



Promiscuous kinds and individual minds

Jennifer Corns^a(jennifer.corns@glasgow.ac.uk)

Abstract

Promiscuous realism is the thesis that there are many equally legitimate ways of classifying the world's entities. Advocates of promiscuous realism are typically taken to hold the further thesis, often undistinguished, that kind terms usefully deployed in scientific generalisations are no more *natural* than those deployed for any other purposes. Call this further thesis *promiscuous naturalism*. I here defend a version of promiscuous realism which denies promiscuous naturalism. To do so, I introduce the notion of a *promiscuous kind*: a kind that is maximally usefully referenced in predictive and explanatory generalisations, none of which are scientific generalisations. I first defend the claim that pain is a promiscuous kind before extending these considerations to everyday mental kinds more generally. I draw on further reflections from both everyday life and contemporary psychology to make credible the novel suggestion that our everyday theory of our minds is *for* the explanation and prediction of individuals. Combined with the complex idiosyncrasy of individual minds, this suggested aim of everyday theory gives us reason to think that promiscuity is prevalent among everyday mental kinds.

Keywords

Eliminativism · Natural kinds · Naturalism · Pain · Psychological explanation · Realism

1 Introduction

Upon reaching early childhood, we cannot but classify our encountered entities. On my desk, I see a pen, a computer, a phone, a water bottle. Just outside my window, I hear a honking car, a cursing teenager, a melodious bird, and the rustling leaves. Moreover, these entities heard and seen are heard and seen *as* tokens of particular types of things: cups, computers, cars, and children. And, of course, I might well see, hear, or think about these entities as tokens of yet other types: I see two machines and three tools; I consider the two animals and three living creatures; and so on. As an educated adult, I have many classificatory schemas at my disposal. Different schemas will be more or less salient at different times and, crucially, for different purposes.

As I will roughly characterise it here, promiscuous realism is the thesis that there are many equally legitimate ways of classifying the world's entities. I will

^a University of Glasgow.

maintain that legitimacy here comes from utility: classificatory schemes are legitimate when they are useful. More specifically, the ontological legitimacy of a posit is earned through its explanatory and predictive utility. Utility both comes in degrees and is relative to certain purposes. Sometimes, I do best by referring to birds and teenagers separately and as such. Sometimes, I do best to classify them together as animals. The realism here on offer is promiscuous, as it holds that both ways of classifying, and many more, are ontologically legitimate. Notice that accepting promiscuous realism thus involves rejecting any putative requirement that kinds must be *reducible* to some supposedly privileged domain or ontological 'level', i.e. kinds need not reduce to those of some other supposedly privileged domain, e.g. physics, to be real. In particular, as will be especially relevant below, notice that mental kinds may be as ontologically robust as any others in virtue of their explanatory and predictive utility, independent of their relation to any non-mental kinds.

The term 'promiscuous realism' was introduced in John Dupré's classic (1981). He there writes (p. 82):

The position I would like to advocate might be described as promiscuous realism. The realism derives from the fact that there are many sameness relations that serve to distinguish classes of organisms in ways that are relevant to various concerns; the promiscuity derives from the fact that none of these relations is privileged.

We should, however, introductorily consider: *privileged* in what sense? There are two relevantly distinct ways in which a classificatory schema may be privileged. The first, already seen, is ontological; that is, a schema may be privileged relative to how *real* the kind and the members thereby delineated may be. Dupré surely intended to deny the ontological privilege of a benighted, authoritative classificatory schema over all others. To use his own example: trees are just as real as angiosperms. A second way in which we might privilege a classificatory schema, however, concerns its naturalness; that is, a schema may be privileged relative to how *natural* the kind and the members thereby delineated may be. To continue with the same example: we may think that trees are just as natural as angiosperms. These two ways of privileging a classificatory schema are typically, regrettably, conflated.

To see the typical conflation of ontological and natural privilege, consider the entry on natural kinds in the Internet Encyclopedia of Philosophy (IEP). Zdenka Brzović (2018) there writes: "One of the hallmarks of promiscuous realism is that it does not prioritize scientific classification over folk categories [...]" While we may initially have interpreted the priority here as mere ontological priority,¹ the IEP

¹ I set aside explanatory priority, but it is worth nothing that a promiscuous realist need not be committed to the claim that no classificatory schema is explanatorily prior to any other. This seems to me good news for the promiscuous realist.

goes on to say: “All these classifications can [appropriately, according to promiscuous realism] be considered natural [...]” The promiscuous realist need not accept this. Moreover, I mean here to defend the idea that they *shouldn't* accept this further, often undistinguished thesis. Some equally ontologically legitimate categories are nonetheless more natural than others.

Call the thesis that kind terms usefully deployed in scientific generalisations are no more *natural* than those deployed for any other purposes *promiscuous naturalism*. Once the real and the natural are distinguished, the promiscuous realist has no reason to accept promiscuous naturalism: as promiscuous realists themselves have sometimes stressed, there are many ontologically robust classificatory schemas beyond the boundaries of science. Of course, we must say something about when a kind is *natural*.

I will here employ what I've elsewhere dubbed *the scientific utility criterion* for natural kinds (Corns, 2020). According to this liberal criterion, natural kinds are those kinds usefully referenced in scientific generalisations for explanation and prediction. For all purposes here, we can stay neutral on further questions concerning *why* some kinds will satisfy this liberal criterion and further features of science's generalisations. In particular, we can be neutral concerning whether: homeostatic mechanisms are required; microstructural or any other essence are required; or the relevant generalisations are laws. And, again, we need not require that a kind be *reducible* to some benighted “level” of ontological reality to be natural, any more than we required reduction for ontological reality. Nor do we require that natural kinds are physical kinds, as against mental kinds. The central issue remains explanatory and predictive utility. What should be noticed is that even using this liberal notion of natural kinds, establishing ontological legitimacy does *not* establish naturalness. After answering the question of whether a kind is usefully referenced for explanation and prediction in a non-scientific domain such as everyday life, the question of its utility for any scientific inquiry remains.²

Kind terms should be retained for all and only those purposes for which they are maximally useful. A kind term should be eliminated from an inquiry whenever reference to alternative kinds would provide more explanatory or predictive power for that inquiry. Discovered utility thus rightly guides the deployment of the kind terms in any classificatory schema, as we aim for the best available explanations and predictions. *Scientific eliminativism* is the position that a kind term should be eliminated from scientific generalisations. This notion was introduced by Eduard Machery (2009) as a contrast to *traditional eliminativism*, the well-known position that a term should be eliminated *tout court*. Traditional eliminativism is directly supported by ontological considerations; elimination is justified on the grounds that the referring expression fails to refer. Either kind of eliminativism, however, may be justified by considerations of utility; a kind term should be eliminated

² The idea that everyday life just *is* a kind of scientific inquiry is addressed below. For now, note that the key point here remains: maximally useful reference for everyday life does not entail maximally useful reference for any (other, if one likes) scientific inquiry.

from any discourse in which it is not useful. Traditional eliminativism can then be understood as the extreme case in which the referring expression is never useful. Remembering that utility comes in degrees: a kind term should be eliminated from any discourse in which it is not maximally useful when we discover that it is not maximally useful. We stand by our best theories at, but only at, any given time.

We are now prepared to introduce the key notion of a promiscuous kind. A promiscuous kind is a kind that is maximally usefully referenced in predictive and explanatory generalisations, none of which are scientific generalisations. Promiscuous kinds are not natural using even our liberal notion of natural kinds; they are not (maximally) usefully referenced for scientific inquiry. The ontological legitimacy of these kinds, as with any others, may nonetheless be earned through their explanatory and predictive utility for other purposes. This is so even when the remaining utility is limited only to everyday purposes, i.e. the explanations and predictions deployed in everyday life. Though not natural, on even a liberal notion, such kinds are then nonetheless real. Our realism, remember, is promiscuous. Accordingly, we should be scientific eliminativists, but not traditional eliminativists, about any promiscuous kinds.

As outlined above, accepting the ontological legitimacy of promiscuous kinds is to accept a novel and unnoticed version of promiscuous realism which denies promiscuous naturalism. This version of promiscuous realism rightly denies the *ontological* privilege of scientific classification but respects its privileged claim to discovering which kinds are *natural*. But are there any promiscuous kinds? While we may have considered many domains of life, to address this question here, I focus on mental kinds. I here defend that at least some mental kinds, as posited in everyday theory, are promiscuous kinds and that we should therefore be promiscuous realists, but not promiscuous naturalists. Moreover, I offer some reflections on the nature of individual minds and our everyday theory of our minds to support the suggestion that promiscuity is likely prevalent among our everyday mental kinds. As a preview, note that the prevalence of this promiscuity does *not* concern reduction or any supposed ontological divide between the mental and the physical. Rather, the central issue remains explanatory and predictive utility.

I proceed as follows. In the following section, I begin with pain. I have elsewhere argued that pain is real, but not natural, and therefore defended *scientific eliminativism* for pain. (Corns, 2016, 2020, 2023) I now extend and capture this view as the claim that pain is a promiscuous kind. I review the key claims in favour of pain promiscuity and respond to recent objections in section 2. This discussion and defense is used to inform the more general consideration of the prevalence of promiscuity among everyday mental kinds offered in section 3. Having extended the considerations from pain to further everyday kinds, I then draw on further reflections from both everyday life and contemporary psychology to make credible the novel suggestion that our everyday theory of our minds has been developed for the explanation and prediction of the behaviour of the *individuals* with whom we interact for everyday purposes. If the aim of our everyday theory of our minds is the explanation and prediction of individuals, then two key claims are supported.

First, it is an empirical question whether the posits of everyday theory will be maximally useful for any scientific inquiry. Second, if the aim of our everyday theory is individual explanations and predictions, then the complex idiosyncrasy of individual minds gives us reason to think that many of our everyday mental posits are promiscuous. I conclude in section 4 by briefly highlighting some implications.

2 Pain: a promiscuous kind

In this section, we focus on pain. If pain is a promiscuous kind, then there is at least one such kind, and promiscuous realism without promiscuous naturalism is thinly vindicated. As I have argued for the non-natural reality of pain at length elsewhere (Corns, 2020), I here merely offer some key claims before providing a more extended discussion of their defense from four key objections. The discussion is developed to inform general considerations of everyday kinds and promiscuous realism without promiscuous naturalism. Details particular to pain are addressed only when helpful for these larger objectives.

Like most of our mental states, processes, and episodes as we understand them, pain is a posit of our everyday theory. It's not a posit from any specialised inquiry, but is instead a part of how we have come to understand ourselves and our behaviour in our everyday lives. Because we so usefully reference pain in explaining and predicting behaviour, we should accept that pains are real. Recall that utility comes in degrees, such that a posit will be more or less useful for different purposes. While it may eventually turn out that we can do better than 'pain' for all the explanatory and predictive purposes to which we currently put it in everyday life, at the moment, I think we *can't* do any better; references to pain are maximally useful for everyday purposes. So, we should accept that pains are real and reject traditional eliminativism for pain—references to pain should be retained as they are sometimes successful.

We may then ask the further question whether pains are natural. This question is an empirical one and it is scientific inquiries themselves which can settle the matter. Using our liberal, pragmatic notion of natural kinds, to ask if pain is natural is to ask if we can offer any scientific generalisations about pain that are maximally useful for explanation and prediction. The same questions may be raised for pains' sub-types, e.g. headaches, stomachaches, myofascial pain, labour pains, and so on. Recall again that utility comes in degrees. It should not be denied that there are many scientific generalisations about pain and pain subtypes which are more or less explanatorily or predictively useful. Nonetheless it seems to me to have turned out that we can do better for all the explanatory and predictive purposes for which we deploy references to pain in scientific inquiry. So, we should accept that pains are not natural kinds. We should, then, accept scientific eliminativism for pain—references to pain should be eliminated from scientific inquiry for failing to be maximally useful.

Now is not the time to review the arguments for these claims in detail, but the key claim should be highlighted.³ First, pain is idiosyncratic and mechanistically complex. By this, I mean that each token pain is determined by a complex and highly idiosyncratic convergence of the activity of multiple mechanisms—including, at least, genetic, chemical, biological, psychological, and social—none of which is most usefully understood as being a *pain* mechanism, i.e. a mechanism *for* pain. Notice that many of these mechanisms, and many other processes, pathways, and neural channels identified in scientific inquiries into pain, may turn out to be maximally usefully referenced in scientific generalisations. I shall return to this point later. Second, the complex idiosyncrasy determined by the discovered mechanistic convergence is what turns out to undermine the scientific utility of pain as such and, further, of pain subtypes. This, notice, does not entail anything about intertheoretic reduction or about the ontological status of pain on the current view. Whereas traditional eliminativism entails that pains are not realised by anything at all (since it denies that there are any pains), scientific eliminativism for pain is consistent with each pain being real and being realised by the activity of natural kinds without being reducible to any of them. Pain remains maximally usefully referenced for everyday purposes and is thereby rightly accepted as real. Pain thus turns out to be promiscuous as a result of this complex idiosyncrasy. Exactly why complex idiosyncrasy undermines references to pain for scientific inquiries but not for everyday practice is an important question to which we will turn in the following section.

Many objections have and may be offered to the promiscuity of pain. We now focus on those four which I take both to be most cogent and most relevant to evaluating the promiscuity of mental kinds more generally and the plausibility of promiscuous realism without promiscuous naturalism.

Objection 1: Pain isn't (relevantly) complex and idiosyncratic.

While many versions of this objection may be offered, the most relevant and cogent deny that pain is relevantly complex.⁴ Instead, it is claimed that pain is a distinct phenomenal kind or has a distinctive content such that, the objection continues, pain is therefore natural, i.e. appropriately referenced in scientific generalisations.⁵

I have argued elsewhere that pain is *not* a distinct phenomenal kind and does *not* have a distinctive content (Corns, 2014, 2020), but what I wish to stress here is that the inference captured in the objection is poor. Even *if* pain is a distinct phenomenal kind or has a distinct content, that *by itself* is insufficient for concluding that the kind is natural. Indeed, *any* mental kinds may be appropriate targets for scientific inquiry—a distinctive quality or content is not even required for this and is certainly not sufficient for showing that the kind is natural. Pain has been

³ See again Corns (2020) for detailed arguments for these key claims.

⁴ I note that the *mechanistic* complexity of pain is nigh universally recognised. Nonetheless, as seen in text, simplicity of quality or content continues to be the dominant philosophical view.

⁵ See for example Coninx (2021) or Klein (2021), who both pursue this line of objection.

appropriately targeted for scientific inquiry, but we must then follow where those inquiries lead. For pain, I have argued that they have led to the identification of complex idiosyncrasy and massive mechanistic heterogeneity which undermines its scientific utility. Those arguments may be faulty. But granting distinctive phenomenology or content would not yet be to identify a fault. The question, on even a liberal definition of natural kinds, is whether the kind is maximally useful when referenced in any scientific generalisations. It may never be useful for any science to generalise about e.g. believing or desiring *that p*, for a great many *ps*. It may never be useful for any science to generalise about the particular way it is like for one e.g. to stretch upon rising from sleep, be confused, be lost, or eat an orange—even if there *are* particular ways these are like for one. The moral is that neither phenomenal unity nor distinct intentional content establishes the *naturalness* of a posited mental kind. This moral holds not only for pain, but for any other everyday mental kind we may investigate.

One might, of course, similarly deny that pain is relevantly idiosyncratic. Matthew Fulkerson (2023), for instance, directly states (p. 546):

Idiosyncrasy per se isn't an issue as long as there are enough actual shared properties among the collected instances to support generalisation. [...] And it seems clear that, despite token variations in some of their features, specific pain types do tend to share many interesting features.

This objection may be interpreted in two ways. On the one hand, the objector may have some identified shared properties in mind which they take to support generalisations. Any potentially unifying candidates must be individually addressed. For pain, I continue to maintain that no unifying candidates are successful and address the leading philosophical contenders above. On the other hand, however, the objector may be simply stressing that there *must* be enough similarities to support maximally useful generalisations since there patently are such generalisations. This, I take it, is Fulkerson's central objection, to be now directly addressed.

Objection 2: Scientific generalisations referencing pain are useful.

It may seem obvious that if we take the pragmatic criterion for natural kinds offered above seriously, then pain and its subtypes are surely natural kinds, since there are so many patently useful scientific generalisations which reference them.⁶ Moreover, the objection may continue, if pain is usefully referenced for scientific explanation and prediction, then it is not promiscuous, and we should eschew scientific eliminativism for pain despite any identified idiosyncrasy, complexity, mechanistic heterogeneity, or anything else we may be granted to have discovered.⁷

⁶ See e.g. Baetu (2020b), Coninx (2021, 2022), Fulkerson (2023), and Klein (2021) who all pursue this line of objection.

⁷ This would also render pain natural according to my liberal scientific utility criterion, but a more restrictive notion may require more e.g. Boyd's popular HPC account of natural kinds (e.g. Boyd,

The general reply is that scientific generalisations referencing pain have been useful despite these references, but referencing pain is not maximally useful. Instead, explanatory and predictive utility for any particular science improves when references to pain are replaced with references to other, more natural, kinds. It should be stressed that the question here is not whether generalisations about pain have *exceptions*—at least most scientific generalisations are *ceteris paribus*. The relevant question is whether references to pain are more or less useful than references to other kinds for the relevant explanatory and predictive scientific purpose. This question is plainly a comparative one. Making good on this general reply is of course then as difficult as making good on this objection: generalisations which supposedly reference pain in a maximally useful way are offered, diffused, and followed swiftly by further candidate generalisations.

In the service of legitimating a more general picture whereby many mental kinds turn out to be promiscuous, it is nonetheless worth considering a few particular generalisations concerning pain which have been offered as objections to its promiscuity. Responding to these counterexamples can illuminate how references to an everyday kind may come to be gradually replaced with references to other more natural kinds uncovered through scientific inquiry into the kind. This is important since we might have been tempted to think that scientific elimination requires reduction to a single posit in a single scientific theory. This needn't be so. (Cf Baetu, 2020a). Eliminative replacement may be piecemeal and driven by local utility, as against global theory change.

Consider first Sabrina Coninx's (2021) family resemblance approach to pain. The approach is explicitly intended to vitiate the utility of references to pain for scientific inquiry, while granting the complex idiosyncrasy and mechanistic heterogeneity that might otherwise support the claim that pain is promiscuous.⁸ Deploying this approach, Coninx laudably offers some specific candidate scientific generalisations. For one instructive example among many, she suggests that we may be able to get useful generalisations that allow us to predict chronic pain based on identifiable neural patterns. She rightly admits that we don't yet in fact *have*

1999) would require a homeostatic mechanism which best explains the success of the relevant generalisations and best predicts yet others.

⁸ Taking pain to be phenomenally unified, Coninx (2021) defends a multidimensional phenomenal space for pain which she takes to aptly characterise the structure of the "family" of pain. As argued in text, even if the phenomenal unity of pain is granted, this does not establish that pain is natural. Beyond this, it is worth noting that it is inappropriate to take the delineated space to be a pain phenomenal space for two key reasons. First, the dimensions of the offered space are neither specific for pain nor such that all pains take a value for all of them. Both of these requirements need to be met and it seems to me that they cannot be met. Second, and equally problematic, the identified dimensions apparently aren't all qualitative. Notice that even if the delineated space is not a phenomenal space for pain, a key substantive claim remains: that a supposed pain family has been delineated and its members, or subsets of its members, are similar in ways that support scientific generalisations. That is the central question being considered in text.

any such generalisations, but she thinks identifiable neural patterns may soon facilitate these predictions. Crucially, for our purposes, she writes (p. 13):

One possible limitation is that such predictions are not perfectly selective with respect to pain. People at risk of developing chronic pain might also be at risk of developing chronic itch, for example. This is due to the fact that the central processes associated with different homeostatic bodily sensations might not have sharp boundaries [...].

Well, quite. There is, in fact, no reason here offered for even the *hope* that we will soon arrive at generalisations which are maximally useful and predictive when focused on *pain*, as against the other kind squarely in view, i.e. homeostatic sensations. About the lack of specific predictions concerning pain, she nonetheless goes on to write (p. 13):

This [lack of specificity] is not necessarily a negative thing. Neuro-markers for the chronification of pain, itching, dyspnea, and the like could motivate the selection of measures with quite broad effectivity which might prevent the need for further measures.

In the current dialectic, this won't do. Even if we accepted her claim and joined her in this hope, there is no example here of useful scientific generalisations about *pain*; more explanatory and predictive power—and more useful treatment interventions—are likely found by referencing homeostatic sensations. We are here considering whether referencing pain in some candidate scientific generalisation is maximally useful. In this case, even by Coninx's lights, it isn't.

Rather than replacing references to pain with references to some more general class, other candidate scientific generalisations are better improved by adopting alternative reference to some typical component of pain. Consider, for example, one of Fulkerson's (2023) examples. He claims that if we ignore differences between pains and focus on their similarities, then this "can yield useful generalisations, like the fact that listening to music or being exposed to pleasant smells can help alleviate pain [...]" (p. 543). In this case, however, the more explanatorily and predictively powerful kind in the neighbourhood seems clearly to be *negative affect*. Negative affect is one typical component of pain, so we might expect that anything which reduced unpleasant affect would "help alleviate pain." But listening to music and being exposed to pleasant smells apparently decreases negative affect *quite generally*.⁹ Thus, if we reference negative affect we can then *better* explain the results found in those studies mentioned by Fulkerson and offer broader and

⁹ Concerning music and affective processing see e.g. Geethanjali et al (2018), concerning smell and affect see e.g. Thomas (2002). The reader is also here simply invited to reflect on their own practices of affect regulation.

more accurate predictions in further cases.¹⁰ If this is right, the generalisations are succeeding *despite* references to pain, and increase when we focus instead on another kind revealed in the investigation—negative affect. Of course, we may still colloquially take listening to music to be useful for alleviating pain, though notice that it is much more common to say things like “listening to music makes me feel better.” But we can anyway explain the supposed pain generalisations best—even in everyday life, in this case—by saying that listening to music reduces negative affect. Replacing references to pain with references to negative affect here increases explanatory and predictive power.

For the legion scientific generalisations referencing pain which I have encountered and considered, there is some other candidate kind in the neighbourhood which better serves for explaining and predicting. When Baetu (2020b) extols the utility of activity in the neuromatrix for explaining and predicting *pain*, it seems to me that *threat detection* would do far better. There is, here, no neurosignature for pain and no mechanism for it either. When Coninx (2021) extols the utility of identifying ‘pain signatures’ as in, e.g. Reddan and Wager (2018) for predicting pain, it seems to me that we would do better to continue to focus on those components, and their related regions, which go into the offered models, e.g. attention, affect, and nociception. And so on. My claim is that it *turns out* that references to pain are not as useful as references to these other kinds. The complex idiosyncrasy and mechanistic heterogeneity that we have discovered gives us reason to think this pattern will continue. Further reasons concerning the *purpose* of everyday theory are offered in the following section.

Before leaving this objection, however, we must separately consider the utility of generalisations for diagnosis. I think it is no surprise that most supposed counterexamples to pain promiscuity have been taken from diagnostic practice, as everyday theory here makes close and direct contact with scientific generalisations. As long as patient reports remain a central feature of the diagnostic context, then reference to everyday theory and its posits will and should remain—this includes not only pain, but any other reported everyday kinds which turn out to be promiscuous. The utility of references to pain in diagnostics, however, does not come from any scientifically useful generalisations about pain. Rather, it comes from the necessity of accepting and interpreting pain reports in the diagnostic context.

Consider two examples recently offered as objections. Fulkerson (2023) discusses myofascial pain as a potentially distinct subtype of pain, reference to which is supposedly useful enough to vitiate its naturalness. Fulkerson does not identify a scientific inquiry for which he thinks reference to this kind is maximally useful, but his focus on the role of pain reports in diagnosing athletes suffering from my-

¹⁰ Note that one of the studies offered by Fulkerson in support of the claim that listening to music alleviates pain was offered by Bradshaw et al (2015). This same lab (Bradshaw et al., 2012) offers a study focusing on individual differences in the effects of music engagement on responses to painful stimulation. Both studies yield more powerfully explanatory and predictive generalisations when they reference negative affect as against pain.

ofascial pain suggests that the utility is indeed *for diagnostics*. I am much inclined to think that the natural kinds in the neighbourhood may be muscle tension, negative affect, and the numerous specific mechanisms rightly identified and targeted for treatment in response to these pain reports. Of course, the pain reports are not eliminable from the diagnostic context, even though *myofascial pain*, as such, is—if I'm right—not the appropriate treatment target. Similar considerations apply to Colin Klein's (2021) offered example meant to establish the utility of references to pain in scientific generalisations. Klein offers generalisations focused on the comparative reliability of certain kinds of pain reports as against certain other kinds of tenderness reports in diagnosing appendicitis. Any utility here is clearly utility for diagnostic practice, and I again agree that we cannot and should not simply eliminate references to pain from diagnostic practice. As Klein (2021) notes, however, "I doubt there could be a distinct science of pain treatment—treatment is applied theory, not a science in its own right" (p. 994). Quite so. Treatment provides a crucible for *testing* scientific generalisations about pain by making interventions on specific mechanisms identified through the diagnostic process. We can thus test any claims that pain, or some pain subtype, as posited and reported in everyday theory, correspond to some particular mechanistic activity. Even if it does not, however, the utility of reference to pain for diagnostics will remain. The utility of reference to pain for diagnostics thus doesn't show whether a kind is promiscuous—the utility may all be on the everyday, reporting side and not e.g. on the mechanistic side.¹¹ The patient report is given in everyday theory and may thereby reference pain. The scientific generalisations that allow us to identify appropriate treatment targets need not and—if I'm right—should not reference pain. The diagnostician, on this picture, acts as something of a translator from everyday theory into scientific theory (See again Corns, 2020). Diagnostics, as an applied science, stands at the interface of everyday theory and scientific theory. Candidate generalisations from diagnostic practice are thus not direct counterexamples to the promiscuity of pain.

Having begun to see how scientific inquiries may progress by replacing references to a promiscuous kind with references to kinds which are more natural, we may begin to think that we should then eliminate references to the promiscuous kind altogether. If we don't need references to pain for science, then perhaps we do not need them at all—not for everyday theory and therefore not in fact for diagnostics either. At least, not ultimately. This takes us to the third key objection.

Objection 3: Elimination is all or nothing.

In an important challenge, Colin Klein (2021, p. 995) offers a dilemma which, if fatal, would be fatal to scientific eliminativism without traditional eliminativism full stop, and thus to the promiscuity of any kind. He writes:

¹¹ Note that scientists themselves, as people armed with their everyday theory, may well often use their everyday theory when engaged in the *practice* of science in useful ways that it would be absurd to suggest eliminating. So, for example, during a day of scientific investigation they may refer to *tables*, *absurd ideas*, *lunch time*, *grants*, *Monday*, and all sorts of things which may not turn out to be maximally usefully referenced in the generalisations delivered by their specialised scientific inquiry. I am grateful to an anonymous reviewer for pressing this point.

[...] it seems to me there is a real dilemma for the sort of [scientific eliminativist] picture that Corns presents. On the one hand, one might insist that the world is (at least) bifurcated into folk and scientific realms, each with their own theories and corresponding variety of ontological commitment. The problem is that, absent special pleading, it is hard to see why the folk theory of pain would underpin ontological commitment [...]. On the other hand, one might have a broadly Quinean view, on which science ‘differs from common sense only in the degree of methodological sophistication’. [...] But then pain science is the thing that’s developed precisely to discuss and capture the everyday notion: the two stand or fall together [...].

The dilemma, in sum, is that everyday and scientific theories either offer distinct classificatory schemas or, ultimately, only one. If the former, horn A, then “absent special pleading” there is no reason to take everyday theory as ontologically committing. If the latter, horn B, then scientific eliminativism is sufficient for traditional eliminativism; “the two stand or fall together.” As Klein (2021, p. 995) concludes “Either way, however, the attractive halfway version of eliminativism is a mirage [...].”

We should unblushingly accept horn A and accept promiscuous realism: there are many distinct, ontologically legitimate ways of classifying the world’s entities. The relevant “special pleading” is utility for explanation and prediction. We may have this in everyday life, even though we lack it for any scientific inquiry. This situation is not unusual; amongst the sciences themselves classificatory schemas may be maximally useful for some inquiries and not others. Even if water is maximally usefully referenced in the generalisations of biology, it is an open question whether it is maximally useful to refer to water in the generalisations of chemistry. But we do not think that biology here requires any “special pleading” to retain its references to water beyond their utility for explanation and prediction *in biology*. For pain, ontological legitimacy is secured through the utility of reference for explanation and prediction in everyday life.

Despite routinely taking it for granted, we may now begin to doubt the alleged everyday utility. Klein suggests that I let “our ordinary talk off the hook all too easily” (2021, p. 994). Are references to pain maximally useful in everyday life? It should be admitted that we can *sometimes* replace references to pain with references to other kinds that would be more explanatorily and predictively useful, just as we should for scientific inquiries. It nonetheless seems to me that there are a great many for which pain *is* maximally useful.

Upon encountering a stranger (a dog, a cat, a human) wailing and writhing on the ground, pain may be the maximally useful kind to posit to begin engaging with them and to begin explaining and predicting their behaviour. Attributing pain to them may be maximally useful for shaping the manner with which to approach them, what sort of questions to begin to ask, what sort of help to begin to seek, and so on. To attribute an unpleasant experience more generally, for example,

would be to attribute something broader and less useful, if they are exhibiting behaviour suggesting pain, more specifically. We now know that a whole range of biological, neural, genetic, chemical, or social mechanisms may be converging to determine their particular pain. These may be more useful for a range of scientific inquiries, but I submit that none of these are more useful for my initial everyday interactions with this stranger; I have little idea which of these mechanisms may be involved, they are not observable, they are not reportable, and they are not my primary interest. In preview of the following section, notice that this remains so even though references to pain may become *even more* usefully posited to explain and predict the behaviour of individual agents as I come to know them better.

We return to the tailoring of promiscuous mental kinds to individual minds in the following section, but for now I note again that if we accept promiscuous realism, then the lack of the utility of reference for any *scientific inquiry* gives us no good reason to begin to doubt the utility of reference for everyday life. Most of us (including Klein) are firmly committed to the ontological reality of pain, and our commitment to this is justified through the maximally useful references to it which we make in everyday life. This remains so, even if references to pain fail to be maximally useful for any other purpose.

The nature of this “special pleading” on behalf of everyday classification, however, leads to our final objection. Harkening again to Quine, one might think that the sorts of everyday generalisations to which I have appealed just *are* scientific generalisations. Insofar as everyday theory is a good theory which justifies ontological classificatory schemas in virtue of its explanatory and predictive power, it just *is* a science. Thus, our final objection.

Objection 4: Everyday generalisations that explain and predict are scientific generalisations.

In explicitly rejecting scientific eliminativism for pain, Matthew Fulkerson (2023) offers a liberal demarcation of science. According to Fulkerson, “practice counts as a science if it helps us gain knowledge about reality.” (p. 543) If Fulkerson is right, then one should accept that any promiscuous kinds are natural kinds, even if they only remain usefully referenced in everyday explanations and predictions, since these tell us about reality. Fulkerson accepts promiscuous realism, and his liberal demarcation criterion thus offers a reason for additionally accepting the promiscuous naturalism with which it is often conflated. Any supposed arguments for scientific eliminativism without traditional eliminativism are here taken to merely, instead, identify the science for which references to the kind are useful. Fulkerson advocates for pain pluralism on the grounds that pain is usefully referenced for many different explanatory and predictive purposes across many cross-cutting kinds. Some of these are scientific generalisations within which references to pain are supposedly maximally useful, which I reject as above. Others, however, are everyday generalisations. Using a liberal demarcation criterion, these everyday generalisations are enough to establish the naturalness of pain.

I am sceptical of the utility of offering necessary and sufficient conditions for when a practice is a science. Fulkerson's own characterisation seems to me far too liberal; most any practice can help us gain knowledge about reality, suitably engaged—from day-dreaming, to ten-pin bowling, to interior design. This could be addressed while maintaining the liberal spirit of the criteria. Perhaps, for example, some practices are especially well-suited to telling us about reality in virtue of some feature that could be highlighted as criterial. We might think my own ontological criterion can do the work: perhaps every explanatory and predictive practice is a science. After all, I take everyday theory to be ontologically committing for just the same reason as scientific theories. This is a problematic solution to the demarcation problem however; science is not merely the pursuit of ontology. Again, many non-scientific activities allow us to better identify and understand what exists and science sometimes has other aims, e.g., practical aims or claims about relations. My own view is that we do better to consider the many particular inquiries in which we are interested, some of which are paradigmatically scientific and some which are not. Taking this approach: everyday life, it seems to me, is paradigmatically, *not* a science. This seems to me to be so, even if everyday theory and scientific theory are both ontologically committing.

I am nonetheless here prepared to grant *all* the requested linguistic ground. The heart of the objection is that everyday theory, as I have characterised it, is a science. *As long as we keep clear what we mean*, I am prepared to grant this. The substantive claims can all be recast with this liberal demarcation. The core claim is that pain is maximally usefully referenced only for everyday explanation and prediction. Fulkerson argues that it isn't. This substantive issue remains however we respond to the demarcation problem. I set this linguistic variant aside in all that follows, speaking in my preferred way, but if one insisted, the substantive issues could be recast.

Before leaving this objection, I note that I am likewise prepared to grant all the desired linguistic ground to a similar sort of objection raised in William Ramsey's (2021). According to Ramsey, no view which accepts the existence of something should be classed as an eliminativism—of any kind—about that something. Ramsey's key claim is that a kind-term being appropriately eliminated from scientific inquiry should not be taken to entail anything about the ontological status of the kind. "After all..." he writes, "...there are plenty of real and wonderful things in the world that should nevertheless not be a part of a serious scientific taxonomy" (p. 11711). From this, he concludes that the term 'eliminativism' is inappropriate for mere so-called *scientific* eliminativism. He alternatively advocates 'kind-dissolution' as a preferred label for the view, further suggesting potential others: 'type-decomposition', 'category deconstruction', 'conceptual fragmentation'.

I am not convinced that the label 'scientific eliminativism' need be misleading, but I fully agree with the claim motivating Ramsey. The label 'scientific eliminativism' is intended to flag the discourses from which a term is to be eliminated, not the ontological status of anything. For those troubled by the sorts of worries which exercise Ramsey, however, we can say that the kind is promiscuous. More-

over, in preference to Ramsey's other suggested alternatives, this label does not suggest the ontological authority or priority of scientific discourse. A kind may be promiscuous without being in *need* of dissolution, decomposition, or deconstruction, and it need not be fragmented—at least, relative to its central non-scientific purpose. Insofar as Ramsey is right that the label 'scientific eliminativism' engenders ontological suspicion, all scientific eliminativists should be happy to endorse an alternative.

3 Everyday mental kinds and individual minds

It is time to take stock. Recall that a promiscuous kind is one that is maximally usefully referenced in some predictive and explanatory generalisations, none of which are scientific. If there are any promiscuous kinds, then we should be promiscuous realists, but not promiscuous naturalists. In the previous section, I defended the promiscuity of pain. If successful, this thinly establishes promiscuous realism without promiscuous naturalism. Drawing on this discussion, I turn to our everyday theory of our minds more generally and aim to support two key claims. First, whether any everyday mental kind is promiscuous is an empirical question. Second, and stronger, there are good reasons to think that a great many everyday mental kinds may indeed turn out to be promiscuous, i.e., that the promiscuity of everyday mental kinds may be prevalent. Both claims receive support through extension of the considerations seen above for pain and, further, from reflections on our everyday practices and developments in contemporary psychology.

Consider first the weak claim that it is an empirical question whether any everyday mental kind is promiscuous. Everyday mental kinds are posited as part of our everyday theory for everyday purposes. As we've seen, there is no guarantee that any of these posits will be useful for any further inquiry or purpose, including any particular scientific inquiry or purpose. As with pain, a particular posit of our everyday theory, so with the rest of our everyday posits. Some of these may be usefully referenced in scientific generalisations. Some, like pain, may not. As with pain: the question of whether an everyday mental kind is promiscuous is appropriately settled by what is discovered to be maximally useful for each considered purpose.

Our discussion of pain likewise directly applies to initial consideration of the second stronger claim that there are good reasons to think many mental kinds will turn out, upon inquiring, to be promiscuous. Kinds may be promiscuous for many reasons, but we highlighted the complex idiosyncrasy of pain as explanatory of its promiscuity. Each token pain is determined by a complex and highly idiosyncratic convergence of the activity of multiple mechanisms—including, at least, genetic, chemical, biological, psychological, and social—none of which is most usefully understood as being a *pain* mechanism i.e. a mechanism *for* pain. Many mental kinds, as posited in everyday life, seem likely to turn out to be promiscuous for this same

reason. That is, it seems quite plausible, given what we already know, that the tokens of many mental kinds—from emotions, to desires, to daydreams—are determined by a complex and highly idiosyncratic convergence of the activity of multiple mechanisms. Insofar as there is good reason to think that many everyday mental kinds are as complex and idiosyncratic as pain, there is good reason to suspect that many everyday mental kinds are promiscuous.

Moreover, notice that all four objections against the promiscuity of pain considered above may likewise be more generally offered. For any candidate promiscuous kind, one might object that it isn't relevantly complex or idiosyncratic (Objection 1) or that it is usefully referenced in some scientific generalisations (Objection 2). Addressing these objections requires looking at the relevant empirical inquiries for the candidate kind. That is to say: promiscuity is, indeed, an empirical question to be addressed for each particular kind. One might more generally object that elimination is all or nothing (Objection 3), but I hope to have shown that this is false; many mental kinds may be maximally usefully referenced for explanations and predictions in everyday life, but not for any scientific inquiry—even as they may be maximally referenced in some sciences and not others. One might finally object that any such explanatory or predictive generalisations from everyday life just *are* scientific generalisations (Objection 4). Though I am sceptical that this should be granted, all substantive claims remain if it is. These objections diffused, I see no general reason supporting the presumption that mental kinds must turn out to be natural. Once these are diffused, it seems to me to be at least an open empirical question whether any particular mental kind as posited in everyday life is promiscuous. Or, to put it another way, it is at least an open empirical question whether any particular mental kind as posited in everyday life will be usefully referenced in any (further, if one insists) science. The seeming complex idiosyncrasy of many everyday mental kinds gives us initial reasons to suspect that promiscuity will be prevalent.

Notice again that while individual differences undermine the utility of reference for scientific inquiries, this does not undermine the utility of reference for everyday purposes. As with pain, our everyday theory of our minds may continue to make maximally useful reference to those kinds which are realised in a highly complex and idiosyncratic way relative to the similarity spaces of interest for scientific inquiries. To see why this might be so, we must consider the relevant similarity space of interest for *everyday* theory. What are the targets of our explanatory and predictive generalisations in everyday life?

Here is a novel suggestion: Our everyday theory of our minds has been developed for the explanation and prediction of the behaviour of the *individuals* with whom we interact for everyday purposes. I stress that this is not merely the trivial claim that psychological generalisations are *ceteris paribus*. This is mostly, if not always, true—certainly, at least, in the 'special' sciences. But I take it that the special sciences *are sciences*, trucking in generalisations as much as any other. A science of *human* psychology is aiming for generalisations that will hold for *all humans*.

If successful, we would get explanations and predictions of individual humans in virtue of these generalisations. It doesn't simply follow from this, however, that *everyday theory* is aiming to explain and predict all humans. The suggestion here is that our everyday theory of our minds is developed *for* individual explanations and predictions, i.e., tailored, individual explanations and predictions are its purpose, function, goal, or aim. If so, the kinds posited in our everyday theory are to be evaluated by how well they support *this* aim and not the distinct aim of explaining and predicting *all humans*. Some successful posits of our everyday theory may turn out to be useful for a scientific human psychology explaining all humans. But, some may not.

If credible, this suggestion about the aim of our everyday theory of our minds provides strong reason to think that a great many kinds will be promiscuous. Individual minds are highly complex and idiosyncratic, and it is individuals which are here taken to be the explanatory and predictive target. This will lead to prevalent promiscuity insofar as the kinds useful for *that* purpose are unlikely to be useful for scientific inquiries which seek generalisations across all members of those other taxa central to those inquiries e.g. mammals, animals, humans, beings with a central nervous system, and so on.¹² Individual differences may undermine utility of reference for *these* inquiries without undermining the utility of referring to them for explaining and predicting the behaviour of individuals *as such*. At the same time, notice that the mental posits of our everyday theory are able to encompass this wide range of heterogeneous realisers in everyday life in part because they are broad, general functional kinds. The allowance of this diversity is very useful if our goal, as I am here suggesting, is to explain and predict the behaviour of any one of the diverse range of individual agents we encounter. Everyday mental kinds are posited with the aim of being tailored for individual attribution. We use our everyday theory for any human we encounter. But our goal in using the theory is to explain and predict each encountered individual. It is not to explain and predict all humans as such.

We have already seen a preview of this suggestion in our earlier discussion of pain. I there pointed out that references to pain may be the maximally useful kind for an initial encounter with an individual stranger, and that references to pain may become even more useful as I tailor the posit upon further interaction with the individual. If the suggestion above is credible, then we should expect this to be true for a great many of our everyday mental posits. Though the individual agents with whom we interact have individual minds that are highly complex and idiosyncratic, we need not start theorising afresh upon each new encounter. Rather, we pull our everyday theory off the shelf and immediately begin tailoring it for increasingly

¹² I have here been inspired by Millikan (1999) in arguing against an empirical psychology for all rational beings and her reasons for thinking that empirical psychological generalisations even across all humans will be broad and narrow. It is clear that she takes all humans to be the target of a scientifically respectable *empirical psychology*. There is admittedly nothing in Millikan which suggests that she thinks that the goal of our everyday theory—"folk psychology"—is instead the explanation and prediction of individuals.

better explanations and predictions. The suggestion, again, is that the posits of our everyday theory are posited *for* just these purposes, i.e. to explain and predict the behaviour of individuals.

If the suggestion here is credible, though many everyday mental kinds will be promiscuous, many may not be. This makes sense as our minds are similar in a great many ways. We share an evolutionary history, resulting in a great many shared, broad, characteristics and dispositions. These similarities may result in the positing of some everyday mental kinds that *do* support generalisations across some other taxa of interest. More generally, if the suggestion is credible, whenever everyday mental kinds are *not* promiscuous it will be because the kind posited to explain and predict the behaviour of individual agents *turns out* to be realised or to operate in a way which is usefully generalised across some other taxa of interest. These may be appropriate candidates for an empirical psychology of *all humans*, for example, or *all primates*, or *all mammals*, or even *all animals*, or perhaps even *all biological organisms*. It seems *prima facie* plausible, for example, that any posited everyday mental kind whose realisation is strongly tied to the operations of a mechanism posited by a scientific inquiry will support at least some explanatory and predictive generalisations across all members of the kind for whom that mechanism is selected. Candidates under this heading seem to me *prima facie* to include e.g. modality-specific sensations of various kinds and perhaps particular affect programmes, e.g., fear. Many everyday mental kinds, however, *prima facie* appear only implausibly tied to any such thing. Beyond pain, for example, consider nostalgia, grief, ennui, shadenfreude, and the general types of *desire* or *belief*. These seem to me to be highly heterogeneous and idiosyncratic in their realisation and to be highly tailored when successfully deployed to explain and predict individual behaviour in everyday life. My own first impressions and intuitions aside: these remain empirical questions.

Nevertheless, if the suggestion is credible, it would be flatly incredible if *all* everyday mental kinds coincided with maximal utility for any other inquiry. If everyday theory is intended for the explanation and prediction of individuals, irrespective of any other taxa of interest, it is an empirical question whether they help explain or predict across any such further taxa. In each case where they *do* apply, there will presumably be some further identifiable reason why this is so, to be discovered through empirical means.

We must finally consider whether the suggestion *is* credible: is it credible that our everyday mental kinds are posited *for* explaining and predicting individuals? In the remainder of this section, I make an initial case by offering three reflections on our everyday practice and brief discussion of a recent movement in contemporary psychology.

Consider first that our criterion for *success* of everyday explanation and prediction focuses on individual explanation and does not require generalisability. An everyday explanation or prediction of an individual agent which fails to generalise to any other, much less every other of some specified group, may nonetheless be perfectly successful. The particular things that explain or predict why our friends,

colleagues, lovers, and children do and say the particular things they do and say are, indeed, sometimes simply accepted to be particular. It is against a mass of complex, specific, idiosyncratic knowledge that the actions of the individuals we regularly encounter are best explained and predicted. Consider that our closest friends know how to engage with us in all of our particularity; they know and are sensitive to our beliefs, cares, concerns, desires, likes, and dislikes. They know our particular histories, strengths and weaknesses, immaturities, and sympathies. It is because they know so much about me that my friend is so reliable about what I will or won't do when angry, tired, in pain, believe there is coke in the fridge, hold a grudge, want attention, and so on, and so forth. It is, crucially, no objection that they cannot explain and predict the action of any other in this same way. The fact that there is some person in my circle of friends whose behaviour I simply cannot understand likewise does not suggest that there is no one in my life whose mind and behaviour I well understand. The core claim is that we needn't be able to explain and predict the actions of every (or even many) agents tokening a mental type to successfully explain and predict the actions of some, or even just one, individual agent through referencing a token of that type.

As a second similar line of reflection, consider our everyday behaviour when *introducing* someone to some other individual or group of individuals. Various details about the individual minds are offered, often as warnings. The goal here is that the newcomer becomes better equipped to interpret and understand the behaviour of the individuals they are about to meet—that is, to be able to best explain and predict using everyday theory and its posits. The most relevant and salient idiosyncrasies of the individual minds are highlighted to facilitate successful interaction. This assumes, of course, that individuals *are* highly complex and individual in their mental functioning. As, indeed, we routinely do. Notice, here, that I think we do *not* assume that everyone is basically the same, with a few slight differences. The assumption is not, it seems to me, of generalisations holding for all with some special circumstances to be highlighted. Our practice here, that is, does not seem to me to merely support the idea that we are adjusting *ceteris paribus* generalisations that are presumed to hold absent further information about the specific case. Rather, the assumption is that individuals are indeed highly individual and idiosyncratic in the workings of their mind. Of course, we don't expect the newcomer to start from scratch and begin explaining what beliefs, fears, perceptions, and desires are. Rather, we begin with those excellently adaptable everyday mental kinds and do some quick, initial tailoring, expecting our newcomer to continue the job for themselves. We provide the details of the relevant individual minds we think may be most salient and most easily accommodated using the posits of our everyday theory. Our everyday posits are maximally useful, I am suggesting, for just this purpose.

As a third and final bit of reflection on our everyday practices, I invite the reader to consider their own practice on encountering a stranger. As obliquely suggested in the previous section and discussed in the previous reflection, one begins with the mental kinds posited by everyday theory and almost immediately

begins tailoring them to the individual they encounter. You immediately begin, that is, to build a theory of their highly complex and idiosyncratic individual mind. Of course, you do not do this from scratch; you begin with the posits of our everyday theory. You also do not do this perfectly; our theories are unfortunately infected with all sorts of prejudice and bias. One may think that most people fall into broad types and begin their theorising from there. If so, I recommend that one be careful—careful to remember that individuals are, indeed, highly complex and idiosyncratic individuals. Even if starting from prejudiced assumptions, I submit that you in fact typically (imperfectly) tailor your theory to the inevitable idiosyncrasies which present themselves. We expect to need to do this tailoring and we do it almost automatically. Again, then, we see that the mental kinds posited by everyday theory are useful for individual explanations partly because of their promiscuity; promiscuous mental kinds can be individually tailored to encompass a diverse range of realisers—not only at the sub-personal levels of genetics and neuroscience, but at the personal levels of biology and psychology. When meeting a new creature which you take to have a mind—a human, a dog, a chimp, and perhaps even some advanced machines—I suggest that you immediately begin to use those promiscuous posits to build a theory of that individual mind in order to explain and predict that very individual. Our everyday posits are maximally useful for these individual explanations and predictions.

In addition to these reflections on our everyday practice, recent reflections from contemporary psychology suggest that mental kinds, as posited by everyday theory, are indeed for explaining and predicting individuals. Following Peter Molenaar's landmark (2004), there is a movement in psychology to 'bring the person back' into psychological research. Molenaar's claim is that psychology should be understood as an *idiographic science*—that is, a science of the individual. The core claim of the movement is that psychological variables, e.g., mental traits, states, and processes, should be studied within, and not across, individuals. Advocates of the movement emphasise ways in which either the methods or the generalisations offered in psychological research, which focus on large populations, do not adequately explain or predict the behaviour of individuals.¹³ Psychological research often involves sampling the human population: looking for generalisations holding in the general human population, which are then simply assumed to apply to particular individuals within that population. It turns out, however, that at least many generalisations seemingly validated at the population level do not hold, not even *ceteris paribus*, at the personal level. For such entailments to hold, those features of the population which concern the manifestation of the variables and their relationships would also have to characterise the individual, but they simply don't. The way the variables are distributed, related, maintained, unfold, and so on, all instead exhibit relevant individual differences. Molenaar (2004) among others Hamaker et al. (2005) aims to show that no matter how large the sample size,

¹³ For a sample, see e.g. Borsboom et al (2005), Conner et al. (2009), Hamaker (2012), Salvatore and Valsiner (2009).



the requisite structural relationships to support predictions or explanations about an individual from information about a population are simply absent. Instead, as Molenaar and Campbell (2009) put it, many psychological states and processes are “person-specific” such that “their analysis should be on intraindividual variation” (p. 116). Advocates thus emphasise models and methods for single-subject approaches and champion research into the development of further methods and techniques for obtaining and analysing single-subject data. Thus does Molenaar advocate in his (2004, p. 202) manifesto that: “Each person is initially conceived of as a possibly unique system of interacting dynamic processes [...]” In sum, then, advocates maintain that the mental kinds posited and manifested in everyday life exhibit individual differences that must be acknowledged if we want to rigorously study these kinds or explain and predict the individuals in which they are tokened. If the purpose of our everyday theory is individual explanation and prediction, this is just what we should expect.

Though the movement for psychology as an idiographic science thus seems to support the suggestion that everyday theory of our minds aims at individual explanation and prediction, it also raises two potential objections. First, idiographic science is apparently *a science*. If so, one might object that everyday kinds which explain and predict individuals are nonetheless perfectly natural. Moreover, notice that gathering single-subject data may be hoped to eventually support generalisations across large populations. As Ellen Hamaker (2012, p. 17) summarises:

If we allow for individual differences in within-person relationships, we may find that these relationships [between tested variables, e.g. psychological traits, states, and processes] are actually identical across individuals, indicating that there is some general law.

This suggests a second objection against any promiscuity: that the suggested goal of individual explanation and prediction merely suggests the form of the relevant scientific generalisations; they hold for individuals, but nonetheless for *all* individuals. Both objections can be adequately addressed.

Concerning the first, I note that I am again happy to cede all linguistic ground to an insistent objector. We may say that we have a science of each particular individual, if we like. Indeed, if one insisted, the reflections on what we *do* in building a theory of each new encountered individual could well be considered reflections on our scientific practice. Our building a theory of each individual mind may be taken to render us scientists of each of the individuals whose behaviour we aim to explain and predict. I am, I might obliquely say, a very good *x* scientist, for all those *xs* for whom I have developed theories. As noted in the previous section, I am sceptical that our everyday explanations and predictions are best considered scientific. Moreover, I am sceptical that we should consider individual explanations and predictions to *be* scientific; as a minimal criterion, I take it that science aims at generalisations across all members of some specified class. But this scepticism could presently be put aside. We could remain neutral about whether

investigations and discovered generalisations limited only to an individual agent—and not a broader class or domain of interest—should be considered science. If one was clear that the relevant “science” of individuals was, indeed, limited to individual explanations and predictions, then all the substantive claims could again be recast. Everyday mental kinds would be taken to be maximally useful *only* when referenced for idiographic science. I again set this linguistic variant aside in what follows, speaking in my own preferred way.

The second objection is more substantive and may appear more problematic. The worry is that idiographic science delivers explanations and predictions not only of *particular* individuals, but of *all* individuals—presumably, all individuals who bear the relevant psychological properties. In response to this objection, I return first to our first weaker claim: whether any everyday mental kind is promiscuous is an empirical question. If we grant, as this objection does, that our everyday theory aims at individual explanations and predictions, we may then indeed rightly investigate to discover whether there are any discovered within-person generalisations which hold across all individuals. I am perfectly willing to grant that there may be. One of Hamaker’s (2012) candidate examples is: yesterdays’ negative affect predicts today’s negative affect within all subjects but not across any two subjects. Perhaps. Of course, it may instead turn out that there are *more* explanatory and predictive kinds available, not only across but *within* subjects, once we go looking. Affect *may* turn out to be a promiscuous kind: this is an empirical question. Turning now to our second point, we stress again: even if there are *some* discoverable within-person explanations and predictions that will hold across all persons, the idiographic nature of psychology granted by this objector gives us reason to expect that there are many that will *not* generalise. The complex idiosyncrasy of individual minds is such that we can expect that many mental kinds—granted to be posited for individual explanation and prediction—will indeed be promiscuous.

The extension of our discussion on pain, these reflections on our everyday practice, and the movement to ‘bring back the person’ into psychology, all thus lend credibility to the suggestion that our everyday theory of our minds is for individual explanation and prediction. If so, we should expect many of our everyday mental kinds to be promiscuous, since individual minds are complex and idiosyncratic. The broad mental types posited by our everyday theory usefully encompass a diverse range of tokens manifested by a diverse range of individuals. We can conclude that it is an empirical question whether any everyday mental kind is promiscuous and that there is good reason to think that the promiscuity of everyday mental kinds is prevalent.

4 Conclusion: applied sciences and why promiscuity matters

I have here argued for a version of promiscuous realism without promiscuous naturalism. To do so, I introduced the notion of a promiscuous kind as a kind that is maximally usefully referenced in predictive and explanatory generalisations, none of which are scientific generalisations. I defended the claim that pain, in particular, is a promiscuous kind. The reasons and defense of the promiscuity of pain were then extended to consideration of our everyday theory of our minds more generally. In particular, it is *prima facie* plausible that a great many of the mental kinds posited in our everyday theory are complex and idiosyncratic, such that reference to them is not maximally useful for the explanations and predictions of any scientific inquiry. Going further, I suggested that our everyday theory is *for* the explanation and prediction of individuals and argued that this suggestion is made credible not only by our discussion of pain, but by further reflection on our everyday practices and developments in contemporary psychology. If the aim of our everyday theory of our minds is the explanation and prediction of individuals, then it is an empirical question whether the posits of this theory will be maximally useful for any scientific inquiry and the complex idiosyncrasy of individual minds gives us reason to think promiscuity will be prevalent among our everyday mental kinds.

In closing, I offer four brief reflections on implications. As briefly mentioned above, consider first that there are implications for diagnostic practice and appropriate treatment interventions on many mental kinds. If we understand appropriate treatment interventions to target specific mechanisms, then promiscuous kinds are unlikely to serve as appropriate treatment targets. Clinicians must instead use patient reports to identify those mechanisms that may be a key contributor to the idiosyncratic convergence which determines a particular case. If the promiscuity of mental kinds is prevalent, then the clinician will often be something of a translator, standing at the interface of everyday theory and scientific inquiry. In particular, I here stress that the clinical practice of psychology will often appropriately involve this kind of translation. Though space precludes elaboration, I note that there are independent reasons to think that this *is* an appropriate model for clinical psychology and, indeed, the direction in which it is moving. Consider movements for individually tailoring treatment across a range of maladies. The idea here is that patient reports are appropriately received, interpreted, and an appropriate mechanistic target is identified for intervention. The targeted mechanism, notice, is typically not a mechanism *for* an everyday mental kind—these are promiscuous and encompass a heterogeneous range of targeted mechanistic realisers. It is my fervent hope that treatment interventions across a range of maladies will continue to improve as we recognise the diverse range of realisers which may most relevantly be targeted in the particular cases of particular suffering individuals.

Second and most briefly, consider that similar implications may hold for the applied sciences more generally. Insofar as these are indeed applications of scientific theory to other purposes or interests, they too will stand at the interface of scientific theory and those purposes—culinary, architectural, aquacultural, and so on. Good practitioners of these applied sciences will often likewise need to translate the kinds maximally useful for the theory which is to be applied to the kinds and particulars maximally useful for the application being developed. Even as in the clinic: the importance of translation for application may illuminate the best practice for an applied science in ways which contribute to its effectivity. As with good clinicians, it seems to me that most good practitioners of these applied fields already know this, if only tacitly.

Third, consider the implications for our understanding of the nature of psychology more generally. The key points here are that the mental kinds we have posited in our everyday life are intended for individual explanation and prediction and the complex, idiosyncratic nature of individual minds is such that there is good reason to think that promiscuity will be prevalent. Quite a lot of ground remains. As granted above, any particular mental kind *may* nonetheless receive maximally useful reference for some further scientific inquiry—perhaps most likely when occurrences are closely tied to a mechanism selected for the stipulated function of the everyday mental kind. As also granted, generalisations holding within individuals but nonetheless across all (or most) individuals may likewise find support. Notice finally, and crucially, for all that I have here argued distinct kinds may be posited within specialised scientific inquiries into minds and behaviour that are maximally useful for explaining and predicting some mental or behavioural phenomena of interest across all e.g. humans, mammals, animals, creatures with a particular neural or biological mechanism, or any other taxa of interest. Where exactly the prevalent promiscuity among everyday mental kinds leaves psychology is (what else) an empirical question.

Fourth and finally, I close with the admittedly impressionistic note that I take the picture here to contribute to validating an understanding of ourselves as complex and idiosyncratic individuals. This is relevant beyond the crucial mentioned context of diagnostic and clinical practice. We are, it seems to me, currently often focused on our memberships of various social kinds, e.g. gender, sex, class, race, and so on. Membership of these kinds is often important, sometimes even vitally important. I heartily acknowledge that being a member of one of these social kinds is sometimes explanatory and predictively very powerful. Nonetheless, there are *also* a great many ways in which one is highly complex and idiosyncratic. In particular: you have an individual mind that is highly complex and idiosyncratic. This individuality, too, is to be remembered and acknowledged. If the arguments above are correct, the individuality of our minds is maximally useful for successfully explaining and predicting the behaviour of the individuals with whom we interact.



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