

## **Do Intuitions about Reference Really Vary across Cultures?**

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### ***Abstract***

We discuss whether intuitions about reference really vary across cultures and how these variations relate ultimately to the McTaggart A-series. We conclude that much more work needs to be done, and suggest how it can.

### ***Introduction***

In the present note we seek to establish and promote new results, in some cases using experimental philosophy (a subject which we have considered for many years (Yates, 2008b)) where it becomes necessary. These new results at present usually relate to the McTaggart A and B series and to the study of time (Yates, 2008, 2008a). So the aim is not to criticise existing X-phi results - I am truly pleased and glad that the field is obtaining a foothold - but in order to obtain practical results it is necessary to point out when or where more work needs to be done, to establish usable answers to existing problems, whilst still keeping research not too lengthy.

### ***Details***

Questions like the one of the current heading query above (Machery, 2009) have been repeatedly raised. Of course they go right back to Mill, Kripke, and more recently Machery (2004), Sytsma (2009), Lam (2009), and now yet again Machery (2009).

Of course, others, in particular Frances (1998) and Sosa (1996), have worked to resolve Kripke's puzzle. In fact on the face of it, the exposition of Frances (1998) on Millian theories sounds fine to me, up to a point. For present purposes, perhaps in the exercise in Machery (2004), there are at least two issues, as many philosophers might say. One is whether or not the name "Godel" in Kripke's fictional scenario has to have the same meaning as the actual name. The second issue is whether acceptance of the coherence of the fictional scenario already commits us to Millianism.

At this point we need to consider Machery (2004) which plainly states that two views, the descriptivist view of reference and the causal-historical view of reference, have dominated the field. In any case, certainly the work of Machery et al becomes important if we are to consider the latter classification. At this very point the power of X-phi arises, whether or not Machery's eventual conclusions are correct.

In this connection Sytsma (2009) points out an objection raised by Sosa (2007). In fact in footnote 5 of Sytsma (2009), Sytsma points out he considers that " "defense against experimentalist objections to armchair intuitions is anchored in the fact that verbal disagreement need not be substantive". In this context, if the results of Machery (2004) reflect divergent interpretations of the probe, then it is not clear that the variability shown reflects differences in the semantic intuitions at issue for the philosophical debate. One issue such an objection raises is how to decide where the burden of proof lies. Sosa continues: "The experimentalists have, so as to show that supposedly commonsense intuitive belief is really not as widely shared as philosophers have assumed it to be. Nor has it been

shown beyond reasonable doubt that there really are philosophically important disagreements rooted in cultural or socio-economic differences". Although we cannot argue the point here .....". Sytsma thus admits that they are not arguing with Sosa but goes on to claim roughly that Sosa is seeking too high standards of proof. Now I would say the problem may be more that X-phi practitioners need to actually reach believable standards of proof with economic amounts of data.

The problems with Kripke have frequently been discussed by the aforementioned experimental philosophers. (Machery (2004, 2009), Sytsma (2009), Lam (2009)) actually seem to be in essence doing armchair work, once the formality of doing brief surveys has been met. In short, the armchairs remain (at least partly) unburnt !



To give a related example of what I am saying, Knobe et al (2009) says of compatibilism and incompatibilism "In our view, the data presently available is not sufficient to decide between these contrasting hypotheses. In short, there is still much work to be done. And while the problem of free will has historically been the prerogative of philosophers, the current study suggests that researchers everywhere who investigate folk psychology, folk physics, and moral cognition have contributions to make in solving this particular puzzle". Now Knobe's work was carried out in United States, Hong Kong, India and Colombia and the authors still have that view. As far as I know only domesticated American and Hong Kong cases were dealt with by Sytsma and Cantonese diaspora cases by Lam and Machery. And Hong Kong is compact, developed, relatively modernised (with a better modern skyline than Manhattan) and not typically Chinese as much of mainland China is.

Of all the above cases, diaspora cases do not sound the best cases to use to attack Kripke's argumentation, as the cultural references presumably refer partly and possibly primarily to the host country, normally the USA for these diaspora studies. It is all very well to effectively go to the local fish and chip shop or deli to make your foreign language queries and in fact Knobe's early work (in English) was done by asking questions in Central Park, NY., and this is a very legitimate way to get a general local feeling, but world anthropology and evolutionary psychology and its conclusions at Tooby and Cosmides level really are another matter. It is far better and often essential to go back to source. At the "Institute for Fundamental Studies" (which at present has main headquarters in UK, Maharashtra (India), and Goa) we normally deal with non-diaspora Hindi, Marathi, Konkani, Tamil and English speaking cases, and we get gratifying results.

Machery (2009) says "So, what's going on?". Well, the above is some of what's going on.

But there is much much more !

I am saying that in a further three ways at least - and simply as a beginning - that more care must be taken.

Firstly I mention Vul (2009) and Haynes (2008). On comments on Haynes' work on free will, for example, Auburn University Professor Roderick T. Long (2009) says "This is a hopelessly bad argument; the results of this study have nothing to do with the free will issue at all. This is simply a case of experts in one field (neurophysiology) thinking they are experts in another field (philosophy) that they seem to know very little about." To be fair, Haynes himself did start his career briefly in philosophy but most free will philosophers either ignore Haynes's work, or deny free will already, or are seeking a work round.

Fortunately I do not seem to need a work round as Haynes's work seems to provide simply more evidence that the McTaggart B series is insufficient and we need the A series as well. Perhaps more details later (Yates, 2008, 2008a, 2008b, 2009).

But my opinion aside, Long's general position (though not necessarily his views on politics or economics) is quite widely held. Professor Colin Blakemore, a neuroscientist and director of the Medical Research Council, apparently said (Guardian, 2009) : "We shouldn't go overboard about the power of these techniques at the moment". I certainly agree ! It seems that X-phi has still largely to come to terms with Haynes' work, but the eager assumption of a very simple interpretation of results such as those of Haynes, should certainly not be made. I refer to particularly to the recent work of Vul (2009) concerning MRI interpretations and also to the implications of the work of Hacker (2003) but detailed discussions on both could add considerable additional material to the above.

Secondly I mention the important work that has recently been done in behavioural economics, and in particular the work of Ariely (2009). This helps to bring yet more clarity to the view that the old idea that market approach which presumes that "the common people know what they want" is actually quite wrong. Ariely (2009), who is Alfred P. Sloan Professor of Behavioural Economics at M.I.T. has written many papers to this effect. This work undoubtedly affects questionnaire design and we all need to consider these angles. This sort of matter goes well beyond minor details of presentation. Most Westerners do not know or care, for example that the colour "white" is a colour for weddings and the like in the West, but anyone who lives in India can hardly miss that in India, "white" is the colour for funerals and "red" is the colour for weddings ! But Ariely's work, which is not per se given cross-cultural connotations in his experiments, must have its conclusions considered in such ways in each and every local context - when we are considering reinterpretations of philosophers like Kripke.

Finally we have prejudice.... Unfortunately it does not begin and end with Engine Charlie Wilson's dictum "What's good for General Motors is good for the USA". The Implicit Association Test (Nosik, 2009) has its most surprising and controversial finding as its indication that about 70 percent of those who took a version of the test that measures racial attitudes have an unconscious, or implicit, preference for white people compared to blacks. This contrasts with figures generally under 20 percent for self report, or survey, measures of race bias. Current studies in the research came from a number of countries including Germany, the Netherlands, Italy, the United Kingdom, Australia, Canada, Poland and the United States. They looked at such topics as attitudes of undecided voters one-month prior to an Italian election; treatment recommendations by physicians for black and white heart attack victims; and reactions to spiders before and after treatment for arachnophobia, or spider phobia.

Obviously the IAT does not apply to white people only. One might apply it to Iranians living in Iran, for example, and their views on non-Iranians. Most certainly it will influence all who give or take

such tests, to a greater or lesser degree. I think it was Joshua Knobe who did somewhat similar tests on philosophers as compared to lay people, and found such a bias there, but in his tests it acted as a reverse bias. At this point we could well become worried about relativism and hermeneutics in the sense of Heidegger and Gadamer.

A further brief point I'd like to make is the question as to whether this approach to Kripke involves modern semiotics quite directly rather than simply semantics. I hesitate to mention Barthes, Saussure, Lacan and so on but their conceivable relevance seems obvious. David Sless (1986) remarks, 'semiotics is far too important an enterprise to be left to semioticians' and it may well be true.

### ***Conclusion:***

This note is not to be in denial of progress, just to say progress may be difficult and when back at the "Institute for Fundamental Studies" in Mumbai after the monsoon I intend to do some investigations myself, bearing in mind the earlier work of Kripke, parallel universe ideas like those of Deutsch, Parfit etc., and the approach of Noe and of Clark and Chalmers. Naturally all this may ultimately give further evidence for the Many Bubble Interpretation, involving the A series of McTaggart.

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