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## "Cultural Capital": A Response to the Comments

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## “Cultural Capital”: A Response to the Comments

by John H. Goldthorpe

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As a preliminary point, I would note that none of the commentators seeks seriously to challenge my claim that Bourdieu's theory of social reproduction, in which the concept of cultural capital is embedded, can now be seen to have failed. *C'est déjà quelque chose*. The issues that are in fact raised in the commentaries fall into two broad categories that I will consider in turn.

There are, first of all, issues relating to Bourdieu's general standing, his originality, and his influence. As regards Bourdieu's place in the history of sociology in the Twentieth century, I clearly take a different view from that of the commentators, or at least from that of DiMaggio and of Savage, Warde and Devine (hence, SWD). But I do not see that this question is all that relevant. My paper is specifically concerned with problems that arise with the concept of cultural capital and does not aim to provide a general evaluation of Bourdieu's work (another time perhaps?). And, in any event, arguments have to be judged on their merits, not by reference to the celebrity of their authors. So I am not much moved by the estimations of Bourdieu that DiMaggio cites, even if from Prime Minister Jospin or President Chirac.

The questions of Bourdieu's originality and influence are more germane. Both DiMaggio and De Graaf describe the influence that Bourdieu exerted on them in their early careers. They are concerned here with facts of their own intellectual biographies, and these I would not, of course, wish to dispute. However, if what DiMaggio and De Graaf chiefly got from Bourdieu was awareness of a possible “cultural mismatch” between home and school that could impede the educational development of children from less advantaged social backgrounds while favouring children from more advantaged backgrounds, then I have to say that Bourdieu was by no

means the only possible source of this revelation. It is a fact of my own intellectual biography – and, I would guess, of that of many other sociologists of my generation – that we appreciated this possibility well before Bourdieu, as a result of reading the sociologists I cite in my paper, and others whose names could be added, who had carried out research often technically superior to Bourdieu's. SWD refer in this regard to "largely forgotten sociologists" whom I perversely "excavate." Forgotten they evidently are; but, I would add, with no credit to the forgetters.<sup>1</sup>

I would agree with DiMaggio that Bourdieu *was* original in integrating "reasoning about educational attainment" into a "comprehensive" theory of society – via the whole conceptual apparatus of "forms of capital," the *habitus*, "the cultural arbitrary," "symbolic violence," etc. This is of course precisely the point that I make in my paper – but with the rider that the theory has turned out to be rather comprehensively wrong. And to revert to the initial reception of Bourdieu, I would say that to many of those reared on the pre-Bourdieu sociology of education, myself included, the theory never appeared persuasive, even before the empirical evidence accumulated against it. That is, because of the dubious analogy between economic and cultural capital, the over-socialised conception of the actor implicit in the notion of *habitus*, the failure to distinguish between the arbitrary and non-arbitrary elements of educational syllabi, the neglect of the resocialising capacity of the school, etc.<sup>2</sup>

Finally, on the subsequent influence of Bourdieu's work, I would also agree that this has been considerable, even if a good deal more patchy than DiMaggio and SWD would seem to suppose. It is, for instance, of interest that among the

<sup>1</sup> In discussing this matter with my colleague, Chelly Halsey, I gather that he too had not found much new in Bourdieu as regards (sub)cultural influences on differential educational attainment. Halsey also draws my attention to the fact that among French educationalists and sociologists there was, pre-Bourdieu, much interest in what was referred to as *la famille educogène*. De Graaf claims that the idea that cultural capital gives access to elite groups and their institutions "is very much Bourdieu's own invention." But, from the British literature, I would note the work of Kelsall, Poole and Kuhn [1972] – no doubt more "largely forgotten" authors – who have a good deal to say on this theme, even if in more modest language than Bourdieu. In addition, educationally aspiring young people of my generation were likely to have learned the lesson first-hand. In my own (failed) attempt to gain admission to Cambridge, I had to answer the question "Would you find anything incongruous in the performance of a Bach cantata in a baroque Catholic church?". I had heard Bach cantatas in the village chapel but baroque Catholic churches were rather rare in the South Yorkshire coalfield.

<sup>2</sup> I must acknowledge that my own scepticism merged with distaste, on account of the combination of pretentiousness and often profound unclarity that characterises Bourdieu's writing – perhaps reflecting what Daniel Dennett [2007, 405, n. 12] has called the tactic of *eumerdification*. This neologism derives from the story of John Searle questioning Michel Foucault about the obscurity of *his* prose – in contrast to his conversation – and being told "That's because, in order to be taken seriously by French philosophers, twenty-five per cent of what you write has to be impenetrable nonsense."

*French* sociologists now doing the most important work on educational inequalities and social mobility – Louis Chauvel, François Dubet, Marie Duru-Bellat, Michel Forsé, Eric Maurin, Claude Thélot, Louis-André Vallet – Bourdieu's work could *not* be said to be a prime source of inspiration. It tends to be mentioned only *en passant*, if at all. And on occasions when I have inquired why this is so, the answer I have most often received has been in the form of an eloquent Gallic shrug of the shoulders. Furthermore, in those cases where Bourdieu undoubtedly *has* been a major influence, the important question that has then to be asked is – for better or for worse? This leads on to the second set of issues that arises in the commentaries: that is, those relating to the concept of cultural capital and its problems, on which my paper focuses.

In this regard, the main critical argument I make is that mainstream (or ISA RC28) stratification researchers who have drawn on this concept have detached it from its proper Bourdieusien theoretical context – as SWD point out; *and* that, in consequence of thus “domesticating” Bourdieu, these researchers have often failed to see the full significance of their own findings. Both DiMaggio and De Graaf do indeed accept that in their work on differential educational attainment, they adapt Bourdieu's concept to their own purposes, and DiMaggio elaborates interestingly on his reasons for so doing. However, I do not think that either he or De Graaf comes to grips with the main point of my criticism.

I do not wish to claim that, as DiMaggio puts it, “a sociologist who borrows another's terminology or concept must take an oath of fealty, committing heart and soul to the entire package.” But what I do hold is that the prime purpose of formulating and testing hypotheses is the evaluation of theory. That is to say, hypotheses that are empirically tested should be derived *as strictly as possible* from theory. Then, in so far as they stand up to test, the theory from which they stem is corroborated while, in so far as they fail, the theory is called into question. And in this latter case, it would seem important to work back from the failed hypotheses to see what exactly their failure tells one about weaknesses in the theory.

Now, researchers such as DiMaggio and De Graaf who start out from hypotheses that involve the concept of cultural capital, but so modified that the hypotheses do *not* follow closely from Bourdieu's theory, face consequent difficulties: that is, in determining just what the theoretical implications of their empirical findings are. They tend in fact towards “fifty-fifty” conclusions: in part their results support Bourdieu, in part not. But my contention is that, if the results they report were to be set against hypotheses informed by the *ur*-concept of cultural capital – that which actually operates within Bourdieu's theory – then these results would have to be seen as overwhelmingly negative: for instance, in bringing out the important effect on

educational attainment (though varying by field of study) of basic academic ability, as fostered by the school, independently of family cultural capital; or, again, the greater effect of forms of cultural capital that have only a rather weak association with parental socio-economic background (e.g. those expressed in reading behaviour) as compared to those having a stronger association (e.g. those expressed in *beaux arts* participation).

To illustrate further here, I note that, in his comment, DiMaggio claims that his research and that of many others does at least validate Bourdieu's key insight "that the educational system rewards families and students capable of appropriating prestigious culture." But this is then to domesticate Bourdieu to a quite extreme degree. If this were *all* that Bourdieu is saying, there would indeed be little to argue about. However, in evaluating the role of cultural capital, as understood by Bourdieu, the crucial question is not whether the mechanism to which Di Maggio refers actually operates but, rather, whether it operates with anything like sufficient force – and even together with other mechanisms linked to cultural or other "forms of capital" – to produce the continuous social reproduction over time that Bourdieu's theory proposes. And the empirical evidence clearly indicates that it does not: that is, the evidence, for all modern societies, of substantial upward educational and social mobility.

What is then the real significance of research such as that of DiMaggio and De Graaf is not that it gives some partial support to Bourdieu's theory but that it helps us see why the theory fails: in particular, because family differences in cultural capital can to a substantial extent be offset via schooling, as the educational system vies with the family in the creation and transmission of cultural capital; because the *habitus* (primary socialisation) is in no way so exigent as Bourdieu supposes; and because, while family differences in cultural capital (cultural resources) are certainly correlated with other aspects of economic and social advantage, the correlation is often not all that strong.

In short, working with domesticated concepts of cultural capital does not make for theoretical clarity. And thus, without questioning the inspiration that Bourdieu provided for DiMaggio and De Graaf, I would still maintain that their sociological thinking would in the end have benefited had they been less under his spell. My proposal follows that, in future, researchers should avoid using the concept of cultural capital *unless* they wish to do so in something close to the full Bourdieusian sense.

I turn now to SWD, as representatives of those who would wish to retain a concept of cultural capital that has, at all events, more in common with that found in Bourdieu wild than with the domesticated forms used by mainstream stratification

researchers. In this regard, the chief question that I pose in my paper is that of just how much of the theoretical baggage that comes with the concept in its original version these sociologists would wish to retain and how much to jettison. I do not see that I get any very clear answer from SWD. They argue that “What stratification analysis needs is refinement of complex explanatory concepts” – with cultural capital then being “a prime candidate for treatment.” But they give little indication of the actual lines on which they believe that such work should proceed.

I would suggest that SWD are hampered here by their neglect of the distinction between concepts and theory – as revealed in their phrase “explanatory concepts.” Concepts cannot in fact explain; only theories can do that. This is so because concepts entail only nominal propositions – “let us look at the world this way” – and have thus to be judged in terms of their *usefulness* as, say, in providing a context for observation or elements in the construction of theories. But for purposes of explanation, we need theories *per se* that entail substantive propositions – “this is the way the world is” – and that can then be judged in terms of their *truthfulness*. Thus, the real challenge that faces SWD as regards rescuing some (relatively) wild concept of cultural capital is not that of its refinement but rather that of going beyond such preparatory work to the incorporation of the concept in explicit theory – whether directed towards explaining aspects of educational inequality or whatever – and theory that is therefore open to empirical test.<sup>3</sup>

So far as my own work on class differentials in educational attainment is concerned – and since SWD again take it up – I would note that it is precisely such theory construction that, along with Richard Breen, I have been engaged in as regards the “relative risk aversion” theory to which De Graaf refers. SWD say they are disappointed not to have further clarification of my position, and in particular on the distinction I propose between cultural values and cultural resources. To this I can only reply that they are somewhat behind the fair and direct them to an already published chapter [Goldthorpe, 2007, vol. II, ch. 4], with the comment that they would in any event be less in need of such clarification were they not to labour under the – further – misapprehension of my views represented in the work of Devine [1998], which in

<sup>3</sup> I do not think that SWD’s attraction to “field theory” is likely to prove helpful in this regard. As applied to sociology, field theory appears quite vacuous, and how it could be subjected to empirical test has never been spelled out. I would note that even its most cited proponent remarks that “field theories may be seen as provisional theories that we are happy to replace when adequate knowledge of mechanisms is gained [Martin 2003, 12]. But how then does field theory help us in moving towards mechanism-based explanations? And why not aim to develop and test these straight off?

the chapter in question I seek to correct.<sup>4</sup> However, its more important purpose is to review the by now substantial, but still growing body, of empirical research that has resulted from attempts at testing relative risk aversion theory and, in the light of this, to assess the theory's present standing (still alive, although with some increasingly well-defined problems that call for attention).

I trust that in the future work to which they refer SWD will aim to present theory, using their own version of the concept of cultural capital, that likewise meets the prime requirement of being clear and explicit enough to be proved wrong.

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<sup>4</sup> SWD follow Devine in the view that in explaining class differentials in educational attainment I "discount the importance of cultural factors." This is simply wrong. I fully recognise the importance of cultural factors in, to follow Boudon, "primary effects" in class differentials. What I argue in the passage referred to by Devine is that in explaining *the persistence of* such differentials, in the context of educational expansion and of rising levels of educational attainment across all classes alike, "secondary effects" – the effects of choice under constraints – have to be the focus of attention; and it is then in this regard that I emphasise "the importance of economic processes." Further, in so far as cultural factors are involved, the evidence would clearly suggest that class differences in cultural values regarding education are of far less importance than are class differences in the resources, cultural and other, that children and their parents have in pursuing values that are in fact largely shared. How, one might ask, does the notion of *habitus* allow this distinction to be made?

## “Cultural Capital”: Some Critical Observations

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Abstract: “Cultural capital” is a key concept in the work of Pierre Bourdieu. It plays a central role in Bourdieu’s account of the generation of class inequalities in educational attainment, which has evident affinities with those advanced by other sociologists of education; but also in his far more ambitious – though empirically unsustainable – theory of social reproduction. Much confusion can then be shown to arise from a failure to distinguish between the uses of the concept in the two quite differing contexts of what might be labelled as Bourdieu “domesticated” and Bourdieu “wild”. Researchers using the concept in the former context often fail to appreciate its radical nature and, in turn, the full extent to which their findings undermine Bourdieu’s theory of social reproduction; while those who would wish to understand the concept in the latter context have difficulty in showing its continuing fitness for research purposes, given the failure of the larger theory in which it is embedded. Advantage would follow from leaving the language of “cultural capital” to those who still seek to rescue this theory, and otherwise replacing it with a more differentiated conceptual approach.

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*Keywords: Bourdieu, cultural capital, social reproduction, educational attainment.*

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