Folk psychology and network theory: Fact or gamble? A reply to Kalis and Borsboom

Freek Oude Maatman
Radboud University Nijmegen

Abstract
Kalis and Borsboom (2020) defend their realism about folk psychology against my challenge to provide a grounding argument for the correctness of folk psychological explanation (Oude Maatman, 2020). In this reply, I show how their clarified realism in fact vindicates this challenge, as it heavily relies on the predictive success of folk psychology. I then proceed by describing how their realist interpretation of “intentional content” complicates the usability of network theory, and show that both their antireductionism and realism are grounded in an empirical gamble against alternatives. I end with a brief defense of my own version of network theory.

Keywords
folk psychology, network theory, psychopathology, realism, reductionism

Kalis and Borsboom (2020) aim to take up my challenge (Oude Maatman, 2020) to support their folk psychology-informed network theory (NT; Borsboom et al., 2019) by providing “an argument for the correctness of folk psychological explanations, through a path that is not biological” (Kalis & Borsboom, 2020, p. 724). They do so by first confirming that they indeed interpret folk psychology realistically, and by clarifying the version of realism they adhere to as related to the accounts of Ladyman and Ross (2007) and Haugeland (1993). Then, they take up my challenge by answering two self-imposed questions: How can folk psychological states make a causal difference and can folk psychological descriptions be falsified; as well as providing a brief attack on my proposed NT.

In doing so, Kalis and Borsboom (2020), however, defend a position that is not my main target. They primarily provide a defense of the philosophical foundations of their...
realism about folk psychology and folk psychology-described mental causation, whilst
my attack was directed at the scientific consequences of this realism for NT (e.g., depend-
ence on the truth of folk psychology, unverifiability and unfalsifiability of symptom
networks with intentional state symptoms) which are complicated by various properties
of folk psychology itself (e.g., instability, cultural specificity, underdetermination). Of
course, how problematic these latter properties of folk psychology truly are depends
largely on the type of realism about folk psychology one adheres to, which also goes for
the consequences I outlined.

So, how does Kalis and Borsboom’s (2020) realism address my critiques? This
remains somewhat unclear, for they only directly address some of my arguments. Under
closer scrutiny, their newly specified realism surprisingly even appears to vindicate my
earlier critiques. I will therefore outline how this vindication comes about, as well as
show how their realism and their NT’s resultant antireductionist force relies primarily on
an empirical gamble against alternatives. After that, I offer a brief rebuttal of their coun-
ter-arguments to my proposed NT.

**Folk psychology: A fact or a gamble?**

As was pointed out above, Kalis and Borsboom (2020) refer to the accounts of Haugeland
(1993) and Ladyman and Ross (2007) to ground their realist interpretation of folk psy-
chology. Whilst both accounts can support an interpretation of intentional states as real
patterns, they are far from identical. This makes Kalis and Borsboom’s (2020) realism
difficult to argue against. Kalis and Borsboom (2020), however, do not discuss the condi-
tions both accounts set on this realism, which, in both cases, is closely related to the
explanatory success of folk psychology. In the case of Haugeland (1993), a repeated
failure of folk psychological predictions violates a rationality assumption necessary for
identification of folk psychological phenomena. This entails that the described person or
system cannot be predicted folk psychologically, or even cannot be said to have inten-
tional states. Such repeated failure then either excludes the to-be-explained system or
person from the folk psychological domain, or, in the case of large-scale failure of pre-
diction across individuals, forces us to give up on folk psychological explanation (and
realism about it) altogether.

In Ladyman and Ross’s (2007, pp. 231–233) view, to be realist about folk psychology,
it needs to possess projectibility (i.e., better-than-chance predictive power) and maxi-
mized information–theoretic utility when considering all possible projectible relations-
ships in the world as compared to all possible alternative lower level explanations. In
contrast to Kalis and Borsboom’s (2020) reading, this means that they consider realism
about folk psychology to be an open empirical question: folk psychological patterns
(e.g., “beliefs that X”) are not real if, for example, “all projectible patterns tracked by
intentional psychology along with others it doesn’t track are projected by a mature cogni-
tive behavioural neuroscience” (Ladyman & Ross, 2007, p. 232).

On both of these accounts, a (continuous) predictive failure of a folk psychologically
specified real pattern then effectively entails that we cannot be realist about this pattern,
which in turn would void Borsboom et al.’s (2019) intentional content-based arguments
against biological reductionism. In Ladyman and Ross’s (2007) view, predictive success
itself is also not enough; folk psychology needs to be more information–theoretically efficient in a “complete” science than all other possible lower level explanations. Kalis and Borsboom (2020), then, are in fact betting on the (continued) empirical success of this “causal language” to ground the biological irreducibility of causal connections such as “delusion → behavior change” (Borsboom et al., 2019). This inadvertently forces them to treat folk psychology as a scientific theory, especially if it is to compete with other scientific theories in Ladyman and Ross’s (2007) scientifically informed approach to metaphysics. On Haugeland’s (1993) account, it is not even clear whether the success of folk psychology would really exclude biological reduction—instead, he appears to claim that any alternative theories would simply pick out different phenomena (Haugeland, 1993, p. 65), which does not preclude their explanatory or causal relevance for psychopathology, nor safeguard that of folk psychology. This necessity to treat folk psychology as a scientific theory also shows that the problematic properties of folk psychology that I previously outlined really must be accounted for (Oude Maatman, 2020), which Kalis and Borsboom (2020) so far have not done. And finally, it shows that their realism is only making my earlier challenge to provide an “argument for the correctness of folk psychological explanations” (Oude Maatman, 2020, p. 712) more salient.

Any answers to this challenge can draw from a large philosophical literature, discussions that are far from settled. Kalis and Borsboom’s (2020) philosophical arguments therefore invite deeper scrutiny, and their position likely requires more support than they so far have given. Notably, this also applies to my own account (Oude Maatman, 2020). However, as stated before, I am primarily concerned with the empirical consequences that the realism of Borsboom and colleagues (2019; Kalis & Borsboom, 2020) leads to, which my account manages to avoid.

The core reason for this difference between our accounts is that I do not commit to realism about intentional content as an irreducible, mind-independently existing property, whilst Kalis and Borsboom (2020) do—which they confirm in their reaction. The relevance of this difference is best illustrated through an example. Say that we attribute “the belief that the outside air is poisonous” to someone based on this person’s refusal to go outside and their repeated warnings about this (to them) apparent fact. What are we really attributing here? On Kalis and Borsboom’s (2020) account, we are attributing a multiply realizable mental state (i.e., a belief) to someone that causes specific behaviors in virtue of its content (i.e., that the air outside is poisonous). Notably, this mental state is in fact realized by an underlying neural state, but the causally relevant content of the mental state cannot be identified through the biological level. Both “beliefs” and their “content” thus really (mind-independently) exist, but these are only identifiable through the intentional stance—which is what gives Borsboom et al.’s (2019) arguments their antireductive force. On my account, we are attributing a neural state to someone that has been formed due to this person having learned a whole swathe of linguistic conceptual associations (e.g., “poison is deadly”), and which, given the internalized logic of this language, should lead to a very predictable set of behaviors. Furthermore, this real pattern is most efficiently described and understood using the language this logic is derived from, especially by individuals who share this internalized structure. The attributed neural state then—on a functionalist view—could be equated with the “belief that the air
outside is poisonous” mental state due to playing the right causal role: it realizes the real pattern associated with this belief.

These two accounts are empirically equivalent in terms of the symptom–symptom relationships we would derive from them, as well as the real patterns that they predict to exist. Yet, our treatment of “content” is very different: in my view, the content of a belief is simply a linguistic specification of which real pattern we are picking out. On Kalis and Borsboom’s (2020) view, this “content” instead is an irreducible property of mental states that is crucial to causation—though it remains unclear whether they consider it causally relevant or causally efficacious. A belief really is about something, and it is this content that determines what causal effects the belief has: a belief about poisonous air can cause one to stay inside, whilst a belief that one is Napoleon and should raise taxes can cause the opposite (Kalis & Borsboom, 2020). Given a change in content of the belief, a different behavior ensues—and thus we end up with Kalis and Borsboom’s (2020) claim that the content of a belief can be said to be a genuine cause of behavior in counterfactual accounts of causation (e.g., Pearl, 2009).

Yet, this leads to several scientific problems (e.g., unverifiability, unfalsifiability; see Oude Maatman, 2020), as this “content” is not open to empirical scrutiny outside of external attributions through the intentional stance, or introspection. In their reaction, Kalis and Borsboom (2020) counter this argument by pointing at scientific psychology as an alternative source of support for their claims, relying primarily on a sweeping generalization of the field as engaged with the “study of mental states.” Like their philosophical grounding, this claim invites both further scrutiny and support. As a provisional critique, I point to the contrast between the folk and scientific psychological vocabularies, which are likely to have (subtly) different intensions and extensions. If scientific psychology is more efficient or precise than folk psychology, this also would undercut any Ladyman and Ross-inspired realism (2007).

I can also muster a counter argument. Say that, using Kalis and Borsboom’s (2020) realism, a causal connection between intentional state symptoms is posited: for example, “rumination → sad mood,” because it “makes sense” that repeatedly obsessing over your failings makes you feel sad. If scientific psychology offers empirical support for this causal connection, this does not exclude any alternative reductionist, eliminativist, or instrumentalist explanations: such exclusion would only happen in the case that we can truly prove the causal efficacy of intentional content, as these accounts are empirically equivalent in all other cases. Kalis and Borsboom’s (2020) position also provides no unique predictions that could otherwise be used to distinguish it from alternatives, because they do not specify any type of causal mechanism or model but instead rely primarily on claims of simple counterfactual causation between intentional states. And in the case that this “rumination → sad mood” causal connection is shown not to exist, this is a strike against the realism about folk psychology that Kalis and Borsboom (2020) claim to adhere to—but, as I argued (Oude Maatman, 2020), this seeming falsification can be explained away by positing further folk psychologically relevant yet unmeasured or unconsidered mental states.

This means that upon adoption of Kalis and Borsboom’s (2020) realism about folk psychology, NT becomes a sitting duck. There is no tractable way to provide new support for their type of realism, but there are many ways in which it could fail or be eclipsed.
Given that their realism also relies on folk psychology’s predictive success to block biological reductionism, antireductionists who accept Borsboom and colleagues’ (2019; Kalis & Borsboom, 2020) NT can only sit on their hands and hope that folk psychological predictions about symptom–symptom networks are not falsified nor outperformed by a lower level theory. Their entire claim of the irreducibility of mental states is then—at its core—an empirical gamble against the success of alternatives, just like Kalis and Borsboom’s (2020) realism.

**Folk psychology: A factor?**

Kalis and Borsboom (2020) also offer three critiques of my proposed NT (Oude Maatman, 2020). Two of these critiques appear to be grounded in differences of opinion, specifically on the need for realism about folk psychology to do “justice to [its] crucial role . . . in the explanation, prediction, and regulation of human behaviour” (Kalis & Borsboom, 2020, p. 727) and whether my position prioritizes lower level explanations over higher level ones (e.g., biological over mental). Neither of these critiques appear to invalidate my proposal for NT. Briefly, I would reply that I believe I am doing more justice to folk psychology than are Kalis and Borsboom (2020) by describing how it gets to play this crucial role. Furthermore, even though my proposed NT prioritizes the lower level neural explanation for intentional state symptom causation, on my account this cannot be separated from the higher level structuring causation stemming from folk psychology and culture—entailing that neither is truly prioritized over the other.

Their remaining critique is that my usage of the term “structuring causes” is misleading if I do not consider such structuring causes to be causes. To this I would reply that I do consider structuring causes to be genuine causes. In Oude Maatman (2020) I admittedly used the term “actual cause” ambiguously, as I used it to indicate the mind-independently real cause underlying an intentional state symptom node in a symptom network. Due to being learned and enforced, folk psychology (i.e., the causal language of beliefs and desires) instead is a genuine, actual cause of the causal connections between the realizing neural states—as was also described previously. Thereby, it also causes these neural states to respect folk psychological prediction and description, as well as allowing their possible identification with folk psychological mental states. It is thus a genuine cause, yet not within a symptom network.

**Conclusion**

To conclude, I have shown that Kalis and Borsboom’s (2020) position—and in extension that of Borsboom et al. (2019)—is a gamble at its core, with (apparently) long odds. Further support and scrutiny therefore appear warranted.

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ORCID iD
Freek Oude Maatman https://orcid.org/0000-0002-4795-435X

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Author biography
Freek Oude Maatman is an external PhD student and junior lecturer at Radboud University Nijmegen. His main research interest is the evaluation of the methodology and theory of psychology, especially those of social and clinical psychology.