



Response

Debating the psychology of tyranny: Fundamental issues of theory, perspective and science

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In our rejoinder, we concentrate on responding to Zimbardo's criticisms. These criticisms involve three broad strategies. The first is to turn broad discussion about the psychology of tyranny into narrow questions about the replication of prison conditions. The second is to confuse our scientific analysis with the television programmes of 'The Experiment'. The third is to make unsupported and unwarranted attacks on our integrity. All three lines of attack are flawed and distract from the important theoretical challenge of understanding when people act to reproduce social inequalities and when they act to challenge them. This is the challenge that Turner identifies and engages with in his commentary.

In his commentary, Turner (2006) engages with the core theoretical questions raised by our work. Are people unimaginative slaves to circumstance? Do groups necessarily abuse power when they have it and succumb to it when they do not? Should people not be held accountable for the systems of tyranny they create and administer? In this, he makes an important contribution to precisely the debate that we hoped to encourage. Zimbardo, by contrast, suggests that our contribution is so flawed that it provides nothing of substance to debate. He too raises some important issues. However, many of his points are based on misconceptions and misleading arguments about our study. It is necessary to address these and – since Turner's piece speaks for itself – we concentrate mainly on Zimbardo's (2006) commentary in this rejoinder. First, though, it is worth placing his commentary within a wider context.

The Stanford Prison Study (SPE) has handed down an ambiguous legacy to our discipline. On the one hand, it was a dramatic illustration of the power of context upon behaviour and advanced debate concerning the conditions under which ordinary people will tyrannise others. It is hard to overestimate the importance of the study in this regard. Along with Milgram's obedience studies, it is one of the very few one can point to when asked, 'Where has social psychology had an impact upon society at large?' (see Blass,

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2004). Indeed, the gripping nature of the phenomena observed in the SPE (and the fact that they were captured on camera) guaranteed coverage of the study far beyond the confines of social psychology. Unfortunately, though, the study simultaneously served to suppress debate inside our discipline. Two factors contributed to this suppression. First, only a limited subset of the study's findings was ever exposed to detailed scientific scrutiny (in particular, none were published in mainstream, peer-reviewed psychology journals). Second, ethical concerns made it exceptionally difficult to replicate the study in a way that allowed the issues it raised to be revisited in such a vivid empirical form.

Together, these factors led to the suspension of normal science. Instead of the democratic process whereby researchers could test the claims of others either by examining their data or by collecting more, here it was possible to do neither. Researchers could no longer study big issues such as tyranny themselves and they could never challenge Zimbardo's analysis. This diminished social psychology and contributed, in part, to contemporary crises of relevance. As we stated in our paper, one of the major aims in undertaking the BBC Prison Study was to resume normal scientific debate surrounding some of the big questions to which the SPE speaks. Indeed, we feel that one of our achievements was to show that it is possible to address big and powerful issues while behaving ethically. More important than whether all the details of our argument are correct is the fact that we should be able to enter into debate about when and why tyranny prevails and thereby attempt to advance understanding. Throughout this process, and throughout his commentary on our paper, Zimbardo has avoided such a debate. How and why?

Misrepresenting the issue

Much of Zimbardo's commentary is devoted to a comparison between the BBC Prison Study and the SPE. This is important, but the outcome of any comparison is obviously dependent upon the dimensions along which it is made. Zimbardo makes it a matter of who best simulates prison conditions. Thus, he provides a long list of differences between the two studies from which he concludes (a) that our study is unlike his, (b) that it fails to reproduce the conditions in any existing prison and therefore (c) that it is worthless.

Zimbardo is right to point out these differences (although there are several misrepresentations and factual errors - for example, in his claim that there were no prison rules). However, he is wrong in the conclusions he draws from them because simulation of a prison is not the central consideration here. We are clear in the paper that we did not set out to make participants think they were in a real prison and that the set up of the BBC study departed from prison conditions in a number of critical ways. But, as both we and others have argued (e.g. Banuazizi & Movahedi, 1975), the SPE is also different from a real prison in critical ways and it is implausible to claim that the participants actually thought they were in a jail rather than participating in an experiment.

Equally, the impact of the SPE would not have been as great if its purpose had merely been to comment on prison conditions. Its influence both within and beyond psychology is tied to the fact that it is used to make *general theoretical claims* about extreme human behaviour and the ways in which group members reproduce social inequalities. Zimbardo has been actively involved in this process and has used the SPE to comment on phenomena far removed from prisons, such as suicide bombings, extremist groups and terrorism (e.g. Zimbardo, 2001). Our study was also designed to address these wider issues. The aim was not to reproduce a real prison in *all* its features, but to use the inequality between prisoners and guards that lies at the heart of a prison

system (and many other social systems) in order to mount a general inquiry into the way that individuals respond to intergroup inequalities.

The important issue addressed by both studies, then, is the broad issue of collective inequality and of tyranny, not the narrow issue of prison behaviour. It follows that the important comparison is which study affords better insights into these issues, not which best reproduces all the features of a prison. Our study (like the SPE) does not stand or fall on whether participants felt they were in prison but rather on whether they felt that their social environment was unequal and whether they cared about it. In this respect, a potentially more serious criticism of our study would be that participants were doing little more than playing a game and never took the situation seriously.

However, this criticism cannot be sustained. We have physiological data (rising cortisol levels indicating rising levels of stress; see Haslam & Reicher, *in press a*, 2005), psychometric data (on a range of measures from organizational citizenship to burnout) and observational data to show that participants engaged with the inequalities in the BBC study. This is apparent, for example, in PP_p's rage at being denied a cigarette and the complaints of the prisoners concerning the inferior quality of their meals. It is apparent too in the following comment, which Ian Burnett, a participant, made to a journalist in one of the sources that Zimbardo cites:

I knew it was an experiment but it honestly felt like a real jail . . . In that kind of environment, when boredom is one of the problems you are fighting against, meals become a highlight of the day, but not for us . . . It was like going back to your childhood, being told what to do and when to do it. We had to get up at 6.30 am, get washed and dressed, then have our breakfast. Anyone who smoked was allowed one cigarette at mealtimes and one in the afternoon. We prisoners quickly struck up a friendship and turned it into a them-and-us situation. We used to whistle to annoy the guards and when we were talking to them we'd look over their left ear, which always put them off slightly. They were stupid little things, but we felt it united us against them (Murfit, 2002, p. 30).

On the basis of such data, we are confident that our study, like the SPE, provides an appropriate setting in which to investigate the processes which determine how people respond to intergroup inequalities. However, if – in this respect – the two studies are equivalent, we believe there are other ways in which our study represents an advance in terms of its ability to clarify these processes. Some are relatively minor, such as the greater diversity of our participants in terms of age, class, 'race' and educational background. Others are more important, such as the breadth of our data sources and the systematic nature of our data collection. For us, though, the most important difference is that, in our study, we drew on a well-developed theoretical perspective in order to produce a transparent social psychology of intergroup inequality. We designed the study in order to investigate how theoretically relevant variables such as permeability would impact upon perceptions and actions, and we took considerable care both to design appropriate interventions and to collect sufficient data to assess their effects. We therefore measure the success of our study by its ability to advance general theoretical understanding of how people respond to social inequality (which we have analysed not only in relation to the broad issue of tyranny, but also, elsewhere, in relation to specific matters of leadership, stress, organizational behaviour and collective agency; Haslam & Reicher, 2005, *in press a*, 2005; Reicher, Haslam, & Hopkins, 2005; Reicher & Haslam, *in press a*, b).

The SPE, by contrast, was not designed to develop theory, and while it obviously led to a strong theoretical claim concerning the inevitability that group power will be misused, Zimbardo seems resolutely opposed to a discussion of matters of theory.

Indeed, one of the most striking features of his commentary is the hostility he displays to all the basic features of normal theory-driven research. This starts with the fact that we have a theoretical perspective in the first place. This, Zimbardo characterizes as an 'evangelical world view'. Theory-driven research typically continues with the use of specific interventions or manipulations that are theoretically informed. Although ours were clearly derived from a long tradition of social identity research (after Tajfel & Turner, 1979), Zimbardo describes these as 'mindless'. In similar vein, he describes our introduction of DM_p, a seasoned trade unionist rather than a randomly chosen applicant, as the 'blatant imposition of experimenter bias'. Yet, our theoretical rationale here was to bring in a person with an alternative perspective and to see the implications of this for the system. But, of course, that could not be achieved unless the person we introduced had such a perspective. Likewise, our use of loudspeaker announcements (of which there were just three) in order to implement our permeability manipulation is described as 'a dominantly intrusive constant element into the research setting' – as if intervention were illegitimate in itself. Finally, Zimbardo concludes with an attack on systematic data collection. Thus, our psychometric and physiological assessments are also cited as evidence of our unwarranted intrusion – even though questionnaires and saliva swabs were self-administered by the participants and involved no contact with the experimenters. To claim that these various elements are 'in opposition to values of psychological science' is to mistake what those values are. Moreover, if Zimbardo were correct, one would have to dismiss as misguided and biased a range of classic field studies that have played a major role in testing and advancing social psychological theory on the basis of identical logic (e.g. Sherif, 1956).

In sum, Zimbardo's criticisms are reminiscent of one of the key stratagems for saving a failing position identified by the philosopher Arther Schopenhauer: 'if you observe that your opponent has taken up a line of argument that will end in your defeat . . . you must effect a change of debate' (2005, p. 95). Zimbardo consistently turns a conceptual debate about tyranny into a technical debate about prison conditions. But the conceptual debate is what is at issue. Everyone agrees that the SPE showed that normal people can produce tyranny and that it raised important questions about why they do so. What many doubt is the set of answers routinely given to these questions. The BBC Prison Study was designed to address such *theoretical* doubts. Accordingly, it is in its capacity to provide and justify a different set of answers that it should be compared with the SPE.

Misrepresenting the data

Another of the stratagems described by Schopenhauer goes as follows: 'if you are confronted with an assertion, there is a short way of getting rid of it, or, at any rate, of throwing suspicion on it, by putting it into some odious category; even though the connection is only apparent' (2005, p. 141). In Zimbardo's case, that category is 'reality TV'. In one sense, we have no problem with this label being ascribed to our project – that is, if it is merely used to refer to the live recording and broadcasting of social interaction that has really taken place. This, indeed, is the meaning Zimbardo employs elsewhere when claiming to have pioneered the genre. Thus, in an interview with the *San Francisco Chronicle*, he states that: 'in a sense, [our] prison study was one of the first examples of Reality TV, because we videotaped the whole procedure' (Stannard, 2002). However, as he applies it to our study, 'reality TV' (and notorious examples such as *Big Brother*) denotes something odious: a shallow and humiliating entertainment

masquerading as something deeper. Zimbardo suggests that we are inevitably sullied by association with such a genre and that to claim our project was science only makes it (and us) more disreputable.

On first reading, this appears to be a powerful condemnation, made all the stronger by the fact that some participants in our study appear to endorse it. So, as the culmination of his case, Zimbardo cites several of them, notably Philip Bimpson, likening 'The Experiment' to 'cheap entertainment'. But look closer at what Bimpson is saying. He is expressing dissatisfaction with the BBC product at an early stage in the editing process. Zimbardo uses this as an attack on our study, yet Bimpson never criticizes the study. In fact, he echoes the concerns of other participants that the television coverage might trivialise *precisely because* he believes the study itself to be serious. Moreover, like Burnett above, his comments concerning the organization of the prisoners and the disorganization of the guards do not 'diverge considerably' from our analysis but are entirely consistent with it.

Under analysis, then, Zimbardo's most damning criticism falls apart. It depends upon confusing the television programmes with the science. So, let us be absolutely clear about two points. First, *the television programmes are not the scientific data, and the scientific data aren't the television programmes*. Each employs different media aimed at different audiences. What is suitable for one is clearly unsuitable for the other. On the one hand, the scientific papers are written for a specialist audience. They develop and sustain our argument through the systematic analysis of the full range of observational, psychometric and physiological data. Moreover, we defend fully the integrity, appropriateness and reliability of our statistical analysis - which, as we note, is shown by supplementary analyses to be uncompromised by non-independence of observations. On the other hand, the television programmes were designed for a non-specialist mass audience. The remit of the producers was to edit down over 800 hours of observational data to make 4 hours of television that would illustrate our argument in an intelligible and compelling manner. In a phrase, the television was a *window* on the science. Nothing more. It was intended to engage the interest of viewers and to encourage them to find out more about the underlying issues for themselves - which a great many of them (particularly psychology students) certainly did.

But accepting that the science and the television should be kept separate still leaves unanswered the question of how they related to each other. Zimbardo asserts that all aspects of the study, from the initial set-up to the final re-edits following complaints by the participants, were dictated by BBC staff and their search for sensationalism. So, second, let us be absolutely clear that all decisions relating to the science of 'The Experiment' were made autonomously by ourselves, the researchers, and that *the science framed the design of the television, the television did not frame the design of the science*. As a condition of participation in the project (and, as part of our case for ethical approval), we negotiated a contract with the BBC that gave us responsibility for the way the study was set up, the way it was run, the way it was analysed and the story that would be told in the eventual broadcasts. The contract also specified that we would involve the participants in this process and use their feedback to refine both our analysis and the television programmes. The fact that we did this is not a sign of weakness or fraudulent 'data selection or modification'. On the contrary, it is a hallmark of good qualitative research and ethical best practice (Stake, 1976; although, as Miles & Huberman, 1994, note, this procedure 'is venerated, but not always executed', p. 275). Amongst other things, this is because it helps achieve what Bronfenbrenner (1976) called *phenomenological validity*.

It is worth recalling too that all aspects of the study were continuously overseen by a five-person ethics committee comprised of a senior Member of Parliament, the co-founder of the Beth Shalom Holocaust Memorial, a Council Member of the Howard League for Penal Reform, a senior representative of the BBC's independent editorial policy unit and one of Zimbardo's own close colleagues. It was the scientific merit of the project that induced them to give up their time to participate in the project. Moreover, their final 22-page report attests to the probity of our various ethical and scientific procedures.

These various arrangements, whereby we set up and conducted a study that was filmed by the BBC (rather than commenting on a situation devised independently of us) are precisely what gave the project scientific credibility. It also made the project unique – the exact reverse of what is normally understood by 'Reality TV' – and it was this uniqueness that was attractive to the BBC. That is why its producers not only tolerated the 12-month period it took us to design, plan and gain ethical approval for the study, but actually insisted on these elements – because they believed the project would be worthless if it did not follow all the procedures that are involved in conducting legitimate science. For all these reasons, we reject the idea that the BBC were interested in cheap entertainment or that we and our work are inevitably and irreparably sullied by association with the BBC.

Yet, once all Zimbardo's complaints about 'the scientific legitimacy of research generated by television programming interests' are overturned, there does remain a serious point about the impact of television broadcast upon our participants and its implications for the conclusions we can draw from the study. We discuss these in the paper, but will revisit them briefly.

First, it is important not to overstate the significance of the cameras and microphones as primary drivers of behaviour. As the participants themselves indicated, as time went by, they became increasingly accustomed to surveillance and forgot it was there. This meant that they tended to become aware of the cameras only when back in their cells or in the silence of night, not when critical interactions were unfolding.

Second, even where the cameras did have an impact, this does not invalidate the study. Instead, it simply means that it is necessary to clarify the processes underlying their impact in order to understand the wider implications of particular findings. Along these lines, Zimbardo acknowledges in his postscript that it is far from trivial to conclude that surveillance may have affected the willingness of the guards to impose their power. As he observes, surveillance may have achieved this outcome by giving participants 'a future orientation' that made them think beyond 'the immediacy of the present moment'. What he does not acknowledge, however, is the way in which this opens up wider conceptual issues. For, as Turner notes, the capacity to imagine different worlds and to orient one's behaviour towards them is an important facet of the human condition and one that lies at the heart of social identity theory's analysis of social change. Evidence of this imaginative capacity and of its importance stands in stark contrast to Zimbardo's situational determinism. It also requires us to ask important questions. What invokes or suppresses the imagination? What social conditions embed us in the present or encourage us to look to the future? In what ways is the identity of actual or imagined audiences a constraint on behaviour? By provoking these questions, the issue of surveillance adds to rather than detracts from the richness of our study. And it leads back to precisely the debate we have been insisting on all along.

Misrepresenting the authors

'A last trick', writes Schopenhauer, 'is to become personal, insulting, rude, as soon as you perceive that your opponent has the upper hand, and that you are going to come off worst' (2005, p. 161). The most serious examples of this in Zimbardo's commentary are those where he implies that our study involved dishonesty and fraud - indeed, this is one of the most serious accusations that can be made against a fellow academic. Were it true, we deserve not only to have our papers rejected, but also to be disbarred from our profession.

Zimbardo's first implied allegation is that we are dishonest in claiming to have divided participants randomly into guards and prisoners. In fact, he insinuates that we deliberately selected 'tough' individuals to be prisoners and 'soft' individuals to be guards. Actually, as we describe, our procedure involved a mixture of matching and random assignment, which is superior to pure random assignment because it mitigates against the law of large numbers (which means that randomly assigned small samples are likely to be less equivalent than randomly assigned large samples - a factor that could easily have contributed to non-equivalence of groups in the SPE). We have documents to show how we planned this procedure and we have witnesses to attest that we carried it out. It would also have been impossible for us to cheat purposefully since we had never met any of the participants prior to making this division. What is more, to have cheated in the way that Zimbardo implies would have gone against our interests since it led to the disconfirmation of our predictions concerning guard behaviour in the study's initial phases.

But all these defences concede Zimbardo's claim that the prisoners were in fact tougher individuals than the guards. This claim is based on three pieces of evidence: (a) that the prisoners had tattoos, (b) that they had 'tough' occupations and backgrounds and (c) that they acted in tougher ways than the guards. We would counter by noting that some of the guards also had tattoos - they were just hidden under their long-sleeved shirts. Several of the guards also had 'tough' jobs (e.g. fireman, ex-soldier), just as many of the prisoners had 'soft' ones (e.g. environmentalist, civil servant, office worker). So Zimbardo's characterization of the groups is misleadingly selective. Most importantly, though, the notion that the prisoners must have been tougher individuals because they behaved in tougher ways is circular since the explanation is based on the thing that needs to be explained.

We suggest therefore, that, far from reflecting individual differences (of which there is no evidence) differences in behaviour were actually the *product* of emergent differences in group dynamics (of which we have clear evidence). As we explain, it was these dynamics that brought *particular* guards and *particular* prisoners to the fore (just as in the SPE). Moreover, the behavioural contrast has to do with effectiveness, not toughness. The guards, including the 'millionaire hi-tech executive', were not personally weak; they were actually rather heroic in repeatedly seeking to confront the prisoners single-handedly. But this was precisely their problem. They were single-handed, and ineffectiveness resulted from their lack of shared identity (and an associated reluctance to assert their power) not their lack of individual will (Turner, 2005). Amongst other things, this also meant that even when they had the opportunity to promote a 'tough' prisoner to be a guard they *chose* not to.

Zimbardo's second insinuation is that we lie when we describe our interventions as theoretically guided because they were 'probably generated by BBC staff to stir the pot that was filled with a very tepid stew at the start of videotaping'. It is certainly true that our manipulations (e.g. of permeability) turned a non-conflictual situation into a conflictual one. However, as stated in the paper, this transformation was of our devising and was a result of planned interventions designed to operationalize and test

a well-developed theoretical position (after Tajfel & Turner, 1979). As we can prove straightforwardly, details of these were included in documents that we developed in consultation with colleagues (and that we also submitted to representatives of the BPS) in order to gain university ethical approval for the study. Without the presentation of this detailed scientific case, that approval would not have been granted.

But there is something even more troubling here. As we explained at the outset, our aim has always been to stimulate debate and (naïvely as it turned out) we originally thought Zimbardo might be similarly interested. We also wanted him to have an informed basis from which to respond to requests for comment about the ethical and scientific justification for our research. Accordingly, on 8 November 2001, we sent him a full set of documents for him to comment upon. These provided details of (a) our ethical protocols; (b) our scientific rationale and procedures (including an outline of theory-derived manipulations of permeability, legitimacy and cognitive alternatives); and (c) our relationship with the BBC (explaining that ‘the experiment has been designed by the psychologists [i.e. us], it will be run and analysed by [us], and the TV programmes will reflect [our] analysis’). We never received a reply. However, Zimbardo did receive these documents. On the basis of an interview with him in 2002, a journalist on the *Stanford Daily* wrote ‘Zimbardo . . . was contacted by Haslam and Reicher in the hope that he would advise them on running the experiment. He refused as a result of what he has learned since 1971’ (Ritch, 2002). This means that Zimbardo himself must have known that we, not the BBC, decided on the interventions, that they were designed in advance and that they were based on clear theoretical principles.

Conclusion: Getting back to fundamental issues of theory, perspective and science

But let us move on. For by getting drawn into refuting Zimbardo’s numerous allegations, there is a real danger that we are distracted from more important arguments about the social psychological dimensions of tyrannical behaviour. Is Zimbardo’s claim that people helplessly reproduce inequalities of power really plausible any more? Does it make sense to say that guard aggression in the SPE was ‘a “natural” consequence of being placed in the uniform of a guard and asserting the power inherent in that role’ (Haney, Bank, & Zimbardo, 1973, p. 12)? And what are the implications of getting drawn into this fatalistic view?

Fortunately, we have Turner to help us answer these questions. Moreover, Zimbardo himself inadvertently points to the dangers of his approach when he alludes in his commentary to abuses of Iraqi prisoners and notes that his analysis was presented in evidence at the Schlesinger Committee. There, he refers to the committee’s observation that events in the SPE should have served as a warning to the military. Indeed, they should. However, as we have argued elsewhere (Reicher & Haslam, 2004), the fact that Zimbardo’s analysis of those events was invoked in order to *deny responsibility* for acts of appalling brutality should also serve as a warning to social psychology. For, as argued by Turner, it points to the way that our theories are used to justify and normalize oppression, rather than to problematize it and identify ways in which it can be overcome. In short, representing abuse as ‘natural’ makes us apologists for the inexcusable.

Whatever else it does, the BBC prison study brings these issues to the fore. There is no escaping this. It is not an invitation to disregard or belittle the empirical findings of the SPE. However, it *is* an invitation to move on from the narrow and depressing view of the human condition that Zimbardo has used his findings to defend. It is also an invitation to resume normal scientific debate on a topic of critical social importance.

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