Discussion paper

The (un)compromise effect

BY
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Abstract

The current study provides the first experimental test of the compromise effect, i.e. the tendency to choose middle options, in a naturally occurring setting. Simultaneously, I propose and evaluate a novel nudge intended to stimulate active choice—the (un)compromise effect—a compromise effect without an explicit middle option. 63,494 recipients of a mail fundraiser were randomly assigned to one of three sets of suggested donations: [$10, $50, $100, $___]; [$10, $50, $100, $250, $500, $___]; or [$10, $500, $___]. The results support both the compromise effect and the (un)compromise effect: extending the range increased the fraction donating $100 as well as the average donation—independent of whether the middle suggested donations were present or not. Hence, by only providing informative end points, organizations can affect decision-making and at the same time promote individuality through active choice. The results also shed light on why suggested

alternatives affect choice in general: they reduce the cognitive cost of figuring out what actions are appropriate.

1 Introduction

A large body of research in psychology, economics and marketing demonstrates that seemingly irrelevant factors influence people's decisions. One of the best known examples of such influence is the compromise effect (Simonson, 1989), which refers to the finding that people have an increased tendency to choose the middle option within a choice set. The typical manifestation of this phenomenon exposes experimental subjects to one of two hypothetical choice sets: either a small choice set containing two options X and Y that differ in terms of quality and price (where Y is of better quality and therefore more expensive), or a larger choice set that also contains a third option Z of even better quality and thus even higher price. A compromise effect occurs if the addition of alternative Z *increases* the share selecting option Y, which violates the regularity condition (Luce, 1959).

Behavior reminiscent of a compromise effect has also been shown in contexts where people can choose among a continuum of options in addition to three (or more) salient alternatives. In particular, Haggag and Paci (2014) find that a larger share of taxi customers tip 25% when it is the middle option of three suggested tip percentages (20%, 25%, 30%), compared to when it is the highest suggested alternative (15%, 20%, 25%). The tendency to avoid extreme options has, furthermore, been proposed to affect decisions ranging from the demand for wine (Mc-Fadden, 1999) to voting (Herne, 1997) and investing (Benartzi and Thaler, 2002). However, the compromise effect has never been evaluated in a natural context using random assignment (Pinger, Ruhmer-Krell, and Schumacher, 2016).

Although the compromise effect violates the regularity condition, both Wernerfelt (1995) and Kamenica (2008) show, theoretically, that the compromise effect can be reconciled with standard utility theory, under the assumption that people can infer pay-off relevant information from the observed choice set.[1] In these models of contextual inference, the increased propen-

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[1]See Simonson and Tversky (1992) for a model based on prospect theory.

sity to choose the middle option arises because some people are uncertain about their absolute preferences, but know that they have intermediate tastes within the relevant population. A subtle prediction of these models is that people presented with only two suggested alternatives, in addition to an open choice alternative, will use the open choice alternative to select an individual compromise. The basic idea is the following: if the two suggested alternatives are interpreted as end points of the relevant distribution, the individual can use the open choice alternative to find the appropriate position given his or her relative taste (assuming a negligible transaction cost of using the open choice alternative and a realistic prior about the shape of the distribution). Hence, the prediction implies a compromise effect without an explicitly stated compromise option—the (un)compromise effect.

In the current paper, I put the compromise effect and the (un)compromise effect to a real test. In a series of mail solicitation campaigns by a large hospital I randomly assign potential donors to receive one of three different solicitation letters that only differ in the set of suggested donations (referred to as ask strings in the charity community). Importantly, in all three cases there is an unspecified option, also called the open-ask alternative, which can be used to fill in a donation not represented in the ask string. The baseline ask string is [$10, $50, $100, $___]. To evaluate the existence of a compromise effect I compare the baseline ask string to an extended version of it, namely [$10, $50, $100, $250, $500, $___]. Notably, the only difference between the two ask strings is the addition of two suggested donations in the extended ask string that are larger than $100. Hence, the $100 alternative has been transformed from the highest suggested donation to the middle (compromise) option, and should therefore *increase* in popularity, if the compromise effect is real. To test the (un)compromise effect, I included an ask string that only contains the range of the extended ask string [$10, $500, $___]. Hence, if people are unsure of how much to give but know that their preferences are middlebrow, they can use the open-ask alternative to fill in an appropriate amount in the range of $10-$500. If, in addition, they expect that $100 is the center of the distribution, donation behavior should be similar across the two treatment conditions. It should be noted, that the prediction of the (un)compromise effect, does not stand unchallenged. The existence of suggested donations could be motivated by reducing the transaction cost of writing in an amount, and there is research indicating that this cost may be real (Reiley and Samek, 2017). In accordance with this alternative hypothesis, an *ex ante* worry on behalf of the hospital was that many potential donors, presented with the novel ask string, would simply give $10 or decide not to give at all.

The study makes three particularly noteworthy contributions to decision research, and to the field of choice architecture more specifically. To the best of my knowledge, the study provides the first experimental test of the compromise effect in a naturally occurring setting using random assignment. As such, it offers clean and compelling evidence to practicing choice architects about the importance of carefully selecting middle options in menus. Second, I propose the (un)compromise effect as a new type of *nudge* (Thaler and Sunstein, 2008). The essence of the nudge is to only make salient the choices that fall within the lower and upper tail of the relevant distribution. Unlike most other nudges, highlighting the two extreme points is not intended to increase the attractiveness of these alternatives *per se*. Quite the opposite, they only serve to indicate which choices are in the right ballpark. As such, the (un)compromise effect can be useful in situations where the choice architect wants to convey some information to decision makers (e.g., to reduce choice avoidance, Iyengar and Lepper, 2000) without excessively steering people to one single alternative, which, for example, a default option or highlighting the majority's choice typically does. In fact, by pushing people away from pre-suggested alternatives and towards active choice, the (un)compromise effect should also side-step some of the critique directed against nudging (Sunstein, 2015; Reisch and Sunstein, 2016). A third contribution is that the results will shed light on whether the impact of suggested alternatives is due to transaction costs inherent in the choice process or cognitive costs of figuring out the optimal choice.

All three contributions have broad implications for the study of choice architecture in organizations. Whether intended or not, the choice environment in just about any organization will tend to favor certain forms of decision-making over others—for better or worse. The present investigation of the (un)compromise effect explores whether a small change in the set of salient choice alternatives can stimulate a more active form of decision-making. This should be relevant both for decision processes internal to the organization, as well as in external interactions with customers and other organizations.

2   Theoretical Background and Research Questions

The theoretical foundation of the current investigation is based on the premise that choice sets can provide pay-off relevant information, and thus affect choice itself. Sen (1993) discussed this idea informally, while Wernerfelt (1995) was the first to write down a formal model, which

he refers to as the rank-order decision rule. As the name suggests, the rank-order decision rule implies that people make decisions based on their expected position in the distribution of tastes, rather than on their absolute tastes (which are unknown). Thus, if the choice set conveys information about the position of the distribution, consumers' ignorance of their absolute, and knowledge of their relative, tastes can lead to a compromise effect. Kamenica (2008) develops the general framework further, and shows that the main findings are sustainable also when choice sets are determined endogenously by profit maximizing firms. The key parameter in Kamenica's model is a global preference parameter, s, which is known to the firm but unknown to (a fraction of) consumers. One interpretation of s is that it transforms technical units of product quality (e.g., megapixels in a camera) to hedonic units of utility. In the model, a compromise effect occurs if consumers use the set of product offerings to update their beliefs about the relationship between technical units and hedonic utility, i.e., the value of the parameter s.

In the above models, choice sets are discrete. However, the general idea can be applied to situations where the choice set is continuous, but a subset of alternatives is made particularly salient (for example, highlighted on a display). If people use the salient alternatives to make inference about the distribution of tastes, they can make an optimal choice by only knowing their relative position within the same distribution of tastes. The important difference, compared to a discrete choice setting, is that people may not necessarily select one of the highlighted alternatives, but rather any alternative that falls within the relevant part of the distribution. In such a decision environment, I postulate that the number and values of the salient alternatives affect the inference people make about the location of the distribution. To be concrete, a single highlighted alternative is likely to be interpreted as the center of the distribution (i.e., the mean, median or modal choice); two sufficiently different alternatives are likely to be interpreted as the empirical minimum and maximum of the distribution; whereas three or more highlighted alternatives provide both the center and the range of the empirical distribution.

Based on this assertion and the models of contextual inference I pose two research questions:

1. Will an extension of the range of salient alternatives lead to a shift in the distribution of choices?

2. How is choice affected if only the range, i.e. the minimum and maximum, of the salient alternatives is provided?

I explore the research questions in the context of charitable giving. Within the charity sector, there is a common practice to highlight one or more particular choices of monetary gifts, in so-called ask strings, while the real choice space is any non-negative number. If my assertion is correct, potential donors use the amounts provided in the ask string to identify the distribution of tastes for the particular charity. Unlike the typical context for testing the compromise effect, however, the trade-off is between money to yourself or money to the charity (not between price and quality of different products). Making this trade-off is far from obvious, and there should therefore be scope for uncertainty about both the individual's absolute preferences, and exactly where the population of tastes lies.[2] With reference to Wernefelts's model, the ask string provides information about the distribution of gifts, which enables potential donors to apply the rank-order decision rule. With reference to Kamenica's model, the amount given to the charity would represent the technical unit, whereas s captures how much utility the donor obtains from a gift of a certain size. Clearly, the individual's utility may depend positively on what other people give and (or) the quality of the charity (Vesterlund, 2003; Smith, Windmeijer, and Wright, 2015). If the amounts in the ask string signal either (or both) of these traits, adding higher amounts in the ask string should push donations upwards. What will happen to donations when the middle option(s) of an ask string is removed, is more difficult to predict. If the suggested alternatives mainly work by reducing the transaction cost of the gift—by allowing the donor to check a box instead of writing an amount—we should see a drastic reduction in the likelihood to give. If the suggested alternatives mainly work by providing information about the distribution of gifts, however, we should predict similar choice outcomes as long as people have correct beliefs about the shape of the distribution, and the non-displayed middle alternative(s) correctly represents the center of the distribution.

In summary, the present investigation was designed to test the following two research hypotheses. First, extending the upper-end of the ask string will push donations upwards, and mainly to the new higher middle option—the compromise effect. Second, for a given range of the ask string, including the middle options is irrelevant in terms of affecting donations—the (un)compromise effect.

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[2]For example, a large literature finds that small changes in the framing of dictator games can drastically affect distributional choices (Dana, Cain, and Dawes, 2006; Dana, Weber, and Kuang, 2007; List, 2007; Bardsley, 2008; Cappelen, Halvorsen, Sørensen, and Tungodden, 2017; Capraro and Rand, 2018).

3   The Experiment

To test the hypotheses, I conducted a field experiment in partnership with a large American hospital, located in Chicago. In a typical year, the hospital runs two mail fund raisers, one appeal in the spring and one in the fall. The experiment involved sending a total of 63,494 donation letters, to potential donors, spread over the spring and fall appeal in 2016. To secure independence across observations, people contacted in the fall appeal had not been approached in the spring appeal.

Within each appeal I randomly assigned recipients to receive one of three different solicitation letters, that only differed with respect to the ask string on the reply card. The control group was exposed to the baseline ask string employed by the hospital, which highlights the three most common gifts based on historical data: $10, $50 and $100. Thus, the control group represents a standard of comparison that is ecologically valid. The first treatment group (T1) was exposed to an extended version of the baseline ask string, which also included the amounts $250 and $500. Hence, the $100 alternative has been transformed from the highest suggested donation to the middle (compromise) option. The choice of the new upper limit in the ask string was based on historical data, since $500 corresponded to the 90th percentile, whereas $10 represented the 10th percentile. The second treatment group (T2) was exposed to an ask string that only highlighted $10 and $500—i.e., there was not an explicitly stated compromise option. Table 1 showcases the three different ask strings used in the experiment and Figure 3 in the Appendix illustrates an actual reply card.

Table 1: Ask strings

	Ask 1	Ask 2	Ask 3	Ask 4	Ask 5	Ask 6
Control	$10	$50	$100			$____
T1	$10	$50	$100	$250	$500	$____
T2	$10				$500	$____

Notes: The headings (i.e., Ask 1, etc.) were not included in the letters. In addition, the relevant ask options appeared, one after the other, without gaps. The gaps in the table only reflect that some suggested amounts were not included in the particular ask string.

Table 2 reports the distribution of subjects across appeals and treatments. It should be noted that more people were solicited in the spring, accounting for roughly 80 percent of the total sample. Furthermore, subjects in the spring round were randomly assigned to treatment

Table 2: Number of individuals solicited by appeal and treatment

	Spring	Fall	Total
Control	17,197	2,398	19,595
T1	17,203	4,749	21,952
T2	17,196	4,751	21,947
Total	51,596	11,898	63,494

Notes: In the spring appeal, subjects were randomly assigned to treatment with equal probability. In the fall appeal, 20% were randomly assigned to the control group and the remaining subjects evenly between T1 and T2.

conditions with equal probability. In the fall, however, I wanted to gain power in the T1 vs. T2 comparison, and therefore allocated relatively more subjects to these treatments compared to the control group. Since people were still randomly assigned to treatment conditions, this choice has no consequences for how to interpret the treatment effects.

4   Descriptive Statistics

Before turning to the results of the experiment I provide some descriptive information of the sample.

Table 3: Background variables (sample means)

	Control	T1	T2	Overall	p(F-test)
Female (%)	41.3	42.1	42.4	41.9	0.558
Birth year	1955	1955	1955	1955	0.620
Last gift ($)	89.2	84.7	86.4	86.7	0.778
Previous donor (%)	6.9	6.8	6.9	6.8	0.986
N	19,595	21,952	21,947	63,494	

Notes: Information about the variables Female, Birth year, and Last gift are only available for a subset of subjects. The last column reports the p-value from a joint F-test.

Table 3 gives an overview of four background characteristics of the solicitees. The first thing to note is that, as expected by random assignment, the background variables balance across treatments (see *p-values* from a joint F-test in the last column). In particular, we note that an individual's last gift (by previous donors) is statistically the same in all three groups ($87

on average). Since the table only reports the means, it is worth pointing out that the median amount of the last gift is $50, which corresponds to the middle option in the baseline ask string. We also note that 40 percent of the sample are female and that solicitees, on average, are 61 years old (S.D. = 14). The oldest person in the sample is 108 years old and the youngest is 35. Finally, we note that seven percent of the sample (about 4,300 individuals) have given before and are therefore defined as previous donors.

Table 4: Outcome variables

	Sum	Mean	S.D.	Min	Max	*N*
Donor (1/0)	241	.004	.0615	0	1	63,494
Gift ($)	79,794	1.257	70.113	0	10000	63,494
Give $100 (1/0)	61	.001	.031	0	1	63,494
Give > $100 (1/0)	69	.001	.033	0	1	63,494
Gift not in T1 (1/0)	82	.001	.036	0	1	63,494

Notes: Gift not in T1 equals 1 if amount given is neither $10, $50, $100, $250, nor $500.

Table 4 presents descriptive information about the outcome variables for the overall sample.[3] We note that the fund raiser collected a total of $79,794 from 241 unique donors. The average gift conditional on giving is thus $331, whereas the unconditional gift (reported in the table) is $1.26. One may be concerned about the cost-benefit of these appeals, but bear in mind that 80 percent of all letters were sent via email, so the marginal cost is close to zero.[4] There is a large spread in gift size, indicated by a standard deviation of 70.11, and the largest amount given is in fact $10,000. The existence of outliers may raise concerns regarding the estimation strategy with respect to the amount given, but it should not affect the more important outcome variable, namely the share giving $100.
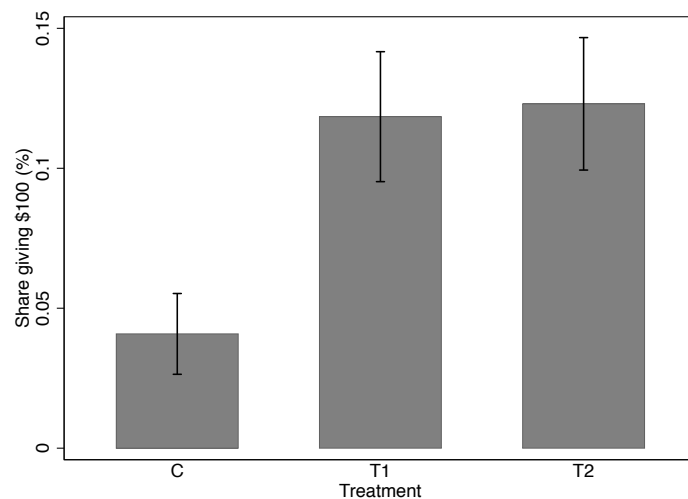
5 Results

Figure 1 reports the share of donors giving $100 by treatment, and thereby provides a first indication of the results. If the amounts in the ask string were irrelevant, which a model based

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[3]A unique code was added to the reply card to keep track of gifts in the experiment. The final date of data collection was January 31, 2017, but most gifts were received within the first 30 days after the letters were sent.

[4]Although emails accounted for 80 percent of the letters sent, they only generated 20 percent of the donations (48 out of 241).

on absolute preferences would predict, the share giving $100 should be the same across treatments. Consistent with a compromise effect, however, the share giving $100 is almost three times higher in T1 relative to the control group. In sharp contrast, the difference between T1 and T2 is miniscule. Evidently, people choose the compromise option to the same extent, even when it is not explicitly stated—the (un)compromise effect. Figure 2 provides additional insights by showing the distribution of non-zero gifts. What is particularly striking in Figure 2, is that the modal gift in the control group is $50, which coincides with the middle option in that ask string, but $100 in both T1 and T2. Except for this pattern, the distributions are quite similar across treatments, in particular there seems to be little effect on giving $500 (or more). Comparing the distributions of T1 and T2, it is again difficult to detect any differences.

Notes: C = ($10, $50, $100, $___); T1 = ($10, $50, $100, $250, $500, $___); T2 = ($10, $500, $___). Error bars indicate one standard error of the mean.

Figure 1: Share giving $100 by treatment

Table 5 reports results from OLS regressions of the treatment effect on five different outcome variables, and thus provides a convenient summary of tests of statistical significance. Starting with column 1, we confirm that the increase in the share of donors giving $100, in both T1 and T2, is statistically significant in pairwise comparisons with the control group. However, the treatment effects of T1 and T2 are not statistically different from each other (p=0.890). In other words, the data support the existence of both a compromise effect, and an (un)compromise effect. In the second column we look at the share of gifts larger than $100. Although the point estimates for both T1 and T2 are positive, they are remarkably smaller relative to the base-
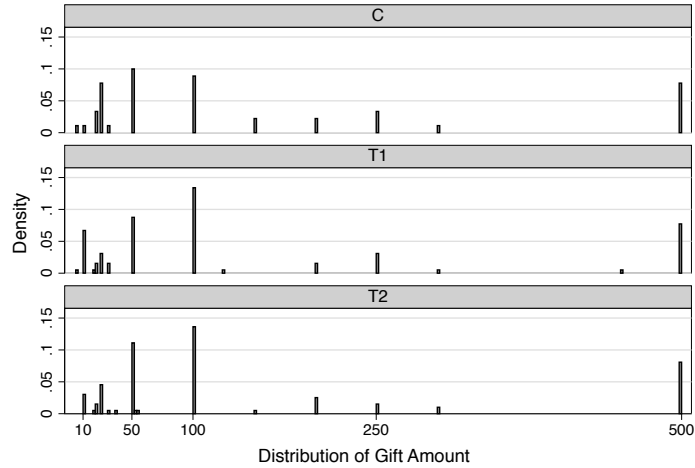
Figure 2: Distribution of gifts by treatment

line category (in comparison to the share giving exactly $100) and not statistically significant. Hence, the increase in the share giving $100 is generated by shifting low-end donors upwards in the distribution, not by shifting high-end donors downwards.

The effect on the amount given is reported in the third column. We note that the point estimates of both T1 and T2 are positive and large. A possible concern with the OLS estimates for this outcome variable is the highly skewed distribution and the existence of outliers. Non-parametric tests do, however, confirm an increase in the amount given in T1 and T2 relative to the control group [Mann-Whitney test: T1 vs. C (*p=0.0002*); T2 vs. C (*p=0.0001*)]. Also for the amount given, the difference is not significant when comparing T1 and T2 [t-test (*p=0.290*); Mann-Whitney test (*p=0.883*]. Hence, the effects we observe are not driven by outliers or hinge on the distributional assumptions of t-tests. Column 4 reports the effect on the decision to donate or not. It is noteworthy that the effect is positive for both T1 and T2. Hence, the baseline ask string turns donors away, not the other way around. This effect is consistent with people preferring to give $100 (or more), but rather choose not to give at all if $100 can be interpreted as an extreme gift in the particular context.[5] In column 5, we look at the share of

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[5]This observation may at first seem at odds with a compromise effect (why did these "missing" donors in the baseline condition not give $50 instead?). However, as long as there are bounds on the uncertainty of an individual's preferences, there will be people that rather opt-out than give $50 when their ideal option ($100) is the highest suggested amount. Consistent with this explanation, Dhar and Simonson (2003) show that the

Table 5: OLS regression results

	Gift = $100 (%)	Gift > $100 (%)	Gift ($)	Donor (%)	Gift ∉ T1 (%)
			Outcome variable		
T1	0.0776***	0.0465	0.621	0.212***	0.0164
	(0.0273)	(0.0308)	(0.396)	(0.0563)	(0.0325)
T2	0.0822***	0.0465	1.453**	0.221***	0.0620*
	(0.000277)	(0.0308)	(0.732)	(0.0567)	(0.0356)
Constant (C)	0.0408***	0.0766***	0.540***	0.230***	0.102***
	(0.0144)	(0.0198)	(0.194)	(0.0342)	(0.0228)
P: T1=T2	0.890	0.999	0.290	0.885	0.203
P: T1=T2=0	0.000	0.199	0.018	0.000	0.177
P: T1+T2=0	0.001	0.073	0.058	0.000	0.209
N	63,494	63,494	63,494	63,494	63,494

Notes: The table reports the unconditional treatment effects using OLS regression analysis. Gift ∉ T1 equals 1 if amount given is neither $10, $50, $100, $250, nor $500. Robust standard errors in parentheses. C = ($10, $50, $100, $___); T1 = ($10, $50, $100, $250, $500, $___); T2 = ($10, $500, $___)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

donors that donate an amount *not* represented in the extended ask string, irrespective of which treatment group the person belongs to. This informs us to which extent people give "unusual" amounts that may be more optimal from the individual's point of view. A positive estimate implies an increase in donations not represented in the extended ask string (i.e., increasing the variety in the amount given), whereas a negative estimate implies more gifts that are represented in the extended ask string (i.e., compressing the variety in the amount given). As expected, the point estimate of T1 is close to zero and not statistically significant. In contrast, the point estimate of T2 implies a 60 percent, marginally significant, increase in the share of "unusual" amounts. This indicates that the ask string containing only the range is superior in terms of preserving individuality (note, however, that I do not have sufficient statistical power to reject that the point estimates of T1 and T2 are identical). Finally, it is worth pointing out that the share using the open-ask alternative to write in the amount varies from 30 percent (T1) to 90 percent (T2), with the control ask string right in between at 60 percent.

In a supplementary analysis (see Figure 4, 5 and 6 in the appendix) I split the treatment effects by appeal (spring or fall), type of contact (mail or email), and type of donor (previous or new). The analysis reveals that the treatment effect is completely independent of these factors. That is, both the compromise and the (un)compromise effect are present both in the spring and the fall appeal, for both previous and new donors, and regardless of whether the letter was sent by mail or email. These findings give support to the robustness of the overall pattern.

6   Concluding Discussion

The current study uses a clean experimental design to provide the first test of the compromise effect in a field setting. I also explore a novel nudge that highlights two extreme options, in addition to an open-form write-in category. The key insights of the experiment are that the compromise effect replicates in the field, and that an explicitly stated compromise option is irrelevant if people can find their own compromise. The results are consistent with models of contextual inference (Wernerfelt, 1995; Kamenica, 2008), where people are uncertain about their true preferences, but know their relative position in the distribution of tastes (e.g., mid-dlebrow). Thus, in a setting where people can choose between a variety of options, providing information about the end points of the distribution is sufficient for the compromise effect to

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compromise effect is weakened when it is possible to opt-out.

occur—I refer to this as the (un)compromise effect. Since the novel ask string, which only contained two unpopular alternatives, increased the fraction of donors relative to the baseline ask string, the experiment also points to why presenting suggested options, in general, stimulates action (Cialdini and Schroeder, 1976; Shang and Croson, 2009; De Bruyn and Prokopec, 2013; Edwards and List, 2014). It is not about reducing the transaction cost of making an active choice (i.e., checking a box instead of writing an amount), it is about reducing the cognitive cost of figuring out what actions are appropriate.

Although the novel ask string and the extended ask string raised equal amounts of money, the large shift in the share of donors writing in the amount they wished to give, coupled with the greater variety in the amounts given, could be important from a business and policy perspective. To be concrete, research in both economics and psychology has found that people place an intrinsic value on making choices (e.g., Bown, Read, and Summers, 2003; Bartling, Fehr, and Herz, 2014). That is, people prefer a particular outcome when it is determined by their own choice over the exact same outcome determined exogenously. Furthermore, it has been shown that affirmatively making a decision increases a decision maker's satisfaction (Botti and McGill, 2006) and commitment (Cioffi and Garner, 1996) above and beyond what the same decision maker would exhibit if the decision was made passively. In addition, it has been shown that people's valuation of a product is affected by their level of involvement in the production process—referred to as the IKEA-effect (Norton, Mochon, and Ariely, 2012). Interestingly, there is also research indicating that the act of writing things down enhances people's memory (Di Vesta and Gray, 1972; Mueller and Oppenheimer, 2014). In light of these findings, it is plausible that writing down the amount, as opposed to checking a box, makes the gift more rewarding and memorable.[6] Hence, the novel ask string could potentially boost long-term commitment and revenues, relative to the extended ask string.

Turning to the policy perspective, it is argued that nudging (i.e., affecting people's behavior via choice architecture) is considered more ethically problematic if decisions are steered towards pre-suggested alternatives (Sunstein, 2015; Reisch and Sunstein, 2016). The (un)compromise effect may offer a remedy to this problem by influencing the choice process towards active choice, and away from pre-suggested alternatives. The idea behind presenting two extreme suggested alternatives is, in fact, similar in spirit to the idea of setting a clearly

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[6]Clearly, people who simply checked one of the suggested alternatives also made an active choice, but my argument is that writing in the amount is likely to make the decision maker feel (even) more involved.

inferior option as the default, which in theory should make status-quo bias more costly and thereby stimulate active choice (Choi, Laibson, Madrian, and Metrick, 2003).

Understanding the generality of the (un)compromise effect, is still an open question, but I see at least two potential applications of relevance in the organizational context. The first relates to contribution rates in savings schemes (including 401(k) plans). There is wide agreement that saving for retirement is important, but exactly how much is optimal to save differs enormously between citizens and among employees within an organization. Clearly, a default savings rate may generate uniformity and thus be potentially harmful (Madrian and Shea, 2001; Altmann, Falk, Heidhues, and Jayaraman, 2014); at the same time can completely open choice, or an abundance of suggested saving rates, lead to choice avoidance (Iyengar and Lepper, 2000; Cronqvist and Thaler, 2004; Bertrand, Karlan, Mullainathan, Shafir, and Zinman, 2010). Based on the current study, a potential solution is to provide two suggested savings rates (e.g. 3 and 20 percent) in addition to a write-in alternative. Such a choice set could provide relevant information about the distribution of savings rates in the population, from which individuals could infer their ideal option. The second application is energy conservation. It has been shown that providing information about the average electricity consumption in the neighbourhood reduces households' usage of energy (Allcott, 2011). The decline in the average household's energy use does, however, disguise that households that consumed relatively low levels of energy to begin with actually increase their energy usage in response to the information—a phenomenon referred to as the boomerang effect (Schultz, Nolan, Cialdini, Goldstein, and Griskevicius, 2007). Given this adverse behavior, a potentially more effective approach is to provide customers with information about usage levels of the highest, respectively lowest, energy consumers in the neighbourhood. Presumably, the competitive spirit would then encourage all households, including those already below average consumption, to reduce their energy usage even further. This prediction is at least partly consistent with the results of the current experiment, since the (un)compromise ask string pushed donations in the lower part of the distribution upwards, while donations in the upper part of the distribution remained unaffected.

Finally, it should be emphasized that more research is needed to understand the promises and perils of adopting the (un)compromise nudge. For example, how sensitive is the effect to uncertainty about the shape of the underlying distribution? What are the pros and cons of increasing (or decreasing) the distance between the two extreme options? And how does the nudge fare in comparison to a symmetric information set with exorbitant middle options? These

are important questions that should be addressed in future research.

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7 Appendix

Yes, I (we) want to support the Rehabilitation Institute of Chicago. Please accept my gift of:
☐ $10 ☐ $50 ☐ $100 ☐ $250 ☐ $500 ☐ Other:_____

☐ Please make this a recurring gift. Each month I would like to give _____.

This gift is:
☐ In memory of:_____ ☐ In honor of: _____

☐ Yes, this gift can be matched by my employer and I've enclosed my matching gift form.
☐ I/We wish for this gift to be anonymous.

☐ My check is enclosed. Please make it payable to the Rehabilitation Institute of Chicago.

Please bill my: ☐ Visa®   ☐ MasterCard®   ☐ American Express®   ☐ Discover®

Name:_____

Card No.:_____ Exp. Date:_____

Signature:_____

☐ **I/We would like to receive news and updates from RIC.**
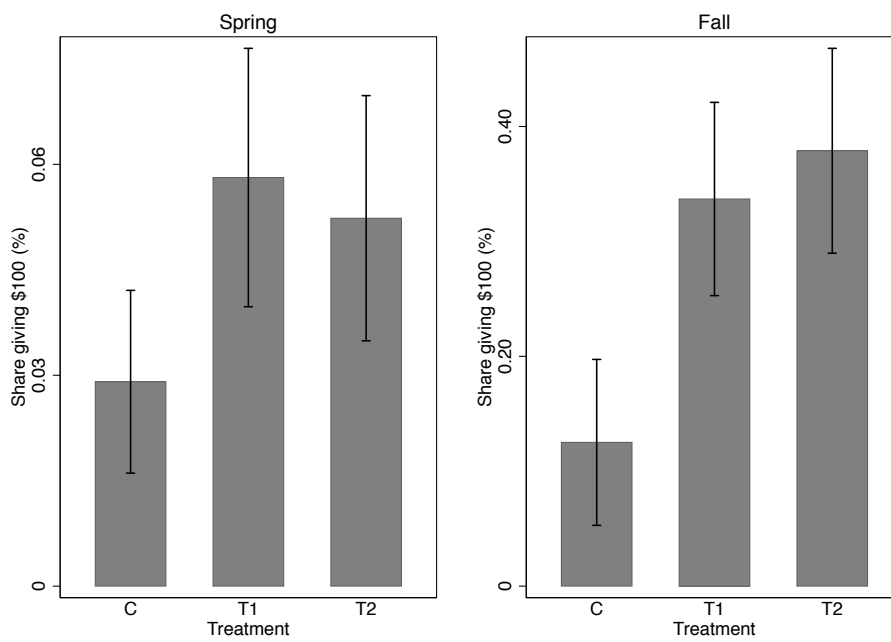☐ I/We do not wish to receive mailings from RIC.

Email address:_____

To make your donation online, please visit donate.ric.org/givenow.
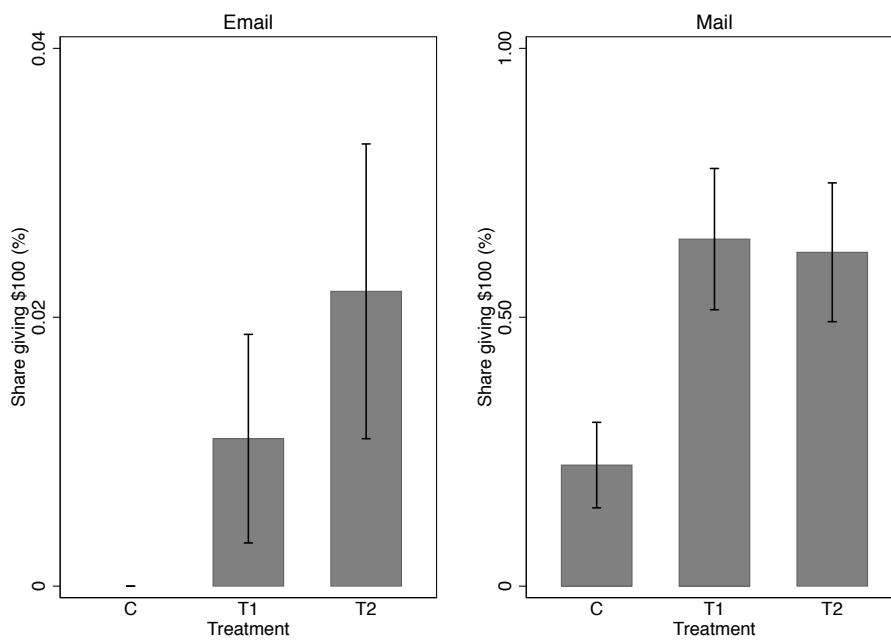Please return this form with your tax deductible gift. Thank you!

<ID> <Apeal Code>

Figure 3: Example of solicitation letter

Notes: To improve clarity the y-axis is scaled to reflect giving behavior in the respective control group.
C = ($10, $50, $100, $___); T1 = ($10, $50, $100, $250, $500, $___); T2 = ($10, $500, $___). Error bars indicate one standard error of the mean.
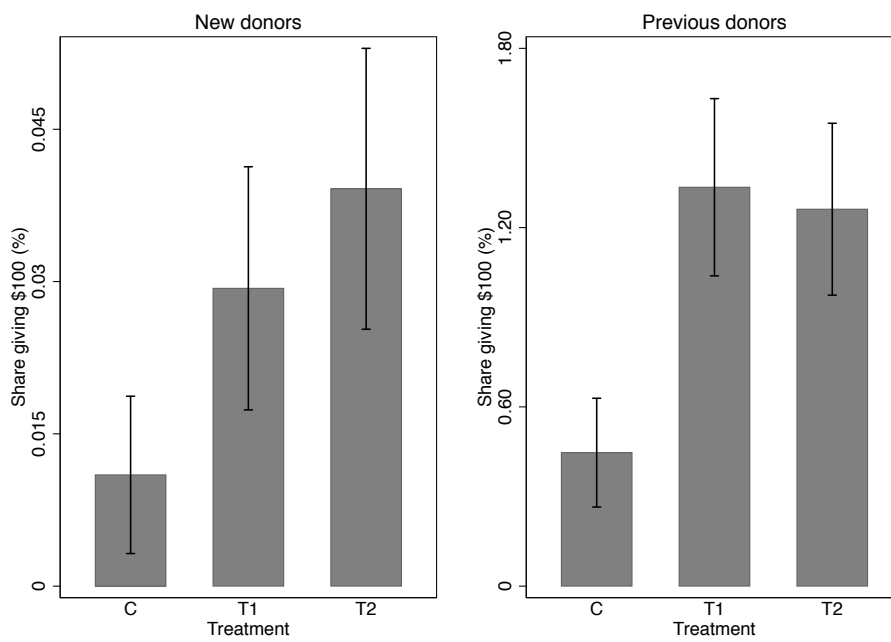
Figure 4: Treatment effect by appeal

Notes: To improve clarity the y-axis is scaled to reflect giving behavior in the respective control group.

C = ($10, $50, $100, $___); T1 = ($10, $50, $100, $250, $500, $___); T2 = ($10, $500, $___). Error bars indicate one standard error of the mean.

Figure 5: Treatment effect by type of contact

Notes: To improve clarity the y-axis is scaled to reflect giving behavior in the respective control group.

C = ($10, $50, $100, $___); T1 = ($10, $50, $100, $250, $500, $___); T2 = ($10, $500, $___). Error bars indicate one standard error of the mean.

Figure 6: Treatment effect by type of donor

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