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PRIME NUMBERS: ANCHORING AND ITS IMPLICATIONS FOR THEORIES OF BEHAVIOR PRIMING

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Subtle primes can influence behavior, often in ways that seem irrational. Anchoring provides a compelling illustration of this: judgments can be influenced by anchors even when the anchors are known to be irrelevant and uninformative. In this article, we selectively examine the anchoring literature in order to evaluate a theoretical framework which has been employed to interpret many social and other priming effects. In this framework, primes are assumed to have broad effects, influencing a wide range of possible downstream behaviors, and these influences are largely automatic. The anchoring literature supports neither of these hypotheses. Anchors have narrow effects on behavior with little transfer across judgments, these effects can be controlled, and deliberate engagement with the anchor is a prerequisite for obtaining influences on later judgments. We question whether priming studies reveal evidence for the sort of automatic and consequential mental processes that are commonly proposed.

Can behavior be influenced by subtle cues in the environment? Can such influences occur when the cues are in some normative or informational sense irrelevant to the behavior in question? And if the answer to these questions is "yes," what are the psychological mechanisms that mediate these influences? We take it that these questions lie at the heart of recent debates about "social" and "behavior" priming (henceforth "priming").

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Although much of the recent controversy in this field has centered on the reality of some particularly eye-catching priming effects, the existence of subtle priming effects in general can hardly be disputed. Whether or not people can be primed to behave more or less intelligently by thinking about professors or soccer hooligans (Dijksterhuis & van Knippenberg, 1998; Shanks et al., 2013) or think differently about their emotional closeness to their family members after graphing a pair of points close or far apart on paper (Pashler, Coburn, & Harris, 2012; Williams & Bargh, 2008), no one seriously doubts that many behaviors can be subtly influenced. There are over 1,600 articles on Web of Science (WoS) on the priming of lexical decisions, for instance, where the speed to decide whether a letter string is a word or not is influenced by a preceding prime event, often the brief presentation of another letter string. Nor can it be doubted that long-lasting influences can occur. There are over 2,000 WoS articles on repetition priming, in which some response to the second presentation of a word, picture, or other item is altered as a result of an earlier presentation of the same item, often a long time (hours or days) previously. A striking illustration (reprinted in Gregory, 2005) shows a Dalmatian dog in a dappled image. Successful identification of this dog in the image can induce one-shot learning (priming) and affect perception of the same image years later.

In that case, why are priming effects so controversial? Why have many investigators been so unwilling to concede that asking participants to read sentences containing words related to the concept "old age" can induce them to walk more slowly down a corridor (Bargh, Chen, & Burrows, 1996; Doyen, Klein, Pichon, & Cleeremans, 2012)? Of course, one answer to this question is that behavior priming studies have focused attention on a range of dubious research practices that probably pervade the whole of psychology. Many priming studies have been underpowered, employed questionable statistical methods, or are simply unreplicable (excellent discussions of these issues in relation to experimental psychology generally can be found in Asendorpf et al., 2013; Bakker & Wicherts, 2011; Bertamini & Munafò, 2012; Francis, 2012; Klein et al., 2012; Kruschke, 2013; Pashler & Harris, 2012; Rouder, Speckman, Sun, Morey, & Iverson, 2009; Schimmack, 2012; Simmons, Nelson, & Simonsohn, 2011).

Another possible answer is that whereas standard priming effects such as lexical and repetition priming seem in some sense to be rational, many other behavior priming effects seem distinctly irrational. If one were designing a system for the rapid decoding of letter strings, then it might make sense for it to be biased by what was perceived a few tens or hundreds of milliseconds previously. If one were designing a system for identifying hidden objects, it might make sense to allow it to access and be influenced by memories of similar objects seen in the past. But how can it be rational for judgments about our emotional closeness to our family members to be affected by the proximity of a pair of points we have connected on a sheet of paper, or for our judgments of risk to be influenced by the activation of romantic thoughts (Greitemeyer, Kastenmüller, & Fischer, 2013)? Although it might be hard to reconcile such findings with rationalistic views of mind and behavior, we do not believe this provides reasonable grounds for doubting the reality of these priming effects themselves. In the present article, our focus will be on one particular priming effect, namely anchoring, and there is abundant evidence that this effect can often be profoundly irrational. For example, people's judgments

can be influenced by an anchor even when they have seen that the anchor was randomly generated (Chapman & Johnson, 2002).

If the seeming irrationality of some priming effects is not a good reason to doubt their reality, then how else can we explain the persistent doubts that researchers have expressed? The answer that we explore here is that priming effects have tended to be couched in a theoretical framework which many researchers find unconvincing and that resistance to the framework has led to doubts about the experimental findings on which that framework is based. In brief, priming effects have been taken as evidence for the idea that primes automatically trigger mental processes, and that this triggering can have widespread consequences (e.g., Bargh, 2006). For instance, in reviewing the literature, Bargh and Huang (2009, p. 128) asserted that:

> [T]his priming research has shown that the mere, passive perception of environmental events directly triggers higher mental processes in the absence of any involvement by conscious, intentional processes

while Loersch and Payne (2011, p. 235) suggested that:

> If, for example, people were exposed to words related to the concept of hostility (e.g., "hit," "punch," "aggress"), it could reasonably be predicted that they would subsequently (a) be faster to identify a gun (semantic priming; Meyer & Schvaneveldt, 1971); (b) perceive another individual as more hostile (construal priming; Higgins, Rholes, & Jones, 1977; Srull & Wyer, 1979); (c) behave in a more hostile manner themselves (behavior priming; Carver, Ganellen, Froming, & Chambers, 1983); and (d) become motivated to actively seek out an opportunity to aggress against some other person or object (goal priming; Todorov & Bargh, 2002).

Thus, priming effects are viewed as arising unconsciously and automatically, beyond the individual's control, and with wide-ranging consequences on behavior. By comparison to typical theoretical models for lexical and repetition priming (Lachter, Forster, & Ruthruff, 2004; McNamara, 1992; Tenpenny, 1995), these are striking assertions because decades of research have raised more questions than answers concerning automatic and unconscious effects generally (Newell & Shanks, 2014), and a wealth of research in cognitive psychology has shown that far from being broad in their consequences, primes tend to have very narrow effects on judgments and behavior.

ANCHORING AS PRIMING

In the present article, we analyze these claims in the context of anchoring. Thus, we take as our domain a priming effect which is both beyond dispute in terms of replicability and which undoubtedly has all the hallmarks of the sort of irrationality that makes many of the headline priming effects so eye-catching. We ask whether the evidence supports the idea that anchors can have automatic and unconscious influences on judgments and whether their effects are narrow or broad. We assume a relatively broad definition of priming as simply reflecting any influence on

later behavior (be it reports of judgments, impressions, attitudes, choices, or any other overt and observable act) of prior stimuli or events without deliberate intent to be influenced by them. Whereas some authors (e.g., Molden, this issue) prefer to include automaticity in their definition, we regard the question of whether examples of priming are or are not automatic as an empirical, not a definitional, matter.

A typical anchoring experiment employs a two-step procedure. In the first step, participants are asked whether the target attribute is higher or lower than the anchor, and in the second step, they give a numerical estimate of the target attribute. Thus, the first step might involve deciding whether John Kennedy first became president before or after 1962, and the second stating in which year he first became president. Anchoring is obtained if the estimate is drawn towards the anchor. Although anchoring effects might seem at first glance entirely consistent with deliberative thinking (and indeed it has been argued that anchoring in many circumstances may be a rational response by the individual to the implied communicative intent of the experimenter to transmit useful information—see Mussweiler & Strack, 1999), they are pervasive even in situations where the individual knows that the anchor value is uninformative, for instance when the person generates it by reading the last two digits of their social security number (Ariely, Loewenstein, & Prelec, 2003). Equally striking is Critcher and Gilovich's (2008) demonstration that incidental anchors can bias judgments: they found that participants' estimates of how much they would pay for a meal in a restaurant depicted in a photograph were higher if it was called Studio 97 than Studio 17, even though they were not explicitly required to think about the restaurant's name (we return to this study later).

Although our focus on anchoring will inevitably restrict the generality of the conclusions we can draw, we contend that anchoring serves as a prototypical example of the sorts of priming effects that have been the subject of so much recent controversy. If priming is defined as an incidental influence of stimuli or events on subsequent behavior, then clearly anchoring is an instance of priming (see Kahneman, 2011). Moreover, the significance of anchoring is probably considerably greater than for some other priming effects. Even if people think differently about their emotional closeness to their family members after graphing a pair of points close or far apart on paper (a questionable claim: Pashler et al., 2012), the wider consequences of such priming would be fairly modest. In contrast, anchors have been shown to influence buying and selling prices, purchasing decisions, credit card repayments, negotiation outcomes, jury verdicts, and so forth. As with repetition priming (Roediger & McDermott, 1993), anchoring can be long-lasting: anchors can bias judgments made even a week later (Mussweiler, 2001).

It is fundamental to emphasize that anchoring typically involves much more than simply priming numbers. First, it involves magnitudes rather than numerical concepts, and second, it is often mediated by priming of semantic features of the target object. When, as a result of some anchoring induction, participants give a larger estimate of the number of calories in a cheeseburger, this is not simply because a particular number has become more mentally accessible: they actually conceptualize the cheeseburger as being located at a different point on the calorie scale. Frederick and Mochon (2012) showed participants a list of 13 food items in ascending order from least caloric (hard-boiled egg) to most caloric (Burger King Whopper with cheese) and asked them to choose the item they judged closest to 400 calories. When participants had previously estimated an average apple's

calories, they chose a more calorific item from the set. As this example illustrates, anchoring effects occur even when no numerical estimation is required. They also occur when the anchor is entirely non-numerical, and hence when no number processing is involved at all. For example, LeBoeuf and Shafir (2006) asked participants in one condition to add pennies to an empty cup, while those in another condition removed pennies from a cup which initially weighed 12 ounces, until the cup weighed the same as another cup they had held and subjectively weighed earlier and which actually weighed 6 ounces. The starting weight of the cup acted as an anchor such that final cup weights were larger in the group adjusting downwards from a high anchor (12 ounces) than in the group adjusting upwards from an empty cup (0 ounces). In other such physical, non-numerical anchoring studies, LeBoeuf and Shafir used lines of different length or music clips of different loudness as anchors.

Moreover, in many instances anchoring is mediated by selective semantic priming of the target's features. A compelling illustration of this was provided by Mussweiler and Strack (2000). They first asked participants to judge whether the annual mean temperature in Germany is higher or lower than 20°C. As a result of this standard high anchoring induction, participants decided more quickly that letter strings like *swim* and *beach* were valid words compared to *frost* and *winter*, while the converse pattern was found for other participants for whom the anchor was low, 5°C. Thus, the anchor changed the way in which participants conceptualized the target, Germany, selectively making some of its features more accessible than others. These demonstrations that anchoring extends beyond just the mental accessibility of numbers are important because if anchoring simply pertained to numerical concepts, one might legitimately wonder whether it has any implications for priming in general, given the obvious difference between the narrow conceptual representations primed by exposure to discrete numbers and the broader representations involved in other forms of semantic, trait, stereotype, goal, or behavior priming.

To what extent is anchoring an automatic process which transfers broadly across a range of judgments and behaviors? Automaticity is of course a complex concept, but here we adopt the standard viewpoint (Bargh, 1994; Moors & De Houwer, 2006) that it is characterized by (some or all of) four key features, which in the context of priming are: (1) absence of awareness of the prime, (2) absence of awareness of the prime's effect on behavior, (3) uncontrollability of the prime's influence, and (4) persistence of the prime's influence even when cognitive resources are diminished. We evaluate anchoring against the first three of these features. The fourth criterion has not been the subject of much research in the anchoring literature (though see footnote 3). To be clear, we do not assume that any one of these features is more important than the others, nor do we assume that anchoring would have to meet all of the criteria to be recognized as (at least in some sense) an automatic and unconscious process.

PROCESSING ACCOUNTS OF ANCHORING

To set the scene for our assessment of whether anchoring is an automatic process which transfers broadly, it is important to briefly discuss the range of information-processing accounts of anchoring that have been developed, as a way of introduc-

ing key explanatory constructs. A considerable body of research has explored two general classes of explanation. In the anchoring-and-adjustment account, which was first proposed by Tversky and Kahneman (1974), individuals are assumed to take the anchor as a reasonable starting point for their judgment and then move away from it as they retrieve relevant information from memory. However, these adjustments are assumed to be conservative and insufficient. In the selective accessibility model (Strack & Mussweiler, 1997), in contrast, the anchor is assumed to render anchor-consistent features of the target of judgment accessible via a process of semantic activation. These activated features then bias subsequent judgments. For example, if participants in the first step are asked to decide whether a typical Mercedes-Benz car costs more or less than €40,000, they might access the knowledge that they are usually classified as luxury cars. When asked in the second step to estimate the cost of a typical Mercedes-Benz car, participants rely heavily on whatever knowledge is most accessible, and the activated knowledge (luxury car) therefore plays a larger role in judgment formation than it would if it had not been activated by the anchor.

A large number of studies (see Chapman & Johnson, 2002) have sought to test these and other accounts of anchoring. A common viewpoint is that some forms of anchoring are best explained by anchoring-and-adjustment and others by selective accessibility. For instance, Kahneman (2011) interprets the former as a System 2 capacity (effortful, slow, conscious) and the latter as a System 1 capacity (automatic, fast, unconscious), each being evoked under different circumstances. Similarly, the attitudinal model of Wegener and colleagues (2010) distinguishes thoughtful from non-thoughtful routes to anchoring. These models share the assumption that anchoring, at least under some circumstances, can be a non-deliberative, automatic process. What is the evidence for this key claim?

SUBLIMINAL PRIMING

One way of demonstrating that anchoring can occur automatically is to show that anchors influence judgments even when they are barely attended to and are not deliberately processed as part of the task. Critcher and Gilovich (2008) examined this possibility by asking participants to make judgments about scenarios that were accompanied by photographs incorporating incidental anchors. In one experiment, for instance, a fictitious college linebacker, Stan Fischer, was described alongside a photograph of him wearing a jersey with either the number 54 (low anchor) or 94 (high anchor). Despite the fact that participants were not required to make any explicit judgment about the jersey number (as they would in a conventional anchoring task)—and indeed may have barely registered it—participants nevertheless judged Fischer more likely to register a sack in the conference playoff game in the high than in the low anchor condition.

It is nevertheless possible that at least some participants did think about the jersey number and that conscious reflection is a prerequisite for anchoring even in situations like this. A more compelling, though controversial, technique for demonstrating the automaticity of priming effects is to present the anchor prime subliminally, outside awareness. It is intriguing that in the wake of a comprehensive methodological debate nearly 30 years ago (Holender, 1986), subliminal processing was afforded a rather modest role in most theoretical debates about the causa-

tion of behavior. Yet in recent years there has been a wealth of claims concerning the importance of the unconscious in behavior, including some striking reports of subliminal priming effects, among them anchoring. Here, we do not attempt to review this extensive literature. We do, however, briefly comment on the pervasive methodological problems that plague interpretation of results in this field (Holender, 1986; J. Miller, 2000; Pratte & Rouder, 2009), and we illustrate these problems with reference to claims about subliminal influences on anchoring.

Adaval and Wyer (2011) asked participants to estimate how much they would be willing to pay for a DVD player or a pair of shoes. Beforehand, prime anchors that were either low (e.g., HK$112) or high (e.g., HK$9,779) were flashed for 16 msec on the computer display and masked to render them invisible. Adaval and Wyer found a typical anchoring effect in that participants were willing to pay more after a high than a low anchor (though the effect was only significant for the DVD player question and not for the shoes question). How did Adaval and Wyer confirm that the anchors were truly invisible? After making their judgments, participants were shown a further masked prime sequence, but this time were asked to write down whatever they saw after each trial. Twenty such trials were presented. No participant reported seeing any of the subliminal primes.

There are substantial problems with the inference that unconscious anchors exerted an influence on judgments in this study. For instance, the form of awareness check employed by Adaval and Wyer is susceptible to bias if participants' confidence about seeing the anchor prime is low. Even if they can actually see the prime occasionally, they may nonetheless give a negative report because their judgment is uncertain and they adopt a conservative response criterion. Bias can easily be reduced or eliminated by employing a procedure in which participants have to make a forced choice, such as "Was the briefly flashed number HK$112 or HK$9,779?"

In one of their experiments, Mussweiler and Englich (2005) asked participants to judge the annual mean temperature in Germany after thinking about this question for 1 minute, during which a briefly presented anchor was flashed 10 times. The anchor value was either high (20) or low (5) and was flashed for 15 msec every 6 seconds during the thinking period and masked by a consonant string. Judgments assimilated towards the anchor value: the temperature was estimated as higher after a high than after a low anchor. To evaluate whether the primes were consciously perceived, Mussweiler and Englich used a funnel debriefing in which a series of more and more specific questions was asked about the priming stage. They reported that 2 of 37 participants indicated some awareness, whereas the remainder did not. These 2 participants were excluded from the analysis. Many commentators have noted the limitations of such recall-based awareness assessments, however (Dawson & Reardon, 1973; Newell & Shanks, 2014), and have pointed out that their retrospective nature means that they are evaluating awareness for events that happened some time previously and that low confidence knowledge may be withheld. We thus need to examine evidence from alternative and more sensitive awareness checks.

In a further experiment (Study 2), Mussweiler and Englich (2005) used a more comprehensive awareness check. In this study, participants judged the average price of a mid-sized car while high (30,000) or low (10,000) anchors were flashed during the thinking period. After making their judgments (which again showed a reliable anchoring effect), participants were presented with a prime phase once

again but this time were told that briefly presented numbers were being flashed and were asked to judge whether 10,000 or 30,000 was the flashed digit string. Ten such prime identification trials were presented. Mussweiler and Englich found that performance in this test was virtually at chance (50% correct) and concluded that the primes were indeed invisible.

A prime identification test such as this is methodologically far more sound and permits stronger inferences than a funnel debriefing or the type of test used by Adaval and Wyer (2011). It is not susceptible to the complaint that it relies on retrospective recall or to the objection that it might be contaminated by response bias: Since the test demands a forced choice between the two anchor values, participants should select the string they saw regardless of their confidence. But despite these advantages, such a test can still contribute towards the reporting of false positive subliminal perception results. One reason for this is that forced-choice tests with few trials are underpowered for detecting what is likely to be weak awareness. For example, imagine that a participant has a "true" long-run probability of 0.6 to discriminate the high and low anchors. This participant may consciously see enough of the anchors to show an entirely standard and supraliminal anchoring effect. But with only 10 binary choice trials, there is a high probability (almost .4) that this participant will be misclassified by the forced-choice awareness test (that is, will make 5 or fewer correct identifications and hence be judged to lack any awareness of the prime). This problem persists even when data are aggregated across participants. A typical statistical test based on only a small number of binary observations per participant is likely to have only low or moderate power to reject the hypothesis that discrimination is weakly but truly above chance (e.g., 0.6) (Rouder, Morey, Speckman, & Pratte, 2007). To eliminate or at least ameliorate this problem, the awareness test needs to employ far more than 10 trials (say 50).

Worse still, Pratte and Rouder (2009) have shown that typical forced-choice tests used to measure awareness in subliminal perception experiments (such as that used by Mussweiler and Englich) may significantly underestimate conscious perception as a result of task difficulty. Because tests assessing perception of near-threshold stimuli are very difficult, participants may lose motivation. In their experiments, Pratte and Rouder maintained participants' motivation by intermixing above-threshold and near-threshold stimuli and found that identification of the near-threshold stimuli increased reliably. Thus, brief stimulus presentations that would have been regarded as subliminal in a conventional awareness test were found to be supraliminal in a modified test designed to be more closely equated to the main priming test in terms of difficulty. Until subliminal priming experiments are able to rule out such artifacts, their conclusions will remain in doubt. Recent methodological advances (e.g., Rouder et al., 2007) offer the promise of more clear-cut tests of subliminal perception in the future.

Even if these subliminal priming experiments fail to provide compelling evidence that anchoring can occur automatically,[1] isn't the type of demonstration provided by Critcher and Gilovich (that incidental and irrelevant numbers can anchor judgments) sufficient to persuade us that anchoring can nevertheless occur automatically? It is common to think of automaticity as a continuum, so doesn't the effect of a jersey number on a judgment establish that anchoring extends up

1. Reitsma-van Rooijen and Daamen (2006) were unable to obtain a subliminal anchoring effect under normal conditions but did obtain an effect when judgments were made under time pressure.

to the automatic end of this continuum? The problem with this conclusion is that the effects documented by Critcher and Gilovich (2008) were remarkably fragile. From a Bayesian statistical perspective, the evidence they reported actually provides more support for the null hypothesis (no anchoring) than for the experimental hypothesis. As Matthews (2011) has noted, a study with a very large sample size and a test statistic that is only just significant provides evidence that should, if anything, persuade us more firmly to believe the null hypothesis. Further studies of incidental anchoring are much needed.

The studies reviewed in this section assess whether the simple presentation of a number can induce anchoring, and thus employ a "basic" anchoring method that is rather different from the standard method in which an explicit comparative judgment is made in relation to the anchor. Basic anchoring effects are extremely fragile though even when some degree of deliberate processing of the anchor is required, a finding which must cast further doubt on the subliminal effects discussed above. Wilson, Houston, Etling, and Brekke (1996) found that numbers influenced judgments if participants had copied 5 pages of these numbers, not if they had copied only one. Brewer and Chapman (2002) found that even this effect was weak and restricted to some very specific circumstances. It is certainly not the case that numbers randomly and incidentally encountered in the environment inevitably induce anchoring effects.

AWARENESS OF THE INFLUENCE OF AN ANCHOR

Studies employing supposedly subliminal stimuli seek to evaluate the effect of anchors when the individual is unaware of the anchor's presence. A related question, which focuses on a different criterion for automaticity, is whether individuals show anchoring even when they are unaware of the influence of the anchor. Even when the anchor is consciously perceived and processed, as it is in a typical anchoring situation, its influence may not be consciously registered, and in that case we would have to conclude that anchoring can be an automatic and unintentional process.

In Wilson and colleagues' (1996) study, participants were asked to estimate the number of physicians in the local phone book after processing an irrelevant numerical anchor, and were subsequently asked to assess the influence that the anchor had had on their physician estimates on a 9-point scale ranging from 1 ("decreased it a great deal") to 9 ("increased it a great deal"). Wilson and colleagues did not report the group mean estimate on this scale, so it is not known whether participants on average believed their physician estimates were affected by the anchor. Wilson and colleagues did state, however, that about three-quarters of the participants gave a rating of 5, labeled "have no effect," and despite believing there was no influence, these participants showed a robust anchoring effect. On the other hand, when evaluated across all participants, estimates of how much they were influenced did correlate significantly with their physician estimates, and as Wilson and colleagues concluded (p. 393), "the higher people's estimates of the number of doctors, the more they believed they were influenced by the anchor value." Thus, the conclusion of this study is not clear-cut: The average participant

reported an influence of the anchor, while at the same time many participants who reported that it had no influence on them were affected by the anchor.

In an applied setting, Northcraft and Neale (1987) found that anchors (suggested listing prices) influenced the pricing decisions that both non-experts (students) and professional real estate agents made when they spent 20 minutes viewing a residential property. On a debriefing questionnaire, about half the non-experts reported that they had given consideration to the anchor in deriving their pricing decisions. Rather fewer (around a quarter) of the real estate agents did so. Thus, sizable numbers of participants (especially experts) did not report an influence of the anchor on their judgments. Interpretation of this pattern is not straightforward, however. First of all, the experts were rather less (though nonsignificantly) affected by the anchor,[2] so reports of an influence of the anchor correlated overall with anchoring itself. Second, it is possible that the anchoring effect was entirely borne by those participants who reported incorporating the anchor into their estimates. This may be unlikely, given how robust anchoring effects tend to be, but future research could usefully separate anchoring effects in aware and unaware individuals. Third, it is notoriously difficult to assess awareness exhaustively (e.g., Ericsson & Simon, 1984; Newell & Shanks, 2014) and experts may have avoided reporting use of the anchor because of the situational demands. As Northcraft and Neale (1987, p. 95) themselves put it, "[I]t remains an open question whether experts' denial of the use of listing price as a consideration in valuing property reflects a lack of awareness of their use of listing price as a consideration, or simply an unwillingness to acknowledge publicly their dependence on an admittedly inappropriate piece of information."

One methodological issue that future studies might address is that in order to accurately report the causal effect of an anchor, participants normatively need to experience both what their estimates would be with and without the anchor, and of course this is unfeasible in what is necessarily a between-subjects design in which different groups receiving different anchors are compared. As Hogarth (2014) has noted, mismatches may occur between verbal reports about causal influences and the reality of those influences as a result of experimenters and participants adopting different perspectives on the "causal field." An experimental participant might deny that an anchor influenced her behavior, whereas an experimenter able to compare behavior between subjects in conditions of low or high anchors might conclude in contrast that there was an influence. Such differences in conceptualization of the causal field might lead to erroneous conclusions, as the participant is surely right that (from her perspective) she only experienced one value of the anchor and therefore does not have the evidence necessary to assign it a causal role.

CAN THE INFLUENCE OF ANCHORS BE INTENTIONALLY AVOIDED?

We argue that it is a misconception to view priming effects as low level, unconscious, and automatic. A powerful reason why early studies of anchoring have

2. Combining all 4 pricing estimates participants made in Experiment 2, the lowest and highest anchors induced a 12% influence on experts' estimates, while the effect was more than twice as large, 27%, in the non-experts.

been taken as providing some encouragement to this viewpoint is that they appeared to show that people find it very hard to avoid being influenced by anchors. Of course, if subliminal anchoring can occur, or if anchors can bias judgments even when individuals are unaware of this influence, then it would follow that anchoring effects cannot always be avoided: If you don't believe the anchor has affected you, then there is subjectively no influence that you believe needs avoiding. Yet the preceding sections have highlighted that the evidence for these effects is rather weak. The controllability of the influence of anchors thus relates to a different aspect of the standard multi-faceted conception of automaticity.

Early studies examining avoidance more directly revealed that the bias is undiminished by forewarning participants about the potential influence of anchors (Wilson et al., 1996) and that increased motivation to be accurate (induced for instance by financial incentives) is usually ineffective (see Chapman & Johnson, 2002). But more recently it has become apparent that considerable control can be exerted over the bias, at variance with the automaticity view. Epley and Gilovich (2005) showed that the effect of self-generated anchors was influenced by financial incentives. For instance, when participants are asked to estimate the freezing point of vodka, they tend to generate the freezing point of water (0°C) and use this as an anchor, an effect that was attenuated by an explicit incentive designed to encourage participants to think more deeply.

Although this result suggests that the proposed automaticity of anchoring has been overstated, Epley and Gilovich also found that incentives had no effect on the size of the bias induced by externally generated anchors. Thus, in a standard situation in which participants first judged whether Mt. Everest is higher than 45,500 feet and then judged its height, the influence of the anchor was unaffected by incentives. Epley and Gilovich proposed that there are multiple (or at least two) forms of anchoring bias, one of which depends on controlled deliberate thought (self-generated anchors) and one of which depends on automatic semantic priming (externally generated anchors), but even this viewpoint may overstate the role of automatic processes. Simmons, LeBoeuf, and Nelson (2010) found that they could attenuate both forms of anchoring with incentives for accuracy. Their key insight was that even if participants are highly motivated and have the deliberative capacity to overcome an anchor's influence, they may have little ability to do so if they are uncertain about which way to adjust from the anchor. Imagine that you are asked to estimate the year in which the actor Jack Nicholson was born (the correct answer is 1937) and are given 1945 as an anchor. In other words, you first decide whether he was born before or after 1945, and then estimate the correct year. Under conditions of high motivation, you are aware that you need to adjust sufficiently from the anchor, but the problem is that you don't know whether the anchor is pulling your estimate up or down. You don't know what your estimate would have been counterfactually in the absence of the anchor, and hence do not know whether the 1945 anchor is pulling a low counterfactual estimate upwards or a high counterfactual estimate downwards. If you believe that the anchor is pulling your estimate upwards, then you will adjust downwards. If you believe that the anchor is pulling your estimate downwards, then you will adjust upwards. But one of these influences will result in a greater, not weaker, influence of the anchor on your answer. It is easy to see that by aggregating across items for some of which the motivated adjustment is in the correct direction and for some of which it is in the incorrect direction, a null effect of motivation can be obtained.

Simmons and colleagues tested this account in a number of ways. For example, they showed that motivation does reduce the anchoring effect when the anchor value is implausible (such as a date of 1977 for Jack Nicholson's birth). Under such circumstances, where the anchor value is so extreme that it can immediately be recognized as being too high, participants were unlikely to be in any doubt about the required direction of adjustment.

The fact that the influence of an anchor on judgments can be attenuated under conditions of heightened motivation (through financial incentives) speaks directly against the hypothesis that anchors affect judgments automatically. Rather, the anchor is one piece of evidence taken into account in the individual's deliberative thinking. Further support for this alternative viewpoint comes from two sources. First, Epley and Gilovich (2006) found that overcoming the effects of self-generated anchors was impaired under conditions of cognitive load. Thus deliberative System 2 capacity is required by whatever process attenuates anchoring.[3] Second, it has been demonstrated that the effects of anchors can be diluted by adopting deliberative reasoning strategies such as "consider the opposite." As Larrick (2004) notes, this strategy simply amounts to asking oneself, "What are some of the reasons that my initial judgment might be wrong?" Mussweiler, Strack, and Pfeiffer (2000) provide an experimental example of the strategy in the context of anchoring by demonstrating that the magnitude of the anchoring effect can be reduced simply by asking people to list anchor-inconsistent arguments. Mussweiler and colleagues presented car experts with an actual car and an anchor estimate, either high (5000 German Marks) or low (2800 German Marks). Following the standard procedure in anchoring experiments, the expert first decided whether the anchor was too high or too low, and then provided his own estimate. The novel manipulation was that before providing an estimate, some of the experts were instructed to consider possible reasons why the anchor value might be inappropriate. The results indicated a clear effect of this manipulation: When the experts were instructed to generate anchor-inconsistent arguments, the anchoring effect was attenuated. For example, experts provided a mean estimate of 3563 German Marks when given the high anchor and not asked to generate opposite arguments, compared to an estimate of only 3130 German Marks when required to generate anchor-inconsistent arguments beforehand.

A related question is whether anchoring effects can be attenuated in experts who have knowledge of the judgment domain, as compared to non-experts who do not. If deliberative processes such as intentional memory retrieval play a role, then an expert who knows a great deal about German cars ought to be able to dilute the effect of an anchor by accessing relevant knowledge. Conversely, if anchoring is as strong in experts as in non-experts, this would imply that it is driven by automatic (System 1) processes. Just as with studies on incentives, several early reports (e.g., Northcraft & Neale, 1987; but see footnote 2) suggested no effect of knowledge level, but more recent research challenges this conclusion. Smith, Windschitl, and Bruchmann (2013) reported 4 studies in each of which anchoring was attenuated (though not eliminated) in individuals with greater expertise. For example, when

3. As with the Epley and Gilovich (2005) work on incentives, Epley and Gilovich (2006) found that cognitive load did not affect anchoring with externally provided anchors. But Simmons and colleagues' (2010) results suggest that this latter failure again may be an artefact of uncertainty about the required direction of adjustment. The same point applies to results reported by Blankenship and colleagues (2008).

asked the questions, "How many US states are west of the Mississippi River?" and "How many states in India have a population of more than 25 million people," anchoring was weaker in US participants for the question about the US and weaker in Indian participants for the question about India.

Plainly, much if not all of the influence of an anchor is mediated by deliberative thinking. We acknowledge the possibility that anchoring effects are not completely controllable and that there may in principle be residual effects that are immune to deliberative processes. It must remain an important question for future research whether or not unequivocal evidence for this can be obtained (see Englich, Mussweiler, & Strack, 2006, for a striking example).

BREADTH OF TRANSFER

As the quotation from Loersch and Payne (2011) above highlights, a common assertion in the priming field is that a prime can have a broad influence on behavior. If true, this would be a surprising finding and would require an account of priming which is very different from the sorts of explanations typically put forward to explain effects such as repetition and lexical priming. These effects are usually assumed to arise from some process in which specific perceptual or semantic features of the prime are activated and can influence responses to a target to the extent that the target shares those features. Lexical decision and repetition priming effects tend to be extremely narrow in the extent to which they show transfer. What gets activated in most situations is a specific representation that is stimulus- and response-bound. For instance, making a man-made (yes/no) judgment of a visual object in the first stage does not prime making a bigger-than-a-shoe-box (yes/no) decision in the test (Horner & Henson, 2009), and other research shows that almost any change in the processing engaged by the target relative to that engaged by the prime dilutes the magnitude of repetition priming (Franks, Bilbrey, Lien, & McNamara, 2000). A large literature documents the dilution of cross-modal compared to intra-modal priming: Whereas responding to the written word *knife* will typically be primed by reading the word previously, this influence will typically be much reduced or even absent if the prime (*knife*) is heard rather than read (Roediger & McDermott, 1993).

This narrowness of transfer applies to many instances of anchoring as well. Frederick and Mochon (2012) reported that while judging the weight of a raccoon or a whale in pounds influenced later estimates of the weight of a giraffe in pounds, no such anchoring influence was obtained when the weight of the raccoon was estimated on a 7-point heaviness scale or if the weight of the whale was estimated in tons. Frederick and Mochon concluded that anchoring effects only occur on the specific scale on which the object has been judged and not on other scales, even if they are related, such as weight in pounds and weight in tons. They explained this narrowness by a scale distortion mechanism in which the initial decision concerning the anchor distorts the psychological scale and hence biases the subsequent judgment. A similar failure to find transfer across scales was reported by Chapman and Johnson (1994) who found that dollar anchors did not influence life-expectancy estimates.

Even more strikingly, anchoring effects can be very narrow even within the same judgment dimension. Strack and Mussweiler (1997) found that asking participants

to make a comparative judgment with respect to one attribute of an object (such as the height of the Brandenburg Gate) had little effect on their absolute judgments of this object with respect to a different attribute (e.g., the width of the Brandenburg Gate), even though both are on the same scale.

Adaval and Wyer (2011), in the study discussed previously, reported a somewhat more nuanced (and complex) pattern of transfer across attributes. In an experiment using supraliminal price anchors, participants were asked to judge whether the average price of an electronics product (e.g., a DVD player) or an article of clothing (e.g., running shoes) was higher or lower than a high (or low) price anchor. The product category (clothing/electronic) and the anchors (high/low) were both manipulated between subjects. Participants were then asked a willingness-to-pay (WTP) judgment about a target product which was the same, related, or unrelated to the product about which they had made the original anchor judgment. The results revealed an asymmetry whereby anchoring on an electronics product led to raised WTP for the *same* target item, but had no effect on related (i.e., another electronics product) or unrelated (i.e., an article of clothing) products. In contrast, anchoring on an item of clothing led to higher (lower) WTP for all three target types (same piece of clothing, another clothing item, and an electronics product) as a function of the originally presented price anchor value.

Adaval and Wyer (2011) suggest that this asymmetric transfer arises because electronics products tend to be evaluated on the basis of product-specific features (e.g., laser quality in a DVD player), whereas clothing is evaluated on the basis of more generic attributes such as style, attractiveness, and so forth. Thus, comparing the price of a piece of clothing with a high or low anchor value will prime price estimates for electronics products because the generic activated attributes will readily transfer across product categories. However, thinking about prices of electronics products will not prime estimates for clothes because the specific activated attributes are largely inapplicable to clothing.

While there might be some limitations to this account—for example, presumably brand status is an activated attribute in both clothing and electronics price comparisons—the findings nonetheless highlight the relatively narrow nature of transfer. It is clearly not the case here that activation of the concept "high (low) value" via a price anchor has general effects on downstream behavior. Rather, the activated attributes have a specific or selective (cf. Strack & Forster, 1995) influence on particular subsequent judgments.

In contrast to these examples of relatively narrow transfer, Oppenheimer, LeBoeuf, and Brewer (2008) reported four experiments in which they found a variety of much broader *cross-modal* anchoring effects. How compelling is their evidence? In their first experiment, Oppenheimer and colleagues gave participants a piece of paper with either three short (1-inch) or three long (3.5-inch) lines and asked them to copy the lines (without using a ruler). In a subsequent (apparently unrelated task), they were then asked to estimate the length of the Mississippi River in miles. Surprisingly, participants who had drawn the short lines estimated on average that the river was shorter (M = 720 miles) than those who had drawn long lines (M = 1,224 miles) (the correct answer is 2,320 miles). Even more surprisingly, in a follow-up experiment (Experiment 2), participants primed with the longer lines estimated the average temperature in Hawaii in July to be higher (M = 87.5F) than those primed with short lines (M = 84.0F). Oppenheimer and colleagues explained these results by arguing that the line-drawing task primed or activated a general

notion of magnitude (largeness or smallness) that then transferred to subsequently encountered stimuli and questions. They say:

> We propose that large or small anchors may prime the notion of their general magnitudes (e.g., "largeness" or "smallness") and that the activated sense of magnitude may be influential when judges next form an estimate, leading to an anchoring effect. That is, merely activating a sense of size, unattached even to a rating scale, may bias subsequent judgments to be consistent with that activated size, regardless of the modality of judgment. Hence, cross-modal effects of anchors may arise, with a large anchor in any one modality leading to a large judgment in any other (or the same) modality. (p. 15)

Thus, we see again the claim that primes can activate very general concepts which can have multiple and widespread downstream effects on behavior. Further evidence for this viewpoint came from their final study in which drawing longer lines led participants to be more likely to complete the word fragments B_G, _ONG, and _ALL with their magnitude-related-synonyms (BIG, LONG, and TALL) than if they had drawn shorter lines.

Oppenheimer and colleagues' (2008) explanation of their results bears a striking similarity to those offered in other examples of behavior priming. The activation of a concept (e.g., "largeness") is observed to have widespread consequences for judgments about stimuli across scales, domains, and modalities. Moreover, although the effects were not automatic—attention had to be drawn to the anchors initially—they did appear to be unintentional because participants did not have to be asked to draw explicit comparisons between line lengths and the quantities to be estimated for the effects to obtain.

It is not easy to reconcile these examples of broad transfer across modality with the much narrower, within-modality (and scale) effects reviewed earlier. Why in the line-length examples does a transferable general notion of "largeness" or "smallness" become activated, when in other arguably more plausible situations (such as the Brandenburg Gate, or whale-weight examples) it does not?

The results raise important questions about the boundary conditions of such transfer effects. For example, would line-drawing also transfer to estimates about weights and sizes of objects? Frederick and Mochon's (2012) account suggests that it would not, whereas Oppenheimer and colleagues have to predict that it would. Is it possible to obtain bi-directional cross-modal priming—such that estimating a numeric quantity would influence a physical task? Oppenheimer and colleagues tested the latter prediction in a follow-up experiment briefly reported in their general discussion. Participants answered a question about the length of the Mississippi River, anchored with either a short (15 miles) or long (4,800 miles) anchor and were then asked to draw a toothpick. Oppenheimer and colleagues argued that the mean lengths of 2.19 inches (long anchor) and 2.08 inches (short anchor) of the sketched toothpicks provide suggestive evidence in support of a bi-directional transfer effect, and thus evidence of a general magnitude priming mechanism. However, with an N of 82 and a reported t-value of 1.7 in that study, the evidence, in fact, weakly favors the null hypothesis under a Bayesian analysis (cf. Matthews, 2011).[4]

4. The effect Oppenheimer and colleagues obtained in their Experiment 2, where lines of different lengths affected estimates of the average temperature in Hawaii, is also judged by a Bayesian t-test to be inconclusive.

The reason for dwelling on Oppenheimer and colleagues' study is that it represents an important challenge to the notion that anchoring effects are typically narrow. The narrowness argument is crucial to many researchers' resistance toward the theoretical framework underpinning priming, and thus if anchoring effects can indeed cross modalities and scales, then the types of transfer highlighted in the Loersch and Payne quotation seem less controversial. However, as far as we can tell, the Oppenheimer and colleagues' study is an outlier in the anchoring literature. The effects in each of their studies, although reliable, were small (as the authors freely admit, e.g., p. 22) and thus would benefit from replication; likewise, many of the predictions of their general activation account await much needed empirical testing.

ANCHORING AS AN EXAMPLE OF A SITUATED INFERENCE?

Our review[5] suggests that (1) subliminal or incidental effects of anchors are difficult to confirm and/or rather fragile; (2) anchoring effects can be intentionally avoided when (additional) deliberative thinking is encouraged; and (3) anchors tend to result in the activation of specific rather than general features (i.e., narrow transfer appears to be the norm). This pattern of effects seems readily reconcilable with widely held views about the nature of priming in other domains, and it can also be accommodated by at least some popular models of priming.

Consider the situated-inference model of Loersch and Payne (2011), developed in an attempt to explain the diverse impacts primes are claimed to have on a range of downstream behaviors (e.g., the *hostility* prime described in the quote above). The emphasis in their model is on a person's ability to assess the content of their own thoughts and to determine the relevance of these thoughts for the task at hand. For example, they write:

> [T]he situated-inference model predicts that metacognitive judgments about the meaning and validity of thoughts are critical… If one's thoughts are viewed as invalid, nondiagnostic, or otherwise inappropriate for use in the inference process, then priming will have no effect on subsequent judgment, behavior, or motivation. (p. 215)

Such a view seems to fit well with the effects of anchoring reviewed here. First, when participants are sufficiently incentivized or induced to think differently, non-diagnostic information is discounted (Larrick, 2004; Simmons et al. 2010). There is no automatic effect of accessible thoughts on behavior: Instead, thoughts are only instrumental if they are interpreted as valid reasons for behavior. Second, in situations involving broader transfer, information that could be activated is, presumably, either assessed as inappropriate or does not enter into consideration because of the distance between the prime and the target. The attribution process at the heart of the model is likely to be highly sensitive to implausible influences of a prime, in the same way that attributions of fluent processing are known to be constrained (J. K. Miller, Lloyd, & Westerman, 2008).

5. Notwithstanding the admittedly selective nature of our review, we think these conclusions are representative of the wider anchoring literature.

A similar notion is discussed by Klatzky and Creswell (2014) in their application of an intersensory interaction model to the priming literature. In essence, Klatzky and Creswell argue that priming effects might result from competition between multiple mechanisms—memory retrieval, associative chaining, heuristic inference—all of which "bid" to influence an outcome. The model explains when and why different types of priming are observed by assuming that these mechanisms are subject to different sources of variability (e.g., cognitive control, semantic context, cue reliability) that can affect the strength of each bid. For example, the influence of an elderly prime on walking speed might have differential effects on US and European participants because of culturally bound differences in the assessed potency, reliability, and weight assigned to old-age stereotypes (e.g., the extent to which old age connotes energy depletion in the two cultures). Klatzky and Creswell (2014) sum up their approach by arguing that their model suggests that "priming should be promoted or discounted, according to whether factors present in the experimental context facilitate or impede access to mediators and heuristics or suggest that indirect sources of information are more or less reliable" (p. 56). The similarity with the situated-inference model is clear, and the ability of both models to accommodate the anchoring effects we have reviewed is readily apparent.

CONCLUSION

Putting aside the recent controversy about the replicability of some striking priming effects, there can be little doubt that behavior can be subtly and irrationally primed. We have focused on anchoring as a particularly well-documented illustration of this. However, the interpretation of such priming effects—and what they reveal about the mind and behavior—is altogether less clear. A common viewpoint is that priming arises from the automatic and unconscious activation of mental constructs, and that these constructs can have wide influences on behavior. In our view, a good portion of the current skepticism about priming is based on dissatisfaction with this framework. Disbelief about the priming effects themselves arises because, on alternative theoretical viewpoints, they appear implausible.

In the anchoring literature, researchers have marshaled evidence from studies of subliminal, incidental, and cross-modal anchoring, and from experiments on the extent to which individuals are or are not aware of and can or cannot control the influence of an anchor, to argue in support of this framework. An example is Morewedge and Kahneman's (2010) proposal that System 1, which carries out fast and automatic operations, is what drives many anchoring effects. We have argued here that this interpretation of anchoring is not strongly supported by the literature and is, indeed, in some respects contradicted by the evidence. For example, individuals can intentionally control the influence of anchors (Simmons et al., 2010) and have considerable insight into the extent to which anchors affect their estimates (e.g., Wilson et al., 1996). Anchors are typically very narrow in their influence across judgment dimensions, and from a Bayesian point of view, the evidence for incidental and cross-modal anchoring is at best inconclusive. We have evaluated the evidence in relation to three of the four standard criteria for automaticity (Bargh, 1994; Moors & De Houwer, 2006), namely absence of awareness of the prime, absence of awareness of the prime's effect on one's behavior, and uncontrollability

of the prime's influence (the fourth criterion, persistence of the prime's influence even when cognitive resources are diminished, has received very little attention in anchoring studies). On each criterion, the evidence does not support the idea that anchoring can occur automatically. It is not necessary, in sum, to accept a dual-systems perspective in order to make sense of the varied phenomena associated with anchoring. Instead, the general principles underlying deliberative (System 2) thought are sufficient (Newell & Shanks, 2014; Shanks, 2007). On this alternative account, there are few (if any) truly automatic or unconscious processes, nor is activation a passive and obligatory phenomenon.

We acknowledge of course that anchoring is only one type of priming and that caution is advised in extrapolating our conclusions to other, perhaps very different, forms of priming. It is highly unlikely that there will turn out to be a single grand theory applicable to all forms of priming; indeed, the term *anchoring* itself refers to a range of phenomena that quite likely depend on distinct mental processes. But despite this, anchoring encompasses two key features that are central to other forms of priming: 1) it occurs without deliberate intent, and 2) it involves rich conceptual contents (e.g., magnitudes, semantic features) rather than narrow mental constructs.

Extraordinary claims require extraordinary evidence. We are not the first to point out that the incentive structures under which psychologists operate appear to discourage attempts at replicating published results (Asendorpf et al., 2013) and that insufficient effort has been devoted to replicating key results in experimental psychology (Makel, Plucker, & Hegarty, 2012; Simons, 2014). This is strikingly evident in the social cognition and anchoring fields. For example, it is very surprising that there have been no published attempts to replicate Critcher and Gilovich's (2008) demonstrations of incidental anchoring or Oppenheimer and colleagues' (2008) cross-modal anchoring effects. These (and other) findings are so important for our theoretical understanding of anchoring that they cry out for further exploration. After all, for many years it was almost universally accepted that accuracy motivation (induced by financial incentives) usually fails to diminish anchoring, and it was only because Simmons and colleagues (2010) undertook further replications of this phenomenon that they discovered that the influence of anchors can in fact be attenuated and that the earlier conclusion was premature. We urge researchers to place more emphasis on replication.

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