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Dark defaults: How choice architecture steers political campaign donations

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In the months before the 2020 U.S. election, several political campaign websites added prechecked boxes (defaults), automatically making all donations into recurring weekly contributions unless donors unchecked them. Since these changes occurred at different times for different campaigns, we use a staggered difference-in-differences design to measure the causal effects of defaults on donors' behavior. We estimate that defaults increased campaign donations by over $43 million while increasing requested refunds by almost $3 million. The weekly default only impacted weekly recurring donations, and not other donations, suggesting that donors may not have intended to make weekly donations. The longer defaults were displayed, the more money campaigns raised through weekly donations. Donors did not compensate by changing the amount they donated. We found that the default had a larger impact on smaller donors and on donors who had no prior experience with defaults, causing them to start more chains and donate a larger proportion of their money through weekly recurring donations.

elections | campaign finance | campaign donations | default effects | choice architecture

Significance

When presenting choices to people, designers sometimes precheck one option such as a shipping speed, choice to be an organ donor, or retirement savings contribution size, to nudge them toward choosing that option. We examine the effects of prechecking a recurring donation box on eight political campaign websites, showing that these prechecked boxes increase campaign donations by over 40 million dollars and increase donors' requests for refunds. Our results suggest, in contrast to previous work, that defaults can sometimes cause people to make decisions by accident that they may later regret. Recently, policymakers in many countries have considered or implemented bans on prechecked boxes in some contexts, including in this specific political donations context.

Donations to political campaigns influence election outcomes and policy. Many citizens, including millions of small donors, donate to candidates through their campaigns' websites. In the 2020 election cycle, online contributions to ActBlue, a conduit used by Democrats, totaled $4.3 billion, while donations to WinRed, a conduit used by Republicans, totaled $2.2 billion (1). These conduits collect, report, and process funds for parties, political action committees, and individual campaigns. In the United States, conduits are required by law to report every contribution to the Federal Election Commission and make these data available online (1). We focus on how the design, or choice architecture, of campaign websites affects donations through these conduits. This setting has several advantages: It is large (we examine 14.8 million donations by 2.6 million donors), it is consequential (the average donor contributes $278), and it allows us to look at how the influence of choice architecture on donations changes over time.

During the general election campaign in 2020, several candidates' websites (all Republican) changed the choice architecture of their donation pages. In the summer of 2020, donors using the websites encountered a prechecked box that caused their contributions to repeat every month unless they unchecked it. Throughout September and October 2020, about half of these large political campaigns made a key change to their websites–the checkbox caused donations to repeat every week instead of every month (Fig. 1) (2). We label these "dark defaults." Dark defaults are subtle changes to a website's default options that users may overlook, causing them to make choices they do not intend. While we do not contact donors and do not directly observe whether they meant to make a donation, we use data on which donations were refunded to infer whether dark defaults cause some donors to give by mistake.

Using data that WinRed reported to the Federal Election Commission, we identify the effect of the weekly default on donations. We focus on whether donors started chains (weekly repeats of donations) and how much these donations contributed to the campaigns. Since campaigns report donors' names and addresses, we can identify which donors made recurring weekly donations (*SI Appendix*, section 1.3). Overall, campaigns in our sample received $730.7 million from August 1, 2020, through November 3, 2020, and refunded $63.1 million of the donations (see *SI Appendix*, Table S2 for additional details). We combine these data with historical captures of campaign websites from the Internet Archive Wayback Machine (https://www.archive.org) to identify when the prechecked boxes were added. The Internet Archive does not have data for every single day of the period under investigation, though larger campaigns have more data. Thus, for smaller campaigns, there is some uncertainty regarding which day the choice architecture change occurred; the largest range of uncertainty is 2 wk. For these uncertain implementation

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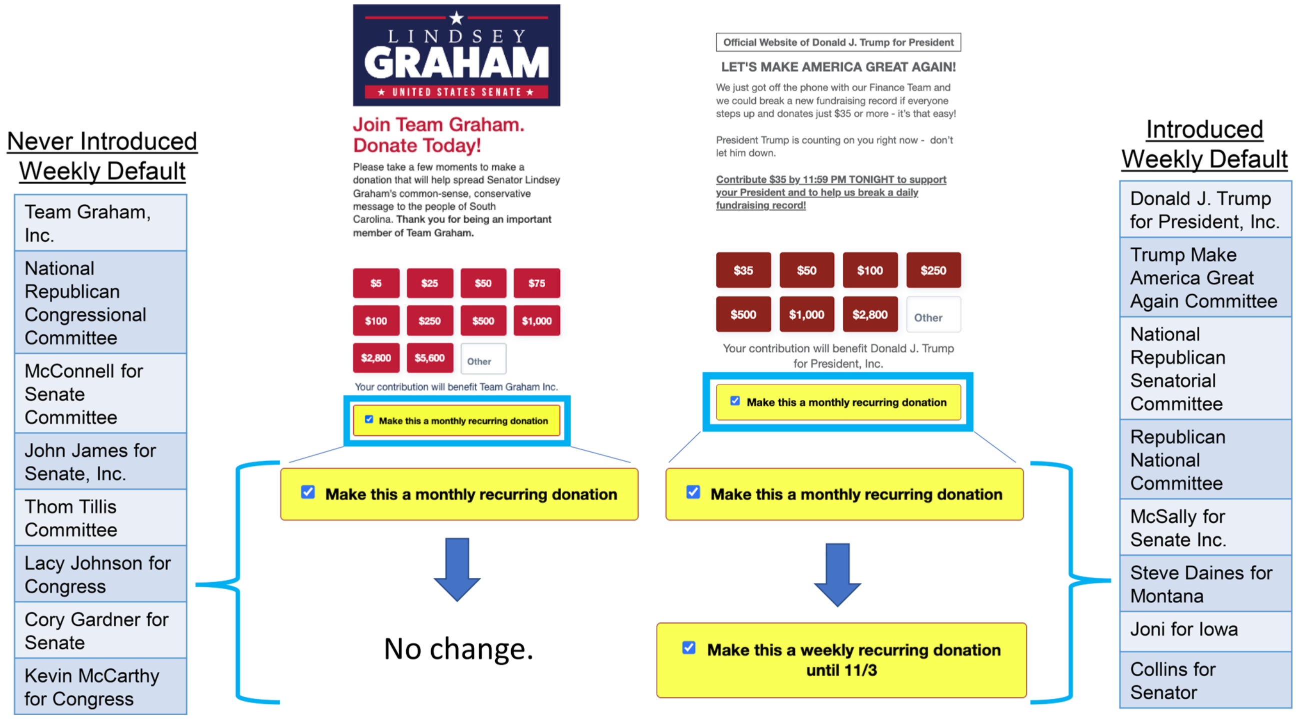

Fig. 1. The donation interface before and after the prechecked weekly recurring donation box was added.

dates, we coded the first possible day that the weekly default could have been added as the day it was added, a conservative assumption. Our sample consists of 16 of the top 20 campaigns by number of donations; we excluded four campaigns because two of them were runoff elections and occurred in a different time period, and two did not have enough data available on the Internet Archive.

Fig. 1 shows examples of the change in the campaign websites that added weekly defaults (*Right*) and those that did not (*Left*). Eight of the 16 campaign websites made this change in their choice architecture (adding prechecked boxes) that made any donations weekly by default, while the other eight did not (*SI Appendix*, Table S1). Together with the staggered timing of checkboxes in other campaigns, this variation helps us identify the effects of choice architecture on donations.

We examine four questions: 1) How large is the default effect? 2) How does the default effect change behavior over time? 3) Are donors aware that they are choosing to make weekly donations? 4) Do defaults impact some types of donors more than others?

To answer these questions, we use an event study comparing donor behavior in campaigns that added a weekly default to those that did not add a weekly default. We use a two-way fixed-effects model to control for variation in campaign donation behavior that does not vary over time as well as for independent day-to-day variation in donation behavior (*SI Appendix*, section 2.1). Our estimates are robust to multiple estimators (*SI Appendix*, section 2.4.3) and are robust when controlling for each campaign's win probability (*SI Appendix*, section 2.4.2).

How large is the default effect? There has been controversy about the impact of choice architecture. Despite some evidence that changes to the default option have large effects on behavior (school programs, energy choice, agreement to be an organ donor) (3–5) there is substantial variability in the effect size of choice architecture interventions in different contexts, including defaults (6–11). Here, we investigate the impact of a choice architecture change on consequential, real-world behavior and find a substantial effect.

The data show a marked increase in weekly donations due to the weekly default. Fig. 2*A* shows the amount donated each day to all campaigns. One week after the weekly default was added (shaded portion), the money from weekly donations (green area) started to increase in the campaigns that added weekly defaults (Panel *A*: *Right*) but not in the campaigns that did not (Panel *A*: *Left*). In the campaigns that added the weekly default, the percentage of donations that started chains of weekly recurring donations increased roughly threefold—from 2.8% the week before it was added to 10.1% the week after it was added (an additional 35,421 chains; Fig. 2*B*[*]). This effect persisted until the election, $t = 4.32$, $P < 0.001$. Treated campaigns received \$63.3 million in weekly repeating donations over the course of the election. Our estimates suggest that \$43.5 million was due to the weekly default (Fig. 2*C*), $t = 6.62$, $P < 0.001$. This is 10.6% of the \$411.2 million the treated campaigns received during the period after they added the checkbox.

How does the default effect on weekly donations change over time? We examine how the default effect varies over time. Defaults often have large effects on decisions that are made once and that have long-term outcomes (5, 12). Giving people multiple opportunities to opt out of defaults can diminish their effectiveness (13, 14). Donors may adapt to the addition of the weekly default by either lowering the size of the donations or by terminating their chains before election day. Finally, donors might lower the amount they donate by reducing the number or size of donations they make outside of the weekly chains.

We find no evidence that donors decrease the size of donations when they start weekly chains. The size of the average initiating donation actually increases as the election approaches, but we do not find evidence that this is because of the weekly default ($P > 0.30$; *SI Appendix*, section 3.1). In addition, we find little evidence that donors reduced the number or size of non-chain donations they made (*SI Appendix*, sections 3.2 and 3.3). On average, 56.5% of weekly donation chains persist until election

[*]Note that the increase appears twofold in the figure because we flattened the baseline number of chains for the figures. See *SI Appendix*, section 4.1.5 for figure construction details and *SI Appendix*, section 4.1 for raw regression results.

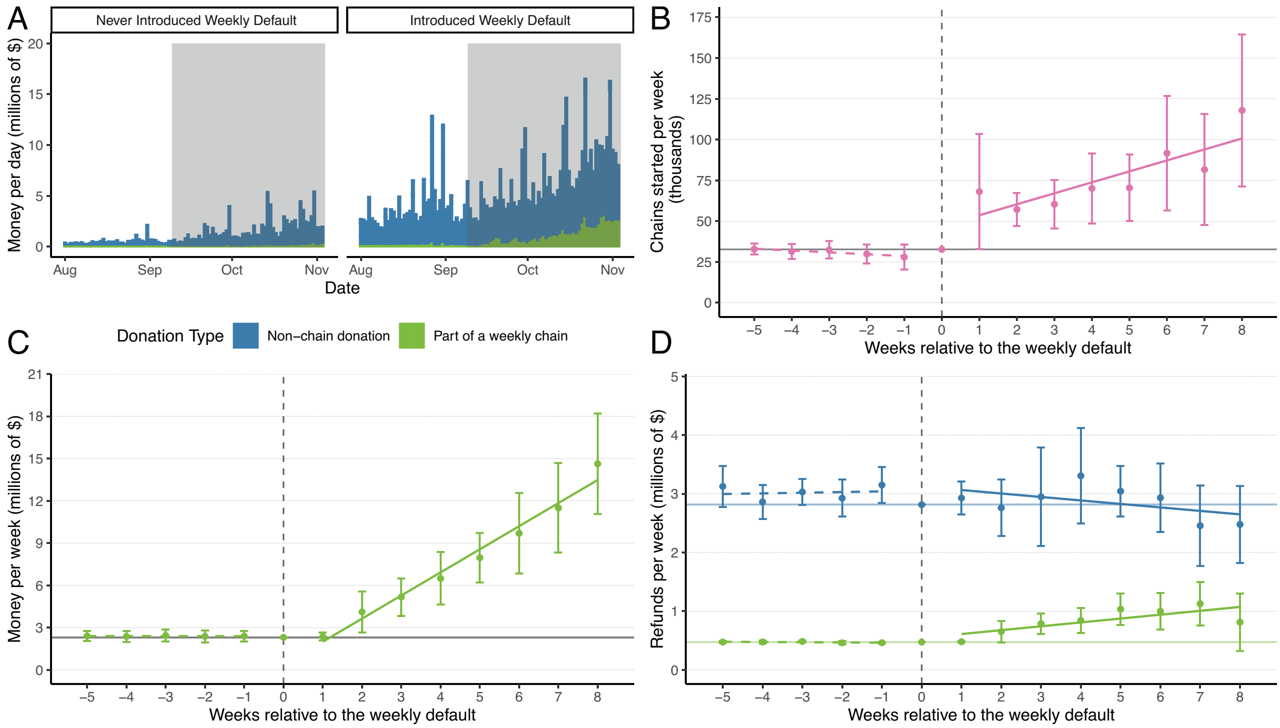

Fig. 2. Responses to default change. (*A*) Campaigns added weekly defaults in the shaded period in panel *A*. All other panels are modeled results. (*B*–*D*) The *X* axis is weeks before and after the treated campaigns added weekly default (vertical dotted line), normalized across campaigns. Panel *B* is the results of Model 1 (*Materials and Methods*) multiplied by the total number of non-chain donations to treatment campaigns. Panel *C* is the results of Model 2 multiplied by the total amount of money from donations to the treatment campaigns. Panel *D* is the results of Model 3 (*Bottom*) and Model 4 (*Top*) multiplied by total weekly donations and non-chain donations to the treatment campaigns, respectively. See *SI Appendix*, section 4.1.5 for details.

day (the date the chains expire). The remaining 43.5% of chains are either misidentified in the data or were canceled by the donors in some way (*SI Appendix*, section 1.3.2). These results suggest that, though donors did not adjust their donation behavior when starting chains, some may have canceled donation chains partway through. The majority, however, persist until the election.

Do donors accidentally start weekly chains? The Federal Election Commission data include reports of refunded donations. Although donors can request refunds for many reasons, we expect some donors to be more likely to request refunds when they donate inadvertently. Most previous research examines situations where decision-makers are aware of how a default they select will affect them (14, 15). In our data, the switch to a weekly default was subtle (Fig. 1). It is possible that donors might not have noticed at the time that they were opting into weekly recurring donations.

The weekly default caused donors to disproportionately request refunds of the donations they made as part of weekly chains but did not affect their propensity to request refunds of non-chain donations. We estimate that the weekly default significantly increased donors' propensity to request donations of weekly recurring donations, causing them to receive an additional \$2.9 million in refunds than they would have received otherwise, $t = 3.21$, $P < 0.002$, (Fig. 2 *D*, *Bottom*). In contrast, the weekly default did not affect refunds of non-chain donations, $P = 0.69$ (Fig. 2 *D*, *Top*). This stark difference in the effect of the weekly default on received refunds of chain and non-chain donations suggests that some weekly donations were made by accident, likely because the weekly default checkbox was small and easy to miss. The inattention mechanism is supported by the anecdotal reports that some donors did not notice that they were opting into recurring donations (2), and pilot data from a lab experiment suggesting that about 50% of people who encountered a similar prechecked box that caused them to make a purchase did not notice that they did so (*SI Appendix*, section 3.9). Some donors may have noticed that they started a chain from an email from the payment processor (2). However, the \$2.9 million in additional refunds is a small fraction of the \$43 million attributable to the weekly default, suggesting either that not all weekly donations were made by mistake or that donors who gave by mistake did not know or chose not to use the refund option.

Does the default effect differ across donors? Researchers have started asking whether defaults and other interventions have heterogeneous effects (16–19). We compare small donors (those who donate less than \$200 in an election cycle) to large donors (those who donate more than \$200) (1).[†] We find that the weekly default causes small donors to start more chains than large donors ($t = 5.05$, $P < 0.001$; Fig. 3*A*) and pay proportionally more in weekly donations ($t = 2.77$, $P < 0.01$; Fig. 3*B*) than large donors.

We also examine the effect of prior experience with starting donation chains on propensity to start a new chain due to the weekly default. The weekly default consistently causes donors who have not started a chain before to start such chains. In contrast, the weekly default effect diminishes over time for donors who have previously started at least 1 weekly chain. By week 4, it is not significantly different from 0 for experienced chain-starters, whereas it had a large consistent effect among donors who had never started a chain before (67% of chains were started by inexperienced donors; *SI Appendix*, section 3.4). Thus, we find some evidence that donors learn to uncheck the weekly default after starting a chain previously.

We imputed income, age, and education from donors' zip codes and census data and imputed donors' genders from their first names

[†] We operationalize "small donors" and "large donors" slightly differently from past work (1) due to our focus on the effect of defaults on the volume of donations. See *SI Appendix*, section 1.3.4 for details on how we identify small donors.

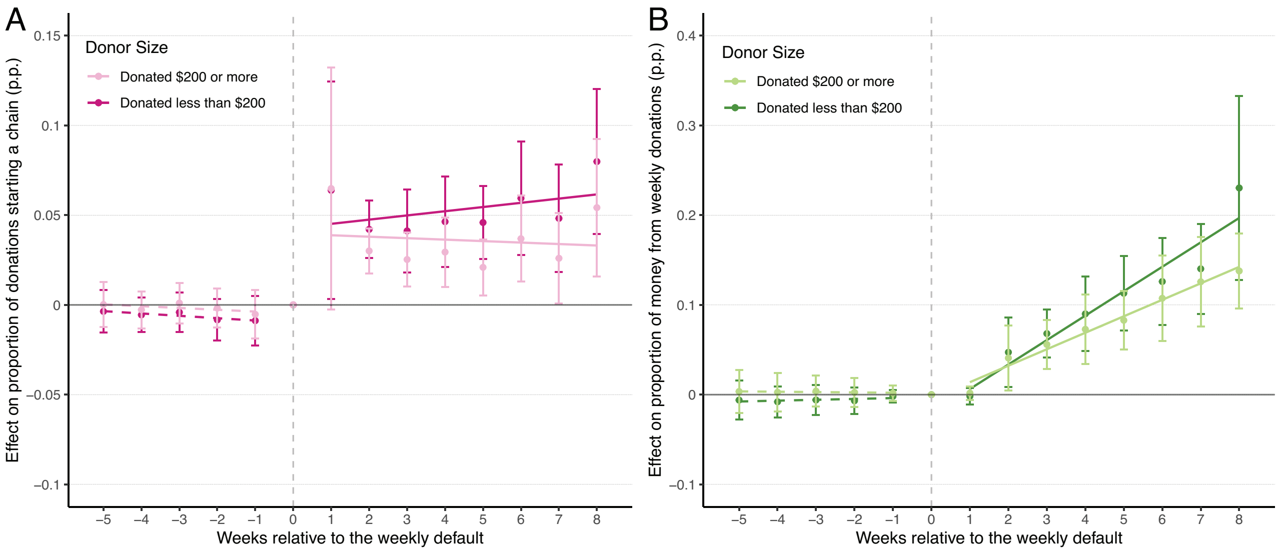

Fig. 3. Heterogeneity of weekly default. The weekly default caused small donors to start relatively more chains (*A*) and a higher proportion of their money in weekly donations (*B*) than large donors. Figures were constructed using model outputs of Model 5 and Model 6. See *SI Appendix*, sections 2.1.1 and 4.2 for details.

(*SI Appendix*, section 1.3.6). We found no significant interactions between income, age, education, or gender and the effect of weekly default. However, this null result could be because the income, age, and education data we used from the American Community Survey were not sensitive-enough measures of individual-level demographic differences (*SI Appendix*, section 3.5).

Discussion

Political campaigns are locked in an arms race, increasingly developing new ways to raise money. We document a simple intervention that contributed over $43 million (11% of total donations received) to the eight campaigns that used it. Changing the default is easy, requiring only an edit to a website's code, but doing so produces a marked increase in weekly donations without a corresponding decrease in nonweekly donations. We find that the weekly default increases refunds of weekly donations, but not of non-chain donations, but that refunds represent a small proportion of the increase. These effects are heterogeneous; smaller donors and those who did not have experience with a default in the past were more impacted by it.

The term dark defaults borrows from two similar, more general terms: "dark patterns" and "dark nudges." In computer science, dark patterns are deceptive design decisions that can cause users to make unintended choices (20). In behavioral science, dark nudges are interventions that nudge people toward behaviors that they do not intend or are not in their best interest (e.g., refs. 21 and 22). We draw from these terms to suggest that some of the donors in our data were deceived by the defaults into making choices they did not intend. We do not directly observe the intent of the donors in the data, but we do show that people did not change how much they donated outside weekly chains (*SI Appendix*, section 3.2), were more likely to request refunds, often continued donating even after the election (*SI Appendix*, section 3.6), and did not reduce the size of their donations after the default was changed (*SI Appendix*, sections 3.1 and 3.3). Each of these patterns would be expected if many people who left the weekly donation box checked did not initially realize they did so. The default effect on amount donated through weekly chains is more pronounced among small and inexperienced donors, who may have less familiarity with the donation interfaces campaigns use. Taken together, these findings suggest that defaults may cause some users to make decisions they would not have otherwise, perhaps because they do not notice they are making that choice. These dark defaults do not account for the entire default effect, but they are a key component of the default effect that can, in some cases, cause users to make choices they do not intend.

Recently, policymakers in many countries have considered or implemented bans on prechecked boxes in certain contexts, presumably because some defaults can be deceptive. In the United States, the bipartisan Federal Election Commission unanimously recommended to Congress a ban on prechecked political donation buttons, 1 mo after the defaults we examined were documented by the New York Times. Bans have been implemented on prechecked boxes for cookie consent in the European Union, and these were recently upheld by the Court of Justice of the European Union (23). More generally, policymakers have explored limitations on dark patterns that might trick and harm unwitting consumers. This trend to protect consumers from defaults contrasts with prevailing theories suggesting that defaults are usually nondeceptive, ethically acceptable, and effective because of people's conscious choices to save effort or infer that the default option is best (15, 23). Our findings also diverge from recent claims that choice architecture has little to no effect or that effects go away after people learn to quickly adapt.

However, defaults often nudge people who are aware of them in ways they view as beneficial and ethically acceptable (24). Future research should seek to better document precursors that determine when defaults will be most likely to go unnoticed and cause harm. Defaults are sometimes helpful nudges and sometimes harmful tricks; they should be used selectively and wisely.

Materials and Methods

We examine the effect of prechecked boxes that automatically opt donors into recurring weekly donations (weekly defaults) on donations to candidates for office in the 2020 United States general election. We use variation in adoption timing of these weekly defaults by political campaigns to estimate causal effects using a staggered difference-in-differences design. See *SI Appendix* for extended methods, robustness checks, and regression results. Data and code are available at https://osf.io/2r9fz/ (25).

Data. We obtain data from four sources: WinRed data (both donations and refunds) from Propublica's FEC Itemizer (26), data on the design of each campaign's website from https://www.Archive.org (27), income, education, and age information from the American Community Survey (28), and poll-based predictions of each campaign's win probability over time from Fivethirtyeight.com (29). We examine the period of August 1, 2020, through election day–November 3, 2020.

Campaign Information. During the August 1 to November 3 (election day) period of the 2020 election cycle, 8 of the 16 campaigns we examined added a checkbox to their donation websites that automatically opted donors into weekly recurring payments unless it was manually unchecked with a click (see Fig. 1, main text, for an example before/after). Before they added this weekly default, they all had prechecked boxes that opted donors into monthly (instead of weekly) recurring donations. We used these campaigns as the treatment. The other eight campaigns either had a prechecked or non-prechecked box opting donors into monthly repeating donations throughout the entire election period. We used these campaigns as the control. See *SI Appendix*, Table S1 for summaries of the campaigns, including the first date they could have had a weekly default based on the https://www.Archive.org data.

Identification Strategy. We estimate the impact of the weekly default on donation behavior. We use staggered adoption of the weekly default to estimate the overall effect across many different campaigns. We aim to estimate the proportion of donations that start chains, the proportion of money that comes from chains, and the proportion of money that is refunded. Once we estimate these values, we can multiply them by overall donations to estimate the monetary impact of the weekly defaults.

A key assumption is that the timing of the switch from a monthly default to a weekly default is exogenous to committee-specific variation in donation behavior. Though it is possible that campaigns added the checkbox due to declining donation revenue, changing poll numbers, or some other factor, we argue that the exact timing of the switch from a monthly to a weekly checkbox was plausibly exogenous to our outcomes of interest (see *SI Appendix*, section 2.2.1 for additional discussion).

We have three main outcomes of interest: 1) the proportion of non-chain donations that start weekly chains, 2) the proportion of money that comes from weekly chains, and 3) the proportion of money that is refunded. We also examine the average size and number of non-chain donations and the average size of initiating donations. We assume that for our dependent variables of interest, there is no difference between our treatment and control campaigns before the weekly default is added (*SI Appendix*, section 2.2.2).

Variable Construction. Our empirical strategy relies on separating out donations that are induced by a weekly default from donations people make through other means.

Though the Federal Election Commission data include information such as the name and address of each donor that made each donation, it does not include donor identification numbers. To determine whether a donation came from a chain of multiple recurring donations, we first identify all donations made by one donor. We identify donors by their first name, last name, address, and zip code. We assign donors that have identical first names, last names, addresses, and zip codes a donor identification number. This technique is similar to other methods used to identify donors in Federal Election Commission data (e.g., ref. 30). See *SI Appendix*, section 1.3.1 for additional discussion of this method.

After assigning donor ID numbers, we look for donations of identical amounts that occurred exactly 7 d apart. This means that we may misidentify donations of equal amounts that a donor makes exactly 7 d apart as part of a weekly chain. In addition, a donor will have to enter their first name, last name, address, and zip code identically (i.e., identical initials, periods, capitalization, etc.). Thus, we assume that this rate of misidentification is roughly equivalent between campaigns that do not have weekly defaults and those that do. In addition, we identify donations that are part of monthly chains. Once we classify donations as weekly, monthly, or non-chain, we identify the donation that started the chain and count the length of the chain.

We label the first donation in the chain as an initiating donation. Initiating donations are not counted as part of chains since these donations are made by donors consciously coming to the campaign's website and choosing to make a donation. Thus, initiating donations are not included when we calculate the amount of money that campaigns get from chains of recurring donations. In addition, we calculate the length of each chain.

We match refunds to donations to determine whether donors may have regretted their donations, whether because they made them by mistake, or for some other reason. Our refunds data indicate who requested each refund, the value of each donation refunded, and the date of the refund (not of the original donation). Thus, though we cannot identify which donations were refunded specifically (since if a donor made multiple donations to multiple candidates of the same amount, there would be no way to discern which they refunded), we know how many refunds donors requested and the size of those refunds. We can identify which donations donors refunded by matching them by the donor's information (first name, last name, address, and zip code) and by the donation amount. This may cause us to overestimate how much is refunded if a donor were to make repeated donations of identical amounts but only refund one of them. To solve this, we weight each refund by the number of times it appears in the refunds data divided by the number of donations it matches in the donations data.

We identify small donors as donors who had donated less than \$200 total in the 2020 election cycle (starting the day after the 2018 election) before September 8 and large donors who had donated \$200 or more by this point. We looked only at donations before the campaigns added checkboxes because status as a small or large donor might be caused by the effects of the weekly default. Our analysis of heterogeneity (*SI Appendix*, sections 4.2.1 and 4.2.2) was run only on those approximately 1.5 million donors who donated before September 8th.

To look at learning, we divide donors into those who previously started weekly chains and those who did not. We identify the point at which donors start their first chain. Before they have started that chain, we classify them as never having started a chain before. After they start their first chain, we classify them as having previously started a chain. This measure can give insight into learning–after donors start their first chain, they can be assumed to be more likely to have experienced the effect of the weekly default (*SI Appendix*, section 3.4).

We impute individual-level gender using the *gender* R package (31). The package uses historical data to estimate someone's gender using their first name. To predict donors' gender, we run the function "gender," limiting the historical data to births in 2002 since people under 18 are not allowed to donate to political campaigns in the United States. Following the package's guidelines, we assign donors with a greater than 50% probability of being male as "male," and those with a greater than 50% probability of being female as "female" (*SI Appendix*, section 1.3.6).

Empirical Strategy. We aggregate the dependent variable of interest at the committee-day level. If the particular model had heterogeneity, we aggregate it at the committee-day-heterogeneity variable level. For example, to examine the impact of the checkbox of the proportion of donations that started weekly chains, we group the data by donation type, committee, and day. Then, we calculate the amount of money each committee received of each donation type and then calculate the proportion of each committees' donations that come from each source. We then run our models on these aggregated committee-day datasets. Our datasets contained 95 d (August 1, 2020 to November 3, 2020) and 16 committees. We limited our analysis to the 5 wk prior to any campaign adding its weekly default. In addition, heterogeneity analysis on donor gender, income, age, and education, as well as robustness checks, were run at the individual level (*SI Appendix*, section 1.4.1).

We use an event study design to examine outcomes of interest. We compare donor behavior in campaigns that added a weekly default to donor behavior in campaigns that did not add a weekly default. Since we do not have Internet Archive data for every day for every campaign, there is some uncertainty as to the precise day when some campaigns added their weekly defaults. We treat the first day a campaign could have added a weekly default (i.e., the first day in which we do not have data that the campaign does not use a weekly default) as the day the campaign added a weekly default. To make causal claims about the impact of prechecked checkboxes on behavior, we assume that trends in donation behavior for treated and control campaigns would have remained the same had the checkboxes not been added to their websites. We use a two-way fixed-effects model with an OLS estimator to control for variation in campaign-level, time-invariant donation behavior, as well as for daily donation behavior independent of campaign-level variation. Since we observe campaign website design changes on the day level, we use campaign-by-day variation to examine the impact of website design on donation behavior. We use two-way clustering of SEs on the day and campaign level. We used the fixest R package version 0.10.4 to do all analyses.

We run our main analyses on donation and refund data from August 1, 2020, to November 3, 2020. See *SI Appendix*, section 1.4 for details on how we construct these datasets.

In our main analyses (those reported in Fig. 2), we use the following model specification:

$$DV_{it} = \alpha_i + \gamma_t + \sum_{\tau=-5,\ \tau\neq 0}^{8} \beta_\tau WeeklyDefault_{i,(t/7)-\tau} + \varepsilon_{it}.$$

The dependent variable (abbreviated *DV in the model)* is different depending on the analysis. Model 1 (Fig. 2*B*) examines the proportion of non-chain donations that started weekly chains. Model 2 (Fig. 2*C*) examines the proportion of money that came from weekly chains. Model 3 and Model 4 (Fig. 2 *D*, *Bottom and Top*, respectively) examine the proportion of money from weekly donations that was refunded and the proportion of money from non-chain donations that was refunded, respectively (regression results in *SI Appendix*, section 4.1). $WeeklyDefault_{i,(t/7)-}\tau$ is a set of indicator variables for weeks relative to the addition of the weekly default for each campaign. For campaigns that never add the weekly default, this variable is 0 for all days. $\beta\tau$ captures the impact of the weekly default on each *dependent variable*. Note that the underlying data are at the day level, but we estimate $\beta\tau$ at the week level for legibility. α_i and γ_t are fixed effects controlling for committee-level and day-level factors, respectively.

To investigate heterogeneity by donor size (Fig. 3*B*), we use the following model:

$$DV_{itd} = \alpha_i + \gamma_t + k_d + \sum_{\tau=-5,\ \tau\neq 0}^{8} \beta_{\tau d} WeeklyDefault_{i,(t/7)-\tau} + \varepsilon_{itd}.$$

For these analyses, $\beta\tau_d$ is the estimator of the weekly default on the *DV* for relative week τ. We estimate a separate set of $\beta\tau$ for small donors (d = 0) and for large donors (d = 1). Model 5 (Fig. 3*A*) examines the proportion of donations coming from weekly chains, while Model 6 (Fig. 3*B*) examines the proportion of money coming from weekly chains (regression results in *SI Appendix*, section 4.2).

Inference. We two-way cluster SEs at the day and committee level. We do this because shocks can be correlated at either the committee level or the day level. Our main model is a traditional two-way fixed effects estimation. We show that our results are robust to excluding committees related to the Presidential campaign (*SI Appendix*, section 2.4.1), to controlling for different campaigns' win probabilities (*SI Appendix*, section 2.4.2), and to using alternative estimators designed to account for bias introduced by treatment effect heterogeneity (32–34; *SI Appendix*, section 2.4.3).

Data, Materials, and Software Availability. Day-by-campaign data have been deposited in Dark Defaults (DOI: 10.17605/OSF.IO/2R9FZ) (25). Previously published data were used for this work (26).

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