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The Fragile Basic Anchoring Effect

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ABSTRACT

The anchoring bias, the effect of uninformative anchor numbers on judgments, is a robust finding. In experiments yielding the anchoring bias, typically participants are explicitly asked to compare the anchor and target. A logical question is whether any manner of considering a number will bias peoples' judgment. Wilson *et al*. (1996) showed that merely presenting a number to people will bias their judgments, a result they termed basic anchoring. The absence of any published studies that followed up on Wilson *et al*.'s work prompted us to examine the basic anchoring effect in three experiments. The three experiments ($N = 881$, 205, and 117) suggest that basic anchoring is a weak effect limited to the precise manipulations used by Wilson *et al*. Trivial changes such as altering the order of a series of anchor numbers, or using different anchor numbers, eliminated the bias. Our findings suggest that basic anchoring has a much narrower scope of impact than traditional anchoring effects. Copyright © 2002 John Wiley & Sons, Ltd.

KEY WORDS basic anchoring effect; anchoring bias

Studies of the anchoring bias usually ask people to compare an anchor number to some unknown quantity, such as the number of physicians in the phone book. Theories that explain the anchoring bias, such as anchoring and adjustment (Tversky and Kahneman, 1974) and anchoring as activation (Chapman and Johnson, 1994, 1999; Mussweiler and Strack, 1999; Strack and Mussweiler, 1997), presuppose just such a comparison. A series of experiments by Wilson *et al*. (1996) suggest that merely presenting a number to people will bias their judgments even if no comparisons are made. Their finding appears to be anchoring but cannot be accounted for by current models of the anchoring effect. This paper presents three experiments that replicate and extend previous research on what happens when anchor numbers are merely presented.

Experiments that yield the anchoring bias typically use a staged series of questions modeled after the classic United Nations experiment by Tversky and Kahneman (1974). In that study, participants spun a wheel of fortune to obtain a random number. Half received the number 10 and the other half received 65. Participants were asked whether the percentage of African countries in the United Nations was greater or less than their number from the wheel of fortune. They were then asked to estimate the percentage of countries. Participants who received an anchor of 10 estimated the percentage to be 25; those receiving 65 estimated the percentage to be 45. Thus, low anchor numbers biased judgments lower relative to the effect of high anchors.

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Since Tversky and Kahneman introduced this experimental design, the anchoring bias has been found to be quite robust and difficult to debias (e.g. Chapman and Johnson, 1994, 1999; Jacowitz and Kahneman, 1995; Northcraft and Neale, 1987; Plous, 1989; Quattrone *et al.*, 'Explorations in anchoring: The effects of prior range, anchor extremity and suggestive hints', unpublished manuscript, 1984; Strack and Mussweiler, 1997). These studies have generally followed Tversky and Kahneman's framework of having participants explicitly compare the anchor number to some target that is later rated. Requiring an explicit comparison of anchor and target leaves unanswered the more general question of whether a number simply presented to people biases their judgments.

A FIRST DEMONSTRATION OF THE BASIC ANCHORING EFFECT

Wilson *et al*. (1996, Studies 2 and 3) presented a series of innovative studies showing an anchoring bias in the absence of any explicit instruction to compare the anchor and target, a result they described as the *basic anchoring effect*. The designs offered new methodological tools for exploring the anchoring bias. More importantly, the results of their studies suggested that anchoring is a much more general process that is more likely to be elicited in natural contexts than the anchoring bias found by Tversky and Kahneman.

Wilson *et al*.'s (1996, Study 2) 'ID number' study presents evidence for basic anchoring. In the study, participants were assigned a 'random' ID number that served as the experimental anchor and asked to perform three tasks. First, they copied the anchor number onto the first page of their survey and then turned to the next page. Second, participants performed one of six manipulations on the anchor number. In the first condition, participants performed the traditional comparison task by explicitly comparing the anchor number to a target question. In the second through fourth conditions, participants compared their anchor number to a second number (100, 1920, or 1940). In the last two conditions, participants either looked back to see what color ink the anchor number was printed in, or they checked whether the anchor number had four digits. Note that neither of these last two conditions required a comparative judgment. Finally, a seventh no-anchor control condition presented no anchor number at all. In the third and final part of the experiment, participants estimated the number of physicians listed in the local phone book.

The first condition using the traditional comparison task showed higher estimates than the control group, a demonstration of the traditional anchoring bias. The next three conditions that required a comparison between the anchor and some other number (but not the target) also showed anchoring. Wilson *et al*. interpreted this result as basic anchoring, arguing that an explicit comparison to the target was not requested. We interpret this result differently, seeing it as an indeterminate type of anchoring somewhere between basic and traditional anchoring. Participants explicitly made a comparative judgment that required them to process the anchor number as a quantity and then meaningfully relate it to another number. Although participants did not compare the anchor number and the target, their task was substantially different from those in the remaining two conditions.

The results that are crucial for supporting basic anchoring lie in the remaining two no-comparison conditions. Only the color-check condition showed ratings that were above the control group, a result Wilson *et al*. described as basic anchoring. Digit-counting showed no effect of anchor. Thus, basic anchoring was obtained in one of the two conditions.

A SECOND DEMONSTRATION OF THE BASIC ANCHORING EFFECT

Wilson *et al*.'s (1996, Study 3) 'graphology' study is a stronger demonstration of anchoring in the absence of a comparative judgment. In this study, Wilson *et al*. instructed participants that the survey they were completing was part of a graphology study. The experimenters varied whether participants did simple mathematical computations on high anchor numbers or simply copied pages of the same anchor numbers. A second factor varied whether participants wrote one or five pages of numbers. The design was fully crossed, creating four experimental conditions. In a fifth no-anchor control condition, participants copied several pages of neutral words but did not receive a numerical anchor. In an ostensibly separate study, all participants then made an absolute judgment of how many students at the University of Virginia would contract cancer within the next 40 years.

The anchors in the study were the numbers copied. Although it is unclear whether the anchor number is the last number copied, or the collective impression created by the series of numbers copied, the numbers were high anchors as they were higher than most ratings of the dependent variable given by a control group. Five pages of copying numbers or calculations on numbers led to higher judgments than one page of the same procedures. The no-anchor control condition produced judgments very similar to the one-page conditions. Wilson *et al*. concluded that sufficient attention to the anchor could produce the anchoring bias, even in the absence of a preliminary judgment comparing anchor and target. In summary, Wilson *et al*. provided two apparently strong demonstrations of basic anchoring.

THEORIES OF ANCHORING

Theoretical models that explain the traditional anchoring bias cannot explain the basic anchoring bias. One popular model, the selective accessibility model (Strack and Mussweiler, 1997), posits that comparing a target to an anchor number leads to a biased search strategy consistent with positive hypothesis testing. For example, consider a person deciding whether the number of African countries in the United Nations is more or less than 10%. The selective accessibility model posits that the person would retrieve information consistent with the hypothesis that there are few African countries. When the person then estimates the portion of African countries in the United Nations, they have a biased set of information in active memory on which to base their judgment. The biased (selectively accessible) information then biases the person's judgment.

In basic anchoring, the target is never compared to an anchor number. Thus, there is no biased search or positive hypothesis testing where target-relevant information relating to low or high magnitude is considered. For this reason, the mechanism of selective accessibility cannot explain the basic anchoring effect.

The finding of anchoring triggered without a comparison task could be a very general phenomenon. Despite the promise of these findings, Wilson *et al*.'s graphology and ID number studies remain the only published example of basic anchoring. The paucity of other research on basic anchoring, along with difficulties that we have had eliciting basic anchoring effects in our own lab, caused us to investigate the robustness of basic anchoring.

Several things about the anchors used in Wilson *et al*.'s experiments merit consideration. First, the experiments used only high-magnitude anchors. We infer that the anchors acted as high anchors because they were higher than a no-anchor control group's average rating of cancer risk for their fellow students. Other anchoring studies commonly use both high and low anchors and then interpret any differences in their effects as a demonstration of the anchoring bias. It remains an empirical questions whether differences between low and high anchor conditions would be found in the absence of a comparative judgment.

Second, the anchor numbers themselves are unusual. In the graphology experiment, anchor numbers were presented in a nested series of repeating patterns. Although the anchors were not presented in the original paper, they are presented in Exhibit 3 to aid the discussion. An inspection of the table shows that the numbers were presented in two patterns, one that repeats for the seven numbers on every page, and another pattern that cycles through the numbers presented at the top of each page such that the numbers appear to converge on the number 4500.

The anchor numbers used in the ID number experiment are also notable in that they had values between 1928 and 1935 that could be interpreted as dates. Thus, in both basic anchoring studies, the anchor numbers had characteristics that may have made them exceptionally salient. We investigated whether these features were necessary to obtain basic anchoring.

The purpose of the present experiments was to replicate and extend Wilson *et al*.'s demonstration of basic anchoring. First, we sought to replicate the ID number experiment and to extend it by using numbers other than those that could be interpreted as years. Second, we sought to replicate the graphology experiment and extend the design by using both low and high anchor conditions. Third, we explored the effect of varying the order of the anchor numbers in the graphology experiment.

EXPERIMENT 1: ID NUMBERS

The anchor numbers that Wilson *et al*. used in their ID number experiment were startlingly like dates, a quality that may have altered the way that participants processed the numbers. The first experiment tested whether the specific numbers that Wilson *et al*. used were, in fact, crucial to obtaining basic anchoring. We added conditions with numbers lower and higher than Wilson *et al*.'s with the expectation that neither would create anchoring. Past research on the anchoring bias offers the opposite prediction, that ratings of the dependent variable are an increasing function of the anchor magnitude, in other words, that higher anchors yield a stronger bias (Chapman and Johnson, 1994; Chapman and Bornstein, 1996). We also added a fourth condition using zip codes, numbers that may be as uniquely identifiable as dates, that we expected would create anchoring. The fourth condition allowed us to test our alternate account that the similarity of the anchor numbers to dates made them additionally salient.

Method

The experiment used a design largely identical to the one used in Wilson *et al*.'s ID number experiment. Participants were told that the survey was part of a study of general knowledge. They first copied an anchor number from a self-adhesive note onto the first page of their survey and then turned to the next page. Second, participants looked back to see whether their ID number was number was red or blue, ostensibly to tell them which part of the survey to complete next. In the final part of the experiment, participants estimated the number of physicians listed in the local phone book.

There were four anchor number conditions, all high anchor numbers (see Exhibit 1). One high anchor number group, 1900s, were the same as were used by Wilson *et al*. Two other groups (600s and 5600s) also served as credible high anchors. A fourth group of numbers (08900s) were the same as the zip codes for our university and adjacent town. The zip codes were presumably numbers that the students had some regular

Exhibit 1. Anchor numbers used in Experiment 1

75%ile	95%ile	100%ile	100%ile
628	*1928*	5628	08901
629	*1929*	5629	08902
630	*1930*	5630	08903
631	*1931*	5631	08904
632	*1932*	5632	08905
633	*1933*	5633	08906
634	*1934*	5634	08907
635	*1935*	5635	08908

Note: Column headings show percentile ranking of anchors relative to a control group's rating of the dependent variable (the number of physicians in the phone book). Italicized numbers were used in Wilson *et al*.'s original ID number experiment. Each participant saw only one anchor number.

contact with and would recognize. In a fifth no-anchor condition, participants did not receive an ID number. Low anchor conditions were not included as the number of experimental participants required to have sufficient power would have been prohibitive.

One convention for selecting particular anchor numbers is to use the 85th percentile rating of the dependent variable by a no-anchor control group as a high anchor and the 15th percentile as a low anchor (Jacowitz and Kahneman, 1995). This was the rationale used by Wilson *et al*. to select the mean values for their high anchor numbers (1900s). The magnitude of our high anchors relative to a control group is shown at the top of Exhibit 1. The anchor numbers we used were at the 75th, 95th , and 100th percentiles of a control group's ratings. The decision to use more extreme anchors has substantial empirical support as more extreme anchors have been shown to produce equal or larger effects (Chapman and Johnson, 1994; Chapman and Bornstein, 1996).

At the end of the survey, participants who had copied an ID number also answered questions about their answers. Two questions assessed knowledge: how confident they were that their answer was correct, and how much they felt that they knew about the number of physicians in the phone book. Two questions assessed demand effects: whether they believed that copying their ID numbers altered their answers, and that copying the ID number would alter other people's answers. These questions were not asked of the no-anchor control group.

Surveys were completed by 831 participants. Data were dropped for 97 participants who did not adequately answer the question abut the number of physicians in the phone book presumably because they found the question to be odd. Most of these participants either left the question blank or declined to answer the question (writing comments such as 'Who cares?'). In Wilson *et al*.'s original study, there were approximately 57 people in each cell of their design. The present experiment had about three times as many participants per cell.

Results and discussion

A basic anchoring effect was found only when Wilson *et al*.'s anchor numbers were used. Four planned contrasts compared the control group to each of the four anchor groups. The data were first log transformed because of substantial skewing for this and all other experiments presented here. The planned contrasts showed that only the group presented with Wilson *et al*.'s year-like anchor numbers (i.e. 1900s) was significantly higher than the no-anchor control group ($F(1, 729) = 5.80$, $p < 0.02$). None of the other conditions, 600s, 5600s, and 08900s, differed significantly from the control group (Fs$(1, 729) = 2.04$, 2.96, 1.55, *ns*).

Our first prediction, that more extreme anchors would not yield more extreme anchoring, was supported. Our second prediction, that zip code (i.e. 08900s) anchors would be higher than the control group, based on

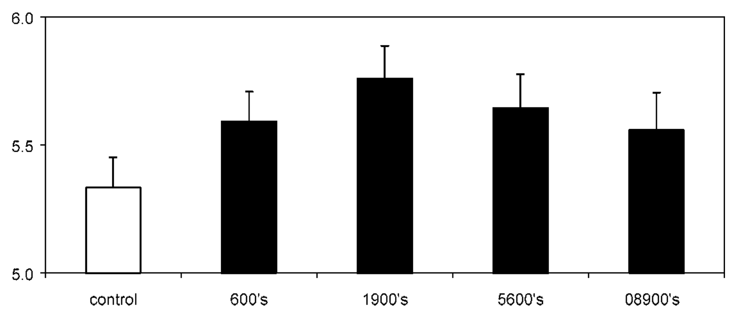

Exhibit 2. Mean ratings of the target question in Experiment 1. (*Note*: DV = Log transform of rating of number of physicians in the phone book. Error bars show standard errors. $N = 734$).

our observation that Wilson *et al.*'s numbers were similar to years, was not supported. One explanation for the failure of zip codes to yield anchoring may be that year-like numbers are unique in their salience. Regardless, the basic anchoring effect did not generalize to other numbers and the present study did not show an increasing bias with increasing anchor extremity. The results of the present experiment strongly suggest that basic anchoring is particular to the manipulations used by Wilson *et al.*

One explanation for the findings is that participants believed instead that the ID number anchor would affect their estimates and reacted in accordance with what they believed the wishes of the experimenter to be. Previous research suggests the opposite, that the perceived informativeness of anchoring numbers cannot account for the anchoring bias (Chapman and Johnson, 1999). The latter appears to be the case in the present experiment. Among those who received an ID number, 84% felt that the ID number did not affect them. Interestingly, only 48% felt that others would be unaffected by the anchors. We repeated the main analyses for those who believed they were unaffected by the anchors. As noted previously, the control group did not answer the influence questions as they did not see any ID number. The results were the same as reported above: only the 1900's group differed from the control group ($F(1, 634) = 3.78$, $p = 0.052$).

Wilson *et al.* reported analyses for the 91% of participants who rated themselves low in knowledge about the dependent variable, as a study in their paper had shown anchoring only among people who rated themselves lower in knowledge. We were unable to exclude participants in our control condition as they were not asked about their knowledge of the outcome measure. However, limiting analyses to low-knowledge participants (87%) did not change the results reported above.

EXPERIMENT 2: GRAPHOLOGY

Although the ID number experiment appears to be limited to year-like numbers, Wilson *et al.*'s graphology study remains a potentially generalizable demonstration of basic anchoring. Experiment 2 attempts to replicate the graphology experiment, extending its finding to both high and low anchors. However, as with the graphology study, there appear to be methodological anomalies that may be responsible for the anchoring bias, an issue that is explored in a third experiment.

Method

The second experiment employed a 2(Pages) × 2(Operation) × 2(Anchor Level) and control group design largely identical to the one used by Wilson *et al.* (1996, Study 3) with an added factor manipulating the magnitude of the anchors presented. In exchange for course credit, 205 college students participated in the experiment. Data for six participants with incomplete surveys were dropped from analyses. Wilson *et al.*'s original study used a comparable sample size, with approximately 20 students per cell; the present experiment had 22 students per cell.

Participants each completed two surveys for ostensibly unrelated studies. The first survey, described as a study of graphology, asked participants to write down five pages of numbers or words. In the five-page condition, participants wrote only numbers. There were seven numbers per page, making a total of 35 numbers copied. In the one page condition, participants wrote four pages of neutral words, such as 'sofa' and 'file', followed by one page of numbers, thus making seven numbers copied.

A second condition manipulated the operation that the participants conducted on the numbers. In the 'copy' condition, participants simply copied seven numbers on each page. In the 'compute' condition participants did small computations on a number presented at the top of the page. Participants copied the number, then wrote the number coming after the original number, then wrote the one after that, and then the one after that. Next they wrote the number coming before the original number, and wrote the one before that, and the one before that. The same pattern of the numbers was written down as a result of copying or calculating (see Exhibit 3).

Exhibit 3. Anchor numbers used by Wilson *et al.* (1996) and in Experiment 2.

	Anchor magnitude		
Page	Low	Wilson	High
1	**575**	**4575**	**45750**
	574	4574*	45749
	573	4573*	45748
	572	4572*	45747
	576	4576	45751
	577	4577	45752
	578	4578	45753
2	**425**	**4425**	**44250**
	424	4424	44249
	423	4423	44248
	422	4422	44247
	426	4426	44251
	427	4427	44252
	428	4428	44253
3	**450**	**4450**	**44500**
	449	4449	44499
	448	4448	44498
	447	4447	44497
	451	4451	44501
	452	4452	44502
	453	4453	44503
4	**550**	**4550**	**45550****
	549	4549	45549
	548	4548	45548
	547	4547	45547
	551	4551	45551
	552	4552	45552
	553	4553	45553
5	**500**	**4500**	**45000**
	499	4499	44999
	498	4498	44998
	497	4497	44997
	501	4501	45001
	502	4502	45002
	503	4503	45003

*In the Wilson *et al.* study, anchors that were 100 less than these numbers were presented to participants in the five-page copy condition, presumably due to an error.

**Due to experimenter error, the numerical series for page 4 begins 50 points higher than that suggested by adding a '0' to the end of Wilson *et al.*'s anchor number.

A third condition manipulated the magnitude of the anchor numbers presented to participants. Wilson *et al.*'s anchor numbers were presented in a carefully scripted order that we maintained in the present study. The purpose of paying such close attention to Wilson *et al.*'s order of anchor numbers was that we suspected, based on previous failures to replicate the basic anchoring effect, that the particular order was needed to obtain the effect. A low anchor condition presented anchor numbers that were approximately one tenth the magnitude of Wilson *et al.*'s series of anchor numbers, created by dropping the first digit of each number they used. As shown in Exhibit 2, the low anchors placed at the tops of pages one through five were the numbers 575, 425, 450, 550, and 500, respectively. The high anchor condition followed a similar logic. High anchors were created that were approximately ten times larger than Wilson *et al.*'s by adding a 0 to the end of each anchor. The numbers at the top of pages one to five were, respectively, 45750, 44250, 44500, 45550, and 45000.

Because the three factors were fully crossed, the experiment had eight experimental conditions. There was also a ninth condition that served as a no-anchor control where participants did not receive any numerical anchors and copied five pages of words.

After completing the initial survey, participants completed another survey that asked them to rate the likelihood of several health hazards. First they estimated the number of current Rutgers University students they expected would contract cancer in the next 40 years. Four other questions asked about treatment for alcohol-related problems, eating disorders, cocaine use at least once a semester, and testing 'positive for AIDS or for the HIV virus that causes AIDS'. The first question about cancer risk served as the dependent variable. All participants in this and the next experiment received the knowledge and influence questions described in Experiment 1.

As noted previously, one convention for selecting particular anchor numbers is to use the 15th and 85th percentile rating of the dependent variable (Jacowitz and Kahneman, 1995). This was the rationale used by Wilson *et al.* to select the mean value for their high anchor number (4500). We again opted to use the convention, but at the same time wanted to be able to match closely Wilson *et al.*'s anchors. We used the 92nd percentile rating (45,000) of a no-anchor control group for the high anchor and the 12th percentile (500) for the low anchor.

Results and discussion

The results of the second experiment are presented in four sections. First, analyses are performed on the data from the high anchor condition, the condition that is presumably equivalent to the Wilson *et al.* study. Second, the same analyses are repeated with the low anchor data. Third, the results of an omnibus ANOVA on all the conditions is reported. Last, the effects of knowledge and perceived anchor influence are examined. Means for each of the nine experimental conditions are presented in Exhibit 4.

The first experiment failed to replicate the findings of Wilson *et al.* for participants in the high anchor condition. A 2(Pages) $\times$ 2(Operation) ANOVA revealed no significant findings. Neither of the main effects nor the interaction were significant ($Fs(1, 98) < 1$, *ns*). The absence of significant results is surprising given Wilson *et al.*'s finding of a difference between ratings in the one and five page conditions.

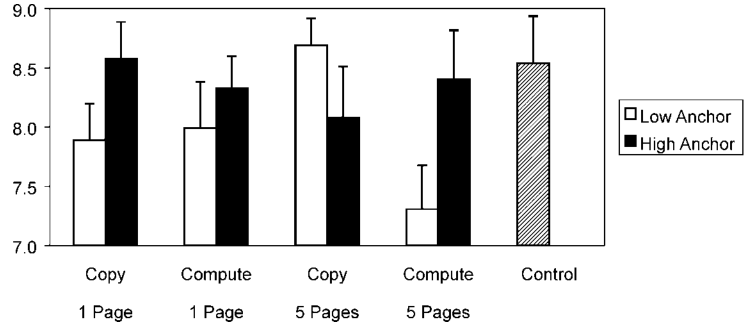

Exhibit 4. Mean ratings of the target question in Experiment 2. (*Note*: DV = Log transform of rating of number of students expected to contract cancer in next 40 years. Error bars show standard errors. $N = 199$).

Among those who received low anchors, only one of four conditions showed the anchoring bias. Participants in the five page-computation condition gave lower estimates than those in the other three conditions. When we repeated the ANOVA for participants in the low anchor condition, we found that the interaction of Pages and Operation was significant ($F(1, 93) = 5.01$, $p < 0.03$). Neither of the main effects was significant. A *post-hoc* contrast revealed that the five page, computation cell was significantly lower than the mean of the other three ($F(1, 93) = 5.30$, $p < 0.03$).

It is common in anchoring studies to use both high and low anchors, and then to consider any difference between high and low anchor conditions an anchoring effect. The final analyses adopt this frame. The effects reported in the following analyses are largely driven by the anchoring already shown for the low anchor, five-page-computation cell.

An omnibus three-way ANOVA examined the effects of number of pages of anchor numbers, the operation participants engaged in, as well as the effect of differing anchor magnitude. Only the three-way interaction of Pages $\times$ Operation $\times$ Anchor Level was significant ($F(1, 191) = 4.38$, $p < 0.04$). We explored the interaction with four *post-hoc* contrasts that examined whether the low and high anchor conditions differed for each of the four cells in the Pages $\times$ Operation interaction. Only one cell, the five pages of computation, showed a significant anchoring effect ($F(1, 191) = 4.92$, $p < 0.03$).

We also compared the no-anchor control condition to the two cells that showed anchoring, that is, the high and low anchor conditions of the five-page-computation cell. A *t*-test showed that the low anchor condition was significantly lower than the no anchor control ($t(43) = 2.27$, $p = 0.03$). However, the high anchor and control conditions did not differ from one another ($t(44) = 0.23$, *ns*).

As discussed in Experiment 1, an alternative explanation for the findings is that participants acted in accordance with what they believed the wishes of the experimenter to be. A large majority (80%) felt that the ID number did not influence their own estimates and 42% felt that the anchor would not influence others' estimates. We dichotomized the influence-to-self variable to reflect two groups, one that stated their estimates were unaffected and another that stated they were. The influence-to-self variable was entered as an additional factor in the ANOVAs described above (i.e. as a main effect and a moderator of other effects). The new analyses showed no effects of the new variable. As in the previous experiment, there appears to be no effect of perceived influence on participants' estimates.

Wilson *et al.* reported analyses for the 84% of participants who rated themselves low in knowledge about the dependent variable, the number of students who will get cancer. In the present experiment, we separately analyzed the data for participants (83%) who scored below the midpoint on the average of the knowledge and confidence scales. Limiting analyses to these participants did not change the results reported above. Five pages of computations on anchor numbers were still required to obtain anchoring in the low anchor analysis and in the omnibus analysis. Using a different strategy, where the knowledge variable was dichotomized and used as a moderator variable, also yielded no effects of knowledge.

In summary, we found a complex pattern of relationships among the experimental factors once low anchors were introduced into the design. Basic anchoring appears to require both a large number of anchors and a computational exercise that presumably leads to deeper cognitive processing.

EXPERIMENT 3: GRAPHOLOGY, AGAIN

Experiment 2 demonstrated basic anchoring, although under slightly more restricted conditions than reported by Wilson *et al.* A question remains as to whether basic anchoring requires the ordering of the anchors used by Wilson *et al.* The anchor numbers were presented in a scripted order the effects of which may not generalize to other orders. The final experiment explores this question by presenting the same anchors used in the previous experiment, but in a random order.

Method

Similar procedures and surveys were used in the third experiment as were used in the second experiment, except that the anchor numbers were presented in a random order. The present experiment also manipulated one fewer variable than the previous one, creating a 2(Computation) $\times$ 2(Anchor Level) design. The five-page procedure was used for all participants. The one-page manipulation was dropped because both Wilson *et al.* and our second experiment found anchoring only in the five-page condition.

In exchange for course credit, 117 students participated in the experiment. Data for two participants with incomplete surveys were dropped from the analysis. Thus, each cell of the experiment had approximately 29 students, about half again as many as were in Wilson *et al.*'s original study.

Two random orderings of the anchor numbers were created to counterbalance any effects of order. The new orders of anchor numbers necessitated presenting all numbers upon which calculations were to be performed (as opposed to the previous experiment where participants performed a series of math problems starting with a single number at the top of the page). For example, participants in this experiment were asked, 'Write down the number that comes after 45,552' followed by 'Write down the number that comes before 44,250', and so on.

There was an added benefit to this change in that it made the copying and the computation conditions more equivalent than in the last experiment. In the computation condition, participants usually operated on a number that they had previously written and presumably had in short term memory. In the copy condition, the number written was always a number that was preprinted on the survey. In the present experiment, the copy and computation conditions are highly similar in that the number being operated on had little relation to the previous one written down.

Results and discussion

No basic anchoring effect was detected. The data were analyzed using a 2(Computation) $\times$ 2(Anchor Level) $\times$ 2(Anchor Order Counterbalance) ANOVA. The means for each of the four cells of the Computation by Anchor Level interaction are shown in Exhibit 5. There were no significant results for the main effects of computation, anchor level, or their interaction (Fs$(1, 114) \leq 1$). Order of the anchors also had no effect. Removing participants high in knowledge of the dependent variable had no effect nor did removing participants who believed that the anchors had affected their answers.

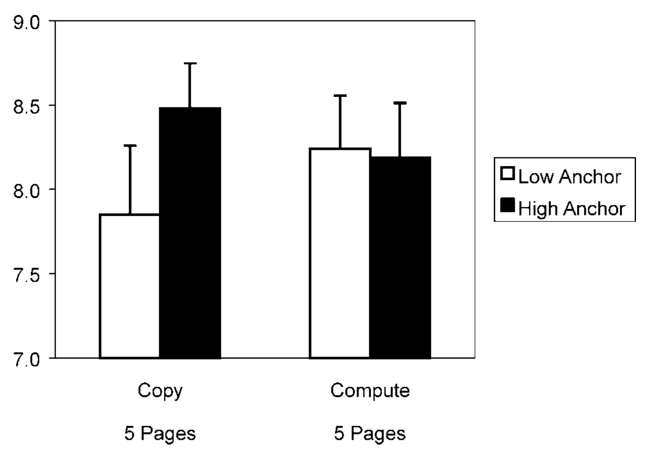

Exhibit 5. Mean ratings of the target question in Experiment 3. (*Note*: DV = Log transform of rating of number of students expected to contract cancer in next 40 years. Error bars show standard errors. $N = 115$).

GENERAL DISCUSSION

The only demonstrations of the basic anchoring effect that we are aware of are the studies in Wilson *et al.*, and the first and second experiments presented here. The present experiments show that obtaining the basic anchoring effect requires highly specific manipulations that all point to a requirement of carefully orchestrated cognitive processing of anchors, and limited generalizability. The ease with which the anchoring bias is disrupted suggests that basic anchoring is fragile in ways that the traditional anchoring bias is not.

Generalizability

One of the most engaging properties of basic anchoring is its apparent application to numerous natural situations where numbers are present such as tracking the value of a stock or bidding at an auction. If the mere presentation of numbers could elicit anchoring, then it would be an insidious and widely prevalent bias. However, the fragile nature of basic anchoring that we have found indicates that the phenomenon is likely to have minimal effect on judgments in most contexts.

Basic anchoring can only be reliably obtained with a large number of anchor numbers or repeated exposure to the same number (but only if that number is a year). The graphology experiments reported here and by Wilson *et al.* show that 35 anchors are required to obtain basic anchoring and that 7 are too few. Experiment 2 suggests that, contrary to the findings of Wilson *et al.*, mere copying of anchor numbers is insufficient to elicit basic anchoring. Additional processing in the form of computation (or possibly carrying the numbers in short-term memory) is required. The ID number experiments reported here and by Wilson *et al.* show that basic anchoring can be reliably obtained with anchors resembling years (i.e. 1928–1935). However, Experiment 1 also suggests that other anchor numbers will fail to yield basic anchoring.

The basic anchoring effect is easily disrupted by apparently trivial changes. Basic anchoring was eliminated in the graphology experiment by scrambling the order in which the anchors are presented, and in the ID number experiment by using alternate anchor numbers. These restrictions are troubling. Basic anchoring, anchoring in the absence of a comparison between anchor and target, becomes a very limited phenomenon if it can only be elicited by one deliberate order of anchor numbers or an arcane series of tasks that consist of copying and later noting an anchor's color (but not the number of digits).

The limitation of needing particular numbers, or a particular order of anchor numbers, may point to a fundamental process at work that is important to understanding when and how basic anchoring happens. The experience of performing the calculations required by the five-page condition of Wilson's graphology experiment instills a sense of 'a-ha' at the moment when the number 4500 (or 500 or 45,000) is presented. The manipulation seems to create an anticipation that there is a purpose to the number ordering, a purpose satisfied by the revelation of the number 4500. Similarly, the experience of noting that one's ID number resembles a year may cause additional processing of the number. There are other more common situations when such an 'a-ha' occurs, such as solving a difficult math problem that has a simple, even intuitively obvious, result. Such insight experiences may make the anchor number salient enough that it influences later judgments.

Traditional versus basic anchoring

Basic anchoring appears to differ from traditional anchoring studies in the ease with which it is debiased. The traditional anchoring procedure of preceding the estimation task with a comparison to the anchor produces a robust anchoring effect, an effect that is resistant to various debiasing strategies (Chapman and Johnson, unpublished data; Quattrone *et al.*, unpublished manuscript, 1981; Tverksy and Kahneman, 1974). In the same paper that presented basic anchoring, Wilson *et al.* (1996, Studies 4 and 5) presented experiments using a traditional anchoring design that showed anchoring to be impervious to debiasing attempts including offering a

$50 payment to the most accurate participant, warning participants that the anchor would bias their answers, and even warning them of the direction in which the anchor would bias their answer. All attempts at debiasing failed. In contrast, basic anchoring appears to be very easily disrupted by small procedural changes.

The two forms of debiasing are notably different. Attempts at debiasing traditional anchoring have emphasized cueing participants to react against or compensate for anchoring. The present experiments simply eliminate the effect through procedural changes. It has yet to be examined what effect extant debiasing procedures would have on basic anchoring.

Implications for theory

As discussed in the Introduction, theoretical models that explain the traditional anchoring bias cannot explain the basic anchoring bias. In basic anchoring, the target and an anchor number are never compared. This eliminates the potential explanation that biased search or positive hypothesis testing, concepts central to the anchoring as activation models, are responsible for the bias. The experiments that yield the basic anchoring effect suggest a process where mere activation of numerical information biases judgment. Wilson *et al.* (1996) describe this process as a kind of numerical anchoring, an interesting contrast to Strack and Mussweiler's (1997) semantically driven selective accessibility model. Two other papers have also suggested a numerical priming process (Mussweiler and Strack, in press; Wong and Kwong, 2000) based on several experiments that employ comparative judgments. Mussweiler and Strack draw the conclusion that, although numerical priming probably exists, semantic processes override (but do not add to) the biasing effects of numeric priming.

It is puzzling that the basic anchoring effect does not generalize to the new stimuli in the present experiments and is not robust. Nonetheless, basic anchoring is a replicable (if not widely generalizable) demonstration of an anchoring bias that cannot be explained by existing models and presents an challenge for future theoretical work.

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