn't really surprising anymore because the alternatives are either that mathematical knowledge is analytic which doesn't look promising because so many bright people failed at making that story work correctly. The other alternative is that mathematical knowledge is synthetic a prior, which also had a really great run but was really founded on Kant who didn't want to say that mathematical knowledge is synthetic a priori but rather that our knowledge of geometry is synthetic a priori, and Euclidean geometry has been pretty thoroughly thrashed qua not holding up physically.

16.12 For all the statements that are well-confirmed, can you discern the nominalistic and platonistic content from these. In each case, the world has to be a certain way in order for each to be true. So we can think of them as conjunctions of nominalistic and platonistic content, but all the confirmation comes from just the nominalistic content and we never have any confirmation of the platonistic content. Vann thinks that this is the perfect question because next time we are going to talk about Hartry Field. To anticipate what is going to happen then, it's really hard to make out what's the nominalistic content of the law of gravitation. Things move without numbers, but things may not have instantaneous velocity without numbers. If you can succeed in

singling out the nominalistic content of whatever scientific theory, then we can say the things we now say about forces and masses without bringing in any kind of numerical measurement. Maybe the nominalistic content could be phrased in terms of "if there are abstract mathematical objects, then XYZ." This is different from Russell because people couldn't agree on whether or not there were actually infinitely mathematical things as an empirical hypothesis. But the standard nominalist story is not just that there aren't any numbers, but if God had worked on the 7th day there would've been numbers but in fact there aren't any numbers. That doesn't seem like the standard nominalist position; it's more like, we know what things there are (physical things), and they just deny the existence of abstract things. Those things aren't even possible under the nominalist view; a standard mathematical view upon platonists is that these objects are possible and the nominalist is going to say that such abstract objects are impossible.

17 May 10, 2017: Quine on Quantified Modal Logic and Hilbert's Program

17.1 If I asked Quine whether somebody else would've killed Kennedy if Oswald hadn't, it seems like that would be a yes or no question. It seems like either there was or there wasn't a person who would've otherwise killed Kennedy. This seems like a counterfactual, but if you can express it such that you don't really have a counterfactual (either there was or there wasn't). So Quine pushing back on quanitified modal logic. Quine would actually like that answer, but there is a question of whether you need quantified modal logic to express counterfactuals. Something Quine would resist to his last breath would be trying to explicate this in terms of possible worlds. He wouldn't necessarily object to that if it is regarded as a vivid way of speaking. He's definitely going to resist any idea of possible-world realism.

17.2 Goodman back in the 40s had this analysis of counterfactuals where he was trying to get at the idea that the conditional is true if you can derive it from the antecedent together with the laws of nature. That doesn't quite work because there are non-laws that come into play ("if I strike a match then it will light" requires it not be raining, etc.). So we need some range of empirical facts to be admissible. First he said just take some true empirical facts and then you can derive the consequent. But then that's not right because one of the empirical facts is either the match wasn't struck or else it is lit. So you have to pick the right kind of facts; the best he was able to do was to say that you derive the consequent from the laws of nature and empirical statements that are co-tenable with the antecedent. The way Goodman understood counterfactuals was that the antecedent has to be false. So you take factual statements that are co-tenable with the antecedent such that changing the

antecedent changing wouldn't affect the facts. An example of this is "the match didn't light" which would not play well with the antecedent. Vann thinks that's the direction that Quine would want to go in. He would not want to go into hardcore metaphysics with some sort of explanation in terms of deriving it from the laws of nature.

17.3 Quine is going to have a general attitude that there is this hard theoretical work of trying to take the empirical truths and you divide them into the ones that are the ones that aren't cotentable. If you can do that then you will have your theory of counterfactuals. But you're not going to be able to do this because you are not going to be define "co-tenable" without referencing counterfactuals. It's a matter of rebuilding the boat while you sail on it. If you wanted to do this, you could use our modern understanding of modality to create a definition of co-tenable that doesn't require already knowing the counterfactuals. So you can start with the counterfactuals and then get a consistent definition of co-tenable and then hopefully at that point you can kick away the ladder. But this requires a lot of work, it is not going to be just a simple theory.

17.4 Quine really didn't like quantified modal logic, but recall that he thinks there are three different levels of modal engagement. One (like Provability Logic), you can think of as a species of necessity and there is not anything sketchy about it at all. The second grade that is with the modal operator (?) - his real objection to that is that you don't get anything useful that you don't get from treating necessity as an attribute of sentences. But instead you get embroiled in all kinds of pseudo-questions. He complained about the iterative application of modal operators. The third grade is where you have quantified modal logic. That he objected to because he could see no basis in our modern scientific worldview for trying to distinguish necessary from accidental attributes. What we now think of as common sense is often just the remains of outmoded scientific theories.

17.5 If I say something along the lines of "I could not existed", usually you make some kind of modal statement. If Quine thinks that this is non-sensical. So everything that exists, in fact exists, but it seems like you could push it to the future and ask questions like "Is it possible that my future children do not exist". He is not going to deny that you don't have any children. It seems like you can't distinguish from the chair that actually exists (but contingently so). But it seems like what Quine wants is to say that the distinction is meaningless. So if the only meaning of the existence predicate is "exists in the actual world", It seems as if you can't there is no way to distinguish between "Water is H20" and "The chair is black". The laws of nature aren't metaphysically necessity.

17.6 What about moral necessity? How does that work with only the actual world. Diego: If you only have one world to worry about, then when you say like "I ought to do

something" then thinking of the ought as necessity. One understanding of what necessity is is analyticity, about which Quine has worries. Those worries aren't the same as worries about modal operators. He despairs at being able to draw the analytic/synthetic distinction. But if you make the distinction then there are still some sentences that are true by virute of meaning and some that are contingent on what is the case. This is either a semantic or epistemological separation. Bringing in the other possible worlds doesn't seem to add anything to help you understand. The analytic/synthetic disinction is hard at best, and bringing in necessity isn't going to make it any easier. There seems to be a problem in separating de dicto and de re modality - the first is just necessity as an attribute of sentences, but the second is cashed out as whether an individual has a certian property in all possible worlds.

17.7 There is a situation where there is a man who Ralph met on the beach who he suspects of espionage, and there is the fact that Ralph accepts like everyone else that there are spies. On the de re reading it's he like all of us thinks there are spies, and the de dicto reading means that there is actually a person who he thinks is a spy. Something to point out is that bringing in possible worlds doesn't really seem to work with Ralph.

17.8 Hilbert had the idea that you have a mathematical theory as a syntactic object, and you ask what is this theory about. That question came up for Hilbert when he was writing his book on the foundations of geometry, and he didn't feel that you could any longer accept that geometry is about the structure of space. The connection between geometry as a mathematical discipline and space had become tenuous at best. He wanted to do geometry but didn't see the traditional answer to the question of what geometry is about. So he came up with a different way of responding to the question which may seem more like rejecting the question than answering it. He said that you shouldn't go outside mathematics to figure out what geometry is about. How we should think of geometry is that the theory implicitly defines the subject matter. So the theory has a bunch of axioms, geometry is about whatever systems satisfy those axioms. That's all that geometry is about. He wanted to say that for a mathematical statement to be true means that it is true in every system that satisfies the axioms. But then any arithmetic statement is true because just make up axioms to prove it. For us to be able to say that mathematics is about anything at all is that the mathematical theory is consistent. If the mathematical theory is consistent then its true, but that just seems like an abuse of language. But it's better than that; it's the idea that to say its consistent is to say that its theorems are true when the language is interpreted. So you think of a language as a string of empty symbols; to get true and false you have to interpret the language. To say that its true means that it is true on its intended interpretation. If anything is going to tell you what a theory is about you have to look inside the mathematics.

17.9 This is a development from after the idea of mathematics as a game like chess. You

can identify a way of moving your bishop because of what is permitted by the rules, but you wouldn't say that these are true. That was the old version of formalism. So Hilbert's idea was that for the axiom of geometry to be true, all that is required was that the they are consistent. Why this isn't just an abuse of language is for the axioms of geometry to be true, what we mean is "true in the intended models" and the intended models are given by the theory. And the indispensable prerequisite for that is that the theory is consistent. A question Hilbert would've been unlikely at the time Hilbert was writing but today seems natural is: "By 'consistency' do you mean syntactic consistency (cannot derive contradition) or do you have a semantic notion in mind ('consistency' means has a model)." To identify consistency with truth in the intended models, it must be that in order to have consistency you have to have a model. So sometimes it looks like he is requiring semantic consistency, but he also wants to talk about consistency as a syntactic notion. Vann is not sure that Hilbert was making that distinction (nobody was making that distinction at that time). What you need to erase the distinction is a completeness theorem (to say that anything that is syntactically consistent has a model, we have this for the first-order predicate calculus). Obviously, when you move beyond the first-order predicate calculus you're not going to achieve this completeness result and so you will have disparate ideas of consistency that cannot be reconciled.

17.10 What Hilbert was giving was very programmatic - he is not giving the results of a program but rather he was giving an idea of the way he wants the program to go. Maybe he just thought the program would determine a suitable set of axioms so that the two types of consistency coincide.

17.11 Say we have a theory with non-logical axioms extended from first-order predicate logic. It is not possible for it to be syntactically consistent but not have a model and it is not possible to have a model but not be syntactially consistent. Why doesn't this mean they are the same? Hilbert's axioms for geometry were not quite formalized, but you can see how you would formalize them in a second-order theory. A lot of his aximos you could just write down as first-order formulas, but there are two places where you can't: the Archimedean principle, for example. Geometry was under suspicious but there were not problems with arithmetic and so he was perfectly happy to use arithmetic for the geometric axioms. So the Archimedean Property can be phrased as: for any A, B, C on a line in that order, you can make line segments congruent to AB and lay them end to end and there is some number n such that n copies of AB starting at A will eventually pass C. This is stated in terms of arithmetic. The other place where he goes beyond is the completeness axiom. There are going to be a lot of systems that satisfy the axioms (for example $\mathbb{R}^3$ satisfies all the axioms). What you really want if you want to capture what the lines are like, you need the idea that there are no gaps between the numbers. You could use LUB principle as an axiom, but what Hilbert takes as an axiom is that the system

is as large s it could be, so that you can't embed the model of the axioms within any larger model that still satisfies the axiom. So you can't get like non-standard numbers because one of the axioms is the Archimedean Principle and if you try to blow it up you will violate it.

17.12 Now we have a distinction that Hilbert didn't really make between semantic and syntactic notions of consistency. To get a notion of truth that isn't just consistency by another name, we really need to take the relevant notion to be semantic consistency. So to say that geometry is consistent (and also that it is true) means that it has a model.

17.13 When somebody says that in the future it could be proven that there is a contradiction in arithmetic - in that case we have semantic consistency but then we do not have syntactic consistency. But this isn't true because if it is syntactically inconsistent then every sentence is derivable and so our model is no longer valid.

17.14 So a conclusion that Vann wants to draw is that if we take this idea from Hilbert that what is required for a mathematical theory to be true is just that it have a model. If you think that way, then you look at teh arguments Field gives about being able to do things like being able to do classical Euclidean geometry on purely physicalistic terms. The conclusion that we should draw from this is that Field has shown how to get models of classical mathematics, but interpreting the mathematical terms is about various appropriate physical things. So Field has convinced us that modern physics shows that there are models of the axioms that describe real analysis. So Field has shown that classical mathematics is true (exactly what Field does except the opposite, since an argument against nominalism and instead for Hilbert-style structuralism). So with the modern scientific world which is filled with fields it does not seem like you are going to get very far without having actual points in spacetime. Field thinks that if you look at the natural world then you will find in nature all kinds of continuous magnitudes, and whenever you have a continuous magnitude you have an isomorphic copy of the real numbers. So physics gets you that there are models of real analysis.

17.15 We're about to get this part of Field that is really baffling which is the thesis that for any extrinsic explanation there has to be an intrinsic explanation. That looks like the guiding principle that is fundamental for his treatment of geometry. On page 44: "underlying every good extrinsic explanation there is an intrinsic explanation. So then real numbers unlike electrons have to be eliminable from physical explanations, the only question is how this ought to be done." Texts that have the word 'intrinsic' in them are often completely useless. Vann can see strong motivations for wanting to be a nominalist, but Vann doesn't see any reason why we should upend our worldview in order to follow this principle.

18 May 17, 2017: Vann's Argument from Utility

18.1 Vann was thinking back to something we talked about earlier. Duhene insisted that when you get an experimental result there is an explanation for the result you get. There is one part of the explanation that is central to your thinking where you think that this aspect of the system was what is being tested. But, in fact, events don't have a single cause and so the result of the experiment does not have a single cause (there are background assumptions like how the measuring apparatus works). Those assumptions aren't usually made explicit because they aren't what you're interested in (although they can be made explicit if you keep getting unexpected results). If you had enough reason to think the hypothesis was true, then you have reason to go test the background assumptions that you thought to be sound.

18.2 Going the other way, you go for a long time using the apparatus and never questionsing it. Whenever you get unexpected results, you sooner or later are able to resolve them by adjusting some part of the hypothesis. That looks like you're practice in using the instruments never breaks down can serve as a confirmation that the instruments are working well because the issue of whether the instruments work is never at issue.

18.3 Vann is going through this because it seems like a good way to think of mathematics that appears as everyday science as part of the measuring apparatus. You have these phenomena that you are trying to explain, and you have to use these instruments to inspect that, and part of those instruments is the mathematical tools that are background parts of the measurement apparatus. So there is no way you can understand what is going on with the experiment if you think of the experiment as testing whether the needle moves left to right. In reality, your experiment is a test of the voltage, and voltage is a numerical calculation. There are circumstances that are going to lead you to question the reliability of your instruments. But the more reliable the instruments have proved in the past, the less likely you are to blame the instruments if they are at fault.

18.4 Classical applied mathematics is the most established background assumption there is. Metaphysicians have thought about whether the results are really literally true. But there is some sense outside of literal truth whether the mathematical results you use in your scientific experiments are either correct or incorrect. People have been doing mathematics for a long time, and have not found reason to question its reliability.

18.5 Quine and Putnam thought that QM showed that the law of the excluded middle is false, that is the only case David could think of where we might question our background assumptions on mathematics if an experiment produces unexpected results. The only thing that Vann could think of that might be the other way is that Einstein showed us that geometry was wrong (he didn't show that theorems in Eu-

clidean textbooks is wrong, but you understand what is meant by this). Although it's a strange way to see what happened, but it's only a strange way to think of it in retrospect. What Einstein did was to change our concept of geometry so drastically. He didn't show that geometry was wrong, he just showed that geometers were just looking at this tiny, tiny corner of geometry. Einstein's results also messed up a lot of non-Euclidean geometry, in which space was also thought of as uniform (Einstein showed that space is going to change from place and place and is going to change based on something non-geomtrical, notably mass).

18.6 Ancient Greek mathematics was developing tools for describing space, and once you had those tools you could use them for other purposes (e.g. pulley ratios). But the structure of mathematics was the structure of space. Mathematics today is much more versatile; mathematicians think of themselves as providing tools for describing whatever reality is like. As it seems with General Relativity, the mathematics of the day didn't succeed in describing what geometrical reality was like–so, we just need new mathematics to handle this. But the new mathematics encompasses the old mathematics in the sense that it was developed off of it. So how it looked to the mathematicians was never that the old mathematics was shown to be wrong, but rather that we have a bigger mathematics which contains as a subset the old mathematics and the new maths makes up for the physical explanatory power that was lost when the old maths failed.

18.7 Electron microscopes are not found in nature. Electron microscopes exist because somebody designed and constructed them, and it was important that whoever did that knew what they were doing. It is important that somebody understands how electron microscopes work, but it's by no means something a user of that tool necessarily needs to know intimately. What the virologist needs to understand is how to use the electron microscope. Engineering questions about how the microscope works might come into play if the virologist is using the electron microscope in ways beyond what it was designed to use, and so it is dubious whether it is functionality as intended. But what you need to know to get scientifically valid results from electron microscopy is that you need to know how to use the instruments. I'm wanting to suggest something like that in terms of thinking of mathematics as part of the measuring apparatus in our big pursuit of a description of reality. What you need to know to do this is to use the apparatus. But what you need to know to use in order to make rational number talk is knowing how to use them. And using rational numbers to measure things is mostly about being able to divide something of the same kind as the kind of thing you're trying to measure into a bunch of little same-sized pieces. To determine the mass of something, you need to get a bunch of little unit weights and then ask how many of them do you need in order to weigh the same thing. So the practical part of it is knowing how to take whatever you're trying ot measure and divide it up, and if you can't do that then take something else that is

either smaller/bigger/equal than what you are measuring and then divide that up and check the ratio.

18.8 In terms of the physics you need to be able to have divide something physical into equal pieces and a scale to compare different bodies. In terms of mathematics you need to be able to count the pieces. So there are a variety of things we'd like to measure, and for each one there is a task like with dividing something. But the rational mathematics you can do this, but for real numbers you can't do this except for theoretically. You get real numbers as the hypothesized limit of that process. So you could narrow down the error in the thing that is being weighed into smaller and smaller intervals, and you hypothesize the existence of the limit of this process.

18.9 Numerals are singular terms, singular terms outside of mathematics have referents, and fixing the meaning of the singular term requires determining what its referents by picking out a single one of them. For maths, you can pick out positions within particular structures but you can't pick out particular things. Vann is inclined to say that you don't need to pick out referrents for mathematical objects in order to do science correctly. What you need to know in order to use mathematical language correctly is knowing how to use mathematical language correctly and doing measurements - and small children are able to do that. There is no real mystery of learning about what are the referents of the natural numbers if all we require for what the terms mean is how to use them to carry about our measurement.

18.10 In learning how to use the numbers to measure, one part of that is learning to use rational numbers to measure lengths. Part of doing that is learning how to divide the line segment into a bunch of equal sized parts. Some kind of understanding of wholes and parts allows you to be able to divide something, and if you want to compare line A and line B you divide line A into pieces and see if when put back together you get line B. So there is a physical, geometrical part of that process and then there is a purely mathematics part of just being able count how many pieces there are and calculate the fraction. If you want to relate $\frac{X}{100}$ and $\frac{Y}{500}$ where you have divided something into 100 and 500 equal parts, then you need to be able to do this process which is an easy algorithm. The thought is that what you need to know to know what this numerical language means is how to use these numerical methods to get results of measurements. Doing that is just a matter of learning an algorithm. What makes that an anti-realist sentiment is the idea that you've only learned how to use the language, you haven't learned anything about what the terms refer to. Some people would say that genuine realism requires that the mathematical terms refer, and learning how to use numbers and counting doesn't enable you to be able to pick out referents. Another way of thinking of realism is that you get results, they are either objectively true or false. One part of it is the idea that what is crucial is that you get objective answers that don't depend on the whims or the mental processes of particular individuals, rather you get definite answers that are the same for everyone

and only dependent on the features of the things being measured. In one sense, that is enough to make it realist (although this is softer than the first requirement).

18.11 Question: It seems like you aren't able to refer to a specific thing because you don't know all the different ways to use it (so you don't know all of its properties). But you might be able to say that you are referring just to the useful subset of its features. In a sense, you are kind of like refering to something that has a vague background because you don't know all of its properties, but it seems like knowing how to use it you have to have some understanding of some parts of features. Vann: It's never going to be that you know all the properties of anything. You could try saying that in knowing how to engage in the process of measuring things and giving rational numbers as the outcomes of measurements you're never going to get an exact measurements and are just going to be able to measure it to a certain level of acuracy.

18.12 Quine's ontology is just to include everything and he thinks that serious metaphysical inquiry that tries to asnwer the ontological questions needs to pose the questions in those terms (in terms of formalizing the first-order predicate calculus?). If you can't do that, then you are not in a position to hold what you say as true. That is what Quine says, but now we're liberated and can talk all kinds of ways. One thought is that then modal methods or counterfactual reasoning because we don't need unification anymore. That seems very liberating. But then that just seems liberating because you're no longer trying to say stuff that is true. Once you start talking in fiction, you can really go wild.