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Social Rejection, Control, Numbness, and Emotion

How Not To Be Fooled by Gerber and Wheeler (2009)

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ABSTRACT—*Emotional numbness remains an empirically supported and theoretically intriguing pattern of response to social exclusion that warrants further research, and it would be a loss to the field if such research were prematurely terminated or hampered by the unwarranted conclusions from misleading meta-analyses. The meta-analyses by Gerber and Wheeler (2009, this issue) are based on a biased sample that omits much relevant work. Worse, the authors misinterpret what evidence they do assemble, even interpreting strong evidence for numbness as if it contradicted numbness. Their conclusions about control are similarly unfounded and misguided.*

What is the immediate emotional impact of social rejection? Most people imagine that it must contain intense emotional upset, including sadness, grief, and anxiety.

A contrary prediction has emerged from an assortment of sources, however. The initial response to social exclusion may often be shock, marked by an emotional numbness or lack of feeling. MacDonald and Leary (2005) established that many animals respond to social rejection by becoming somewhat insensitive to physical pain. Because reactions to social events often use the same physiological systems that evolved to respond to physical pain and injury (Eisenberger, Lieberman, & Williams, 2003; Panksepp, 1998), the human emotion system might also temporarily go dead in response to rejection.

There is evidence that exclusion can increase numbness to both pain and emotion. DeWall and Baumeister (2006) found that laboratory manipulations of social rejection caused significant changes on behavioral measure of pain, indicating numbness among rejected persons. Moreover, pain insensitivity and tolerance were correlated with emotional numbness.

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No one can seriously dispute that some rejections cause some distress (e.g., Baumeister & Tice, 1990, for review). The existence of this second pattern of response, however, is a fascinating possibility that may stimulate further research and add a thought-provoking dimension to the field's understanding of human nature. The purpose of this brief article is to prevent that inquiry into being stifled by a controversial and flawed meta-analytic review by Gerber and Wheeler (2009, this issue).

SAMPLING BIAS AND KEY OMISSIONS

Journal reviewers cannot be expected to check all the articles cited in a manuscript, but this circumstance entails that meta-analyses can get published with highly misleading results. As we looked at the Appendix furnished by Gerber and Wheeler, we could see things that journal reviewers would not know. These furnish a cautionary tale about the ease with which meta-analysis can mislead.

The first thing we looked at in our attempt to understand how Gerber and Wheeler could have reached such questionable conclusions was their Appendix. The second article it lists was by Baumeister, DeWall, Ciarocco, and Twenge (2005). We know it well. It is a good candidate for inclusion in a meta-analysis, because it contains six different experiments using different measures of emotion. Three of the experiments found significant differences on emotion, whereas three found no effects whatsoever on the emotion measure.

Gerber and Wheeler's meta-analysis included only the three studies that found significant differences. They omitted the three that found null effects. The selective omission of null results from a meta-analysis will almost inevitably distort the findings and mislead the scientific community.

If one were to flip a coin to decide which studies to include and which to delete, the odds of keeping only the three favorable to their preferred conclusion and deleting the other three would be 0.5 to the 6th power, which is a probability of .0156. Thus, the first article we checked already yielded significant ($p < .05$)

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evidence of nonrandom deletion of information from their meta-analysis. (The significance, by definition, speaks against a carelessness interpretation, because carelessness would presumably produce random omissions.) What a meta-analysis concludes is based on what it includes. In this case, a pattern of including studies that show mood effects while deleting null results will inevitably overstate the strength and consistency of mood effects.

We found similar problems throughout the Appendix. Two major papers in the same journal in consecutive years were by Twenge, Catanese, and Baumeister (2002, 2003). Gerber and Wheeler omitted the 2002 paper entirely from their meta-analysis, along with its barrage of null results on mood. The 2003 paper had six studies, of which Gerber and Wheeler included only two. The first of these two used the Positive and Negative Affect Schedule (PANAS; Watson, Clark, & Tellegen, 1988) with positive and negative affect subscales, as well as a one-item measure. Gerber and Wheeler included the one-item measure that yielded a significant difference, but they omitted the PANAS, which did not find even a slight hint of a difference (both Fs < 1). It seems inappropriate to conduct a meta-analysis of mood purporting to assess whether a difference exists when one selectively includes the significant result and deletes the two null results from the very same experiment. Whereas omitting the entire 2002 paper could conceivably be ascribed to carelessness (albeit rather severe carelessness, insofar as it was published in the field's premier journal), one cannot easily invoke carelessness to explain their treatment of the 2003 paper. Obviously they obtained the article and read it, but they systematically selected only its results congenial to the point they wanted to make and omitted the same article's other results.

Although they ignored direct tests of the hypothesis published in top research journals, Gerber and Wheeler included some obscure sources such as unpublished undergraduate papers. One broad weakness of meta-analysis is that it treats all results the same regardless of source. Publication outlets are strongly differentiated by methodological rigor, and so essentially a meta-analysis will inevitably muddy the waters (unless results are consistent) by its failure to respect methodological rigor. Even so, excluding results published in the field's premier journal while including unpublished work by untrained researchers seems especially unacceptable.

As we wondered how Gerber and Wheeler could have omitted so many relevant results from their meta-analyses, one suggestion emerged. Many articles do not report means for null effects, and so if Gerber and Wheeler used a low-effort strategy of only using results for which means were published, they might achieve the bias we have documented, namely deleting many null results. We are reluctant to attribute such a manifestly unsuitable and ill-advised strategy to Gerber and Wheeler, because any reasonable person would surely know that it would severely distort the results of one's analysis, thereby rendering them worthless. Indeed, in our own experiences with multistudy papers, editors have instructed us to delete statistical details from nonessential null results on mood and emotion. More generally, if meta-analysts were to follow such strategies when editors likewise typically follow the procedure of saving space by suppressing means for null results, then the entire literature using meta-analysis should be regarded as systematically flawed. It seems incumbent on the field to ascertain whether such flaws are endemic to the majority of meta-analyses or are limited to particularly flagrant cases.

The selective exclusion of relevant null results is not even our main complaint against Gerber and Wheeler's meta-analysis. Still, we note that most scholars would already dismiss a work that purported to compile all the literature but in fact selected results favorable to its conclusion and preferentially excluded findings that went against it.

HOW NUMB CAN YOU GET?

Our main objection to Gerber and Wheeler lies not in their selective omission of large amounts of data contrary to their preferred conclusion. Rather, it lies in their distorted and misguided interpretations of the data they did include. These distortions are so severe that in many cases they interpret evidence that is directly opposite to their conclusion as if it were supportive of their conclusion. It was these profound misinterpretations, even more than their heavily biased exclusion of relevant results, that enabled them to reach their flawed conclusion that the numbness hypothesis is wrong.

We checked a fair number of the studies included in Gerber and Wheeler's Appendix—as many as the limited time and space we had for this reply would permit. They did report many findings that indicate significant results on emotion measures, usually indicating relatively more favorable moods among accepted and/or neutral control participants than among rejected ones. Such findings are presented as congenial to their conclusion that "rejection makes people feel bad" (p. 479).

Yet that conclusion is completely unjustified. In many cases, the mood means were on the positive side of the scale, indicating that rejected people felt good, not bad. Thus, Gerber and Wheeler took literal evidence that rejected people felt good (or neutral) and used it to conclude that people felt bad.

In many studies, participants in the control and acceptance conditions usually felt mildly happy. Those in the rejection condition often scored almost precisely on the neutral midpoint of the scale. On a scale of 1 (*bad*) to 7 (*good*), 4 is the midpoint, and so condition means close to 4.0 in the rejection conditions indicate the spot exactly between feeling good and feeling bad. Here are some of the means from rejection conditions that Gerber and Wheeler used to reject the numbness hypothesis when using that scale (which, incidentally, is one that Gerber and Wheeler recommend researchers use as furnishing results most congenial to their conclusion): 4.00 and 4.22 (Baumeister, Twenge, & Nuss, 2002), 4.29 and 4.21 (Van Beest & Williams,

2006), and 4.40 (Twenge et al., 2003). Twenge et al. (2002) also reported a mean of 4.07 among rejected persons, but that entire article was omitted from the meta-analysis.

It would be hard to imagine a result that confirms the emotional numbness hypothesis better than these means of 4 on a 1 to 7 scale. Yet Gerber and Wheeler repeatedly interpreted these results as contradicting the numbness hypothesis. The crucial point is that the evidence they present actually supports the conclusion opposite to the one they espoused.

Gerber and Wheeler ended up recommending that researchers use ad hoc, nonvalidated scales (or the PANAS) rather than some well-validated scales, because these furnish the sort of results that Gerber and Wheeler prefer. This recommendation strikes us an irresponsible attempt to influence the field in a scientifically inappropriate manner. Yet even on these made-up, nonvalidated scales that they said they prefer, the results frequently supported the numbness hypothesis, though Gerber and Wheeler denied this and insisted on treating them as contradictions to that hypothesis.

CATERING TO PREJUDICES: HOW REJECTION PROBABLY FELT

In seeking to show that "rejection makes people feel bad" (p. 479), Gerber and Wheeler were selectively uncritical. They criticized any evidence of numbness with great energy, but they covered evidence against numbness, even seriously flawed evidence, without critique.

Yet surely most biases in data collection would diminish any appearance of numbness. Therefore, the widespread evidence in favor of it (though mostly ignored or misinterpreted by Gerber and Wheeler) is especially impressive given that it had to overcome these biases.

First, people assume that rejection causes distress. A study by Twenge et al. (2008) is the only one we know that randomly assigned people either to be rejected and report their feelings or to imagine undergoing the same procedure and predict how they would feel. Participants who experienced the actual exclusion reported a neutral, numb state, whereas those who merely imagined the rejection experience predicted that they would be extremely upset if such a thing were to happen to them. Thus, intuitive predictions severely overstated actual emotional reactions, consistent with much evidence about affective forecasting (Wilson & Gilbert, 2003). Viewed in that way, Gerber and Wheeler's conclusion that rejection makes people feel bad is an instance of catering to prevailing prejudices: It confirms what people predict and imagine, though not necessarily what actually happens in many cases.

The strong expectation that rejection will produce distress will likely produce several biases. One is the simple demand characteristic. When researchers ask "How did social rejection make you feel in this experiment?", participants likely assume based on intuition that the correct answer is "terrible." Such a formidable and unavoidable demand characteristic ought to be noted, though it seems not to have occurred to Gerber and Wheeler.

Second, even apart from demand characteristics and participants' inferences about what responses are expected, participants' expectations can bias the most sincere responses. Such biases seem especially strong with memory. An impressive program of research by Ross (1989) and his colleagues has shown that people's memories are routinely distorted in the direction of their a priori expectations, assumptions, and theories. Memory is constructive, and remembered emotion and distress have been shown to change in such reconstruction.

In connection, we have noticed an important methodological difference that may help account for conflicting results. Some procedures (including our own) ask participants to report how they actually feel now, and these measures often indicate that participants feel rather neutral and numb. In contrast, some studies use retrospective measures that ask people how they felt earlier when they were rejected. These seem to find more reports suggesting actual distress (though typically still quite mild). This pattern precisely fits Ross's (1989) theory of memory distortion: Recollections of feelings are altered to conform to intuitive, a priori theories. In fact, some studies ask people to report on long-ago instances of rejection or to imagine how they would feel if they were rejected, in which case participants have nothing else to go on other than constructive memory and a priori assumptions about how one ought to feel.

Of equal importance is the difficulty of remembering that one felt nothing. A lack of emotion, by definition, does not leave a memory trace, and so when one is later asked what one felt, there is nothing in the memory to retrieve. Hence, the reconstruction process must rely heavily on a priori assumptions. Can someone vividly recall not having any particular emotion at any specific time even yesterday? A rigorous meta-analysis would separate studies that measure current emotion from those that ask how one felt previously.

In particular, retrospective (memory) measures will almost certainly pick up the maximum rather than the minimum emotion. Suppose that, as our theory predicts, someone received a stunning and unexpected rejection, reacted with some moments of emotional numbness, and then felt upset about it later. When asked to narrate the story long after this, the person would be more likely to remember the moments of most intense distress more than the moments of not being upset.

MISINTERPRETING CONTROL

Gerber and Wheeler's most novel conclusion is that many responses to rejection involve attempts to assert control. We attempted to reconstruct how they reached that conclusion. Our efforts were hampered by the many errors and inconsistencies in their Appendix. Gerber and Wheeler used a 5-point coding scale for the 78 studies they coded on control, in which

4 = unknown and 5 = "nonscale, nonindigenous measure" (whatever that means). Unfortunately, their Appendix indicates they coded many studies as 6, which was off the scale and had no meaning. Combining those with the unpublished studies they cited, we were unable to check one fourth of their codings.

The remaining codings were however sufficient to cast severe doubt about whether Gerber and Wheeler's conclusions should be taken seriously. For example, they coded sitting still and doing nothing as exerting control, whereas actively moving about to pick up spilled pencils was coded as not exerting control. (They offered a citation to Tedeschi & Felson, 1994, to justify this coding, but we contacted Felson and he disavowed having said any such thing.) They coded defection responses on prisoner's dilemma games as exerting control, whereas cooperative responses were coded as not exerting control. To our knowledge, no one in the long history of use of the prisoner's dilemma research has ever treated it as a measure of control. The same goes for the pain tolerance measures, which they misinterpreted as measures of assertion of control, contrary to a long literature. A paper by Litt (1988) was used by Gerber and Wheeler to support the notion that pain tolerance is "a way for rejected people to demonstrate their strength and power" (p. 469). But Litt's research was on perceived control, as in the belief that one could exert efforts to reduce the painfulness of the event. This is quite a different notion from tolerating pain in order to show one's strength.

Gerber and Wheeler were curiously silent about the biggest and best documented confound between control and social rejection: the procedure used for manipulating ostracism. Williams (2001, 2008) has repeatedly asserted that ostracism is not just a manipulation of social exclusion but that it also has a strong, palpable impact on people's sense of control. He and his colleagues have provided evidence that sense of control is diminished by ostracism (e.g., Van Beest & Williams, 2006). When ostracism research yields evidence of change in emotion, it is entirely possible that the emotion comes from deprivation of control rather than thwarted belongingness. It is quite possible that thwarting control may produce different effects from social exclusion. Whereas belongingness and exclusion are issues for adaptation to social life, control may be older and more basic than that, insofar as control over the physical environment is vital for adaptation and life already at very primitive levels (e.g., Baumeister, 2005).

Researchers interested specifically in the effects of rejection should perhaps avoid the ostracism procedures. At least, researchers interested in the effects of exclusion on emotion or other variables should hesitate to include ostracism research. The ostracism paradigms are rich in implications and the phenomenon is of interest in its own right, and there is definitely some overlap with social exclusion, but the very complexity of ostracism entails that it cannot be lumped in with manipulations aimed specifically at social exclusion.

CONSEQUENCES AND CONCLUSIONS

The debate about emotion loses some of its importance given that emotion is essentially irrelevant to the behavioral effects of rejection, as all sides (including Gerber and Wheeler) agree. So if emotion does exist, it does not seem to matter, at least in terms of the behavioral consequences. Gerber and Wheeler's focus on emotions following exclusion thus adheres to a recent tradition in the field that some of us have criticized (Baumeister, Vohs, & Funder, 2007): namely, the exploration of cognitive and affective phenomena that have little demonstrable relevance to anything that actually happens.

The ostensible, debatable emotional distress that supposedly follows from all social exclusions has not been shown to predict behavior except for one idiosyncratic case (Chow, Tiedens, & Govan, 2008). In contrast, the numbness we have documented does affect behavior. Twenge, Baumeister, DeWall, Ciarocco, and Bartels (2007) have shown that social exclusion decreases prosocial behavior and tested over half a dozen hypotheses about possible mediators. The only significant evidence of mediation came from emotional numbness: Rejected people experience an emotional shutdown, which reduces their capacity to feel empathy toward others, and this lack of empathy translates into a reduction of helping. Further work has suggested that the emotional numbness contributes to the heightened aggression among recently rejected persons (DeWall, 2008). Hence, we think it important to continue research with the numbness hypothesis.

Thus, the main contribution of Gerber and Wheeler has been to compile a biased sample of studies and misinterpret their results so as to provide ostensible but unwarranted support for the prevalence of emotional reactions that have no known consequences. Their conclusions about emotion, numbness, and control should be disregarded. The publication of their meta-analysis based on erratic and incomprehensible codings, omissions of substantial amounts of relevant data (mostly contrary to their theory), distorted and unjustified interpretations, and misuse of cited sources casts doubt on the ability of journal reviewers to evaluate meta-analyses and hence contain a strong implicit warning about reliance on meta-analyses in general.

The possibility that some individuals (both human and animal) respond to social exclusion with a numbness to physical pain that carries over into temporary emotional shutdown remains a fascinating and important possibility that warrants further research. It would be tragic for the field if an incompetent meta-analysis could stifle research by authoritatively proclaiming that this phenomenon does not exist. The field's ultimate capability to understand the full range of human experience and response should be carefully built up based on an inclusive and thorough appreciation of all relevant findings rather than wrong conclusions from neglectful work.

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