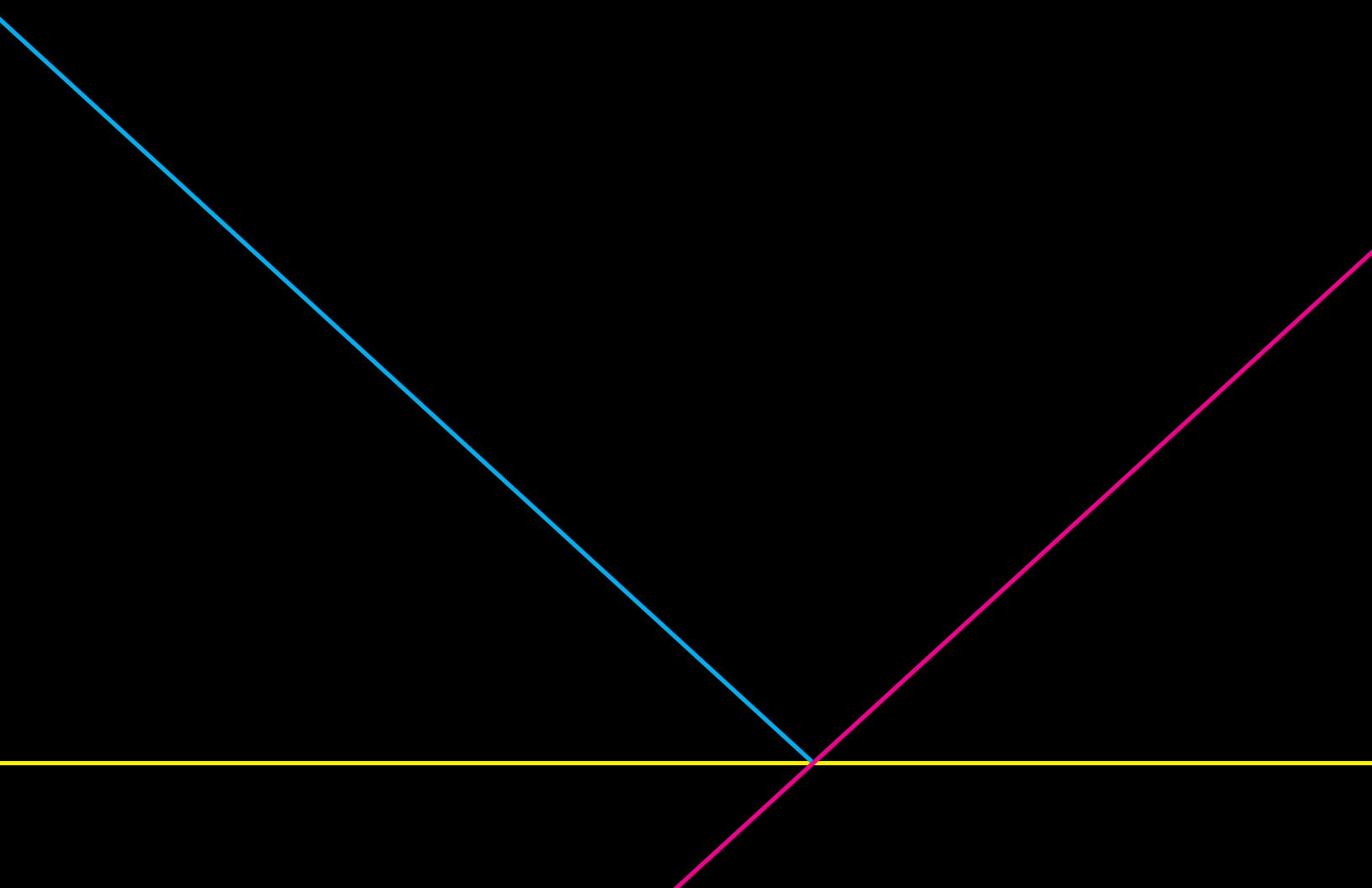


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PREFACE

This fourth edition of *Rerum Causae* represents an international collaboration. In May of this year, graduates and undergraduates from the LSE Department of Philosophy, Logic, and Scientific Method hosted their counterparts from the Universität Bayreuth Philosophy & Economics Programme at the first LSE-Bayreuth Student Philosophy Conference. Over two full days, participants in this conference saw debates on topics ranging from the limits of the market to cognitive materialism and problems of global justice to feminist epistemology. The proceeds of this exchange have been compiled and edited by LSE students, and they form the body of the journal you are now reading. As a bonus, the editors have personally contributed an interview of the philosopher James Ladyman and a review of Brian Leiter's book *Why Tolerate Religion*. We hope that you are entertained and challenged by what follows, and we thank you for supporting what has truly become a global forum dedicated to understanding the causes of things.

The Editors

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WHY WE SHOULDN'T ALL BE ELIMINATIVE MATERIALISTS (YET): UNDERSTANDING THE FAILURE OF CHURCHLAND'S ARGUMENT

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INTRODUCTION

This essay takes issue with Paul Churchland's argument for eliminative materialism. In Churchland (1981), he attempts to demonstrate the theory of folk psychology is radically false and should be replaced by a thoroughly neuroscientific account of the mind. Here, I summarize the thesis of eliminative materialism within the cognitive sciences (Section I), and then recapitulate Churchland's argument in favor of this view (Section II). In the final section (Section III), I assess the successes and failures of Churchland's argument, and ultimately conclude that his argument fails to provide a compelling case for eliminative materialism. However, one caveat is in order. Within this essay, I do not argue in favor of the claim that we will never transcend a folk psychological theory of mind. Rather, my claim is the much weaker one that Churchland's argumentative structure fails to demonstrate the falsity of folk psychology.

I. THE THESIS OF ELIMINATIVE MATERIALISM

Folk psychology, according to the strong thesis of eliminative materialism, is fundamentally mistaken. The general claim of the eliminativist is that certain categories, entities, and properties utilized in commonsense explanations and descriptions of the world do not exist and should therefore be dropped. Within cognitive science, the proponents of eliminative materialism identify folk psychology as a primary candidate for immediate elimination. They claim that the theoretical entities this commonsense theory of mind posits—such as mental states and propositional attitudes—have no basis in reality. On their view, our everyday attribution of thoughts, beliefs, desires, and other propositional attitudes to others in order to explain human behavior is based upon a fundamentally false theory of mind. We should, therefore, disabuse ourselves of this commonsense theory and replace it with a theory firmly grounded within the conceptual and theoretical framework provided by neuroscience.

The eliminative materialist *par excellence* is the philosopher Paul Churchland. He argues that we have no good reasons to assume that the theoretical entities posited by folk psychology will play any role once neuroscience is able to offer a complete theory of mind. Instead, he suspects neuroscience, ultimately, will afford us a theory of mind that is incommensurable with the everyday propositional attitudes of folk psychology. In order to provide support for the thesis of eliminative materialism, Churchland attempts to demonstrate that folk psychology is a radically false theory.

II. CHURCHLAND'S ARGUMENT FOR ELIMINATIVE MATERIALISM

The eliminativist argument proceeds in two critical steps. First, it must demonstrate that a particular theory, which posits certain ontological entities, processes, or functions, is false and should be rejected. Second, it must offer a better theory to replace the rejected one.¹ Churchland (1981) attempts to carry out this program by first arguing against the theory of folk psychology and then advocating a thoroughly neuroscientific theory of mind. In this section, I sketch out Churchland's argument against folk psychology before offering my assessment of its shortcomings in Section III.

Churchland views folk psychology as the principal obstacle preventing the general acceptance of the thesis of eliminative materialism. In particular, he claims the inability to reduce propositional attitudes to neuroscience gives purchase to those who wish to maintain that propositional attitudes are fundamental and that folk psychology is irreducible. Therefore, he directs his philosophical sword toward the heart of this commonsense theory, in an attempt to demonstrate that propositional attitudes are spurious ontological entities not grounded in the reality of the brain. If he can successfully prove the theory of folk psychology to be false, then the eliminative materialist perspective is that much more likely to be true. Unfortunately for Churchland, even if he succeeds in discrediting folk psychology, he will not have demonstrated that eliminative materialism is the correct view. His argument in favor of the eliminativist thesis takes the form of a disjunctive syllogism. However, in this case, there are more than two possible theories of mind. Simply demonstrating folk psychology is not the case, therefore, does not logically prove eliminative materialism to be true. Of course, Churchland is well aware of this fact, but he suspects that once folk psychology and the propositional attitudes it posits are out of the way, the argument for eliminative materialism will meet with less resistance.

Before arguing against folk psychology, Churchland finds it necessary to argue that folk psychology is, in fact, a theory. He claims that the explanatory apparatus of folk psychology constitutes an empirical theory because it posits theoretical entities and nomological relationships that obtain between those entities.² Moreover, we use this theory of mind in everyday situations to predict the behavior of others. Here, Churchland is at pains to demonstrate folk psychology is an 'empirical theory', since this makes it susceptible to refutation.³

He then provides three arguments to demonstrate that folk psychology and the categories it

1 This general structure of the eliminativist argument is drawn from Ramsey *et al.* (1990).

2 Churchland provides a richer argument to support his claim that folk psychology is a theory, but I, and other philosophers (cf. Jackson and Pettit 1990: 33) accept this fact. Since I will not be arguing against this point in Section III, it is unnecessary to fully reconstruct Churchland's argument for this claim.)

3 Indeed, Churchland highlights the '*perils*' (emphasis in original) that come with the status of theory (Churchland, 1981, p. 68)

posits are unable to provide an accurate description of the human mind and mental activity. First, he claims a good theory must be sufficiently complete so as to be able to account for all the relevant phenomena. Despite its many successes in predicting human behavior, the theory of folk psychology is silent on many aspects of mental life, such as sleep, imagination, and mental illness. For Churchland, this fact is a serious shortcoming. Second, Churchland contends a good theory must exhibit the feature of continued growth and development. That is to say, a good theory should be improved upon and refined throughout the course of history, but this is not the case with the theory of folk psychology. According to Churchland, our contemporary folk psychology is equivalent, in all its key features, to the folk psychology we inherited from the Greeks, and our ability to predict human behavior has improved not a whit. Churchland's third challenge to the theory of folk psychology is considered by some to be the most compelling argument.⁴ Here, he claims that a good theory in one field should mesh with similar theories in related fields. However, for folk psychology that is not the case. The propositional attitudes posited by this theory have no equivalents in neuroscience, biology, or physics, nor do they seem to be reducible to the key concepts in these scientific fields. Thus, the empirical sciences do not lend their support to the theory of folk psychology.

Although Churchland admits these criticisms of folk psychology do not prove it to be false, the fact that this commonsense theory lacks all three hallmarks of a good empirical theory is enough for him to suggest we would be better off without folk psychology. Now that we have reconstructed the key claims of Churchland's argument, we are in a position to assess whether or not Churchland has provided sufficient evidence to discredit folk psychology.

III. AGAINST CHURCHLAND'S IMPLICIT DEFINITION OF A GOOD THEORY

To succeed in his eliminativist argument against the theory of folk psychology, Churchland must first demonstrate this commonsense theory is false and should be rejected. He attempts this proof by arguing that in three particular cases folk psychology fails to meet the criteria required of a good theory: 1) it is too limited to explain all the relevant mental phenomena, 2) it has not grown or developed in millennia, and 3) it does not cohere with theories in related fields. Here, I argue against all three of the selection criteria Churchland uses to evaluate the theory of folk psychology. By demonstrating that none of these criteria are uncontroversial, I aim to show that Churchland has provided insufficient evidence to convince us to reject folk psychology out of hand.

Theories, like the laws of physics, have a limited domain of applicability. Nancy Cartwright forcefully argues this position in *How the Laws of Physics Lie*.⁵ If a theory can explain a limited

4 Cf. Clark (2001: 45)

5 See in particular: 'The Truth Doesn't Explain Much' in Cartwright (1983: 44-53). She also argues against

set of phenomena very well, we have no reason to suspect another theory will come along that explains those same phenomena *and additional phenomena* in one stroke. In other words, we should not expect too much from our theories. The mere fact that folk psychology fails to have much (if any) explanatory salience over certain aspects of mental life (e.g., sleep or mental disorders) is not a serious challenge to the theory, so long as it still more or less accurately predicts a certain subset of normal, wakeful human behavior.

An example from chemistry will reinforce my point. Boyle's law⁶, also known as the ideal gas law, provides us with a general law that relates the temperature (T), pressure (P), and volume (V) of a gas. As the name implies, it only holds for 'ideal gases' under standard conditions. It does a great job quantifying the relations between P, V, and T within certain standard temperature and pressure ranges, but it does a crummy job predicting the behavior of gases at high temperature and pressure. This fact, however, is not a shortcoming. So long as we know in which cases the theory holds, we have no problem employing the theory to make useful predictions. The same holds true for folk psychology. If folk psychology works well at predicting human behavior in most cases when humans have normal physiology and are awake, then we have no reason to reject it for those cases. Thus, Churchland's argument that the limited explanatory power of folk psychology is a reason to reject it is not in accord with the practices in the empirical sciences.

To address Churchland's second argument against folk psychology, we could turn to the history of science to assess the veracity of his empirical claim that the theory of folk psychology has remained stagnant for millennia. Horgan and Woodward (1985) follow this line of argumentation. They claim that rather than remaining unchanged for millennia, folk psychology has in fact been empirically progressive. For instance, in the 18th and 19th centuries, it was quite common to appeal to invariant personality traits to explain behaviour, whereas, in the 20th and 21st centuries, we are much more likely to consider contextual and situational factors. Horgan and Woodward contend that modifications such as this have given the theory of folk psychology greater explanatory power and have improved its predictive success.

There is also another line of argumentation worth pursuing against Churchland's second argument. Instead of contesting the empirical facts of the matter, it is possible to question the criterion itself. Perhaps the evolution of a theory through time is not a necessary condition for a good theory. Mathematics seems to be replete with counterexamples. For instance, we do not value the Pythagorean theorem any less because it has not been improved upon since it was first set forth by Pythagoras. This fact suggests we could, in good faith, question whether or not

a metaphysical picture that believes we will find a few general laws that can explain most everything and instead suggests nature provides only a 'patchwork' of laws with limited applicability in *The Dappled World* (Cartwright 1999: 1-21).

6 Boyle's law describes the relation between pressure (P), volume (V), and temperature (T) of a gas: $PV=nRT$. (n is moles of gas and R is the universal gas constant).

growth, development, and refinement are necessary characteristics of good theories.⁷

In Churchland's third and final argument against folk psychology, he contends the theory is likely false and therefore a candidate for replacement because its fundamental unit, the propositional attitude, has not found support in any other theories. On his view, a theory in biology, for instance, is a better theory if it fits into the explanatory framework of other theories in biology, chemistry, and physics. We can take issue with this claim on two levels. We can offer evidence that propositional attitudes play a critical role in theories in other fields, and we can also question whether or not this is a necessary feature of a good theory. If we look to fields outside the natural sciences, we can find many instances that refute Churchland's claim that the theory of folk psychology does not fit into a larger web of theories. Although the propositional attitudes posited by folk psychology have not been reduced to neuroscience or other theories within the natural sciences, they do, nevertheless, seem to be salient features in a variety of other fields, such as, sociology, criminal justice, law, and public policy.⁸ Indeed, we have found the propositional attitudes of folk psychology to be extremely useful, even foundational, in a variety of different domains and disciplines. In this respect, folk psychology does seem to be part of a much larger web of theories and practices, and for this reason, I contend it demands a higher standard of proof to demonstrate its falsity.

However, before we continue to defend folk psychology in this way, we must ask ourselves if we are willing to concede that a good theory must fit in well with theories in other disciplines. To this point, Jackson and Pettit (1990) offer an example from the natural sciences.⁹ The thermodynamic theory of gases, which gives us the concepts of temperature and pressure, has no explanatory salience within the kinetic theory of gases. In fact, once we move to the micro level of statistical mechanics, the macro level concepts of temperature and pressure have no meaning whatsoever. Nevertheless, we still find it useful to retain these macro level concepts. Thus, this example from the natural sciences provides at least a modicum of doubt against Churchland's third criterion for a good theory, and thereby further weakens his argument that folk psychology is radically false.

In attacking folk psychology, Churchland attacks a theory that almost everyone subscribes to, at least to some extent. The very structure of the eliminativist argument lays the burden of proof on him. He must, therefore, convince us that, like the theory of phlogiston and the ether, the

7 Granted, in contrast to mathematics, the history of science does not offer as fertile a source of counterexamples of static theories. Nevertheless, even if we do, ultimately, decide in favor of this second criterion for a good theory, Churchland still needs to convincingly rebuff the arguments of Horgan and Woodward (1985) and explain why their examples do not demonstrate that folk psychology has in fact been modified and refined over time. Even if he can prove his assertion, he still would need to address our concerns about the first and third criteria for a good theory. If only one of three of his arguments against folk psychology holds, then the force of his conclusion that folk psychology is false will be significantly lessened.

8 Churchland admits folk psychology forms a 'central part of our current *lebenswelt*', but this fact, in itself, according to Churchland, is no reason to retain the theory (Churchland 1981: 76).

9 Cf. Jackson and Pettit (1990: 46-47)

theory of folk psychology is radically false and should be eliminated. With the three arguments set forth in Churchland (1981), he has not provided irrefragable evidence that *either* our current incomplete *or* a possible future complete neuroscience will yield a theory of mind that proves folk psychology to be radically false.¹⁰ Churchland's argument that we should eliminate the theory of folk psychology and replace it with our current neuroscientific theory is, therefore, unconvincing.

IV. THE FUTURE OF ELIMINATIVE MATERIALISM

In conclusion, I feel compelled to remind the reader that in arguing against Churchland I have *not* argued in favor of the strong thesis that folk psychology will be with us forever. While Churchland does not provide sufficient evidence to demonstrate folk psychology is radically false, this shortcoming of his argumentative strategy has no bearing on whether or not we will ultimately transcend this commonsense theory of mind. Instead, I prefer to withhold judgment on this issue. To his detriment, Churchland has forgotten a key tenet from the philosophy of science of Sir Karl Popper and Imre Lakatos: the falsification of a theory requires a rival theory *ready to take its place*.¹¹ In his haste, Churchland attempts to convince us to drop the theory of mind from folk psychology in favor of a neuroscientific theory, which is *as yet incomplete*. Perhaps, we would be more prudent to wait than to 'aspire to some foresight for a change'.¹²

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10 To clarify this point, Ramsey *et al.* (1990: 354) introduce a distinction between two types of theory change: ontologically conservative and ontologically radical. Churchland must argue that the transition from folk psychology to a neuroscientific theory of mind is the latter type. However, he has not provided conclusive evidence to demonstrate that the currency of folk psychological theory (i.e., the propositional attitude) is radically incompatible with neuroscience.

11 Popper (1963) and Lakatos (1968) rightly claim that a theory cannot be refuted unless a rival theory is ready to take its place. In this case, Churchland advances an incomplete neuroscientific theory of mind as a substitute for folk psychology. At present, the theory offered by neuroscience is woefully inadequate—if we dropped folk psychology now and only employed neuroscientific concepts, our ability to understand and predict human behavior would suffer. We might find Churchland's argument much more persuasive once neuroscience surpasses folk psychology's predictive and explanatory power.

12 Churchland (1981: 68)

Clark, A. (2001). *Mindware: An Introduction to the Philosophy of Cognitive Science*. Oxford, UK: Oxford University Press.

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WHAT SALLY HASLANGER'S 'DAUGHTER' SHOULD BELIEVE

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Abstract. Sally Haslanger (2007) is concerned with epistemic questions resulting from feminist critique. Her discussion is illustrated by the concrete example of a 12-year-old girl involved in an argument with her parents about whether she should be allowed to wear a crop-top. Haslanger believes that a contradiction arises because the daughter's seemingly false statement that crop-tops are cute also appears to contain "important social knowledge" and thus seems to be false and correct at the same time. Haslanger tries to resolve this contradiction by making use of the concept of context-dependent truth. In this paper, I argue that Haslanger's approach cannot sufficiently solve the problem she has formulated. However, I claim that if we carefully analyze the different components of the disagreement, its epistemic, aesthetic and normative dimensions, we will come to the conclusion that there is in fact no epistemic contradiction to be resolved.

INTRODUCTION

In her essay " 'But Mom, Crop-Tops *Are* Cute' – Social Knowledge, Social Structure and Ideology Critique" Sally Haslanger (2007) investigates epistemic aspects of feminist critique. Specifically, she is concerned with the following problem: we sometimes hold particular belief systems to be "illusory". Yet, if such belief systems are widely shared, they can change social reality. In some cases this constructed social reality will make the initial belief system true. Haslanger considers this a contradiction and tries to resolve it.

In this paper, I will try to reconstruct the problem with which Haslanger is concerned using her own example of a daughter disagreeing with her parents over the cuteness of crop-tops. I will then argue why I believe that Haslanger's proposed solution, "Milieu Relativism", does not fully solve the problem she has formulated. However, I claim that if we carefully analyze the different components of the daughter's statement, its epistemic, aesthetic and normative dimensions, we will come to the conclusion that there is in fact no epistemic contradiction to be resolved.

I. THE PUZZLE

Haslanger's research question is taken over from Catharine MacKinnon (1989) who is *inter alia* concerned with building up a feminist epistemology. Although visibly influenced by MacKinnon, Haslanger seems unsatisfied with the clarity of MacKinnon's positive answers to the questions she raises (cf. Haslanger 2007: 71). It might partly be in pursuit of this missing clarity that

Haslanger chooses a clear-cut example around which she builds her discussion.

i. Haslanger's example

In this example a seventh-grade girl is involved in an argument with her parents about whether she should be allowed to wear a crop-top. While the parents reject crop-tops because they contribute to the sexualization of young girls, the daughter argues they are “cute” and that “[e]veryone knows” this (Haslanger 2007: 72). As Haslanger points out, it seems as if the girl and her parents disagree over the truth-value of the proposition:

(P) Seventh grade girls who wear crop-tops to school are cute [and are dorky otherwise].¹

According to Haslanger, when considering whether the daughter or her parents are justified in believing or rejecting (P), there arises an epistemic problem:

“If the social reality is organized around the cute/dork dichotomy, then there are cute girls and dorky girls, and it would be a mistake not to recognize this. This is important social knowledge. But at the same time it is tempting to say that the cute/dork dichotomy is an illusion. (...) So it appears that the daughter should believe that, say, seventh grade girls who wear track suits to school are dorks, and yet, if her parent is right, she should also not believe it.” (Haslanger 2007: 73)

In other words, we have an intuition that parents and daughter disagree about something beyond their mere personal tastes, and still there is a sense in which both seem to be right.

ii. The epistemic/moral distinction

When analyzing this seemingly paradoxical intuition, Haslanger begins by discussing the response that the use of the term “should” in “she should also not believe it”, is a moral “should” and not an epistemic one. Let us call this the epistemic/moral distinction (EMD). The way to understand this response is, I suggest, that while an “epistemic should” obviously only depends on what is true (or on the reasons we have for believing that something is true), a “moral should”, when applied to beliefs, must also take into account the effects these beliefs might have. The two “should”s can conflict. For example, it might be that if a certain true belief would motivate us to

¹ In fact, Haslanger at first offers two propositions. However, the second is close to being simply the contraposition of the first and contributes little to the analysis. Haslanger herself ignores it in the subsequent analysis for the very same reason (Haslanger 2007: 74).

act in a wrong way, we are morally obliged to believe something false.² EMD would suggest that such a conflict is the case in the disagreement over (P).

However, Haslanger dismisses EMD as not completely satisfactory. She does this for two reasons. First, she raises doubts whether a moral “should” can apply to beliefs at all, as they are not a matter of choice: “The daughter experiences her friends as cute in crop-tops and the track suited others as dorky, and this may not be something she can change at will.”

Second, our rejection of (P) “seems to involve a charge of inaccuracy or misrepresentation.” (Haslanger 2007: 74); the daughter’s belief in (P), built on the cute/dorky-distinction, seems “illusory” to us. But, of course, holding illusory convictions is an *epistemic* failure: thus, the framework seems not (only) morally wrong but epistemically false.

II. HASLANGER’S SOLUTION: MILLEAU RELATIVISM

Instead of EMD, Haslanger’s solution involves the concept of a *milieu*. An individual’s milieu is constituted by the “social structures within which he or she operates” (Haslanger 2007: 80), where *social structures* are very broadly understood: Haslanger uses the term as a “general category of social phenomena, including, e.g., social institutions, social practices and conventions, social roles, social hierarchies, social locations or geographies and the like” (Haslanger 2007: 77). After an extensive discussion of these concepts, in which she moves to more general questions of social ontology, Haslanger comes back to the discussion about (P) by analyzing in which way its truth might be relative to an individual’s milieu. The guiding idea is that truth is context-dependent. Following John MacFarlane (2005), Haslanger hereby distinguishes between *context of use* and *context of assessment*. Context of use captures the familiar idea that a certain proposition’s truth might depend on the context in which it is stated, either because it is *use-indexical*, i.e., the context determines its meaning, or because it is *use-sensitive*, i.e., the truth-conditions might be fulfilled in some contexts, but not in others (MacFarlane 2005: 326f.). In addition to this, “relativist” accounts of truth might accept dependence on the context of assessment. A proposition’s truth in this case depends on the standards of assessment of the assessor who states the proposition. This idea is widely accepted in the realm of aesthetic judgements. For example, the proposition “x is beautiful” does not seem to have a truth-value independent of a standard for the assessment of beauty. MacFarlane’s aim is to extend this idea to the truth-predicate in general (MacFarlane 2005: 328).

Haslanger applies MacFarlane’s framework by setting the girl’s and parent’s respective social

² An example for this can be seen in the famous “Paradox of Hedonism” (Sidgwick 1904): hedonism states that we should do whatever creates the greatest happiness. If Hedonism is (epistemically) true, however, we should probably (morally) believe it to be false because the immediate pursuit of happiness often creates the greatest unhappiness.

milieu as their context of assessment. With regard to their own milieu, both are right. Haslanger calls this position “Milieu Relativism” (Haslanger 2007: 81). Still, we can account for the feeling that the assessment made with respect to the girl’s milieu is illusionary by *criticizing* her milieu. Although Haslanger only sketches on which grounds such a criticism could be made, she believes that this solution is insofar satisfactory as we can claim that there is a sense in which both daughter and parents are correct, without creating a contradiction or ruling out further argument and critique.

i. Why Milieu relativism is not enough

Nevertheless, I believe Haslanger’s solution is not completely satisfactory. To see why, let us suppose we have succeeded in showing that the girl’s milieu is inferior to her parents’. The daughter would then be right in her belief in (P) only in a very weak sense: she can hold to it only by holding to an epistemically inferior milieu – something which does not seem like a reasonable option, thus she would eventually have to give up on (P) completely.

However, this weakens the daughter’s epistemic position too much. Haslanger’s initial justification for granting the daughter any right to believe that (P) was that she possesses “important social knowledge” as “social reality is organized around the cute/dork dichotomy”. But this fact supports (P) precisely because it is independent of the belief system shared by the milieu which created the “cute/dork dichotomy”: if categorizing girls in cute and dorky leads to a social structure in which these categories indeed apply, then this fact is true also with respect to the epistemically superior parents’ milieu, leaving us with the initial puzzle. It is not clear how milieu relativism would help out here. For this reason, I believe that it cannot account for everything that is puzzling about our attitudes towards (P) and fails as a solution.

III. AN ALTERNATIVE SOLUTION – THE DIFFERENT COMPONENTS OF (P)

If my critique of milieu relativism is correct, we might want to consider other solutions. The starting point, again, is that the girl seems to be both right and wrong at the same time in believing that (P). It would obviously be a possible way out to show that (P) consists of different components, some of them correct, some of them false. Haslanger discusses this possibility in form of EMD and rejects it for good reasons. However, there might be ways of structuring the components of (P) that both resolve the paradox and withstand Haslanger’s critique of EMD. This is what I want to achieve in the following.

i. Social knowledge

First, I would like to consider in which way the girl possesses “important social knowledge”. This

is not clear from Haslanger's paper because the inference "If the social reality is organized around the cute/dork dichotomy, then there are cute girls and dorky girls" is never further explicated or justified.

What seems to be meant by social reality to be "organized around the cute/dork dichotomy", is that there are two categories of girls, referred to as being "cute" or "dorky" and that these girls behave and are treated in different ways (for example, "cute" girls are accepted, "dorky" girls are dismissed). As this behavior is observable (even if it might be subtle at times), an external observer of the girl's milieu could distinguish between these two categories of girls. This latter fact explains in which way there indeed *are* "cute" and "dorky" girls – independent of the observer's own milieu. I believe that one component of (P) relates to these considerations:

(I) There are two categories of girls, referred to as "cute" and "dorky" in the girl's milieu where the first wear crop-tops and the latter track suits.

That the girl is aware of these categories, and thus believes that (I), can be seen by her fear that she will be a dork if she wears a tracksuit instead of a crop-top (Haslanger 2007: 72). I suggest that it is by believing that (I) that the girl possesses "important social knowledge".

ii. The normative and aesthetic components of (P)

If the girl is correct about (I), then in which way is her believing that (P) illusory? I propose that there are two other important components of the proposition:

(II) Crop-tops *are* cute.

(III) Girls wearing crop-tops are superior to those wearing track suits.

The first refers to the girl's own *aesthetic* attitudes. She simply finds that crop-tops are cute. The second is a normative judgement that crop-tops also reveal something about the girl wearing them: as opposed to girls wearing track suits, which are dorky, girls wearing crop-tops are cute. This judgement is more than merely aesthetic: It is clearly value-laden. It is an evaluation of a girl's character and determines the attitude towards her (compare also Haslanger 2007: 72).

Now, the girl cannot be epistemically criticized for component (II): this component is the expression of an aesthetic attitude, which is indeed milieu-dependent. But it lacks the possibility of critique which Haslanger is looking for, which is that aesthetic attitudes are both socially constructed *and* verified, such that if a certain group of people shares an aesthetic attitude, this constitutes an aesthetic norm and with regard to this group everything which applies to this

norm is beautiful (or “cute”). There is no point in saying that a group is aesthetically wrong about its norms: “De gustibus non est disputandum”.

This leaves component (III) as the objective of epistemic criticism. And indeed I suggest that the normative dimension of the girl's conviction are what can be best considered as illusory. This fits to an observation made by Haslanger when comparing the disagreement over (P) and a possible, similar disagreement where the daughter states: “But Mom/Dad, the girls who wear track suits to school are all on the track team.” (Haslanger 2007: 74). In the latter case, Haslanger plausibly asserts, we could not make much sense of a parents' reply “But sweetie, you won't be on the track team if you wear a track suit.” If we make the distinction between components (I)-(III), we can easily explain this. As opposed to the disagreement over (P), in this disagreement component (III), which is the component of (P) that constitutes an illusion, is missing.

iii. How (P) is illusory

There is, I suggest, a simple way in which (III) makes (P) illusory: (III) is false. Contrary to the girl's beliefs, girls wearing track suits are not inferior to those wearing crop-tops. We believe this not because of the aesthetic properties of crop-tops or track suits but because it is principally wrong to morally evaluate humans based on their outer appearance. It is in this sense in which the daughter should not believe that “seventh grade girls who wear track suits to school are dorks”.

This claim is unaffected by Haslanger's arguments against EMD. There is no “moral should” involved, the should is purely epistemic, it only applies to moral content.³ In this way the analysis of the different components of (P) as proposed here is different to EMD.

iv. How (P) manifests oppressive structures

It could be contested if the falsity of (III) is all that we find “illusory” about (P). What is missing in this account, it might be said, is that the daughter ignores how her belief manifests the oppressive social structure constituting the distinction between “cute” and “dorky” girls. I suggest that the daughter can indeed be criticized for this (as much as we can criticize 12-year-old girls for issues concerning social construction). But this critique would not be an epistemic one. On the contrary, it is at this point where we indeed get a moral “should”.

³ As becomes apparent, the analysis in this essay is built – as is Haslanger's paper (see, for example, p. 86) – on a cognitive understanding of morality, i.e., the position that moral statements can be true or false. Still, many non-cognitivist positions allow for moral statements to be true or false in light of a common ground that has been agreed upon. In case there is such a common ground for daughter and parents, the analysis could be meaningful from a non-cognitivist reading.

The daughter can manifest the cute/dorky structure in two ways: first, by having a certain aesthetic attitude and thereby contributing to the establishment of an aesthetic social norm, and second, by acting according to the norm, by publicly stating that girls wearing track suits are dorky, seeking only friends that wear crop-tops, advising her friends to adhere to the norm, etc.

The second point is obviously only a matter of moral considerations. But so is the first: contrary to beliefs, aesthetic attitudes are susceptible to *moral* criticism because they are, at least to some degree, a matter of choice. For example, if you like a certain joke and later realize that it is racist, it will not only change your normative attitude towards this joke but you will also be likely to find it less funny (“I cannot laugh about that”).

Similarly, for Haslanger one reason why the daughter should not believe that (P) is because believing (P) “will contribute to the patterns of beliefs and expectations that constitute the social fact that such girls are dorks, which would be bad.” (Haslanger 2007: 73). I propose to understand this, contrary to Haslanger’s own intention, as a morally based criticism of the girl’s aesthetic attitude (II).

CONCLUSION

To summarize, I claim that there is no epistemic problem in agreeing with the daughter’s parents and still granting the daughter to be right about something if we differentiate between the different components of (P): the daughter is right about the epistemic component (I), wrong about the normative judgement (III) and can be morally criticized for (II). As aesthetic attitudes can be changed, and there is something illusory about (P) in form of component (III), Haslanger’s objections to EMD are avoided.

Therefore, while Haslanger’s conceptual repertoire might be valuable for various purposes, it cannot serve to resolve an epistemic contradiction posed by the disagreement between the parents and the daughter - precisely for the reason that there is no such contradiction.

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IS ECONOMICS NORMAL SCIENCE: DO ECONOMISTS SHARE A PARADIGM?

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Abstract. Thomas Kuhn provided a scheme for the development of the natural sciences. According to Kuhn, this development is marked by periods of cumulative normal science on the one hand and non-cumulative turning points, so-called scientific revolutions, on the other. This essay is meant to answer the question as to whether economics matches the picture Kuhn draws of normal science. I argue that economics should in fact be considered normal science since it has acquired efficient ways of puzzle-solving and a high degree of professionalization. However, economics' paradigm, neoclassic theory, is not as widely shared as paradigms in the natural sciences. It seems that this divergence is due to more general differences between the social and the natural sciences.

I. INTRODUCTION

In *The Structure of Scientific Revolutions* Kuhn provided a scheme for the development of the natural sciences. According to Kuhn, this development is marked by periods of cumulative 'normal science' on the one hand and non-cumulative turning points, so-called scientific revolutions, on the other.

This essay is meant to answer the question as to whether economics¹ matches the picture Kuhn draws of normal science. Normal science is research firmly based on one or more paradigms. Thus, if economics is normal science depends on whether economists share a paradigm. I argue that economics should in fact be considered normal science. Neoclassic theory should be regarded economists' paradigm, even though not all researchers are equally committed to it. It seems that this lack of consensus is due to the special properties of the social sciences as opposed to the natural sciences.

The argument proceeds in two steps. Section 2 tackles the question of whether Kuhn's scheme, initially meant to describe the development of the natural sciences, can be applied to economics. I argue that such an application is possible. Section 3 investigates economic science itself in order to determine if it meets Kuhn's notion of normal science. Considering the dominant status of neoclassic theory within economics and the resulting efficiency of puzzle-solving and high degree of professionalisation, I argue that economic science can indeed be regarded as normal science and that neoclassic theory is its paradigm. Section 4 concludes.

¹ It should be noted, however, that this essay focuses on microeconomics. Even though I believe that extending the question to macroeconomics would not change the answer fundamentally, it would clearly exceed the boundaries of this paper.

II. THE CASE FOR THE APPLICABILITY OF KUHNIAN TERMS

Before I turn to the question as to whether economics matches Kuhn's notion of normal science, another related, but more fundamental question has to be considered: to what extent is such an application of Kuhn's conception to economics, being a social science, even possible?

It might be doubted that the two questions can and should be dealt with separately. However, what we have to keep in mind is that a discipline can in principle match the picture Kuhn draws of the historical development of a science, but is currently not firmly governed by a paradigm and thus is not in the phase of normal science. Such a discipline could currently be in the pre-paradigmatic phase, i.e. approaching but not yet having reached the state of a mature science, or in the revolutionary phase, i.e. a time in which the recent paradigm has been abandoned by the scientific community but not yet replaced with a new one. Also, it might be the case that an application of Kuhnian terminology to economics is impossible in principle (e.g. economics might intrinsically be too different from the natural sciences), in which case the search for a paradigm guiding economic science would be futile.

Redman (1991: 151) contends that "none of the Kuhnian terminology is applicable" to economics. I will evaluate her arguments and argue that she overemphasizes the differences between economics and the natural sciences and thus fails to show that the application of Kuhnian terminology to economics is impossible.

First, Redman (1991: 151) notes that Kuhn in fact developed his conception of the structure of scientific revolutions in order to point out the major difference between the natural and the social sciences. She is certainly right in noting that Kuhn observed controversies about the very foundations of research in the social sciences which hardly occur in the natural sciences. However, it does not follow that Kuhnian terms can indeed never be applied to any of the social sciences.

Second, she argues that not even the Kuhnian term *preparadigmatic* should be applied to economics, since a social science cannot be expected to achieve something similar to the paradigms of physics that guide research and allow scientists to utter precise predictions (Redman 1991: 151). Again, she is perfectly right in noting that economics cannot be expected to be as precise and successful at prediction as the physical sciences for several reasons (cf. Rosenberg 1994, Rosenberg 2009). However, it does not follow that the attempt to find something similar to paradigms and revolutions in economics must be a fruitless enterprise. Kuhn himself did never claim that in every field of science the paradigm should lead to predictions as precise and successful as the ones of physics. In the *Postscript to The Structure* he contends that values concerning the accuracy, precision and quantifiability of predictions are usually part of a paradigm. He points out that

these “should be consistently satisfied in a given field” (Kuhn 1996: 185), but does not demand that they be satisfied consistently across all fields.

Third, Redman (1991: 151) argues that, while Kuhnian revolutions are usually put forward by young scientists or people who are new to the field, “an argument that the younger economists are better than the older ones necessarily stretches the imaginations” considering the amount of study that is necessary in order to become a good economist. However, even if Redman was right in stating that there is a difference between the natural sciences and economics in this respect, this would not be of sufficient significance to show that Kuhnian terms are not applicable to economics. Kuhn merely mentions that revolutionary scientists usually happen to be young because younger scientists are less committed to the paradigm, since they have spent less time researching within it (Kuhn 1996: 144). Hence, this property of revolutions and revolutionary scientists is an accidental rather than a necessary one.

Finally, Redman (1991: 151) argues that the Kuhnian terms *normal* and *revolutionary* should not be used to describe economics since change in economic thought is different to change in the natural sciences in two respects. First, theory change in economics is linked to political considerations. Second, a model in economics is usually not replaced but slightly modified. Both claims might be correct, but none of them is an argument sufficiently persuasive to show that Kuhn’s conception is not applicable to economics.

First, with regard to the presumed difference concerning political considerations, it should be noted that Kuhn (1996: 69) himself mentions that in mature sciences “external factors [...] are principally significant in determining the timing of breakdown, the ease with which it can be recognized, and the area in which, because it is given particular attention, the breakdown first occurs”. There is no obvious reason why external factors having impact on scientific revolutions should not include political considerations.

Second, her claim that economic models are slightly modified rather than replaced also fails to establish that economic science is too different from the natural sciences as to render an application of Kuhnian terminology to economics impossible. One of Kuhn’s central theses is that there are not only large-scale revolutions, but smaller ones as well (Kuhn 1996: 6 and 49). So a change in theory does not have to be a dramatic event similar in scale to the Copernican revolution in order to be regarded as a Kuhnian scientific revolution. Furthermore, an anomaly (e.g. the discovery of a new phenomenon) does not necessarily have to cause a revolution, but may be solved within the paradigm or by adjusting the paradigm’s categories without replacing it completely (Kuhn 1996: 52-66). Therefore, even if such small changes of theory in economics are not to be regarded as scientific revolutions, it still does not follow that Kuhn’s concepts do not apply.

In this section, I hope to have shown that Redman's argument fail to establish the inapplicability of Kuhnian terms to economics. Hence, I suggest having a closer look at the field in order to determine whether there is any such thing as a paradigm that would indicate that economics actually is to be regarded as normal science.

However, in search of a paradigm in economics, one should bear in mind some particular properties of economics as a social science. First, as Redman correctly notes, a paradigm in economics cannot be expected to be as successful and precise at prediction as the ones of the natural sciences. Second, it is to be expected that consensus in economics will not be as strong and unequivocal as in the natural sciences. Proper experiments for the test of economic theory are seldom available and, thus, economic theory is even less determined by observation than physical theory (cf. Friedman 1966). Finally, as both Redman (1991: 151) and De Vroey (1975) observe, factors external to pure scientific enterprise, such as political and social factors, may play a more important role in economics than in the natural sciences. Although these special properties, as I argued above, do not necessarily render the application of Kuhnian concepts impossible, they indicate that one should be careful to view economics as the perfect match for Kuhn's conception.

III. WHY ECONOMICS IS NORMAL SCIENCE

In the previous section I have argued that an application of Kuhn's terms to economic science is possible. In this section I turn to the question entitling this essay: Is economics normal science? I argue that economics should in fact be considered normal science. Neoclassic theory should be regarded as economists' paradigm, even though not all researchers are equally committed to it. It seems that this lack of consensus is due to the special properties of the social sciences as opposed to the natural sciences.

Normal science, according to Kuhn (1996: 10), is firmly based on one or more paradigms, "achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice". A paradigm supplies problems it renders worthy of research, promises that these problems are soluble and at the same time provides strict rules regulating what their solution should look like. Most of normal scientific research consists in solving such puzzles and is not at all aimed at the discovery of major novelties, let alone the introduction of new theories. Paradigms enable scientific communities to engage in highly precise, progressive and efficient research that would not be possible if it was not for its strict guidance. Hence, a scientific community is in a phase of normal science if and only if its members share a paradigm.

But what does Kuhn's notion of the paradigm entail? What is it that scientists commit to when they commit to a paradigm? Unfortunately, Kuhn's concept of the paradigm is somewhat vague.

In the 1969 *Postscript* he admits to have used the term in different ways and consequently tries to clarify the concept. In *The Structure* as well as in the *Postscript* he mentions certain features that paradigms have in common.

First, it seems safe to say that a paradigm equips its scientific community with an own language. This language is not known to the layman, but only to the particular scientific community (Kuhn 1996: 20). It often includes symbolisations and definitions which are taken to be tautologies (Kuhn 1996: 183-84). This particular feature of paradigms enables a very efficient way of puzzle solving by allowing scientists to state problems in a familiar language such that they know how to approach them.

Second, a paradigm usually supplies certain metaphysic views of what entities the world consists in and how they are interrelated (Kuhn 1996: 184). Such a metaphysic guides research by providing a pattern for puzzles that can be solved within the paradigm. Kuhn's claim is that a scientist does not know what to look for, unless she is guided by an overarching theoretical framework that dictates what entities there are. Thus, this feature of the paradigm is crucial in enabling efficient puzzle-solving. Commitment to such a metaphysic can also be described as commitment to certain models (Kuhn 1996: 184).

Third, a paradigm usually gives rise to certain values and norms of accuracy (Kuhn 1996: 185). Specifically, a Kuhnian paradigm does not only supply certain puzzles to be solved, but also dictates what the solution should look like (Kuhn 1996: 38). Thus, a scientist's puzzle-solving attempts will only be accepted by the community if they accord with the paradigm's values and standards of method.

As a result of these features a paradigm enables a high degree of professionalisation. If it was not for the paradigm, scientists would constantly be arguing about the foundations of their field and therefore address competing scientists in order to convince them of their approaches (Kuhn 1996: 12-13). In contrast, once a paradigm is taken for granted highly specific literature addressing only the initiated scientific community arises. Researchers that do not accept the paradigm are ignored and no longer regarded as participants in the field (Kuhn 1996: 20). While the more advanced literature in the field rests on the paradigm, the paradigm itself is usually taught to students of the field through textbooks (Kuhn 1996: 10).

If economic science has a paradigm, what is it? Some authors argue that the dominant paradigm did not change throughout the history of the discipline and consists in the "theory of economic equilibrium via the market mechanism" (Coats 1969: 292). Others identify the marginalist revolution as a paradigm change and therefore distinguish between a classic and a neoclassic paradigm (cf. De Vroey 1975, Jalladeau 1978).

The bottom line is that the basic theory, assumptions and methods of the so-called neoclassic school, may they be the same as the classic's or not, are often identified as a Kuhnian paradigm (cf. Coats 1969, De Vroey 1975, Jalladeau 1978, Rosenberg 2009, Solo 1991). But what does neoclassic theory entail? Joël Jalladeau (1978: 601) outlines it as follows:

“The neoclassical model is an analytical system based upon subjective choices of individual economic agents. It is a network of exchange relationships linking separate primary units. The theoretical effort involves the concept of economic equilibrium, which means the economy is an interdependent system leading to a stable equilibrium, and it is the conditions by which this stability is realized that are studied.”

Neoclassic economics, I take it, establishes what might be referred to as “mainstream economics”² (Solo 1991: 39), which is represented at most of the world's universities. But does neoclassic theory really supply the epistemological and sociological features of a Kuhnian paradigm?

First, with regard to the proprietary language Kuhn takes a paradigm to provide its scientific community with, Rosenberg (2009: 55) argues that neoclassic economic theory has a language of its own, one that is highly mathematical. Before one can really understand the models of mainstream economics, one has to acquire an education in advanced mathematics, thus learning economics' language (Rosenberg 2009: 55, Solo 1991: 40). The mathematical apparatus of neoclassic theory enables mainstream economists to formulate problems in the discipline's language such that they come in a familiar structure. Once a problem is sufficiently formalized, they know how and where to look for a solution to the puzzle and what that solution should look like. The mathematic language of neoclassic theory thus clearly counts as a reason to consider it a paradigm.

Second, Rosenberg (2009: 56) contends that “the discipline has identifiable proprietary laws, albeit inexact ones, and a set of proprietary kinds”. What he refers to is the kind of metaphysical views that has been outlined above, although in the case of economics the formulation “beliefs in particular models” (Kuhn 1996: 184) seems more appropriate. Concepts such as supply, demand and equilibrium and their interrelations enable scientists to apply neoclassic theory to more and more puzzles, which leads to the phenomenon often referred to as “economic imperialism” (Rosenberg 2009: 56). Similarly, Friedman (1966: 7) refers to an “analytical filing system” that allows economists to subsume phenomena under fixed categories.

Third, with regards to paradigms' values of accuracy, a solution to an economic problem will

² In this essay the terms “neoclassic economics”, “the neoclassic school” and “mainstream economics” will be used interchangeably. Instead of “the basic assumptions, method and theory of neoclassic economics” I will often simply write “neoclassic theory”.

hardly be accepted unless it is sufficiently formalized and comes in the form of a mathematical proof (cf. Jalladeau 1978, Blaug 1984). Moreover, as Jalladeau (1978: 602) notes, economists are not only able to apply neoclassic concepts to problems, but they have to do so. Likewise, Rosenberg (2009: 56) speaks of “the discipline’s ‘discipline’” requiring that the proprietary kinds and laws of neoclassic theory be applied to the solution of puzzles.

Finally, economics is a highly professionalized discipline. To begin with, Rosenberg (2009: 55) emphasises the uniformity of the field’s largest selling textbooks. What is also striking about economics textbooks is that, apart from the fact that they are indeed very similar to each other, they supply the basic theory and assumptions of neoclassic economics. Mankiw’s best-selling textbook *Principles of Economics*, for instance, in its very introduction provides a catalogue of the so-called “Ten Principles of Economics” (Mankiw 2008: 3). They include statements, such as “Rational people think at the margin”, “Trade can make everyone better off” and “Markets are usually a good way to organize economic activity”, which clearly reflect the neoclassic research tradition. Moreover, it seems that neoclassic economists often equate neoclassic thought with economists’ way of thinking in general and, thus, do not even regard writers that are not committed to neoclassic theory as economists. Again, consider Mankiw’s *Principles* implicitly regarding something as the principles of all economics which, say, a Marxist would object to. Finally, neoclassic economists also do not write books addressing competing schools trying to convince them of their research foundations. By contrast, there is a huge apparatus of highly specific journals for mainstream economics (Redman 1991: 163).

Apparently, neoclassic theory has striking features of a Kuhnian paradigm. However, one could object that not all economists are neoclassic economists. Redman (1991: 151) argues that “there is no paradigm [...] that is unquestioned by all economists.” As Coats (1969: 292) notes, there has always been and still is a variety of heterodox schools of economic thought including, for instance, socialist or institutionalist approaches. Considering that Kuhn (1996: 170) denied the possibility of several coexisting paradigms in a mature science that has crossed the border from the pre-paradigm stage to normal science, from the variety of schools in economics one may be tempted to conclude that neoclassic economics is not in a phase of normal science.

However, Solo (1991: 39) argues that the neoclassic school of economics has reached a far wider scope than any other social science or alternative approach to economics. Considering the differences between the social and the natural sciences that have been outlined in the previous section it was to be expected that consensus would not be as strong in economics as in the sciences Kuhn initially described. It might be that the consensus and scope that neoclassic theory has reached in economics is the status closest to that of natural scientific paradigms a theory in the social sciences can possibly reach. Hence, neoclassic theory might have reached some sort of a paradigm-like status tailored to the social sciences.

Furthermore, since Kuhnian terms may be applied to economics, we may raise the question of which term suits best. Now, if economics was not normal science, according to Kuhn's scheme, we would have to regard it either as an intellectual discipline in the pre-paradigmatic stage or as a science in the middle of a revolution. The latter cannot be the case since the heterodox schools competing with neoclassic economics did not just pop up, but partly are even older than the latter and never replaced it (Coats 1969: 292). Considering neoclassic economics' efficient ways of puzzle solving and high degree of professionalisation it does not seem plausible that economics should be in the pre-paradigmatic stage either. So, if economics is neither a field of discovery in the pre-paradigm stage, nor a once paradigm-based science in a revolutionary phase, it should be regarded as being in the stage of paradigm-based normal science, although there are heterodox schools.

In this section I have argued that economics should in fact be regarded as normal science. Neoclassic theory provides economists with their own mathematic language, proprietary concepts and laws and values of accuracy. Thereby it enables highly efficient puzzle-solving and a high degree of professionalisation. Thus, although Redman is partly right in stating that there is no paradigm that all economists take for granted, there are good reasons to consider neoclassic theory a paradigm. The fact that consensus in economics is weaker than in the natural sciences seems to be grounded in the very nature of social sciences.

IV. CONCLUSION

In section 2, I have argued that the application of Kuhnian terms to economics is possible, even though these terms were not designed to describe the social sciences. Certainly there are differences between economics and the natural sciences that should be taken into account, but this is not a sufficient reason to give up on the enterprise completely.

In section 3, I turned to the question if economics is in fact normal science. It turned out that if economics is normal science there should be a paradigm that economists take for granted. I argued that neoclassic theory should be regarded as economics' paradigm, since it provides economists with an own, mathematical language, proprietary concepts and laws, and values of accuracy. Since these features enable efficient ways of puzzle-solving and a high degree of professionalisation, economics should be regarded as normal science. It seems that the fact that not all researchers in the field are committed to the neoclassic paradigm is due to the very nature of the social sciences.

Therefore, economics should be regarded as normal science, even though its paradigm is not as widely shared as the paradigms in the natural sciences.

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MARKETS, MATHS AND VALUE: SMITH VERSUS JEVONS

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Abstract. Can market prices misrepresent the value of commodities? I address this question from a historical perspective, comparing the views of Adam Smith and William Stanley Jevons. Smith held that the value of a unit of labor was, in some sense, objective, and could be used to approximate the true value of a commodity. Since market prices do not always track this value, and since we always face uncertainty when determining whether or not they do, there is reason to be skeptical that market prices accurately represent the value of commodities. In contrast, Jevons defines value as a ratio of utilities that only has meaning in a market context. This definition increases the precision with which Jevons can formulate his economic theory, but it precludes any possibility that market prices could misrepresent the value of a commodity. I encourage the reader to question the wisdom of granting markets this infallibility.

INTRODUCTION

Piety, Euthyphro contends, is that which is dear to the gods, and impiety is that which is not dear to them. Well put, Socrates replies, but do the gods love that which is pious simply because it is so? Or is it pious because the gods love it?

Markets are today's fickle gods and we are still caught on the horns of Plato's ancient dilemma. Do markets generate prices determined by the value of the goods and services traded? Or is the value of goods and services determined by market prices? Modern oracles of economic science have embraced the second alternative and developed an increasingly technical theoretical apparatus on its foundation. Yet devastating housing bubbles and debt crises still elude analysis, suggesting perhaps that we – both consumers and economists, devotees and priests – have more to learn about what value is and how it is reflected in market prices. As a preliminary investigation, I propose a historical exposition of this quandary as it surfaces in the comparison of classical and neo-classical economic reasoning about exchange value, as embodied in the work of Adam Smith and William Stanley Jevons.

In order to circumvent a stilted rehearsal of Econ 101 and a series of largely irrelevant qualifications, I allow myself the oversimplification that Smith and Jevons have an approximately similar understanding of market dynamics. Scarcity and desire, production and consumption, supply and demand interactively determine the market price of commodities. Granting this relatively common ground, the present aim is to contrast their respective interpretations of the

value represented by that market price and its roots in their divergent etiologies of individuals' commercial choices and actions. By stipulating an objective measuring rod, Smith's labor theory of value enables his now antiquated distinction between a good's market price and its natural price. The natural price is the true value of the good and markets can sometimes get it wrong. In contrast, Jevons' pioneering mathematical analysis of utility commits him to a view of markets as infallible indicators of commodities' true worth.

In the first section of the paper I am concerned to show that a Smithian market price can misrepresent the exchange value of a commodity. Such misrepresentation is made possible by a definition of exchange value that is practically unmeasurable but completely independent of a market context. In the second section I argue that a Jevonsonian market price cannot misrepresent the exchange value of a commodity. This infallibility is a logical consequence of Jevons' utilitarian definition of exchange value, a definition which he explains is absolutely necessary if the analysis of human wants and desires is to be made precise and scientific. I could not say if this increase in precision is worth the loss of a healthy skepticism about market prices; I argue here only that such an exchange has been made. Whether the trade-off was worthwhile is undoubtedly a question of faith.

I. SMITH AND THE LABOR THEORY OF VALUE

After explaining the causes and character of the division of labor and discussing the origin of money, Smith (1776) sets out three distinctions in Book I of *The Wealth of Nations*: use value versus exchange value, real price versus nominal price, and natural prices versus market prices. Fleshing out the meaning and implications of these distinctions will clarify the sense in which a market price can misrepresent the value of a commodity.

i. Use value versus Exchange value

Use value denotes what Smith calls the utility of an object – simply, its usefulness. Exchange value denotes the purchasing power of an object, that is, how much can be gotten in exchange for it. Smith does not posit or explain any relationship between these first two types of value. Citing water as highly useful but hardly exchangeable and diamonds as hardly useful but highly exchangeable, he remarks that these different values are frequently incommensurate, but there is no necessary relationship between them. He raises the distinction mainly to focus his ensuing exposition on the proper measure, components and determinants of exchange value. If he does conceive a relationship between value in use and value in exchange, he does not mention it in the canonical water/diamonds example (Smith 1776: I.4.13). Later on we will see how Jevons' mathematization of utility enables him to pin down and justify a necessary relationship between these conceptions of value.

The exchange value of any commodity, Smith (1776: I.5.1) writes, “is equal to the quantity of

labour which it enables him to purchase or command.” This is so for two reasons. First, it is, for Smith, a self-evident premise that every person is better off or worse off to the extent that she or he “can afford to enjoy the necessities, conveniences, and amusements of human life.” Successful pursuit and enjoyment of these goods just *is* valuable; it is constitutive of wealth. Second, these goods are procured or achieved through labor, either one’s own or, especially in a developed commercial society, someone else’s. The more labor at one’s disposal, the wealthier one is. Whenever a commodity is purchased with money or bartered in exchange for other goods, the items exchanged represent units of labor saved, units that would have been otherwise expended in pursuit of necessities, conveniences and amusements. These units of labor saved may or may not be roughly equal to the units of labor needed to produce the acquired commodity and bring it to market, referred to by later value theorists as the ‘labor embodied’ in the commodity. Assuming the equality of these amounts of labor, however, will simplify the exposition of Smith’s two remaining distinctions, so I leave the separation of labor-commanded from labor-embodied to a more Prodician commentator.¹

For our purposes, Smith (1776: I.5.2) holds simply that commodities derive their exchange value from labor because “[t]he real price of every thing, what every thing really costs to the man who wants to acquire it, is the toil and trouble of acquiring it”. That is to say, exchange value measures what you have to give up in order to get something, and in almost all cases what you will be giving up is labor. It certainly seems then that a ‘unit of labor’ is a reasonable unit in which to express measurement of exchange value. Exactly how to standardize this unit, however, is a seriously intractable problem, as we will see in the next sub-section.

ii. Real price versus Nominal price

The real price, denoted in units of labor, stands in contrast to the nominal price of a commodity, denoted in units of money. Real prices, as defined in the last paragraph, are, by that definition, accurate representations of exchange value.

Nominal prices are not. Whether due to the changing availability of precious metals in Smith’s day or to the fluctuating credibility of sovereign states in our own, money is “a commodity which is itself continually varying in its own value, [and thus] can never be an accurate measure of the value of other commodities.” The real versus nominal distinction hereby warrants our attention because it provokes Smith to his starkest declaration of the objective, immutable value of human labor, which I quote at length for its significance. He proclaims:

“Equal quantities of labour, at all times and places, may be said to be of equal value to the labourer. In his ordinary state of health, strength and spirits; in the ordinary degree of his skill

¹ Prodicus was an ancient Greek sophist caricatured by Plato as one inclined to make fine distinctions between terms very close in meaning.

and dexterity, he must always lay down the same portion of his ease, his liberty, and his happiness. The price which he pays must always be the same, whatever may be the quantity of goods which he receives in return for it. Of these, indeed, it may sometimes purchase a greater and sometimes a smaller quantity; but it is their value which varies, not that of the labour which purchases them. At all times and places that is dear which it is difficult to come at, or which it costs much labour to acquire; and that cheap which is to be had easily, or with very little labour. Labour alone, therefore, never varying in its own value, is alone the ultimate and real standard by which the value of all commodities can at all times and places be estimated and compared. It is their real price; money is their nominal price only” (1776: I.5.7).

So not only is the exchange value of a commodity equal to the labor it places at the disposal of its owner (i.e., saves her from expending in pursuit of wealth), but one unit of that labor is, to the laborer, worth the same at all times and places. Labor is, in this qualified sense, an objective, consistent metric of the value of commodities.

Certain qualifications are appropriate because, even granting that the value of labor to a single laborer may be equal across time and space, the value of labor may vary across laborers and will almost certainly not match the value attached to labor by those who demand it (i.e., firms and other employers). Putting aside the issue of interpersonal comparisons, note that employers value labor for what they can exchange it for or produce with it. This is the real price of labor and it will vary, as Smith (1776: I.5.15) points out, with the value of the other goods on the market and the availability and quality of productive capital. On top of this and his later division of labor into productive and unproductive categories, Smith (1776: I.6.3) also readily admits that species of labor requiring “an uncommon degree of dexterity and ingenuity,” or “superior hardship and superior skill,” are naturally more highly prized than easier, more common exertions.

So while a unit of labor would perfectly measure the exchange value of a good if such a unit were available, there are, for Smith, at least two barriers to any specification and valuation of the unit based on real-world observations. First, it is impossible to standardize a quantity of labor expended that can consistently apply to laborers of differing skill levels working at varying levels of intensity. A ‘man-hour’ is a commonly heard unit of labor that blatantly fails to capture either of these sources of variation. Perhaps one could speak of an hour’s labor by a college graduate with such-and-such IQ working at full intensity, but even these controls seem drastically inadequate to capture the constancy to be sought in an appropriate unit. After all, one person’s ‘full intensity’ is another’s half-hearted lackluster. The second barrier to the specification of a unit of labor as a metric of exchange value is that, even if the quantity could be appropriately standardized somehow, the value of that standardized quantity would be unknowable. The only way the exchange value of something can be specified is by noting how much of another good would be exchanged for it. If the values of all other goods, other than labor, are always changing across time and locale, then it will be impossible to identify a constant value of a unit of labor

(even if the quantity of labor is pinned down), since at one time the unit of labor will exchange for, say, two bushels of wheat, at another for four, and at another for some commodity completely incommensurable with wheat.

With these considerations in mind, it is clear that the value of a unit of labor is, for all practical purposes, unmeasurable. Nonetheless, Smith's avowed commitment to the intertemporal and interspatial objectivity of the value of labor to the laborer herself, in combination with his reference to labor as "the only universal, as well as the only accurate measure of value," (Smith 1776: I.5.17) justifies attributing to Smith a belief in the objective reality of a good's value. He must think there is a, perhaps unknowable, truth of the matter, otherwise it would be meaningless to speak of the accuracy of any measure. While "the exchangeable value of every commodity is ... estimated by the quantity of money [or] by the quantity either of labour or of any other commodity which can be had in exchange for it," such an estimate is merely that. An estimate. The real value being estimated is something else entirely, less well-defined, more abstract and more peripheral to Smith's concrete explanations of the nature and causes of wealth. It is likely that he treats the reality of the value of labor much the way he treats that of moral virtue in *The Theory of Moral Sentiments*, namely, by leaving it unaddressed. V.M. Hope (1989: 84), commenting on Smith's moral ontology, notes his "careful avoidance of the topic." Smith "is anxious not to take issue with whether virtue is in the mind of the critic or the external world," Hope writes, but "he would not dream of saying that moral propriety and excellence have no external reality." I think the same can be said of his view of the value of labor.

iii. Natural prices versus Market prices

The final distinction, between natural prices and market prices, highlights the discrepancy between the cost of producing a good and bringing it to market and the amount that consumers are willing to pay for it once it is there. Smith posits that in an advanced economy in which all the land has been appropriated, the labor-commanding value of any commodity is composed of contributions from the land, labor and capital needed to produce and distribute it. He explains that each of these contributions will earn a natural rate, determined by the sophistication of the economy, the type of commodity and the relations between owners of these factors. The natural rate of each factor ideally corresponds to the value of the labor it commands on its own. Smith continues:

"When the price of any commodity is neither more nor less than what is sufficient to pay the rent of the land, the wages of the labour, and the profits of the stock employed in raising, preparing, and bringing it to market, according to their natural rates, the commodity is then sold for what may be called its natural price" (1776: I.7.4).

The commodity is then sold, he says, "precisely for what it is worth." The market price at which it is actually sold, however, reflects its scarcity and the intensity of consumer desire for it. This

market price will tend to “gravitate” towards the natural price, but various accidents affecting supply and demand will oftentimes prevent the market from accurately valuing the commodity (Smith 1776: I.7.15).

II. JEVONS AND MARGINAL UTILITY

In the very first sentence of the Introduction to his *Theory of Political Economy* Jevons has already designated his subject matter a scientific one. By way of explanation he says, “It is clear that Economics, if it is to be a science at all, must be a mathematical science,” for the simple reason that it deals with quantities (Jevons 1871: 3). Whether expressed in words or mathematical symbols, economic reasoning entails comparisons between greater and lesser quantities of goods, capital, people, labor, wealth and, crucially, pleasures and pains.

These last two quantities are crucial because, for Jevons, Economics is not just a science, it is a moral science. That is, it concerns itself with principles of human choice, action and interaction – specifically, of course, commercial interaction. His theory of choice and action is thoroughly utilitarian, meaning that a comparison of the pleasures and pains expected from a course of action is the sole motivational factor behind it. Following Jeremy Bentham, Jevons (1871: 23) writes that, “pleasure and pain include all the forces which drive us to action. They are explicitly or implicitly the matter of all our calculations, and form the ultimate quantities to be treated in all the moral sciences.” This is not to deny the existence of nobler, “higher” motives of uprightness, honor or duty; however, as a “mechanics of utility and self-interest,” Jevons’ theory treats only “the lowest rank of feelings [aimed] at supplying the ordinary wants of man at the least cost of labour” (Jevons 1924: 27). Because his theory of value and exchange is ultimately grounded in this utilitarian behavioral framework, it is worth examining it a bit more closely.

To emphasize his intended meaning of the word *utility*, Jevons (1924: 43) explains that it is a quality or “circumstance of things arising out of their relation to man’s requirements.” “Requirements” should here be understood in the broadest possible sense, encompassing or coincident with Smith’s “necessaries, conveniences, and amusements.” The important point is that utility is not an intrinsic quality of a thing, but a relational quality. Specifically, it is a quality that commodities acquire when they bear a certain relation to “the will or inclination of the person immediately concerned” (Jevons 1924: 39). Jevons (1924: 38-39) extols Bentham’s seminal specification of that relation as one that “perfectly expresses the meaning of the word [utility] in Economics,” and quotes him as saying:

“By utility is meant that property in any object, whereby it tends to produce benefit, advantage, pleasure, good, or happiness (all this, in the present case, comes to the same thing), or (what comes again to the same thing) to prevent the happening of mischief, pain, evil, or unhappiness to the party whose interest is considered.”

The strength of any commodity's tendency to promote happiness (i.e. its amount utility) has a magnitude that Jevons (1924: 47) treats "as a quantity of two dimensions, one dimension consisting in the quantity of the commodity, and another in the intensity of the effect produced upon the consumer." It is his analysis of this quantity by means of differential calculus that marks Jevons' unique and catalytic contribution to the sea change in economic reasoning historically known as the marginal revolution. A brief explication of this analysis will help clarify Jevons' conception of value as it relates to Smith's two notions of value in use and value in exchange.

As a consumer acquires more and more of a commodity, one dimension of utility is increasing (the quantity possessed) and the other is decreasing (the intensity of the consumer's desire for more) since most things lose their usefulness or appeal in overabundance. This phenomenon, known today as decreasing marginal utility, is represented in Fig.1 (with quantity on the x-axis and intensity of desire on the y-axis) and helps Jevons illustrate three kinds of value where Smith saw only two. The "Total Utility" experienced by a consumer as result of his possessions, represented by the total area under the curve, is equivalent to Smith's value in use. This total utility, or value in use, of a commodity, however, is not very helpful for explaining behavior. The economist really wants to know how a person's utility depends on very small additions or subtractions of the commodity, since this information ostensibly enables the moral scientist to know whether the person will want more or less of it. But the way in which utility (U) varies with an infinitesimally small change in the quantity of commodity (X), can be identified, Jevons explains, by expressing U as a function of X and then taking the derivative of that function with respect to X . The "degree of utility of the last addition, or the next possible addition of a very small, or infinitely small, quantity to the existing stock," is represented by the line nq and dubbed by Jevons (1924: 51) the "Final Degree of Utility." This is what economists today call marginal utility. The ratio of the marginal utilities of two commodities, Jevons demonstrates, is equal to the inverse of their ratio of exchange, that is, the amount one commodity exchanged for one unit of another. It is this ratio, what Smith referred to imprecisely as exchange value, that Jevons holds to be the proper representation of the value of a commodity. He insists that "the word Value, so far as it can be correctly used, merely expresses the circumstance of [a good's] exchanging in a certain ratio for some other substance" (Jevons 1924: 77). Value derives from a ratio of utilities.

To justify his treatment of utility as a quantity amenable to measurement, ratios and mathematical analysis, Jevons (1924: 9-10) requires that observable economic behavior accurately reveal the true pains and pleasures of market participants. While "there can be no doubt that pleasure, pain, [and] utility, ... are all notions admitting of quantity", he reasonably doubts the possibility of exact or direct measurement of these subjective feelings. The only way they can be quantified is by comparison with each other in the mind of a single individual, as when the pain endured by forfeiting the cost of an additional loaf of bread, for instance, is compared to the pleasure of consuming it. Even then measurement of these feelings is achieved only indirectly, by observing their effects on the voluntary "buying and selling, borrowing and lending, labouring and resting,

producing and consuming” (Jevons 1924: 11) they induce.

If such market behavior were potentially uninformative or misleading with regard to the individual’s preferences, then no inferences about the relative utility of various courses of action would be justified and utility would lose its claim to quantifiability. In order to defend this claim, and the integrity of Economics as a mathematical science, Jevons (1924: 13) pronounces that “we cannot make a choice, or manifest the will in any way, without indicating thereby an excess of pleasure in some direction.” In other words, he assumes economic behavior always reveals the truth about pains and pleasures.

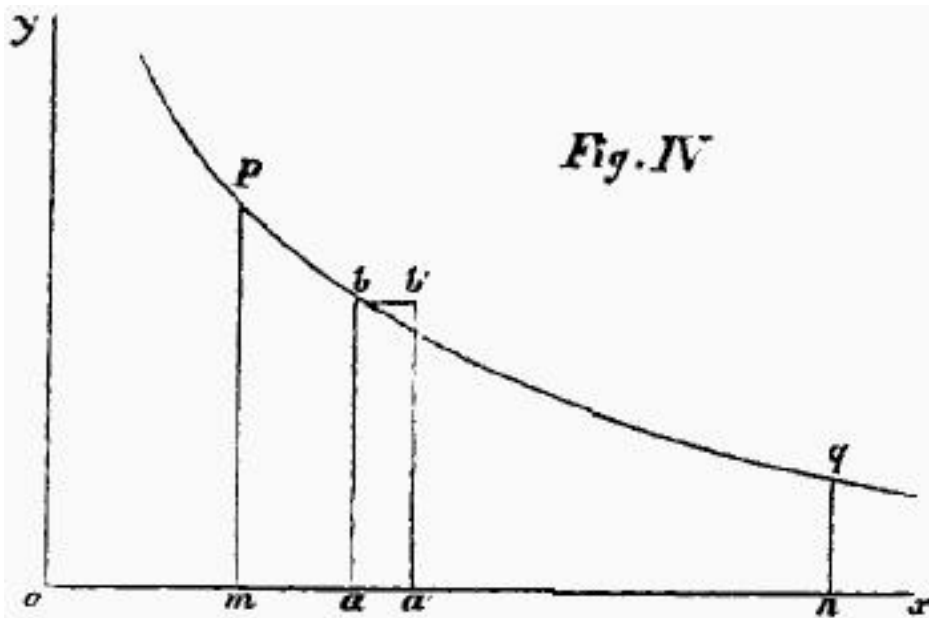


Fig. 1 The law of variation of the degree of utility (Jevons, 1871)

Essential as this pronouncement is for the validity of his quantitative treatment of utility, it has the further consequence that the price of a good in a perfect market cannot possibly misrepresent the value of that good. Market prices, arising from the aggregated behavior of well-informed buyers

and sellers, do not estimate an exogenously determined worth of the exchanged commodity. Instead, the worth of a commodity has become inseparable from the esteem attached to it by the trading parties. More than inseparable, its worth is *defined* by the intensity of that esteem or need; Jevons’ marginal utility analysis of market equilibria premises that valuable things are, by definition, the things that we want. Moreover, they are valuable because we want them and are more or less so in accordance with the strength or weakness of our desire. As simply a collective expression of this desire, market prices become an infallible indicator of worth.

CONCLUSION

I suspect that any earnestly attempted cost-benefit analysis of the mathematization of economic reasoning, would, for a variety of interesting but hardly germane reasons, soon become self-defeating and uninformative. Nonetheless, the preceding discussion has identified at least one apparent pro and con of mathematization. To Jevons' credit, his differential calculus of utility enables him to uncover three conceptions of value where Smith saw only two. Moreover, his mathematical notation deftly characterizes the precise relationship between these conceptions; namely, between a commodity's total utility, marginal or final degree of utility and ratio of exchange. This characterization would serve as a reference point, if not a stepping stone, for subsequent theorists conducting similar marginal analyses of other important economic concepts, such as productivity or cost. Jevons pioneered a method of reasoning that now undergirds almost all of economic thought.

To achieve such precision, however, and simultaneously maintain that his quanta of utility represent some actual human motive, rather than a merely handy construction, Jevons identifies market behavior with an excess of pleasure over pain in the mind of the consumer. This identification, combined with the stipulation that the value of a commodity is most correctly conceived as its ratio of exchange with another, leaves no room for thought of a true value other than that indicated by the market. Whereas Smith's market price was kept from converging to the good's true, labor-commanding value by the scarcity of the good or the intensity of consumer desire for it, Jevons' market price *is* the true value of the good since there is no other available measure of its worth. Smith was willing to countenance the value of a good as an objective but practically unmeasurable reality. This enabled a cautious skepticism about the accuracy of market prices, since whether they truly represent a good's real value is a question plagued by the irresolvable uncertainty of measurement error. While this particular uncertainty was exorcised by marginal utility analysis in the name of scientific integrity, the cost of cutting that aporetic tension could be as large as it is unquantifiable.

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WHAT DO WE OWE THE GLOBAL POOR?

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Abstract. Discourse on socioeconomic globalization has become unconceivable without calls for distributive justice. Among different philosophical accounts widespread allegiance is gained by the idea of humanitarianism. However, we ought to ask whether the poor's sufficiency to survive discharges the comparatively better-off from further redistributive obligations. This essay attempts to advance an answer to this question by investigating which grounds of justice appear plausible in the given context. First, I will reject purely relational accounts of redistributive justice, which – in light of the empirical facts – de facto exclude large parts of the global population from the realm of justice. Second, I will outline that distributive justice comprises relational and non-relational features. However, to bestow content on the former the preliminary condition of equal opportunity to participation in the socioeconomic world order has to be satisfied, the imposition of which then becomes the liability of the comparatively better-off.

I. INTRODUCTION

Our world has become a 'global village'. Yet, it is a village that is characterized by intruding inequality. While the most blessed enjoy safari trips in sub-Saharan Africa, high-technology toys manufactured in East Asia and indulge in Caribbean's exotic fruits, one fourth of the global population struggles to survive with less than \$1.25 a day (United Nations 2012). The question of justice imposes itself on everybody who does not turn a blind eye on these empirical facts. So, what do we owe the global poor?

First of all, the stylization of this question begs for clarification. Who are 'we' - the ones whose potential responsibility is called upon? What is 'poverty' anyway? I use 'we' to refer to all the comparatively better-off. Inversely, the term 'poor' shall include all those who are comparatively worse-off. One may ask whether these standards hold. Prominent concepts such as the UN Millenium Development Goals or Sustainable Development impressively show the versatility of poverty. Among others it encompasses issues of education, health, general living conditions, income, gender equality and environmental concerns. All of these issues are important and deserve treatment in their own right. Therefore, I will draw on the more general notion of relative socioeconomic deprivation, which directly and indirectly touches upon many of the concerns mentioned above. In this spirit, the question at hand may be rephrased as follows: what do the socioeconomically better-off owe the socioeconomically worse-off? Although this restatement eliminates some of the ambiguities of its predecessor, it remains almost prohibitively sketchy.

What goods exactly are socioeconomic in character? How does their simultaneous presence or individual absence impinge on the notion of ‘poverty’? According to which threshold is somebody better or worse off? These are reasonable questions, which nevertheless cannot be addressed here. I trust that the general understanding of the following exposition will not be inhibited by this imperfect specification, and I am convinced that its conclusions will remain unaffected by this inconvenience.

Second, it seems unreasonable to determine what the better-off owe the poor without a prior understanding of why they owe the poor anything at all. Therefore, this work will establish its conclusions by exploring the underlying features that justify a demand for global justice. In this spirit, humanitarianism serves as a starting point that gathers allegiance by most theories of distributive justice. Consequently, I will critically examine the internationalist account. After establishing its implausibility, a version of globalism will be outlined, according to which a sound account of socioeconomic justice must always comprise both, a relational and a non- relational component. Once furnished with a clear understanding of the grounds of justice in global socioeconomic matters, a dyad of obligations for the better-off will be formulated.

II. THE OVERARCHING CONSENSUS

Many people would argue that we should provide relief to large scale famines if we are in a position to do so. Similarly, many would claim that we have an obligation to save the life of another human being, unless an intolerable sacrifice would be imposed on us. From a theoretical perspective these intuitions for the need of humanitarian intervention can be substantiated using different routes. On the level of monist theories, the Kantian could call for the necessity of famine relief based on the belief that in absence of a disposition to beneficence, the principle of action failed the test of contradiction in the will (O’Neill 1998). Henceforth, only the maxim to help would be universalizable with regard to large scale famines. On the contrary, the utilitarian could derive the same conclusion from the principle of utility maximization, establishing a duty to help until the marginal utility gains of the helped are outweighed by the marginal utility losses of the helper (Singer 1972). A justification in the dualist tradition is outlined by Nagel (2005). Being faced with the constraints of the internationalist doctrine of justice (see section 3), he unceremoniously excludes the need for humanitarian intervention from the realm of justice. According to his version, humanitarian intervention merely reflects “a minimal concern” for fellow human beings, while it is not a requirement of justice as such.

Based on this shared understanding among different theories, one can draw the following intermediate conclusion regarding the question at hand: irrespective of any contingencies, human beings have a right to the basic goods, which are needed for their survival. In case these needs cannot be catered for auto-sufficiently, the necessary goods ought to be provided by those in a

position to do so – a duty naturally involving the better-off.

Yet, the crucial question is whether the liability of the socioeconomically better-off transcends this minimal threshold of sufficiency.

III. THE IMPLAUSIBILITY OF INTERNATIONALISM

Roughly sketched, the internationalist doctrine claims that the call for redistribution does not apply beyond what has been outlined above as the demand of humanitarianism. Rather, the scope of distributive justice remains confined to the nation state. There are distinct justifications for such a view. According to one, the submission to the coercive power in the domains of taxation and private law must be justified by equality in economic goods (Blake 2001). A second version establishes that the submission to the regulatory forces of the state reflects an implicit appraisal of the values embodied in these legal provisions. In order to find this appraisal, they must be acceptable to all citizens, entailing a demand for distributive justice (Nagel 2005). On the contrary, Sangiovanni (2007) detects the call for distributive justice in the reciprocity which we owe to all those who contribute to the perpetuation of the state – for their part putting us in a position to “act on a plan of life”. While such a contribution is made by all compatriots, it is not by non-citizens. Despite these differences, all versions of the internationalist doctrine hold in common that the nation state provides a distinct framework of relationships¹ among its citizens which is not established by the institutions of international cooperation as they exist today. Consequently, the principles of distributive justice only apply within the borders of the nation state.

From my perspective, it is debatable to what degree international institutions are insufficiently coercive, demand negligible appraisals from nation-state citizens, or contribute too little to assure the continued existence of the state in order to warrant a limitation of distributive justice to the nation state. Yet, I think it is reasonable to accept the claim that on average the institutions internal to the nation state put fellow citizens in a comparatively stronger relationship with each other than with non-citizens. The question remains whether this particular relationship alone exhausts the conditions for distributive justice to apply or whether it is merely a sufficient, but not necessary condition we can refer to in questions of socioeconomic justice. To borrow a distinction from Sangiovanni, I accept the empirical claim that domestic institutions portray a special nature, but in what follows I will argue that this does not establish moral significance in a way to confine redistribution to the national level.

The crucial flaw of the internationalist doctrine is that it designs distributive justice as an exclusive relational concept. The internationalist does not discriminate a priori as to whom are ineligible to

¹ In the following the term ‘relationship’ will be used as a collective term to represent the specific conditions for distributive justice marked-off by coercion (Blake), appraisal (Nagel) and reciprocity (Sangiovanni).

the benefits of justice, yet she does discriminate in the light of the empirical facts. Let's assume the internationalist is right in confining distributive justice to the national level. Looking at the state of the world, I feel urged to ask: what about those who live in states where coercion is not justified by the realization of a fair share of economic holdings? How can the right to justice be realized in case of those whose state lacks the resources to build national institutions firm enough to be responsive to the justified quests of its citizens? Is the right to justice for them a pure formality with no hope for actual realization? According to Sangiovanni (2007), "[w]e owe obligations of egalitarian reciprocity to fellow citizens and residents in the state, who provide us with the basic conditions and guarantees necessary to develop and act on a plan of life, but not to noncitizens, who do not." To be frank, I am convinced many Somalians would love to bear some of the Frenchmen's burden in guaranteeing the continued existence of the 5th Republic, if these burdens put them in a position to benefit from its redistributive regime. Similarly, many would love to subject themselves to the coercion of Finnish authorities, if it entitled them to access the country's widely praised general education system. But unfortunately the internationalist does not mention global freedom of movement as a necessary prerequisite for her account of justice. Quite the opposite, "everyone may have the right to live in a just society, but we do not have an obligation to live in a just society with everyone" (Nagel 2005). If we accept the first part of the quote and take the matter of global justice serious enough to stand off cynicism, we cannot accept the internationalist account. The alleged claim of right to justice would be reduced to a mere formality, with no chance of realization whatsoever for a vast share of the global population. The assumptions of the internationalist simply square with the observation that some state authorities lack capacities, will, or both to build appropriate institutions that cater the purposes of redistributive justice. Therefore, if we want justice to be more than a luxury good open to the exclusive club of peoples from developed countries while being an empty concept for all others, we must reject the internationalist doctrine.

IV. GLOBALISM – BUT WHY?

Having suggested an argument against the internationalist doctrine, one should accept the fact that the scope of distributive justice transcends state borders. Still we are left to define whether demands for justice are derived from a pattern of relations we entertain across borders, or whether they would persist even in absence of cooperation.

Some may argue that the non-relational content of distributive justice was exhausted by the humanitarian concern proposed above. From this perspective any further quests for redistribution could only arise from the global web of relations among different actors. However, I disagree with such a minimal conception of non-relational justice.

To be sure, distributive justice has relational content. So far, the treatment of this essay's issue

bore an implicit reference to the global community as a system of international cooperation. This assumption obtains credibility in the light of soaring cross-border migration, the increasing establishment of free trade areas, and the evolvement of international organizations to coordinate and promote these newly evolved institutions. The very degree of socioeconomic integration now paves the way for a relational principle of distributive justice:

Each country and its citizens shall reap a fair share of benefits and shall carry a fair share of burdens evolving from the interactions they entertain. If these shares are not established by transmission of the market mechanism, the just outcome shall be consummated by means of redistribution.

We do not need to specify the underlying egalitarian principle of justice, or, put differently, the meaning of the term ‘fair’ in order to make this provision credible. Obviously, interactions yield benefits and burdens for each of the affected parties. If one party objectively bears a disproportionate share of burdens, it would not voluntarily choose to engage in this very interaction given a rational and informed decision process. If it still does, there must be outer influences determining the decision outcome. These outer influences are coercive in the sense of obstructing the party to decide sovereignly for its own good and therefore need additional justification. In absence of such a justification, the party has a right to be compensated for such interference at its disadvantage by the ones who benefit from it. In this sense the call for distributive justice only arises between specific parties and does not extend to others. Hypothetically assume resource R to be the only commodity of interest in a stylized world, where country S is a resource monopolist and country C one of its customers. S has a right to be proportionally compensated by C for the cost related to the exploitation of R. It has a claim for a fair share of benefits from the interaction even if markets do not reflect the total cost, i.e. exclude environmental damages, in terms of prices.² Nevertheless, any other country O has no such justified claim on C, as it does not engage in the exploitation of R.³ Similarly, O has no duty to compensate S if it does not source R since it does not benefit from the cost incurred by S. The claims and duties related to distributive justice only arise from the R-specific interaction between S and C – they are relational in character.

Yet, distributive justice additionally requires a concern for the capacity of willing parties to engage in such mutually beneficial relationships. For illustration, consider the case of a paralyzed person. Intuitively we would, and empirically in most societies we do, compensate a paralyzed person for her to become a cooperating member of society. We provide her with mobility devices, assistance in educational institutions or specific physiotherapies, wanting her to overcome the additional challenges she is facing in developing skills and knowledge that are valuable to others in society.

2 For instance, consider price collusion by the total set of customers.

3 Naturally, it may have other justified claims for compensation based on the interactions it entertains with the C, say with regard to resource R2.

As far as possible we strive to put people in positions of auto-sufficiency, in which they can sustain their livelihood based on interactions with others independently of continued assistance. Wouldn't we be repulsed by the claim that the paralyzed person deserves nothing more than her minimum needs because distributive justice only requires compensation if backed-up by already existing economic transactions?⁴

Transferring this intuition from the individual level to the field of global justice, we should hold that relational distributive justice is not enough. As has been argued in section 3, many countries do not have the capacities to develop institutions firm enough to act on principles of distributive justice. Mirroring this 'internal paralyzation', others bear an 'external paralyzation' in the sense of being incapable to become a member of the system of global cooperation without prior assistance.⁵ As in the internationalist case, distributive justice would remain an empty concept for these countries and their people if we based the globalist account solely on relational grounds. To strengthen this intuition, consider the conclusions from modernization theory which postulate that institutional quality follows economic prosperity (Glaeser et al. 2004). In this spirit, 'internal paralyzation' could follow from 'external paralyzation'. Prior to the application of the relational principles outlined above, every member of the global community has a right to be put in a position for cooperation with other countries. We cannot simply say: "Pace Somalia, but right now you do not happen to be an attractive trading partner; a fact which also undermines any of your claims to redistribution beyond humanitarian concern. Care packages of rice and tents to keep your citizens alive is all you could reasonably hope for." Rather, a preliminary demand of distributive justice could be formulated as follows:

Each country and its citizens willing to be part of the global community should be given a fair opportunity to become an equal member in the given system of international socioeconomic cooperation.

However, this request embodies a non-relational aspect of distributive justice, the preliminary satisfaction of which bestows content upon the relational principles of justice.

V. CONCLUSION

This essay started by highlighting humanitarianism as a common ground of different theories of global justice. Beyond this consensus different conceptions of distributive justice would yield vastly diverging answers regarding the question 'What do we owe the poor?'

From my perspective, a plausible version of distributive justice must comprise both, relational

4 The same held true if we would abstract from the instrumental perspective and substituted the sub-clause "that are valuable to others in society" by something with a more absolutist flavour like "that enable her to live a decent life".

5 The concepts of 'external paralyzation' and 'internal paralyzation' of course are not mutually exclusive.

and non-relational principles. Essential for this conclusion is the assumption that a conception of justice must not be tailored in a way to de facto exclude some people in the light of the empirical facts. Justice is no luxury good whose accessibility is confined to the ones who were lucky enough to satisfy the contingencies that give rise to its application. On the contrary, everyone must have a non-conditional and fair opportunity to experience its realization. Therefore, it has non-relational content. Such a view necessarily excludes any internationalist conception, the proponents of which derive the claim for distributive justice from the special relations among compatriots. Similarly, it excludes globalist conceptions that solely rely on relational principles of justice. Furthermore, the internationalist cannot capture the non-contingent fact that the scope of justice includes every human being. In this spirit, justice furnishes the relatively worse-off with the following two claims:

1. To become an equal member of the system of international cooperation if they desire to be so.
2. Once a member of the system of international cooperation, to realize a fair share of burdens and benefits from the arising interactions.

These claims apply to the entire global community and therefore include the relatively better-off. While the second principle assigns a clear responsibility to the individual parties of interaction, the first one remains vague regarding the ones whose responsibility is called upon. Naturally, such a conception involves a notion of capacity and also historic liability. In both cases it seems reasonable to assume that the relatively better-off bear a particular responsibility to realize the first principle. However, a detailed disentanglement of these questions must be consummated in an independent treatment.

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I WANT TO DRINK PAINT: A RATIONALIZED ACCOUNT OF DESIRES

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Abstract. Donald Davidson has an interesting view on rationalizing acts; however, the crucial element of “pro-attitude” on which it rests remains under-developed. This paper first sets out the Davidsonian system with focus on rationalization and in terms of pro-attitudes. It then develops, using Lacan’s account of the necessarily linguist structures of pro-attitudes, an elucidation of desire. It closes with a few thoughts on further research.

I. INTRODUCTION

Davidson’s understanding of human intention is very nuanced and sophisticated. However, the crucial element of “pro-attitude” on which it rests remains wanting. In this paper, we will attempt an explication and elaboration of this concept. This will allow for a more nuanced account of “preferences”, “desires”, “pro-attitudes” and “demand.” It will also suggest further avenues of research into the nature of the subject and intentionality as a whole. First, we shall set out the Davidsonian system, focusing on the various components involved in rationalization. Here we review the little Davidson says about pro-attitudes. Second, we shall find a most unlikely ally in Lacan’s account of the necessarily linguist structures of pro-attitudes. We will show how this synthesis sheds new light on pro-attitudes and their relationship to the Davidsonian system. We will, finally, close with some brief thoughts on possible further inquiries.

II. DAVIDSON’S SYSTEM

i. Intentional explanation as necessary and irreducible

Davidson begins with the simple fact that we describe human actions in a way that is radically different to the language of physics. This immediately raises two questions: (1) why do we do this (i.e., what, if anything is *sui generis* about human acts qua human acts) and (2) can it be changed - more specifically, can intentional explanation withstand reduction (into a mature neuroscience or some other conceivable alternative)?

Why do we make intentional explanations?

To this first question, the answer is that human acts have an intentional component. Davidson begins by asserting that all human acts are token physical acts. Walking has a corresponding set of physical events, as does eating and thinking. However, the vocabulary we use to make sense

of the acts is not type-reducible to physics. For example, I can say my name loudly, quietly, in a fake voice, in a funny accent, in another language, etc. Each of these token physical events will, qua a physicalist vocabulary (L_p), be *radically different* from each other. However, in each token instances, *I have said my name*. Why? Partly because I intended to. Intention is a necessary component of the individuation of human movements into coherent actions. Let us call this the language of intention (L_i).

Let us unpack this further. We have an event, E. We can describe E in L_p, in which case we will see forces, particles, interactions, etc. The same E, described in L_i, is, say, a basketball game. Interestingly, the kind terms of the basketball game do not translate into the kind terms of L_p. What is a “foul” or a “point” in L_p? Further, if all we had access to was an L_p description, it is highly unlikely we would be able to follow the game. A player moving his right leg may be a re-positioning, a foul, a stepping out of bounds, a free throw, etc. But because (a) these demarcations are utterly senseless in L_p and (b) the physical movement of moving one’s leg is indeterminate (i.e., variably realisable) in L_i, we cannot map basketball descriptions in L_p onto basketball description in L_i in any non-ambiguous (or one-to-one) way so as to see the implications of the actions. That is to say that there is no way of comprehending the (social) *implications of actions* that are articulated in L_i from *descriptions of actions* in L_p. Description of the action needs to be ‘translated into’ L_i first. Thus, we seem to need L_i to make sense of the game - otherwise our L_p description becomes radically indiscriminate as to what actions are the meaningful ones with the implications; L_p admits all physical acts from a three point shot to a nose itch as token physical acts, thereby confounding our understanding of the game.

So what then is the nature of L_i? Davidson tells us that L_i relies on the following relationship: *cause* (belief & pro-attitude) > act. That is to say: beliefs and desires cause acts. This notion of a cause is necessary to individuate acts correctly. To use the famous example: I come home, turn on a light, scare some thieves who run into the night, and, as they leave, they escape my sight. To say when my act stops and the unintended consequences begin properly, we must assume that my intent causes me the proper act. I *intended* to turn on the light. That is my act. I did not *intend* to scare thieves and so that is not my act.

If an event can be described in these terms, invoking the belief and pro-attitude that serve (in the right way) as the cause of the action, Davidson calls it *rationalized*. A key part of this is that my belief and pro-attitude must *cause* the act. This cause is my intention carried out. First, we will elaborate this rationalization of events. Then we will look at each element of rationalization.

Let us take a toy example to frame this discussion. I see my friend Jerry. Jerry goes into a coffee shop. From this, I infer that (a) Jerry *wanted* tea, that (b) Jerry *believed* correctly that in the UK one can buy tea at a coffee shop, and a whole host of other things. Note that from fairly overt

behavior that could- for the sake of argument- be given in L_p , I have created a complex set of mental states and ascribed them to Jerry. Furthermore, I hold certain expectations inductively over past empirical observations and, similarly, expectations based upon my general notion of “coffee shop”, “tea”, “Jerry” and other concepts involved - I don’t expect Jerry to walk out of the coffee shop wearing a dress and carrying a watermelon. Finally, if he does walk out of the shop wearing a dress and carrying a watermelon, I will have no way of rationalising his actions - for I would have in mind neither a belief nor a pro-attitude necessary to ascribe an intentional state by which to rationalize his action. More generally, where one can make intentional ascriptions, one can rationalize one’s acts; where one cannot, the acts remain unrationalizable relative to one’s knowledge of the situation.

Is intentional explanation reducible?

The careful reader will notice that this process is circular. To understand Jerry’s actions, I attribute rationality and intention. To attribute rationality and intention to him, I must view his actions in a certain way. According to Davidson, this circle is inescapable for very much the same reason that a basketball game can’t be watched in L_p . In both cases, I need a set of categories, orthogonal to L_p , to individuate events correctly. Otherwise, Jerry’s walking and Jerry’s stumbling due to being pushed are indistinguishable. This means to answer our second question- can we reduce L_i into some other language? The answer is no. L_i is a self-supporting and self-reinforcing circle.

ii. Belief and pro-attitude

To summarise the above observations, intentional explanation is at once the only type of explanations that we do make and the only type of explanation that is adequate. Stuck in this way with intentions, we will now further characterise the two ingredients of intention (beliefs and pro-attitudes) towards evoking the peculiarities involved that we intend to capitalise on in this paper.

One element of intentional explanations of actions is beliefs. Beliefs, for Davidson, are (i) capable of being common to or relatable across persons in a significant way, (ii) constructible in words, (iii) inferable from acts, and (iv) internally consistent at the moment they are stated (i.e. I cannot hold two contradictory beliefs at the moment I act). From this minimum, Davidson is able to make sense of a vast array of human acts. For us, child sacrifice is wrong and is pejoratively called “irrational.” However, it is rational if one grants a certain ontology: there are demons, the demons have a nature that demands innocent blood, etc. The natives killing the child hold a belief about demons which is (a) consistent- they do not believe that there are and are not demons- (b) guides the act- they kill the child- and (c) is coherent- given their ontology, these beliefs make sense of the world.

The final part of this formula is what Davidson calls a pro-attitude. He does this to capture all possible intentional states. Wanting, planning, needing, hoping for, fear of, etc. can all be pro-attitudes. Other than stating that pro-attitudes have a vaguely emotional/biological component, Davidson is surprisingly vague on what these are and how they work. Davidson (1994b: 677) says “fortunately, it is not necessary to classify and analyze the many varieties of emotions, sentiments, moods, motives, passions and hungers whose mention may answer the question “why did you do it.”” He then gives some strange examples of desires, our favorite being the want to drink paint (Davidson 1994b: 675).

This seems deeply odd for a number of reasons. It seems like the nature of the pro-attitude should alter the rationalization. Buying someone a beer because *I am in a friendly mood* is radically different from buying a beer because *I want to sleep with them*. Also, it is unclear how pro-attitudes connect to beliefs and allow rationalizations at all. It is the latter that we shall discuss.

III. LACAN ON DESIRE

As a caveat, when we talk about desires, we are not referring to urges, pre-dispositions or preferences. We consider these elements as separate, distinct features that are brought under the notion of pro-attitude. As they are different, our account will be restricted to the domain of desires. The reason for treating this notion specifically is that, by clarifying it, we will be able to elaborate on its function in rationalization. To elucidate desires, we draw upon a distinct account made by Lacan. In this section we will introduce Lacanian theory as one that upholds the primacy of language as fundamental to socialisation, yet does this without the conventional correspondence between words and the objects that they signify.

Lacanian theory is a system of interwoven premises that progressively evolved throughout years of work. The major concern for Lacan, throughout his writing, was the construction of the Subject. Language is the determinate factor in the process of constructing Subjectivity (Chiesa 2007). In Lacanian theory, language is neither an instrument nor a mere capacity. It is through language that the subject is introduced into the social realm. By this acquisition of linguistic capacities the person is able to understand a huge variety of situations within her own life: from the conceptual schemes concerning norms and rules, which are expected to be followed, to the definition of her relation towards others - the mother, siblings, friends etc. Language literally enables the person's *socialization*, that is, the need to conform to linguistic rules in order to be able to communicate and participate in a society. This is a crucial feature of language acquisition, namely, that the person is not using the language arbitrarily, based on her will, but according to the existing rules and norms of language practices. While this seems a rather trivial truth, Lacan notes that it is at the stage of language acquisition when the individual understands the impasse

of expressing herself literally. This impasse is best seen via reference to the moment when a small child starts to learn new words. She learns that there is a correspondence between the object and the word to describe the object. For physical objects such as a glass, a window, a car this might seem obvious. But what about abstract ideas like “love”? What does “love” describe or refer to?

Our reason for discussing correspondences between signifier (“chair”) and signified (chair-object) is due to Lacan’s move to criticize Saussurian notions of linguistics. He attacks the presumed relation between the signifier and the signified. The major problem with Saussurian linguistics, according to Lacan, is the kind of presumed relation best seen between chair-object and “chair” is not coherent in the example of “love.” Nor does it capture the possibility of expressing what “love” is. Lacan is arguing that this relation is problematic because a word cannot simply represent the whole variety of the notion of “love”. There will always be some aspect of the notion represented that will evade a complete representation by the word.

Why/how language is brought into an account of desires? Lacan’s theory provides a possible answer to the question. By acquiring a language at the early stages of development, human beings can verbalize their desires. The linguistic expression of desires is what makes it possible for the person to understand (and have an account of) what is desired (both for what she uses and hears as used). This kind, the linguistically expressed desires, is precisely our concern. These are the desires that could be brought into Davidsonian framework because the crucial element for them is language.

We then have a way to address desires via language. Yet the language used in an expression and its grammatical form need not reveal the function of the expression, and so its meaning or use in rationalizations. Consider three classes of utterances: “I want X”; “I demand X”; “I desire X”. For example, in our everyday life it is unlikely to hear someone say “I desire a glass of water.” Instead, we hear “I want a glass of water” or more politely “I would like a glass of water”. Lacan’s argument here is that desire through language will take the form of a demand. It is not, however, usually *stated* as a demand in practice. The implementation of expressing, “I want water”, “give me water!”, “may I please have water” differ, but the basic demand is the same for each token. Once a desire enters language it becomes a demand and gains some crucial features. It acquires a subject, that initially expresses this demand, and it also acquires an object; demands are always about something. This transition from non-linguistic to linguistically expressed desire allows us to have an account of a subject that demands and the object of its demand.

Let us, quickly, re-articulate our position. Language is the essential element in the formation of the Subject because the former enables the latter to function in the social realm. The relation between signifier and signified lacks the possibility to fully articulate all the aspects of the notion represented. Linguistically expressed desires inevitably follow the rules of language. In this transition a desire obtains the form of a demand, which means “need to be satisfied.” This

account concerns how we are to understand demands. It is crucial, then, to notice again that the kind of desires that we are concerned with are those that are linguistically expressed and not, for example, unconscious desires. Ultimately, it is language that plays a determinate role of how to account for desires. Additionally, we are here providing a treatment of desire only insofar it is expressed through language.

IV. CONCLUSION

It seems to us that in order to introduce desire as an element in the analysis of action, desire has to be linguistically expressed. Introducing this sort of clarification allows us to forge a link between elements that Davidson groups under the notion of “pro-attitude”. They are all linguistically expressed. By using some crucial remarks from Lacan’s theory, we have showed that language is a fundamental element for the definition of desire as such. Additionally, we have maintained the distinction between a desire, an urge, a preference, a convention etc. This distinction is crucial to our argument because it allows us to isolate the notion of desire without mixing it with other crucial but distinct elements of pro-attitudes.

With this in view, the relation between belief and pro-attitude becomes much less problematic. It is not that non-linguistic desires are, somehow miraculously, mapped onto necessarily linguistic beliefs. It is rather that desires, in order to count in rationalizations at all, are themselves linguistic. Thus, to Davidson’s necessary criteria of beliefs- that they are consistent and coherent- we can add necessary criteria to the account of desires- that they be objected, directed and that they imply a subject. These criteria of desires give us a valuable foothold to begin to further elucidate the relationships between desire, preference formation, language, culture and belief.

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THE GLOBAL FINANCIAL CRISIS AND THE METHODOLOGY OF ECONOMIC RESEARCH PROGRAMMES

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Abstract. Following the failure of standard economic models to predict the global financial crisis, interest in competing research programmes has increased. This paper seeks to critically assess the state of four distinct economic research programmes (the neoclassical synthesis; complexity economics; prospect theory; and neuroeconomics) using the methodology of scientific research programmes (MSRP). Two central conclusions are drawn. Firstly, complexity economics can be judged as progressive *in light of the global financial crisis*, whereas new variant theories in the protective belt of the neoclassical synthesis remain to be empirically corroborated. Secondly, the global financial crisis cannot be used to argue for either the progressivity or the degeneration of either prospect theory or neuroeconomics, since neither of these programmes predicts novel *macro-economic* phenomena.

INTRODUCTION

“I can sum Lakatos up in five words: he has revolutionised my thinking.”

(Sir Karl Popper, in correspondence with Sir Isaiah Berlin, 16th February 1964)

Last November would have been the 90th birthday of Imre Lakatos. His methodology of scientific research programmes (MSRP) continues to influence the philosophy of science half a century after it was first articulated. This paper is a contemporary application of MSRP to the field of economic science.

The global financial crisis has become the defining economic event of recent times. However, it was not predicted by standard macroeconomic models. Since the crisis, many non-standard research programmes in economics have been refreshed by the imperative for more accurate models of economic phenomena. However, there is widespread disagreement about the fruitfulness of these programmes, with discussion often boiling down to methodological criticisms (frequently propagated on the blogosphere) (see, for example, Kirman 2012).

This paper seeks to critically assess the state of economic science using Lakatos’ methodology of scientific research programmes. As such, I hope to clarify the economists’ debate about a methodology that is informed by the philosophy of science. The paper is structured as follows: Part I introduces the concept of MSRP informed by a discussion of the philosophy of science; Part II establishes four distinct scientific research programmes in contemporary economic science (the neo-classical programme, complexity economics, prospect theory, and neuroeconomics);

Part III critically assesses these programmes in light of the global financial crisis. Unfortunately, it lies beyond the confines of the present paper to give a more general critique of progress in economics as a whole, but focusing on macroeconomics and the global financial crisis in particular will allow us address some immediate concerns. Part IV concludes.

I. THE METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMMES

“Philosophy of science without history of science is empty; history of science without philosophy of science is blind.” (Lakatos 1970: 91)

Lakatos (1968, 1970) proposed his methodology of scientific research programmes along the following lines:

- a) A scientific research programme is defined as a set of theories T, T', T'' ... divided (exhaustively) into two non-empty subsets: the *hard core* and the *protective belt*. The hard core constitutes those theories that are integral to the scientific research programme, and which, according to the programme, cannot be falsified. The protective belt refers to those theories of the programme that are specified as falsifiable.
- b) Whenever a prediction of a scientific research programme is falsified, the *negative heuristic* is a methodological command that points the arrow of falsification firmly in the direction of those theories that lie in the protective belt and away from the hard core. In addition, the *positive heuristic* of a scientific research programme specifies ways in which new and refutable variants of the programme’s theories can be produced.
- c) A scientific research programme is defined as *theoretically progressive* if each new theory (as produced by the positive heuristic) predicts novel facts not predicted by its predecessors. A research programme is defined as *empirically progressive* if those predicted novel facts are then corroborated. If a programme is both theoretically and empirically progressive, then it is called, simply, *progressive*. If a programme is not progressive, then it is defined as *degenerating*.

This methodology of scientific research programmes was proposed in response to the perceived flaws in the two existing accounts of scientific method that were dominant at the time: the Popperian approach and the Kuhnian approach. Popper’s approach provided a logic of theory refutation, but did not adequately account for theory rejection, which often happens (or is reversed) long after a particular theory is refuted (see, for example, Agassi 1966). On the other hand, while Kuhn provided a powerful narrative of how scientific revolutions occur, he did not provide an adequate logic of theory-appraisal (Nickles 2009). Lakatos’s approach attempts to combine the relative merits of both accounts. To this end, the unit of appraisal in MSRP is no longer a single theory, but a set of theories. Thus, Lakatos is able to maintain the rationality of scientific development through the use of falsification, theoretical progression, and empirical

corroboration that takes place in the protective belt, all the while maintaining the dynamic progression and degeneration of scientific programmes that arises from a dogmatic commitment to a hard core.

The original application of MSRP was in the domain of the natural sciences, especially physical science (see Lakatos 1970). However, several authors have successfully applied MSRP to the social sciences, and economics in particular (see, for example, the collection of case studies in Latsis 1976).

II. IDENTIFYING CONTEMPORARY RESEARCH PROGRAMMES IN ECONOMIC SCIENCE

Having introduced the concept of MSRP, I now proceed to identify contemporary research programmes in economic science to which the logic of MSRP may be applied (see Table 1). The following is not an exhaustive treatment of all possible candidates. However, I do identify the most significant research programmes, highlight their most salient features, and provide a justification for choosing these programmes over others.

a. The neoclassical synthesis

The first (and perhaps most obvious) research programme in contemporary economics is 'the neoclassical synthesis' (Blaug 2010). This research programme can be considered as the dominant standard approach to modelling economic behaviour. Specifically, the hard core of the neoclassical programme consists of tenets such as the theory of economic equilibrium, the theory of the firm, and the (weak) axiom of revealed preference, as well as many other theories (Weintraub 1985). The hard core theories of this research programme distinguish between private agents, firms, and the government, as well as the theoretical environments of perfect and imperfect competition, perfect and imperfect information, and perfect and bound rationality. The protective belt of the neoclassical synthesis includes theories such as relative purchasing power parity and the DSGE models that are used to forecast macroeconomic trends (Backhouse 1998). The positive heuristic of the neoclassical programme demands that new theories in the protective belt are cast in terms of agents, firms, and the government, that they use techniques such as constrained optimisation to attain equilibrium, and that they ultimately predict, and are corroborated by, novel *choice* data (ibid.).

b. Complexity economics

The increase in computational power over the last two decades has fostered an emerging programme in complexity economics (Beinhocker 2006). Essentially, complexity economics seeks to model economic phenomena in terms of the interactions between multiple agents using environment-free computer programme simulations.

	The neoclassical synthesis	Complexity economics	Prospect theory	Neuroeconomics
Hard core	<ul style="list-style-type: none"> • Theory of economic equilibrium and comparative statics • The theory of the firm • Axiom of revealed preference • Long run neutrality theory of money 	<ul style="list-style-type: none"> • Agent-based modelling: the economy as an organism • Bounded rationality • Reiterated transactions engender reinforced learning 	<ul style="list-style-type: none"> • Agents characterised by their biases and heuristics • Agents' evaluations are relative to a neutral reference point • The theory of loss aversion 	<ul style="list-style-type: none"> • Economic behaviour is, in principle, reducible to neuronal activity • Non-choice data are essential to explaining economic behaviour
Protective belt	<ul style="list-style-type: none"> • Theory of relative purchasing power parity • Dynamic stochastic general equilibrium (DSGE) models 	<ul style="list-style-type: none"> • Financial market 'herding' => pro-cyclical leverage cycles • Continuous economic boom and bust 	<ul style="list-style-type: none"> • Theory of conformity to default option (and other theories of <i>Nudge</i>) 	<ul style="list-style-type: none"> • Not well established with respect to macroeconomic phenomena • Reference-dependent utility theory
Positive heuristic	<ul style="list-style-type: none"> • Use choice data to evaluate hypotheses • Use tools such as constrained optimisation and equilibrium analysis to model economic phenomena • Entities in new models must come in the form of either agents, firms, or the government 	<ul style="list-style-type: none"> • Characterise agents as engaged in a learning process such as Q-learning • Use computer programmes to simulate multiple agent interactions in arbitrary environments 	<ul style="list-style-type: none"> • Use basic prospect-theoretic model to articulate new theories • Evaluate new theories in light of psychology/ decision experiments 	<ul style="list-style-type: none"> • Use fMRI and other neurological techniques to evaluate economic models, in addition to choice data

Table 1 Scientific research programmes in contemporary economics

Agents are constantly adapting to, and as a result of, the interactions they engage in. In this sense, complexity attempts to capture the full intricacy of the economy as a living organism that does not necessarily tend towards equilibrium. As such, its hard core constitutes a commitment to agent-based modelling and the theory of continuous adaptation (see Holland and Miller 1991 for an example of this approach). The protective belt in complexity economics includes, most notably, the theory of ‘herding’ in financial markets, which is derived from the hard core commitment to adaptive learning through repeated interactions (Rosser 1999). Generally, the positive heuristic in complexity economics emphasises the need to model economic phenomena using multiple agents engaged in repeated interactions in a computer simulation upon which no exogenous factors act. Given this interpretation of the methodology of complexity economics, it can be considered as a distinct scientific research programme that is not incorporated by the standard neo-classical programme and its dependence on equilibrium analysis.

c. Prospect theory

Prospect theory has largely arisen out of results in behavioural economics not predicted by standard expected utility theory. The basic theoretical framework is outlined in Kahneman and Tversky (1979). The hard core of prospect theory consists of the commitment to the principle of loss aversion and evaluation relative to agents’ neutral reference points. The protective belt of prospect theory is perhaps best exemplified by the theories and predictions laid out by Sunstein and Thaler (2009). These include theories such as non-adaptation to changes in default states which predicts that people will not respond to being opted-in to previously opt-out employee retirement schemes. The positive heuristic in prospect theory commands new theories to be constructed in terms of reference points, losses and gains, and the automatic-deliberative distinction in cognitive processes (Kahneman 2011). Given these commitments, prospect theory clearly constitutes a very different kind of scientific research programme to that of either neo-classical economics or complexity economics.

d. Neuroeconomics

Neuroeconomics is essentially the application of neurological insights to explaining and predicting economic phenomena (Camerer et al. 2005). By using data from neuroscientific techniques such as fMRI, researchers infer details about how the brain works and use this both to create better models of economic decision-making and to directly test economic hypotheses. The hard core of the radical neuroeconomic programme consists of ‘learning based on consumption experiences’, the theory of automatic processes, and the theory of emotion (affective) systems (Camerer et al. 2005: 10-11). Owing to the relatively new emergence of neuroeconomics, it is hard to define very precisely what constitutes its protective belt, although Gul and Pesendorfer (2010) have argued that reference-dependent utility theory is a typical falsifiable theory of the neuroeconomic research programme. The positive heuristic in neuroeconomics is very clear in

commanding the use of neurological data, in addition to choice data, as a tool for developing and testing new neuroeconomic theories. As such, these features distinguish radical neuroeconomics from the other economic research programmes described above.

In addition to the four programmes above, many scholars have identified new-Keynesian economics as a separate research programme (see, for example, Backhouse, 1998). However, it is debatable how distinctive the new-Keynesian programme is relative to the new-Classical programme. In particular, since Lucas (1972) and Prescott and Kydland (1977) there has been significant conversion between the two schools of thought (see the discussion in Akerlof, 2007). As such, the respective explanations of the global financial crisis as provided by both new-Classical economics and new-Keynesian economics are very similar. The purpose of this essay, as explained in the Introduction, is to evaluate contemporary economic research programmes *in light of the global financial crisis*. For these reasons, therefore, I treat both new-Classical economics and new-Keynesian economics as constitutive of one central scientific research programme: the neo-Classical synthesis.

One could identify several further economic research programmes, including many sub-programmes of those already mentioned; the level of abstraction is ultimately an artefact of the MSRP and determined by the methodologist with reference to the purposes of her case study. For the purposes of this essay, it is necessary to focus discussion on the four programmes outlined above, as they will give us a sufficient idea of the state of contemporary economic science in light of the global financial crisis without creating an overly-complex analysis.

III. IDENTIFYING PROGRESSIVE AND DEGENERATING RESEARCH PROGRAMMES IN LIGHT OF THE GLOBAL FINANCIAL CRISIS

Having identified four distinct scientific research programmes in contemporary economics, I now proceed to critically assess the relative progressivity/degeneration of these programmes in the light of the global financial crisis (GFC).

Under ‘global financial crisis’ is meant the period from August 2007 (when BNP Paribas first suspended withdrawals from three of its hedge funds) to April 2009 (the date of the London G20 summit). This period was characterised by a ‘credit crunch’ in international liquidity markets and the failure of several global systemically important financial institutions (G-SIFIs), including Lehman Brothers. Despite its intensity and duration, the global financial crisis was not predicted by standard economic models (Krugman 2009)¹. As such, the GFC can be said to have falsified the

¹ Some economists may wish to argue that the neo-Classical synthesis may not actually have been falsified by the observation of the GFC. In particular, if we take recourse to the efficient markets hypothesis (EMH) then the neo-Classical synthesis remains unfalsified because EMH predicts that financial markets cannot be predicted. However, this would constitute theoretical degeneration since EMH predicts phenomena that cannot be falsified. Thus, from the viewpoint of MSRP and for the purposes of this essay, EMH does not constitute a valid scientific theory).

empirical predictions of the neo-Classical synthesis. Under MSRP, the arrow of this falsification is directed towards theories in the protective belt. In particular, the standard theory of financial intermediation is falsified. The existing theory of financial intermediation hypothesised that the financial sector acts simply as a 'veil' between savers and investors (Brunnermeier et al. 2012). As such, dynamic stochastic general equilibrium models (DSGE models) did not model the financial sector at all, prior to the crisis. This falsified theory is now in the process of being replaced by a new variant in the protective belt. Adrian and Schin (2010) and Geanakoplos (2010) have integrated financial intermediation into standard DSGE models and thus have sought to capture observable dynamics in the financial sector. Thus, it could be argued that the research programme is theoretically progressive because the new theory in the protective belt predicts novel phenomena (such as 'the leverage cycle').

Nonetheless, it remains to be seen whether or not this theoretical progression is complemented with the empirical corroboration of these new phenomena. For the moment, without empirical corroboration, the neo-Classical synthesis can be characterised as a degenerating research programme with respect to macroeconomic phenomena such as financial crises. Several authors have suggested that the commitment to equilibrium analysis in the hard core and positive heuristic of the neo-Classical synthesis renders the programme unsuitable for analysing endogeneity in the macroeconomy (Ormerod 1994, Kirman 2012, Taleb 2010). If this is the case, then there is reason to suspect that the neo-Classical synthesis will continue to be empirically falsified with respect to phenomena like financial crises.

Complexity economics, on the other hand, does not have a hard core commitment to equilibrium analysis. Thus, when predicting phenomena such as financial crises, it may be that complex systems analysis are better able to predict sporadic cycles of boom and bust that result from no significant exogenous shock to the international macroeconomy (Hahn 1991). Indeed, the Economic Complexity Index has already demonstrated greater accuracy than standard DSGE models used by the World Bank to predict future rates of economic growth (Hausmann et al. 2011). In particular, the positive heuristic in complexity economics, with its focus on endogeneity and constant adaption, perhaps makes complexity economics a more fruitful research programme with respect to economic phenomena such as financial crises.

It is not entirely clear how the status of both prospect theory and neuroeconomics as scientific research programmes is affected in the light of the financial crisis. Many commentators have argued that these programmes are vindicated by the crisis (Krugman 2009, Taleb 2010). However, it is not obvious why this is the case, particularly given that both of these research programmes are primarily concerned with explaining and predicting micro-economic behaviour. For example, prospect theory attempts to explain why individuals often violate the principle of expected utility. In Koeszegi and Rabin (2004), the domain of application is a single individual's choices between various risky prospects. Thus, even if both of these programmes are progressive in

the light of psychological and laboratory experiments, there is a further analysis to be made for the progressivity of these programmes with respect to the global financial crisis and other macroeconomic phenomena. In this way, MSRP clarifies the economists' debate by highlighting the fact that in order to be considered progressive in the light of the global financial crisis, a scientific theory has to make falsifiable predictions about the relevant phenomena. Neither prospect theory, nor neuroeconomics currently make such predictions about macroeconomic phenomena. Thus, using the global financial crisis to argue for the progressivity of either prospect theory or neuroeconomics is not valid according to MSRP.

IV. CONCLUSION

In this paper, I have presented an updated application of Lakatos's methodology of scientific research programmes to the field of economic science. In particular, I have focused discussion on the progressivity of four central economic research programmes in the light of the global financial crisis. Two central conclusions can be drawn from such an analysis. Firstly, complexity economics can be judged as progressive in light of the global financial crisis, whereas new theories in the protective belt of the neoclassical synthesis remain to be empirically corroborated. Secondly, MSRP demonstrates that the recent trend of using the global financial crisis to credit or discredit the progressivity of either prospect theory or neuroeconomics is illegitimate. Neither theory provides falsifiable macroeconomic theories and thus cannot be judged in light of macroeconomic observations such as the global financial crisis.

This paper has not provided an exhaustive treatment of all possible scientific research programmes in light of the global financial crisis. For the purposes of clarity and focus, I have concentrated on those four competing research programmes that have been most closely associated with the renewed diversification of (and debate within) economic methodology since the crisis. In addition, future research will need to expand the scope of economic events in the light of which we assess the progressivity of distinct research programmes. Here I have focused solely on research programmes *in the light of the global financial crisis*. However, it is feasible that there are other relevant events in recent economic history in the light of which we need to reassess the methodology of economic research programmes.

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THE CASE FOR KIDNEY SALES

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Abstract. In every country where transplantation of human organs takes place, there is a shortage of transplant kidneys. As a result, many people die or suffer severely while waiting for a kidney. In almost all of these countries it is illegal to buy or sell human kidneys. This short paper is a contribution to the current debate about whether we should introduce a regulated living donor organ market in order to increase the number of available kidneys. The moral question central to this debate is whether we should allow people to sell their own body parts and have them removed while they are alive. Here it is argued that while there are possible objections to permitting kidney sales, they ultimately do not succeed in justifying a complete ban, and the current ban of kidney sales is therefore morally highly problematic.

INTRODUCTION

The number of people waiting for a kidney transplant by far exceeds the number of available kidneys. In the UK alone there are currently about 9000 people on the waiting list. It can be expected that only 2000 of these will have received a transplant organ within one year (see NHS 2012). The others will either still be suffering or will already have died. In other words, there is a critical undersupply of human kidneys. In recent years, one possible way of dealing with this problem has found increasing support among economists, medical doctors and moral philosophers. This is the possibility of allowing people to sell one of their kidneys and have it removed while they are still alive to make it available for transplantation into a person in need of a kidney transplant. This practice is illegal in almost every country. In the following I want to argue that the current ban of kidney sales is morally highly problematic, if not unacceptable, and that a regulated market for human kidneys should be introduced. There are four short sections to this essay. In the first section, some conceptual difficulties are clarified and three moral arguments for permitting organ sales are identified. The remaining three sections each deal with one of these arguments and possible objections against it.

THE ISSUE AT STAKE

There are, broadly speaking, four different categories of human organ donation. Organs can either be retrieved from deceased or from living donors. These organs can then either be donated directly to an identified receiver or can be donated without being directed to an identified receiver. The only category I am concerned with here is non-directed donation by living donors. This is because this is the most problematic of the four cases and raises many interesting moral issues.

It is also the category on which the market would likely have the greatest impact, because non-directed organ donations by living donors are so rare that this category hardly exists under the current system (see NHS 2012). Since I am only concerned with living donors/vendors, I am, for obvious reasons, only concerned with donations/sales of kidneys, rather than other organs like hearts or livers.

There are in general three moral arguments that can be put forward to promote the permission of kidney sales from living vendors:

- A)** Allowing people to sell one kidney would promote the well-being of people in need of a kidney transplant.
- B)** Not allowing people to sell one kidney is a violation of their rights to their own bodies and their autonomy.
- C)** Allowing people to sell one kidney would promote the well-being of poor people by giving them an additional option to raise funds.

Below I will discuss these three arguments, defending them against objections that have been and could be raised against them.

A. PROMOTING THE WELL-BEING OF PEOPLE IN NEED OF A TRANSPLANT

In the following section I want to present the idea that we ought to allow kidney sales, since by doing so we could help many people in need of an organ transplant. This is the most undisputed of the three above arguments and it rests on three premises:

1. It is our goal to reduce the suffering and increase the well-being of human beings.
2. If the supply of human kidneys were higher, we could reduce the suffering and increase the well-being of those who are in need of a kidney transplant.
3. If we permitted kidney sales, many people would want to sell one of their kidneys in order to raise money and the supply of human kidneys would rise significantly.

Therefore, we should permit kidney sales.

I believe that the only premise of the above three that could sincerely be contested is premise 3. In the following section I will discuss objections against it that have been put forward.

POSSIBLE OBJECTIONS

While it might seem obvious that a market for organs would increase the number of organs available for transplantation, the claim is not beyond doubt. Richard Titmuss (1970), for example, argued that, regarding blood donations, a market for blood would actually decrease the supply of blood because it would diminish altruistic motivations to donate. According to Simon Rippon (2012), this is also possible in the case of kidneys. This is a claim of empirical matter that I cannot satisfactorily examine in this philosophical analysis. From my point of view, however, there are at least two reasons to be skeptical about said claim.

First of all, it is hard to see why the mere possibility of selling would stop people from giving. In fact, introducing the opportunity to sell one kidney could even increase altruistic motivations. After all, giving away something one also could have sold is even “nobler” than giving away something that cannot be sold.

Second of all, the case of organ donation is a very specific one and very different from blood donation. Unlike blood donations, almost all cases of living kidney donations occur between family members or close friends. The so called non-directed donations only account for a fraction of all kidney donations. Out of 1009 kidney donations from living donors in the UK between April 2011 and March 2012, only 35 were non-directed (see NHS 2012: 32). It seems to be this small number that might be diminished by the possibility of selling kidneys. We can hardly imagine that family members would stop donating kidneys to their relatives if the mere possibility to sell kidneys were introduced. Moreover, even if a marginal decline in altruistic donations would occur, it seems reasonable to expect that an increase in the supply of transplant-organs due the introduction of a market would more than compensate for it. We can therefore reasonably expect that, as a consequence of permitting sales, the supply of human kidneys would rise.

Now it can be argued that even if the supply in kidneys rose, there would still be those who would not be able to afford to buy a kidney on the market. Is the current system not better for them? While this might be a valid objection against an unfettered market (like the black market that exists now), it is not an objection against a regulated market. Nearly all proponents of kidney sales, including myself, argue for the introduction of a regulated market, not a free market. We can imagine a system, where “there would be only one purchaser, an agency like the National Health Service (NHS), which would buy all organs and distribute [them] according to some fair conception of medical priority” (Erin and Harris 2003: 1). This way it could be secured that individual wealth does not play a role in the distribution of kidneys.

Let me now turn to the “supply side” of a market for kidneys. In the following I will argue that the permission of kidney sales is mandatory if we want to promote autonomy or a notion of self-ownership.

B. PROMOTING AUTONOMY OR A NOTION OF SELF-OWNERSHIP

There is a wide range of moral issues surrounding claims about physical autonomy and notions of self-ownership. In fact, this field of ethical enquiry is so wide that I can hardly do it justice here. For the purpose of this essay, let me assume that we have a strong intuition that every person has at least some form of a fundamental right to her body and that this intuition is more or less correct. What follows from this assumption is that every person has the right to decide what happens to her own body, unless there are other considerations that override this right (also see Wilkinson 2012: 5). There are, broadly speaking, two categories of considerations that could possibly override this right. Either the action in question is considerably harmful to third parties, or it is harmful to the very person whose right is the object of investigation. The argument based on a promotion of autonomy or notion of self-ownership, then, is the following:

1. Every person has some fundamental right to her own body and can autonomously choose what happens to it, as long as other substantial considerations do not override this right.
2. A ban on kidney sales is a violation of this right.
3. There are no substantial considerations regarding kidney sales that override this right.

Therefore, we should permit kidney sales.

POSSIBLE OBJECTIONS

Objections against this argument usually take up two forms. They either attack premise 2 or premise 3. Let me start with possible objections against premise 2.

When discussing this argument, it is helpful to consider who might want to sell a kidney. It is most likely that almost only poor people would want to donate one kidney in exchange for money. Losing one kidney is connected to a number of health risks. A sale “leaves the seller vulnerable to future problems if the remaining kidney becomes damaged or if its filtering capability declines” (Satz 2010: 196). Selling a kidney might be an act of desperation, an act of last resort. Therefore, it has been argued that sellers are likely to practically have been coerced into selling. According

to Paul Hughes (2009), this would then mean that they in fact did not act autonomously when deciding to sell one kidney. Hughes argues that this is a case of coerced consent and does not count as genuine consent. Such a choice then is deemed to be far from autonomous. And if they did not autonomously choose to sell but had been coerced into selling, a ban on sales does not violate autonomy. It also does not violate the right to one's own body, since sellers would not have done anything to their bodies if only they had not been coerced into selling.

There are a number of problems with this argument. First of all, suppose that the objection is true and many poor people in fact would be coerced into selling a kidney. This would still not show how a ban on sales would not violate the autonomy of those vendors who in fact would be making a voluntary and autonomous choice. It would still not justify keeping those, who, for example, already have enough funds not to be forced (by the above definition) into selling, from taking part in a transaction. "Our usual principles about autonomy and consent demand our assessing competence [to choose or decide] on a case-by-case basis" (Radcliffe Richards 2007: 257) and do not justify a complete ban. This objection then could be met by the introduction of a system which provides information and medical counseling to potential vendors.

Second of all, if it is argued that vendors would be coerced into selling by poverty, it could just as well be argued that donors under the current system are practically coerced into donating by other factors than poverty. "They are, metaphorically, coerced into it by the impending death of someone they care about," (Radcliffe Richards 2007: 258) by family pressure and so forth.

There is another problem with the objection against (B) on the grounds of coercion. It is highly inconsistent with the fact that it is often the financially worst of members of society that take up the most dangerous jobs. If we believe that poor people would be forced into selling a kidney we could just as well argue that poor people are forced into working in power plants, coal mines or into joining the army. They are coerced to do so, because these jobs pay exceptionally well, just as selling one kidney would (see Wilkinson and Garrard 1996). We would therefore also need to stop poor people from "selling their labour", and not just their kidneys.

But even if we, for the sake of argument, accepted the claim that potential vendors would be forced into selling by poverty, it does not justify a ban on kidney sales. The problem is that it would be poverty and not the mere possibility of selling a kidney that would force them into selling. If this was our belief, the right reaction would be to eliminate the force that coerces them into selling, namely poverty and not the possibility to sell. This is a very subtle point, but an important distinction to make. If a family of four was forced by poverty to live in a tiny room with no heating and no water, our natural reaction would not be to burn down the house they live in because we find their situation unbearable. The right reaction would be to help them overcome poverty. So if we believed that potential vendors of kidneys would only sell because

they are forced by poverty to do so, then the right reaction would be to help them overcome poverty and not take away their option of last resort. There are other important considerations, how the permission of kidney sales would influence the situation of people in poverty. These are dealt with in section (C).

We have seen that the objections against (B) on the grounds of coercion are not successful or at least highly inconsistent with other widely accepted practices, like unpaid organ donation or our acceptance of the fact that those who are financially worse off often take on dangerous professions. Let me now turn to objections against premise 3 of (B).

The remaining question is whether, in the case of kidney sales, there are more important considerations that possibly override a right to one's own body. As mentioned above there are two categories of objections. It can either be argued that the sale of kidneys is considerably harmful to third parties or that it is considerably harmful to those very people who would sell one kidney if it were allowed. Let me start with the first category.

Under (A) I have already shown that permitting kidney sales is at least very likely to help those in need of a kidney transplant and will therefore not harm them. What are other third parties that might be affected by the permission of kidney sales? A market for kidneys could be harmful to those who are not able to sell a kidney, even if they wanted to. Those could be the people who only have one healthy kidney. Is a market harmful to them? They would have a competitive disadvantage against those who are able to sell one kidney and raise money.

But is this objection strong enough to ban a market for kidneys? I do not think so. If we were to ban kidney sales for this reason, then we ought to ban all kinds of markets. We, for example, would need to ban a market for professional sports. Nobody should be allowed to play sports for money. Professional athletes have a very rare gift. They are so talented that they earn a lot of money by playing sports. Is that not unfair to the rest of us, who are not as gifted as professional athletes? Of course, becoming a professional athlete requires a lot of hard work, but it also requires a lot of talent and luck. Hard work alone is hardly enough. Should we ban professional sports because it is unfair to the rest of us, who are not as talented? Hardly anybody would agree to this. The claim, that a market for kidneys might harm third parties, does not appear to be strong enough to justify a complete ban of it.

Let me now turn to the second category of objections against (B), that a market for organs would considerably harm potential vendors and that the harm is so great that it overrides considerations of autonomy or self-ownership. I will discuss these objections in the next section, as they are the same as the objections against (C), that a market for organs would promote the well-being of poor people as potential vendors.

C. PROMOTING THE WELL-BEING OF POOR PEOPLE

In section (A) it has been established that a regulated market for organs is very likely to help those in need of a transplant kidney. In the last section it has furthermore been admitted that it is likely that only the poorer members of society would want to sell one of their kidneys. It is therefore important to consider whether a market would also benefit potential sellers. The argument for promoting the well-being of poorer people is the following:

1. It is our goal to alleviate poverty and to make poor people better off.
2. Giving poor people the legal opportunity to sell one of their kidneys would make them better off by giving them an opportunity to raise funds they currently do not have.

Therefore, we should permit kidney sales.

Although the first premise may not be undisputed by some scholars, I assume it to be true for the purpose of this essay. This is partly because it is a widely held view and partly because nearly all opponents of kidney sales only attack premise 2, which is highly controversial. In the following I want to present two objections that have been put forward against premise 2 and will argue that they ultimately do not succeed in justifying a ban on kidney sales.

POSSIBLE OBJECTIONS

The first objection against premise 2 is that, even though potential vendors have autonomously consented to selling one kidney, “they are simply wrong about the transaction being to their benefit” (Radcliffe Richards 2007: 259). This is the claim that selling one kidney can never be to the seller’s advantage. We all know the horror stories about a black market in organs, where people who have sold a kidney suffer severely or even die from the consequences of the operation. But these are stories about an illegal black market, where surgeries take place in horrible circumstances and where nobody cares about the organ donor after a healthy kidney has been retrieved.

Quite the opposite is the case regarding current legal practices of unpaid living kidney donation. This practice is almost unanimously encouraged by doctors all over the world. While living with only one kidney, of course, bears some risks, the procedure itself is deemed safe. There is no reason to believe that the mere fact of payment should change anything about this. If paid organ donation was carried out in the exact same way, with the same doctors and the same care, it would be just as safe as unpaid organ donation. Anybody who opposes legal and controlled organ sales on the ground that it is unsafe and dangerous for the seller must therefore also oppose unpaid

living organ donation. It is highly inconsistent to encourage organ donation but ban organ sales on grounds of safety. Let me now turn to the second objection against premise 2.

The underlying idea of premise 2 is that giving someone the option to sell one kidney while leaving all remaining options untouched cannot make him or her worse off.¹ According to Simon Rippon (2012), this is false. Rippon argues that, while it might be true that it would be better to sell one kidney, given the fact that one had the option, it does not follow that this is the best option overall. He claims that not even having the option to sell is better than having the option and selling. The preference structure, he implicitly develops, then, is the following:

1. Not having the option to sell one kidney.
2. Having the option to sell one kidney and taking it.
3. Having the option to sell one kidney and not taking it.

If this were correct, giving poor people the opportunity to legally sell one kidney would actually make them worse off and premise 2 would be false. Rippon creates a scenario where having the option to sell one kidney and refusing to take it would have devastating consequences. As Richard Dworkin wrote in another context: “Once I am aware that I have a choice, my failure to choose now counts against me. I now can be responsible, and be held responsible, for events that prior to the possibility of choosing were not attributable to me” (as quoted by Rippon, Dworkin 1982: 50). According to Rippon, kidneys would then count as economic resources, as “collateral”, that can be taken away from someone who is in debt, just like a house or a car. Kidneys would be commodified. This would lead to social and legal costs for failing to sell them.

This is a valid objection against an unregulated market in organs. It, however, does not pose any problems that could not be dealt with by carefully regulating a market for kidneys. Even if kidneys were seen as “commodities”, they still do not have to count as collateral. In most countries there is a range of commodities that cannot be taken as collateral (basic shelter, clothing, or even a TV). Just as nobody can be legally forced to sell these objects to pay off his or her debt, or just as nobody can be legally forced prostitute himself to pay off his or her debt, nobody should be legally forced to sell a kidney. As many other objections, the objection raised by Rippon can therefore be dealt with by regulating, rather than banning a market for kidneys. The economic principle that, “by allowing individuals to either barter or sell something, we increase their level of well-being” (Dworkin 1994: 9. 156) remains untouched by the objections against (C).

¹ It is important to note that I do not argue that additional options can never make the person choosing worse off. I simply claim that this is true in this specific case.

CONCLUSION

We have seen that almost all objections against a market in kidneys are either wrong, inconsistent with a number of other policies, or can be met by regulating, rather than banning a market. We have furthermore seen that a regulated market would benefit people in need of a transplant kidney and people who are willing to sell one kidney. It has also been shown that the current ban is at least very likely to violate autonomy and considerations of self-ownership. If there are no more important considerations than the ones that have been dealt with here, I believe that the demand for a regulated market for kidneys is morally warranted.

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AN INTERVIEW WITH JAMES LADYMAN

BY SOPHIA CHO AND NELSON HOSLEY

James Ladyman is the Head of Philosophy Department at Bristol University. His work has primarily been in philosophy of science, especially the philosophy of quantum mechanics and the debate about scientific realism.

SC&NH: One general observation of your book *Everything Must Go* (co-authored with Don Ross; Oxford University Press, 2009) is that the center of gravity of the philosophy discipline has moved too much in the a priori direction, specifically in metaphysics. Could you give us some background on this claim?

It happens periodically in the history of philosophy that people find themselves reacting against scholasticism, by which we mean - addressing issues as they are defined by the tradition, without regard for things that are going on outside of it.

That is by no means universal in philosophy. Lots of people even in metaphysics actually do their best to keep up with science. Its a polemical point to say that I think that the *central gravity of the discipline* has moved a bit too far in the a priori direction; that is, too far in the direction of addressing questions that have been internally set up by the tradition rather than looking in the outwards direction.

At the point that we wrote *Everything Must Go* we thought that things were just overall too much in that direction. And I still think that.

I found myself realizing that the questions that I was interested in weren't being discussed and the questions that were being discussed I wasn't interested in. I began thinking "is this because I'm not interested in philosophy or because I'm not interested in metaphysics?". Metaphysics is a question of 'what the world's like' - its some kind of general or basic, special version of that question - and i'm interested in that. The questions that interest me are special science questions: like 'what's a utility function?', 'whats a market?', 'what's a species?', 'what's an atom?' All these questions have to do with the relationship between and compatibility between ontologies at different levels. For example, how can there be heat while having the story we have about the underlying stuff that makes heat. What is the relationship between the physical stuff that we talk about and the stuff in the special sciences?

"If you brought Kant into the 21st century, I imagine that he would completely ignore what was being done by philosophers in metaphysics, all he would want to do would be to learn about the science."

When philosophers tackle questions about how everyday material objects arise out of whatever else the little bits are - that's the bit they seem to do the wrong way. Like in saying that a table is a collection of particles. Science always gives us a *dynamical* story about how something like a table is made of particles. Too often when we think about the composition of macroscopic

things by microscopic things, we think about a kind of spacial boundary with a load of little particles inside of it. It is more evasive than that.

So are questions about everyday things, such as tables and baseballs, misguided or distractions?

Insofar as I think people are asking the wrong questions I think they are distracted and misguided. But we could say “didn’t they raise a really important question, which is ‘what is the principle that governs composition (?)’ or a related question of ‘when is it true that some simples are composed of something else

“One can definitely notice the coincidence of the rise of theism within mainstream analytic philosophy and the rise of analytic metaphysics.”

(?)’, whereby we mean by ‘simples’ things that are *not* composed of anything else.” This is in contrast to things that are composed out of other things.

What I think is problematic about that is the completely non-contextualized idea of simples, where there’s one final set of simples, and one hierarchy of composition out of them. If you ask how a market works you might think that a market is a compositional entity, which is to say that you cannot have it without its parts. But the simples of markets, something

like individual transactions for instance, is a very different kind of simple than the simples of, lets say, population genetics where the simples might be individuals (or we may think of an individual as a bundle of heritable traits so the simples may be the heritable traits). This in turn is a very different kind of simple than atoms that compose gases. Here, we don’t even think from the point of physics that atoms are simple. Nonetheless any story in thermodynamics is not going to go down to superstrings but to the level of atoms because thermodynamics ultimately comes down to a kinetic theory of gases, particles and collisions at that level, where particles are the molecules of the gas in question.

Somehow the assumption seems to be that there are the ultimate physical simples and everything is composed out of them. So what I want to say is that I don’t really see how markets can be ultimately composed out of whatever makes up quarks. The other thing is that it’s just not obvious that there *are* simples in physics. So spending so much time is not terribly helpful. Because we end up in a position where we are saying there are really gases and there is really heat, but there aren’t really markets.

So really quickly we’re going to end up with the idea that most of what the special sciences talk about is not really a general ontology. Only kind of direct aggregates of physical things, spacio-temporally located, can have a proper ontological existence *at best*. You may end up with the conclusion that simples is all there is. Even among the physical stuff only the simples exist. But that is ignoring all the interesting questions that all the special sciences throw up

about ontology. Then you want to say that they aren't really ontological questions. But they sure look like ontological questions.

I don't want to say that it's just conceptual what markets are. That makes it sound like we just define what markets are, but we actually *learned* what markets are. We've *learned* that its useful and possible to describe the world in terms of markets.

There are many really interesting phenomena in special sciences. There is enough work keeping an army of philosophers at the special sciences and how they ultimately interrelate, but we're worrying about whether matter is ultimately gunky, made of tiny spacial parts that can't be cut in half anymore. This just doesn't seem to have anything to do with reality to me. I don't want to disparage the work that any individual philosophers do (though we did pick some examples in the book because we had to) - it's more about the centre of gravity of the

“People have been going around for years saying that I stated ‘there are relations without relata,’ which I never said.”

discipline. If you want to think about how simple physical parts could compose, if there were such things - fine, I don't want to stop you - but if that becomes a dominant question and there is a methodology for answering it...I suppose one of my main worries is that you can open an article or book in metaphysics and can find no indispensable information in it that came out of the advance of science in the past few

hundred years. That's a bit of a worry. I cannot imagine any of the founders of metaphysics wanting them to do this; Descartes as an example, but also the Presocratics, Kant...They all thought they were at the cutting edge of what could be known at the time. So, if you brought Kant into the 21st century, I imagine that he would completely ignore what was being done by philosophers in metaphysics. All he would want to do would be to learn about the science. He based his whole metaphysical theory on knowing about Newtonian mechanics, which he used to teach and which was really important to him.

So if we said, okay, you are now in the 21st century Kant and guess what - we've now got quantum mechanics and relativity - I imagine that he wouldn't want to do anything else until he'd understood them.

But individually, loads of metaphysicians do try to read up on and understand the science. But it's just the *discourse* - what we were complaining about was a certain kind of *discourse* that seems not to be conversant of it. I think that has changed to some extent, but then sometimes the worry is that it is paying lip service to the scientific image and operating a domesticated version of the scientific image.

As a practical question, for students of metaphysics, what type of education program emerges from this? To what extent should philosophers or philosophy students involve themselves with the special sciences and how practical is that?

Many people have some area that they know

about and they do their philosophy informed by it. That's often what we call philosophy of science, but it's really just metaphysics. So many people who do philosophy of mind know about psychology or neuroscience, and they're doing metaphysics of the mind in light of that. There are all those different bits of metaphysics that you can do knowing about something. You don't have to know about everything.

The big integrative project that we try to do for ourselves in philosophy is pretty hard to do. It's something that needs to be done in collaboration. Somehow philosophy has to be slightly more of a social product. If you look at the way science works now you often have to have teams of people with different bits of expertise. You might have one person who knows about genetics and one person that knows about behavior, and another person's really good at doing the stats or something and their producing papers together - because there's so much to know and things are so specialized that it's hard to do.

The advice to philosophy students is [to realize that] it's really difficult. It's hard for me to advise someone who only wants to read about philosophy and isn't interested in anything else. In that case, I would say to do philosophical logic. Or history - some people do really good *a priori* work analyzing concepts but its often semi-historical - that you trace the evolution of ideas and concepts like 'markets' by having gone back and read a load of works with the benefit of hindsight.

Do you see your work as in the spirit of the

enlightenment?

We very explicitly say that what we do is in the spirit of the enlightenment, without being pretentious - we think that there is a great unified project of finding out about the world through a combination of both empiricism and the best that is produced by pure thought, which is mathematics, logic, conceptual analysis - the

“The big integrative project that we try to do for ourselves in philosophy is pretty hard to do. It's something that needs to be done in collaboration. Somehow philosophy has to be slightly more of a social product.”

traditional analytic philosophers idea of conceptual analysis is an important part of it. But that should be a kind of unified project. And so we want to inform what we say about metaphysics by physics and behavioral science and everything in between. But yeah, agreeing with Descartes that how things seem to us on the basis of our senses isn't a good clue into how they really are.

You mention that intuitions are culturally specific and you also mention that a 'domestication of science' is often if not always suited for ethical and political interests. How do you feel that your views compare with those that elaborate on intuitions and operate on a 'domesticated image of scientific theory' in terms of

being value laden?

That's really difficult. I think of materialism and there is a strong current in materialism going back through Lucretius, through Lenin that says that there is a political reason why you should get your ontology right - a political, ethical reason. So Lucretius thinks that you want to liberate people from superstition and religion because it controls people's lives. That is important to him. And there's a strong tradition of people like Lenin saying that religion is the opium of the people... if people believe in all this non-material stuff than they can be

"I don't really know the best way of getting the idea of structural realism across is. You've got to look at examples and see that, look, theory change can be like that and we could be just as wrong about that kind of thing, but we can be sure about our equations."

suckered into thinking that there's something else other than their material existence that matters. And *that* might stop them from doing something about the fact that we all have one life and, you know, if people grow up in poverty and have really poor life opportunities and then die young because of bad nutrition and environmental degradation - *that's it, that's* there only life, they don't get another go. For some people that's a strong motivation for materialism.

Whether or not you then agree on some version of materialism is a separate question. Of course lots of people are completely opposed to Marxist Leninism say exactly the same thing and then say that its actually the best way of lifting children out of poverty.

In fact Don Ross (the co-author of *Everything Must Go*) works in development economics - its what he really cares about is lifting the people in Africa out of poverty, or anyone out of poverty, and he thinks that somehow the materialism is part of what informs the fact that there isn't any other project to care about - the salvation of your soul and of other people's souls is not a genuine project for him. The salvation of their material conditions, is.

Then at the same time, there are people in theistic traditions who would freak out if you tried to suggest that there is anything contradictory between theism and caring about child poverty. Fom the *Old Testament* prophets that are massively concerned with justice, onwards.

I'm not very confident about making a direct connection between values here by saying something such as "well, if you're doing analytic metaphysics then you're not living in the real world and therefore you don't care anything about child poverty." It's just not that direct. At the same time, I definitely have noticed, one can definitely notice the coincidence of the rise of theism within mainstream analytic philosophy and the rise of analytic metaphysics - I mean those are two noticeable trends of the last two decades. I mean the fact that first of, metaphysics came back on the agenda, and

now theism is back on the agenda in a way that it wasn't before. But those two things may have nothing to do with each other - it could just be that theism has something of a resurgence in intellectual circles independently. So it's really difficult. I'd be very wary of making any real links here.

Is there any place you feel that your views have been misunderstood?

It's a common misrepresentation that *Everything Must Go* is an attack on metaphysics. Its not. It's an attempt to do metaphysics.

Another misunderstanding, and maybe we're responsible for this - is that we were so polemical [arguing against *a priori* or *analytic* metaphysics]. No one would have talked about it if we'd been much more mild about it so the situation did justify a kind of interpretation that was a little bit polemical and a little bit extreme - but our views are a more nuanced than the kind of polemic. I think it's really to do with shifting the center of gravity and getting us to think about it. And also about what the methodology is. Just get those questions talked about rather than completely eliminating everyone doing something *a priori* and suggesting that everyone has to be a scientist or something. And people have been going around for years saying that I say there are relations without *relata* which I never said.

One very important misrepresentation is one that we ourselves are responsible for when we say that 'everything must go,' because the slogan [for our work then becomes] that "there are no things," but actually if you read what

we say, we say there are things like tables, there are markets, there are utility functions, but what we end up saying that a thing is is just the real patent idea of 'what a thing is' rather than "it's some little material like extended stuff or something made of little parts like that." It's more against a philosophical conception of a material thing or individual applied to physics and science than actually - in that sense. In that sense, we're really kind of Moorian idealists, right. We don't want a philosophy that says there are no hands and that baseballs can't break windows. And then we try to work out a metaphysics compatible with science that allows us to say that there isn't anything more to 'being hands' than there being phenomena that go together in a way that makes quantifying over hands massively reduce your information task. You can track the dynamics of the world at a level that those degrees of freedom rather than all the little parts' degrees of freedom and that's what we say about everything that exists. It's on that basis that everything exists.

What's the best decider of what exists?

Usually science, but often it's just folk practice. If the folk are immersed in doing something and getting around in the world - if you think about cooking or something, it has its own ontology. For example from a botanical point of view, a lilly, a leek, and an onion are exactly the same thing, but from a culinary point of view they're not. That has to do with indispensable parts and ones explanatory grip of the world and that's a matter of degree. Its domain relative and scale relative. There are scales at which tables don't exist and scales at which atoms don't exist. You can't make sense

of a market as existing from one iteration of transactions - it just wouldn't be a market.

There is an argument that structural realism is really misguided and what we really need to look for is invariance over the history of scientific theories and entities. Do you see the relative merits of this type of argument?

Of course we're interested in what is invariant in our understanding. For example, when we think of 'light' with Maxwell's or Fresnel's optical aether theory of light and even like the particle and ray theory of light, we might ask what's invariant? - Everyone thought that 'light' is a thing that refracts and there's interference, reflection phenomena and polarization phenomena, refraction phenomena - these theories have been better and better descriptions of all of those optical phenomena. And we can further ask what is optics? We can say it's the behavior of *this* thing, *this* entity - 'light' - it's a bit of the world. So if you're a structural realist and you're saying that there's no such thing as light, then I'm going to say "no, I'm not saying that there's no such thing as light." So if that's what I take to be advocating when I advocate structural realism then I must not do a very good job of explaining myself.

No 'light', we can say, is an entity and our theories try to tell us more and more about it. So insofar as structural realism says "no, we only know about structures," that means structuralism is denying that obvious fact. But that's not right. I think the point is supposed to be "whatever particular account we've got right

now, considering that the ultimate constituents of the nature of light are quite likely to be modified and further developed, then the thing that we can be most confident about is that the mathematical structure that we arrive at in our descriptions of it become more and more defined such that we can recover ray optics from wave optics and Fresnel's optics from Maxwell's electromagnetism and Maxwell's electromagnetism from quantum field theory, and so on. Well it's changing our mind quite a lot about what the ultimate nature of light is. And we should expect that to go on in the future.

We shouldn't believe that necessarily there is an ultimate nature to light. We just should assume that that this process will ever end. Maybe it will but will we be able to describe within the resources manifest in its image the nature of that thing and almost certainly not. Already now if you want the best story of what light is you have to read quantum electrodynamics. According to my friend Mark Dennis, he used to give a certificate to people who he thought were qualified to use the word 'photon', because what a photon *is* is really difficult to understand. It's *not* just a kind of little particle, like a little thing: it's just not. That's what structural realism is about.

One might say "that's not structural realism if you believe in entities" - I say fine, I don't care about the name. Call it "structural realism," call it whatever you'd like, I just want to get clear about what we should believe about the world. We shouldn't believe that the view of realism that arose out of the failure of logical positivism wasn't a kind of "there are these

entities, we have these terms, they refer to them...” - no, it’s much more complicated than that. How the term ‘photon’ refers is much more complicated than that. It’s not really analogous to how names refer.

There was something right about the reaction against realism that says that it’s too naive, it’s not taking account of how much ontology would change. I still think that’s right.

I also think it’s right that most entities, although they might be entities, they’re scale relative. Let’s move from light to atoms - they don’t exist now at the quark scale. There’s nothing like that there. They’re low energy approximations that you only get by coarse graining and leaving out details.

... We can only say what’s been invariant up until now. So maybe the argument you mentioned against structural realism is about how structure changes too when there’s theory change - which is true. The totally wrong way to think of this (and another common misunderstanding to continue from above) is to think that that we’re saying that structure doesn’t change and nature does. That’s not what we’re saying. Of course structure changes. The structure of Newtonian mechanics is not the structure of Special relativity.

For instance, I can see how the equations of Fernel are recovered in Maxwell’s theory. I can see how Maxwell’s theory is recovered in a theory of quantum thermodynamics. I *can’t* see how the kind of *entities* are recovered in the limits of other entities - that doesn’t really make sense.

I don’t really know the best way of getting the idea of structural realism across is. You’ve got to look at example and see that, look, theory change can be like *that* and we could be just as wrong about that kind of thing, but we can be sure about our equations. We’ve got limited cases. And basic causal regularities and phenomenological laws. It’s always going to be true that water dissolves salt. The theory is that you’re not going to have an completely different understanding of what the ultimate nature of ‘salt’ is. It’s not very satisfactory.

Take Newton’s metaphysics. There’s clearly a bigger gap between Newton’s metaphysics and ours with him having absolute space as one thing, time as something completely different, material particles different again, moving around in absolute space - that’s completely different than whatever we think the world is like with quantum fields, etc. But yet there’s a much closer connection between Newton’s mathematics and ours now in the sense that we can recover all his laws of limited cases - they’re still there implicit in the structure that we have now. Though with all of this extra-complicated further structure.

Is it because we like simplicity? - no, I don’t think that it is because we like simplicity. Even in principle we can’t recover higher level stuff without higher level descriptions: you have to coarse grain. So if you just describe everything in the terms of quantum theory you won’t ever get ‘baseballs break windows’ - there won’t be any baseballs, there won’t be any windows. And it’s not up to us which coarse grainings are the ones by which you can then state laws and equations that describe change of state.

BOOK REVIEW: BRIAN LEITER'S *WHY TOLERATE RELIGION?*

JOSEPH WOLFE

MSc Philosophy and Public Policy, LSE

In March of 2012 I attended the Reason Rally on the National Mall in Washington, D.C., which was intended to be a sort of Woodstock for secularists and religious skeptics. A number of prominent irreligious people spoke, and Richard Dawkins gave the keynote address. I very distinctly remember him urging attendees to mock religious believers in public, because I found the call startling. Religion, Dawkins said, “makes specific claims about the universe which need to be substantiated and need to be challenged and, if necessary, need to be ridiculed with contempt.”

Dawkins and other so-called New Atheists have gone further still. The 1925 Scopes Monkey Trial in the United States overturned a \$100 fine on a Tennessee public school teacher who defended evolution. Some New Atheists want to push for penalties the other way: they say science teachers, physicians, and even applicants to certain science graduate programs should be excluded from these positions if they admit to being Creationists. If they reject cornerstone scientific theories and methodologies, an argument goes, then how can society certify them to teach or practice modern science *qua* scientists?

This sort of approach is what sprang to my mind when I saw the title of Brian Leiter's *Why Tolerate Religion?* (Princeton University Press, 2013). But Leiter—a law professor, blogger and Nietzsche scholar—isn't writing to rouse a crowd. He keeps his University of Chicago Professor of Jurisprudence hat on quite snugly for the duration. In fact, you'd be better off forgetting the title altogether: “Why Tolerate Religion?” is a bit misleading. Instead, the book gives a very narrow and considered argument against granting religious practices special status as an object of toleration in a liberal society, above and beyond other claims of conscience.

Leiter's argument is insightful and provocative. As he notes, a wide array national founding documents, as well as international documents like the Universal Declaration of Human Rights, explicitly protect religious beliefs or religious beliefs and other claims of conscience. Other Western democracies treat religious beliefs as special in practice. But what characteristics do religious beliefs have that make them worth distinguishing from other types of sincerely held beliefs?

A primary test case here is the Canadian Supreme Court's decision in *Multani v. Commission scolaire Marguerite-Bourgeoys*, which upheld the rights of a Sikh student to carry his sharp ceremonial knife, or *kirpan*, to school. Yet, Leiter reasons, it is unlikely that a similar exemption would have been granted to a student whose family had a longstanding but nonreligious tradition of presenting a

potentially dangerous “coming of age” knife, to be worn at all times, to its male offspring.

In Leiter’s distinction, toleration could be practical—meaning we have no effective and reliable way to end a disfavored group’s practices, or it could be principled—meaning that there are moral reasons to put up with those practices even if it were possible to end them. Leiter is happy to grant principled toleration to all conscientious beliefs, including religious beliefs, with the constraint of some version of Mill’s Harm Principle. However, he claims some additional justification is needed for a practice to qualify for exemption from the laws that govern everybody else in a society.

Leiter proposes a definition of religion that involves three major characteristics. First, in his view, religions issue in categorical demands on action, as experienced by believers. Second, they do not ultimately answer to evidence and reasons. Third, religions offer adherents existential consolation, including ways of understanding and bearing suffering and death.

He considers each characteristic from deontological and utilitarian perspectives, principally those advanced by John Rawls and John Stuart Mill. Among others, the discussion includes Rawls’ veil of ignorance argument for protecting liberty of conscience, an argument about enabling individuals to pursue their own conceptions of the good, an argument about the utility of religious existential consolation, and an epistemic argument about the place of religious beliefs within the marketplace of ideas. Leiter also devotes a chapter to Martha Nussbaum’s conception of respect as the basis of religious toleration, where he distinguishes between minimal respect for an individual’s moral right to hold a belief, and affirmative respect for that person’s beliefs themselves.

Leiter concludes that religion succeeds as a belief system that should be tolerated and minimally respected as a claim of conscience. It does not succeed, however, as a system deserving of special consideration over and above other claims of conscience.

Consequently, he favors a No Exemptions approach to claims of conscience within a societal culture, where a practice could violate Mill’s Harm Principle. Thus, he says, when it comes to carrying a knife to school, both the Sikh boy and the other boy should be “out of luck.” The French policy of *laïcité*, however, which has banned students from wearing many religious symbols altogether, is a case of impermissible intolerance. The *hijab* does not convincingly violate Mill’s Harm Principle.

Yet defining religion is an ambitious project in itself, and it isn’t clear that the definition offered in this book can ultimately hold up. Many secular, deontological moral systems would seem to issue in “categorical imperatives,” for instance, and religion is not unique in this regard. Second, those who do not share Leiter’s epistemic views on the relationship between scientific knowledge

and religious beliefs, and the nature of evidence in each, will have plenty of room to dispute his claims. Leiter ignores the fact that many theologians and religious people have rethought traditional views in light of scientific findings—Christians, for instance, who subscribe to the theory of evolution. Third, not only does Leiter give short shrift to the significance of religious existential consolation in many people’s lives, but there are many nonreligious perspectives which could provide consolation to adherents.

With the attempt to offer a categorical definition of religion, Leiter must also set aside the diversity of religions themselves. Each system of religious beliefs might have vastly different relationships with his three characteristics. They might have different relationships with common epistemic and moral arguments advanced in the public sphere. And we might find different religions, or religious practices, worthy of differing degrees of respect.

To be clear, Leiter’s argument is that nonreligious belief systems that share the characteristics of religion are not generally supposed to require special exemptions from the law, while religion is. Yet this point becomes muddled when Leiter suggests that pure liberal neutrality is undesirable. Society, he claims, should pursue a general Vision of the Good with aims like health, public safety, and individual well-being.

It is here that Leiter distances himself from the New Atheists. Though the question “Why tolerate religion” doesn’t seem the type to occur to a religious devotee, he also does not clamor for Dawkins-style reason, science, and secularism as the only acceptable basis for a societal culture. Society might be Muslim or Catholic, complete with religious state schools, so long as each person is free to live according to her own sincerely held beliefs and to attend the school of her choice. As Leiter writes, “British establishment of Anglicanism provides the natural counterpoint to French *laïcité*, as an example of an establishment of a *religious* Vision of the Good that does not impinge on the principle of toleration.” The United Kingdom funds some non-Anglican religious schools, for instance, and other religious institutions and their followers do not have their rights restricted.

Yet it is unclear how, in practice, institutionalizing a particular public religion could be considered anything but a type of special consideration. More than minimal respect for Anglicanism in Britain is implied through its practice by the royal family and the role of the Prime Minister in appointing Church of England bishops. Despite the obvious and significant efforts at equal treatment, the beliefs of religious minorities do not receive the same types of sanction.

While Leiter’s focus in this book is on whether there are principled reasons for special treatment of religion, his stance here does seem to be a nod to the practical and historical dimensions of religious toleration, which he otherwise sets aside. Indeed, it is interesting that he does not consider

the institutional characteristics of religion more strongly, even independently of the weight these institutions have in the public sphere. His decision to set aside the histories of conflict that have given rise to special protections for religious liberties in the first place, meanwhile, leaves open the possibility that pragmatic concerns might be needed to bolster his conceptual account in real policy making.

Nonetheless, *Why Tolerate Religion?* is a densely argued book, and on its narrow central inquiry—Are there principled reasons to treat religious beliefs differently from other claims of conscience?—Leiter makes some interesting clarifications. Though the arguments in *Why Tolerate Religion?* are rich for the irreligious, the book is open for debate from both sides. Its discussion of the contours and limits of religious tolerance make it an insightful and relevant contribution to modern applied philosophy.

ABOUT LSE

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THE DEPARTMENT OF PHILOSOPHY, LOGIC AND SCIENTIFIC METHOD AT LSE

Karl Popper, the founder of the Department of Philosophy, Logic and Scientific Method, moved to the LSE after publishing *The Logic of Scientific Discovery* (1935), *The Poverty of Historicism* (1944) and *The Open Society and its Enemies* (1945). Popper was one of the foremost defenders of democratic liberalism in political philosophy, which made the London School of Economics and Political Science a natural home for him. Rational criticism was the cornerstone not only of his political philosophy but also of his views on the nature and practice of the natural and the social sciences. It was through Popper's efforts that falsificationism entered public consciousness as the defining criterion of the nature of science. Both he and his successor Imre Lakatos, who joined the department in 1960, were instrumental in shaping 20th century philosophy of science. The Department's longstanding reputation in the philosophy of natural science is now matched by its strength in the philosophy of economics and social science, most notably in methodology and rational choice theory and philosophy of public policy. We are committed to teaching and research that makes a significant difference not only in philosophy and the philosophies of the various sciences, but also in the practice of the sciences themselves—from economics and political science to physics, biology and medicine. The range of postgraduate masters degrees offered by the Department reflects its strengths: it offers four different MSc programmes, each dedicated to the teaching of one of its particular specialties. The MSc programmes are complemented by a PhD programme which creates an active intellectual environment with students working in philosophy of science, philosophy of physics, philosophy of economics, philosophy and public policy, rational choice and scientific methodology.

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EQUALITY STATEMENT

The Department and the School are committed to diversity and equality in education and employment. The Department has a number of exceptionally successful individuals from underrepresented groups, and especially seeks applications from women and minority groups for student, research and faculty positions.

CONTACT INFORMATION

philosophy-dept@lse.ac.uk

www.lse.ac.uk/collections/philosophyLogicAndScientificMethod/

DEGREE PROGRAMMES

3-year Undergraduate Programmes:

BSc Philosophy, Logic and Scientific Method

BSc Philosophy and Economics

BSc Politics and Philosophy

12-month Masters Programmes:

MSc Economics and Philosophy

MSc Philosophy of Science

MSc Philosophy and Public Policy

MSc Philosophy of the Social Sciences

MPhil and PhD programme

MSc DEGREES

We will consider applicants who will have finished a Bachelor degree in any discipline with sufficiently strong results (for UK applicants, Upper Second or better) in any discipline and who have a considered interest in the area covered by the MSc.

The MSc Economics and Philosophy

Taught jointly by two internationally respected departments at LSE, this degree is unique in offering students the opportunity both to further their studies in economics and to acquire a deeper understanding of the nature and significance of its methods, normative implications, and conceptual foundations. The degree offers a good preparation for doctoral research in both economics and the philosophy of economics and the social sciences, as well as

for employment in such fields as financial and economic journalism, consulting and policy formulation. Applicants must have a strong undergraduate training in economics.

The MSc Philosophy of Science

This degree attracts primarily two types of students: those with first degrees in a science who would like to study the methods and foundations of science, and those with first degrees in philosophy who would like to specialise more in philosophy of science. As well as further developing the virtues of clear thinking, analytical argument and appreciation of the rules of evidence that are so useful in a range of high-level occupations, the course provides a solid foundation for doctoral work in the philosophy of science and related fields, and for employment in such fields as science administration.

The MSc Philosophy and Public Policy

Taught at an institution which is a major centre for national and international public policy debates, this degree provides a foundation in the conceptual and normative questions underlying public policy formulation. It prepares you for policy-oriented careers in intergovernmental, governmental and non-governmental organisations as well as for Ph.D. work in philosophy or related disciplines. It is distinctive in three ways. First, it is resolutely interdisciplinary. We take philosophical analysis to be continuous with scientific approaches to the study of political, social and economic problems. Second, it offers a thorough background in the classics of moral and political theory, which students learn to apply to issues in public policy. Third, students have

access to a wealth of courses and resources at the Philosophy Department and LSE generally that are relevant for the philosophical analysis of public policy.

The MSc Philosophy of the Social Sciences

Taught by a department recognized as one of the best in the world for Philosophy of Social Science, this degree offers students the opportunity to further their study in either the philosophy of social science or the philosophy of economics, in addition to studying one course selected from the many social science offerings at the LSE. Some of the questions that will be addressed during your studies in the MSc in Philosophy of the Social Sciences: Are the social sciences truly scientific? Are they objective? What accounts for the diversity of methods in the social sciences (and lack of agreement as to which ones ought to be followed)? Do evolutionary explanations of social phenomena provide an accurate account of why societies are the way they are, or are such explanations merely unverifiable just-so stories? What relations exist among rationality, choice, action, and interpretation?

Rerum Causae

JACOB NEAL

Why we shouldn't all be eliminative materialists (yet): understanding the failure of Churchland's argument

BENJAMIN TEREICK

What Sally Haslanger's 'daughter' should believe

JANIS SCHAAB

Is economics normal science: Do economists share a paradigm?

LUKE SPERDUTO

Markets, maths and value: Smith versus Jevons

PAUL HUFÉ

What do we owe the global poor?

VASILEIOS NIKORELOS & CHARLIE DJORDJEVIC

I want to drink paint: a rationalized account of desires

HARVEY DANIELL

The global financial crisis and the methodology of economic research programmes

KORBINIAN RUEGER

The case for kidney sales

SOPHIA CHO & NELSON HOSLEY

Interview with James Ladyman

JOSEPH WOLFE

Brian Leiter's Why Tolerate Religion?