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David J. Buller

Jerry Fodor

Tessa Crume

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The Emperor Is Still Under-Dressed

David J. Buller
Department of Philosophy
Northern Illinois University
DeKalb, IL 60115
USA

Jerry Fodor
Department of Philosophy
Rutgers University
26 Nichols Avenue
New Brunswick, NJ 08901-1411
USA

Tessa L. Crume
Colorado Department of Public Health and Environment
4300 Cherry Creek Dr. South
Denver, CO 80246-1530
USA

Cosmides et al. (CTFB), by Buller and Fodor

CTFB miss the main point. The 'Buller-Fodor hypothesis' concerns only the logical form of *mental representations* of obligation rules [1]. A subject's performance on reasoning tasks is determined by *his/her mental representation of the logical form* of the stimulus material (for Wason tasks, a 'conditional rule'), not by its surface grammar. The mental representation of logical form is, in turn, a function of contextual variables, including background information ([2], p. 279). Predicting performance on a reasoning task thus requires information about both the stimulus material and the factors that influence how subjects interpret it. Nor is it denied that there may be residual effects of content on subjects' performance when the logical form of the experimental stimuli is properly controlled. If, however, logical form is *not* properly controlled, then it's fallacious to attribute observed response asymmetries to content variables. CTFB are repeatedly guilty of this fallacy. Correspondingly, the predictions that CTFB claim follow from the 'Buller-Fodor hypothesis' actually don't, since they derive them from the surface grammar of the conditional rules alone. Consider 'If you clean up spilled blood, then you must wear rubber gloves' [3]. This formula can express either an *obligation* (an employer, for fear of liability, requires it of employees) or a *prudential recommendation* (issued as a public health advisory). Since the wording is the same in either case, whether a subject represents it as an obligation or as a recommendation will, of necessity, be determined by contextual variables.

Strictly speaking, only rules expressing obligations (and their converse, prohibitions) are deontics, though we have no objection to calling all 'must' statements deontics. But there will then be crucial logical differences between deontics that express obligations and deontics that express recommendations (what CTFB call 'precautions'). For 'It's obligatory that P' entails 'It's

prohibited that not-P', whereas 'It's recommended that P' does not. Given this *logical difference*, subjects will apply one set of inferential principles to rule formulations construed as obligations and another set to formulations construed as recommendations. So, *of course* CTFB have been able to manipulate performance with 'deontic conditionals' to produce 'double dissociations' of obligations and recommendations. The question is: What accounts for this dissociation? We suggest that CTFB have merely manipulated contextual variables that determine whether subjects *represent* a 'must' as expressing the *logic* of an obligation or that of a recommendation. Where CTFB consistently see only content effects, there are actually logical differences that none of their 'tests' of SCT and HMT have *ever* controlled. (How 'perspective change, switched rules, and "wants" problems' fail to control for these factors is discussed in ([4], pp. 167-190).) The evidence for a scientific hypothesis is supposed to exclude competing explanations. The results CTFB cite consistently fail to do so.

By the way, the hypothesis that subjects solve reasoning problems by applying content-neutral logical principles predates the early tests of SCT and so is not 'post-hoc'.

Buss and Haselton (BH), by Buller

Nowhere was it claimed that Evolutionary Psychologists propose only hypotheses (6) and (7). But, since they are distinct from the other hypotheses, it's legitimate to evaluate evidence for them independently of the others, as the literature has done repeatedly.

BH don't deny that their data don't support the hypothesis that 'men respond primarily to cues of sexual infidelity' ([2], p. 280). Rather, they claim that this formulation 'misrepresents' their prediction, which was that men are more upset by sexual infidelity *than women are*, not that

men respond more to sexual infidelity *than to emotional infidelity*. But the prediction regarding male jealousy in [5] is ambiguous. It's unambiguous elsewhere: 'Women's jealousy, in short, is triggered by cues to the possible diversion of their mate's investment to another woman, whereas men's jealousy is triggered primarily by cues to the possible diversions of their mate's sexual favors to another man' ([6], p. 128). And: 'A man's jealousy has been hypothesized to focus on cues to sexual infidelity because a long-term partner's sexual infidelity jeopardizes his certainty in paternity' ([7], p. 125). (See also [8], p. 360.) These are the predictions discussed in [2, 4]. Moreover, the physiological experiment in [5], comparing male arousal in response to sexual infidelity with male arousal response to 'emotional infidelity', could test only this prediction regarding male jealousy, not that of (6). And the evidence *has not* confirmed it.

If BH are now retrofitting their predictions to the data, that's fine. But this requires modification of the adaptationist rationale for sex differences, which to date has focused on distinct sets of selection pressures independently modifying sex-differentiated modules. Even then, however, the theory won't account for the anomalous data discussed in [2, 4], but not addressed by BH. And the overall data will still be more parsimoniously explained by a hypothesis that doesn't postulate sex-differentiated modular 'design features' [4].

Daly and Wilson (DW), by Buller and Crume

DW's Canadian data regarding fatal abuse are discussed in [4], but the arguments of [2, 4] focus on American data because evidence regarding (potentially biased) underascertainment of child maltreatment fatalities derives from U.S. records from the 1980s and 1990s. To determine whether recording bias may account for the overrepresentation of stepparents in official

records, it's appropriate to examine abuse records in the same society and reporting period. So the argument compares NIS-3 data (U.S., 1993) with data regarding bias in the U.S. in the same reporting period.

DW say it's asserted 'falsely and on no apparent basis, that things like "failing to secure a child with a seat belt" were included as "abuse"'. The claim was that they were included as *maltreatment*, not *abuse* ([2], p. 281). DW's study included all cases in which care was 'so poor or unreliable as to imperil the child' ([9], p. 207), which may include unintentional omissions, but they nowhere report how many of the cases were due to omissions, rather than acts, although only the latter are appropriate to testing their hypothesis. Thus the need to focus on the U.S. data in which 'unintentional omissions' could be separated from injurious acts. And the kinds of omission included among maltreatment are detailed in the reporting form for NIS-3 and the standard definition of child maltreatment endorsed by the U.S. National Institute of Child Health and Human Development [10], as explained in [11] and [4] (p. 374).

DW make several mistaken claims regarding the Colorado study [11], of which one of us (Crume) was lead author. First, the perpetrators of the cases added after death review were *not* 'by definition' the genetic parents; they were any adult in a caretaker role at the time of the fatal event (a genetic parent, 'stepparent', other relative, or a care provider). Second, DW suggest the actual increased likelihood of ascertainment for 'other unrelated including live-in boyfriends' is 2-fold, rather than the 8.71 increased likelihood reported in [11]. However, the 2-fold difference represents a relative risk and is an inappropriate measure given the retrospective design of this study. A relative risk is reported for a prospective study of non-exposed and exposed groups; this study retrospectively measures the association of exposure (perpetrator) and outcome

(ascertainment by the death certificate). So the odds ratio of 8.71 is the accurate measure of increased likelihood of ascertainment. Thus, third, this study *does* show a systematic recording bias of child maltreatment fatalities by death certificates that ‘favors’ genetic parents by significantly underrepresenting deaths at their hands.

Therefore, the magnitude of the discovered reporting biases in official records of fatal maltreatment is sufficient to account for the degree of overrepresentation of stepparents in non-fatal abuse data *from the same society and reporting period* ([4], p. 379), even though DW argue that bias effects should be *lower* in fatality data than non-fatal-abuse data. The evidence of bias suggests *there may be no real ‘Cinderella effect’* in the U.S. data. The argument was *not* that bias in U.S. data accounts for overrepresentation in Canadian or other data, but that overrepresentation in those data may be found to be an artifact of recording bias *if* empirical research into bias, such as that of the Colorado study, were conducted. DW have only offered a priori argument against the confounding effects of a recording bias, but the Colorado study shows that we can draw no conclusions (like those DW favor) until the empirical research is done.

Finally, as [4] makes clear and explicit, and as those who’ve actually read it know (see, e.g., [12]), the intent is not to ‘stop’ evolutionary psychology (in the broad sense), but to encourage higher standards of argument and evidence in it. Moreover, it is *not* argued that stepparents abuse children less than records show, but that evidence indicates genetic parents abuse them *more* than records show. If investigative agencies became aware of research into bias, it could serve to *help children* who are now being lost to the system. In that case, insisting that we already ‘know’ everything about child abuse is what would ‘do real harm in the practical realm of child protection’.

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