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12.1.24

Replies to Carroll, Horwich and McGrath[1]

I am grateful to Sean Carroll, Paul Horwich, and Sarah McGrath for their stimulating responses to *Morality and Mathematics (M&M)*. Their arguments concern the reality of unapplied mathematics, the practical import of moral facts, and the deliberative and explanatory roles of evaluative theories. In what follows, I address their responses, as well as some broader issues.

1. Reply to Carroll

M&M argues for a 'partial vindication of the Kroneckerian view', according to which God 'created a unique set of…integers, but myriad variations on other structures (179).'[2] This position combines mathematical realism (§1.2) with pluralism, modulo arithmetic objectivity (§1.6). It says, at first pass, that all of mathematics, except arithmetic, is like pure geometry, as the traditional platonist conceives of it. The caveat about arithmetic is added because 'a fragment of objective arithmetic is not only indispensable to science but even for intelligibly framing' the reliability challenge (understood as the challenge to show that our mathematical beliefs are *safe*, i.e. roughly, that we could not have easily had systematically false ones).

Carroll suggests that I have not gone far enough. Following empiricists like Quine and (early) Putnam, he segregates mathematics in two sets. The first 'is the set of claims that are (or were, or will be) relevant to describing some part of physical reality (XX).'[3] 'In such cases,' Carroll maintains, 'mathematical statements can be translated into statements about physical reality.' Carroll does not say how to carry out the translation (or what he means by 'relevant to' or, indeed, 'physical reality' – see below). Whether it is possible to translate *impure* mathematical claims (e.g., claims about spinor fields on spacetime) into statements about physical reality is just the question of whether a nominalization program like Field (1980) can succeed.[4]

Consider the universal state vector of Everettian quantum mechanics (to which Carroll subscribes in Carroll & Singh (2019)) and the Hilbert space in which it lives. State vectors and Hilbert spaces are normally thought to be mathematical objects that correspond to, or represent, physical aspects of the world. They are not supposed to be physical objects themselves. What, for Carroll, are the physical correlates of the universal state vector and its Hilbert space?

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[1] Thanks to Will Cavendish for feedback on Section 1.
[2] All references to my own work without a date are to *M&M* unless otherwise noted. Undated references to page numbers in discussion of Carroll, Horwich and McGrath refer to their contributions to this symposium.
[3] Carroll's epistemology of applied mathematics is actually more similar to the empiricism of Mill (1882/2009) and Kitcher (1985). But Quine (1986, 400), like Carroll, explicitly jettisons unapplied mathematics as literally false.
[4] I should say 'usefully translate' because *Craig's Theorem* ensures that for any first-order theory incorporating mathematical language, there is a recursively axiomatizable theory lacking that language with the same nonmathematical consequences. But the latter theory has no theoretical appeal (*M&M*, n. 19).

It might be thought that Carroll could just take the likes of state vectors to be physical in their own right – albeit mathematical in an obvious sense too. He speaks of 'subsets of the world' (XX), which are presumably sets (!), as if they were physical (*cf.* Maddy 1997). The problem is that this would obfuscate Carroll's nominalism, the view that 'mathematics does not exist in the same way that the physical world does (XX)'. Do sets of apples exist in the sense of sets or apples?

Carroll is more explicit about (what he designates) unapplied mathematics. Carroll says that, in the case of unapplied mathematics, 'We are…left with a form of 'if-thenism'...in which true statements are of the form 'if these axioms are accepted, this theorem can be proved…(XX).' A familiar problem with if-thenism, thus understood, is that it engenders if-thenism about (classical) logic, by Godel's Second Incompleteness Theorem.[5] Consider a theory, *T*, that is (classically) consistent, *Con*(*T*), and interprets *PA*. Then *M* |- *Con*(*T*) while *M** |- ~*Con*(*T*), for select metatheories *M* and *M** (for instance, let *M* be *T* + *Con*(*T*) and *M** be *T* + ~Con(*T*)). Is there no non-conditional – or, let us say, 'categorical' – fact as to whether extensions of *PA*, like *Zermelo-Fraenkel* (*ZF*) set theory, are (classically) consistent?[6] Carroll responds, 'the question of whether a…theory is consistent is not really the important one…(XX).' But let the theory, *T*, be (a regimentation of) Carroll's favored physical theory. If ~*Con*(*T*), then *T* implies, and thus predicts, everything. So, if there are only conditional facts about *T*'s consistency, then there are only conditional facts about whether *T* is confirmed by an experiment. That seems important!

Carroll adds, 'To be clear, *reality* is consistent, essentially by construction (XX, italics in original).' But I do not know what this means. Does it mean that no true theory of reality implies a contradiction (or, by the Completeness Theorem, lacks a model)?[7] If any such theory of reality is *categorically* consistent, then we can piggyback on its interpretation of consistency to give categorical content to the claim that any other theory, *T*, is consistent. If consistency is always conditional, then so is 'reality's consistency'. (Perhaps by 'consistent', Carroll means *possible*? Even if the claim that 'reality is possible' makes sense (what is its logical form?),

[5] Carroll could retreat to the view that unapplied mathematical sentences, '*S*', is typically shorthand for the claim that *S* is a *second-order consequence* of some contextually specified axioms (perhaps adding that the axioms are consistent and the implication is necessary, as in Hellman (1989)). But since knowledge of second-order consequence is at least as mysterious as knowledge of arithmetic, interpreted at face-value, it is unclear what this would accomplish. (Of course, no one advocates the view that '*S*' is shorthand for the claim that *if* [insert the conjunction of finitely-many contextually given axioms], *then S*, where 'if…then' is the material conditional. This is true , no matter what *S* is, if there are no numbers to satisfy the axioms.)

[6] Carroll could regard this as applied mathematics. But then I am unsure how to draw even a vague distinction between pure and applied mathematics. In fact, Carroll does not regard it that way, as I discuss presently.

[7] I set aside Carroll's remark, 'essentially by construction'. Dialetheists believe that (truths about) reality are *actually* inconsistent. So, they certainly deny that something's being real guarantees its consistency. See Priest (1995). (Sometimes Carroll appears to suggest that I have mixed up belief in the reality of theories with belief in the reality of what they talk about, i.e., first-order quantify over. He writes, 'there is an important distinction between 'the Standard Model is real' and 'the Standard Model represents real things.' The scientific realist must be committed to the reality of nature, not to any particular representation of it. The Standard Model is not reality, it is – as the name indicates – a model of it. It would be a mistake to attribute reality to any tools we might use to describe reality (XX).' However, I certainly recognize a distinction between belief in electrons and belief in representations of electrons. I suggest (§1.2) that a realist about an area, *F*, believes in the independent truth of some *atomic F*-sentences, interpreted at face-value. This implies, by Existential Generalization and the *T*-schema, the existence of *F*s.

possibility is considerably more obscure than (first-order) consistency (Quine (1953), § C.2, 2019).)

If there are no categorical facts about the consistency of theories, then there are also no such facts about many other things – including what a theory, language, well-formed formula and, of course, natural number is. Consider a computer running a program which, from our standpoint, will never halt. Perhaps it looks for proofs of '$0 = 1$' from *PA*. It will not halt after *1* minute, *2* minutes, *3* minutes, and so on. From the standpoint of a nonstandard model, it does halt – it finds a proof of '$0 = 1$' after a nonstandard number of minutes. Is there a categorical fact about what *would* happen if such a computer *were* run? Carroll responds, 'If we physically construct an automated theorem-proving machine that starts with seemingly reasonable axioms, will it ever prove a contradiction? Like most people, I'm happy to believe that it would not. But by making the worry more vivid, we've changed it into a question about physical reality (XX).' However, 'physical reality' is the crux. We are considering a counterfactual. We can call this question 'about physical reality' if we want. But it is not about the actual world. We have not, and – let us stipulate – will not run such a computer. Like exercises in physics textbooks, our question is about what *would* happen if counterfactual conditions *were* realized. If there are no categorical consistency facts, then there is also no such fact as to whether such a computer *would* halt.[8]

I do not mean to dismiss skepticism about unbounded quantification in fundamental physics. As Hilbert (1936/1983) emphasizes, and some quantum theories of gravity illustrate, it seems (epistemically) possible that the universe is finite. We should not *build in* to every physical theory that there are at least *n*-many things, $\forall n \in \mathbb{N}$. The problem is that even when a theory lacks unbounded quantification, its metatheory, as ordinarily understood, will not. It will interpret *PA*. So, an authentic physical theory according to which the universe might be finite needs an *ultrafinitistic* surrogate for the theory of syntax. I regard the development of such a surrogate as a central open problem not just for logic and philosophy, but for physics. Despite provocative proposals (Nelson (1986)), Quine & Goodman (1937), Yessenin-Volpin (1961)), it remains unclear what 'formal system' should even mean once we give up on concepts like recursion. (One can replace the requirement that a set of axioms, well-formed formula, proof and so on be recursive with the condition that it be 'feasibly recursive', under some analysis of

[8] Carroll could renounce all talk about counterfactuals, and limit himself to predictions about actuality. But, then, why does he believe that no contradiction will be found in the Peano Axioms, if not because those axioms are consistent? In some passages, Carroll indicates sympathy for the view that many, maybe even most, counterfactuals lack the truth-values that they seem to have. He imagines that the universe contains *N* things, for some natural number, *N* (*cf.* Zielberger (2004)). He writes, 'If *N* is sufficiently large, there are no physically realizable numbers (or numerical values for the quantity of a collection of objects) that we *could* ever add together to read it. In that case [a theory with ordinary addition and another with addition modulo *N*] would be physically equivalent. There would be no Platonic truth of the matter concerning what answer one *would* get when adding *N-1* to itself (XX, italics added).' Carroll appears to suggest that counterfactuals involving the addition of things that will never be added, but whose sum is less than *N*, have the truth-values that we expect them to. But counterfactuals involving the addition of larger sums do not have the truth-values that we naively compute. (Carroll does not say what he means by 'physically equivalent'. He clearly does not mean empirically equivalent. But he also regards the Schrödinger, Heisenberg and Dirac pictures of quantum mechanics as physically equivalent (personal correspondence), despite their *prima facie* disparate metaphysical commitments. My own view, in keeping with *C.2*, is that there are simply different criteria of equivalence that are useful for different purposes. Even 'equivalent in meaning' is ambiguous insofar as there are different kinds of meaning and propositional content. See McCarthy & Clarke-Doane (2022).)

‘feasible’. The problem is to define feasible recursion as something other than ordinary recursion – which an ultrafinitist claims not to understand – meeting additional constraints.)

Before turning to Horwich’s contribution, let me note that the categorical/if-thenist distinction, which corresponds to the objective/pluralist one (§1.6), craves clarification. Pluralism should differ from the view that our mathematical concepts, including our concept of consistency, are indeterminate and indeterminable. (Even this view requires care. If we can determinately specify the different models between which the object language is supposedly indeterminate, then our mathematical concepts are not indeterminate in the metatheory, at least. If we cannot, then what do we mean by ‘indeterminate’?) But everyone (barring finitists and ultrafinitists) should agree that there are nonstandard models if there are standard ones. So, what is there to dispute if not just how we do, or can, use words? The set-theoretic case is even more vexed. The claim that every consistent theory has a model is just the Completeness Theorem (a theorem of standard set theory). But the claim that every such theory has an *intended* ‘model’ is not naturally formulatable in first-order *ZF(C)*.[9] So, how should the pluralist communicate her ‘mental picture’ (Shelah 2003, 211)?[10] Obscurities like these lead many to treat pluralism as unformalizable (Balaguer 1995, 6; Hamkins 2012, 417).[11] Maybe it is a non-cognitive attitude, like (a common reading of) Carnap’s Principle of Tolerance (Awodey and Carus 2001).

2. Reply to Horwich

Another thesis of *M&M* is that, contrary to widespread belief, ‘there is no *epistemological* ground on which to be a moral anti-realist and a mathematical realist (177).’ Horwich disagrees. He argues that our ordinary mathematical beliefs are on better epistemic footing than our moral beliefs because the former are justified by their role in our scientific theories. This distinguishes our mathematical beliefs from our moral beliefs, for which no uncontroversial source of justification is known. In fact, Horwich is skeptical that such a source of justification is even desirable. What, he asks, would be the *practical* import of knowing the moral truth? Horwich concludes, ‘My bottom line….is the intuitively unsurprising contention: that our mathematical beliefs tend to be easier to justify than our ethical beliefs…because….The standard claims within any area of substantive mathematics are justified by their roles in the best scientific explanations of naturalistic phenomena…But no norms of…justification for ethical beliefs have ever been established – presumably because truth in that domain…[has] no practical importance (XX).’

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[9] *ZF(C)* does not permit quantification over classes. The best that one can do is speak of defining formulas.

[10] Shelah does not, to my knowledge, identify as a pluralist, though his ‘mental picture is that we have many possible set theories….I do not feel ‘a universe of *ZFC*’ is like ‘the Sun’, it is rather like ‘a human being’...(2003, 211).’

[11] Cohen’s method of forcing appears to have been the impetus for pluralism among set theorists. Bell (1981, 358) writes, ‘The techniques [Cohen] invented have led to an enormous proliferation of essentially different models of set theory and the rise of a ‘relativistic’ attitude toward the set-theoretical foundations of mathematics. This attitude involves abandoning…the idea that mathematical constructions should be viewed as taking place within an ‘absolute’ universe of sets with fixed and predetermined properties. Instead, one works in suitably chosen models of set theory having the properties required to carry out the construction in question (italics in original).’ However, for reasons surveyed in *M&M* Chapter 2 (*cf.* 2012), I do not recognize an *epistemological* difference between axioms whose independence is proved using forcing and others, like Foundation or Infinity. Of course, this is not to say that all axioms are on an epistemic par. As stressed in the first section, those of (first-order) arithmetic are special.

Before rejoining, let me note a point of agreement. I *concur* that moral truth has no practical importance, if this means that knowledge of the moral facts fails to settle what to do. That is the point of the 'New Open Question Argument' (§6.5-6.6) (to be discussed in my response to McGrath).[12] But Horwich's speculation complements this argument with an error theory of moral disagreement. Why is there persistent moral disagreement? Perhaps because it reflects *practical* discord, which answers to idiosyncratic attitudes, not (shared) truth. If moral inquirers *were* seeking the truth, then we might expect them to unite in agnosticism. As Mackie observes, 'scientific disagreement results from speculative inferences or explanatory hypotheses based on inadequate evidence, and it is hardly plausible to interpret moral disagreement in the same way (1977, 37).' An analogous genealogy might also explain persistent disagreement in mathematical and logical foundations, if ''debates' about new axioms are…practical in this way (*M&M*, 183).'

Why, then, does Horwich suppose that some mathematical beliefs 'are justified by their roles in the best scientific explanations of naturalistic phenomena'? Not just because 'In explaining the observations that support a physical theory, scientists typically appeal to mathematical principles…(Harman 1977, 9–10).' Horwich *agrees* with *M&M*'s criticisms of Harman that explanatory indispensability is not sufficient for empirical justification (§3.7 & 3.8, Chapter 4). (He does not say whether he also agrees that it is not necessary, as per §3.6.) Horwich takes belief in a mathematical theory (like a pseudo-Riemannian geometry that is applied in General Relativity) to be empirically justified only when that theory figures into our best explanation of naturalistic phenomena *and it is interpreted as being about physical things* (e.g., spacetime).

The problem is that this again requires a useful distinction between mathematical and physical things – a distinction that I have found wanting. Consider the local gauge theories of the Standard Model. These are fiber bundle theories.[13] Charged particles 'curve' internal spaces, and are affected by the curvature in turn. Potentials are connections giving the curvature of the space. Each fiber is a copy of the space and has the symmetry of the gauge group. The fiber bundle is the collection of all fibers. (The theories are 'local gauge' theories because, at each point in spacetime, we are free to choose a phase in charge space, which axes to call electron and neutrino axes in isospin space, and which axes to call 'red', 'green', and 'blue' in color space, like we are free to choose coordinates in General Relativity. The objective facts are those which are indifferent to the local gauges.) Suppose now that we 'nominalize'' the internal spaces, as Field (1980) does Newtonian spacetime. Perhaps the result is even 'intrinsic' (Field 1980, 44-50). Still, what is the *epistemological* advance? Even if knowledge of spacetime, with its extensionless points, and $P(\mathbb{R})$-many regions, is more tractable than mathematical knowledge (contra Resnik 1985), I cannot see why reliable belief about *internal spaces* would be. Outside of classical mechanics, there is little epistemological difference between mathematical and physical things.

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[12] I am unsure whether this is what Horwich does have in mind. He writes, 'it may be that…all that really matters with regard to ethical beliefs is which ones people actually have, and not at all which ones are true. For the practical implications of moral beliefs…are completely independent of whether that belief is true (XX).'

[13] The interpretation of local gauge theories is debatable. But I know of no interpretation of the Standard Model, according to which there is an important *epistemological* distinction in general between mathematical and physical things, which is the point I seek to illustrate. See Healey (2007) and Arntzenius (2012, Ch. 6).

The upshot is that any application-based restriction on mathematical structures would be problematic.[14] It would require distinguishing mathematical from physical entities, and would reinvite the reliability challenge (Chapter 5). We could have easily had systematically false mathematical beliefs insofar as we could have easily had different ones. *If* one is a mathematical realist, *then* one should be a 'pluralist' – a realist about every intelligible mathematical structure.[15]

Note that everyone already accepts such a position (however unwittingly) in logical case, and logic and mathematics are treated similarly in scientific practice. Nobody denies that *there is* intuitionistic consequence, *FDE*-consequence, and quantum consequence if there are any kinds of consequence. The live questions are semantic and normative. Does 'consequence' out of our mouth pick out classical consequence or one of these other relations? Whatever it picks out, is that the relation that we *ought* to consult in regulating our reasoning? (Actually, this last question does not quite get to the bottom of things, as I discuss in Section 3.) Questions in the foundations of set theory, like which concept of set is most fruitful for a purpose, are analogous.

Pluralism does have dizzying implications for physics.[16] Consider a family of sets of points in spacetime. Does their intersection exist? Relative to *ZFC*, it must. But relative to, say, Kripke-Platek set theory, it may not. Does it *really* exist? If pluralism about set theory is right, this is like the question of whether two events are really simultaneous. There is no once and for all answer – no final structure to the spacetime manifold (or whatever replaces it in quantum gravity). The logic case is parallel. Consider a quantum spin system, *S*, with eigenstates |↑> and |↓>. Do the laws of physics allow that *S* is in the *indeterminate* state of being neither |↑> nor |↓> nor *a*|↑> + *b*|↓>, for any complex numbers, *a* and *b*? What about the *contradictory* state |↑> and |↓>? If our background logic is classical, then the answer to both of these questions is 'no'. But if it is paracomplete, tolerating indeterminacies, then the state, neither |↑> nor |↓> nor *a*|↑> + *b*|↓>, is in *S*'s state space. If it is paraconsistent, then the state |↑> and |↓> is in that space. And if First-Degree Entailment (*FDE*) is our background logic, then neither |↑> nor |↓> nor *a*|↑> + *b*|↓> as well as |↑> and |↓> are all logically compatible with the laws. There is no categorical question of *how the spin system could have been*. There is just the question of how it (likely) will be – and, hence, which logic to use in specifying the states that we might (epistemically) see.

What, though, do I mean by 'intelligible' in 'every intelligible mathematical structure'? I take the boundaries of intelligibility to be the central point of contention in the philosophy of mathematics (and a source of its relevance to systematic philosophy). Predicativists deny the intelligibility of impredicative definitions (outside of arithmetic); finitists, infinite sets; ultrafinitists, Graham's number of particles; and classical logicians contradictions and indeterminacies. Whatever the right account of intelligibility, it will be *indefinitely extensible*.[17]

[14] This allows that there are good reasons to focus one's work on certain structures as opposed to others. The overwhelming majority (in some intuitive sense of 'majority') are useless and uninteresting.

[15] I have in mind here 'ineffable' rather than simple pluralism, in the sense of Clarke-Doane (2023), Sec. 1.3. But I do not have room to discuss the distinction here.

[16] This paragraph closely follows one in Section 12 of Ash and Clarke-Doane (forthcoming).

[17] One possibility is that there is no fact of the matter about the boundaries of intelligibility (and not just in the sense to be noted momentarily). Maybe 'intelligibility' is like 'set', 'possible', 'follows from' 'grounds' and so forth according to *M&M, C.2*. Then debates about its limits, insofar as they are not just about what we happen to mean by

Given any intelligible set of principles for a structure, it is intelligible to imagine them weakened somehow. But *not* every set of principles is intelligible (despite Mortesnen (1989), Priest 2016))! Therefore, any alleged collection of all and only the intelligible structures must contain too little or too much. Whenever we lasso only intelligible structures, we find that we could have included more. Contra Wittgenstein (2014, Sec. 4), we cannot *draw the line* of intelligibility.

3. Reply to McGrath

M&M concludes with *pluralism* about not only mathematics, but logic, modality, evaluative inquiry, and more. Pluralism combines realism with plentitude. There is enough *mind-and-language independent* reality to go around. So, truth factors out. The only non-semantic questions (i.e., questions that are not just about what we happen to mean by words, or what is 'packed into' our concepts) are *practical*. What concepts to use for a purpose at hand?

Notice that I did not say: 'what concepts *ought* we to use…'. According to the 'New Open Question Argument' (§6.4 - 6.6), evaluative facts – whether moral, prudential, epistemic, or 'all-things-considered' – do not settle practical questions. The argument is a radicalization of Hume's reasoning that one cannot derive an 'ought' from an 'is', and an extrapolation from Moore's dictum that one can know how things are in all descriptive respects while competently wondering whether it is good. One cannot even derive *what to do* from what we *ought* to do!

The New Open Question Argument (*NOQA*) may alternatively be understood as a more general and non-semantic version of Horgan's and Timmons' argument by the same name. They write:

> [Suppose] that...human uses of 'good'...are regulated by certain functional properties; and that, as a matter of empirical fact, these are consequentialist...whose functional essence is captured by some specific consequentialist normative theory; call this theory T_c….Now consider Moral Twin Earth [where] Moral Twin Earthlings have a vocabulary that works much like human moral vocabulary….The properties tracked by twin English moral terms are...non-consequentialist moral properties, whose functional essence is captured by some specific deontological theory, call this...T_d….*[The problem is that m]oral and twin-moral terms do not [seem to] differ in meaning or reference, and hence...any apparent moral disagreements that might arise between Earthlings and Twin Earthlings [are] genuine*...(1992, 460, italics added).

Horgan and Timmons argue that 'new wave' moral naturalism is false because it implies that two cultures whose use of moral terms is causally regulated by different properties do not disagree when they seem to (*cf*. Hare 1997). The *NOQA* of *M&M* expands on this reasoning in two ways. First, the *NOQA* does not just target naturalism. It applies to non-naturalist theories like Huemer (2005), Enoch (2012) and Scanlon (2014) equally. On Scanlon's view, for example, 'as long as some way of talking [is] well defined, internally coherent, *and [does] not have any presuppositions or implications that might conflict with those of other domains, such as science*', such talk is true (2014, 27, emphasis in original). This means that talk of

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the word 'intelligibility' (but carried out in the object language) are really practical – that is, intellectual policy debates.

consequentialist-goodness and deontological-goodness may both be true, even if both properties are non-naturalistic.[18] Second, the point of the *NOQA* is deliberative, not semantic (or metasemantic). *Even if earthlings and twin earthlings happen to pick out the same properties* with 'ought' (or 'good'), the Twin Earth thought experiment shows that there is a gap between what we ought to do and what *to* do, in any sense of 'ought' you like (and similarly for what we have most reason to do, what is good to do, and so on).[19] Even if we ought not kill the one to save the five, there is another property like the original -- call it $\text{ought}_{\text{Twin}}$ – according to which we $\text{ought}_{\text{Twin}}$ not kill the one. Ought and $\text{ought}_{\text{Twin}}$ are 'rivals' in that they may both be ascribed to praise, blame and evaluative conduct.[20] Nevertheless, they diverge in extension. Recognizing this, the *practical* question remains open – whether to do what we ought, or $\text{ought}_{\text{Twin}}$, to do.

McGrath criticizes the New Open Question Argument on two grounds. First, the idea of facts that settle what to do is at least coherent, because there are uncontroversial examples of facts that do this. She writes, 'clearly, there are facts the knowledge of which would settle what to expect [and so do]: namely, facts about what *will* happen (XX, italics in original)!' As McGrath highlights (XX), one can read the *NOQA* as a variation or elaboration on Mackie's argument from 'metaphysical queerness' (1977, 38). Evaluative realists mix up psychology with metaphysics. They try to 'pack' a resolving attitude into the facts, and the result is nonsense. One can stipulate that evaluative facts 'settle' what to do. But, then, evaluative* facts settle* what to do! Whether to settle or settle*? Such stipulations succeed at most superficially.

However, even if the idea of facts that settle what to do makes sense, I do not see how McGrath's example shows this. One cannot derive what we *ought* expect from what *will* happen by (an epistemic application of) Hume's original is/ought argument. But if what will happen does not imply what we ought to expect, then why would it settle what to expect? Consider the logical case. McGrath writes, 'We are convinced of arguments' conclusions by being convinced of their premises; we don't settle on a conclusion by recognizing an epistemic ought fact (XX).' While true, this does not show that the premises settle whether to infer the conclusion – at least in reflective contexts (or what McGrath, following Williamson, calls 'explicitly evidence-based' ones). Perhaps we are dubious of *modus ponens*. In the logic, *LP* (Priest 1987), for instance, this rule is invalid. If Q is false and P is both true and false, then P and $(P \rightarrow Q)$ are both true (though the latter is also false), while Q is false. So, the inference fails to preserve truth (the 'designated value'). Nor would it help to add that P and $(P \rightarrow Q)$ *logically implies* Q – even if logical implication is understood as a 'thick' evaluative term as per Glymour (2015, 6). We can still ask: whether to infer logically or 'shlogically', where shlogic is *LP* (and, correlatively, whether to do what we ought or 'shought' to do, to promote what is good or shgood, etc.)? The right view, I think, is that there are *no* facts knowledge of which settles what to do, not that

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[18] Enoch (2012, 124-126) makes essentially the same point about Scanlon (2014), but speaks of reasons and counter-reasons. He does not appear to appreciate that his 'robust realism' is vulnerable to the objection to be outlined insofar as he assumes platonism about properties (a la Bealer (1982), Plantinga (1974), Jubien (1997) or Wolterstorff (1970)).

[19] 'Ought' may be an operator in natural language. Nothing turns on this. One can consider rival semantic values, whatever they are, for operators. I use 'ought' rather than 'good' (like Horgan and Timmons) because that is the language used in *M&M* in connection with the New Open Question Argument.

[20] It might be objected that praise, blame and so on are themselves evaluative. If so, then what strictly true is that $\text{ought}_{\text{Twin}}$ is like ought in that it may be used to $\text{praise}_{\text{Twin}}$, $\text{blame}_{\text{Twin}}$, and $\text{evaluate}_{\text{Twin}}$ conduct.. In practice, the problem can be circumvented by appealing to examples.

descriptive rather than evaluative ones settle this. Settling what to do is an *act*, an expression of agency.

McGrath's second argument is the *prima facie* plausible one that there might not be any evaluative-like properties.[21] '[T]he realist might deny that [evaluative] notions are starrable (XX).' For example, the property of being good (or what we ought to do, or what we have most reason to do) might be like the property of consciousness, according to some philosophers of mind. They argue that consciousness cannot have 'rivals'. It cannot be that I am conscious, but not conscious* for some consciousness-like property, consciousness* (Simon 2017).[22]

The idea that there is a precise moment in our evolutionary past, between the development of single celled organisms and complicated mammals, at which consciousness popped into existence is wild. But for our purposes, it does not matter. As McGrath notes (*n*. 3), the *NOQA does not assume that there actually are evaluative-like properties* (much less that they are instantiated). It does not even assume that such properties are possible. We may suppose that, necessarily, there are *only* evaluative properties. The question remains: whether to regulate our behavior by consulting them? According to many realists themselves, we ought *not* regulate our behavior by consulting evaluative properties. To settle whether to help a drowning child, we need to know that he is drowning, that we are able to help, that he will die if we do not, and so forth. We do not *also* need to know that it would be *good* to help. Suppose, counterpossibly, that it were not good. Still: help the child![23] The view that we need to check the distribution of the evaluative properties, in addition to the distribution of the non-evaluative ones on which they supervene, in order to settle what to do attributes to us 'one thought too many' (Williams 1981, 18).' It makes us 'moral fetishists' , to which realists themselves may object (Smith 1994, 71).

Whether the Williams-Smith view of moral deliberation is correct is not pertinent. What matters is that it is coherent. For if it is, then so is the question of whether to regulate our behavior by consulting evaluative properties (instead of only non-evaluative properties) – even if there are not, and could not be, evaluative-like ones.

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[21] I use 'evaluative' where McGrath uses 'normative'. The view that there are also evaluative-like properties is an apparent consequence of typical formulations of platonism about universals, as in Bealer (1982), Jubien (1997), Plantinga (1974), and Wolterstorff (1970), as well as the platonism of Enoch (2012), Huemer (2005) and Scanlon (2014). These views generally hold that properties are 'abundant as can be', consistent with the paradoxes. (How finely they are individuated is a point of contention.) But we will shortly discover that this is not relevant here.

[22] Alternatively, the evaluative realist might argue that 'unlike the concept 'is fashionable', the concept 'ought'...fixes its extension (Wedgewood 2007)....[So,] if people are using 'ought' with the conceptual role of deciding what to do, then it automatically gets a certain extension (XX).' Indeed, evaluative-like properties might be unintelligible, not just impossible (*M&M* 171-2). But this is consistent with the New Open Question Argument. One reason is that '[f]or typical descriptive areas, *F*, the idea of *F*-like properties makes sense. We can imagine set-like properties, grounding-like properties, possibility-like properties, essence-like properties, consequence-like properties, privilege-like properties, and so on….If there are such things as moral properties, then why can we not imagine them 'tweaked,' just as we imagine the property of being a set tweaked? [Because] in natural language, we do not use 'ought to be done' to express a property at all. We use it to answer what to do questions. *And pluralism about what to do does seem to be unintelligible.* But this truism is no thanks to special facts that we cannot even *assume* to be nonobjective. It is thanks to the banal fact that we can only do *one thing* (*M&M*, 164).'

[23] See Hayward (2019) for a similar sentiment.

McGrath allows for this model of deliberation, but does not seem to see that it undercuts her argument from the nonexistence of evaluative-like properties. She suggests that perhaps 'practical deliberation takes as input only the underlying reasons, and issues as output…what to do…(XX).'[24] She asks: 'what [could] normative theories…be for, if not settling deliberation (XX)?' Her answer is that they could *'explain* why the things to do are the things to do (XX, italics in original).' But I fear that this would leave evaluative inquiry as practical as psychology, sociology or economics.[25] As Smith puts it, 'moral theories must serve a practical role… (2020, 12).'[26] '[I]f a moral theory cannot…guide decisions, it must be rejected as inadequate (*Ibid*., 12).' I would say, relatedly, that if a metaethical theory cannot vindicate the 'to be done-ness' of the moral facts that it postulates, it must be rejected as inadequate. 'We do not determine what we ought to do…for the sake of accumulating evaluative theorems. We do so to issue in action. But, then, the fact that knowledge of…evaluative facts fails to settle practical questions…shows that [they fail] to do the primary thing [that they] should do—tell us what to do (*M&M*, XX)!'

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[24] McGrath mentions Korsgaard as an example of such a view. She writes, 'Korsgaard concludes that…settling the question of what to do will be a matter of grasping the facts that make the action right and being appropriately motivated by them (XX).' But I would like to distinguish deliberation from motivation. Perhaps I am motivated to *X* after considering the reasons to *X*. It does not follow that my deliberation as to whether to *X* is settled. When weak in will, I am motivated to *X* despite having not settled whether to *X*, or having settled on doing *~X*.

[25] Perhaps McGrath would be comfortable with this result. She writes, 'Compare: when linguistics write down the rules of grammar, they are not doing that in order to tell people how to talk….[T]he suggestion is that the explanatory aim of moral theory might help the factualis to reconciled the idea the practical deliberation seeks to settle what to do with the idea that for any fact, we can ask what to do about it (XX).'

[26] Note that this is Holly Smith, not Michael Smith mentioned previously. The latter may well reject this position.

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