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Two sets of studies illustrate the comparative nature of disclosure behavior. The first set investigates how divulgence is affected by signals about others' readiness to divulge and shows a "herding" effect: Survey respondents are more willing to divulge sensitive information when told that previous respondents have made sensitive disclosures (Study 1a). The authors provide evidence of the process underlying this effect and rule out alternative explanations by showing that information on others' propensity to disclose affects respondents' discomfort associated with divulgence (Study 1b) but not their interpretation of the questions (Study 1c). The second set of studies investigates how divulgence is affected by the order in which inquiries of varying intrusiveness are made and suggests that divulgence is anchored by the initial questions in a survey. People are particularly likely to divulge when questions are presented in decreasing order of intrusiveness and less likely when questions are presented in increasing order (Study 2a). The authors show that the effect arises by affecting people's judgments of the intrusiveness of the inquiries (Study 2b). The effect is altered when, at the outset of the study, privacy concerns are primed (Study 2c) and when respondents are made to consider the relative intrusiveness of a different set of questions (Study 2d). This research helps illuminate how consumers' propensity to disclose is affected by continual streams of requests for personal information and by the equally unavoidable barrage of personal information about others.

Keywords: survey design, self-disclosure, electronic commerce, privacy

The Impact of Relative Standards on the Propensity to Disclose

The central thesis of this article is that disclosure behavior is comparative in nature: People's willingness to divulge sensitive information depends on judgments that are inherently comparative, such as signals about others' readiness to divulge or the order in which inquiries of varying intrusiveness are made. In seven studies, we investigate disclosure behavior by asking respondents personal, and often incriminating, questions under different circumstances and monitoring their propensity to respond affirmatively—that is, to admit that they have engaged in sensitive behaviors.

Narrowly, this research attempts to shed light on the question of when survey respondents will reveal personal information in response to intrusive, or sensitive, questionnaires. This issue has received considerable attention in both the marketing literature (e.g., Reinmuth and Geurts 1975) and the survey methodology literature (e.g., Tourangeau and Ting 2007), but it is also directly relevant to contemporary phenomena on the Internet: Advances in information technology have been a boon to marketers, who can use personal information to tailor messages to individual consumers (Blattberg and Deighton 1991). However, information technologies that consumers deem too intrusive can elicit reactance (White 2004), countering the marketing benefits of those tools. To predict and make sense of consumers' reaction to modern marketing strategies, we need to understand how they respond to the continual stream of

requests for personal information that is an unavoidable feature of the Internet, as well as to the equally unavoidable barrage of personal information disclosed about others.

Beyond this narrow focus, we believe (and have designed our experiments accordingly) that the research reported here addresses larger issues related to consumer privacy. Although numerous privacy-related decisions do not involve responses to intrusive questions, many of the causal factors we examine are common to disparate privacy-related issues. For example, people probably share information on social media sites depending, in part, on what other people do (the focus of our Studies 1a–1c) and, in part, on what they have themselves previously revealed (Studies 2a–2d). Because examining responses to intrusive survey items is an effective and efficient methodology for testing the impact of situational factors on disclosure of information, much prior and contemporary research on privacy, including our own, has adopted such a methodology (e.g., John, Acquisti, and Loewenstein 2011).

A first set of studies (Studies 1a–1c) examines the impact of receiving information about others' disclosures on a person's own propensity to disclose. Online surveys often provide information about other respondents' answers, including their propensity to answer; we study how such feedback influences subsequent responses. Thus, these studies provide clues about how self-disclosure might be affected by the growing availability of friends' and strangers' personal information on the Internet. Thus far, this effect had proved elusive in the literature. Ong and Weiss (2000) surprisingly find that "normalization"—suggesting that a behavior is reputedly commonplace or rare—had *no* impact on the propensity to admit to behaviors carried out in private. The authors attribute the null result to "weak implementation." In a prior, related study, Moon (2000) finds that people reciprocated the revelation of intimacies, even from a computer. In Moon's experiment, participants were more likely to disclose when interviewed by a computer that prefaced questions with information about itself. This effect is related to our study. Whereas Moon's findings could result either from reciprocity (the tendency to respond to another's revelations with one's own) or from herding (the tendency to conform to the norm set by others' behavior), our results are only interpretable as a herding effect because we focus on the impact of *strangers'* revelations (with whom no interaction or reciprocity is likely) on our propensity to reveal sensitive information.

Moon (2000) also finds that participants were more likely to disclose to a computer that had "warmed" them up with introductory questions compared with participants who had answered the same questions but on another computer that did not do this. Because in Moon's study all participants actually answered the same questions in the same order across the conditions, the result evokes, but does not resolve, the question we try to address in the current research: How is people's propensity to disclose affected by the order in which inquiries of varying sensitivity are made? Do consumers reveal more when they are first asked unintrusive questions and are warmed up as the questions become more intrusive? Or does asking questions in a progressively intrusive order cue consumers to perceive subsequent questions as sensitive, causing them to "clam up"

relative to consumers who immediately faced the more sensitive questions?

Although the effect of question order has been studied extensively (e.g., Barnes, Banahan, and Fish 1995; McFarland 1981), the impact of questions of different degrees of intrusiveness remains underexplored. Hui (2007) finds no statistically significant impact of the order of personal questions on people's propensity to answer them in an online shopping task. Moon (2000) studies how people's propensity to answer personal questions asked by a computer changed with "familiarity" (the time a participant had previously spent working on that particular computer), but again question intrusiveness was not varied between conditions. Altering the order of intrusiveness of a set of questions is akin to asking respondents to comply with requests of different magnitude and therefore is comparable to the literature on "foot-in-the-door" (FITD) (Freedman and Fraser 1966) or "door-in-the-face" (DITF) techniques (Cialdini et al. 1975). However, to our knowledge, no study has investigated the impact of such techniques on self-disclosure.[1]

The current article is related to three streams of marketing research. First, our studies contribute to the literature on survey design and, in particular, to the stream of studies on the impact of contextual factors on "self-reports" (e.g., Schwarz 1999; Schwarz and Bienias 1990). Second, we build on the social psychology literature on self-disclosure (e.g., Altman and Taylor 1973; Derlega et al. 1993; Mikulincer and Nachson 1991). This literature has investigated numerous drivers of self-disclosure, some of which we use in our comparative account of the propensity to reveal sensitive information. Third, we contribute to the literature on the relationship between privacy concern and willingness to divulge (e.g., Joinson, Woodley, and Reips 2007; Margulis 2003) and its relevance to marketing (Culnan and Armstrong 1999; White 2004). Insofar as our results can be extrapolated beyond the narrow domain of survey responses, they suggest that privacy concerns (exemplified by unwillingness to reveal sensitive information) are malleable to non-normative factors.

CONCEPTUAL BACKGROUND, HYPOTHESES, AND EMPIRICAL APPROACH

Human judgment and decision making are inherently comparative in nature. A wide range of research shows that people tend to judge stimuli and make decisions comparatively and that they do so automatically and without conscious awareness. Comparative judgments are especially likely when there is no objective basis for evaluation, which is likely the case for self-disclosure. How much of a net gain (if any) does a person experience by disclosing his or her personal information to find out biological age on data-gathering sites such as www.realage.com? When attributes are difficult to evaluate in the absolute, people naturally seek points of comparison (Hsee et al. 1999). We consider

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[1]Freedman and Fraser (1966) and Furse, Stewart, and Rados (1981) investigate whether the magnitude of initial requests affects people's propensity to participate in a survey but not their disclosure of sensitive information. Reingen and Kernan (1979) and Mowen and Cialdini (1980) manipulate the length of the survey respondents were requested to comply with but not the intrusiveness of its questions.

two such points in our analysis: the effect of others' disclosures and the effect of the ordering of question sensitivity.

The Effect of Others' Disclosures

Research from a variety of literature streams has shown that people are powerfully influenced by the behavior of those around them. Applied to self-disclosure, research and theorizing about herding (Devenow and Welch 1996) suggests that if large numbers of people are revealing some kind of information, there is probably not great risk (and there may even be a benefit) in doing so oneself. Similarly, research on social norms finds that people adapt their behaviors to conform to that of those around them (Asch 1955, 1958; Cialdini and Trost 1998; Jones 1984; Krupka and Weber 2008; Sherif 1966) and infer injunctive norms (what one *should* do) from observations of descriptive norms (what people actually do; Bicchieri 2006). Applied to self-disclosure, these findings imply that when people are surrounded by others who are revealing intimate details about their lives, they may conform to the prevailing norm of divulgence. In addition, research focusing on self-disclosure has found that motives such as desire for social approval (Baumeister and Leary 1995) and reciprocity (Kenny 1994) promote disclosure. Observing other people's willingness to answer intrusive questions, and in particular to admit to sensitive behaviors, may lead respondents to be less concerned about social disapproval and, in turn, to reciprocate disclosure. These theories suggest that we should expect higher admission rates to sensitive behaviors when respondents observe other people more frequently admitting to having engaged in sensitive behaviors and, conversely, lower admission rates when people observe others either denying engaging in behaviors or refusing to answer the questions:

H$_1$: Information about higher admission rates of engagement in sensitive behaviors by others leads to increased admission rates among respondents.

We test H$_1$ in Study 1a. This "herding" effect may arise because it alters the experience of responding affirmatively. Specifically, we hypothesize that people experience less discomfort in disclosing information when they are told that others tend to disclose. In two follow-up studies, we provide supporting evidence for this explanation—namely, that the feedback affects people's expectations about the experience of divulging sensitive information (see Study 1b)—and rule out alternative explanations—namely, that the manipulation alters the interpretation of the questions (see Study 1c).

The Effect of the Ordering of Question Intrusiveness

Other people are only one point of comparison; previous experiences are another. Evidence from a variety of literature streams indicates that disclosure may indeed be influenced by comparisons to a previous disclosure or request for disclosure. From a psychophysics perspective, an intrusive question about engaging in a sensitive behavior may appear more (less) intrusive when contrasted with tamer (more sensitive) enquiries, which in turn would affect a respondent's propensity to admit to having engaged in the behavior (for an account of the relationships between psychophysics and embarrassment, see Latane 1981). Similarly, heuristics such as coherent arbitrariness (Ariely, Loewenstein, and Prelec 2003) and comparative ignorance (Fox and

Tversky 1995) suggest that a respondent's likelihood of answering intrusive questions may be affected by the contrast between the current and the previous questions in a survey. Marketing accounts of FITD (Freedman and Fraser 1966) and DITF (Cialdini et al. 1975) techniques similarly predict that admission rates in a sensitive survey may differ because the order of intrusiveness of questions alters people's perceptions of the intrusiveness of the questions.

Specifically, previous research has identified factors that predict whether DITF or FITD will apply in a given situation (Dillard 1991). Tybout, Sternthal, and Calder's (1983) "availability hypothesis" predicts that a person's compliance with a request depends on the favorableness of the issue-relevant information available in memory, where "issue-relevant information" refers to the behavior of either the requester or the requestee. A tame request is an example of favorable request behavior; a person's compliance with a request is an example of favorable own behavior. Tybout, Sternthal, and Calder propose that FITD enhances compliance only when favorable own behavior (in our context, complying by answering survey questions about engaging in behaviors of various sensitivity) is more readily available in the person's memory than unfavorable request behavior (in our context, the increasing intrusiveness of the questions). Critically, this occurs only if respondents are first asked to accept a substantial initial request (Seligman, Bush, and Kirsch 1976) and if the availability of request behavior has been reduced by making noncontiguous requests (Freedman and Fraser 1966; Seligman, Bush, and Kirsch 1976). Conversely, DITF should enhance compliance when favorable request behavior information (in our context, the intrusiveness of questions decreases through the survey) is more "available" than own behavior information. Presenting successive questions within a single survey and by a single requester should make request behavior more salient than own behavior and should therefore lead to a DITF, rather than an FITD, effect. Thus:

H$_2$: Respondents presented with questions in decreasing (increasing) order of intrusiveness are more (less) likely to admit to having engaged in sensitive behaviors than respondents presented with questions in random order of intrusiveness.

We test H$_2$ in Study 2a. We propose that this effect occurs by altering people's perceptions of the intrusiveness of the questions; specifically, we expect people to judge the questions about sensitive behaviors to be less intrusive when they are presented in a decreasing order of intrusiveness. Accordingly, a corollary hypothesis we test in Study 2a is that the effect of our manipulation should be more significant for the most intrusive questions (compared with tamer ones), while a corollary hypothesis we test in Study 2b is that the ordering of questions directly affects people's judgments of the intrusiveness of the inquiries. Finally, we investigate conditions under which the effect can be altered. In Study 2c, we test whether the effect can be nullified by priming concerns over intrusiveness and privacy at the outset of the study. In Study 2d, consistent with the "perceptual contrast" account of DITF and FITD dynamics (Cantrill and Seibold 1986; Shanab and O'Neil 1982), under which the initial request acts as an anchor against which further requests are interpreted, we test whether the effect can be

nullified by forcing participants to consider the relative intrusiveness of questions before the survey begins.

Although we believe that both herding (H_1) and order (H_2) effects operate through comparative mechanisms, there is reason to believe that the specifics might be subtly different. Presenting information on others' admission rates should naturally draw attention to the act of admitting. Therefore, the mechanism for the herding effect pertains to this experience; when told that others tend to respond affirmatively, we predict that people anticipate less discomfort in responding affirmatively, in turn increasing admission rates. In contrast, the explanation of the effect of the ordering of question sensitivity pertains to people's perceptions of the intrusiveness of the questions rather than the act of admitting itself.

Empirical Approach

All studies were online questionnaires in which participants were asked questions about a series of different behaviors. Between subjects, we manipulated a factor expected to affect comparative judgments (feedback on others' admissions in Studies 1a–1c and the intrusiveness order in which the questions were presented in Studies 2a–2d). Because each study included multiple questions that participants answered in sequence, we analyzed responses using econometric methodologies for panel data. Because participants' answers were (depending on the study) either dichotomous or ordinal, we estimated random-effects probit or ordered probit models. The random-effect specification enables us to measure the effect of the treatment on the dependent measure (the respondents' answers), controlling for the nonindependence of observations by the same respondent and unobservable individual differences (e.g., privacy sensitivity, desire to disclose). The Web Appendix reports additional methodological details (see http://www.marketingpower.com/jmr_webappendix).

In the primary experiments (1a and 2a), participants indicated how frequently (and, therefore, whether) they had engaged in the behaviors. Because most of the behaviors were of a sensitive nature, admitting to having engaged in them carried potential costs, whether subjective (e.g., embarrassment) or objective (e.g., incrimination), which, we posited, would create an obstacle to responding affirmatively. Therefore, our dependent measure in the primary studies is the propensity to respond affirmatively (hereinafter, we refer to this as "admissions"). In the follow-up studies, participants rated various aspects of the questions as a function of the experimental manipulations, such as their perceived intrusiveness or clarity.

To make admissions more directly relevant to marketers, we also asked participants to provide e-mail addresses. To provide an incentive for participants to respond truthfully, we offered them the option of receiving "personalized results, including where [they] fall relative to others on the traits and attitudes the survey measures." Insofar as participants were interested in accurate feedback, this feature created an incentive for truthful responding. However, and importantly, by "admission" we do not necessarily refer to *truthful* admissions: Our interest is not in the true underlying prevalence; rather, we study the comparative nature of people's willingness to divulge sensitive information by openly admitting to having engaged in embarrassing, socially unappealing, and even illegal behaviors. In other words, we were not attempting to measure the true prevalence of these behaviors per se; rather, we investigate how incentives for truthful responding would interact with resistance to embarrassing or incriminating self-disclosure as a function of our experimental manipulations.

Missing answers. It was possible for participants to leave items blank. In the analyses reported in the main body of the manuscript, we treat such nonresponses as neither admissions nor denials. However, missing answers may signal a participant's unwillingness to answer a question or may simply be due to attrition. Both scenarios are of interest to us. Willing refusal to answer a question implies the absence of an explicit affirmative admission. Thus, we also analyzed our data in a specification that treats missing answers as nonadmissions. The results are equivalent to those we obtained when ignoring nonresponses; we report and discuss these in the Web Appendix (http://www.marketingpower.com/jmr_webappendix). Conversely, attrition could generate survivor bias. However, our results are not altered by survivor bias, either because the number of participants who did not complete the survey did not differ across conditions (Study 1a) or because the differences do not affect our conclusions (Study 2a). We also present the related analysis in the Web Appendix.

The behaviors. The behaviors varied in sensitivity, from tame (e.g., failing to tip a waiter in a country in which tipping is customary) to highly sensitive (e.g., having sex with the current partner of a friend) and even illegal (e.g., cheating on a tax return). We assessed the perceived intrusiveness of questions about these behaviors in a pilot study in which an independent sample of 25 students at a North American university rated each question on a four-point scale ("not at all intrusive," "mildly intrusive," "intrusive," and "very intrusive").

STUDY 1

Study 1a

Study 1a was a three-condition between-subjects randomized experiment in which we manipulated the distribution of answers ostensibly supplied by other participants in the same survey; in reality, this was a fictional distribution. Participants were told that a relatively large proportion of previous respondents had responded affirmatively (high condition), had responded with denials (low condition), or had not responded at all (missing condition).

Procedure. Participants were directed to the questionnaire by a link titled "Test your ethics" in the online version of the *New York Times* and were randomly assigned to one of the three experimental conditions. Participants were told that they would be presented with descriptions of a series of behaviors and questions about them. They were also told that they would be informed, *after* answering each question, of the current distribution of other respondents' answers. Participants were then asked to provide their e-mail address and to answer a series of demographic questions. Participants were presented with six pairs of questions; each pair pertained to a specific behavior and was presented on its own page. In the first question of each pair, participants were asked to rate the ethicality of the behavior ("not at all unethical," "somewhat unethical," "quite unethical,"

"extremely unethical," "it depends," and "nothing to do with ethics"). In the second question of each pair, participants were asked to indicate how frequently, if ever, they had engaged in the behavior ("never," "once or twice," "sometimes," "frequently").
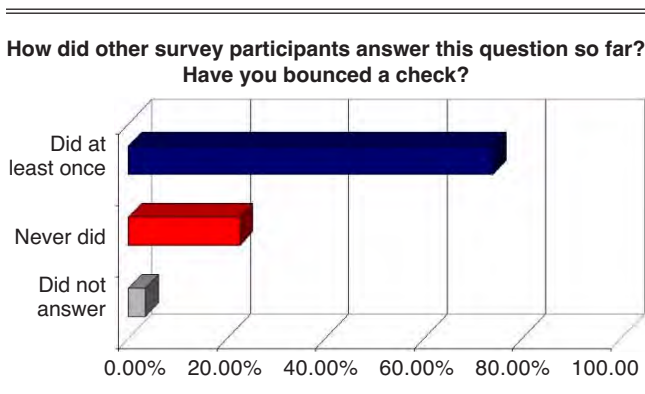
After answering each question, participants could observe the distributions of answers ostensibly given by previous respondents. The distribution showed the percentage of respondents who admitted to having engaged in the behavior, had denied having engaged in the behavior, or had not answered the question. To increase the salience of this information, the distribution of answers was presented visually, in histogram format. In the high condition, the histograms depicted that a majority of other respondents had responded affirmatively. In the low condition, the histograms depicted that a majority of other participants had denied having engaged in the behaviors. In the missing condition, the histograms depicted that a majority of other respondents had left the questions blank. Although the admission rates were always either high or low within a given condition, the exact rates varied between behaviors within each condition to make the feedback credible (see Figure 1). The questions were presented in the same order across the conditions. All questions, except the first one, were picked from the set judged as very intrusive in the pilot study (Appendix A).

Because we were interested in differences in affirmative admission rates (AARs), we hypothesized that participants in the high condition (those who observed high AARs) would be more likely to report having engaged in the behaviors than those in the low or missing conditions (who observed low AARs).

Importantly, because the ostensible distribution of answers to a given question was shown only after the participant had answered the question (and could not go back to change his or her answer), the effect we tested was not the trivial impact of other people's admission to a given behavior on the person's propensity to admit to that same behavior, but rather whether the overall admission to sensitive behaviors would make the participant more likely to admit to other, also sensitive, behaviors. Therefore, our results are not due to mere imitation of other respondents' exact responses to the same question.

Empirical approach. The dependent variable of primary interest was whether participants admitted to having engaged in a behavior. Because we were interested in whether participants admitted to having engaged in a behavior as function of our manipulations rather than their reported frequency of engagement, we collapsed the four frequency categories ("never," "once or twice," "sometimes," "frequently") into one dichotomous variable, which we analyzed using a probit specification (0 = "never engaged in the behavior," and 1 = "engaged in the behavior at least once"). However, the results we present are robust to the consideration of the original, four-point ordinal dependent variable in an ordered probit specification.

Results. Visitors to the *New York Times* website took the survey (N = 1722, M_{age} = 40 years, 45% male, and 82% Caucasian; men were slightly more represented in the high condition [significant at the 5% level], however the results do not change when we control for gender or other demographics; all other demographic traits were similarly distributed between conditions). Table 1, Column 1, presents the results of the random-effects probit specification. As we hypothesized, AARs were significantly higher in the high condition than in the low and missing conditions. The coef-

Figure 1
STUDY 1A: SCREENSHOT FROM THE HIGH CONDITION

How did other survey participants answer this question so far? Have you bounced a check?

Table 1
STUDY 1A: RANDOM-EFFECTS PROBIT ESTIMATES

	Column 1 (Dependent Variable = Engaged in Behavior)		Column 2 (Dependent Variable = Engaged in Behavior)	
	Coefficient	p	Coefficient	p
Constant	.0964245	.026	–.0588783	.327
High Condition				
Low condition	–.107732	.018	.1907377	.024
Missing condition	–.0974721	.032	.0673135	.424
Question 1 (Bouncing Check)				
Question 2 (cheating on tax return)	–.9451133	.000	–.8125121	.000
Question 3 (false insurance claim)	–1.467787	.000	–1.342974	.000
Question 4 (desire for minor)	–.6812493	.000	–.4382892	.000
Question 5 (cheating on partner)	–1.265129	.000	–1.113772	.000
Question 6 (fantasizing about nonconsensual sex)	–.5596698	.000	–.2796096	.001
Interaction Terms				
Low × Question 2			–.3168739	.011
Low × Question 3			–.2844941	.046
Low × Question 4			–.4395456	.000
Low × Question 5			–.3107992	.021
Low × Question 6			–.4955467	.000
Missing × Question 2			–.0837896	.495
Missing × Question 3			–.0916801	.515
Missing × Question 4			–.2937041	.014
Missing × Question 5			–.1453321	.275
Missing × Question 6			–.3524286	.003
	Prob > χ^2 = .0000 n = 1538		Prob > χ^2 = .0000 n = 1474	

ficients for both the low condition dummy and the missing condition dummy are negative and significant at the 5% level (the baseline condition is the high condition).[2] Table 2 presents admission rates to each individual question. Given the different base rates of admissions between questions, we examine the mean percentage difference in admissions rates between conditions, averaging over questions. On average, participants in the high condition were 27% more likely to admit to having engaged in the behaviors than participants in the low condition (t[948] = 3.74, p = .0001) and 21% more likely to admit to having engaged in the behaviors than participants in the missing condition (t[942] = 2.99, p = .0014).[3] In contrast, there were no statistically significant differences between the AARs in the low and the missing conditions. Table 2 also shows how—except for the first question ("Have you ever bounced a check"), which was asked before the manipulation actually started—AARs in the high condition were always higher than the AARs in the other two conditions. This was statistically significant for

[2]Regression results with demographics are equivalent for all studies; these are available on request.

[3]We excluded responses to the first question from the results because the ostensible distribution of answers to each question was shown to the participant after he or she had already answered it. The overall difference between conditions is weakened, but remains significant, when we include the answers to the first question.

Table 2

STUDY 1A: PERCENTAGES OF PARTICIPANTS ADMITTING TO HAVING ENGAGED IN VARIOUS BEHAVIORS

	Never	Once or More	No Answer	p
Bouncing a Check				
High condition	45.72	41.88	12.39	.08
Low condition	40.78	49.65	9.57	
Missing condition	44.44	44.96	10.6	
Cheating on a Tax Return				
High condition	68.94	18.32	12.74	.386
Low condition	73.94	15.96	10.11	
Missing condition	69.74	17.95	12.31	
Making a False or Even Somewhat Inflated Insurance Claim				
High condition	78.18	8.38	13.44	.551
Low condition	81.74	7.27	10.99	
Missing condition	78.29	8.03	13.68	
While an Adult, Having Sexual Desires for a Minor				
High condition	57.07	27.23	15.71	.002*
Low condition	66.67	21.45	11.88	
Missing condition	63.42	21.37	15.21	
Having Sex with the Current Husband, Wife, or Partner of a Friend				
High condition	72.6	11.69	15.71	.298
Low condition	76.6	9.93	13.48	
Missing condition	74.7	10.6	14.7	
Fantasizing About Having Violent, Nonconsensual Sex with Someone				
High condition	52.36	31.59	16.06	<.0005**
Low condition	62.23	23.58	14.18	
Missing condition	59.83	23.59	16.58	

*p < .05.
**p < .01.
Notes: Last column presents Pearson χ²(1) *p*-values for relationship between condition (comparing high with low and missing) and admission rates (comparing "once or more" with "never") only for participants who provided an answer. Significance levels include Bonferroni correction for n = 6. Significance levels are comparable for Pearson χ² calculated over the three conditions separately.

two of the three last questions (also after we applied a Bonferroni correction for multiple comparisons).

Because we applied the experimental treatment sequentially and repetitively following each question, a corollary of H_1 would predict that the impact of the manipulation would increase as participants answered successive questions. This was indeed the case. A version of the random-effects probit with interaction confirms that interactions between the last questions in the survey and the missing and low conditions are negative and significant (see Table 1, Column 2). Furthermore, Figure 2 shows the cumulative distribution of AARs across conditions (excluding answers to the first question). In addition to illustrating the increasing departure of the high condition from the other two conditions, the figure highlights the equivalence of the AARs in the low and missing conditions.

Across all conditions, the majority of participants provided e-mail addresses. Overall, participants who provided e-mail addresses were not only more susceptible to the manipulation than those who did not but also more likely to admit to having engaged in the behaviors. Participants were equally likely to complete the survey across conditions, and our results are robust to the provision of potentially identifying information. Our results are also robust to coding missing answers as nonadmissions (participants in the high condition remain 19% more likely to admit to having engaged in the behaviors than participants in the low and missing conditions). Details about e-mail and nonresponse analyses appear in the Web Appendix (http://www.marketingpower.com/jmr_webappendix).

Study 1b

Participants in Study 1a were more likely to admit to having engaged in sensitive behaviors when told that a relatively high proportion of previous respondents had made similar admissions. In Study 1b, we test a possible explanation for this effect: Seeing that many other respondents felt comfortable responding affirmatively may have affected expectations about the experience of responding affirmatively; specifically, it might make the respondents anticipate

Figure 2

STUDY 1A: CUMULATIVE ADMISSION RATES (IN PERCENTAGES) THROUGH QUESTIONS 2–6 ACROSS CONDITIONS
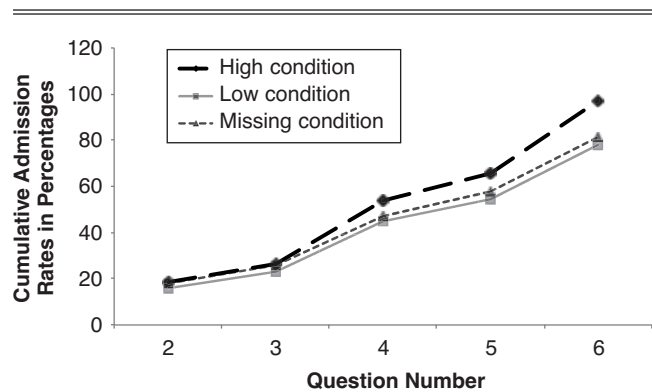
Notes: The question numbers reflect the order in which the question was presented to the participants.

less discomfort in responding affirmatively. We test this idea by measuring people's perceptions of how uncomfortable it would be to respond "yes" to each question as a function of the manipulation used in Study 1a. We hypothesized that participants in the high condition would expect to experience lower discomfort in responding affirmatively compared with those in the low condition.

The design was equivalent to that which we employed in Study 1a, except that, instead of indicating whether they had engaged in the behaviors, participants were asked, "How uncomfortable would it be for you to respond 'yes' to this question?" Participants responded on a four-point scale: "not at all uncomfortable," "somewhat uncomfortable," "uncomfortable," "very uncomfortable." The behaviors were the same as those used in Study 1a. As in Study 1a, after each answer about a given behavior, participants were shown a histogram that depicted the proportion of people who ostensibly had previously indicated that they had engaged in the behavior. We ran two conditions in Study 1b: We manipulated the histograms to depict either relatively high or relatively low AARs.

Results. Participants (N = 247, M_{age} = 34 years, 39% male, and 83% Caucasian) were recruited online and were randomly assigned to the high or low conditions; they were given a small fixed payment at the end of the study. As we hypothesized, a random-effects ordered probit model indicated that discomfort ratings were statistically significantly lower in the high condition than in the low condition (Table 3, Column 1). When we averaged across Questions 2–6, the mean reported discomfort in the low condition was 1.90; in the high condition, it was 1.72 (t[243] = 1.76, p = .0402; as we expected, discomfort ratings were not different between conditions for Question 1, which was asked before the feedback manipulation took place).

Table 3

STUDIES 1B AND 1C: RANDOM-EFFECTS ORDERED PROBIT ESTIMATES

	Study 1b (Dependent Variable = Discomfort)		Study 1c (Dependent Variable = Clarity)	
	Coefficient	p	Coefficient	p
Constant	.226441	.086	–2.276073	.000
Low Condition				
High condition	–.3005449	.045	.060797	.809
Question 1 (Bouncing Check)				
Question 2 (cheating on tax return)	–.071444	.544	.475004	.110
Question 3 (false insurance claim)	–.5091979	.000	.0087372	.979
Question 4 (desire for minor)	.6122745	.000	1.104345	.000
Question 5 (cheating on partner)	.351803	.002	.585664	.048
Question 6 (fantasizing about nonconsensual sex)	.6326698	.000	.9916652	.001
	Prob > χ^2 = .0000 n = 247		Prob > χ^2 = .0002 n = 121	

Alternative Explanations and Study 1c

Study 1a suggests that people are more likely to report having engaged in sensitive behaviors when they are led to believe that others have also admitted to engaging in other sensitive behaviors. Study 1b suggests that believing that a majority of other people are willing to make such admissions makes it less uncomfortable for a person to do so him- or herself. These findings suggest that herding behavior affects the propensity to disclose sensitive information. There are, however, several alternative and more mundane possible explanations for this effect.

First, in the high condition, seeing that a large proportion of other participants had responded affirmatively may have simply made affirmation a more available response. However, a closer examination of the results of Study 1a suggests that this is unlikely. The histograms representing other respondents' ostensible AARs collapsed the response options into three categories ("never did," "did at least once," "refuse to answer"), while actual respondents used a different, four-point scale ("never," "once or twice," "sometimes," "frequently"). If the results were merely driven by "did at least once" being a more available response, we would expect participants to be more likely to simply affirm that they had engaged in the behaviors once. Instead, our manipulation is also significant in an ordered probit specification of Study 1a, in which the dependent variable is ordinal (from "never" to "frequently") instead of dichotomous. The percentage of participants claiming to have engaged in a behavior "more than once" is larger in the high condition (10.20%) than in the low and missing conditions (6.99% and 4.38%, respectively; Pearson $\chi^2(2)$ = 9.16, p = .0100). This suggests that the entire distribution of reported frequencies shifts to the right in the high condition: Participants do not simply admit to having engaged in more behaviors; they actually report higher frequencies of engagement.

Study 1c. A second alternative explanation is that the information about other people's admissions affected people's construal of the behaviors in question, consistent with previous research demonstrating how a survey's design (e.g., question order, response options) can shape respondents' interpretations of the questions posed therein (e.g., Schwarz and Scheuring 1988). In the high condition, participants may have inferred the behaviors to be broadly defined; believing that more people had admitted to these behaviors may have led participants to think of more instances in which they had engaged in sensitive behaviors. For example, believing that a large proportion of previous respondents admitted to "cheating on a partner," participants may have interpreted this item broadly to include a wide range of activities, from flirting with a person other than the relationship partner to having sexual intercourse with such a person. In contrast, in the low or missing conditions, believing that only a few previous respondents had responded affirmatively, participants may have inferred the behaviors to be narrowly defined, resulting in lower AARs. However, a follow-up experiment (Study 1c) suggests that this explanation cannot account for our results.

The design was similar to Study 1a, except that participants were told that we were "not interested in whether you have engaged in the given behavior. Instead, we are inter-

ested in how you think other subjects interpret those behaviors. We will present you with two descriptions of each behavior, and ask you to choose the one you think best describes how other [subjects] interpret that behavior." As in Study 1a, participants were first presented with a behavior (e.g., "while an adult, having sexual desires for a minor"). Then, they were asked to check which of two descriptions best described how they thought other respondents would interpret the phrase. For each behavior, they were presented with two descriptions: a literal description (e.g., "as an adult, feeling desires of a sexual nature towards a minor") and a broader one (e.g., "as an adult, finding a minor attractive"). After each answer, participants were shown histograms depicting the proportion of participants who had indicated, in a previous survey, that they had engaged in the behavior.

Results. We recruited participants online (N = 126, M_{age} = 32 years, 47% male, and 77% Caucasian; there were no significant differences between conditions). A random-effects probit model shows no significant difference between the breadth of definitions chosen by participants in the high versus low conditions (Table 3, Column 2). This result suggests that the herding manipulation does not affect participants' interpretation of question breadth.

STUDY 2

Study 2a

In Study 2a, we tested the effect of ordering questions along different gradients of intrusiveness (determined by ratings from the pilot study) on the propensity to respond affirmatively. As in Study 1a, participants first judged the ethicality of each behavior and then reported whether they had engaged in the behavior. The response options were the same as those used in Study 1a. The study was a 2 × 4 (between subjects) × 3 (within subjects) randomized experiment. In the front conditions, participants were asked to provide potentially identifying information (e-mail address) at the beginning of the questionnaire; in the end conditions, they were asked to provide this information at the end of the questionnaire. More important, we manipulated the order in which questions of different sensitivity were presented. In the increasing conditions, the questions were presented in an increasing order of intrusiveness. In the decreasing conditions, we reversed this order (i.e., participants first faced questions about the most sensitive behaviors, and the questions became progressively tamer through the questionnaire). In the random conditions, the questions were presented in a pseudorandom order of intrusiveness (i.e., the questions were placed in a jumbled order with respect to their intrusiveness). We included the random condition to pinpoint whether the decreasing conditions facilitate admissions and/or whether the increasing condition inhibits admissions. The sudden conditions consisted of only tame questions, except the last three questions, which were identical to the last three in the increasing condition (and were therefore highly intrusive). The sudden conditions served as an alternative control to test the propensity to admit to the most sensitive behaviors for participants who initially faced tame questions. Finally, within subjects, we examined the propensity to answer affirmatively to questions of different sensitivity (tame, moderate, and intrusive).

As in Study 1a, the dependent variable of interest was the propensity to respond affirmatively, which we estimated through a probit model. We tested whether this propensity depends on (1) the order in which questions are presented with respect to their intrusiveness and (2) the sensitivity of the questions. The results we present are also robust to the consideration of the actual reported frequencies of engagement in an ordered probit specification.

Empirical approach. The empirical approach was equivalent to that used in Study 1a. To take into account the differences in question intrusiveness, we used the results of the pilot study to include categorical dummies in the regressions, representing the ten tamest questions (e.g., "Have you littered in a public space?"), the ten moderate questions (e.g., "While in a relationship, have you flirted with somebody other than your partner?"), and the ten most intrusive questions (e.g., "Have you masturbated at work or in a public rest room?"). The complete list of questions appears in Appendix B.

Missing observations are of even greater importance in Study 2a because the order in which questions were presented varied between conditions (and, in turn, order effects) could interact with the participants' propensity to leave questions blank (e.g., because the participant chose to abandon the questionnaire altogether). In the Web Appendix (http://www.marketingpower.com/jmr_webappendix), we analyze missing observations relative to their placement within the questionnaire. The results presented there, however, are equivalent (for the intrusive questions) to those presented in this article: Our main findings are robust to the consideration of missing answers as nonadmissions and are not determined by survivor bias.

Results. Readers of the online edition of the *New York Times* participated in the study (N = 2310, M_{age} = 38 years, 65% male, and 88% Caucasian; there were no significant demographic differences between conditions). Participants were significantly more likely to provide e-mail addresses in the front conditions than in the end conditions; however, the point at which they were asked to supply their e-mail address did not interact with the question order manipulation. Therefore, we collapse across the front and end conditions for the rest of the analysis and refer to increasing, decreasing, and random conditions in the singular.

We begin by focusing on the increasing, decreasing, and random conditions (which, unlike the sudden condition, are comparable because they contained the same questions, albeit in different orders). Table 4, Column 1, presents the results of the random-effects probit specification. As we hypothesized, across all questions (tame, moderate, and intrusive), participants in the increasing condition were less likely to admit to behaviors than those in the decreasing condition; there was no such difference between the random and decreasing conditions. Table 4, Column 2, includes the interaction terms. It shows that participants in the decreasing condition were significantly more likely to admit to the most sensitive behaviors than participants in both the increasing and the random conditions. This finding confirms that our manipulation is particularly significant for questions associated with the most sensitive behaviors. Furthermore, in both specifications, AARs were lower for sensitive and moderately intrusive questions relative to the tame questions.

Table 4
STUDY 2A: RANDOM-EFFECTS PROBIT ESTIMATES

	Column 1 (Dependent Variable = Engaged in Behavior)		Column 2 (Dependent Variable = Engaged in Behavior)	
	Coefficient	p	Coefficient	p
Constant	.3701102	.000	.3393673	.000
Decreasing Condition				
Increasing condition	−.1596418	.000	−.1173054	.000
Random condition	−.0119736	.614	.0348463	.274
Tame Questions				
Moderate questions	−.8508995	.000	−.8373933	.000
Intrusive questions	−.9094224	.000	−.831757	.000
Interaction Terms				
Increasing × moderate			−.0350908	.348
Increasing × intrusive			−.0935159	.013
Random × moderate			−.0041377	.912
Random × intrusive			−.1368417	.002
	Prob > χ^2 = .0000		Prob > χ^2 = .0000	
	n = 1581		n = 1581	

Table 5 presents admissions rates for each individual question, ordered from most intrusive to least intrusive. Note that the decreasing condition exhibits high nonadmission rates for the first question (which is very sensitive). While the low AARs for that question are not significantly different across conditions, significant differences emerge in subsequent questions, with participants in the decreasing condition being more likely to admit to the subsequent (still sensitive, but less so) behaviors. This pattern is consistent with the DITF dynamics we highlighted previously. (As a caveat, we note that the analysis of AARs to individual questions is confounded by idiosyncrasies specific to each question, whereas categorical dummies such as "tame questions," which we used in the regression, provide a more robust analysis.) Averaging the percentage differences in AARs across questions, we find that participants in the increasing condition were 19% less likely than participants in the decreasing condition to admit to having engaged in the behaviors (t[917] = 6.64, p < .0005) and 18% less likely than those in the random condition (t[939] = −6.58, p < .0005). As for the most intrusive questions, participants in the increasing condition were 20% less likely to admit to the sensitive behaviors (t[980] = 3.92, p < .0005) than participants in the random condition, while participants in the decreasing condition were 15% more likely to admit to the most sensitive behaviors (t[969] = 2.79, p = .0027) than participants in the random condition. Furthermore, participants in the increasing condition were 51% less likely than those in the decreasing condition to admit to having engaged in the ten most sensitive behaviors (t[967] = 6.84, p < .0005). In contrast, the differences in AARs between conditions are much less dramatic for the less intrusive questions.

Panels A and B in Figure 3 display the cumulative AARs, question by question, across the three main conditions.
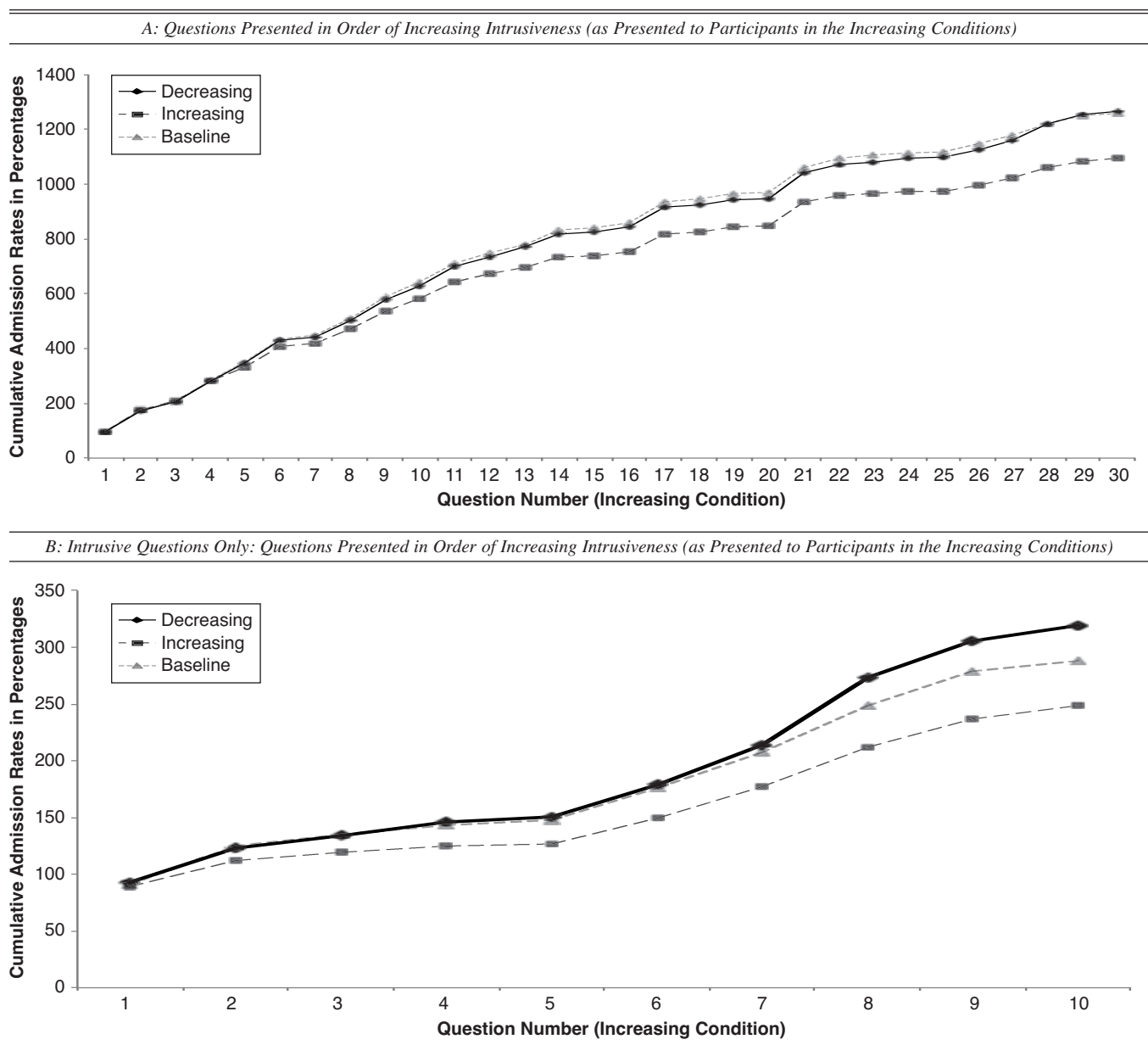
Table 5
STUDY 2A: PERCENTAGES OF PARTICIPANTS ADMITTING TO HAVING ENGAGED IN VARIOUS BEHAVIORS

	Decreasing	Random	Increasing	p
Had sex with the current husband, wife, or partner of a friend?	13.24	9.84	11.62	.242
Masturbated at work or in a public restroom?	32.09	29.96	25.15	.046
Had a fantasy of doing something terrible (e.g., torturing) to someone?	59.6	41.32	34.81	<.0005***
Fantasized about having violent nonconsensual sex with someone?	35.05	31.49	27.25	.029
While an adult, had sexual desires for a minor?	28.63	28.35	22.89	.068
Neglected to tell a partner about a sexually transmitted disease from which you were currently suffering?	4.37	3.88	1.8	.057
Had sex with someone who was too drunk to know what they were doing?	11.86	8.41	5.7	.002*
Stolen anything worth more than $100?	11	10.65	6.88	.046
Tried to gain access to someone else's (e.g., a partner, friend, or colleague's) e-mail account?	30.41	33.85	23.38	.001**
Looked at pornographic material?	92.65	90.74	89.02	.139
Made a false insurance claim?	4.89	5.34	2.55	.061
Cheated on your tax return?	18.29	19	21.1	.504
Claimed to have education that you didn't actually have?	6.75	9.91	6.63	.081
While in a relationship, flirted with somebody other than your partner?	74.23	75.98	65.37	<.0005***
Taken credit for someone else's work?	16.16	19.42	12.06	.005
Known about or witnessed a serious crime and failed to report it or stop it?	7.79	7.75	5.26	.192
Let a friend drive after you thought he or she had had too much to drink?	48.97	54.44	37.48	<.0005***
Made up a serious excuse, such as grave illness or death in the family, to get out of doing something?	35.66	30.2	21.98	<.0005***
Lied about your income to someone?	34.78	38.26	31.03	.051
Called in sick when you were not sick?	70.81	68.99	60.8	.001**
Visited an internet dating website, even just to check out what types of people might be available?	50.83	53.97	45.4	.021
Pretended not to see a beggar to avoid being seen as stingy?	74.12	79.17	65.58	<.0005***
Downloaded pirated songs from the Internet?	61	59.25	53.92	.058
Gone on a date only to make somebody else jealous?	12.06	15.56	10.94	.069
Drunk so much that you got a hangover?	83.82	81.85	75.37	.002*
Littered in a public space?	64.02	65.67	49.72	<.0005***
Failed to do chores in a shared house or apartment?	75.37	78.25	71.67	.047
Failed to tip a waiter in a country in which tipping is customary?	34.38	32.14	33.58	.751
Failed to turn the lights out at home or work, just because you were feeling lazy?	78.15	79	79.63	.848
In the last year, eaten meat, poultry, or fish?	94.98	96.83	97.2	.135

*p < .10.
**p < .05.
***p < .01.
Notes: Questions presented in decreasing order of intrusiveness. Percentages are of participants who provided an answer. The last column presents three-way Pearson $\chi^2(2)$ p-values, including Bonferroni correction for n = 30.

Figure 3

STUDY 2A: CUMULATIVE ADMISSION RATES (IN PERCENTAGES) ACROSS CONDITIONS

A: Questions Presented in Order of Increasing Intrusiveness (as Presented to Participants in the Increasing Conditions)

B: Intrusive Questions Only: Questions Presented in Order of Increasing Intrusiveness (as Presented to Participants in the Increasing Conditions)

Across the entire survey, AARs in the increasing condition lag behind those of the other conditions (Panel A); this difference in AARs is particularly pronounced for the most intrusive questions (Panel B). In addition, for the most intrusive questions, AARs are higher in the decreasing condition than in all other conditions.

As we noted previously, our main results are also robust to the consideration of missing observations as nonadmissions and are not altered by survivor bias: Overall, AARs are lower in the increasing conditions, and AARs to the most intrusive questions are higher in the decreasing condition. (Details are available in the Web Appendix at http://www.marketingpower.com/jmr_webappendix.)

Sudden condition. The sudden condition included only tame questions, except for the last three, which were the same questions as the last three in the increasing condition (i.e., the three most intrusive questions). The mean admis-

sion rate to the three intrusive questions was the same as that of the increasing condition (.24 vs. .24; t-test $p > .6$), implying that, similar to the increasing condition, participants in the sudden condition were significantly less likely to admit to having engaged in the three most sensitive behaviors compared with the decreasing (t-test $p < .0005$) and random (t-test $p < .0355$) conditions. These results confirm that regardless of whether the shift in question intrusiveness is gradual or sudden, answering intrusive questions after nonintrusive ones inhibits admission.

Study 2b

Study 2a showed that people's willingness to admit to having engaged in sensitive behaviors depends on the intrusiveness of previous such inquiries: Participants who faced questions of increasing (decreasing) sensitivity were less (more) likely to admit to the most intrusive questions than

participants presented with questions in random order of intrusiveness. However, whereas the manipulation in Study 1a, by emphasizing the AARs from other respondents, draws attention to the act of admitting, the manipulation in Study 2a does not. In other words, whereas the herding manipulation affects people's anticipated discomfort in responding affirmatively (Study 1b), we hypothesized that, in Study 2a, the question ordering manipulation would affect judgments of the intrusiveness of the questions. Consistent with AARs, this would imply that people judge questions to be less intrusive when they are presented in a decreasing order of intrusiveness than when they are presented in an increasing order of intrusiveness. We test this idea in Study 2b.

Procedure. Study 2b had a two-condition between-subjects design similar to Study 2a. Participants were presented with a series of questions about different behaviors, ranging from tame to intrusive. To test Study 2a's robustness to a different sequence of questions and to further reduce the risk of survivor bias, we shortened the number of items from 30 to 6 (the 3 tamest and the 3 most intrusive questions in Study 2a). In the decreasing condition, the questions were presented in a decreasing order of intrusiveness; in the increasing condition, the questions were presented in an increasing order of intrusiveness. For each question, participants were asked to "rate how intrusive (if at all) the question is" on a four-point response scale ("not at all intrusive," "mildly intrusive," "intrusive," and "very intrusive").

Results. Participants (N = 133, M_{age} = 33 years, 42% male, and 33% Caucasian; there were no significant differences between conditions) were recruited online and randomly assigned to one of the two conditions. Table 6 presents the results of a random-effects ordered probit model: As we hypothesized, participants in the increasing condition judged the questions to be more intrusive than those in the decreasing condition. The mean intrusiveness rating was 2.09 in the decreasing condition and 2.44 in the increasing condition. (We also ran an additional specification to test the interaction between question intrusiveness and the experimental manipulation: The manipulation remains significant, but the interaction is not, because the mean intrusiveness rating is higher in the increasing condition both for the three least intrusive questions and the three most intrusive ones.)

Study 2c

Taken together, Studies 2a and b suggest that people are less likely to admit to having engaged in sensitive behaviors when the questions are presented in escalating order of sen-

sitivity, altering their perceived intrusiveness. A possible interpretation of these results, as suggested in some of the literature we reviewed previously in this article, is that the differential propensity to disclose is linked to people's malleable concerns about the privacy of their personal information. In other words, it is possible that by altering the order of questions (and therefore their perceived intrusiveness), our manipulation affects privacy concerns. If this were the case, priming participants with considerations of privacy before the survey is administered should have a similar effect to changing the order of question intrusiveness from decreasing to increasing, making them "clam up" and admit to fewer sensitive behaviors. We test this prediction in Study 2c.

Study 2c was a 2 × 2 between-subjects design in which we manipulated whether participants were cued to think of privacy from the outset (privacy cue vs. no cue), along with the order in which the questions were presented (increasing vs. decreasing order of intrusiveness). We hypothesized that the privacy cue would lead participants in either condition to admit less, regardless of the order of the questions. We also hypothesized that, holding the privacy cue manipulation constant, the results of Study 2a would be replicated (i.e., AARs would be higher in the decreasing condition). More important, we hypothesized that after participants are cued to think about privacy, those in the decreasing condition would be no more likely to respond affirmatively than those in the increasing condition, whose privacy concern had not been roused. In other words, we hypothesized that the impact of changing the order of the questions from decreasing to increasing on the propensity to admit would be similar to the impact of cuing participants to think about privacy concerns. We did not need to hypothesize an interaction between the privacy cue and order of questions: Participants in both the increasing and decreasing condition faced extremely intrusive questions that were likely, by themselves, to trigger concerns; thus, the privacy cue manipulation did not elicit a previously nonexistent concern but rather would heighten the concerns already aroused in either condition.

Procedure. All participants were asked to complete two surveys. In the first survey, participants completed a "photo identification task." Participants were asked to either "Phind the phishing e-mails" (privacy conditions) or to "Find the endangered fish" (control conditions). In the privacy conditions, participants were given definitions of phishing (e-mails fraudulently claiming to be reputable that attempt to lure recipients into divulging sensitive information) and spam taken from Wikipedia. In the control conditions, participants were given a definition of endangered species, also from Wikipedia. To reinforce the manipulation, on the subsequent pages, participants were asked to define the term "phishing" or "endangered species" (depending on the condition). On the following six pages, participants were asked to categorize various images presented to them. In the privacy conditions, the images were screenshots of e-mail messages; participants indicated whether each image constituted phishing or spam. In the control conditions, the images were photos of fish, and participants indicated whether each species was endangered. After finishing the first survey, participants clicked a button to begin the second survey. At that point, participants were randomly assigned to one of two versions of the survey used in Study 2a (increasing or decreasing).

Table 6

STUDY 2B: RANDOM-EFFECTS ORDERED PROBIT ESTIMATES

	Study 2b (Dependent Variable = Intrusiveness)	
	Coefficient	p
Constant	.2044809	.000
Decreasing Conditions		
Increasing conditions	.481771	.000
Tame Questions		
Intrusive questions	1.258393	.000
	Prob > χ^2 = .0000	
	n = 123	

Results. Participants were recruited online from a pool of *New York Times* readers; each was randomly assigned to one of four experimental conditions (N = 161, M_{age} = 47 years 51% male, and 88% Caucasian; there were no significant demographic differences between conditions, except an overrepresentation of men in the decreasing condition [significant at the 10% level]; the results presented here do not change when controlling for demographics).

A random-effects probit model revealed that both the privacy cue manipulation and the increasing order manipulation significantly decreased participants' propensity to admit (Table 7, Column 1; the interaction is not significant). Table 7, Column 2, presents the results of an additional specification in which we contrasted only two conditions: the increasing condition without privacy cuing and the decreasing condition with privacy cuing. The higher propensity to admit elicited in the decreasing manipulation disappears: The coefficient for the dummy representing the order of questions manipulation (increasing condition) is no longer significantly different from zero (p > .6). These results are also reflected in the mean AARs across conditions. The mean admission rate is highest in the decreasing condition with no privacy cue (.43) and lowest in the increasing condition with privacy cue (.33), but it is virtually the same for the decreasing condition with privacy cue (.38) and the increasing condition without (.37).

Beyond replicating the results of Study 2a, Study 2c shows that cuing people to think about privacy from the outset of the experiment decreases their propensity to admit: After participants are cued to think about privacy, those in the decreasing condition are no longer more likely to respond affirmatively than those in the increasing condition, whose privacy concerns had not been roused.

Study 2d

Study 2d bridged the designs and goals of Studies 2b and 2c. It tested whether Study 2a's results would also disappear (as in Study 2c) when participants are induced to think about and rank the relative intrusiveness of a set of personal questions (as in Study 2b) before answering questions about their own behavior. Such a finding would support the interpretation of Study 2a's results as an outcome of "perceptual contrast" DITF/FITD dynamics (Cantrill and Seibold 1986;

Shanab and O'Neil 1982): The perception of the intrusiveness of personal questions changes with the order in which those questions are presented, which in turn affects participants' propensity to admit to the behaviors. Thus, if participants are primed at the outset to think about and contrast questions of varying intrusiveness, the differential impact of the order of questions on propensity to admit should be nullified.

Study 2d was a 2 × 2 between-subjects design in which we manipulated whether participants were cued to compare the intrusiveness (or wordiness) of a set of questions about personal behaviors from the outset, along with the order in which a different set of questions was presented to them (increasing vs. decreasing order of intrusiveness). We hypothesized that in the conditions in which participants were asked to rate the wordiness of questions about behaviors, we would replicate the findings of Study 2a (i.e., question order would affect propensity to admit, with higher AARs in the decreasing condition), whereas in the conditions in which participants were asked to rate the intrusiveness of questions about behaviors, we would no longer find any impact of question order on propensity to admit.

Procedure. All participants were asked to complete two surveys. In the first survey, participants were asked to rate the intrusiveness of 18 questions about various behaviors (intrusiveness conditions: "How intrusive, if at all, is each of the following questions? Note: please rate the intrusiveness, independently of whether or not the behavior in question applies to you") or the wordiness (control conditions: "How wordy, if at all, is each of the following questions? Note: please rate the wordiness, independently of whether or not the behavior in question applies to you"). The behaviors ranged in sensitivity from low to high (as measured in a pilot survey). Questions about the behaviors were presented in pseudorandom order of sensitivity. Participants then continued on to the second survey, which was equivalent to the survey used in Study 2a: Participants were presented with six behaviors (three sensitive, three nonsensitive) and were asked how frequently, if ever, they had engaged in them. Depending on the condition, the behaviors were presented in either increasing or decreasing order of intrusiveness. There was no overlap between the set of behaviors included in the first survey and those included in the second survey.

Table 7

STUDIES 2C AND 2D: RANDOM-EFFECTS PROBIT ESTIMATES

	Column 1 (Dependent Variable = Engaged in Behavior)		Column 2: Study 2c— Reduced Conditions Set (Dependent Variable = Engaged in Behavior)		Column 3: Study 2d— Wordiness Conditions (Dependent Variable = Engaged in Behavior)		Column 4: Study 2d— Intrusiveness Conditions (Dependent Variable = Engaged in Behavior)	
	Coefficient	p	Coefficient	p	Coefficient	p	Coefficient	p
Constant	−.0047733	.928	−.1579762	.001	.6439973	.000	.5868037	.000
Decreasing Condition								
Increasing condition	−.1692851	.019	−.0312338	.653	−.2336612	.018	−.1151977	.213
Tame Questions								
Intrusive questions	−.5681603	.000	−.5210749	.000	−1.229157	.000	−1.163632	.000
No Phishing Cue								
Phishing cue	−.138234	.038						
Phishing cue × increasing condition	.0437733	.671						
	Prob > χ^2 = .0000 n = 161		Prob > χ^2 = .0000 n = 94		Prob > χ^2 = .0000 n = 129		Prob > χ^2 = .0000 n = 139	

Results. Participants were recruited online and were randomly assigned to one of four conditions (N = 276, M_{age} = 35, 37% male, and 81% Caucasian; there were no significant demographic differences between conditions). Participants were given a small fixed payment at the end of the study. A random-effects probit model shows that in the control conditions (in which participants were primed to think about the wordiness of questions about different behaviors), the results of Study 2a are replicated: The increasing order manipulation significantly decreases participants' propensity to admit (see Table 7, Column 3). However, in the intrusiveness conditions (in which participants were primed to think about the intrusiveness of questions about different behaviors), the order of questions no longer differentially affects participants' propensity to admit (see Table 7, Columns 4 and 5). In addition to replicating the results of Study 2a, Study 2d suggests that cuing people to think about the intrusiveness of questions from the outset of the experiment nullifies the impact of order of questions on propensity to admit.

Analysis

Study 2a shows that people are less likely to admit to having engaged in sensitive behaviors when the questions are presented in escalating order of sensitivity than when questions are presented in descending order. Studies 2b and 2c, in addition to the finding that the reduction in propensity to admit is more pronounced for the most intrusive questions, shed light on the process underlying this effect. Study 2b shows that question order affects perceptions of the intrusiveness of the questions: When the questions are presented in decreasing order of intrusiveness, they are judged to be less intrusive than when they are presented in an increasing order. Moreover, this difference in intrusiveness ratings is not a mere reflection of the participants in the decreasing condition also being more likely to admit to having engaged in the behavior (i.e., it cannot be a simple by-product of the increased tendency to respond affirmatively in the descending condition) because participants judged the intrusiveness of the questions without indicating whether they had engaged in the behaviors. Studies 2c and 2d provide further evidence of the explanation for Study 2a's results by showing that when participants in the decreasing condition are cued to think about privacy concerns (Study 2c), their disclosure levels are similar to those in the increasing condition who are not cued to think about privacy, and when they are cued to think about the intrusiveness of various questions before reporting their actual behavior, their propensity to admit is similar to those in the increasing condition (Study 2d).

DISCUSSION

To the delight of marketers, new technologies have facilitated the acquisition, storage, and integration of consumers' personal information on a mass scale. However, these technological advances have also made it increasingly difficult for consumers to navigate issues of self-disclosure—that is, to choose an "optimal" balance between information protection and information sharing in different situations. This can also be a problem for marketers: The deluge of requests for personal data may lead consumers either to reveal more or to clam up and become less willing to disclose.

In this article, we provide evidence that the inherently comparative nature of human judgment and decision making plays out in the way people decide to reveal personal, potentially embarrassing, and even incriminating information. Specifically, we showed that judgments of and responses to requests for sensitive information depend crucially on two points of comparison: the judgments and responses of other people and the order in which questions of different sensitivities are presented. We also found that our manipulations seem to affect the feeling of discomfort or intrusiveness associated with the surveys but not the perceived clarity of its questions. In combination, therefore, our studies support the hypothesis that people's decisions to disclose sensitive information are comparative in nature.

It is important to note that our studies focused on determining how comparative valuations affect a person's propensity to report to others certain information about him- or herself. However, our studies were not designed to establish "true" prevalence estimates of the behaviors in question and were limited to a specific type of information that consumers may feel uncomfortable divulging (engagement in embarrassing or sensitive behaviors) as opposed to other types of information, such as Social Security numbers. Thus, we cannot tell whether, beyond affecting people's propensity to admit to behaviors they had engaged in, our manipulations may have also caused people to admit to having engaged in behaviors in which they had actually never engaged. Our results tend to be strongest for intrusive items, such as having sexual desires for a minor, which seems to weigh against such an effect. Furthermore, in the high admission condition in Study 1a, 27.2% of participants claimed to have had sexual desires for a minor; 11.7% claimed to have had sex with the current husband, wife, or partner of a friend; and 31.6% claimed to have fantasized about having violent, nonconsensual sex with someone. The percentages virtually match those provided in the decreasing condition in Study 2a (24.6%, 11.4%, and 30.2%, respectively). Either two completely different treatments led participants to lie to the same degree, or participants in the high and decreasing conditions were more comfortable responding affirmatively to behaviors in which they had engaged relative to other participants in the respective studies.

Marketing researchers and professionals frequently use online surveys, games, and quizzes aimed at inferring people's personal information. Our results highlight some challenges in choosing the structure and timing of personal inquiries in a context in which consumers are influenced by multiple requests for personal information and surrounded by streams of information about others. The current research has implications for the design of marketing surveys, especially those involving intrusive questions and sensitive behaviors. In general, instructions on survey design suggest that researchers open their questionnaires with general, milder questions.[4] In contrast, and somewhat at odds with the guidance from this literature, our results suggest that

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[4]For example, "Ideally, the early questions in a survey should be easy and pleasant to answer…. Whenever possible leave difficult or sensitive questions until near the end of your survey" (from Creative Research Systems at http://www.surveysystem.com/sdesign.htm), and "First questions should be relevant and easy…. Potentially objectionable questions are placed near the end" (from Penn State Survey Research Center at www.ssri.psu.edu/survey/qd.ppt). See also Payne (1951, p. 34).

beginning with milder questions only to move toward more intrusive questions may actually elicit lower overall willingness to divulge. Furthermore, our studies also suggest that when survey takers are given (or otherwise have access to) information about the occurrence of certain behaviors, this information can significantly affect their propensity to reveal personal and sensitive information about themselves.

Perhaps the most important implications of our results pertain to consumer welfare. New information technologies have enhanced consumers' ability to communicate and interact, but they have also raised novel and troubling issues about the privacy and security of personal data. These considerations have generated renewed interest in the trade-offs between privacy and (for example) personalization, which has been described as the future of interactive marketing (Deighton 1996). Implicit in much of the literature dealing with privacy trade-offs is the assumption that consumers are rationally informed agents with stable preferences for self-disclosure and privacy (Posner 1978). However, our results suggest a different story. Self-disclosure seems to be affected by information about others' divulgences and the mere order in which sensitive inquiries are presented. Insofar as our privacy account is valid, our results suggest that privacy concerns are also malleable to the influence of comparative judgments. If privacy preferences are indeed instable, doubts arise about which behavior represents the "true" desired level of information protection and revelation and, more important, whether consumers can make self-interested decisions with respect to their data when interacting with increasing complex information technologies—decisions, in other words, that they do not stand to later regret.

APPENDIX A: SURVEY QUESTIONS ASKED IN STUDY 1A

1. Have you bounced a check?
2. Have you cheated on your tax return?
3. Have you made a false or even somewhat inflated insurance claim?
4. While an adult, have you had sexual desires for a minor?
5. Have you had sex with the current husband, wife, or partner of a friend?
6. Have you fantasized about having violent, nonconsensual sex with someone?

APPENDIX B: SURVEY QUESTIONS ASKED IN STUDY 2A (AS PRESENTED TO PARTICIPANTS IN THE DECREASING CONDITIONS)

1. Have you had sex with the current husband, wife, or partner of a friend?
2. Have you masturbated at work or in a public rest room?
3. Have you had a fantasy of doing something terrible (e.g., torturing) to someone?
4. Have you fantasized about having violent nonconsensual sex with someone?
5. While an adult, have you had sexual desires for a minor?
6. Have you neglected to tell a partner about a sexually transmitted disease from which you were currently suffering?
7. Have you had sex with someone who was too drunk to know what they were doing?
8. Have you stolen anything worth more than $100?

9. Have you tried to gain access to someone else's (e.g., a partner, friend, or colleague's) e-mail account?
10. Have you looked at pornographic material?
11. Have you made a false insurance claim?
12. Have you cheated on your tax return?
13. Have you claimed to have education that you didn't actually have?
14. While in a relationship, have you flirted with somebody other than your partner?
15. Have you taken credit for someone else's work?
16. Have you known about or witnessed a serious crime and failed to report it or stop it?
17. Have you let a friend drive after you thought he or she had had too much to drink?
18. Have you made up a serious excuse, such as grave illness or death in the family, to get out of doing something?
19. Have you lied about your income to someone?
20. Have you called in sick when you were not sick?
21. Have you visited an internet dating website, even just to check out what types of people might be available?
22. Have you pretended not to see a beggar to avoid being seen as stingy?
23. Have you downloaded pirated songs from the Internet?
24. Have you gone on a date only to make somebody else jealous?
25. Have you drunk so much that you got a hangover?
26. Have you littered in a public space?
27. Have you failed to do chores in a shared house or apartment?
28. Have you failed to tip a waiter in a country in which tipping is customary?
29. Have you failed to turn the lights out at home or work, just because you were feeling lazy?
30. In the last year, have you eaten meat, poultry, or fish?

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