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Opt In? Opt Out?

Alex Chan[1]
Ayush Gupta[2]
Yetong Xu[3]

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[1]Harvard University & NBER
[2]University of Mannheim
[3]Harvard University

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[1]Harvard University & NBER

[2]University of Mannheim

[3]Harvard University

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Abstract

Cadaveric organ shortages leave thousands without life-saving transplants each year. Countries differ in using opt-in (informed consent) or opt-out (presumed consent) systems for donor registration. Using newly assembled cross-country panel data and an event-study design, this paper shows that presumed-consent laws are associated with increased organ donation only when strictly enforced and family veto power is limited; weak opt-out regimes show negligible or even negative changes. A theoretical signaling model provides a plausible mechanism that determines when opt-in or opt-out yields more donations, emphasizing the roles of donation propensity, signaling costs, and the family's ability to overturn defaults. A large laboratory experiment further tests this mechanism, showing that opt-in generally produces equal or higher donation rates unless signaling is costly and family veto power is minimal. The results underscore that defaults alone rarely increase donations unless paired with strong institutional enforcement.[†]

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1 Introduction

Human organs are critical medical resources in shortage. Over 100,000 Americans are waiting for an organ transplant and over 5,500 died waiting for an organ in 2022. Tens of thousands more die without getting a needed transplantation due to the lack of access around the world.

The lion's share (70%+) of transplanted organs in the United States comes from deceased organ donors. Various proposals have been made to improve incentives (Chan and Roth (2024), Elías et al. (2019), Elias et al. (2015)) and the organ donor registration system (Kessler and Roth (2012), Kessler and Roth (2014), Kessler and Roth (2025)) to increase the supply of organs. Under the principle of informed consent, potential donors in the U.S. have to "opt in" to becoming organ donors while they are alive. Many countries have shifted to a presumed consent system where everyone is presumed to be an organ donor by default unless they "opt out" of being donors. Defaults have been very successful "nudges" (Thaler and Sunstein (2009)), and countries with a presumed consent system have been shown to have dramatically more people who did not opt out than people who opt in within informed consent countries (Johnson and Goldstein (2003)). This might suggest a switch to an "opt out" system as a way to increase organ donations. However, the differences in actual, deceased organ donation (as opposed to registration) between opt in and opt out countries are far less dramatic (Abadie and Gay (2006), Gimbel et al. (2003), Johnson and Goldstein (2003)).[1] Indeed, a substantial body of comparative work concludes that presumed consent alone is no guarantee of more donations (e.g. Rithalia et al. (2009), Dallacker et al. (2024)). If families of the potential donor can overturn donor consent readily, it is not clear whether switching to opt out will cause actual organ donations to increase. Furthermore, aside from the ability to overturn consent, families or other decision proxies for the deceased can hold very different beliefs regarding the true intentions of the deceased, potential donor. The question of whether opt in or opt out is better has a more nuanced answer.[2]

[1] In the updated "Final Edition," Thaler and Sunstein (2021) thoughtfully revise their earlier support for opt out policies. They offer a nuanced explanation—highly consistent with the core insight developed in the theoretical section of this paper—regarding why a weak opt-out regime is unlikely to generate meaningful increases in actual organ donations. Their discussion anticipates some of the mechanisms formalized here.

[2] A related policy alternative is mandated or "active" choice, which requires individuals to explicitly state whether they wish to donate. While active choice is conceptually distinct from both opt in and opt out, our theoretical framework extends naturally to that setting. We also note that existing evidence

In this paper, we create and utilize a cross-country panel dataset and an event study design. We report new evidence on the impact of presumed consent or "opt out" laws on deceased organ donation rates, and account for the heterogeneity of such impact. The results reveal substantial heterogeneity: while opt out reforms are associated with increased donations in contexts with strict enforcement—where family veto power is limited—switches to weak opt out frameworks often yield negligible or even negative effects. We then theoretically delineate conditions under which opt in frameworks may outperform opt out regimes and vice versa, emphasizing the critical roles of population donation propensity and the authority granted to families in overriding donor registrations. Complementing our theoretical framework, we implement a laboratory experiment modeled on the decision to register as an organ donor and to consent to donation as a proxy decision-maker. This experimental design allows for nuanced investigation into how defaults and family veto powers interact to influence ultimate donation decisions.

This paper makes several contributions. First, using a novel cross-country panel dataset spanning 94 countries across six continents, we provide new motivating evidence on the heterogeneous impact of opt out laws, distinguishing outcomes under strict versus weak enforcement regimes. Second, we develop a parsimonious theoretical model that explicitly incorporates signaling and the role of family decision-makers to explain this heterogeneity. Third, we experimentally test the model's core comparative statics in a controlled laboratory setting, providing robust evidence on the mechanisms at play.

To explain the differential impacts of strict opt out versus weak opt out regimes, we use a stylized two-player signaling game to model the decisions of the potential donor and the family, as well as actual institutional details around deceased organ donation. The model offers predictions on donation rates under different scenarios: opt in or opt out, strict or weak consent, high or low family priors on the willingness to donate on the part of the deceased, and high or low costs of signaling. Although deliberately simple, the model is flexible enough to capture a wide range of institutional settings and individual attitudes. The cost of signaling captures how easily a country lets people register – a box to tick when renewing a license, or a form to be notarized and filed. The cost of overturning captures the legal force of the default and the practices of procurement teams, which is what separates strict from weak opt out. The family's disutility from

(Kessler and Roth (2025)) suggests that active choice is no more effective at increasing registrations than opt in.

authorizing donation captures its own attitudes, whether emotional, moral, or religious. The lab experiment then tests the full set of comparative statics for each of the scenarios against theoretical predictions. These (theoretical and experimental) insights offer, to our knowledge, novel clarification on the conditions under which opt out will outperform opt in for organ donations.

Under either opt in or opt out, potential donors can change the default while they are still alive and express their preference by registering as an organ donor under informed consent or opting out of being one under presumed consent. In the United States, people can opt in to being an organ donor at the Department of Motor Vehicles (DMV) when they are getting their driver's licenses or go online and register as an organ donor with the National Donate Life Registry. At a later time, if a potential donor unfortunately passed away in a manner compatible with organ donation,[3] their next of kin will be consulted to "authorize" an organ donation. The U.S. is an example of a country with "weak consent," such that even if a potential donor has consented, the family is often able to overturn the consent and block donation at low or no cost in practice (Chan (2020)). When presumed consent legislation is applied strictly, i.e. under "strict opt-out," if the deceased individual did not opt out, they are deemed an organ donor, overriding family approval. Some families have taken the issue to court and/or are able to challenge the "strict opt-out," but the costs are far higher. In some other cases, organ recovery might even commence before the families are notified when there is presumed consent under a "strict opt-out" regime. Strict opt-out is increasingly uncommon, if not entirely unused in the organ donation space today.[4]

A key friction arises from the fact that families often serve as default or proxy decision-makers, and their ability to overturn prior consent introduces a wedge between expressed preferences and actual donation outcomes.

Despite widespread public support for organ donation, few people across multiple continents explicitly communicate their donation preferences to family members. In fact, studies from the United States and Europe consistently find that the majority of families approached about organ donation were unaware of their loved ones' wishes or had never

[3]Most eligible deaths are deaths of individuals under 75 who are legally declared brain dead. These deaths are typically caused by traumatic, sudden events like a car accident, gun shot wound, or hypoxia due to drug overdose (Chan and Roth (2024)).

[4]In the U.K., our collaborators at the National Health Services have worked on and helped deploy a new system of layering in an "opt in" on top of a "weak opt-out" regime in the early to mid-2020s. We are exploring the impact of such a mixed system in our collaboration.

discussed the topic beforehand (Schulz et al. (2012), Siminoff and Lawrence (2002), Jacoby and Jaccard (2010)). The awareness is likely even lower for non-Western cultures (Ralph et al. (2016)). One possible explanation for the lack of these conversations is the influence of behavioral factors, including a visceral discomfort or "yuck factor"—a fundamental feeling of disgust or unease associated with thoughts of bodily mutilation or disfigurement after death (Morgan et al. (2008)). Such emotional or subconscious reactions may prevent individuals from openly discussing or even considering organ donation, leaving families ill-prepared to make informed decisions during highly emotional situations. Signals outside of the organ donor registration system seem to not be used in practice by potential donors to express their preference. For many families, the act of opting in (or out) is therefore the only signal of the deceased's wishes they will ever observe – precisely the friction our signaling model is built around.

Our simple signaling model of deceased organ donation captures this dynamic where opting in/out is relied upon as a signal of a potential donor's willingness-to-donate and explains the patterns in the observational data. In the model, a potential donor can pay a cost to explicitly signal their donation preference. Under an opt-in system, willing donors can opt in, credibly signaling their wish to donate and helping families authorize donation. Under opt-out, however, willing donors lack an affirmative mechanism to reinforce their willingness to donate, whereas unwilling donors can pay to opt out. When signals are not too costly, this asymmetry favors opt-in regimes: willing donors have a lever to help families authorize donation, while unwilling donors under opt-out can undermine donations by opting out. Thus, when signaling is cheap, opt-in yields at least as many donations as opt-out.

Conversely, when signaling is very costly, few individuals opt in or out. In this case, family decision-making is guided primarily by priors and the structure of consent. Under strict opt-out regimes, presumed consent laws impose real costs on families who attempt to block donation, shifting the belief threshold toward authorizing donation even in cases of moderate uncertainty. As a result, under strict enforcement and high signaling costs, opt-out regimes can result in more donations.

To empirically test the model's predictions, we design and implement an incentivized laboratory experiment with about 1,000 participants. Subjects play a stylized donation game using a neutral asset ("wug") rather than actual organs. We abstract away from

the emotional and moral connotations associated with real organs to isolate the signaling channel. We do so because changing the default does not, by itself, alter the preferences of potential donors or what donation costs their families. It only changes which preferences can be expressed and what remaining in the default conveys. Participants make decisions both as potential donors (whether to incur a cost to express a preference) and as family proxies (whether to authorize donation), under randomly assigned opt-in or opt-out regimes. The experimental design varies three key parameters: the cost of signaling, the cost of overturning defaults, and the family's prior beliefs about the donor's type.

Signaling games typically admit many equilibria. We emphasize the ones we regard as most plausible and the experiment allows us to verify that our selection describes actual behavior. Under weak consent regimes, opt-in consistently yields weakly higher donation rates than opt-out. Under strict consent regimes, opt-out improves donation rates only when signaling costs are high and family priors about donor willingness are intermediate. In the latter case, the experimental results indicate a significant doubling of donation rates. Otherwise, opt-in remains superior. These findings suggest that default policies interact critically with institutional enforcement and behavioral frictions, and that naive switches to opt-out are unlikely to uniformly improve organ donation outcomes.

Taken together, the paper offers new observational and experimental evidence on an important question of market design: how legal defaults, signaling costs, and proxy decision-making jointly shape high-stakes organ supply outcomes. The results inform ongoing policy debates in the U.S., Europe, and beyond, and illustrate broader principles for the design of consent mechanisms in healthcare and beyond.

The experimental results are summarized in two simple figures (Figures X and XI), each spanning the full parameter space defined by the three key dimensions. This structure allows readers and policymakers to flexibly interpret the results based on their own assumptions about how parameters might shift with a policy change—for instance, if presumed consent laws alter population donation propensities or family beliefs.

Finally, this paper also contributes broadly to the literature on nudges and default-based interventions, showing that a default can be an information structure as well as a nudge. Prior studies, such as Beshears et al. (2024), have demonstrated how the intended effects of defaults can be partially offset by compensating individual behaviors—for instance, automatic pension enrollment leading to increased debt. Our research uncovers

another offsetting dynamic, identifying how defaults can remove opportunities for individuals to explicitly express or signal preferences, thereby reducing information available to decision-makers. When implementation depends on a later decision by an imperfectly informed proxy, a system that records only departures from the default determines which preference types can send an affirmative signal and what inaction conveys. Changing the default can therefore increase recorded participation while making recorded status less diagnostic of underlying preferences, thereby altering the proxy's ultimate choice. This mechanism may arise more generally across a broader class of delegated-choice mechanisms when one individual holds private information about her preferences but another ultimately implements the outcome, as with advance-care planning (Chan et al. (2019)), beneficiary designations, or inheritance rules. Thus, our findings highlight a generalizable caution that defaults should be evaluated not only by their effects on initial take-up, but also by the information they preserve for downstream decisions.

The paper is organized as follows. Section 2 presents the background information on consent regimes and describes the empirical design and results for our observational data. In Sections 3, we describe the model. In Section 4 and 5, we describe the lab experiment and results from the experiment. Section 6 concludes.

2 Presumed Consent, Informed Consent, and Deceased Organ Donation

2.1 Deceased Organ Donation: Importance, Shortages, and Behavioral Frictions

Organ transplantation largely depends on deceased donors (Chan and Roth (2024)). A single donor can save multiple lives, yet organ supply dramatically lags behind demand. Despite widespread public support, actual donor registration remains frustratingly low. Even though surveys show over 85% of Americans support organ donation, only about half have formally registered—perhaps procrastinating due to the grim reality of signing up for something they'll never personally benefit from.

A key reason for the shortage is the limited pool of medically eligible deceased donors—about 35,000–40,000 annually in the U.S.—with fewer than 8,000 actual dona-

tions. Much of this gap arises from authorization hurdles. Without explicit registration, hospitals must rely on grieving family members, about half of whom refuse consent in the U.S. and U.K. (Chan and Sweat (2025)), compared to just 20–30% in countries like Spain or France. Such disparities highlight the role of institutional defaults and cultural norms in donation decisions.

Behavioral barriers further limit donor registrations (Kessler and Roth (2025)). The emotional difficulty of confronting one's own mortality can impose a cognitive cost. While registering as a donor is as easy as checking a box online or at the DMV, the psychological weight often leads to procrastination or avoidance. As a result, family members often make difficult decisions under emotional duress without clear guidance. Understanding these behavioral and institutional frictions is crucial for effectively addressing the shortage.

2.2 Informed Consent vs. Presumed Consent: History, Policy, and Global Debate

Countries differ greatly in how consent for organ donation is structured, reflecting varying legal philosophies and historical developments. Broadly, systems are classified as informed consent (opt-in), where explicit permission is required, or presumed consent (opt-out), where everyone is considered a donor unless they explicitly refuse.

Historically, most of continental Europe adopted presumed consent in the late 20th century, including France, Belgium, and Austria, each with nuanced implementations (Gevers et al. (2004)). In the 1980s, Austria employed a strict presumed consent policy allowing procurement without family approval, whereas Spain uses a "soft" approach, consulting family members despite the default assumption. By contrast, the U.S., U.K., Germany, and others retained informed consent systems emphasizing individual autonomy and explicit registration.

Asia and South America have recently seen shifts as well. Singapore implemented presumed consent in 1987, after which donation rates rose, while Japan maintains an informed consent policy, partly due to cultural sensitivities around death. In South America, Brazil briefly experimented with presumed consent in the late 1990s but reversed the decision after public backlash and widespread distrust (Csillag (1998)). Argentina and Chile adopted presumed consent more recently, resulting in mixed impacts and renewed

public debates (the $N = 1$-observation of how "opt out" worked in South America is well summarized by the title of Domínguez and Rojas (2013): "Presumed consent legislation failed to improve organ donation in Chile").

The U.S. remains firmly committed to explicit opt-in consent, reinforced by first-person consent laws. Under these statutes, registered donors' decisions legally override family objections. While this approach respects autonomy, its effectiveness relies heavily on proactive individual registration. Without it, hospitals must defer to families, frequently resulting in refused consent. (However, even when first-person consent legally applies, US organ procurement teams often defer to families in practice to avoid conflict, negative publicity, or litigation risk, effectively allowing de facto family override.)

Advocates of presumed consent argue it significantly boosts donation rates by leveraging inertia—if people delay paperwork under informed consent, they'll similarly avoid opting out. Indeed, presumed consent countries often experience higher donation rates, as observed in Belgium and Austria following policy shifts in the 1980s (Michielsen (1996), Gnant et al. (1991)). Skeptics caution, however, that correlation doesn't imply causation; presumed consent nations often differ culturally, institutionally, and in health infrastructure. For example, Spain's high donation rate is frequently attributed not only to its opt-out framework but also to its extensive transplant coordination and public awareness campaigns (Matesanz (2001)). In particular, studies that focus on realized donations rather than registrations often find no clear advantage for presumed consent (Rithalia et al. (2009)); and its estimated effect depends heavily on surrounding institutions such as family consent practices and registry design (Bilgel (2012)).

The debate remains unresolved due to primarily observational data. Few natural experiments exist; policy shifts typically coincide with broader institutional changes or public campaigns, complicating clear causal inference (a potential violation of the parallel trends assumption that can plague our design as well). Thus, while presumed consent is sometimes associated with higher donation rates, the extent to which the default setting itself drives these outcomes remains uncertain.

Given the high stakes — thousands of lives annually — there is a critical need for rigorous research. The remainder of this paper responds to that need: we first document cross-country patterns that motivate the analysis, and then develop a theoretical framework and a controlled experiment to isolate how consent policies truly influence organ

donation outcomes, enabling more informed policy decisions worldwide.

2.3 Motivating Evidence from a New Cross-Country Panel

Despite the extensive policy interest in increasing organ donation, relatively few studies have attempted to evaluate the causal effect of consent regime changes on donation rates. A key reason is the scarcity of rich, cross-country panel data that envelops the event of a switch from one consenting regime to another. In particular, longitudinal data tracking the period before and after a legislative switch—especially on a per-death basis—are rarely available at the level needed for empirical identification.

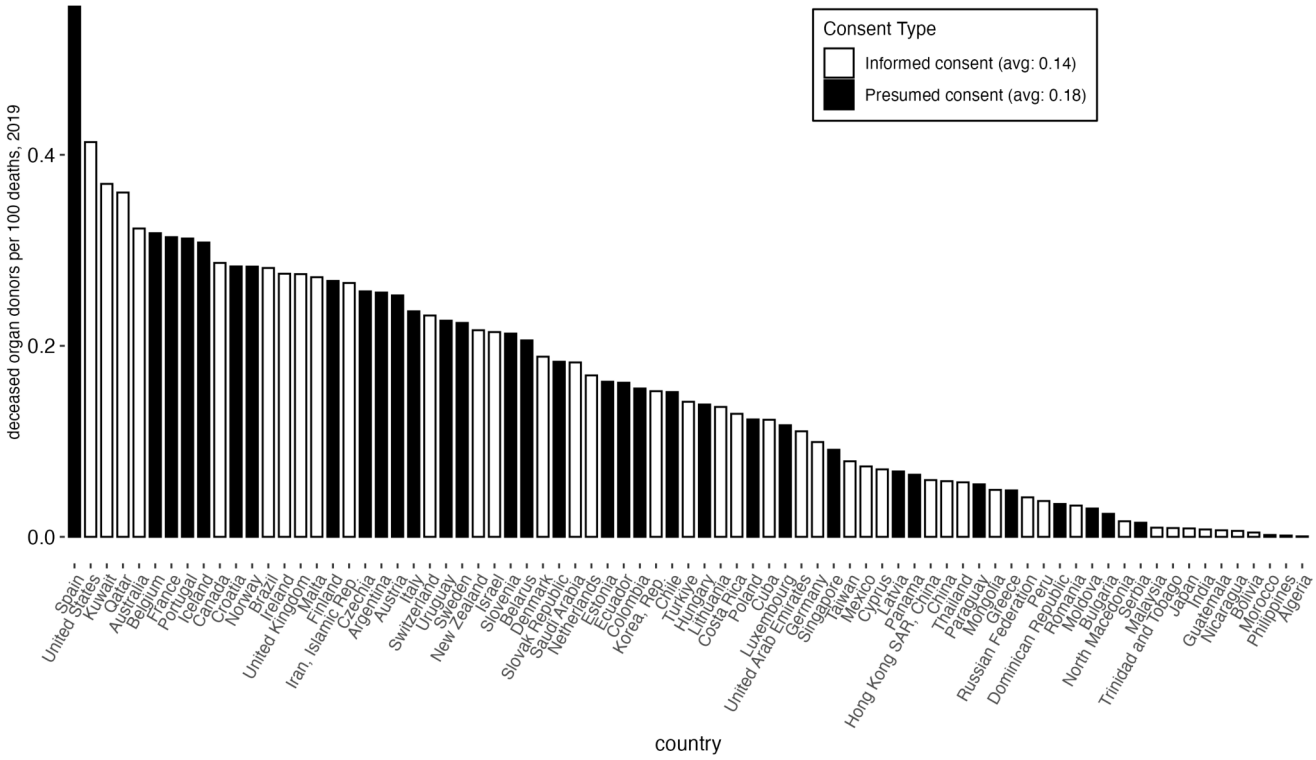
This paper contributes the strongest yet, if suggestive, evidence by assembling a novel panel dataset that spans 94 countries,[5] encompassing six continents and multiple legislative regimes. To our knowledge, it is the richest cross-country panel on deceased organ donation assembled to date: it includes detailed annual information on deceased donation and transplant rates, as well as country-level demographic and health-related controls. Population, death rates, and health expenditure data are sourced primarily from the World Bank's World Development Indicators, while the number of deaths caused by traffic accidents and cerebro-vascular failures are extracted from the World Health Organization Mortality Database, and information on legal systems is drawn from the CIA World Factbook archives and other secondary sources. Data on transplant and donation rates are compiled from a range of sources, including IRODaT, GODT, EuroTransplant, Scandiatransplant, and various national transplant agencies. A full description of the data sources used for each country and variable is provided in the Online Appendix.

Figure I presents the cadaveric donation per 100 deaths in 2019 across the 77 countries in our sample, disaggregated by consent regime. A clear pattern emerges: countries operating under an "opt-out" (presumed consent) regime exhibit (32.69%) higher donation rates per death compared to their "opt-in" counterparts, though the two distributions overlap substantially. Similar patterns emerge if we look at cadaveric donation per million population.[6]

[5] We will refer to a region as a "country" based on what the World Bank Databases label as "country" and not necessarily by current political sovereignty.

[6] In Abadie and Gay (2006), a plot similar to our latter one in the Online Appendix but with fewer and more dated data was presented and was for the year 2002 and 36 countries. Donation per population could be increased by policy measures that have less to do with increased propensity for organ donation (e.g., executing more prisoners), in this paper we focus on deceased donations per 100 deaths.

Figure I: Deceased organ donors per 100 deaths in 2019 across 77 countries in 6 continents



Notes: Death rates are sourced primarily from the World Bank's World Development Indicators, while donation rates are compiled from a range of sources, including IRODaT, GODT, EuroTransplant, Scandiatransplant, and various national transplant agencies.

Table I: The effect of presumed consent legislation on cadaveric organ donation

	Dependent variable: Log cadaveric organ donors per 100 deaths										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Presumed consent	0.238	0.205	0.260	0.287	0.228	0.211	0.267	0.228	0.170	0.224	0.179
	(0.229)	(0.230)	(0.176)	(0.175)	(0.174)	(0.180)	(0.186)	(0.185)	(0.179)	(0.185)	(0.183)
Log GDP per capita			0.707***		0.701***	0.727***	0.687***	0.694***	0.799***	0.764***	0.777***
			(0.146)		(0.180)	(0.206)	(0.190)	(0.189)	(0.205)	(0.191)	(0.188)
Log of health expenditures per capita				0.549***							
				(0.129)							
Catholic country					0.246	0.275			0.276		
					(0.216)	(0.203)			(0.207)		
Common law					0.225	0.251	0.203	0.230	0.341	0.301	0.337
					(0.287)	(0.266)	(0.280)	(0.275)	(0.278)	(0.290)	(0.286)
Log of MVA and CVD deaths (per 1000 population)						0.102	0.043	0.081			
						(0.242)	(0.253)	(0.252)			
Log of MVA deaths (per 1000 population)									0.217	0.220	0.249
									(0.171)	(0.178)	(0.173)
Log of CVD deaths (per 1000 population)									0.085	0.041	0.065
									(0.190)	(0.198)	(0.196)
Include Spain	Yes	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	No
Num. Obs.	851	837	839	825	839	823	823	809	823	823	809
R^2	0.352	0.348	0.559	0.573	0.570	0.562	0.552	0.555	0.570	0.560	0.565

Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: This table reports OLS estimates of the effect of presumed consent legislation on cadaveric organ donation. The estimating equation is $ActualDonation_{ct} = \alpha + \beta\,\text{OptOut}_{ct} + \mathbf{X}'_{ct}\theta + \gamma_r + \delta_t + \varepsilon_{ct}$, where $ActualDonation_{ct}$ is the log of cadaveric organ donors per 100 deaths in country c and year t, OptOut_{ct} is an indicator for whether presumed consent is in effect, $\mathbf{X}_{ct}$ includes time-varying controls such as GDP per capita, health expenditures, mortality rate from motor vehicle accidents (MVA) and cerebro-vascular disease (CVD) and country-level characteristics (Catholic, Common law), γ_r are region fixed effects, and δ_t are year fixed effects. Countries are grouped into eight regions (South America, Oceania, Europe, North America, Central America and the Caribbean, East and Southeast Asia, Middle East, and Africa) following the CIA World Factbook classification. Standard errors are clustered at the country level.

By using data from 62 countries over the period 2006 to 2019, Table I shows the estimates of the effect of presumed consent legislation and other predictors on the deceased organ donations per 100 deaths. Note that, the dependent variable is the natural logarithm of cadaveric donors per 100 deaths. Generally, while considering other predictors, the estimates suggest that countries operating under an "opt-out" (presumed consent) regime have, on average, 20–30% (insignificantly, at conventional levels) higher cadaveric organ donation rates than their "opt-in" counterparts.

To examine how donation rates evolve around actual switches from opt-in to opt-out regimes, we employ an event study framework. Our analysis focuses on countries that changed policy nationally from opt-in to opt-out, specifically Austria (1982), Luxembourg (1982), Finland (1985), Belgium (1986), Sweden (1996), Chile (2010), and Uruguay (2013). For each treatment country experiencing a regime switch, we construct a comparison group made up of all other countries from the same continent that maintained their consent regime throughout the analysis period and for which data are available at least two years before and three years after the policy change in the treatment country; we require each treatment country to have at least three such control countries.

We analyze effects on organ donation with a two-way fixed effects specification, exploiting the staggered adoption of opt out and comparing before and after each country's policy shift. Let Y_{ct} denote the outcome of interest for country c in year t, measured as the deceased donation rate per 100 deaths. Let τ_{ct}^{k} be an event time indicator equal to one if country c is k years from the implementation of the opt-out regime (with $k = 0$ representing the first year of adoption). The estimating equation is:

$$Y_{ct} = \sum_{k \neq -1} \beta_k \cdot \tau_{ct}^{k} + \gamma_c + \delta_t + \mathbf{X}_{ct}'\theta + \varepsilon_{ct}, \tag{1}$$

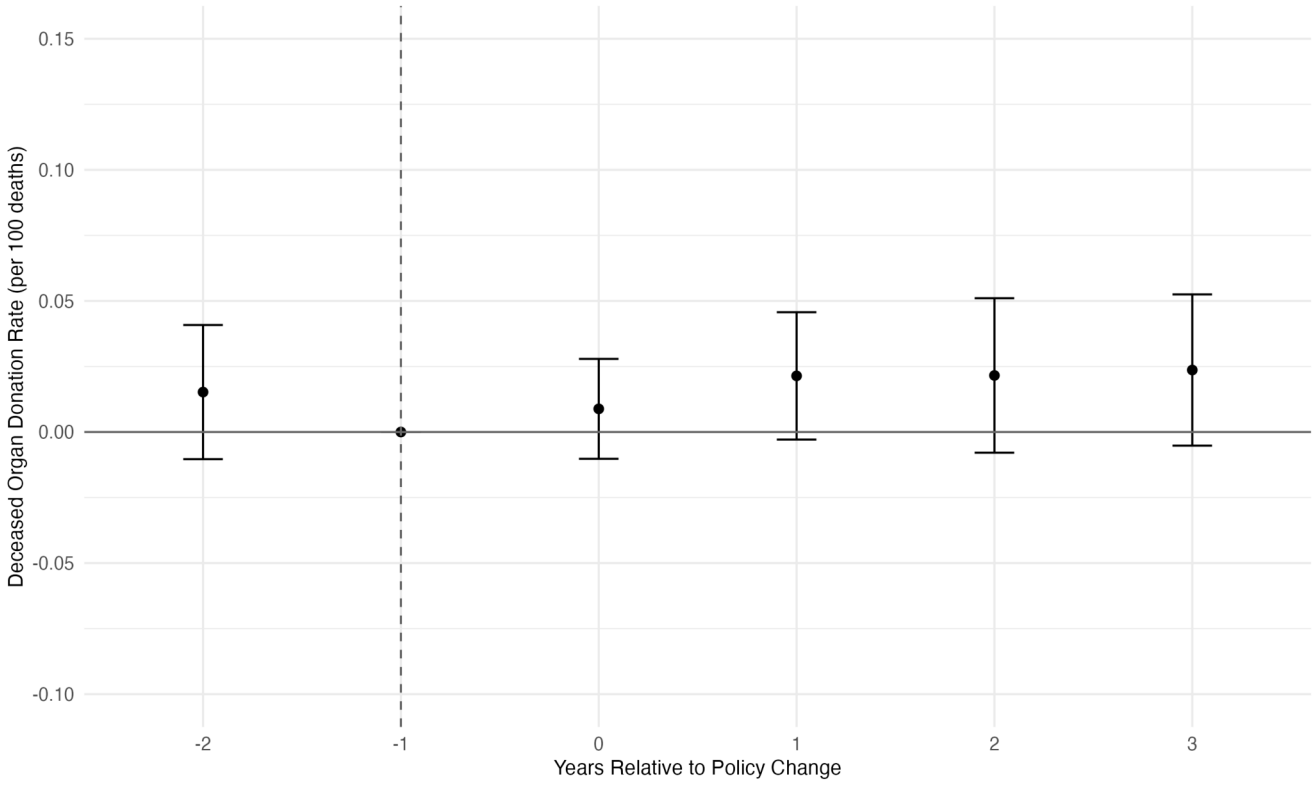
where γ_c and δ_t are country and year fixed effects, respectively, and $\mathbf{X}_{ct}$ includes time-varying controls (e.g., health expenditure per capita, road mortality, population, GDP). We omit $k = -1$ to normalize the event-time coefficients relative to the year prior to the reform. This specification allows us to test for dynamic treatment effects and examine whether changes in donation rates follow the implementation of opt-out laws.

A causal reading of these estimates would require (i) parallel trends in the absence of treatment; (ii) no anticipatory effects prior to the switch (which we assess via pre-trend coefficients); and (iii) no time-varying unobservables correlated with both the timing of

policy adoption and donation rates. We do not claim that these conditions are fully satisfied in our setting: the number of regime switches is small, and the timing of reform may coincide with broader shifts in public attitudes, media attention, or transplant infrastructure that country and year fixed effects cannot fully absorb. We therefore interpret our estimates as motivating patterns rather than definitive causal effects. That said, the observed patterns do not appear to be artifacts of such confounds: a series of placebo checks shows no significant structural shifts in relevant placebo observables (e.g. motor vehicle accident deaths) around the times of policy shifts (see Online Appendix Section A.1.4).

Furthermore, these constraints are not unique to our analysis. Evidence on what happens to donations when a country changes its consent regime has come primarily from comparisons across regimes rather than from transitions between them (Gimbel et al. (2003), Abadie and Gay (2006)), from single-country experiences (Domínguez and Rojas (2013)), or from within-country time series without contemporaneous comparison countries (Dallacker et al. (2024)). Our analysis contributes to the existing body of evidence by drawing on a panel spanning 94 countries to track donation rates over time and to compare countries that switched their consent regime with contemporaneous controls from the same continent.

Figure II: Event Study Graph for All Countries

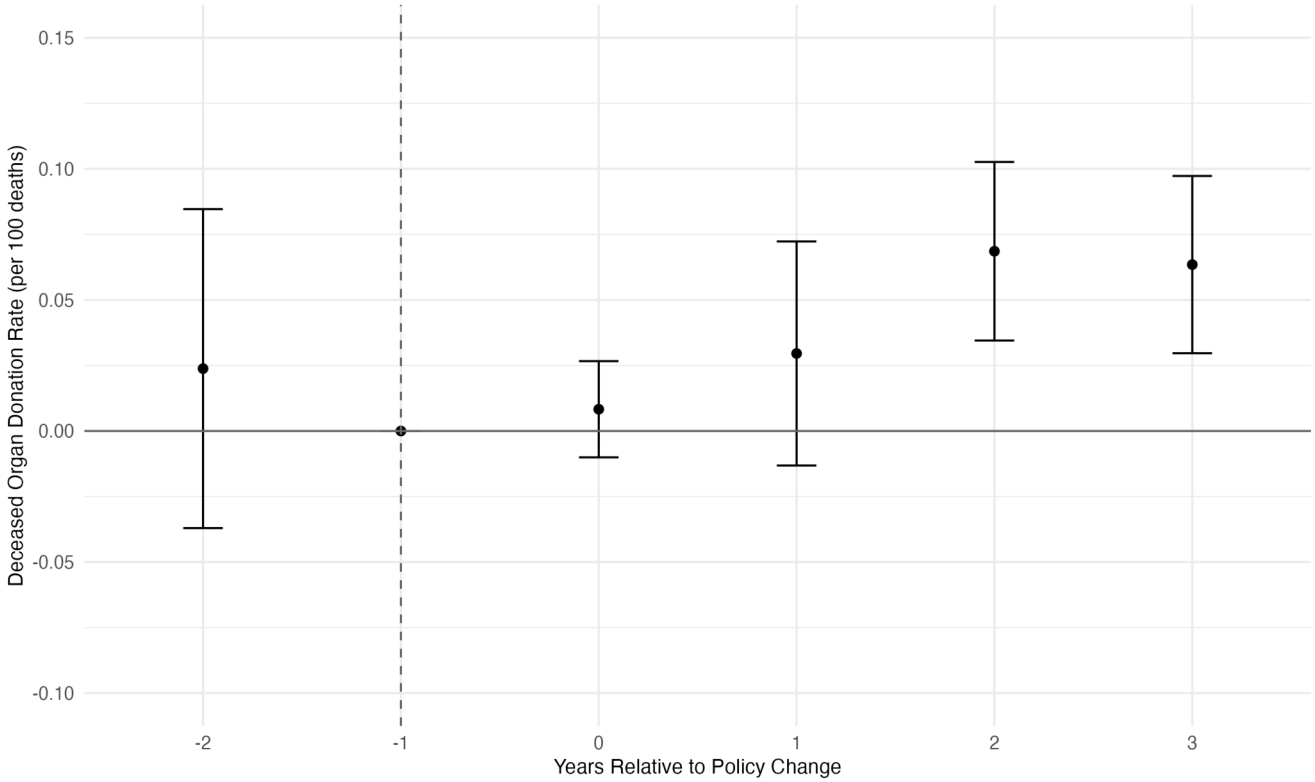


Notes: Estimated based on Equation 1 including panel data on all countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria, Luxembourg* (1980-1985), Belgium* (1984-1989), Finland* (1983-1988), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). The sample also includes Sweden* (1994-1999), Greece (1995-1999), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Uruguay* (2011-2016), Bolivia (2008, 2010-2016), Ecuador (2009-2016), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2016). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals.*

The results of the baseline event study are shown in Figure II. While the pre-trends are similar across the regime-switching and control countries, we begin to see a divergence in trends for donation per death starting with the year of the switch to opt-out. On average, countries that switch to opt-out see a marginally significant increase in donations per death post-reform. However, the estimates exhibit substantial heterogeneity. Chile, for instance, experienced a sharp decline in donation rates following its 2010 adoption of an

opt-out regime—a drop of over 3 percent relative to other Latin American countries.[7]

Figure III: Event Study Graph for Strict Opt Out Countries



Notes: Estimated based on Equation 1 including panel data on all strict opt out countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria, Luxembourg* (1980-1985), Belgium* (1984-1989), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

To further investigate this heterogeneity, we disaggregate the treatment sample based on the "strictness" of consent enforcement. We define a regime as "strict opt-out" if the family veto is limited or rarely exercised (e.g., Austria, Belgium); "weak opt-out" regimes, by contrast, continue to rely heavily on familial consent in practice (e.g., Sweden, Chile).[8]

[7]Luxembourg's data are particularly noisy and have been excluded from some previous analyses (e.g., Abadie and Gay (2006)). To maintain transparency and avoid discretionary sample selection, we retain Luxembourg in the main figures (Figure II, Figure III, and Figure IV). Excluding Luxembourg yields virtually identical results.

[8]We focus on direct evidence based on primary or secondary sources from physicians/surgeons who operated in the years surrounding the policy shift. For instance, Belgium is categorized as "strict opt-out" because we know from a published source, by a Belgian doctor who operated in the years following Belgium's shift to opt-out, that "As a rule when death is notified, the family is informed of the intention

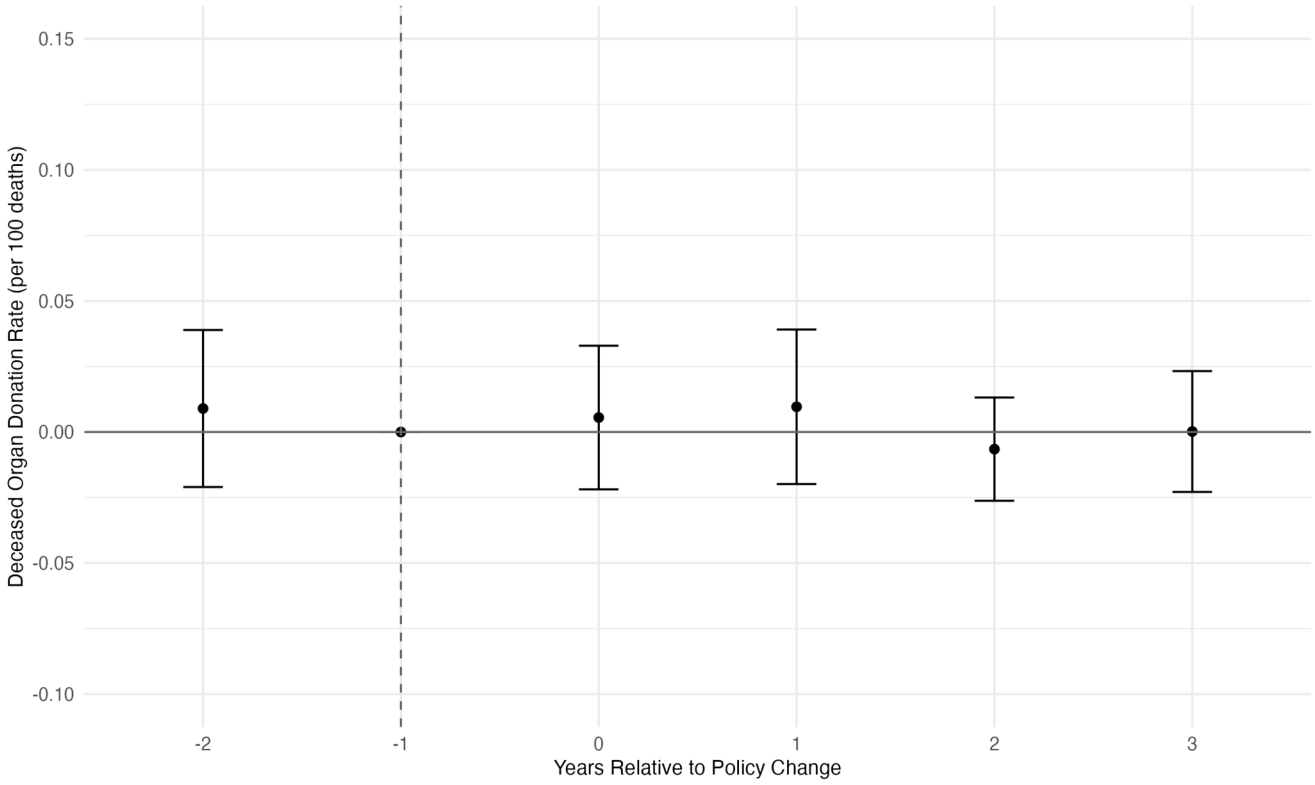
Re-estimating the event study model separately for these subsamples reveals a striking contrast. In strict opt-out countries, donation per death rises significantly shortly after the switch, with estimates remaining high over time (see Figure III; and for twice as many post years see Appendix A97). In weak opt-out countries, by contrast, the coefficients remain flat and statistically insignificant in both short (see Figure IV) and longer terms (see Appendix A98). These null results echo recent within-country evidence: Dallacker et al. (2024) analyze five switches to opt-out (Argentina, Chile, Sweden, Uruguay, and Wales) and find no increase in deceased donations – and three of those five (Chile, Sweden, and Uruguay) are weak opt-out switchers in our classification.

Recent work shows that TWFE with staggered adoption can be biased under treatment-effect heterogeneity, dynamic effects, or near-universal treatment. We therefore implement Sun and Abraham (2021), Callaway and Sant'Anna (2021), and Borusyak et al. (2021) estimators as robustness checks. Across all three approaches, the estimated dynamic effects closely match our baseline TWFE results in sign, magnitude, and timing. Given this concordance, and because OLS/TWFE is more transparent and parsimonious for our setting, we report OLS estimates in the main text and provide the alternative estimators in the Online Appendix.

A potential concern is that strict opt—out adopting countries started from lower pre-treatment levels ("more room to grow"), whereas weak opt-out adopters were closer to saturation. We address this in a few ways. First, we stratify the sample by baseline outcome quartiles and compare strict and weak opt-out countries within strata that include at least one of the "treated countries" and one of the "control countries"; the strict–weak contrast persists in the middle quartiles, where treatment and control countries exhibit similar average baseline levels. Next, we apply the estimator of De Chaisemartin and d'Haultfoeuille (2024), and find results consistent with our baseline—positive, stable effects for strict opt-out regimes with no evidence of bias from pre-treatment levels or dynamic adjustments. Collectively, these exercises (reported in the Online Appendix) suggest that mean reversion and concavity are not plausible explanations.

to proceed with organ removal, but explicit permission is seldom asked. This information is usually given by the doctor in charge and not by the transplant coordinator, whose role is often limited to technical and administrative support. No information is given when ... the relatives cannot be contacted in due time." (Michielsen (1996)). Similarly, Chile is categorized as "weak opt-out" because we know from a published source, by Chilean doctors who operated in the years following Chile's shift to opt-out, that "In our society the family plays a crucial role. They are always approached for consent for organ donation making the real applicability of the law debatable." (Domínguez and Rojas (2013)).

Figure IV: Event Study Graph for Weak Opt Out Countries



Notes: Estimated based on Equation 1 including panel data on all weak opt out countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Finland, Germany, Netherlands, Norway, and UK (1983-1988). The sample also includes Sweden* (1994-1999), Greece (1995-1999), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Uruguay* (2011-2016), Bolivia (2008, 2010-2016), Ecuador (2009-2016), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2016). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

These results suggest that legal regime classification alone may be insufficient to predict donation outcomes; the institutional enforcement context plays a critical role. While opt-out reforms can be followed by higher donation rates, especially under strict implementation, policy effectiveness appears contingent on the operational design and societal norms surrounding organ procurement. We treat these patterns as motivating evidence rather than definitive causal estimates: the remainder of the paper develops a theoretical framework and a controlled experiment to isolate a mechanism capable of

generating precisely this heterogeneity.

3 Conceptual Framework

In many countries, the default policy plays a critical role in determining realized organ donation rates. On one hand, an "opt-in" regime requires willing donors to clearly signal their intent. The cost of doing so may dissuade some willing donors from opting in, but the ones who do opt in send a strong message to their families and medical professionals. On the other hand, an "opt-out" regime positions donation as the default. Consequently, willing donors are more likely to be registered as organ donors, but the informational content of the signal is diluted. Importantly, families often override defaults due to uncertainty about the decedent's true preference. Such uncertainty is the norm, rather than the exception: most families approached about donation report never having discussed the topic with the deceased (Siminoff and Lawrence (2002), Schulz et al. (2012)), which is why the informational content of opting in or out carries so much weight in what follows.

This section provides a simple theoretical framework to clarify how these regimes function as *signaling games*. We consider an environment with two players, a *potential donor* (Player 1) and a *decision maker* (Player 2). Player 1 has a binary type reflecting willingness to donate his organs, and Player 2 must ultimately choose whether to proceed with donation. We focus on *pure strategy perfect Bayesian equilibria* throughout.

3.1 Types and Actions

Types. Player 1's type is $d \in \{0, 1\}$, where $d = 1$ indicates willingness to donate and $d = 0$ indicates unwillingness. The ex-ante probability that $d = 1$ is $p \in (0, 1)$. Player 2 does not observe d but knows p.[9] We interpret p as the decision maker's prior belief or as the population's average willingness to donate.

Actions. Player 1 chooses an action $s \in \{0, 1\}$ whose interpretation depends on the *regime*. In an *opt-in* regime, $s = 1$ indicates paying a cost $c > 0$ to *opt in* (i.e. explicitly authorize donation). In an *opt-out* regime, $s = 1$ indicates paying cost $c > 0$ to *opt out* (i.e. explicitly refuse donation).[10] Player 2 observes the choice of s and chooses

[9] We assume throughout that p is common knowledge.
[10] We assume throughout that c is common knowledge.

$D \in \{0, 1\}$, where $D = 1$ means the organs are donated and $D = 0$ means no donation.

Interpretation. The fact that Player 2 does not observe d is the primary informational friction in our model. It reflects the empirical observation that most families never discuss organ donation and so do not know their relatives' preferences. We therefore assume that direct communication between the two players is not possible. The parameters also capture important features of the setting. The values of p and c vary across countries, and c reflects deliberate institutional design – checking a box when applying for a driver's license versus submitting a notarized letter.

3.2 Payoffs

Player 1 (Potential Donor). If the final donation decision D aligns with d, Player 1's payoff is $x - c\,s$; if $D \neq d$, then the payoff is $-c\,s$. Formally:

$$u_1(d, D, s) \;=\; \begin{cases} x - c\,s, & \text{if } D = d, \\ -c\,s, & \text{otherwise.} \end{cases}$$

Here, $x > 0$ captures the potential donor's utility when the outcome matches their true preference. It can be interpreted as anticipatory utility. For example, when Player 1 is a willing donor, x represents the value of the "warm-glow" feeling from knowing that his organs will be donated.

Player 2 (Decision Maker). Player 2 obtains a benefit $X > 0$ if $D = d$. It can be interpreted as the utility derived from honoring a deceased relative's preference. The additional costs incurred by Player 2 are:

- $\varepsilon > 0$, the cost or disutility of authorizing donation, incurred whenever $D = 1$.
- $\delta > 0$, depending on the regime, the cost of *overturning* the default (presumed consent) or explicit choice (informed consent).

We interpret ε as the emotional cost of donating a loved one's organs.[11] However, it can also represent the decision maker's own preferences, moral or religious views etc. For

[11] We assume throughout that ε is fixed but allowing ε to vary across individuals does not change our results qualitatively. In particular, aggregate donation rates will be interior but the ordering of regimes will remain unchanged when ε is heterogeneous.

example, if Player 2 is opposed to organ donation on religious grounds, then ε captures the disutility of acting against those convictions and authorizing donation. Similarly, if Player 2 believes that organ donation is a moral obligation, then $\varepsilon < 0$ captures the benefit (rather than the cost) of acting in accordance with that conviction.[12]

Under an opt out regime, δ represents the cost of overturning presumed consent and blocking donation when Player 1 does not explicitly opt out. The value of δ separates weak and strict opt out regimes. Under strict opt out, it is difficult for families to overturn presumed consent, and they must incur significant cost (in terms of time and effort) to do so. A large δ captures such policies. On the other hand, under weak opt out, it is easy for families to block donation, which is captured by a small δ. Both regimes are legally opt out but the differences in δ can produce very different donation rates, as the analysis below shows. For simplicity, we assume that δ is also the cost of blocking donation when Player 1 explicitly opts in under an opt in regime. However, the two costs may be different in practice and our results are not sensitive to this symmetry.

Opt-In Regime. When Player 1 does not opt in ($s = 0$), there is no presumed consent; thus,

$$u_2^{OI}(s = 0) \;=\; \mathbf{1}\{D = d\}\, X \;-\; D\,\varepsilon.$$

If Player 1 opts in ($s = 1$), overturning that expressed consent costs $\delta > 0$. Hence,

$$u_2^{OI}(s = 1) \;=\; \mathbf{1}\{D = d\}\, X \;-\; D\,\varepsilon \;-\; (1 - D)\,\delta.$$

Let $p'_s \in [0, 1]$ be Player 2's posterior belief that $d = 1$ after observing Player 1's choice of s. Then Player 2 donates ($D = 1$) if the expected benefit exceeds the cost. In particular:

- If $s = 0$, $D = 1$ iff $2p'_0 - 1 > \dfrac{\varepsilon}{X}$.
- If $s = 1$, $D = 1$ iff $2p'_1 - 1 > \dfrac{\varepsilon - \delta}{X}$.

Opt-Out Regime. When Player 1 does not opt out ($s = 0$), the default is to donate, so overturning it ($D = 0$) imposes cost $\delta > 0$. Thus,

$$u_2^{OO}(s = 0) \;=\; \mathbf{1}\{D = d\}\, X \;-\; D\,\varepsilon \;-\; (1 - D)\,\delta.$$

[12] We assume throughout that $\varepsilon > 0$ but our analysis and results generalize readily to the case when $\varepsilon < 0$.

If Player 1 opts out ($s = 1$), then no donation default exists, so

$$u_2^{OO}(s = 1) \;=\; \mathbf{1}\{D = d\}\, X \;-\; D\,\varepsilon.$$

Hence Player 2 donates ($D = 1$) if, given the posterior belief p'_s, the expected benefit exceeds the cost:

- If $s = 0$, $D = 1$ if $2p'_0 - 1 > \dfrac{\varepsilon - \delta}{X}$.
- If $s = 1$, $D = 1$ if $2p'_1 - 1 > \dfrac{\varepsilon}{X}$.

3.3 Equilibria in Pure Strategies

We focus on *pure strategy perfect Bayesian equilibria*. Multiple equilibria can arise, so we restrict attention to those that are especially *reasonable* or *intuitive*. Specifically, we consider equilibria in which an explicit declaration is taken at face value: the posterior after opting in is one, and the posterior after opting out is zero.[13] Beliefs following inaction are determined by Bayes' rule in every equilibrium we consider. This selection criterion reflects the institutional meaning of opting in or opting out, which are legally defined declarations rather than abstract messages. The criterion applies symmetrically across regimes. What differs between regimes is not how declarations are interpreted but which declarations are available. We represent these equilibrium predictions on Figure V.

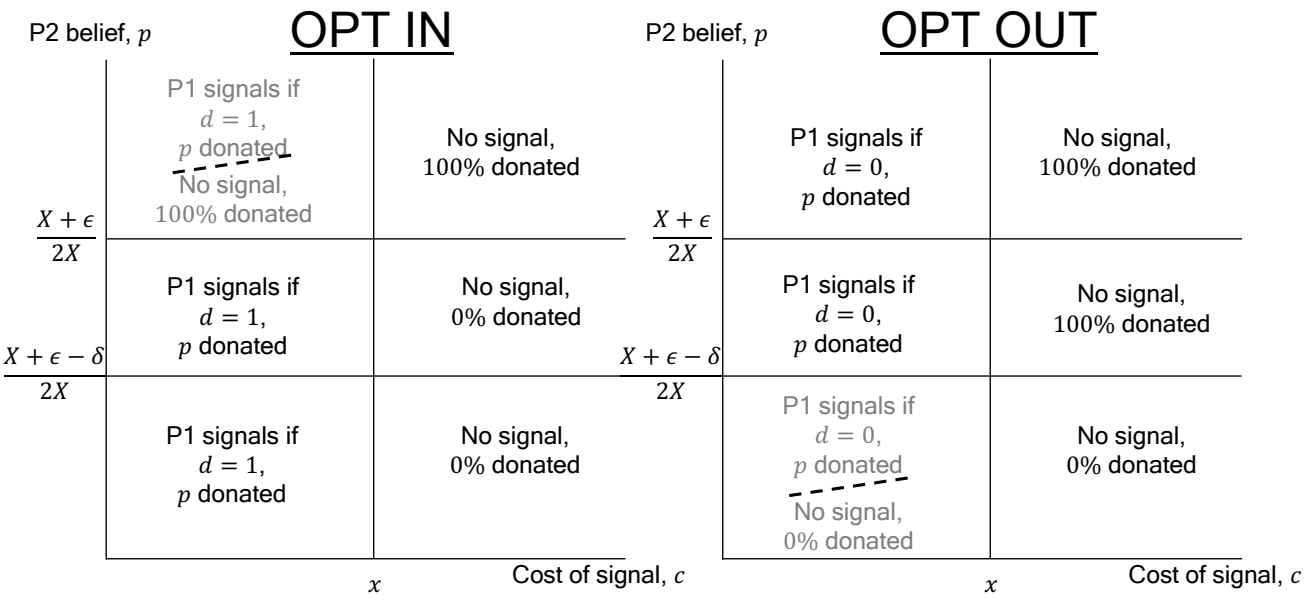
Key Parameters. We define:

- *Large c*: $c > x$. Signaling is too costly, so Player 1 never opts in/out. Player 2 simply bases D on the prior belief p and the relevant donation threshold.[14]
- *Large p*: In the opt-in regime, we say p is large if $2p - 1 > \dfrac{\varepsilon}{X}$. In the opt-out regime, we similarly say p is large if $2p - 1 > \dfrac{\varepsilon - \delta}{X}$.

Additionally, we assume throughout that $X > \varepsilon$. This ensures that Player 2 is willing to authorize donation whenever she is sufficiently confident that Player 1 was a willing donor.

[13] Recall that a posterior of one means that Player 2 is certain that Player 1 is a willing donor.
[14] Player 1 never opts in/out so the posterior belief is equal to the prior.

Figure V: Pure Strategy Perfect Bayesian Equilibria under Opt In and Opt Out



Notes: Theoretical Predictions based on the model outlined in Section 3. We plot the pure strategy perfect Bayesian equilibria in various parts of the parameter space.

Opt-In Equilibria

- **Pooling on $s = 0$:** If c is large, Player 1 never opts in ($s = 0$), and Player 2 donates if and only if $2p - 1 > \frac{\varepsilon}{X}$.[15]
- If c is small ($c < x$), there can be a fully **separating** equilibrium:

$$s = 1 \text{ if and only if } d = 1, \quad D = 1 \text{ if and only if } s = 1.$$

Here, the willing donor signals at cost c, ensuring donation; the unwilling type does not opt in, avoiding donation. Player 2's posterior beliefs are given by:

$$p_0' = 0$$
$$p_1' = 1$$

Player 2 is certain that Player 1 is a willing donor if Player 1 opts in. Otherwise she is certain that Player 1 is not a willing donor.

[15] This equilibrium is supported by any off-path belief for Player 2.

- **Pooling on donation** can also occur if p is high enough to justify $D = 1$ even absent explicit consent, leaving no incentive to pay c, leading both types to choose $s = 0$. Player 2's posterior beliefs are given by:

$$p_0' = p$$
$$p_1' = 1$$

  Not opting in leaves Player 2's prior belief unchanged but opting in makes her certain that Player 1 is a willing donor.

Opt-Out Equilibria

- **Pooling on $s = 0$:** If c is large, no one opts out ($s = 0$), so Player 2 donates whenever $2p - 1 > \frac{\varepsilon - \delta}{X}$.[16]
- If c is small, a fully **separating** equilibrium can have

$$s = 1 \text{ if and only if } d = 0, \quad D = 1 \text{ if and only if } s = 0.$$

  Player 2's posterior beliefs are given by:

$$p_0' = 1$$
$$p_1' = 0$$

- A **zero-donation pooling** equilibrium may arise if p is sufficiently low. Player 1 never opts out so Player 2 has to rely on her prior belief and always overturns presumed consent.[17] Her posterior beliefs are given by:

$$p_0' = p$$
$$p_1' = 0$$

[16] This equilibrium is supported by any off-path belief for Player 2.

[17] If Player 1 does nothing (i.e. does not opt out), Player 2 relies on her prior belief and blocks donation. The only action available to Player 1 is opting out. But if Player 1 opts out, Player 2 believes that $d = 0$ and does not donate.

3.4 Comparison and Key Outcomes

The clearest comparison emerges when c is large. In both opt-in and opt-out, Player 1's signal is absent. However, the *threshold* for donating differs:

$$\text{Opt-in: } 2p - 1 > \frac{\varepsilon}{X},$$
$$\text{Opt-out: } 2p - 1 > \frac{\varepsilon - \delta}{X}.$$

Because $(\varepsilon - \delta)/X < \varepsilon/X$, opt-out makes donation more likely, all else equal. Intuitively, refusing donation under opt-out imposes an additional cost δ on Player 2, whereas under opt-in, default non-donation is cheaper to maintain. This difference in thresholds would lead to different predictions for the two regimes, as represented by the differences in the middle-right area for the panels in Figure V. In particular, under a strict opt out regime, δ is large and the difference between the two thresholds may be significant. On the other hand, when δ is small (as is the case under a weak opt out regime), the two thresholds may be very similar.

When c is small, *separating equilibria* can emerge in both regimes: the willing donor can signal (by opting in) or *not* signal (by remaining in the default of donation). The difference is that under opt-out, if p is very low, Player 2 might systematically overturn presumed consent, generating a scenario in which *no* one opts out but *no* donations occur. In contrast, under opt-in, a strictly positive fraction of individuals can still donate, because a willing donor always has the option to opt in and separate.

Remark on Other Equilibria. Various equilibria beyond those considered here may exist, including partially separating equilibria in mixed strategies. We restrict attention to pure strategies for clarity and tractability. Within pure strategies, the selection criterion above rules out equilibria in which a declaration conveys the opposite of its plain meaning. For instance, an equilibrium in which only unwilling donors opt in and Player 2 donates if and only if Player 1 does *not* opt in. Such equilibria are supported by consistent beliefs but sit poorly with the institutional setting we model.

Remark on the Cost of Signaling. Our model assumes that $c > 0$ i.e. the potential donor must incur a non-zero cost to opt in/out. However, the mechanism we emphasize does not rely on the precise value of c. For example, under opt-out, the zero donation

equilibrium survives even when $c \approx 0$ i.e. Player 1 never opts out but Player 2 always overturns presumed consent and no organs are donated. This equilibrium exists because Player 2 blocks donation after either signal; so Player 1 never opts out; and Player 2 is forced to make a decision based on her prior alone. Intuitively, the key is that the only action available to Player 1 is opting out. But that convinces Player 2 that he is *not* a willing donor. Willing donors thus have no way to signal their true preference–lowering the cost of opting out makes no difference because it does not change the set of actions available to Player 1.

4 Experimental Design

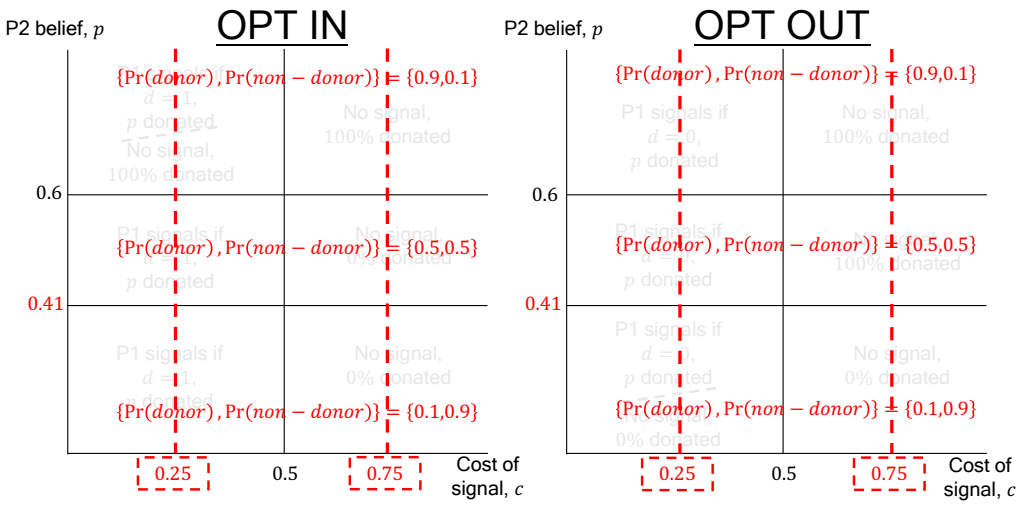
We conduct an incentivized online experiment to study the behavioral effects of different organ donation default policies. Participants ($n = 1004$) are recruited via Prolific for an experiment lasting approximately 18 minutes. The core asset in this experiment is a neutral object called a "wug," which serves as an abstract representation of a human organ, specifically designed to avoid emotional or moral connotations associated with real organs. The experiment comprises three distinct stages that reflect key phases in organ donation decision-making.

Participants are randomly assigned to treatments in a 2×2×2 factorial design with three main manipulations:

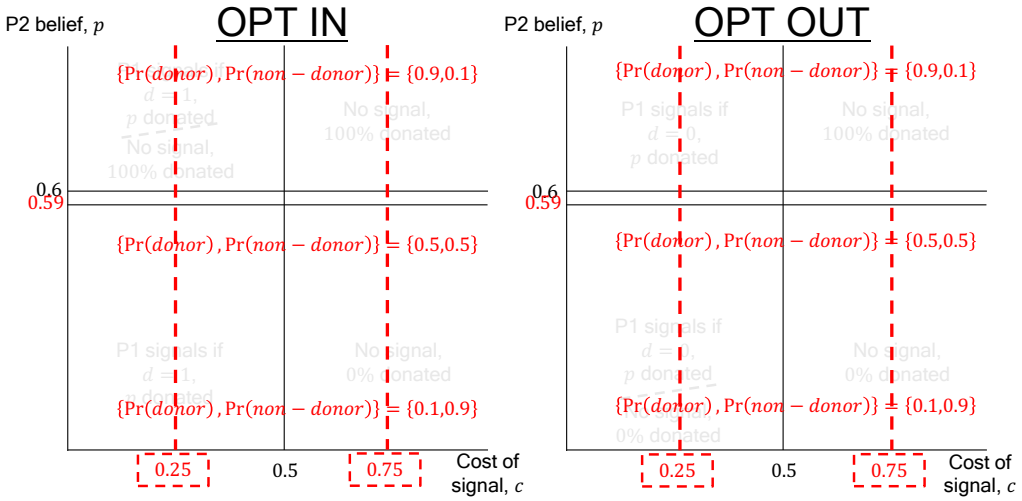
- **Default regime:** Opt-in (default is non-donation) vs. opt-out (default is donation).
- **Signal cost to wug owner:** High ($0.75) vs. low ($0.25).[18]
- **Cost to decision proxy of overturning defaults or signals:** High ($0.19) vs. low ($0.01).[19] (Represented by the top two panels versus the bottom two panels of Figure VI.)

[18] The signal cost is represented by the variable c in our theoretical model.
[19] The cost of overturning defaults is represented by the variable δ in our theoretical model.

Figure VI: Experimental Treatments in Parameter Space



A. High ($0.19) cost to decision proxy of overturning defaults or signals:



B. Low ($0.01) cost to decision proxy of overturning defaults or signals:

Notes: Various parts of the parameter space, in particular cost to Player 2 to overturn "consent", Player 2 beliefs and Cost of signaling for Player 1.

4.1 Participant Payments and Incentives

Participants receive a baseline completion payment of $5.00 and a default bonus of $0.94. Additional earnings ranging from $0.50 to $1.00 can be won through strategic decisions made during the experiment. Earnings can potentially decrease, but losses are capped at $0.94, guaranteeing a minimum payment of $5.00.

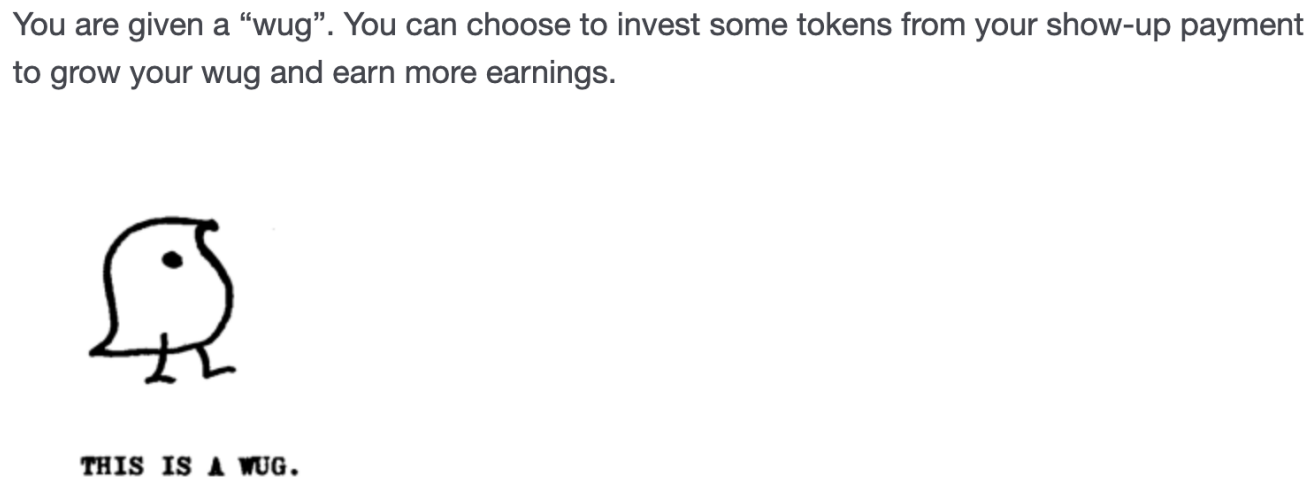
Participants who successfully pass an initial attention check proceed through three main stages of the experiment, detailed below.

4.2 Stage 1: Living Utilization of the Wug (Organ)

Participants first complete a minimal-group task Chen and Li (2009) where they select preferred paintings from two artists (Klee or Kandinsky) and are grouped anonymously into "families" based on their preferences. (See Online Appendix A.3.2) This step establishes the context of "in-group" interactions.

Each participant is then given a digital pet—a wug—which symbolizes a functioning organ during life (See Figure VII).[20] Participants name their wug to foster psychological ownership and make a single investment decision: they can feed the wug at a cost of \$0.10, yielding a guaranteed return of either \$0.15 or \$0.20. This decision models the utility and care individuals derive from an organ while alive.

Figure VII: Decision Screen: "Wug"



Notes: The wug which symbolizes a functioning organ during life.

4.3 Stage 2: Opt-in/Opt-out Decision

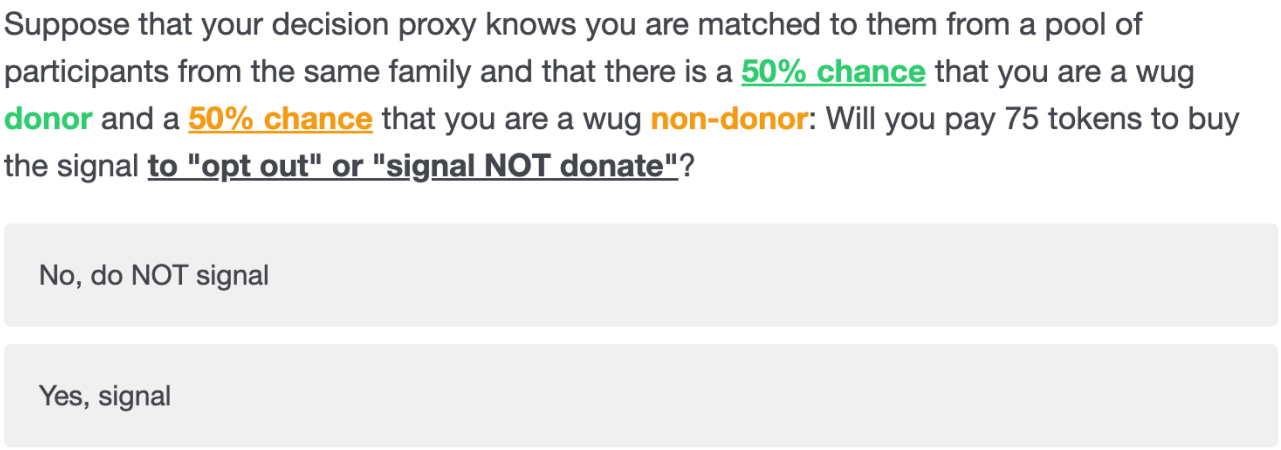
Stage 2 analogously represents the donor's decision to register (opt-in) or deregister (opt-out) from organ donation while alive. Participants learn their randomly assigned type:

- **Wug Donor:** Gains \$0.50 if their wug is eventually donated.
- **Wug Non-Donor:** Gains \$0.50 if their wug is not donated.

[20] See Berko (1958) who developed the "wug".

Participants do not directly control their wug's final donation but can send a non-binding signal to a matched "decision proxy" from their minimal group, indicating their preference. The default donation status (opt-in or opt-out) depends on their assigned treatment. Sending a signal (opting-in or opting-out depending on the default) involves a treatment-specific cost ($0.25 or $0.75). Decisions to send signals are elicited under three informational conditions reflecting different priors (10%, 50%, 90%) that the decision proxy believes the wug owner is a donor type (See Figure VIII).[21]

Figure VIII: Decision Screen: "Signal"



Participants are matched anonymously with exactly one other participant (asynchronously and managed within 24 hours) who serves as their decision proxy. Each participant is matched similarly to act as a decision proxy for another participant, ensuring reciprocal and independent matches.

Participants are also randomly assigned (1/3 each) to subgroups of ten, with different distributions of wug donors (9 donors/1 non-donor, 5 donors/5 non-donors, 1 donor/9 non-donors). This randomized subgrouping affects the scenario under which bonus payments are calculated.

4.4 Stage 3: Decision Proxy After Death

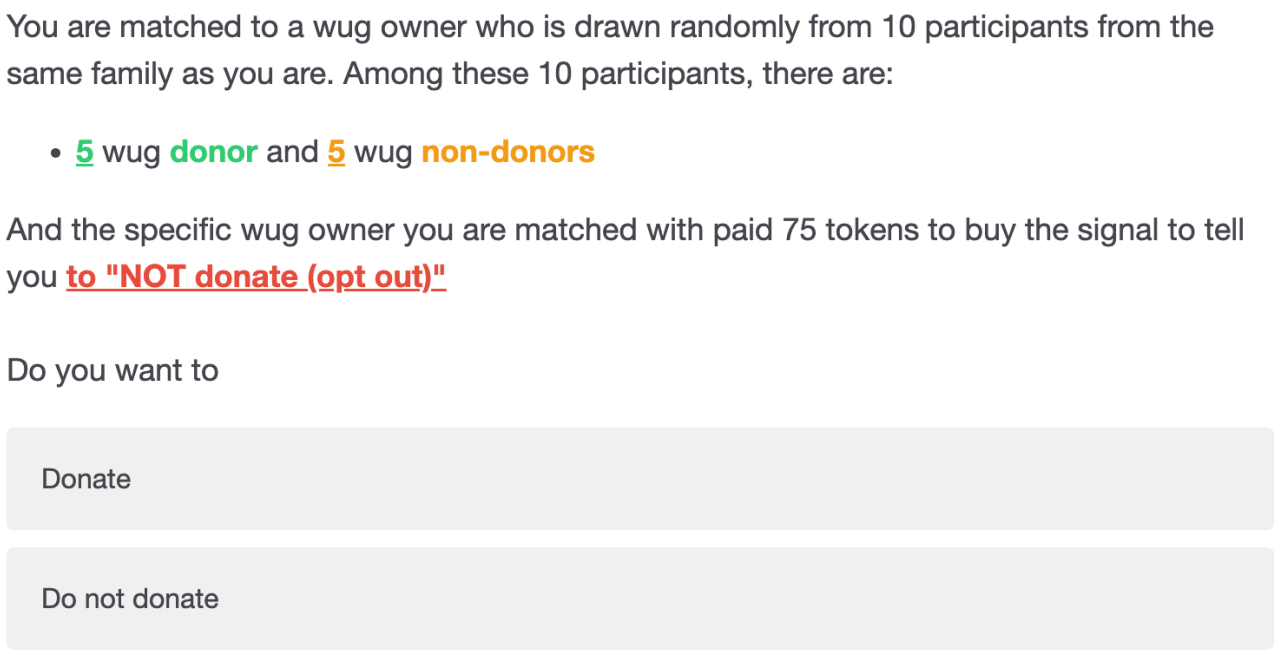
In the final stage, participants switch roles to become the decision proxy for their matched wug owner. This stage corresponds to the post-mortem decision made by families about

[21] The different priors are represented by the variable p in our theoretical model.

organ donation. Decision proxies earn \$0.50 if their decision matches the wug owner's actual type (donor or non-donor), and they must pay a fee of \$0.10 if they choose to donate the wug, regardless of the owner's type.[22] Overturning the default or explicit signals incurs an additional treatment-specific cost (\$0.01 or \$0.19). High cost of overturning the default (\$0.19) represents a strict consent regime while low cost (\$0.01) represents weak consent. These parameters put the empirical observations from this experiment neatly into different parts of Figure V, in particular the higher versus lower cost of overturning defaults or signals is represented by the top two panels versus the bottom two panels of Figure VI.[23]

Proxy decisions are elicited under six conditions: three priors regarding donor likelihood (10%, 50%, 90%) and two signal states (owner sent a signal vs. owner sent no signal). (See Figure IX.)

Figure IX: Decision Screen: "Donate?"



4.5 Matching and Payoffs

The one-to-one matching is done asynchronously to ensure each participant is matched exactly once as an owner and once as a proxy, independent of simultaneous participation.

[22] This fixed fee is represented by the variable ε in our theoretical model.

[23] When the cost of overturning defaults or signals is low, the middle region of Figure V collapses to the small area between $p = 0.59$ and $p = 0.6$ in the bottom two panels of Figure VI.

Payoffs are calculated based on only one randomly realized scenario per match, and final payments, including bonuses, are completed within 48 hours of experiment completion.

4.6 Survey Questions and Data Collection

Finally, participants answer brief demographic and value-related questions adapted from Elías et al. (2019), intended for control variables in analysis.

This experimental framework allows precise examination of how defaults, signaling costs, and proxy decision costs influence the efficiency and outcomes of donation systems, abstracted from ethical or emotional biases.

4.7 Summary of Design

Our experiment combines cross subject (regime, cost of signaling, donor type) and within subject (priors) designs. The experiment was run on Prolific with 1004 participants, 988 participants passed the attention check and are included in the final sample.[24] The experiment was done on September 27, 2024. Balance Table II summarizes the treatments and the corresponding volume of participants. Balance is achieved across subject demographics, reported moral values and life satisfaction, and politics.

[24]Including the subjects who failed the attention check does not change the results.

Table II: Balance Check by Treatment Arm

	D/25T/1T		D/25T/19T		D/75T/1T		D/75T/19T		ND/25T/1T		ND/25T/19T		ND/75T/1T		ND/75T/19T		p-value
	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.	(Across Arms)
Female	0.44	0.50	0.45	0.50	0.48	0.50	0.56	0.50	0.48	0.50	0.45	0.50	0.52	0.50	0.48	0.50	0.62
White	0.66	0.48	0.64	0.48	0.80	0.40	0.70	0.46	0.69	0.46	0.69	0.46	0.72	0.45	0.67	0.47	0.22
Black	0.19	0.40	0.20	0.40	0.14	0.35	0.16	0.37	0.21	0.41	0.20	0.40	0.13	0.34	0.20	0.40	0.68
Asian	0.10	0.31	0.10	0.30	0.04	0.20	0.07	0.26	0.07	0.26	0.09	0.29	0.28	0.11	0.11	0.31	0.62
Hispanic	0.07	0.26	0.09	0.29	0.04	0.20	0.06	0.25	0.06	0.23	0.06	0.23	0.06	0.24	0.10	0.30	0.68
Disability	0.16	0.37	0.12	0.33	0.12	0.32	0.16	0.37	0.16	0.37	0.13	0.34	0.17	0.37	0.16	0.36	0.91
College Graduate	0.57	0.50	0.64	0.48	0.58	0.50	0.66	0.48	0.63	0.49	0.61	0.49	0.57	0.50	0.57	0.50	0.74
High School Grad	0.99	0.09	1.00	0.00	0.99	0.09	1.00	0.00	0.99	0.09	0.99	0.09	0.99	0.09	1.00	0.00	0.89
Married	0.51	0.50	0.45	0.50	0.47	0.50	0.42	0.50	0.42	0.50	0.37	0.49	0.40	0.49	0.43	0.50	0.54
Number of Children	2.02	1.14	2.16	1.37	2.18	1.21	2.06	1.20	1.80	1.10	1.99	1.23	2.10	1.38	1.96	1.09	0.28
Fully Employed	0.52	0.50	0.60	0.49	0.61	0.49	0.61	0.49	0.57	0.50	0.54	0.50	0.58	0.50	0.60	0.49	0.83
Unemployed	0.18	0.38	0.17	0.37	0.20	0.40	0.21	0.41	0.22	0.42	0.22	0.42	0.17	0.38	0.15	0.36	0.77
Life Satisfaction	6.60	2.34	6.96	2.22	6.57	2.28	6.71	2.00	6.44	2.34	6.44	2.19	6.63	2.18	6.35	2.02	0.49
Health Insurance	0.29	0.46	0.29	0.46	0.38	0.49	0.34	0.48	0.27	0.45	0.35	0.48	0.32	0.47	0.30	0.46	0.60
Lower Class	0.42	0.50	0.33	0.47	0.41	0.49	0.34	0.48	0.45	0.50	0.41	0.49	0.40	0.49	0.42	0.50	0.53
Middle Class	0.46	0.50	0.52	0.50	0.45	0.50	0.51	0.50	0.40	0.49	0.47	0.50	0.46	0.50	0.47	0.50	0.65
Upper Class	0.12	0.33	0.15	0.36	0.13	0.34	0.14	0.35	0.15	0.36	0.11	0.32	0.13	0.34	0.11	0.32	0.98
Democrat	0.42	0.50	0.39	0.49	0.46	0.50	0.36	0.48	0.48	0.50	0.36	0.48	0.43	0.50	0.43	0.50	0.38
Republican	0.27	0.44	0.36	0.48	0.36	0.48	0.34	0.47	0.27	0.45	0.27	0.44	0.34	0.48	0.26	0.44	0.32
Moral Score	6.24	1.04	6.32	1.17	6.23	1.13	6.29	1.11	6.05	1.15	6.25	1.06	6.32	1.16	6.18	1.13	0.59
Subjects	124		121		121		125		126		123		126		122		

Note: This table reports the background characteristics of 988 subjects in the main sample, pooled and by treatment group. Treatment arms are labeled as "DefaultStatus / SignalingCost / OverturnCost." For example, "D / 25T / 1T" indicates subjects with a default status of Donated, who could send a signal to others for 25 tokens and override others' decisions for 1 token. Other arms (e.g., "D / 25T / 19T", "D / 75T / 1T", "D / 75T / 19T") follow the same structure, varying the costs of signaling and overturning. The "Not Donated" arms (e.g., "ND / 25T / 1T", "ND / 25T / 19T", etc.) represent subjects with a default status of Not Donated, under the same token cost conditions. "Female" indicates the share of female sex; "White," "Black," "Asian," and "Hispanic" indicate the shares of subjects belonging to each of these categories. "Disability" indicates the share of subjects who reported having a disability or other chronic condition. "High School Grad" indicates the share of subjects who graduate from high school, "College Graduate" indicates the share of subjects who reported that they have a bachelor's or advanced degree. "Married" indicates the share of subjects who are married, while "Number of Children" indicates the number of children the subjects have. "Fully Employed" indicates the share of subjects who are employed full-time, while "Unemployed" indicates the share of subjects who selected are not employed. "Life Satisfaction" indicates the average life satisfaction score on a scale from 1 to 10. "Health Insurance" indicates the share of subjects who covered by Medicare, Medical Assistance, or Medicaid. "Lower Class," "Middle Class," and "Upper Class" indicate the shares of subjects who reported belonging to each of these social classes. "Democrat" and "Republican" indicate the shares of subjects who reported identifying with these political parties. "Moral Score" indicates the average moral score on a scale from -1 to 7. Table shows averages ("mean") and standard deviations ("s.d."). The p-value (Across Arms) column reports the p-value from tests of equality across the eight treatment arms: chi-squared test for binary variables and ANOVA F-test for continuous variables.

5 Lab Experiment Results

This section presents the results of our laboratory experiment on organ donation decisions under different consent regimes.

To analyze the experimental data, we first randomly match Player 1s with Player 2s, ensuring they are matched based on the regime (opt-in or opt-out), the cost of signaling (c), and the strictness of consent (δ). For each matched pair of players, we generate an observation which includes an outcome variable: an indicator variable of whether a donation occurred. The "observations" generated this way number between 244 to 250 across the cost of signaling (c) and the strictness of consent (δ) versions (4 versions in total).

We initially run an Ordinary Least Squares (OLS) regression on this single, randomly chosen matching. Subsequently, we randomly generate a new matching and estimate a separate OLS regression for each of 1000 different permutations. The average coefficients and standard errors from these 1000 permutations are reported in the bottom panel of the regression table (Table III).

Table III: Impact of Opt-out on Actual Donation Rate

	Cheap Signal (p=0.1)		Costly Signal (p=0.1)		Cheap Signal (p=0.5)		Costly Signal (p=0.5)		Cheap Signal (p=0.9)		Costly Signal (p=0.9)	
	Weak Consent	Strong Consent	Weak Consent	Strong Consent	Weak Consent	Strong Consent	Weak Consent	Strong Consent	Weak Consent	Strong Consent	Weak Consent	Strong Consent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Single Permutation												
Opt-out	-0.219***	0.005	-0.025	0.073	-0.024	-0.033	0.063	0.392***	-0.132**	-0.135**	0.023	-0.083
	(0.058)	(0.060)	(0.051)	(0.061)	(0.063)	(0.064)	(0.061)	(0.059)	(0.056)	(0.054)	(0.055)	(0.054)
Constant	0.429***	0.309***	0.206***	0.311***	0.492***	0.528***	0.317***	0.344***	0.794***	0.829***	0.746***	0.811***
	(0.041)	(0.042)	(0.035)	(0.043)	(0.045)	(0.045)	(0.043)	(0.042)	(0.039)	(0.038)	(0.038)	(0.038)
N	250	244	247	247	250	244	247	247	250	244	247	247
Overall for 1000 Simulations												
	Cheap Signal (p=0.1)		Costly Signal (p=0.1)		Cheap Signal (p=0.5)		Costly Signal (p=0.5)		Cheap Signal (p=0.9)		Costly Signal (p=0.9)	
	Weak Consent	Strong Consent	Weak Consent	Strong Consent	Weak Consent	Strong Consent	Weak Consent	Strong Consent	Weak Consent	Strong Consent	Weak Consent	Strong Consent
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Opt-out	-0.160***	-0.049	-0.026	0.083	-0.069	-0.023	0.051	0.420***	-0.154***	-0.120**	-0.013	-0.050
	(0.057)	(0.060)	(0.052)	(0.061)	(0.063)	(0.064)	(0.062)	(0.057)	(0.055)	(0.057)	(0.056)	(0.053)
Constant	0.377***	0.349***	0.225***	0.307***	0.511***	0.543***	0.344***	0.343***	0.808***	0.780***	0.743***	0.800***
	(0.040)	(0.042)	(0.037)	(0.043)	(0.044)	(0.045)	(0.043)	(0.041)	(0.039)	(0.040)	(0.039)	(0.038)
N	250	244	247	247	250	244	247	247	250	244	247	247

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the estimated parameter results for the estimation model $Donation_i = \beta_0 + \beta_1 OptOut_i + \epsilon_i$. The data for the top panel is from a single random matching of Player 1 and Player 2 in the main experiment, while the bottom panel reports the average of OLS coefficients and standard errors estimated across 1,000 permutations. The outcome of interest reported in this table is the indicator variable of whether Player 2 made a donation for that observation. The independent variable in this table is the indicator for "Opt Out". Each column reports the result from one subsample, subsampled by weak-vs-strict consent (δ = \$0.01 vs δ = \$0.19), signal cost (c = \$0.25 or c = \$0.75), and prior for Player 2 ($p = 0.1$, $p = 0.5$, or $p = 0.9$)

5.1 Opt In Produces More Donations Than Weak Opt Out

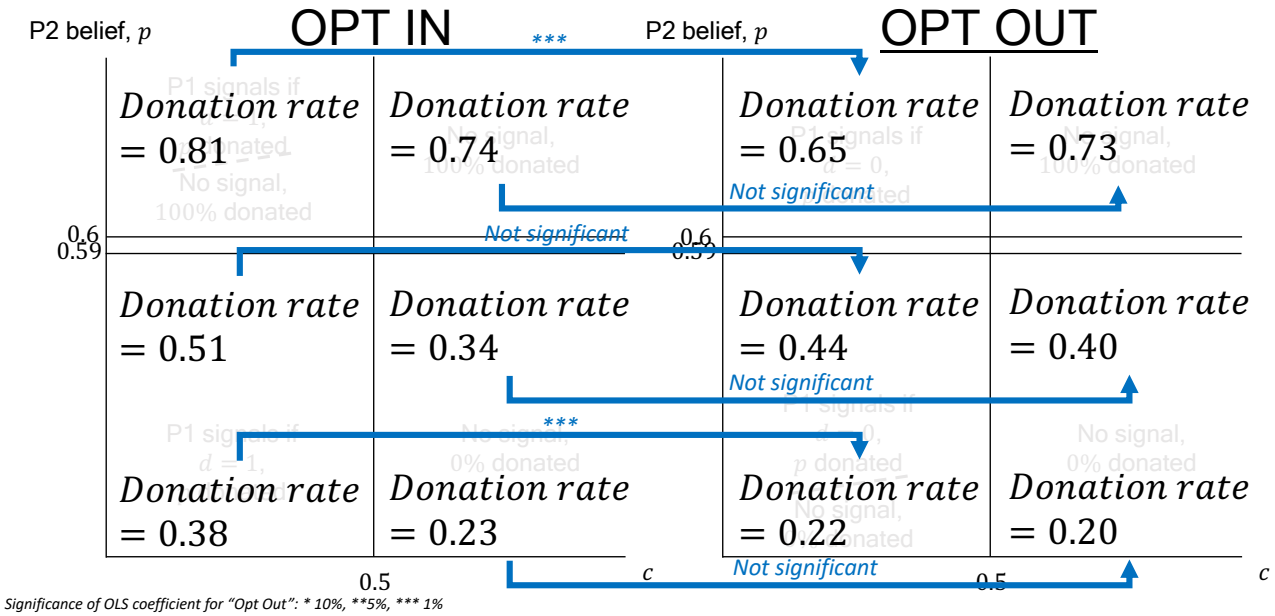
Starting from a weak consent regime ($\delta = \$0.01$) where the cost of signaling is low ($c = \$0.25$), theory predicts that at high Player 2 priors ($p = 0.9$), as we move from opt in to opt out, we will be moving from the multiple equilibria where donation rates are at p or 100% to an equilibrium where the donation rate is p. Thus, we expect the donation rate to weakly decrease (from opt in to opt out). Experimental data indicate that under these parameters the average donation under opt in is 0.81 while average donation under opt out is 0.65 with a significant drop of 0.15 (significant at the 1% level) (see Column (21) of Table III).

When Player 2 priors are middling ($p = 0.5$) or low ($p = 0.1$), we will be moving from an equilibrium where the donation rate is p to the multiple equilibria where donation rates are at p or 0%. Thus, we expect the donation rate to weakly decrease (from opt in to opt out). Experiment data indicate that under these parameters the average donation under opt in is 0.51 for $p = 0.5$ and 0.38 for $p = 0.1$ while average donation under opt out is 0.44 for $p = 0.5$ and 0.22 for $p = 0.1$. These represent a change that is not significantly different from zero for $p = 0.5$ (see Column (17) of Table III) and a significant drop of 0.16 for $p = 0.1$ (significant at the 1% level) (see Column (13) of Table III).

In contrast to the case of low cost of signaling, the theoretical predictions and results are different when the cost of signaling is high ($c = \$0.75$). Theory predicts that at high Player 2 priors ($p = 0.9$), we are moving between equilibria where the donation rates are both 100% when switching from opt in to opt out. And when $p = 0.5$ or $p = 0.1$, we are moving between equilibria where the donation rates are all expected to be 0%. Thus, we expect the donation rate to remain unchanged (from opt in to opt out). Experimental data indicate that under these parameters the average donation rates under opt in are 0.74, 0.34, and 0.23 for $p = 0.9$, $p = 0.5$, and $p = 0.1$; while average donation rates under opt out are 0.73, 0.40, and 0.20 respectively. These represent no significant change at conventional levels (see Columns (23), (19), and (15) of Table III).

Under a weak consent regime ($\delta = \$0.01$), a switch to opt out holding other parameters constant leads to either a decrease or no significant changes to donation rates. These results are summarized in Figure X.

Figure X: Impact of Switching to Opt Out (Weak Consent)



Notes: This figure presents the average donation rate and the significance of estimated parameter from the estimation model $Donation_i = \beta_0 + \beta_1 OptOut_i + \epsilon_i$*, on different parts of the parameter space as presented in Figure VI. The data is from the average of OLS coefficients and standard errors estimated across* $1,000$ *permutations.*

5.2 Relative Advantages of Opt In and Strict Opt Out Depend on Cost of Signal and Priors

Now, let's consider a strict consent regime ($\delta = \$0.19$) where the cost of signaling is low ($c = \$0.25$), theory predicts that at high Player 2 priors ($p = 0.9$), as we move from opt in to opt out, we will be moving from the multiple equilibria where donation rates are at p or 100% to an equilibrium where the donation rate is p. Thus, we expect the donation rate to weakly decrease (from opt in to opt out). Experimental data indicate that under these parameters the average donation under opt in is 0.78 while average donation under opt out is 0.66 with a significant drop of 0.12 (significant at the 5% level) (see Column (22) of Table III). When Player 2 priors are low ($p = 0.1$), we will be moving from an equilibrium where the donation rate is p to the multiple equilibria where donation rates are at p or 0%. Thus, we expect the donation rate to weakly decrease (from opt in to opt out). Experimental data indicate that under these parameters the average donation under opt in is 0.35 for $p = 0.1$ while average donation under opt out is 0.30 for $p = 0.1$.

This represents a change that is not significantly different from zero for $p = 0.1$ (see Column (14) of Table III).

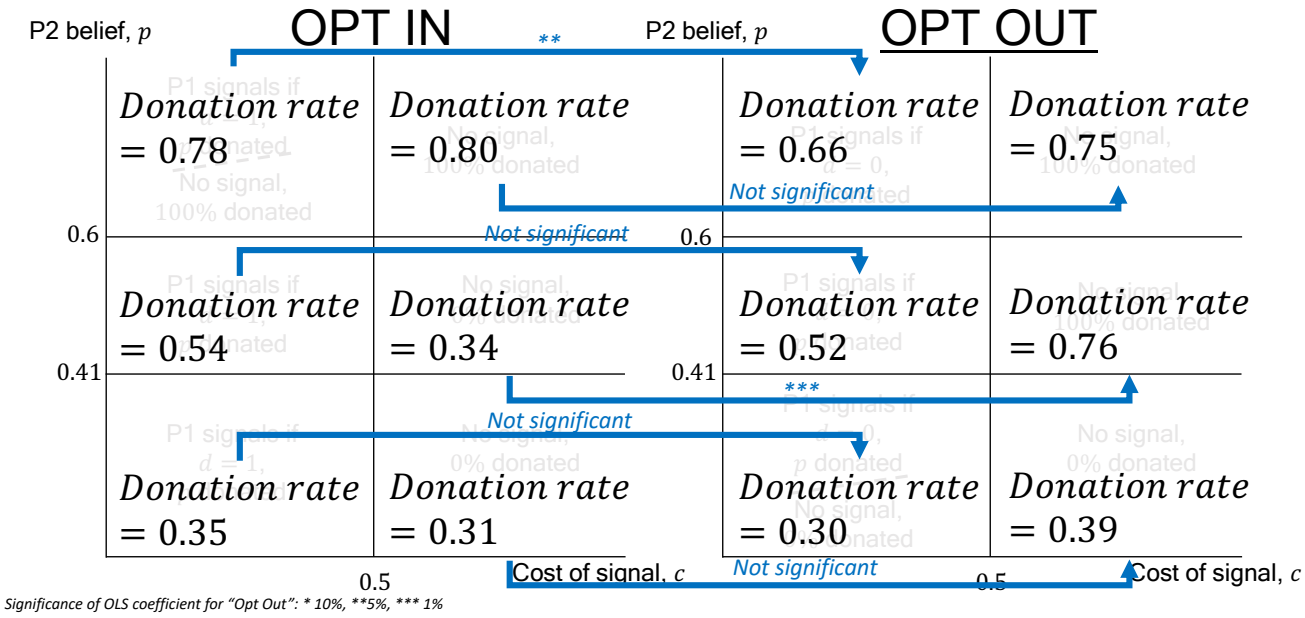
In the case of low cost of signaling ($c = \$0.25$) and Player 2 priors are middling ($p = 0.5$), we are moving between equilibria where the donation rates are both p when switching from opt in to opt out. Experimental data indicate that under these parameters the average donation rate under opt in are 0.54 while average donation rates under opt out is 0.52. These represent no significant changes at conventional levels (see Column (18) of Table III).

In the case of high cost of signaling ($c = \$0.75$) and Player 2 priors are high ($p = 0.9$) or low ($p = 0.1$), we are moving between equilibria where the donation rates are both 100% or 0% when switching from opt in to opt out. Thus, we expect the donation rate to remain unchanged (from opt in to opt out). Experimental data indicate that under these parameters the average donation under opt in is 0.80 for $p = 0.9$ and 0.31 for $p = 0.1$ while average donation under opt out is 0.75 for $p = 0.9$ and 0.39 for $p = 0.1$. These represent changes that are not significantly different from zero at conventional levels (see Columns (24) and (16) of Table III).

Finally, in the case of high cost of signaling ($c = \$0.75$) and middling Player 2 priors ($p = 0.5$), we move from an equilibrium with very low donation rate (0%) to an equilibrium with very high donation rate (100%). The experimental data bear out the theoretical prediction, as donation rate jumped from 0.34 to 0.76, an increase of 0.42 that is significant at the 1% level (see Column (20) of Table III).

Under a strict consent regime ($\delta = \$0.19$), a switch to opt out holding other parameters constant mostly leads to either a decrease or no significant change to donation rates, except for high cost of signaling ($c = \$0.75$) and middling Player 2 priors when switching to opt out strictly increases donation. These results are summarized in Figure XI.

Figure XI: Impact of Switching to Opt Out (Strict Consent)



Notes: This figure presents the average donation rate and the significance of estimated parameter from the estimation model $Donation_i = \beta_0 + \beta_1 OptOut_i + \epsilon_i$*, on different parts of the parameter space as presented in Figure VI. The data are from the average of OLS coefficients and standard errors estimated across* $1,000$ *permutations.*

The experimental design bears on how these results should be interpreted. The experiment is built to test a mechanism rather than to measure its magnitude. Replacing human organs with a neutral asset holds preferences and payoffs fixed, varying only the informational environment, which is what allows the comparative statics above to be attributed to the signaling channel rather than to shifting attitudes toward donation. The emotional and moral weight families bring to an actual authorization decision is deliberately absent, as are the circumstances under which it is made. We therefore read these results as evidence that the asymmetry the model identifies operates in behavior, and in the predicted direction, rather than as an estimate of how much a consent reform would change donation rates in any particular country.

6 Discussion and Conclusion

6.1 What Should Countries Do?

The findings presented in this paper offer crucial insights for policymakers grappling with strategies to increase organ donation rates. While intuitive behavioral nudges might suggest that a simple transition to an opt-out system would universally boost donation, our model and experimental results demonstrate a more nuanced reality. The analysis indicates that, holding other parameters constant, a shift to an opt-out regime in many circumstances leads to either a decrease or no statistically significant change in donation rates. A positive and significant increase in donation under opt-out is primarily observed under specific conditions: high costs of signaling and middling Player 2 priors, particularly within a strict opt-out framework, as shown in our experimental results.

Recent international experiences appear to corroborate these findings. For instance, England and Wales transitioned to a weak opt-out system, with England adopting the Organ Donation (Deemed Consent) Act in May 2020, following a similar move by Wales in 2015. This change was motivated by the persistent gap between public support for donation and the number of registered donors. However, the impact on donation rates has been debated, and evidence suggests that a simple switch in the default may not be the solution. The National Health Service has recognized the signaling value of opting in.[25] England and Wales have recently reintroduced the opt-in approach on top of their opt-out regime, which in practice already resembled an opt-in system—except in cases where family members or next of kin could not be located.

Using this framework requires translating a country's institutions and societal norms into the model's parameters. The cost of signaling (c) is a feature of registry design. The cost of overturning consent (δ) is set by law and by procurement practice together. It is this parameter that separates strict from weak opt out, and, as the observational evidence in Section 2 suggests, it does more to determine outcomes than the legal regime itself. The family's prior (p) reflects how likely families are to believe that their relative wished to donate.

It is also imperative to recognize that the introduction of an opt-out policy can itself influence the very parameters that determine its effectiveness. A change in the default

[25]The first author collaborates with the NHS team on organ donation and transplantation, including work based on the evidence reported now in this paper.

can coincide with or even trigger shifts in social norms and family priors regarding organ donation. Therefore, as countries consider policy changes, it is essential to analyze the potential impact of the entire package of reforms, not just the default rule, on factors such as the cost of signaling, Player 2 beliefs, and the strictness of consent. Policymakers should consider how these interrelated factors might shift, moving the outcomes from one region to another within the parameter spaces illustrated in Figures X and XI.

With appropriate caveats regarding the potential for dynamic shifts in underlying parameters, our results offer a flexible and informative framework to guide policy decisions aimed at increasing organ supply and saving lives. As countries like the United States continue to debate these policy choices (Chan (2020)), our findings suggest that careful consideration of the specific context, including existing social norms, family dynamics, and the infrastructure supporting organ procurement and transplantation, is paramount. Simplistic reliance on a behavioral nudge without considering the broader economic and social context may fail to yield the desired increase in donation rates.

6.2 Conclusion

This paper investigates the complex effects of consent defaults in organ donation, combining new cross-country evidence, a formal theoretical framework, and incentivized laboratory experiments to provide a unified understanding of how legal defaults, signaling frictions, and institutional context jointly determine organ supply outcomes.

Using a newly constructed cross-country panel dataset and an event study design, we document that presumed consent reforms are followed by higher donation rates only when strictly enforced — that is, when family veto power is substantially constrained. In weak enforcement environments, by contrast, switching from opt-in to opt-out is often followed by no improvement and sometimes even by lower donation rates. This heterogeneity motivates the need for a deeper theoretical understanding.

To address this, we develop a signaling model in which potential donors can pay a cost to explicitly register their preferences, and surviving family members must decide whether to authorize donation under uncertainty. The model highlights an important asymmetry: under opt-in, willing donors can affirmatively signal their willingness to donate; under opt-out, by contrast, unwilling donors have the primary opportunity to signal. When signals are not too costly, opt-in thus yields at least as many donations as opt-out.

However, when signaling is prohibitively costly, family authorization decisions hinge on priors and default costs, and opt-out can increase donations if consent enforcement is strict enough to make overturning presumed consent meaningfully costly.

These predictions are tested in a laboratory experiment that systematically varies the default regime, signaling costs, and the cost of overriding defaults. Experimental results closely match theoretical predictions. Under weak consent, opt in weakly outperforms opt out in every cell where the difference is statistically significant. Under strict consent, opt-out can substantially increase donation rates—but only when signaling costs are high and family beliefs about donor willingness are intermediate.

Taken together, the findings caution against the simplistic view that switching to presumed consent will universally increase organ donation. The effectiveness of opt-out depends critically on the broader enforcement environment, on the behavioral cost of signaling, and on the prior beliefs held by families at the time of decision-making. In many realistic settings, preserving or strengthening opt-in systems—particularly those that facilitate and encourage explicit registration—may yield higher donation rates than shifting to opt-out. Conversely, where strict enforcement is possible and signaling frictions are large, opt-out can play an important role in increasing organ supply.

More generally, this research contributes to broader themes in market design and behavioral economics, illustrating how the interaction between legal defaults, costly signaling, and noisy proxy decision-making can produce heterogeneous policy outcomes. Policymakers should be wary of "one-size-fits-all" prescriptions: optimal policies must be tailored to the institutional, legal, and behavioral context of each country. Future research could explore how changes in default rules might endogenously shift social norms and family beliefs over time, and how complementary interventions—such as public education or incentives for explicit registration—can be layered onto default reforms to maximize their effectiveness.

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A Online Appendix

This is the appendix for "Opt In? Opt Out?" by Alex Chan, Ayush Gupta, and Yetong Xu.

A.1 Event Study

A.1.1 Data sources

Global data on the number of deceased organ donors are collected from the International Registry in Organ Donation and Transplantation, and the Global Observatory on Donation and Transplantation. Organ donation and transplantation data for Austria, Belgium, Germany, Luxembourg, and the Netherlands are provided by Agita Strelniece from EuroTransplant. Data on organ donation and transplantation for Denmark, Norway, Sweden, and Finland are provided by Anne Ørskov Boserup from ScandiaTransplant. NHS Blood and Transplant provides the UK's organ donation and transplantation data. Organ donation and transplantation data for France, Portugal, Thailand are given by Michel Tsimaratos from Agence de la biomédecine, Portuguese Blood and Transplantation Institute, and Supanit Nivatvongs from Thai Red Cross Organ Donation Centre, respectively. After compiling all the data we collected, we create a comprehensive organ donation and transplant dataset with around 94 countries across six continents.

We collected data on GDP per capita, total population, health expenditure per capita, birth rate and death rate from the World Bank's World Development indicators. We compiled the World Health Organization Mortality Database to get the number of deaths caused by traffic accidents (MVA deaths) and cerebro-vascular failures (CVD deaths). We obtained the religious beliefs and legal systems from the CIA's World Factbook (mainly refer to four versions: 2024, 2019, 2009, 2000).

Considering the influence of COVID-19, we decided to use the data prior to 2020 for Figure I and Table I. For Figure I, we included all 77 unique countries which have data in 2019. For Table I, there are 90 countries which have data from 2006 to 2019, among them, there are 71 countries having 10 or more observations. Thus, we dropped Algeria, Bosnia and Herzegovina, China, Jordan, Kazakhstan, Moldova, Mongolia, Montenegro, Morocco, Myanmar, Nicaragua, North Macedonia, Oman, Puerto Rico (US), Serbia, Trinidad and Tobago, Ukraine, United Arab Emirates, Vietnam. Due to the MVA and

CVD Deaths data missing problem, we also dropped Belarus, Bolivia, India, Iran (Islamic Rep.), Lebanon, Russian Federation, Saudi Arabia, Taiwan, Tunisia. In the end, we totally used 79 unique countries in our sample for Figure I and Table I and we collect data on legislative defaults on deceased organ donations for all these countries, as listed in Appendix A.1.2

The 62 countries used in Table I are: Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Costa Rica, Croatia, Cuba, Cyprus, Czechia, Denmark, Dominican Republic, Ecuador, Estonia, Finland, France, Germany, Greece, Guatemala, Hong Kong SAR China, Hungary, Iceland, Ireland, Israel, Italy, Japan, Korea Rep., Kuwait, Latvia, Lithuania, Luxembourg, Malaysia, Malta, Mexico, Netherlands, New Zealand, Norway, Panama, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Singapore, Slovak Republic, Slovenia, South Africa, Spain, Sweden, Switzerland, Thailand, Turkiye, United Kingdom, United States, Uruguay, Venezuela.

A.1.2 Legislative defaults

Table IV: Organ Donation Consent Policies by Country (2006 - 2019)

Country	**Policy in 2019**	**Policy Change**[26]	**Evidence**
Algeria	Opt-out	Opt-in → Opt-out (2018)	Benmansour (2025); Cherdoud et al. (2025); Algérie1[27]
Argentina	Opt-out	—	Dallacker et al. (2024); Gondolesi et al. (2018)

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Country	Policy in 2019	Policy Change	Evidence
Australia	Opt-in	—	Shepherd et al. (2014); Rosenblum et al. (2012); Library of Parliament, HillNotes[28]; DonateLife[29]
Austria	Opt-out	—	Abadie and Gay (2006); Eurotransplant[30]; Federal Act from European Federation for Organ Donation (EFOD)[31]
Belarus	Opt-out	—	Rosenblum et al. (2012); Library of Parliament, HillNotes[28]; Euroradio[32]
Belgium	Opt-out	—	Abadie and Gay (2006); Federal Act from EFOD[31]
Bolivia	Opt-in	—	Mizraji et al. (2007); Contreras et al. (2020); Law No. 1716 (1996)[33]

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Table IV – continued from previous page

Country	Policy in 2019	Policy Change	Evidence
Brazil	Opt-in	—	Csillag (1998); Rosenblum et al. (2012); Garcia et al. (2024)
Bulgaria	Opt-out	—	Abadie and Gay (2006); Scholz (2020); Dallacker et al. (2024); Federal Act from EFOD[31]
Canada	Opt-in	—	Abadie and Gay (2006); Rosenblum et al. (2012); Library of Parliament, HillNotes[28]
Chile	Opt-out	Opt-in → Opt-out (2010)	Domínguez and Rojas (2013); Steffel et al. (2019); Aguirre-Villarreal et al. (2023)
China	Opt-in	—	Zeng et al. (2023)
Colombia	Opt-out	—	Mizraji et al. (2007); Rosenblum et al. (2012); Aguirre-Villarreal et al. (2023)
Costa Rica	Opt-in	Opt-out → Opt-in (2014)	Aguirre-Villarreal et al. (2023)
Croatia	Opt-out	—	Abadie and Gay (2006); Rosenblum et al. (2012); Federal Act from EFOD[31]

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Table IV – continued from previous page

Country	Policy in 2019	Policy Change	Evidence
Cuba	Opt-in	—	Mizraji et al. (2007); Rosenblum et al. (2012); Reed (2016)
Cyprus	Opt-in	Opt-out → Opt-in (2012)	Abadie and Gay (2006); Law N. 97/1987 [34]; Law N. 127(I)/2012 [35]; Scholz (2020)
Czechia	Opt-out	—	Abadie and Gay (2006); Rosenblum et al. (2012); Federal Act from EFOD[31]
Denmark	Opt-in	—	Abadie and Gay (2006); Rosenblum et al. (2012); Library of Parliament, HillNotes[28]; Federal Act from EFOD[31]
Dominican Republic	Opt-out	—	Mizraji et al. (2007); da Silva et al. (2024)
Ecuador	Opt-out	—	Mizraji et al. (2007); Rosenblum et al. (2012); Library of Parliament, HillNotes[28]; Huertas et al. (2020)
Estonia	Opt-out	—	Abadie and Gay (2006); Federal Act from EFOD[31]; Scholz (2020)

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Table IV – continued from previous page

Country	Policy in 2019	Policy Change	Evidence
Finland	Opt-out	—	Abadie and Gay (2006); Rosenblum et al. (2012); Federal Act from EFOD[31]
France	Opt-out	—	Abadie and Gay (2006); Rosenblum et al. (2012); Federal Act from EFOD[31]
Germany	Opt-in	—	Abadie and Gay (2006); Rosenblum et al. (2012); Library of Parliament, HillNotes[28]; Federal Act from EFOD[31]
Greece	Opt-out	—	Canellopoulou-Bottis (2000); Abadie and Gay (2006); Library of Parliament, HillNotes[28]; Scholz (2020)
Guatemala	Opt-in	—	Mizraji et al. (2007); Shepherd et al. (2014); da Silva et al. (2024)
Hong Kong (China)	Opt-in	—	Human Organ Transplant Ordinance, Cap. 465[36]; Shepherd et al. (2014)

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Table IV – continued from previous page

Country	Policy in 2019	Policy Change	Evidence
Hungary	Opt-out	—	Abadie and Gay (2006); Shepherd et al. (2014); Federal Act from EFOD[31]
Iceland	Opt-out	Opt-in → Opt-out (2018)	Rosenblum et al. (2012); Iceland Review[37]; INEDYTO[38]
India	Opt-in	—	Transplantation of Human Organs and Tissues Act (1994)[39]; Rosenblum et al. (2012); Lobo Gajiwala (2018); da Silva et al. (2024)
Iran	Opt-in	—	Ghods (2014); Kiani et al. (2018); da Silva et al. (2024); Brain Death and Organ Transplantation Act[40]
Ireland	Opt-in	—	Abadie and Gay (2006); Rosenblum et al. (2012); Scholz (2020); Federal Act from EFOD[31]

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Table IV – continued from previous page

Country	Policy in 2019	Policy Change	Evidence
Israel	Opt-in	—	Scott and Jacobson (2007); Rosenblum et al. (2012); Zaltzman (2018)
Italy	Opt-out	—	Gevers et al. (2004); Rosenblum et al. (2012); Library of Parliament, HillNotes[28]
Japan	Opt-in	—	Rosenblum et al. (2012); Japan Organ Transplant Network[41]
Kuwait	Opt-in	—	Rosenblum et al. (2012); AlSulaiman et al. (2021)
Latvia	Opt-out	—	Federal Act from EFOD[31]; Abadie and Gay (2006); Scholz (2020)
Lithuania	Opt-in	—	Federal Act from EFOD[31]; Abadie and Gay (2006); Rosenblum et al. (2012); Scholz (2020)
Luxembourg	Opt-out	—	Abadie and Gay (2006); Scholz (2020); Eurotransplant[30]
Malaysia	Opt-in	—	Rosenblum et al. (2012); Khoo and Ballesté (2024)

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Country	Policy in 2019	Policy Change	Evidence
Malta	Opt-in	—	Rosenblum et al. (2012); Ellul (2013); Library of Parliament, HillNotes[28]
Mexico	Opt-in	—	Mizraji et al. (2007); Rosenblum et al. (2012); da Silva et al. (2024); Library of Parliament, HillNotes[28]
Moldova	Opt-out	—	Law No 473-XIV on Transplantation of Human Organs, Tissues and Cells (1999)[42]; Codreanu (2015); Arseni (2023)
Mongolia	Opt-in	—	Mongolian Organ Transplantation Association[43]; Gonchigjav et al. (2022)
Morocco	Opt-in	—	Moutabari3[44]
Netherlands	Opt-in	—	Abadie and Gay (2006); Rosenblum et al. (2012); Library of Parliament, HillNotes[28]

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Country	Policy in 2019	Policy Change	Evidence
New Zealand	Opt-in	—	Abadie and Gay (2006); Rosenblum et al. (2012); Organ Donation New Zealand Annual Report 2019[45]
Nicaragua	Opt-in	—	Agence France-Presse[46]; da Silva et al. (2024)
North Macedonia	Opt-in	—	Deutsches Referenzzentrum für Ethik in den Biowissenschaften[47]; Andonovska et al. (2025)
Norway	Opt-out	—	Library of Parliament, HillNotes[28]; Abadie and Gay (2006); Rosenblum et al. (2012)
Panama	Opt-out	—	Mizraji et al. (2007); Shepherd et al. (2014); da Silva et al. (2024)
Paraguay	Opt-out	—	Mizraji et al. (2007); Rosenblum et al. (2012); da Silva et al. (2024); The Asuncion Times[48]

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Country	Policy in 2019	Policy Change	Evidence
Peru	Opt-in	—	Contreras et al. (2020); Law on Donation and Transplantation of Human Organ and Tissues (2004) [49]
Philippines	Opt-in	—	Organ Donation Act of 1991 (Republic Act No. 7170)[50]; Rosenblum et al. (2012); Pizarra (2025)
Poland	Opt-out	—	Abadie and Gay (2006); Rosenblum et al. (2012); Library of Parliament, HillNotes[28]
Portugal	Opt-out	—	Abadie and Gay (2006); Shepherd et al. (2014); Federal Act from EFOD[31]; Library of Parliament, HillNotes[28]
Qatar	Opt-in	—	Law No. 21 (1997)[51]; Law No. 15 (2015)[52]; Al-Sulaiman et al. (2021)

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Table IV – continued from previous page

Country	Policy in 2019	Policy Change	Evidence
Romania	Opt-in	—	Abadie and Gay (2006); Rosenblum et al. (2012); Cotrau et al. (2024); Federal Act from EFOD[31]
Russia	Opt-out	—	Rosenblum et al. (2012); Library of Parliament, HillNotes[28]
Saudi Arabia	Opt-in	—	Al Sayyari (2008); Rosenblum et al. (2012); AlSulaiman et al. (2021)
Serbia	Opt-out	Opt-out → Opt-in (2009); Opt-in → Opt-out (2018)	Baturan and Samardžić (2013); Beyond Emerging Europe[53]; Ninkov (2025)
Singapore	Opt-out	—	Rosenblum et al. (2012); Steffel et al. (2019); Lu (2025)
Slovak Republic	Opt-out	—	Abadie and Gay (2006); Rosenblum et al. (2012); Federal Act from EFOD[31]
Slovenia	Opt-out	—	Abadie and Gay (2006); Rosenblum et al. (2012); Eurotransplant[30];

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Country	Policy in 2019	Policy Change	Evidence
South Africa	Opt-in	—	Rosenblum et al. (2012); Slabbert and Venter (2019)
South Korea	Opt-in	—	Rosenblum et al. (2012); Kim et al. (2020); Organ Transplant Act (2013)[54]
Spain	Opt-out	—	Abadie and Gay (2006); Rosenblum et al. (2012); Federal Act from EFOD[31]
Sweden	Opt-out	—	Abadie and Gay (2006); Rosenblum et al. (2012); Federal Act from EFOD[31]
Switzerland	Opt-in	—	Rosenblum et al. (2012); Library of Parliament, HillNotes[28]; Transplantation Act (2004)[55]
Taiwan (China)	Opt-in	—	da Silva et al. (2024); Lu (2025)
Thailand	Opt-in	—	Rosenblum et al. (2012); Cowie et al. (2023)

Continued on next page

[54]Organ Transplant Act (2013). Available at: https://elaw.klri.re.kr/eng_mobile/viewer.do?hseq=29060&type=part&key=36

[55]Federal Act on the Transplantation of Organs, Tissues and Cells (2004). Available at: https://www.freedomofresearch.org/wp-content/uploads/2019/11/5-CC-810.21-Federal-Act-of-8-October-2004-on-the-Transplantation-of-Organs-Tissues-and-Cells-Loi-sur-la-transplantation.pdf

Table IV – continued from previous page

Country	Policy in 2019	Policy Change	Evidence
Trinidad & Tobago	Opt-in	—	Mohammed et al. (2021); Human Tissue Transplant Act (2000)[56]
Turkey	Opt-in	—	Haberal et al. (2020); Sevimli (2022); da Silva et al. (2024)
United Arab Emirates	Opt-in	—	Al Sayyari (2008); AlSulaiman et al. (2021)
United Kingdom	Opt-in	—	Abadie and Gay (2006); Rosenblum et al. (2012); Library of Parliament, HillNotes[28]
United States	Opt-in	—	Abadie and Gay (2006); Rosenblum et al. (2012); Library of Parliament, HillNotes[28]
Uruguay	Opt-out	Opt-in → Opt-out (2013)	Aguirre-Villarreal et al. (2023); Dallacker et al. (2024)
Venezuela	Opt-out	Opt-in → Opt-out (2011)	da Silva et al. (2024); El Diario[57]; BBC News[58]

[56]Human Tissue Transplant Act (2000). Available at: `https://rgd.legalaffairs.gov.tt/laws2/alphabetical_list/lawspdfs/28.07.pdf`

[57] Fiorella Tagliafico. (2024). Ser donante vivo de órganos en Venezuela: ¿cómo es el proceso?. Available at: `https://eldiario.com/2024/06/06/donante-vivo-de-organos-en-venezuela-el-proceso/`

[58]D.G. Cappa. El dramático limbo de las personas que esperan un órgano en Venezuela, donde el sistema de trasplantes lleva años suspendido (2022). Available at: `https://www.bbc.com/mundo/noticias-america-latina-60470404`

A.1.3 Evidence of Strict or Weak Opt Out

For the seven treatment countries analyzed in our event study in Section 2.3, we rely primarily on direct evidence from physicians or surgeons who operated during the years surrounding the policy shift. Where such direct evidence is unavailable, we instead draw on published research studies.

- Austria: Strict opt-out country by Mehlman (1991), who had personal communication with an Austrian transplant coordinator in 1990 that "One true presumed consent system in Europe is found in Austria. A patient who does not wish to donate organs must state his objection in writing. Donation is not discussed with families unless they raise the issue." And several other secondary sources corroborates the strictness of the opt-out.

- Belgium: Strict opt-out country by Michielsen (1996) that "As a rule when death is notified, the family is informed of the intention to proceed with organ removal, but explicit permission is seldom asked. This information is usually given by the doctor in charge and not by the transplant coordinator, whose role is often limited to technical and administrative support."

- Chile: Weak opt-out country by Domínguez and Rojas (2013) that "In our society the family plays a crucial role. They are always approached for consent for organ donation making the real applicability of the law debatable."

- Finland: Weak opt-out country by Williams (1994) that "Some countries which have laws based on presumed consent such as Finland, Greece, Italy, Norway, Spain, and Sweden, also insist that physicians consult with the deceased's relatives."

- Luxembourg: Strict opt-out country by Bilgel (2012) that "Family Veto is not allowed." Also, interviewed organ transplantation professionals in Western Europe concurs with this view for the time period of policy change.

- Sweden: Weak opt-out country by Gevers et al. (2004) that "If the will of the deceased person is not known, removal of organs is in principle allowed. However, the next of kin have a legal right to refuse, and they have to be informed in advance of the removal and their right to oppose. We found no evidence that actual practice was not in accordance with this legal regime."

- Uruguay: Weak opt-out country by Contreras et al. (2020) that "Family consultation is needed."

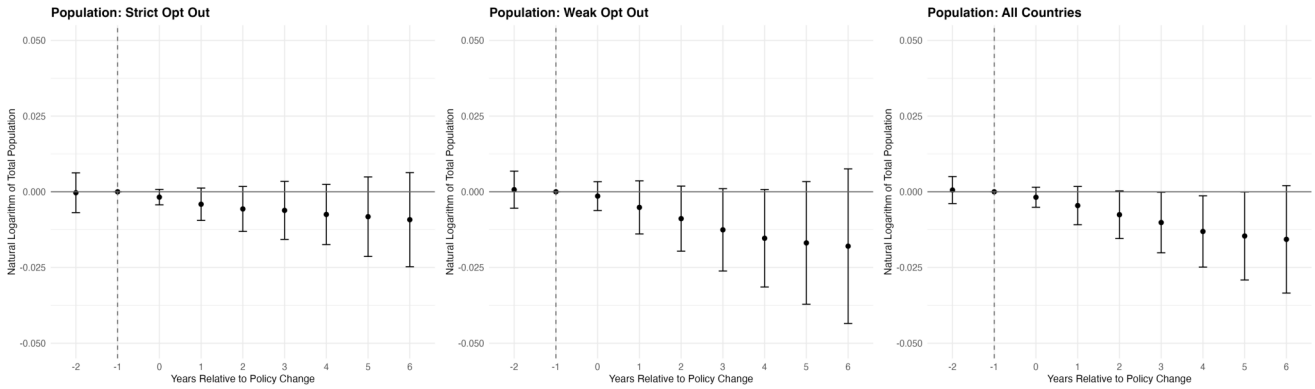
A.1.4 Placebo Tests

As a robustness check, we conduct placebo tests using outcomes that should not be affected by changes in the regime. Figures A1, A2, A3, A4, and A5 report event-study estimates for total population, total deaths, death rate from motor vehicle accidents (MVA), death rate from cerebro-vascular failures (CVD), and health spending. Note that, since health expenditure per capita and GDP per capita are highly collinear and health expenditure accounts for around 10% of GDP constantly (Abadie and Gay (2006)), we use GDP per capita as the outcome variable for the placebo test on health spending.

The same set of countries and years is used as in the original strict opt-out (Figure A97), weak opt-out (Figure A98), and all-country (Figure A96) event-study plots. Except for the weak opt-out MVA and CVD death rate specifications, where Bolivia (2008-2019), Dominican Republic (2019), Poland (1997-1998), UK (2000), and Uruguay (2011) are excluded due to missing data[59].

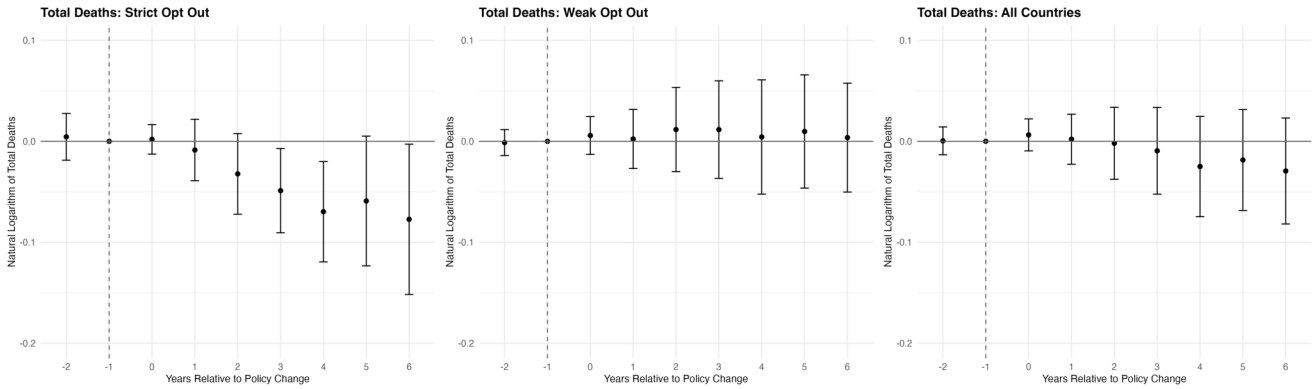
[59] As a result, for weak opt-out MVA and CVD death rate specifications, the sample includes Finland*, Germany, Netherlands, Norway, and UK (1983-1991). The sample also includes Sweden* (1994-2002), Greece (1995-2002), Poland (1994-1996, 1999-2002), UK (1994-1999, 2001-2002), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Portugal, Slovak Republic, Slovenia, Spain, Switzerland (1994-2002), and Chile* (2008-2016), Uruguay* (2012-2019), Dominican Republic (2008-2018), Ecuador (2009-2019), Argentina, Brazil, Colombia, Cuba, Panama, Paraguay (2008-2019). Countries marked with an asterisk * changed policy nationally from opt-in to opt-out;

Figure A1: Placebo Test on Total Population



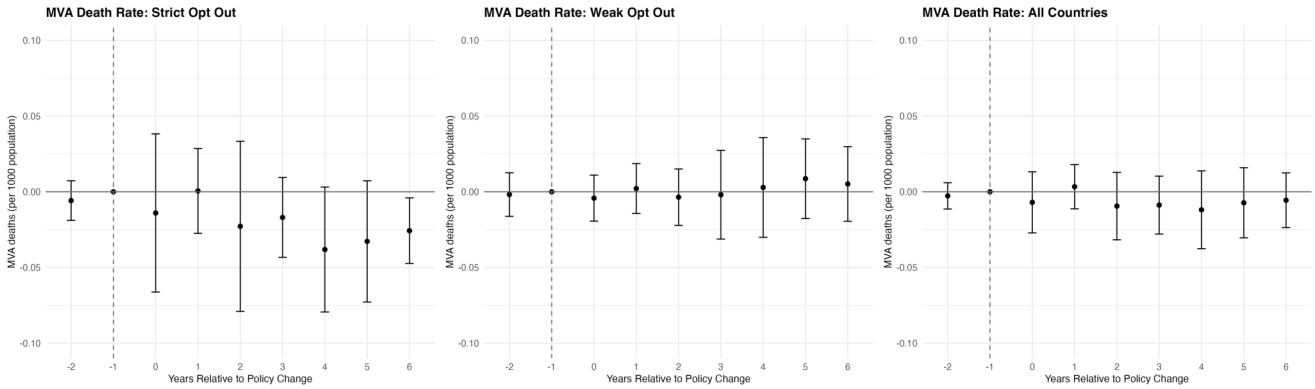
Notes: This plot presents event-study coefficients relative to the year of the policy change, separately for strict opt-out countries, weak opt-out countries, and the pooled sample. Estimates are based on Equation 1, but with the natural logarithm of total population replacing the deceased donation rate per 100 deaths. Points show estimated coefficients; error bars indicate 95% confidence intervals.

Figure A2: Placebo Test on Total Deaths



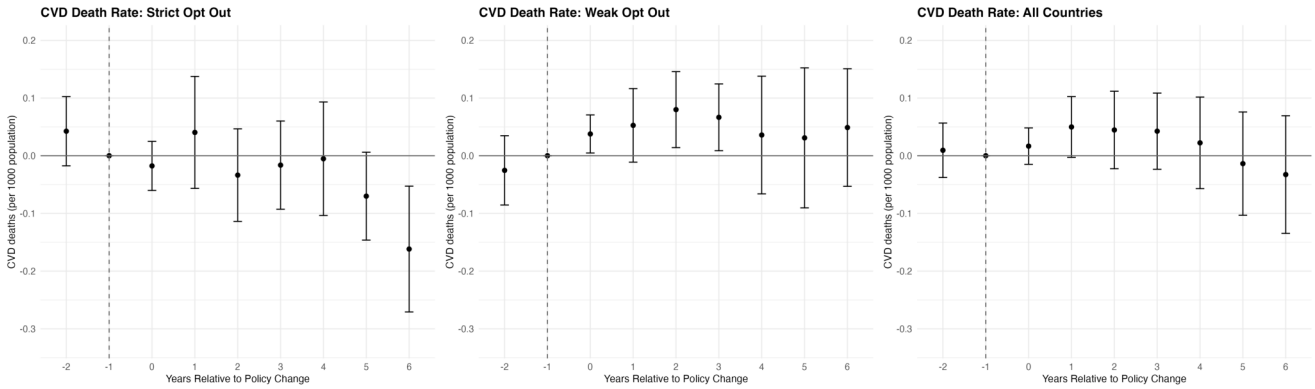
Notes: This plot presents event-study coefficients relative to the year of the policy change, separately for strict opt-out countries, weak opt-out countries, and the pooled sample. Estimates are based on Equation 1, but with the natural logarithm of total deaths replacing the deceased donation rate per 100 deaths. Points show estimated coefficients; error bars indicate 95% confidence intervals.

Figure A3: Placebo Test on MVA Deaths Rate



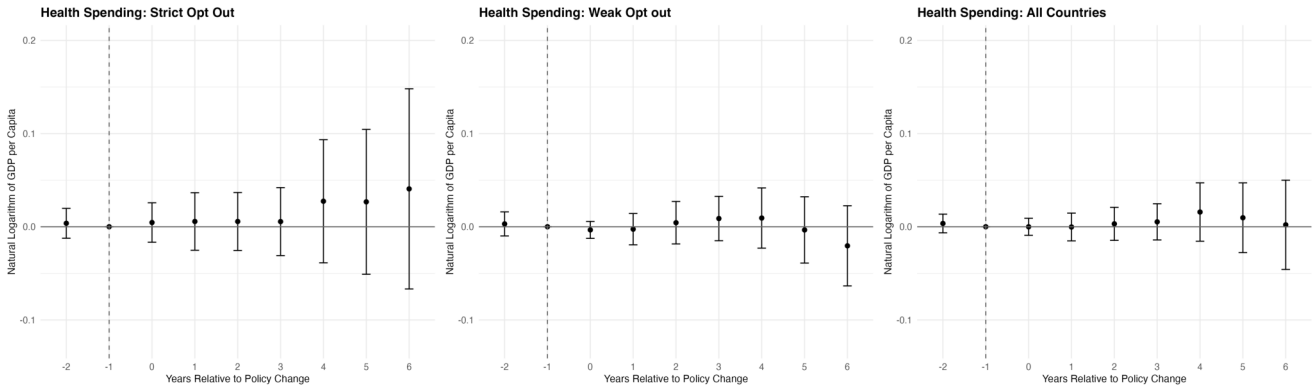
Notes: This plot presents event-study coefficients relative to the year of the policy change, separately for strict opt-out countries, weak opt-out countries, and the pooled sample. Estimates are based on Equation 1, but with the MVA deaths per 1000 population replacing the deceased donation rate per 100 deaths. Points show estimated coefficients; error bars indicate 95% confidence intervals.

Figure A4: Placebo Test on CVD Deaths Rate



Notes: This plot presents event-study coefficients relative to the year of the policy change, separately for strict opt-out countries, weak opt-out countries, and the pooled sample. Estimates are based on Equation 1, but with the CVD deaths per 1000 population replacing the deceased donation rate per 100 deaths. Points show estimated coefficients; error bars indicate 95% confidence intervals.

Figure A5: Placebo Test on Health Spending

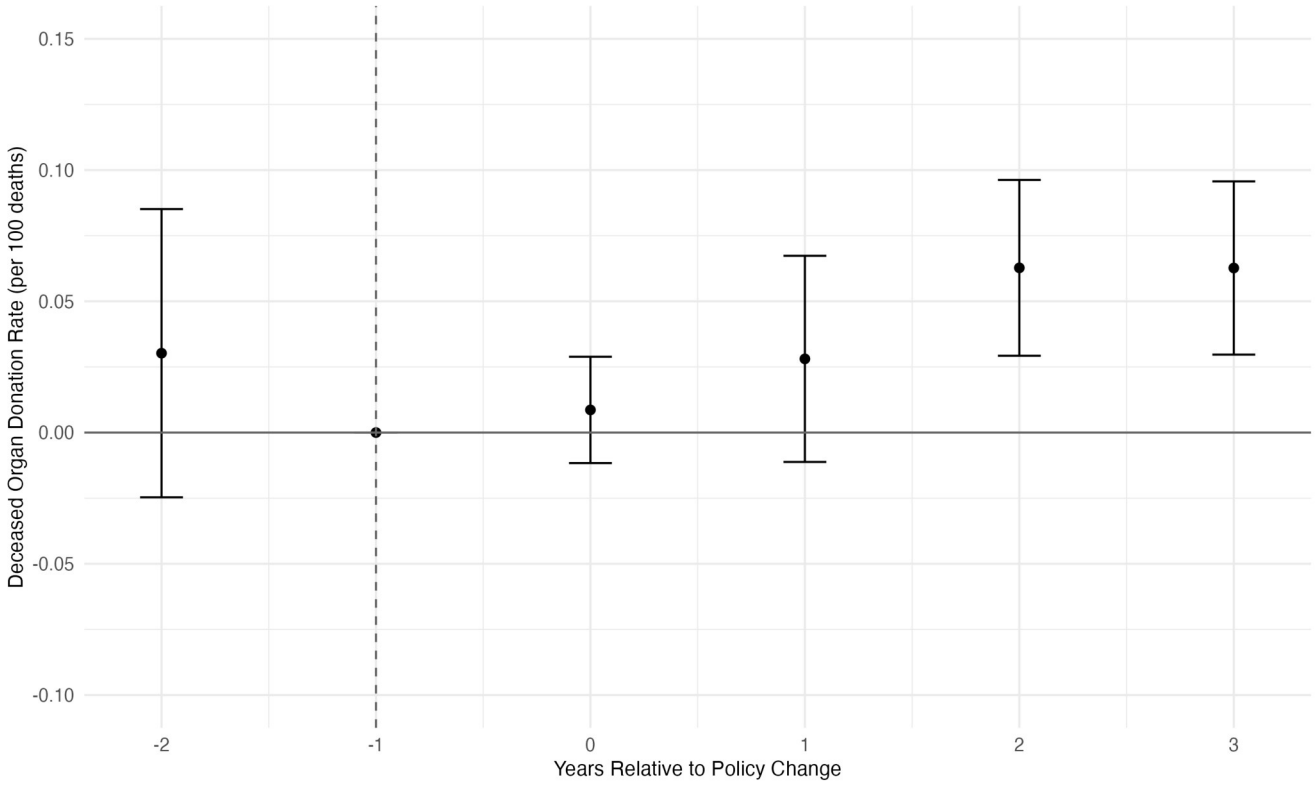


Notes: This plot presents event-study coefficients relative to the year of the policy change, separately for strict opt-out countries, weak opt-out countries, and the pooled sample. Estimates are based on Equation 1, but replace the deceased donation rate per 100 deaths with the natural logarithm of GDP per capita, which is used here as a proxy for health spending. Points show estimated coefficients; error bars indicate 95% confidence intervals.

In all specifications, we find no systematic or significant structural shifts for any of these placebo variables around the time of changing to strict/weak/general opt-out.

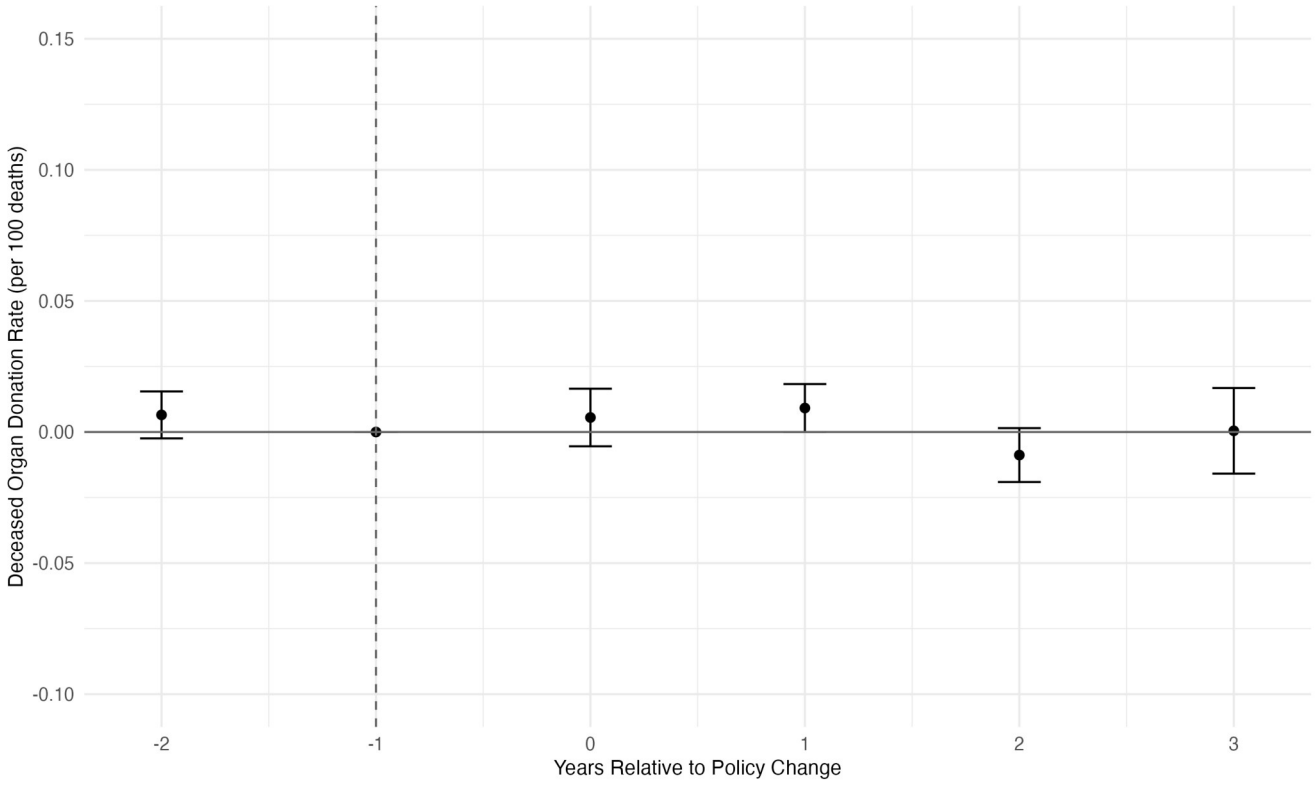
A.1.5 Robustness Check: Sun & Abraham

Figure A6: Event Study Graph for Strict Opt Out Countries (Sun & Abraham)



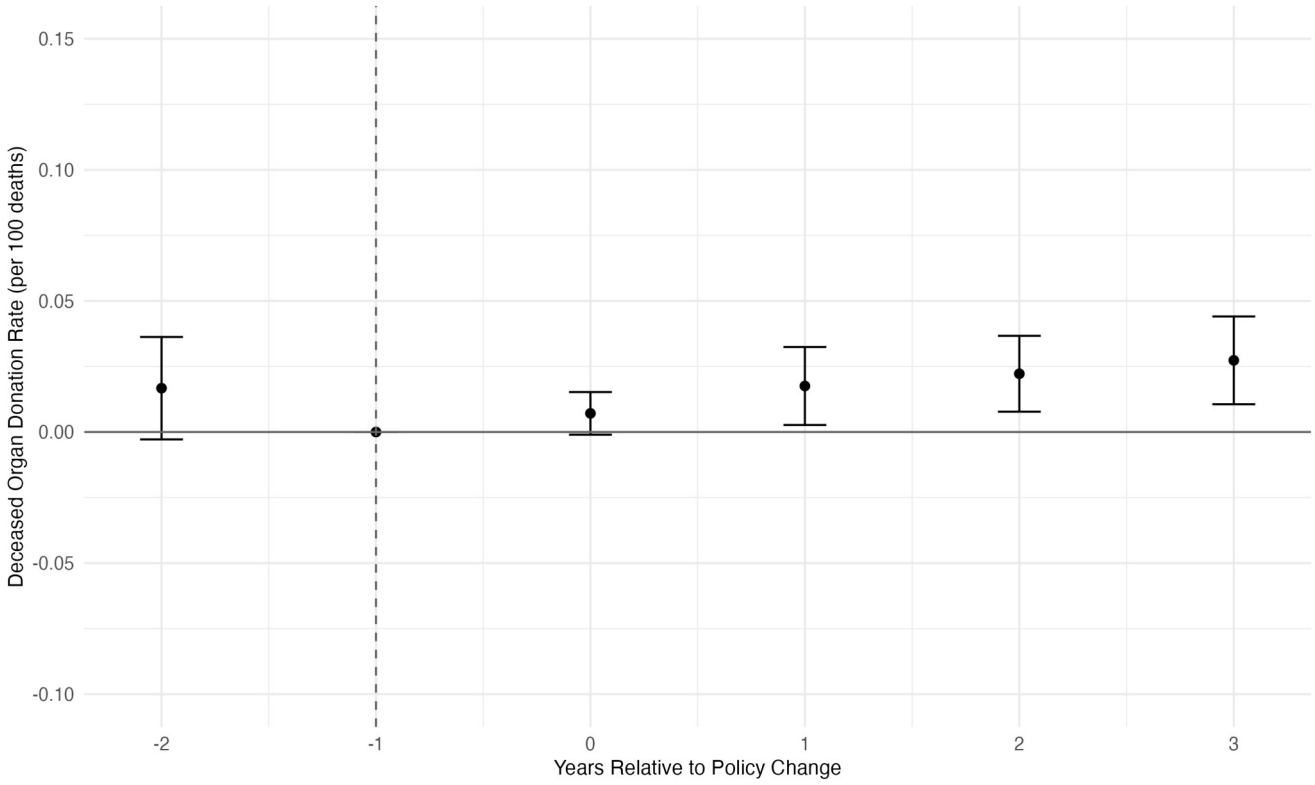
Notes: Estimated using Sun and Abraham (2021) event-study estimator, including panel data on all strict opt out countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria, Luxembourg* (1980-1985), Belgium* (1984-1989), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

Figure A7: Event Study Graph for Weak Opt Out Countries (Sun & Abraham)



Notes: Estimated using Sun and Abraham (2021) event-study estimator, including panel data on all weak opt out countries that changed policy nationally from opt-in to opt-out which has at least 4 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Finland, Germany, Netherlands, Norway, and UK (1983-1988). The sample also includes Sweden* (1994-1999), Greece (1995-1999), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Uruguay* (2011-2016), Bolivia (2008, 2010-2016), Ecuador (2009-2016), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2016). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

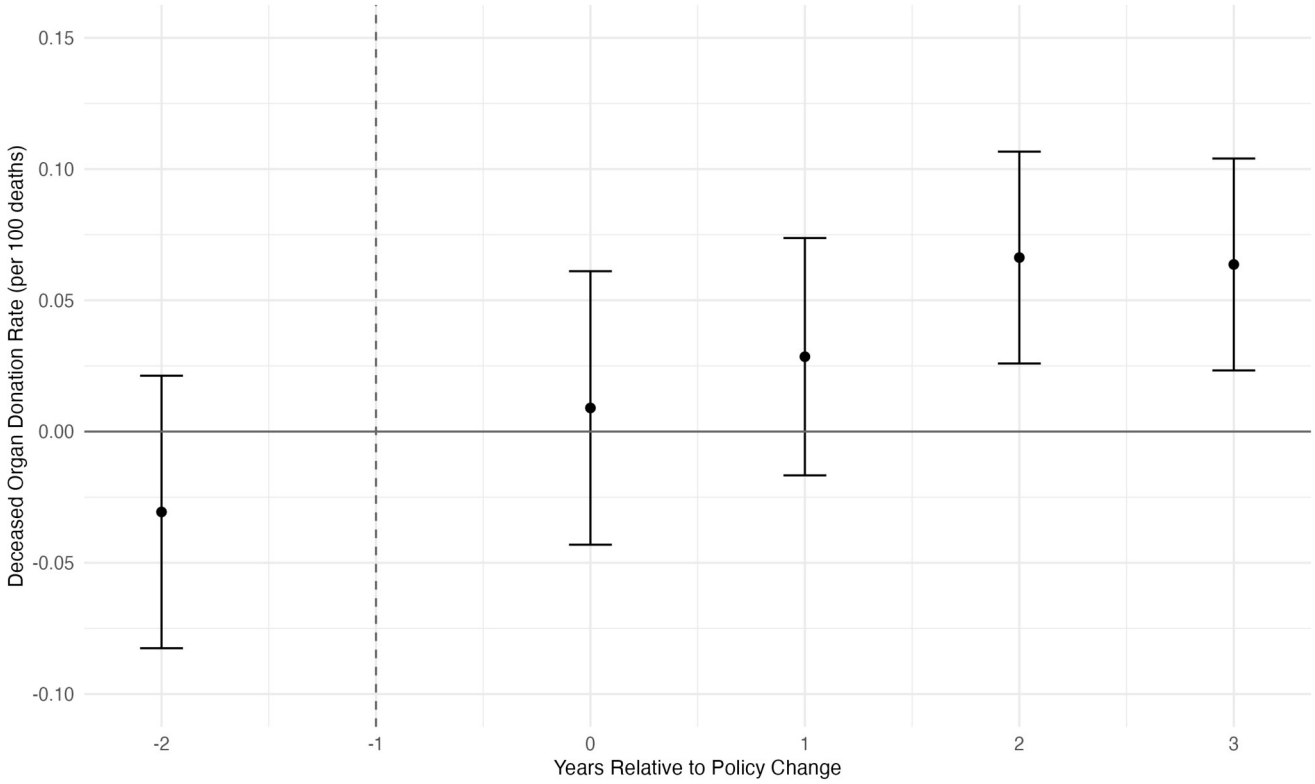
Figure A8: Event Study Graph for All Countries (Sun & Abraham)



Notes: Estimated using Sun and Abraham (2021) event-study estimator, including panel data on all countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria, Luxembourg* (1980-1985), Belgium* (1984-1989), Finland* (1983-1988), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). The sample also includes Sweden* (1994-1999), Greece (1995-2002), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Uruguay* (2011-2016), Bolivia (2008, 2010-2016), Ecuador (2009-2016), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2016). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals.*

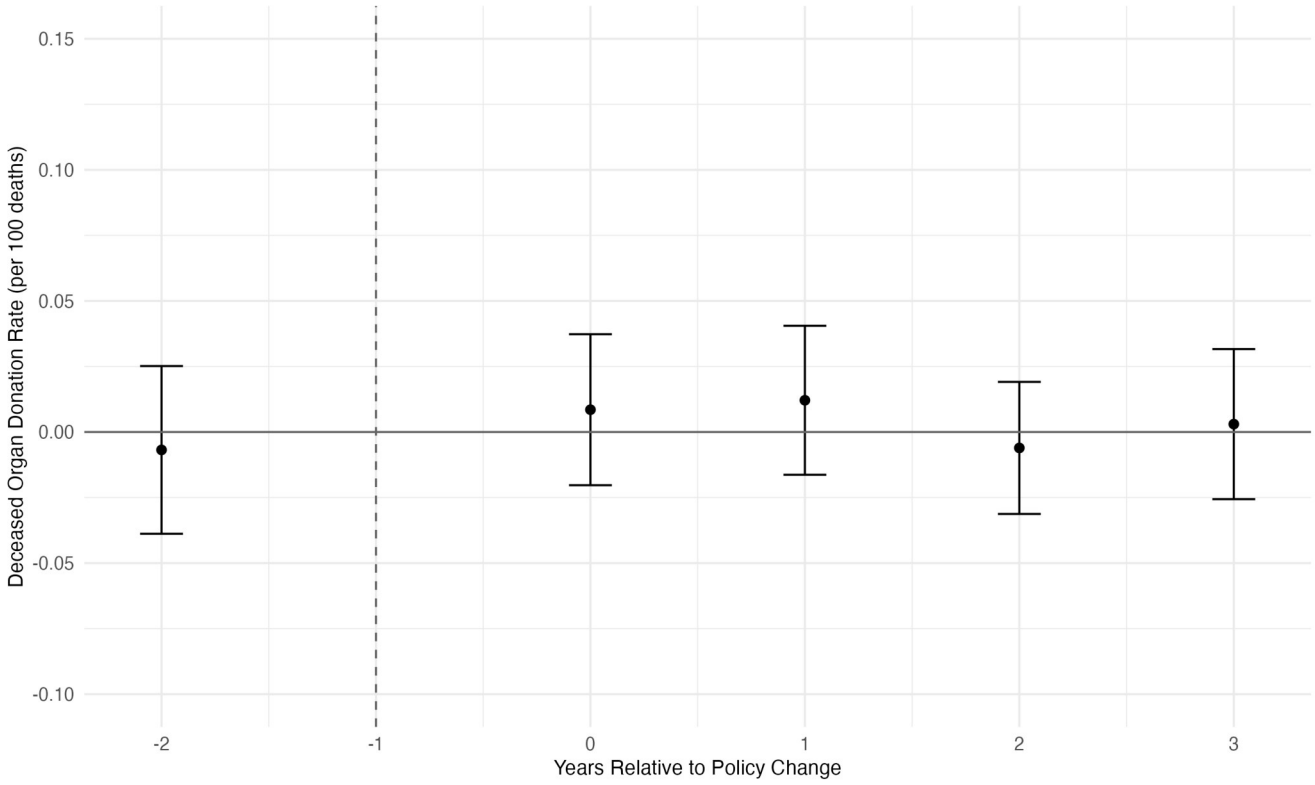
A.1.6 Robustness Check: Callaway & Sant'Anna

Figure A9: Event Study Graph for Strict Opt Out Countries (Callaway & Sant'Anna)



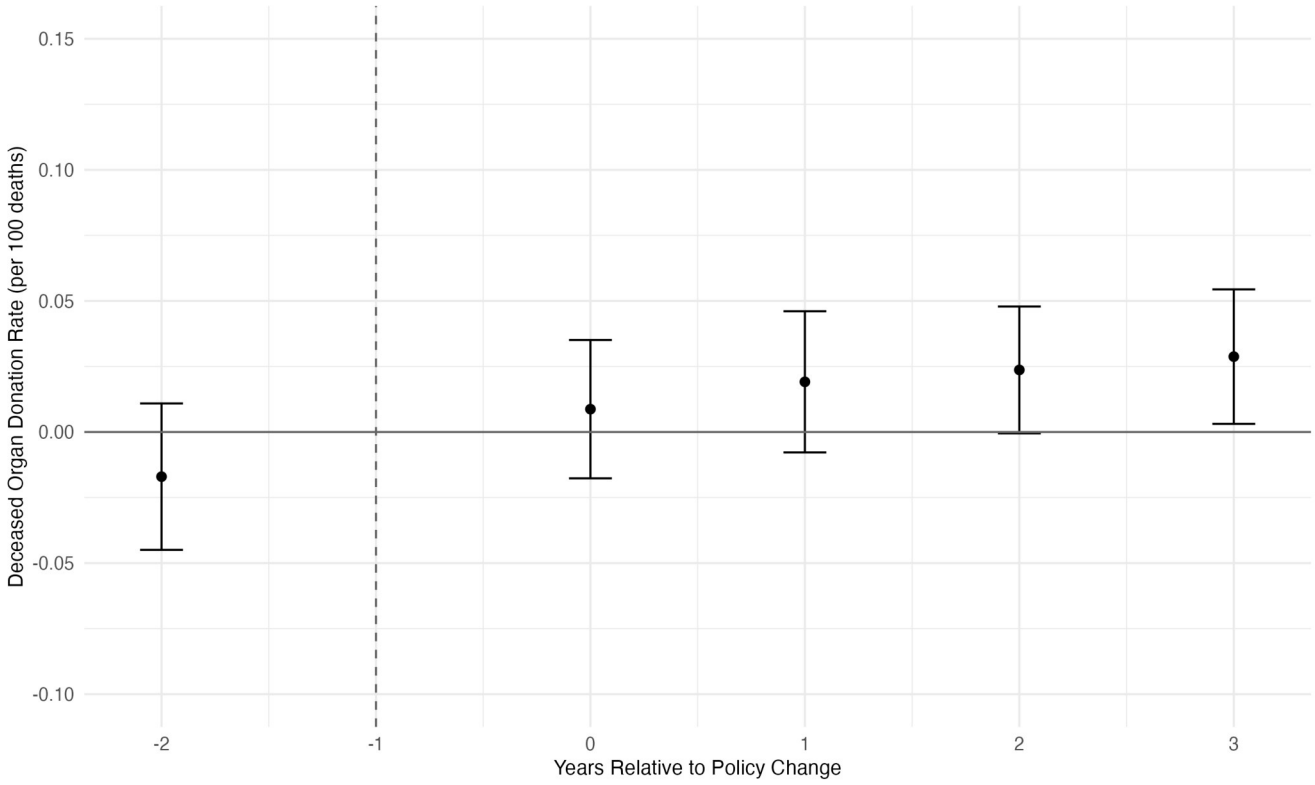
Notes: Estimated using Callaway and Sant'Anna (2021) event-study estimator, including panel data on all strict opt out countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria, Luxembourg* (1980-1985), Belgium* (1984-1989), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

Figure A10: Event Study Graph for Weak Opt Out Countries (Callaway & Sant'Anna)



Notes: Estimated using Callaway and Sant'Anna (2021) event-study estimator, including panel data on all weak opt out countries that changed policy nationally from opt-in to opt-out which has at least 4 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Finland, Germany, Netherlands, Norway, and UK (1983-1988). The sample also includes Sweden* (1994-1999), Greece (1995-1999), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Uruguay* (2011-2016), Bolivia (2008, 2010-2016), Ecuador (2009-2016), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2016). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

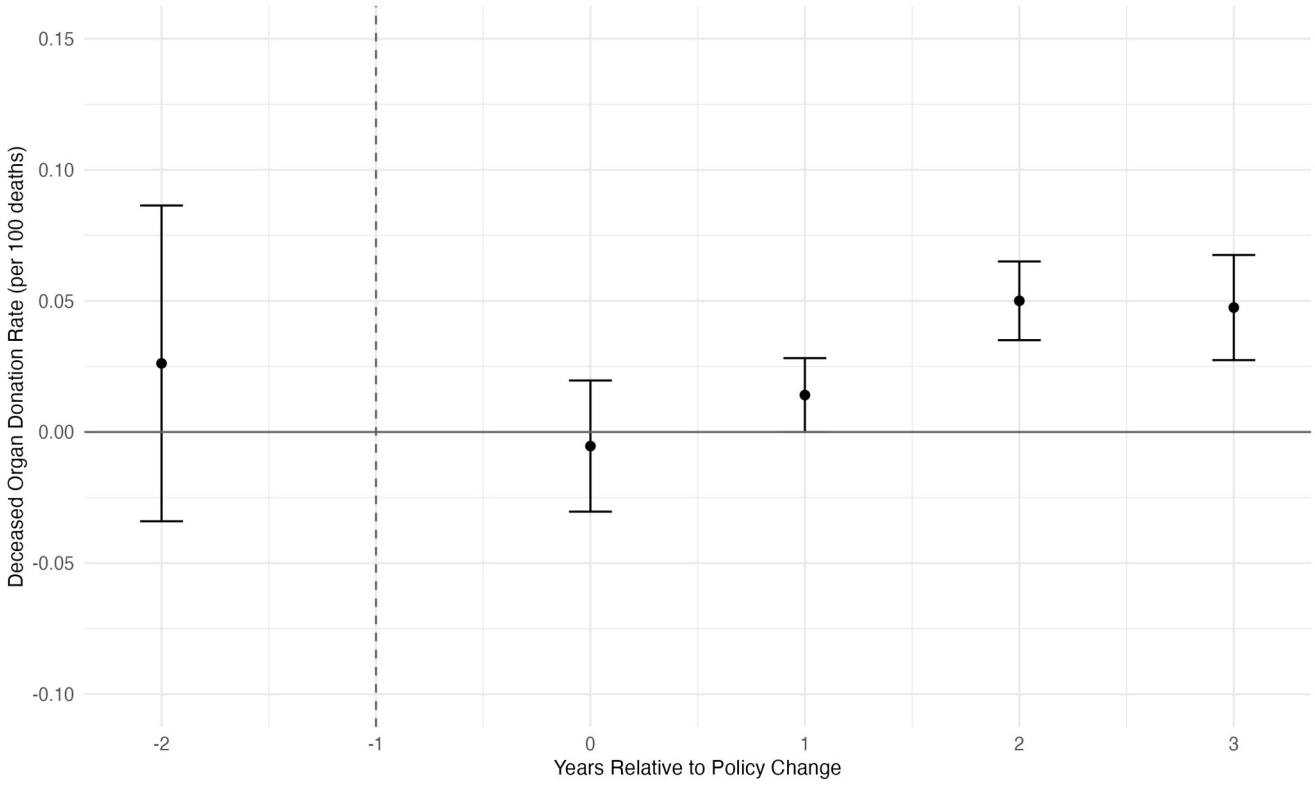
Figure A11: Event Study Graph for All Countries (Callaway & Sant'Anna)



Notes: Estimated using Callaway and Sant'Anna (2021) event-study estimator, including panel data on all countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria, Luxembourg* (1980-1985), Belgium* (1984-1989), Finland* (1983-1988), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). The sample also includes Sweden* (1994-1999), Greece (1995-1999), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Uruguay* (2011-2016), Bolivia (2008, 2010-2016), Ecuador (2009-2016), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2016). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals.*

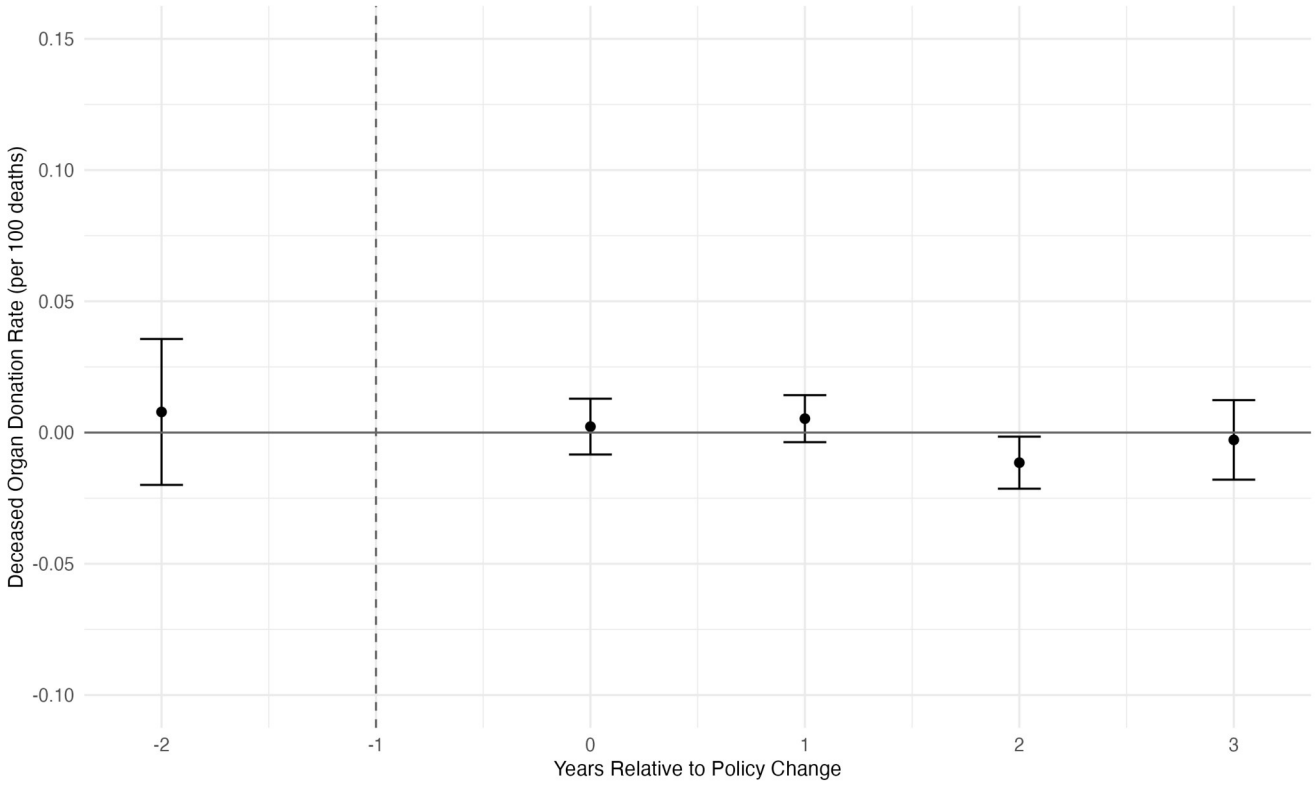
A.1.7 Robustness Check: Borusyak, Jaravel & Spiess

Figure A12: Event Study Graph for Strict Opt Out Countries (Borusyak, Jaravel & Spiess)



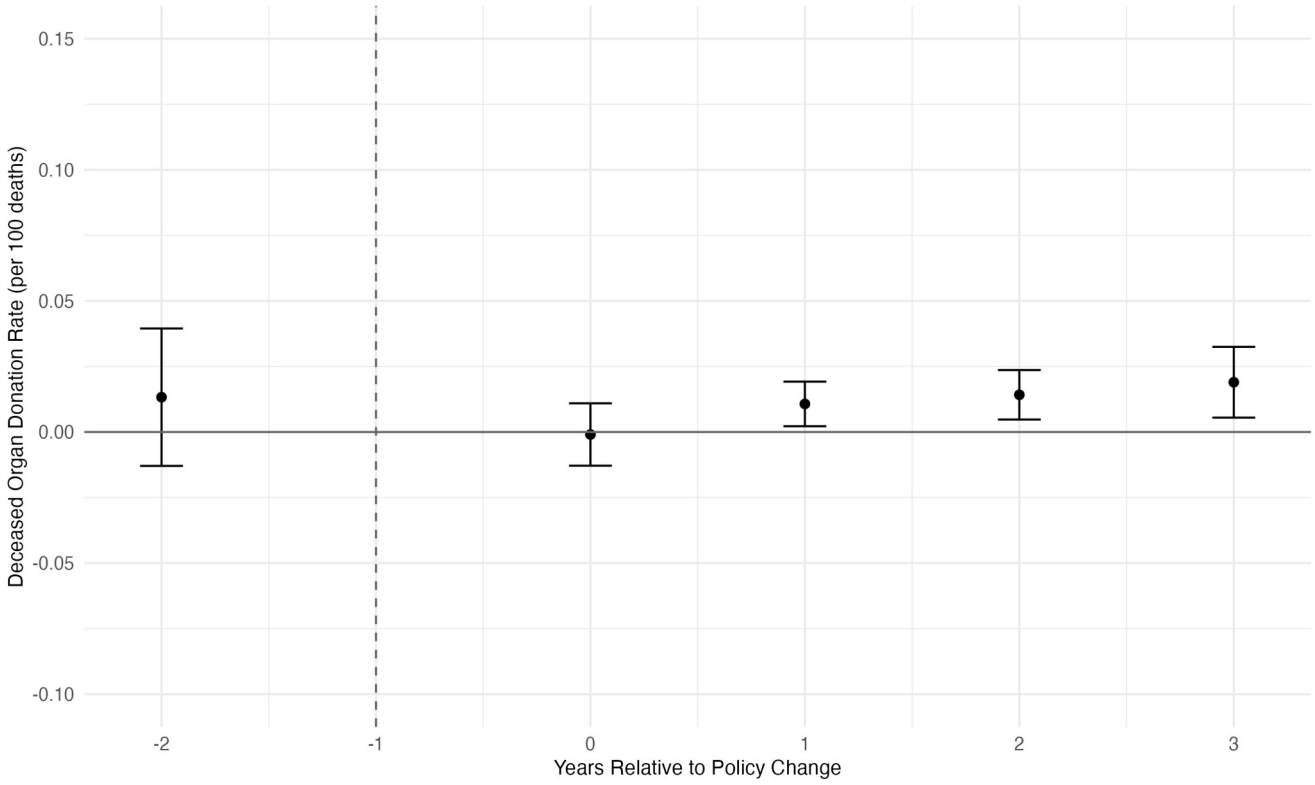
Notes: Estimated using Borusyak, Jaravel and Spiess (2021) event-study estimator, including panel data on all strict opt out countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria, Luxembourg* (1980-1985), Belgium* (1984-1989), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

Figure A13: Event Study Graph for Weak Opt Out Countries (Borusyak, Jaravel & Spiess)



Notes: Estimated using Borusyak, Jaravel and Spiess (2021) event-study estimator, including panel data on all weak opt out countries that changed policy nationally from opt-in to opt-out which has at least 4 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Finland, Germany, Netherlands, Norway, and UK (1983-1988). The sample also includes Sweden* (1994-1999), Greece (1995-1999), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Uruguay* (2011-2016), Bolivia (2008, 2010-2016), Ecuador (2009-2016), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2016). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

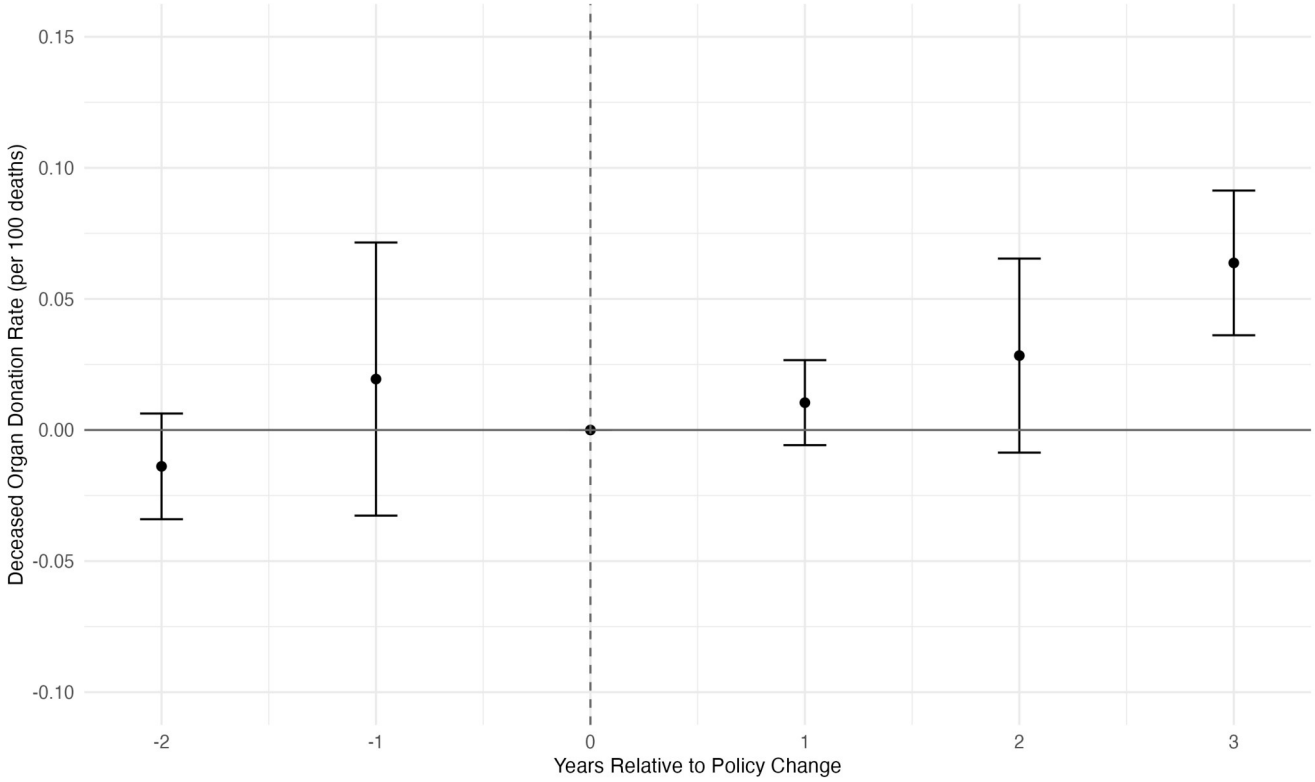
Figure A14: Event Study Graph for All Countries (Borusyak, Jaravel & Spiess)



Notes: Estimated using Borusyak, Jaravel and Spiess (2021) event-study estimator, including panel data on all countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria, Luxembourg* (1980-1985), Belgium* (1984-1989), Finland* (1983-1988), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). The sample also includes Sweden* (1994-1999), Greece (1995-1999), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Uruguay* (2011-2016), Bolivia (2008, 2010-2016), Ecuador (2009-2016), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2016). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals.*

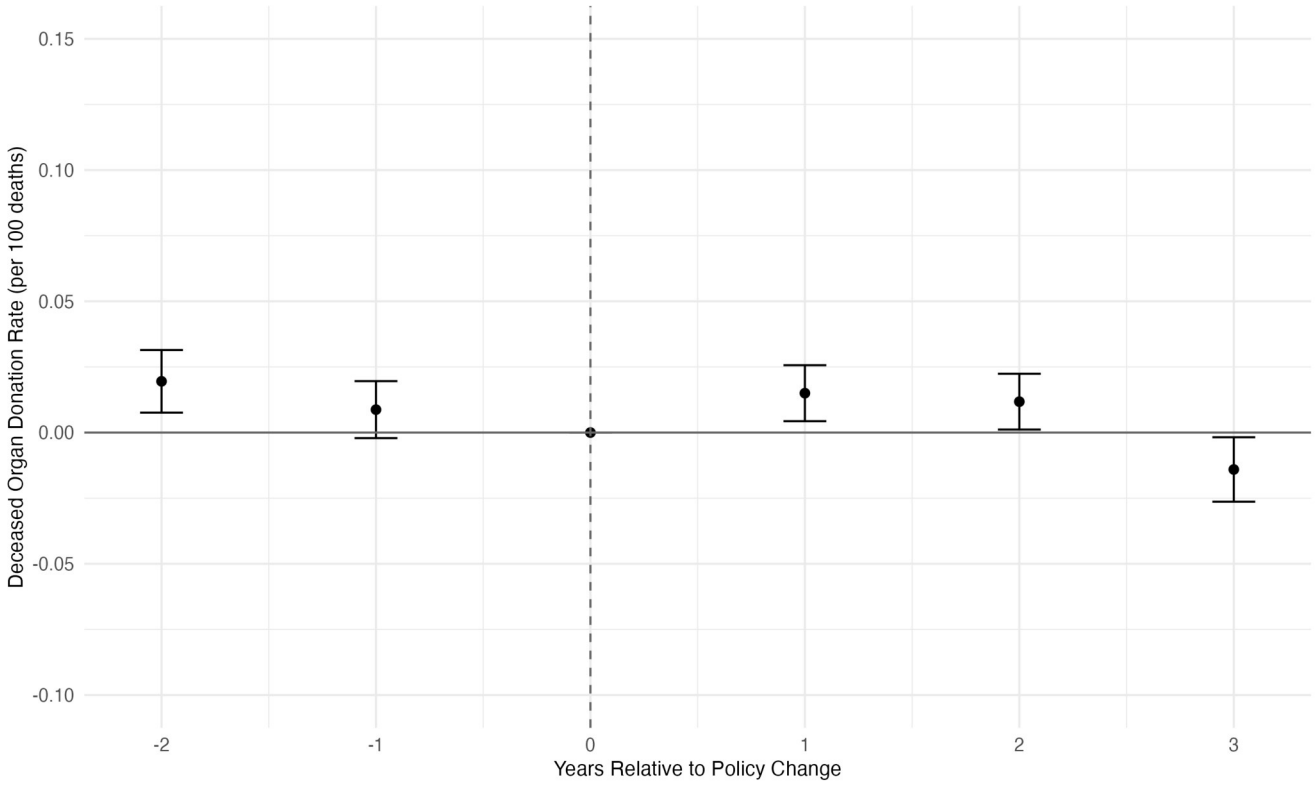
A.1.8 Robustness Check: Chaisemartin & D'Haultfœuille

Figure A15: Event Study Graph for Strict Opt Out Countries (Chaisemartin & D'Haultfœuille)



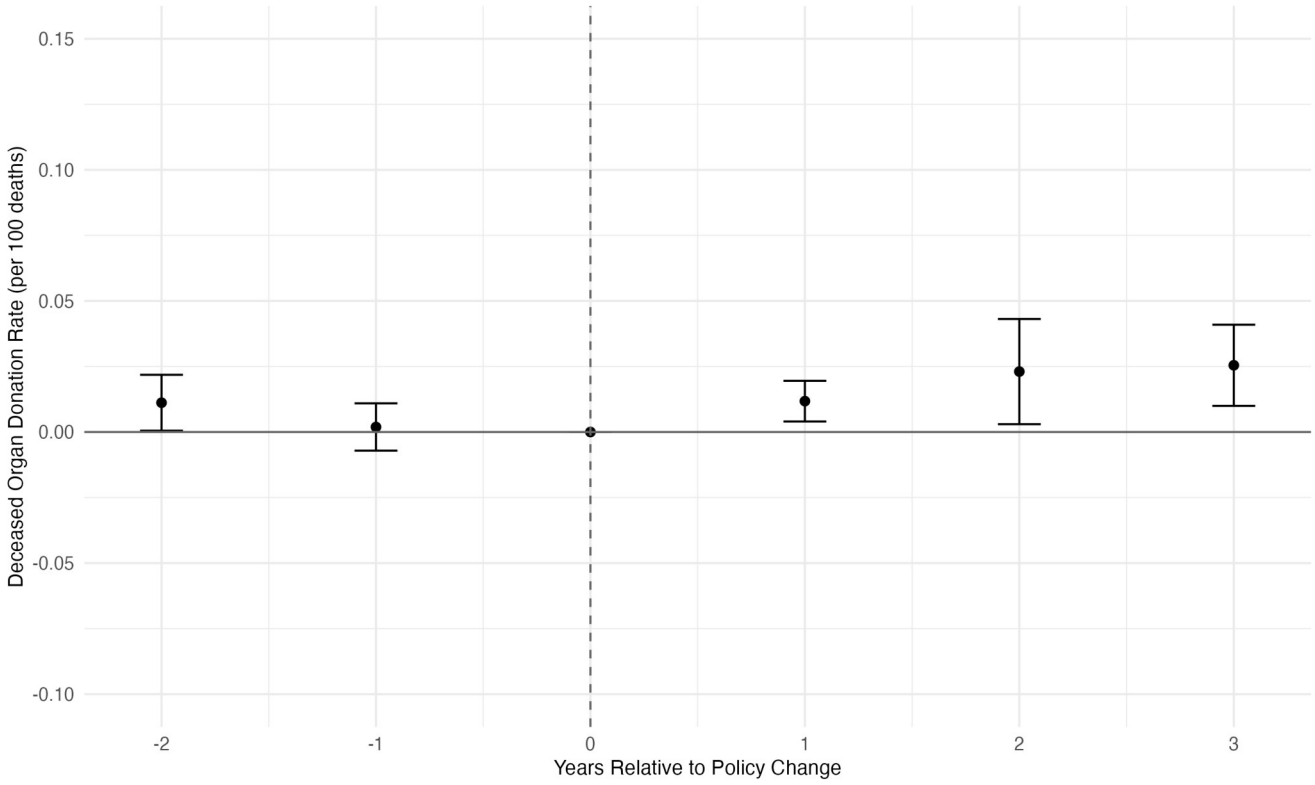
Notes: Estimated using Chaisemartin and D'Haultfœuille (2024) event-study estimator, including panel data on all strict opt out countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria, Luxembourg* (1980-1985), Belgium* (1984-1989), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

Figure A16: Event Study Graph for Weak Opt Out Countries (Chaisemartin & D'Haultfœuille)



Notes: Estimated using Chaisemartin and D'Haultfœuille (2024) event-study estimator, including panel data on all weak opt out countries that changed policy nationally from opt-in to opt-out which has at least 4 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Finland, Germany, Netherlands, Norway, and UK (1983-1988). The sample also includes Sweden* (1994-1999), Greece (1995-1999), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Uruguay* (2011-2016), Bolivia (2008, 2010-2016), Ecuador (2009-2016), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2016). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

Figure A17: Event Study Graph for All Countries (Chaisemartin & D'Haultfœuille)



Notes: Estimated using Chaisemartin and D'Haultfœuille (2024) event-study estimator, including panel data on all countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria, Luxembourg* (1980-1985), Belgium* (1984-1989), Finland* (1983-1988), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). The sample also includes Sweden* (1994-1999), Greece (1995-1999), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Uruguay* (2011-2016), Bolivia (2008, 2010-2016), Ecuador (2009-2016), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2016). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals.*

A.2 Experiment Details

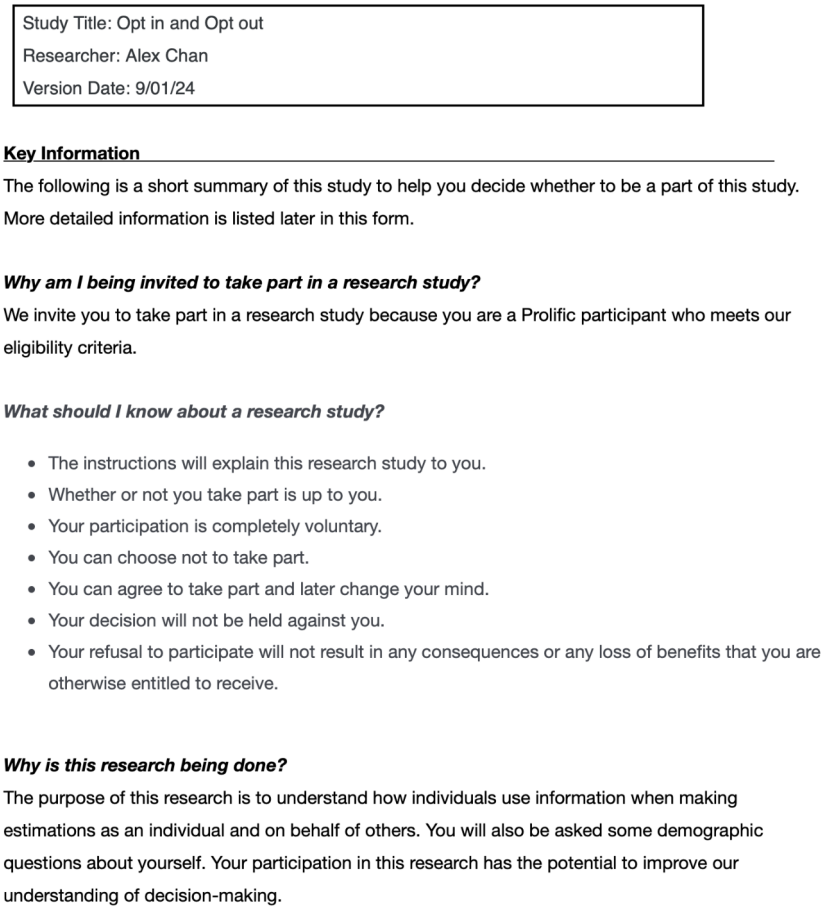
The full experiment is made available as a qsf file as part of the replication package. It is also available in a Supplemental Appendix on the author's website (due to word limits on the Online Appendix).

A.3 Supplemental Appendix Posted On Author Website Only (Not Part of Online Appendix)

A.3.1 Overview of Experiment

After the participation consent screen (See Figure A18, A19, A20, A21), the participants are shown the instruction screen (See Figure A22).

Figure A18: Experiment Consent Form Part 1



Study Title: Opt in and Opt out
Researcher: Alex Chan
Version Date: 9/01/24

Key Information

The following is a short summary of this study to help you decide whether to be a part of this study. More detailed information is listed later in this form.

Why am I being invited to take part in a research study?

We invite you to take part in a research study because you are a Prolific participant who meets our eligibility criteria.

What should I know about a research study?

- The instructions will explain this research study to you.
- Whether or not you take part is up to you.
- Your participation is completely voluntary.
- You can choose not to take part.
- You can agree to take part and later change your mind.
- Your decision will not be held against you.
- Your refusal to participate will not result in any consequences or any loss of benefits that you are otherwise entitled to receive.

Why is this research being done?

The purpose of this research is to understand how individuals use information when making estimations as an individual and on behalf of others. You will also be asked some demographic questions about yourself. Your participation in this research has the potential to improve our understanding of decision-making.

Figure A19: Experiment Consent Form Part 2

How long will the research last and what will I need to do?

We expect that you will be in this research study for 18 minutes. You will be asked to read instructions, learn about different options, and make assessments of those options. You will learn about decisions you can make on behalf of yourself, and about decisions you can make on behalf of another anonymous player randomly matched with you, You will also be asked some demographic questions about yourself. There are understanding questions that you must answer correctly in order to complete the study. If you fail to answer the understanding questions correctly, you may be dismissed from the study without pay. You will receive your full baseline completion payment of $5.00 if you read the instructions and completed the 3 parts of the survey. Besides your baseline completion payment, you will start with a default bonus of $0.94, and can win additional earnings through the decisions on behalf of yourself and on behalf of others (from $0.50 to $1.00) and this could allow you to earn up to $6.94 in total. You can potentially lose some of the default bonus but the deductions will not exceed $0.94, so that you are guaranteed $5.00 for completing the study.

You may not be told everything

As part of this research design, you may not be told everything about the purpose of this study. In addition, while you will have complete and truthful information about the procedures of the version of the study that you are participating in, there may be other versions of the study with different procedures. You will not be told about these other versions. These other versions have no impact on your payments.

Is there any way being in this study could be bad for me?

We think this study poses no more than minimal risk. Some participants may feel discomfort or anxiety from being asked to have discussions and/or make decisions with other participants. There is a small chance of breach of confidentiality.

Will being in this study help me in any way?

There are no benefits to you from your taking part in this research. We cannot promise any benefits to others from your taking part in this research. However, possible benefits to others include greater understanding of decision-making processes.

Figure A20: Experiment Consent Form Part 3

Detailed Information

The following is more detailed information about this study in addition to the information listed above.

What happens if I say yes, but I change my mind later?

You can leave the research at any time; it will not be held against you and no further action is required on your behalf.

If I take part in this research, how will my privacy be protected? What happens to the information you collect?

Efforts will be made to limit the use and disclosure of your Personal Information, including research study records, to people who have a need to review this information. We cannot promise complete secrecy. Organizations that may inspect and copy your information include the IRB and other representatives of this organization.

If identifiers are removed from the data that are collected during this research, that data could be used for future research studies or distributed to another investigator for future research studies without your additional informed consent.

What else do I need to know?

If you agree to take part in this research study, we will pay you at least $5.00 for completing the study. This payment will be made to you via Prolific within 48 hours of your participation. Besides your baseline completion payment, you will win additional earnings through your decisions. Some of your decisions can impact how much another participant can earn in this study.

Who can I talk to?

If you have questions, concerns, or complaints, or think the research has hurt you, talk to the research team at Alex Chan, Harvard Business School, (617) 495-8674 or achan@hbs.edu .
This research has been reviewed and approved by the Harvard University Area Institutional Review Board ("IRB"). You may talk to them at (617) 496-2847 or cuhs@harvard.edu if:

- Your questions, concerns, or complaints are not being answered by the research team.
- You cannot reach the research team.
- You want to talk to someone besides the research team.
- You have questions about your rights as a research subject.
- You want to get information or provide input about this research.

Figure A21: Experiment Consent Form Part 4

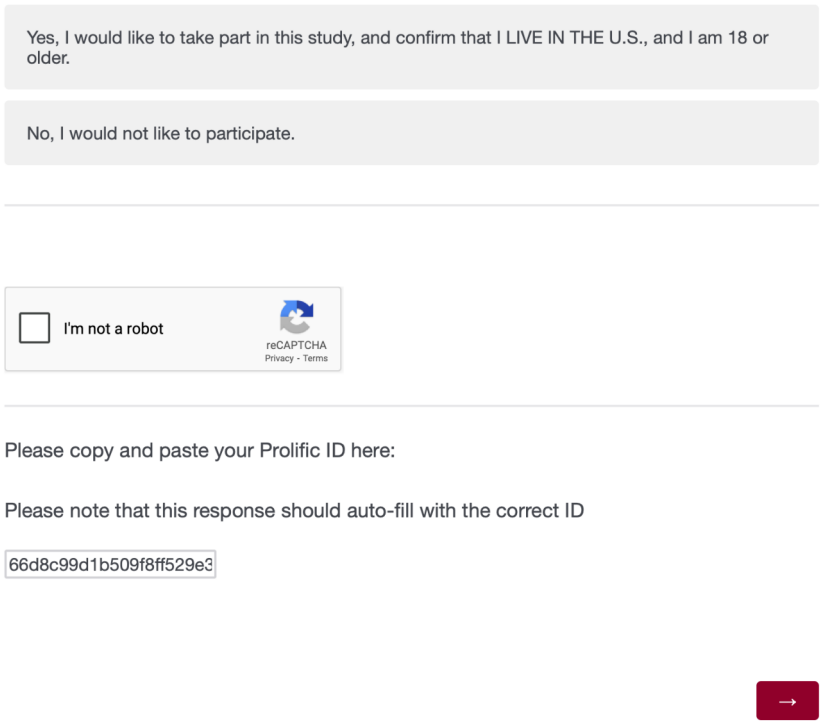
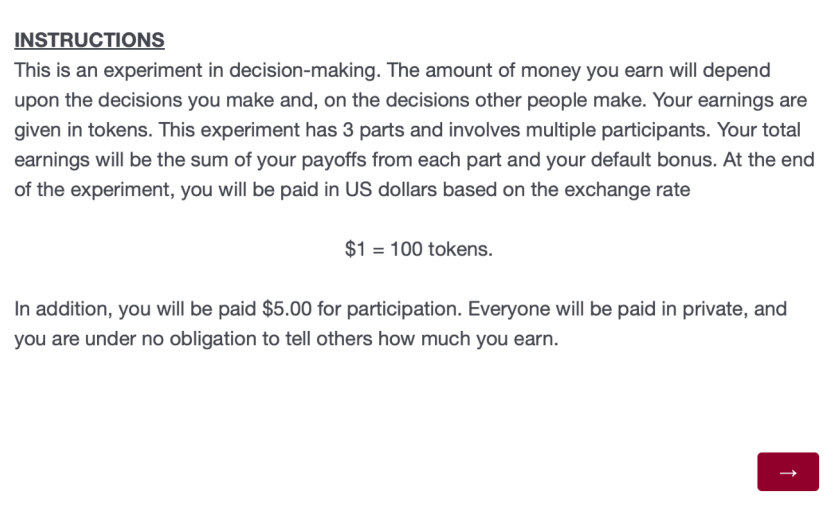


Figure A22: Experiment Page 1



We let this Section of the Appendix provide additional details about the actual experience of this experiment as participants play it on Prolific.

A.3.2 Experiment Part 1

After the instruction screen, participants will go through seven screens before Figure VII. These seven screens serve to explain (Figure A23) and group all subjects anonymously into two "families" based on their preferred paintings by Klee or Kandinsky (Figure A24, A25, A26, A27, A28, A29).

Figure A23: Experiment Page 2

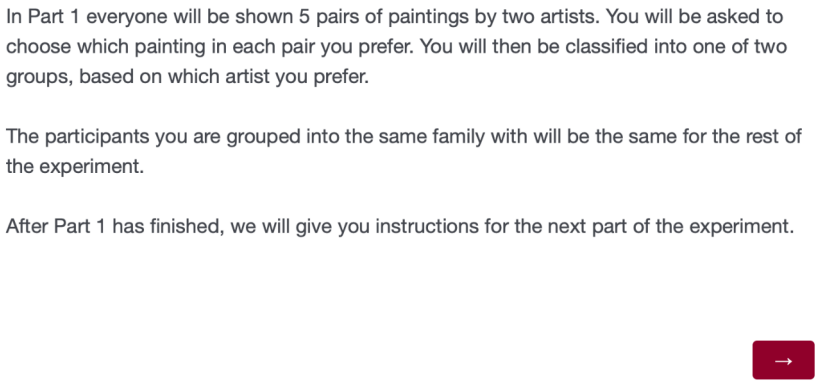


Figure A24: Experiment Page 3

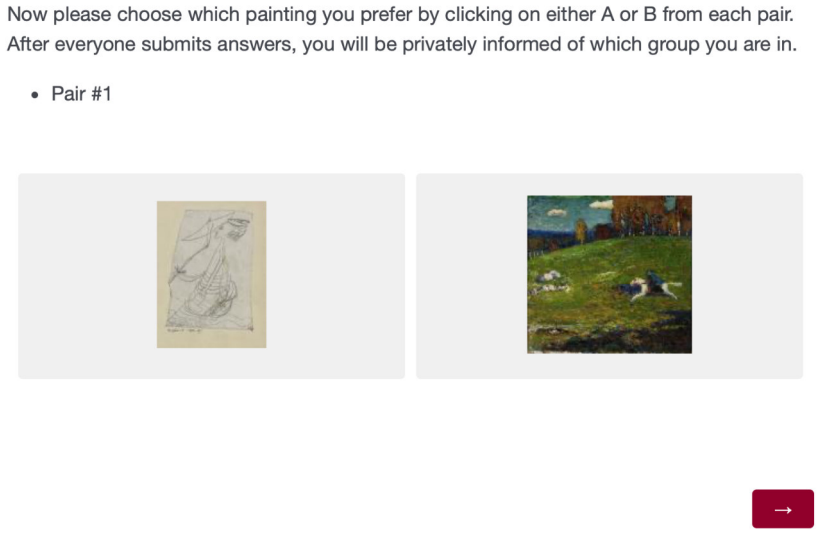


Figure A25: Experiment Page 4

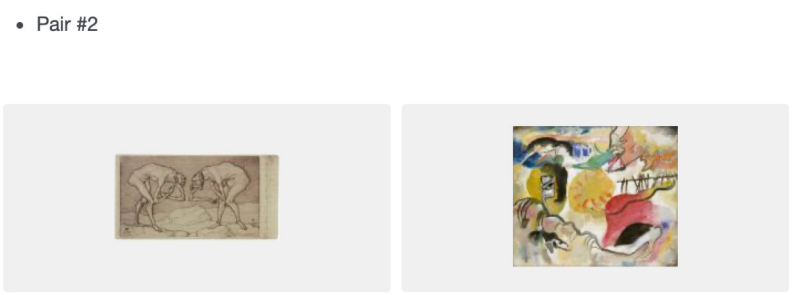


Figure A26: Experiment Page 5

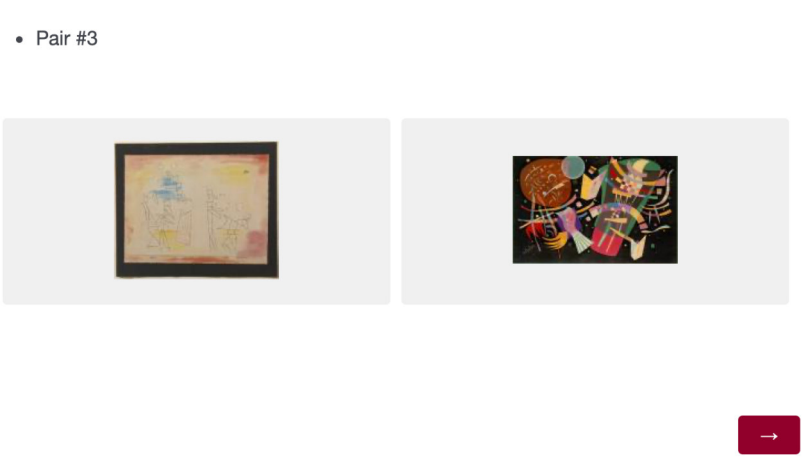


Figure A27: Experiment Page 6

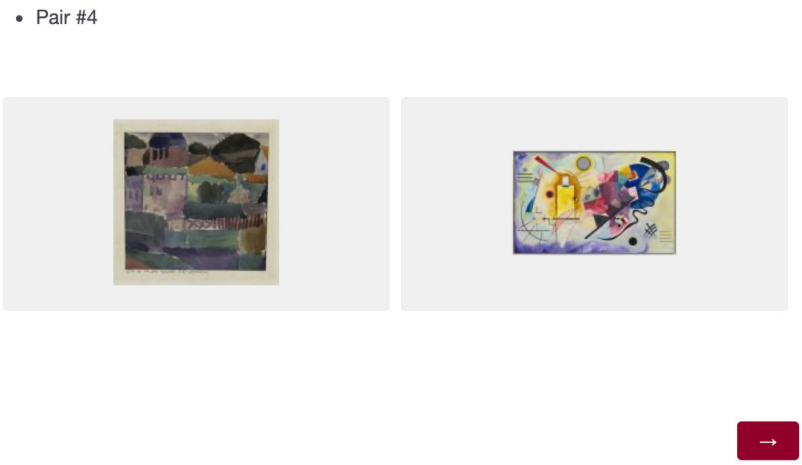


Figure A28: Experiment Page 7

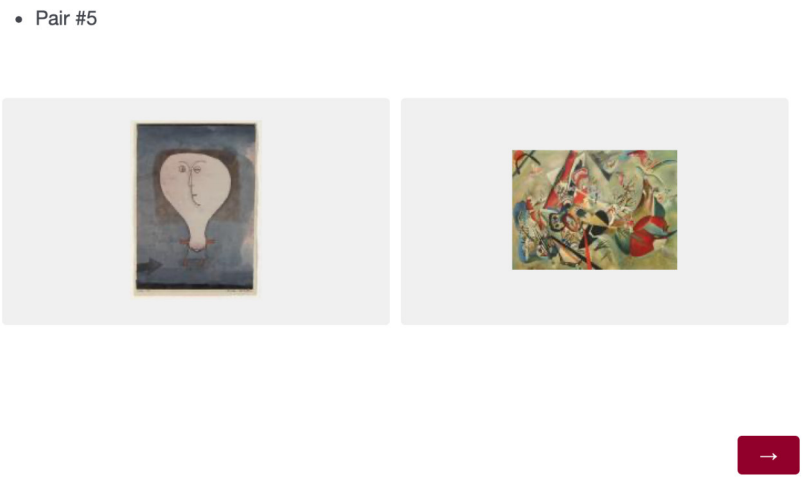
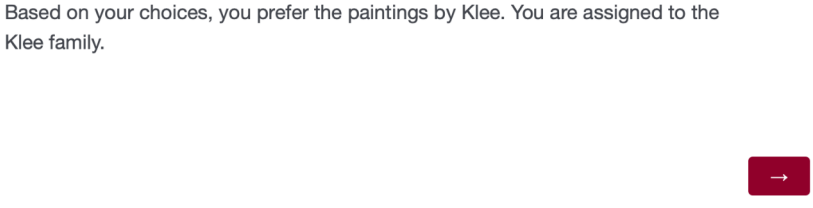


Figure A29: Experiment Page 8

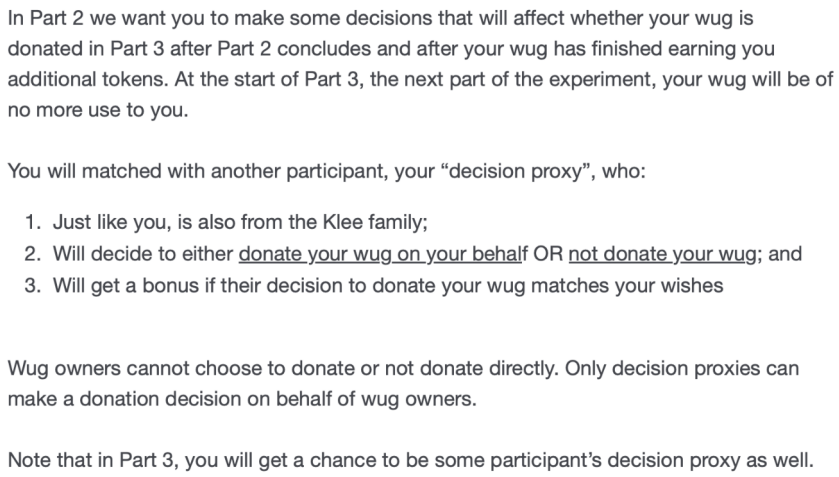


Notes: Whether participants are assigned to Klee or Kandinsky family depends on their choices.

A.3.3 Experiment Part 2

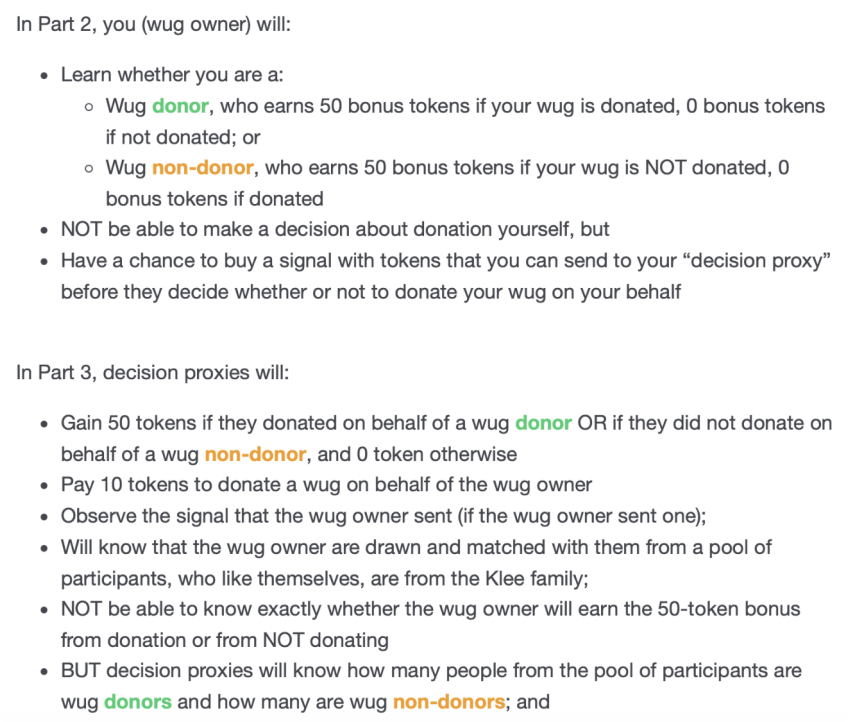
Once the participants move to Part 2, they will see two screens that give the overview of Part 2 and Part 3, as shown in Figure A30 and A31. Then, participants will learn the default setting (opt-in vs. opt-out), signal cost ($0.75 vs. $0.25), and cost to decision proxy of overturning defaults or signals ($0.19 vs. $0.01) (See Figure A32 as an example). All participants will further learn their randomly assigned roles (donor vs. non-donor) (See Figure A33 as an example) after they answer three comprehension questions (See Figure A48, A49, and A50)

Figure A30: Experiment Page 9



Notes: Whether participants are assigned to Klee or Kandinsky family depends on their choices.

Figure A31: Experiment Page 10

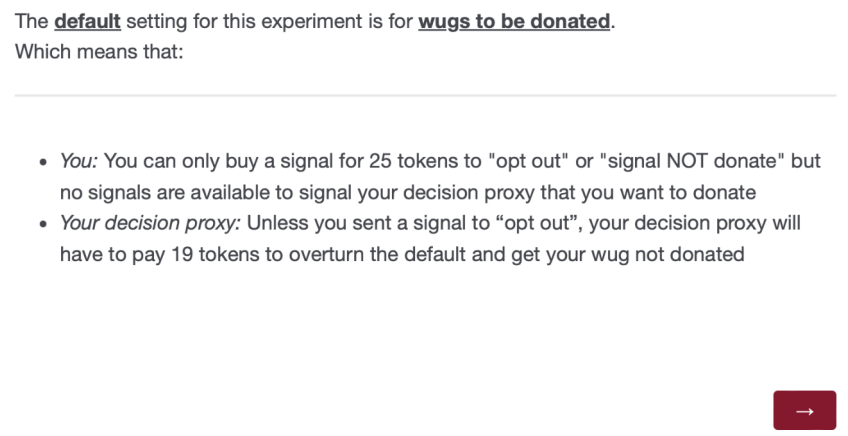

In Part 2, you (wug owner) will:

- Learn whether you are a:
  - Wug **donor**, who earns 50 bonus tokens if your wug is donated, 0 bonus tokens if not donated; or
  - Wug **non-donor**, who earns 50 bonus tokens if your wug is NOT donated, 0 bonus tokens if donated
- NOT be able to make a decision about donation yourself, but
- Have a chance to buy a signal with tokens that you can send to your "decision proxy" before they decide whether or not to donate your wug on your behalf

In Part 3, decision proxies will:

- Gain 50 tokens if they donated on behalf of a wug **donor** OR if they did not donate on behalf of a wug **non-donor**, and 0 token otherwise
- Pay 10 tokens to donate a wug on behalf of the wug owner
- Observe the signal that the wug owner sent (if the wug owner sent one);
- Will know that the wug owner are drawn and matched with them from a pool of participants, who like themselves, are from the Klee family;
- NOT be able to know exactly whether the wug owner will earn the 50-token bonus from donation or from NOT donating
- BUT decision proxies will know how many people from the pool of participants are wug **donors** and how many are wug **non-donors**; and

Figure A32: Experiment Treatment Assignment Screen

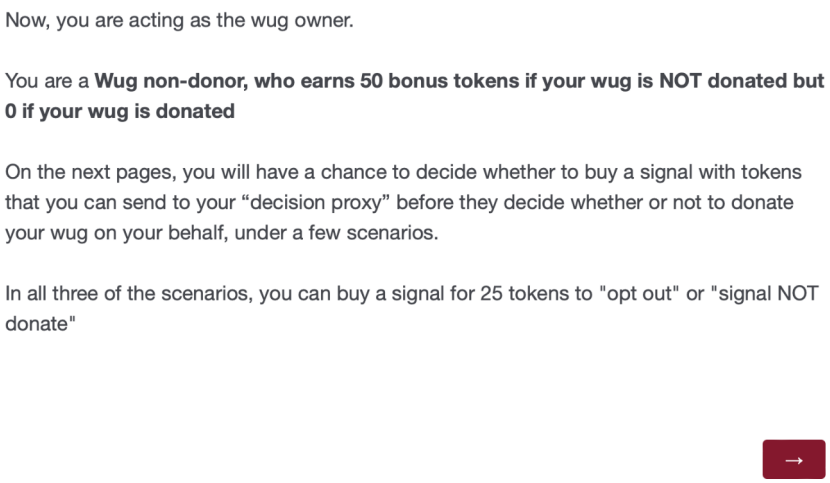

The **default** setting for this experiment is for **wugs to be donated**.
Which means that:

- *You:* You can only buy a signal for 25 tokens to "opt out" or "signal NOT donate" but no signals are available to signal your decision proxy that you want to donate
- *Your decision proxy:* Unless you sent a signal to "opt out", your decision proxy will have to pay 19 tokens to overturn the default and get your wug not donated

→

Notes: Participants are randomly assigned to default regime (opt-in vs. opt-out), signal cost ($0.75 vs. $0.25), and cost to decision proxy of overturning defaults or signals ($0.19 vs. $0.01)

Figure A33: Experiment Wug Role Assigenment Screen



Notes: Participants are randomly assigned to wug donor or wug non-donor.

Next, all participants complete four additional comprehension questions (Figure A51, A52, A53, A54, A55). They are then presented in randomized order with three decision screens, in which they must decide whether to send signals under three informational conditions, reflecting different priors (10%, 50%, 90%) that the decision proxy believes the wug owner is a donor. Figure A34, A35, and A36 provide one example of the signal decision screen shown to participants. The text and payoffs vary depending on the default status (opt-in vs. opt-out), the signal cost ($0.25 vs. $0.75), and the participant's assigned role (wug donor vs. wug non-donor). For illustration, the following three figures display the case where the default is donated, the signal cost is $0.25, and the role is wug non-donor. Equivalent screens are shown for the other parameter combinations.

Figure A34: Decision for Sending Signal Screen 1

Note: The default status is donated. You can only "opt out" or "signal NOT donate". If the decision proxy donated your wug, you will "lose 50 tokens" and your cost of signal is "25 tokens".

- If you signaled AND decision proxy donated, you get **-25 tokens** **(You will lose tokens)**
- If you signaled AND decision proxy did NOT donated, you gain **25 tokens**
- If you did NOT signal AND decision proxy donated, you gain 0 token
- If you did NOT signal AND decision proxy did NOT donated, you gain **50 tokens**

Suppose that your decision proxy knows you are matched to them from a pool of participants from the same Klee family and that there is a **90% chance** that you are a wug **donor** and a **10% chance** that you are a wug **non-donor**: Will you pay 25 tokens to buy the signal **to "opt out" or "signal NOT donate"**?

Yes, signal

No, do NOT signal

→

Figure A35: Decision for Sending Signal Screen 2

Note: The default status is donated. You can only "opt out" or "signal NOT donate". If the decision proxy donated your wug, you will "lose 50 tokens" and your cost of signal is "25 tokens".

- If you signaled AND decision proxy donated, you get **-25 tokens (You will lose tokens)**
- If you signaled AND decision proxy did NOT donated, you gain **25 tokens**
- If you did NOT signal AND decision proxy donated, you gain 0 token
- If you did NOT signal AND decision proxy did NOT donated, you gain **50 tokens**

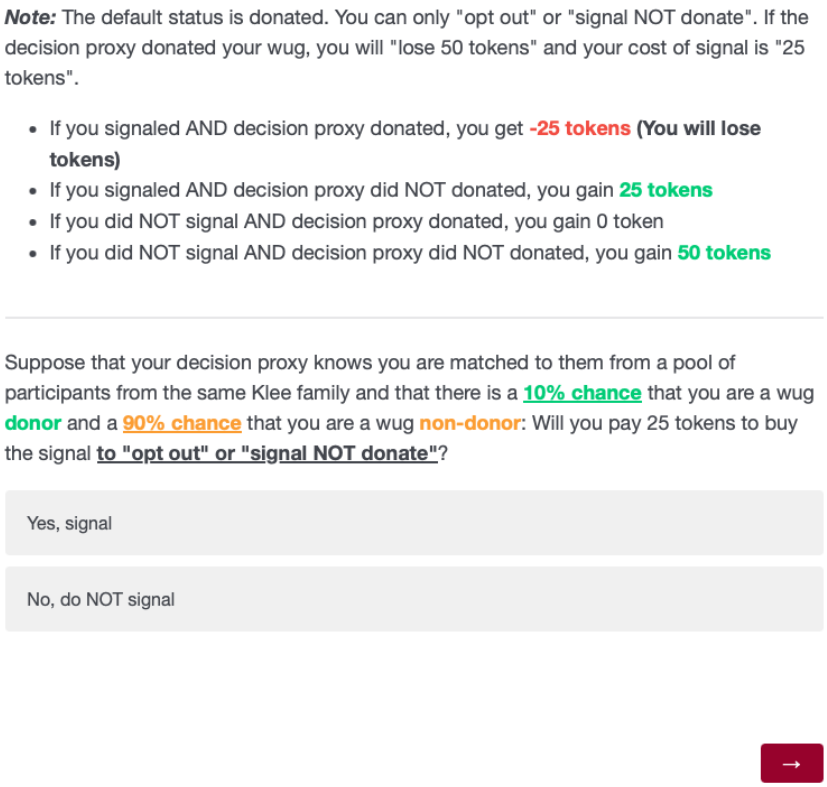
Suppose that your decision proxy knows you are matched to them from a pool of participants from the same Klee family and that there is a **50% chance** that you are a wug **donor** and a **50% chance** that you are a wug **non-donor**: Will you pay 25 tokens to buy the signal **to "opt out" or "signal NOT donate"**?

No, do NOT signal

Yes, signal

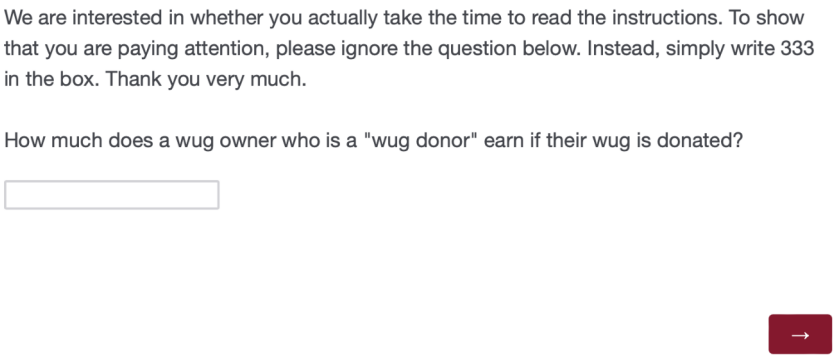
→

Figure A36: Decision for Sending Signal Screen 3



At the end of Part 2, before proceeding to Part 3, participants have to finish one attention check question (Figure A37).

Figure A37: Attention Check Question 1



A.3.4 Experiment Part 3

At the start of Part 3, participants learn that they will switch roles from a wug owner to a decision proxy, who will make a decision to donate or not for another wug owner (Figure A38, A39, A40). Note that the parameters like being in an "opt out" treatment will be

held the same while participants acted as Player 1 and Player 2. Then, they complete four additional comprehension questions (Figure A56, A57, A58, A59, A60).

Figure A38: Roles Switching Screen 1

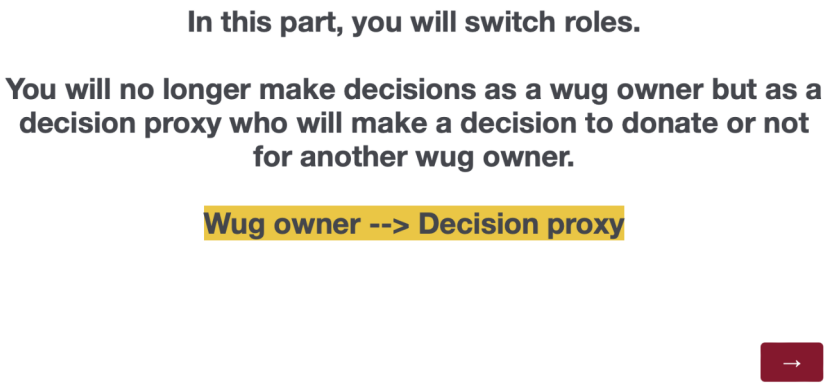


Figure A39: Roles Switching Screen 2

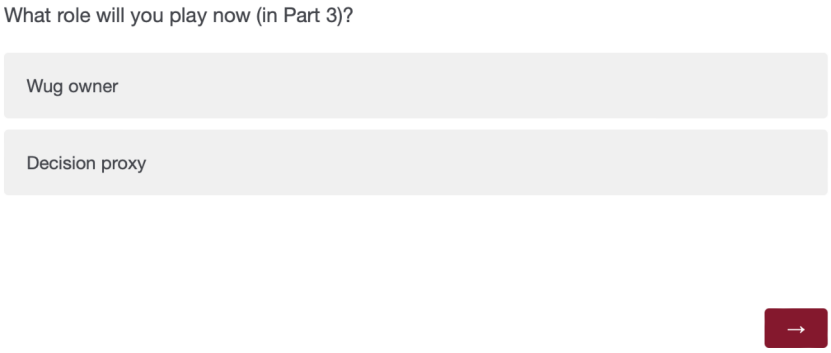
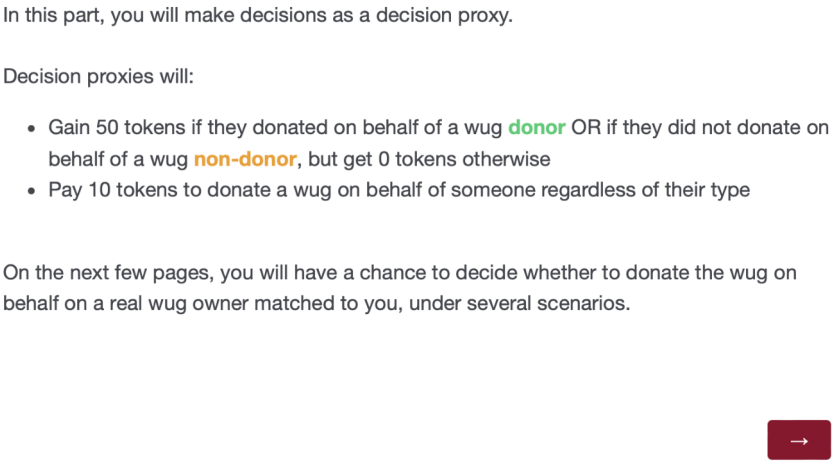


Figure A40: Roles Switching Screen 3



Next, all participants are presented in randomized order with six decision screens, in which they must decide whether to donate for another wug owner under six informational conditions: three different priors (10%, 50%, 90%) that the decision proxy believes the wug owner is a donor and two signal states (owner sent a signal vs. owner sent no signal). Figure A41, A42, A43, A44, A45, and A46 provide one example of the donation decision screen shown to participants. The text and payoffs vary depending on the default status (opt-in vs. opt-out), the signal cost ($0.25 vs. $0.75), cost of overturning defaults or signals ($0.19 vs. $0.01). For illustration, the following six figures display the case where the default is donated, the signal cost is $0.25, and the cost of overturning defaults or signals is $0.19. Equivalent screens are shown for the other parameter combinations.

Figure A41: Donation Decision Screen 1

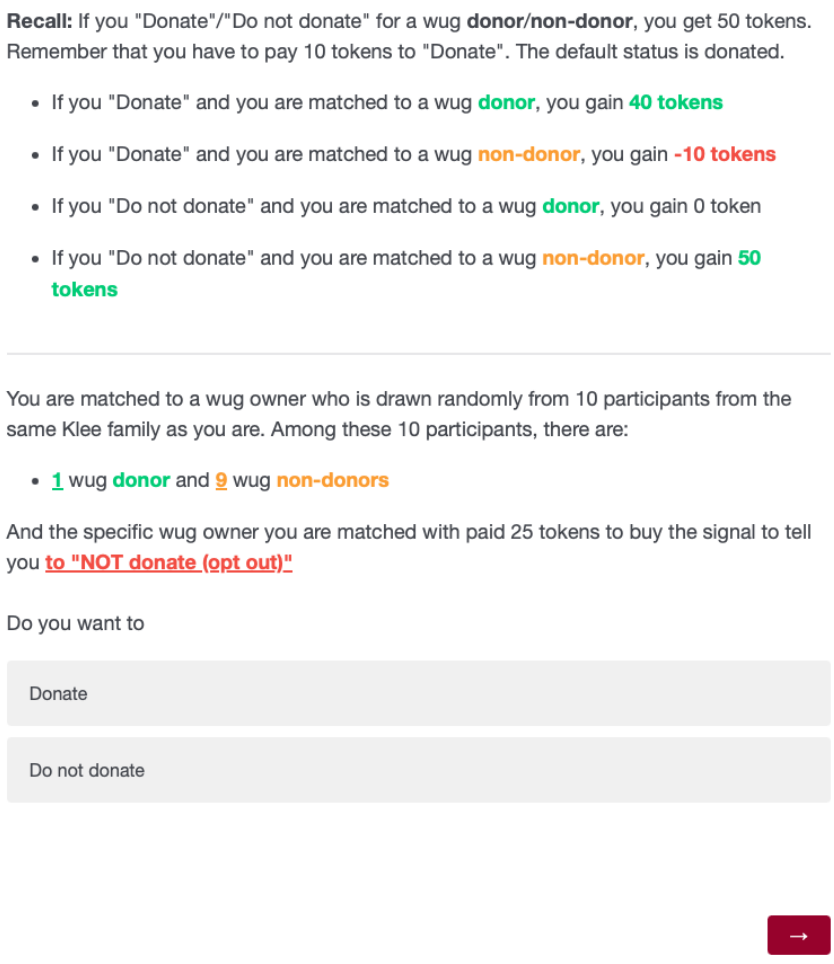


Figure A42: Donation Decision Screen 2

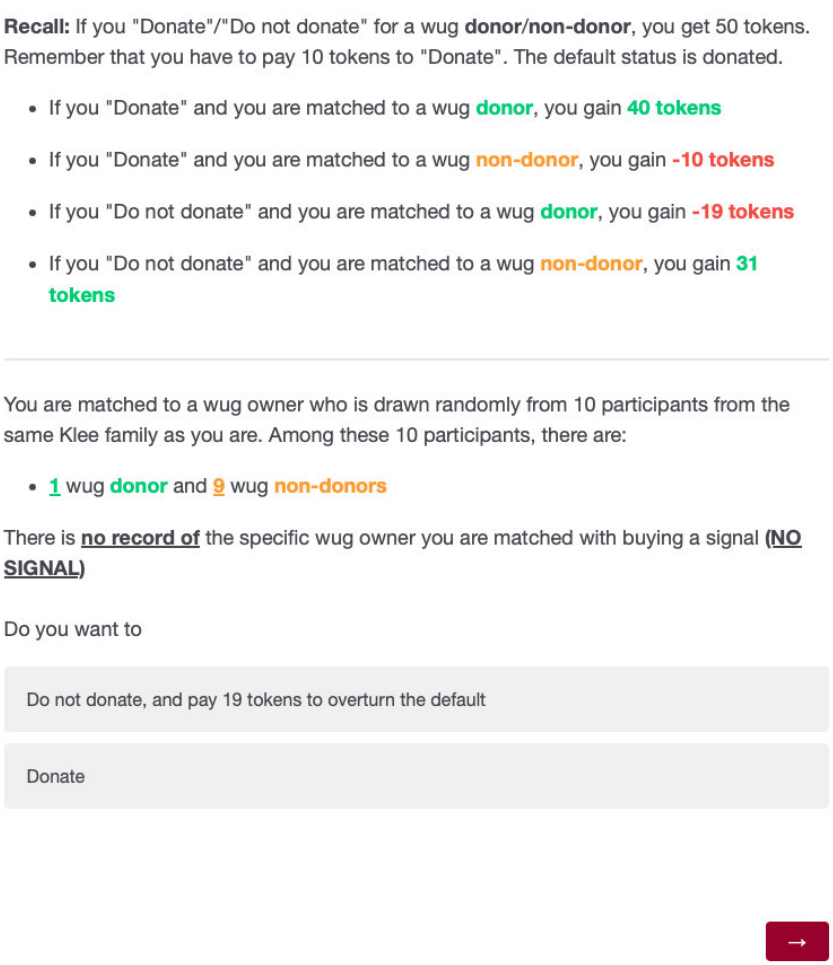


Figure A43: Donation Decision Screen 3

Recall: If you "Donate"/"Do not donate" for a wug **donor/non-donor**, you get 50 tokens. Remember that you have to pay 10 tokens to "Donate". The default status is donated.

- If you "Donate" and you are matched to a wug **donor**, you gain **40 tokens**
- If you "Donate" and you are matched to a wug **non-donor**, you gain **-10 tokens**
- If you "Do not donate" and you are matched to a wug **donor**, you gain 0 token
- If you "Do not donate" and you are matched to a wug **non-donor**, you gain **50 tokens**

You are matched to a wug owner who is drawn randomly from 10 participants from the same Klee family as you are. Among these 10 participants, there are:

- **5** wug **donor** and **5** wug **non-donors**

And the specific wug owner you are matched with paid 25 tokens to buy the signal to tell you **to "NOT donate (opt out)"**

Do you want to

Donate

Do not donate

→

Figure A44: Donation Decision Screen 4

Recall: If you "Donate"/"Do not donate" for a wug **donor/non-donor**, you get 50 tokens. Remember that you have to pay 10 tokens to "Donate". The default status is donated.

- If you "Donate" and you are matched to a wug **donor**, you gain **40 tokens**
- If you "Donate" and you are matched to a wug **non-donor**, you gain **-10 tokens**
- If you "Do not donate" and you are matched to a wug **donor**, you gain **-19 tokens**
- If you "Do not donate" and you are matched to a wug **non-donor**, you gain **31 tokens**

You are matched to a wug owner who is drawn randomly from 10 participants from the same Klee family as you are. Among these 10 participants, there are:

- **5** wug **donor** and **5** wug **non-donors**

There is **no record of** the specific wug owner you are matched with buying a signal **(NO SIGNAL)**

Do you want to

Donate

Do not donate, and pay 19 tokens to overturn the default

→

Figure A45: Donation Decision Screen 5

Recall: If you "Donate"/"Do not donate" for a wug **donor/non-donor**, you get 50 tokens. Remember that you have to pay 10 tokens to "Donate". The default status is donated.

- If you "Donate" and you are matched to a wug **donor**, you gain **40 tokens**
- If you "Donate" and you are matched to a wug **non-donor**, you gain **-10 tokens**
- If you "Do not donate" and you are matched to a wug **donor**, you gain 0 token
- If you "Do not donate" and you are matched to a wug **non-donor**, you gain **50 tokens**

You are matched to a wug owner who is drawn randomly from 10 participants from the same Klee family as you are. Among these 10 participants, there are:

- **9** wug **donor** and **1** wug **non-donors**

And the specific wug owner you are matched with paid 25 tokens to buy the signal to tell you **to "NOT donate (opt out)"**

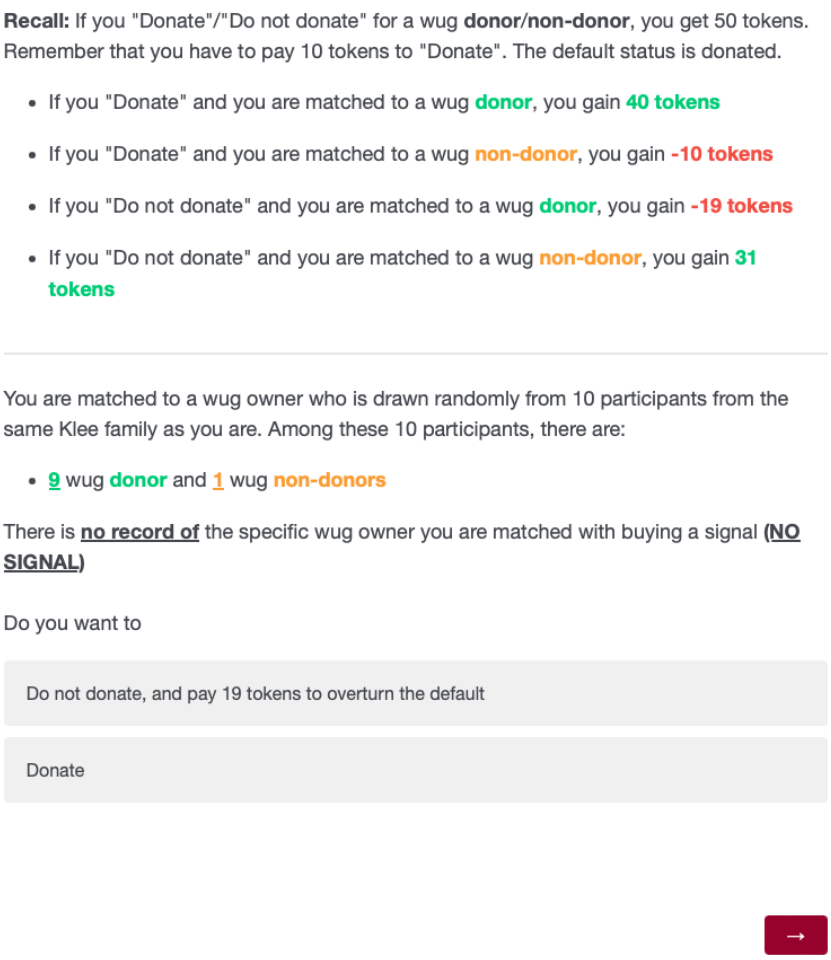
Do you want to

Donate

Do not donate

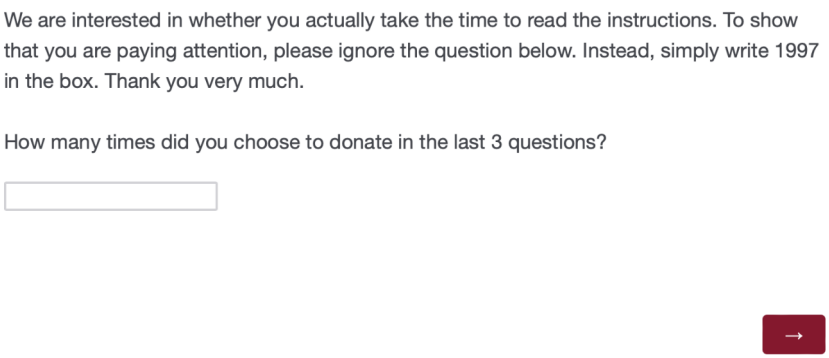
→

Figure A46: Donation Decision Screen 6



In the end of Part 3, we let all participants finish one more attention check question (Figure A47). Then, they are directed to value-related and demographic questions, as shown in Online Appendix A.3.6.

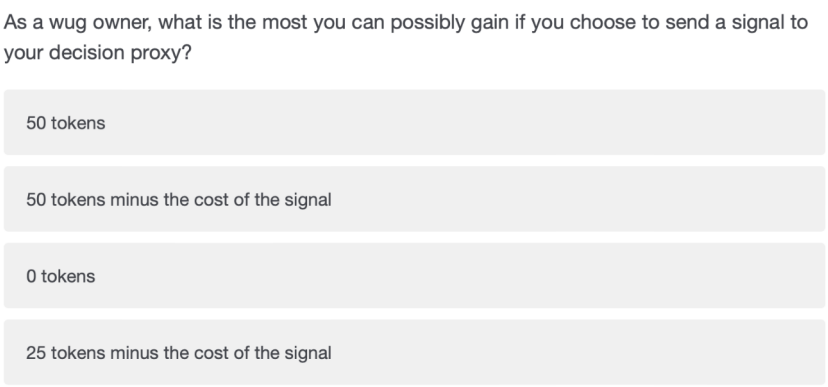
Figure A47: Attention Check Question 2



A.3.5 Comprehension Quiz

To ensure and reinforce comprehension of the rules of the experiment, we add three comprehension questions (Figure A48, A49, and A50) after participants learn the default regime (opt-in vs. opt-out), signal cost ($0.75 vs. $0.25), and cost to decision proxy of overturning defaults or signals ($0.19 vs. $0.01) from the overview of Part 2 and Part 3. Then after participants learn whether they are donors or non-donors, they will be shown another four comprehension questions (Figure A51, A52, A53, A54, A55). Moreover, we also add four comprehension questions after Part 3 instructions. If participants get a question wrong, a text box will pop up to explain why their choices are wrong. They must answer these questions correctly to proceed to the next section. The questions are shown in the figures below.

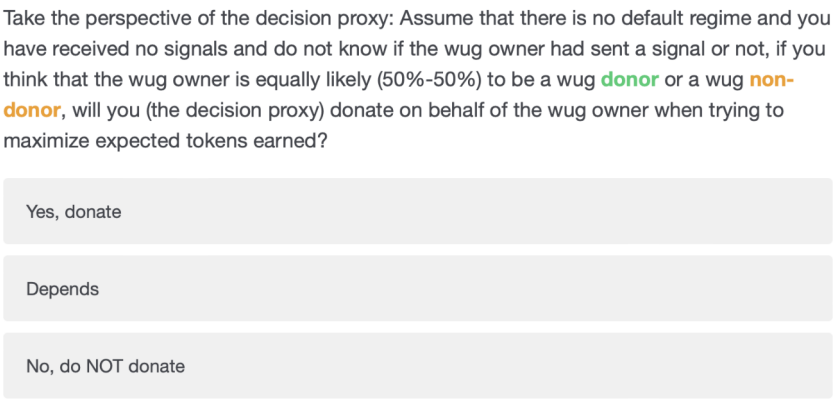
Figure A48: Comprehension Question 1



If the question shown in Figure A49 is not answered correctly, the following explanation will be shown for all treatment conditions:

- "Consider that the decision proxy is indifferent between donating or not if there is no cost to donate someone's wug. However, donating someone's cost you 10 tokens (something that can break the tie)."

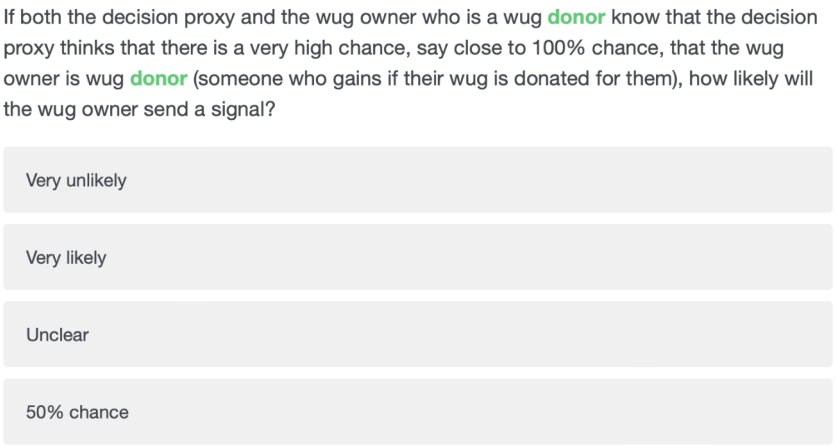
Figure A49: Comprehension Question 2



If the question shown in Figure A50 is not answered correctly, the following explanation will be shown for all treatment conditions:

- "Given the beliefs of the decision proxy, it is almost certain that they will donate the wug on behalf of the wug owner anyway so it might not be worth it for the wug owner to incur a cost to signal."

Figure A50: Comprehension Question 3



If the question shown in Figure A51 is not answered correctly, the following explanation will be shown depending on the treatment condition:

- For default status is donated and the participant is a wug donor: "The default status is donated, so you can ONLY send the signal to opt out. However, you're a wug donor, which means you would not want to send a signal to tell the decision proxy to Not Donate. (donating is best for you!)"

- For default status is not donated and the participant is a wug non-donor: "The default status is not donated, so you can ONLY send the signal to opt in. However, you're a wug nondonor, which means you would not want to send a signal to tell the decision proxy to Donate. (not donating is best for you!)"

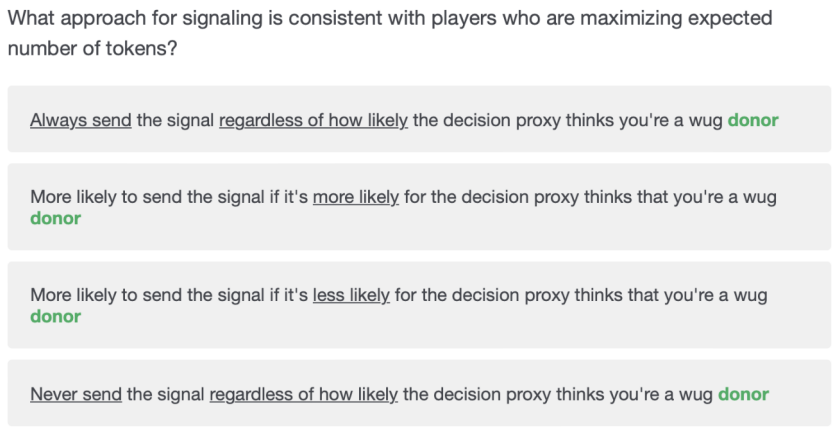
- For default status is donated, the participant is a wug non-donor and the signal cost is $75 tokens: "Even if your role is consistent with the decision proxy's choice, you'll only get a maximum of 50 tokens, but sending the signal will cost you 75 tokens (which is more than 50 tokens!)"

- For default status is not donated, the participant is a wug donor and the signal cost is $75 tokens: "Even if your role is consistent with the decision proxy's choice, you'll only get a maximum of 50 tokens, but sending the signal will cost you 75 tokens (which is more than 50 tokens!)"

- For default status is donated, the participant is a wug non-donor and the signal cost is $25 tokens: "The default status is donated, and you're a wug non-donor. If it's more likely that the decision proxy thinks that you're a wug donor, the more likely they will Donate on your behalf, the more you might want to do something to change that (i.e. send a signal)."

- For default status is not donated, the participant is a wug donor and the signal cost is $25 tokens: "The default status is not donated, and you're a wug donor. If it's less likely that the decision proxy thinks that you're a wug donor, the less likely they will Donate on your behalf, the more you might want to send signal to tell the proxy that you want to opt in."

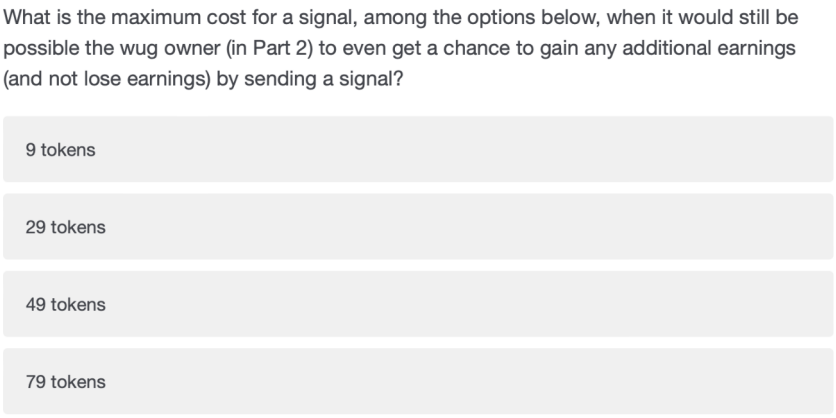
Figure A51: Comprehension Question 4



If the question shown in Figure A52 is not answered correctly, the following explanation will be shown for all treatment conditions:

- "Consider that the most wug owners can earn is 50 tokens if the proxy made the "right decision", the wug owner cannot possibly earn any additional tokens by sending a signal if the signals cost 50 or more tokens."

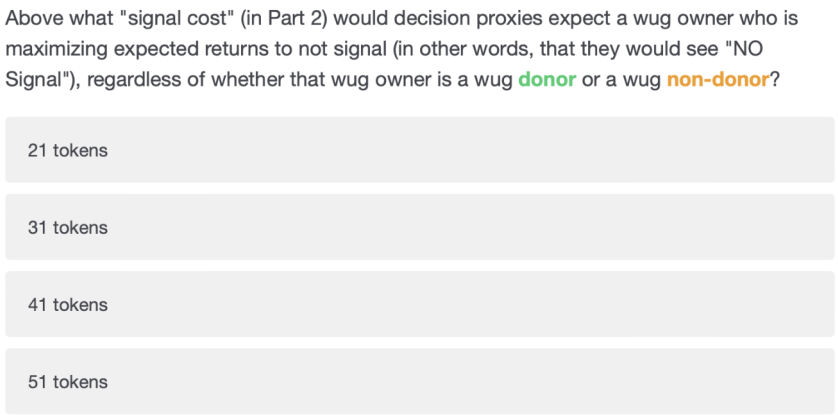
Figure A52: Comprehension Question 5



If the question shown in Figure A53 is not answered correctly, the following explanation will be shown for all treatment conditions:

- "Consider that the most wug owners can earn is 50 tokens if the proxy made the "right decision", the wug owner cannot possibly earn any additional tokens by sending a signal if the signals cost 50 or more tokens."

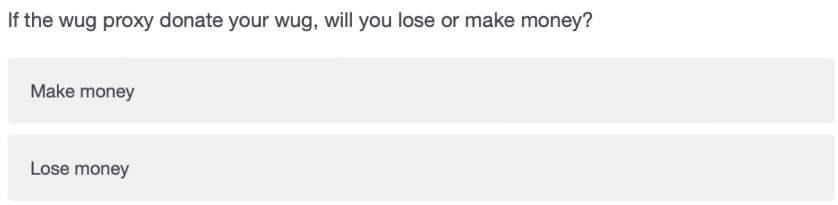
Figure A53: Comprehension Question 6



Only when the participants are wug donors can they see the question shown in Figure A54. And if it is not answered correctly, the following explanation will be shown:

- "You're a wug donor, so if the proxy donate your wug, you can get 50 tokens (instead of 0)"

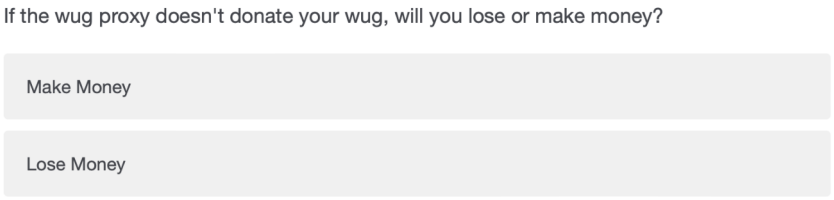
Figure A54: Comprehension Question 7 for Wug Donor



Only when the participants are wug non-donors can they see the question shown in Figure A55. And if it is not answered correctly, the following explanation will be shown:

- "You're a wug non-donor, so if the proxy doesn't donate your wug, you'll get 50 tokens (instead of 0)"

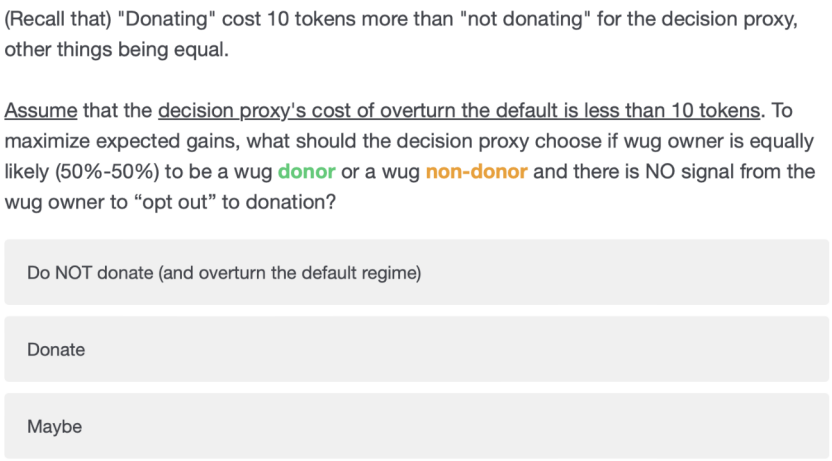
Figure A55: Comprehension Question 7 for Wug Non-Donor



Only when the default status is donated can the participants see the question shown in Figure A56. And if it is not answered correctly, the following explanation will be shown:

- "Given the 50-50, the decision proxy can gain an expected 25 for not donating but an expected 25 minus 10 for donating, therefore, if the cost of overturning is less than the difference, which is 10, the decision proxy might get higher expected tokens by overturning. But if the cost of overturning is too high, they might not want to overturn."

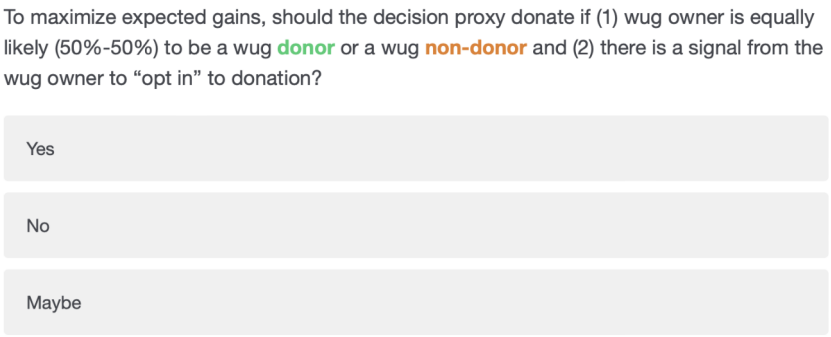
Figure A56: Comprehension Question 8



Only when the default status is not donated can the participants see the question shown in Figure A57. And if it is not answered correctly, the following explanation will be shown:

- "Consider that the most wug owners can earn is 50 tokens if the proxy made the "right decision", the wug owner cannot possibly earn any additional tokens by sending a signal if the signals cost 50 or more tokens."

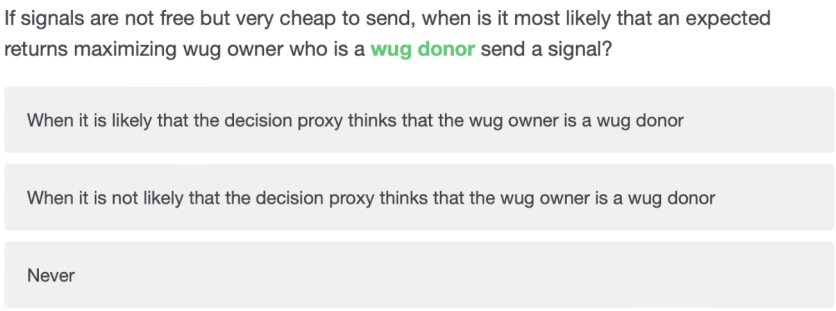
Figure A57: Comprehension Question 9



If the question shown in Figure A58 is not answered correctly, the following explanation will be shown depending on the treatment condition:

- For default status is not donated: "The only signal that can be send is "donate", so signals are useful to send if the wug owner is trying to "CHANGE THE MIND" of the decision proxy who given the information they have might choose a decision that contradicts what the wug owner wants."
- For default status is donated: "The only signal that can be send is "not donate", so signals are useful to send if the wug owner is trying to "CHANGE THE MIND" of the decision proxy who given the information they have might choose a decision that contradicts what the wug owner wants."

Figure A58: Comprehension Question 10



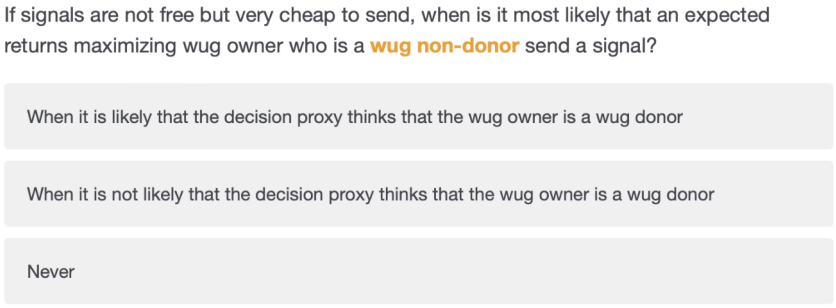
If the question shown in Figure A59 is not answered correctly, the following explanation will be shown depending on the treatment condition:

- For default status is not donated: "The only signal that can be send is "donate", so signals are useful to send if the wug owner is trying to "CHANGE THE MIND"

of the decision proxy who given the information they have might choose a decision that contradicts what the wug owner wants."

- For default status is donated: "The only signal that can be send is "not donate", so signals are useful to send if the wug owner is trying to "CHANGE THE MIND" of the decision proxy who given the information they have might choose a decision that contradicts what the wug owner wants."

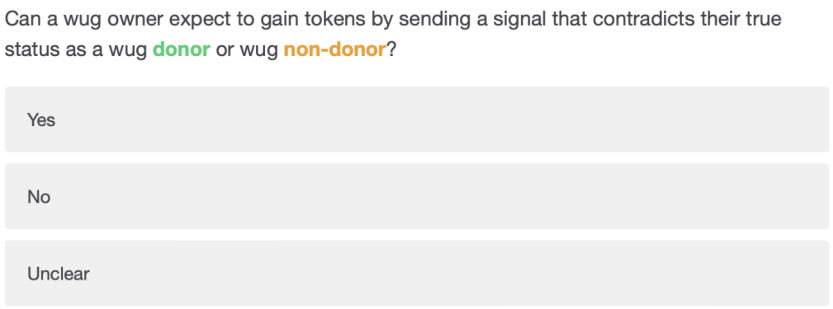
Figure A59: Comprehension Question 11



If the question shown in Figure A60 is not answered correctly, the following explanation will be shown for all treatment conditions:

- "Consider that the decision proxies are aware that the signals are costly for wug owners to send."

Figure A60: Comprehension Question 12



A.3.6 Collection of Value-related and Demographic Information

We used Qualtrics software to collect value-related and demographic information from customers who clicked the URL and chose to participate in the experiment on Prolific.

For value-related questions (presented in randomized order), participants are asked to evaluate the importance of various values as guiding principles in their lives (Figure A61). Each value is rated on a scale from –1 to 7, with higher scores indicating greater personal importance. As shown in the following, the values include pragmatism (Figure A62), purity (Figure A63), justice (Figure A64), social recognition (Figure A65), pleasure (Figure A66), compassion (Figure A67), spirituality (Figure A68), equality (Figure A69), giving (Figure A70), freedom (Figure A71), and respect for tradition (Figure A72).

Figure A61: Instruction of Value-related Questions

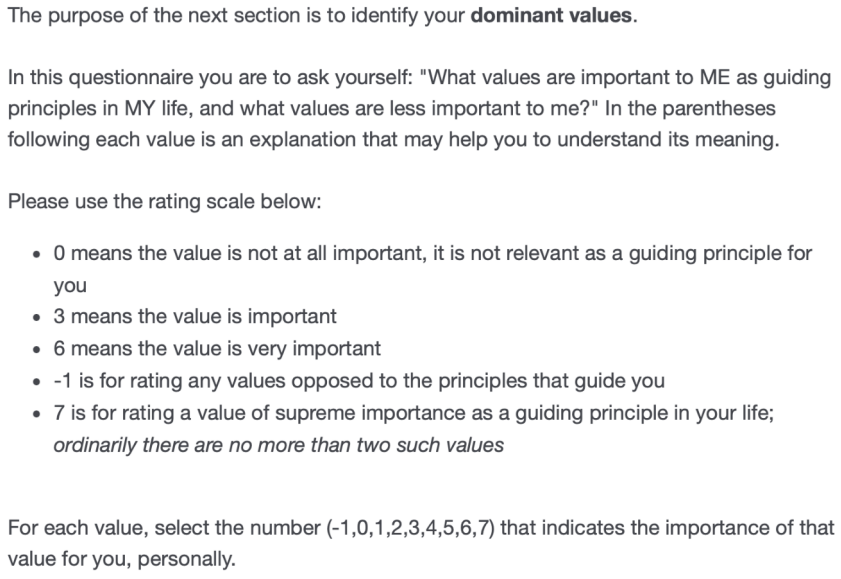


Figure A62: Survey Question on the Importance of Pragmatism

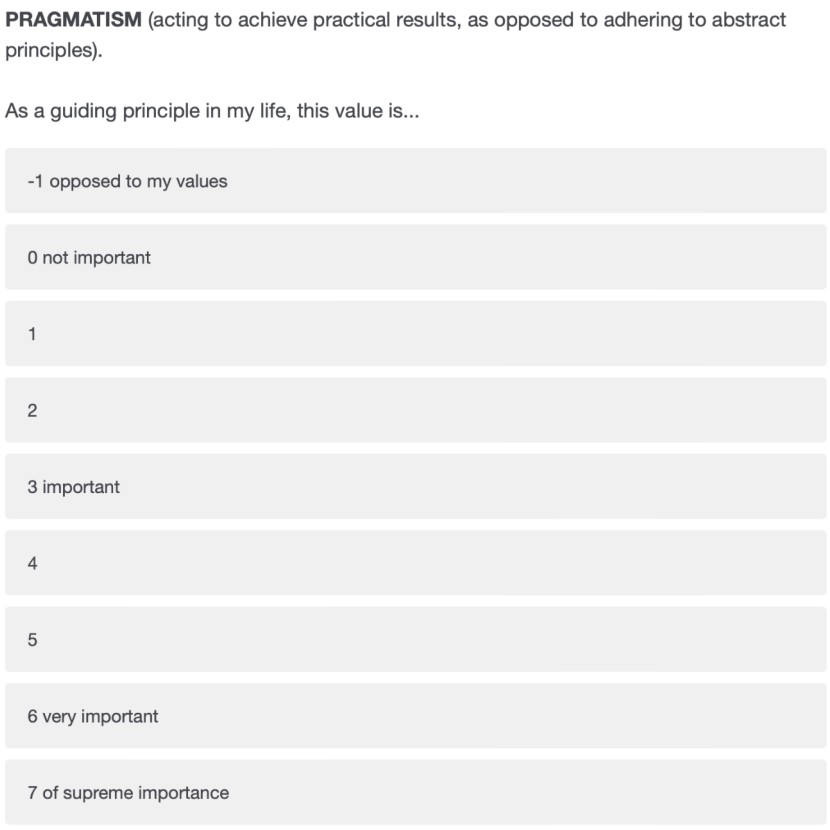


Figure A63: Survey Question on the Importance of Purity

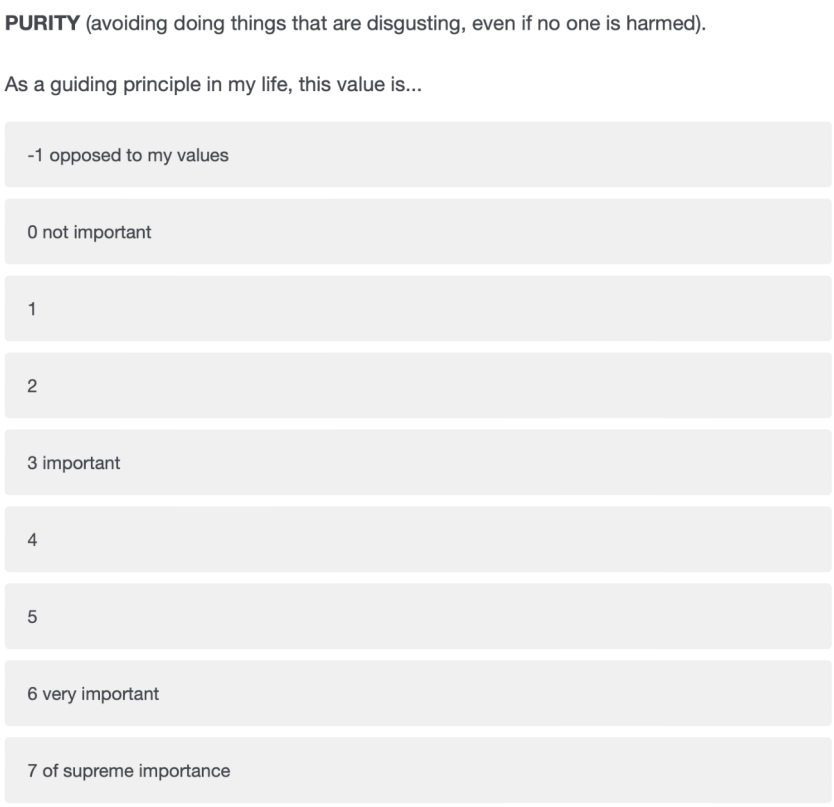


Figure A64: Survey Question on the Importance of Justice



Figure A65: Survey Question on the Importance of Social Recognition

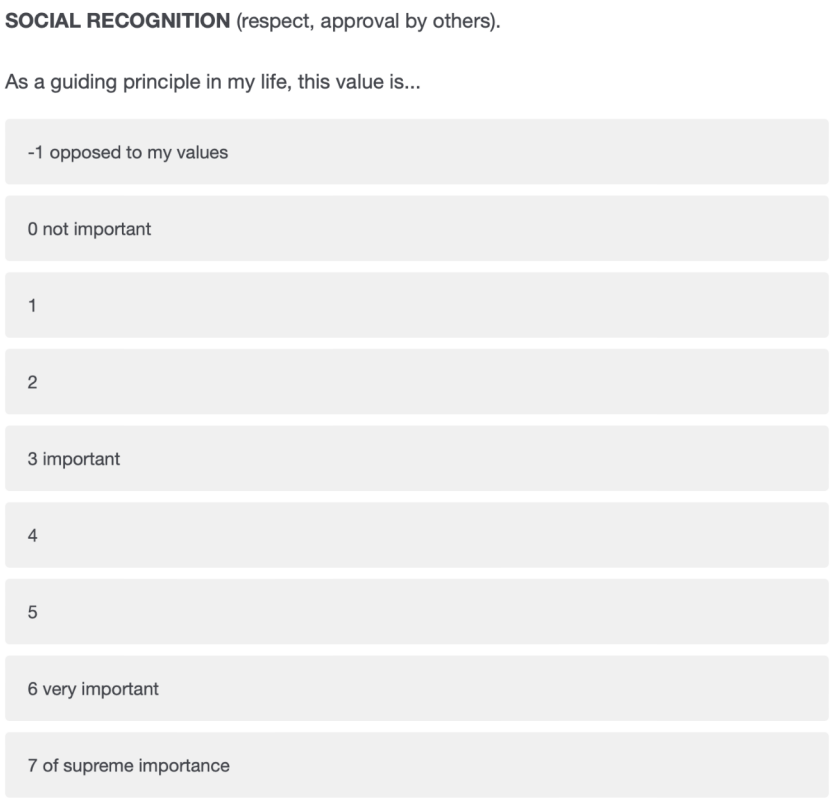


Figure A66: Survey Question on the Importance of Pleasure



Figure A67: Survey Question on the Importance of Compassion

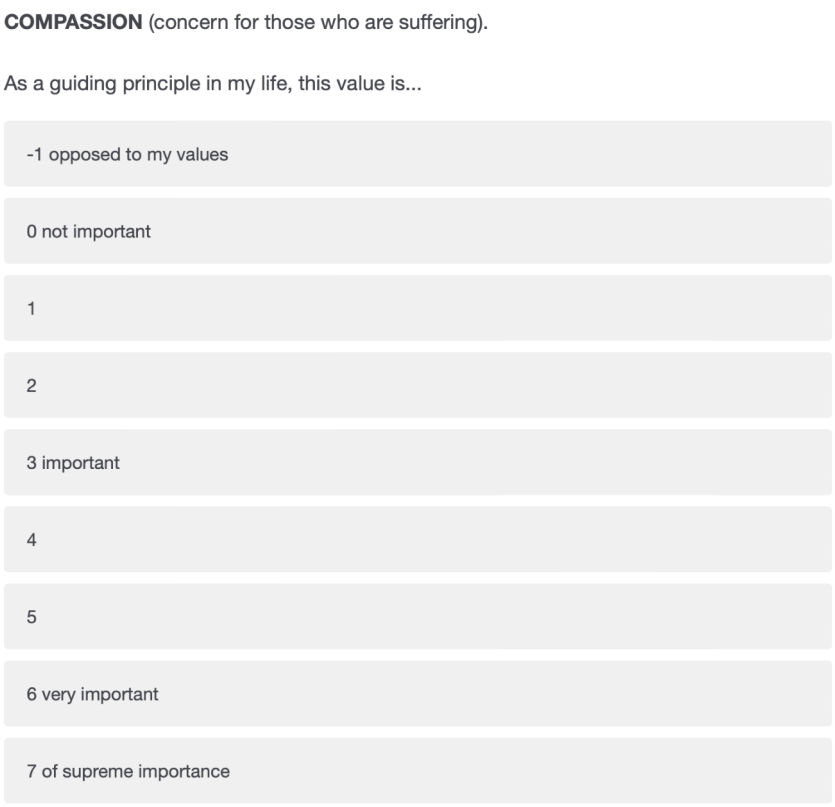


Figure A68: Survey Question on the Importance of Spirituality

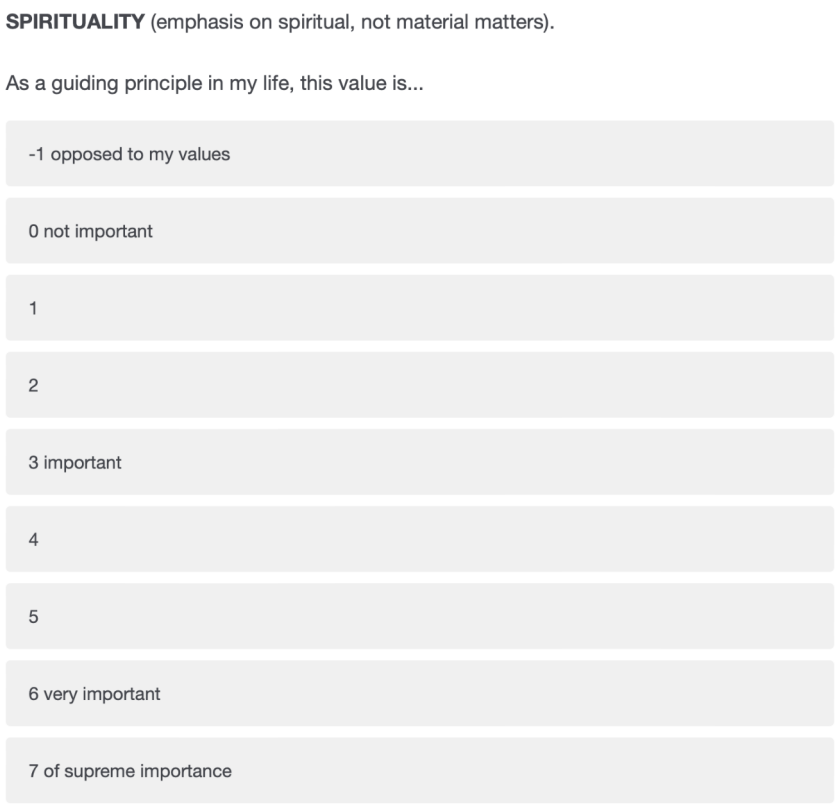


Figure A69: Survey Question on the Importance of Equality



Figure A70: Survey Question on the Importance of Giving

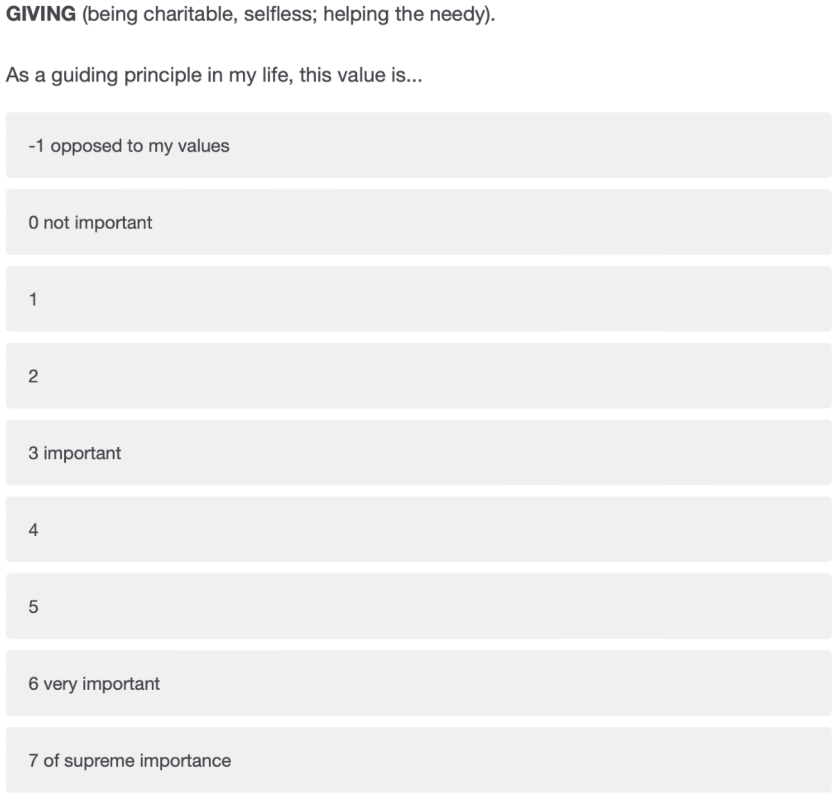


Figure A71: Survey Question on the Importance of Freedom

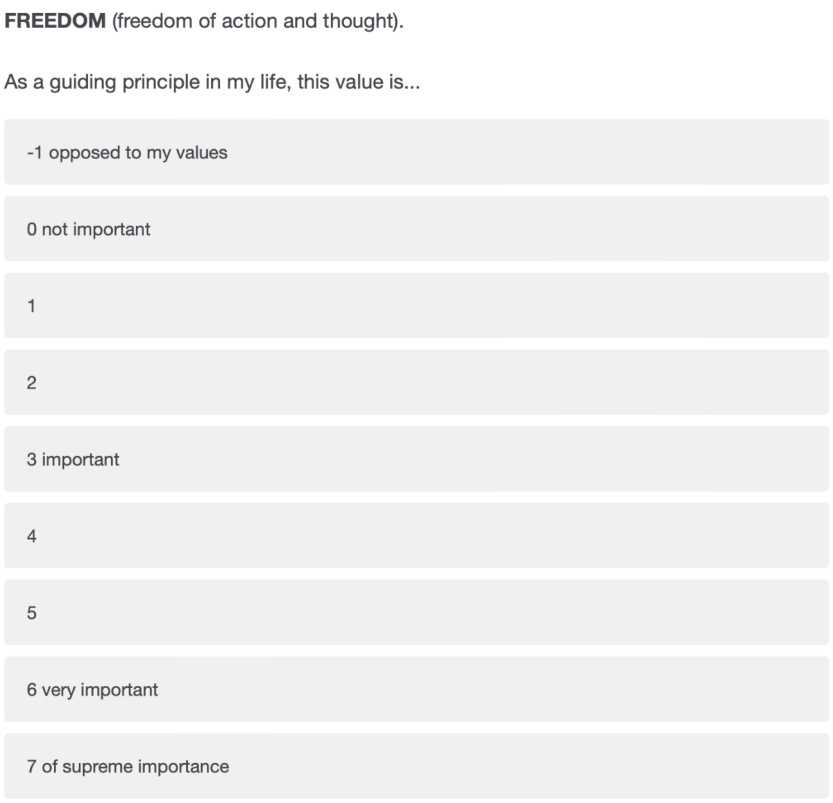
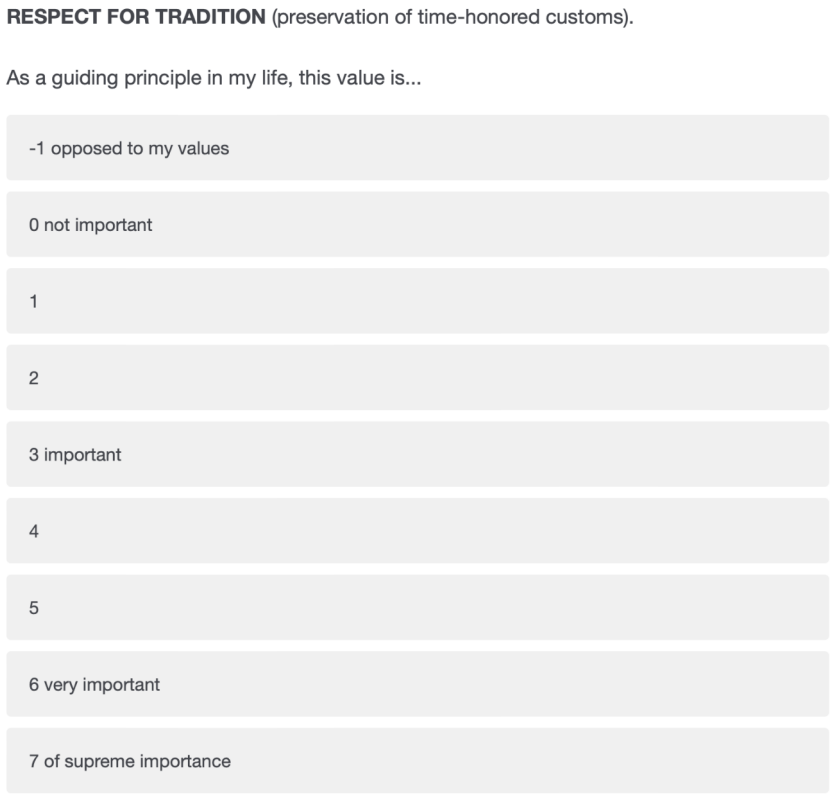


Figure A72: Survey Question on the Importance of Respect for Tradition



In the final section of the experiment, the participants are asked a number of questions (in the order of appearance) about their gender identity (Figure A73), race and ethnicity (Figure A74), sexual orientation (Figure A75), disability and chronic condition (Figure A76), zip code (Figure A77), highest education level (Figure A78), high school type and location (Figure A79, A80), marital status (Figure A81), number of children (Figure A82), employment status (Figure A83), occupation type (Figure A84), present religion and importance in their lives(Figure A85, A86), life satisfaction (Figure A87), health insurance (Figure A88), social class (Figure A89), political affiliation (Figure A90), favorability toward the democratic or republican parties (Figure A91, A92), and health status (Figure A93).

Figure A73: Survey Question about Participant's Gender Identity

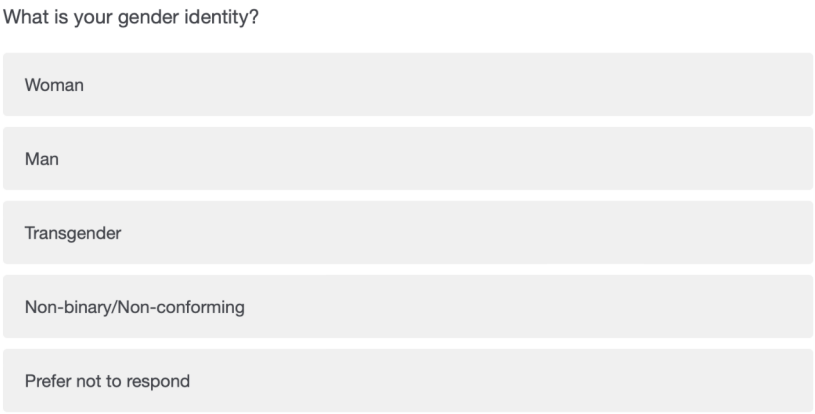


Figure A74: Survey Question about Participant's Race and Ethnicity

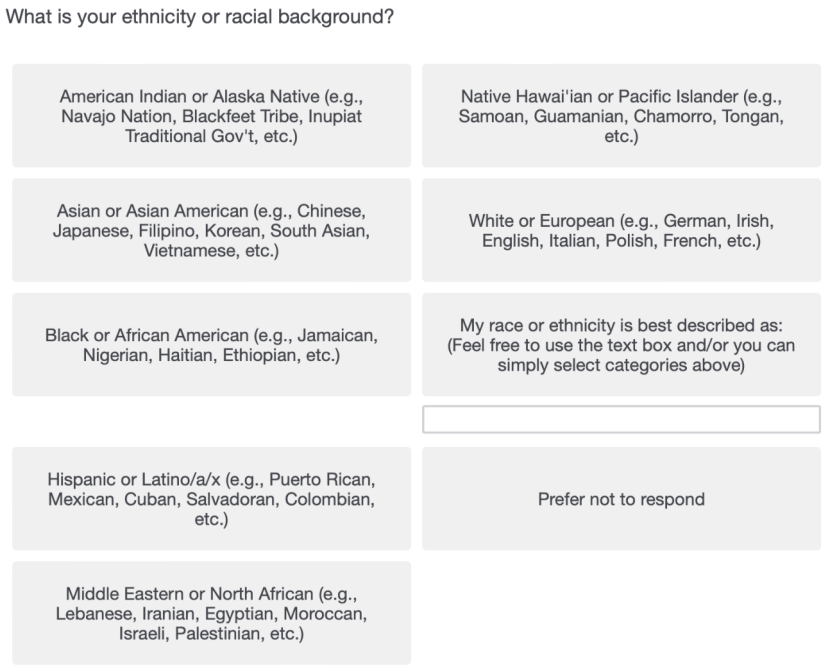


Figure A75: Survey Question about Participant's Sexual Orientation

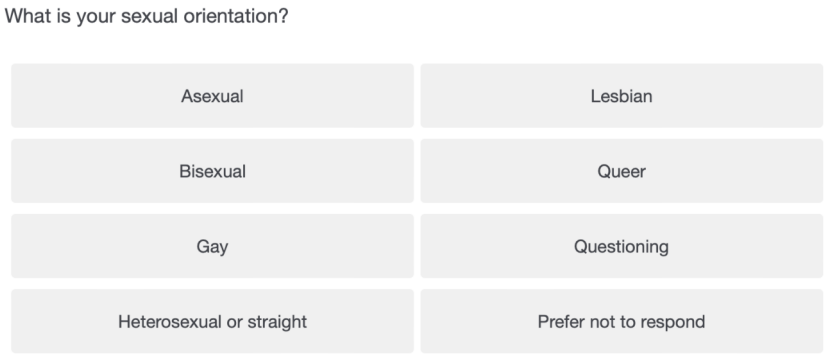


Figure A76: Survey Question about Participant's Disability and Chronic Condition

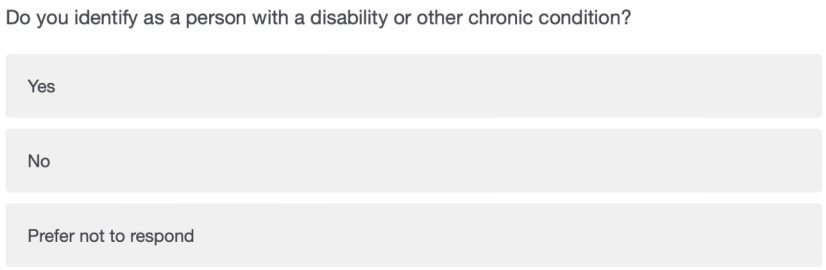


Figure A77: Survey Question about Participant's Zip Code

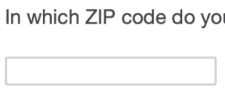


Figure A78: Survey Question about Participant's Highest Education Level

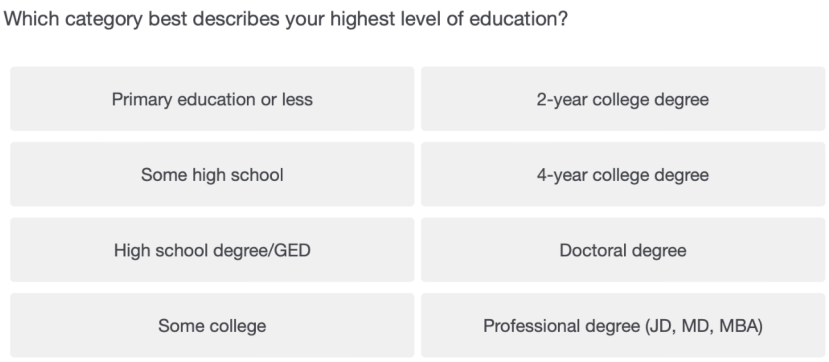


Figure A79: Survey Question about Participant's High School Type

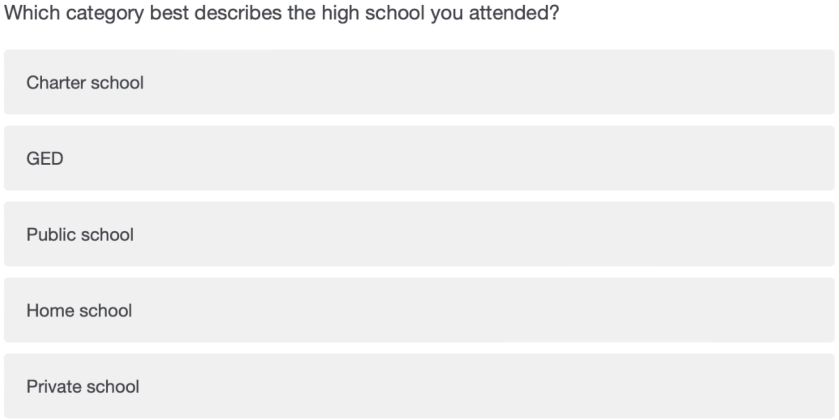


Figure A80: Survey Question about Participant's High School Location

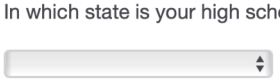


Figure A81: Survey Question about Participant's Marital Status

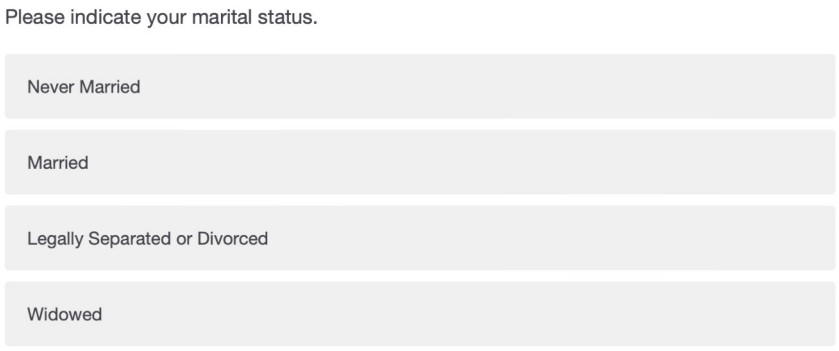


Figure A82: Survey Question about Participant's Number of Children

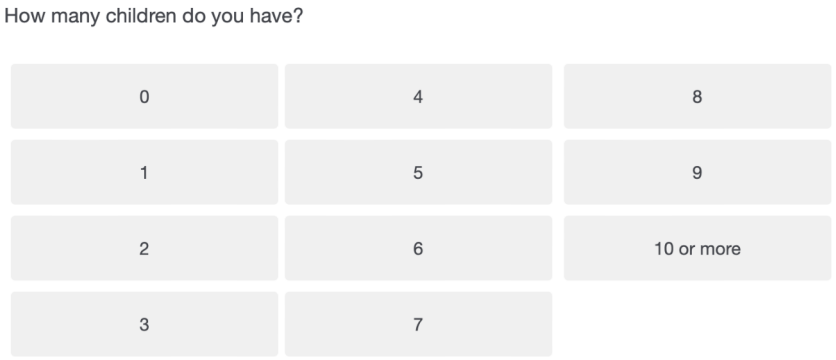


Figure A83: Survey Question about Participant's Employment Status

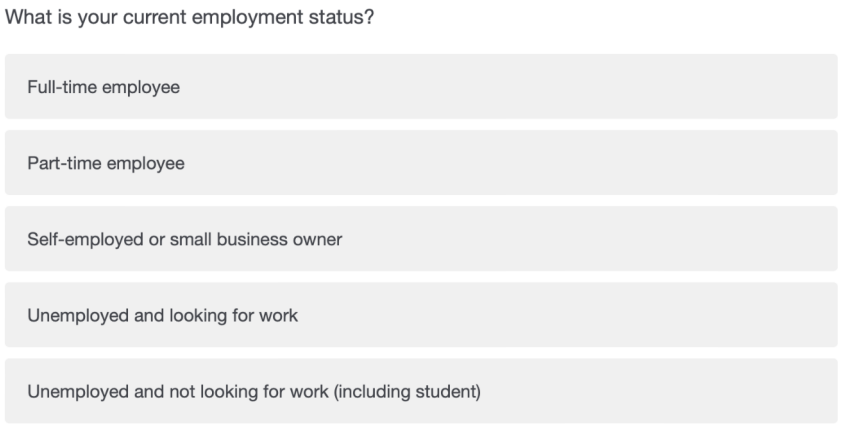


Figure A84: Survey Question about Participant's Occupation Type



Figure A85: Survey Question about Participant's Present Religion

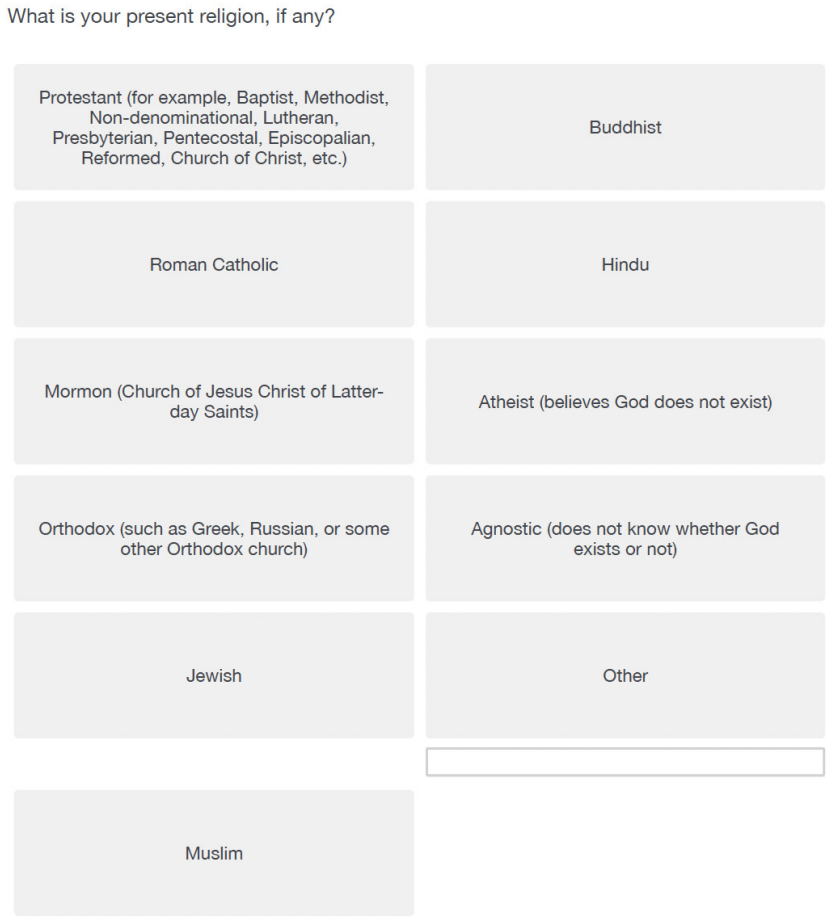


Figure A86: Survey Question on Religion's Importance in Participants' Lives

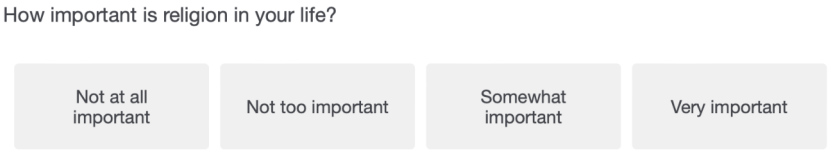


Figure A87: Survey Question about Participants' Life Satisfaction

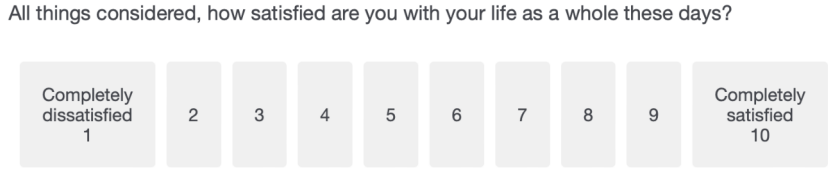


Figure A88: Survey Question about Participants' Health Insurance

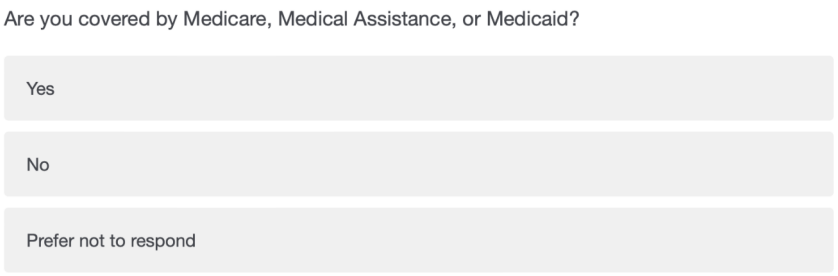


Figure A89: Survey Question about Participants' Social Class



Figure A90: Survey Question about Participants' Political Affiliation



Figure A91: Survey Question on Participants' Favorability Toward the Democratic Party

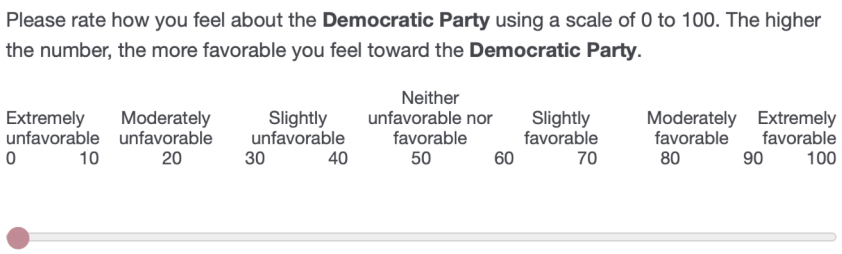


Figure A92: Survey Question on Participants' Favorability Toward the Republican Party

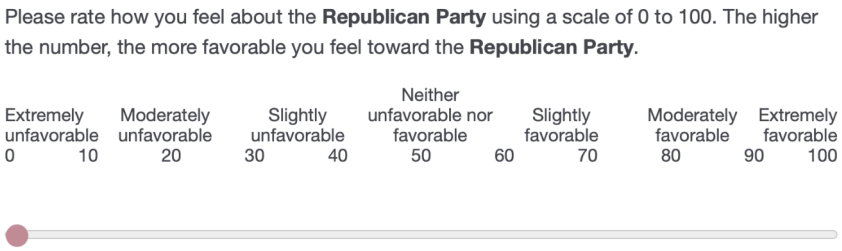
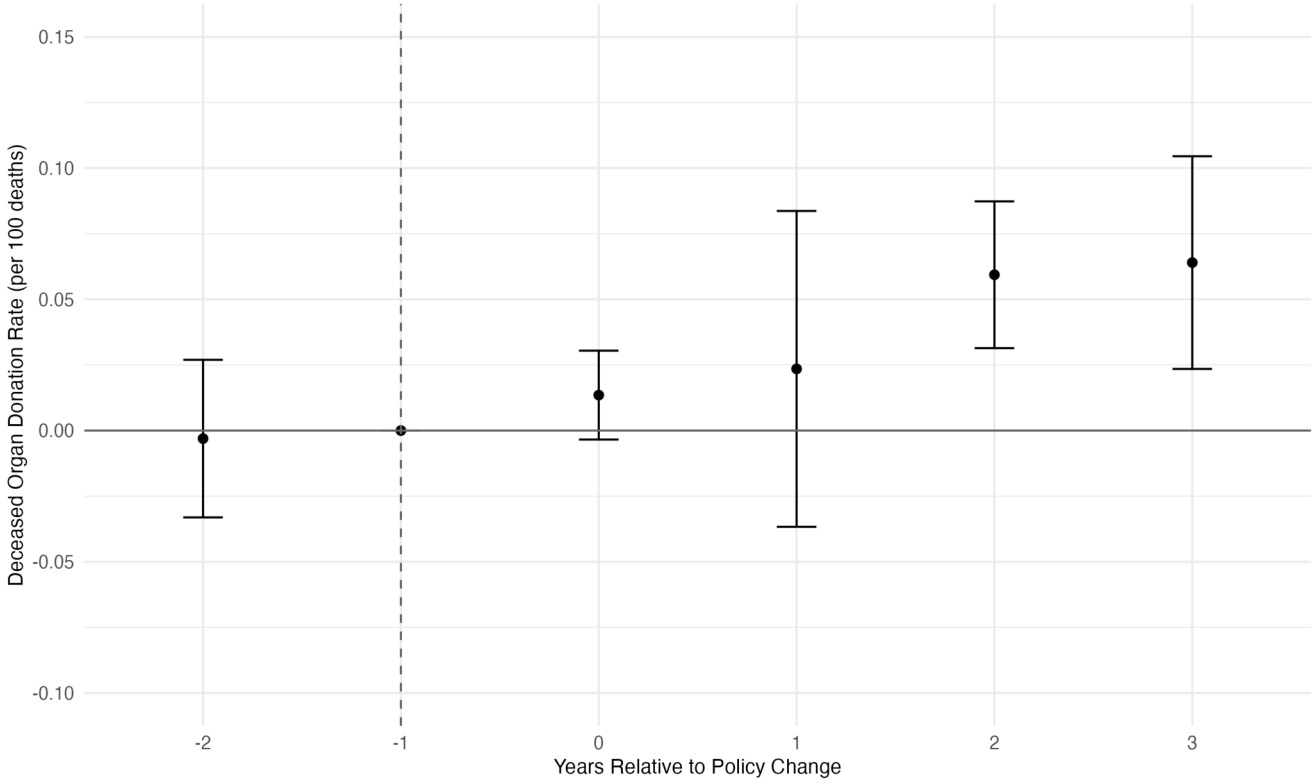


Figure A93: Survey Question about Participants' Health Status



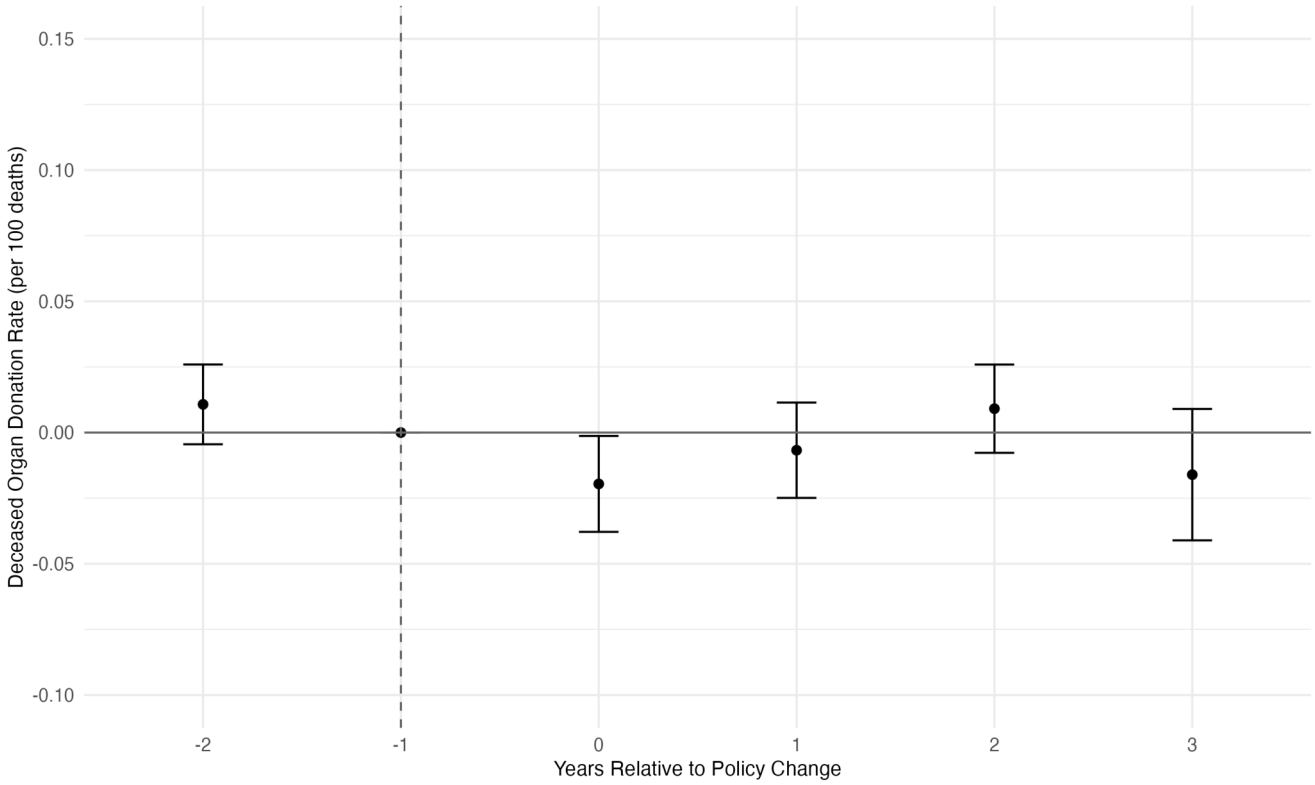
A.4 Supplementary Figures

Figure A94: Event Study Graph for Strict Opt Out Countries within Median Quartiles (Average year-0-donation-per-100-deaths = 0.11)



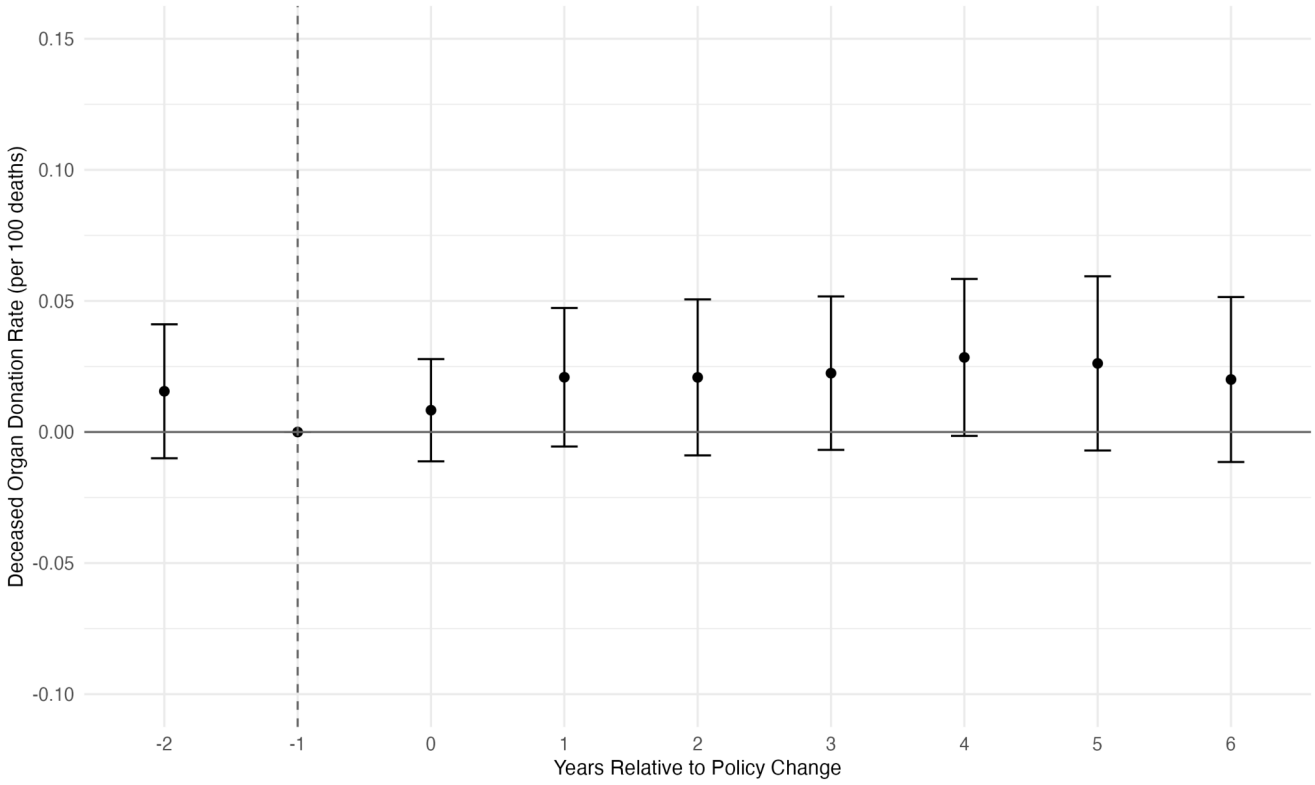
Notes: Estimated based on Equation 1 including panel data on Austria and Belgium that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Austria (1980-1985), Belgium* (1984-1989), Norway (1981-1989), Germany, Netherlands, and UK (1980-1989). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

Figure A95: Event Study Graph for Weak Opt Out Countries within Median Quartiles (Average year-0-donation-per-100-deaths = 0.10)



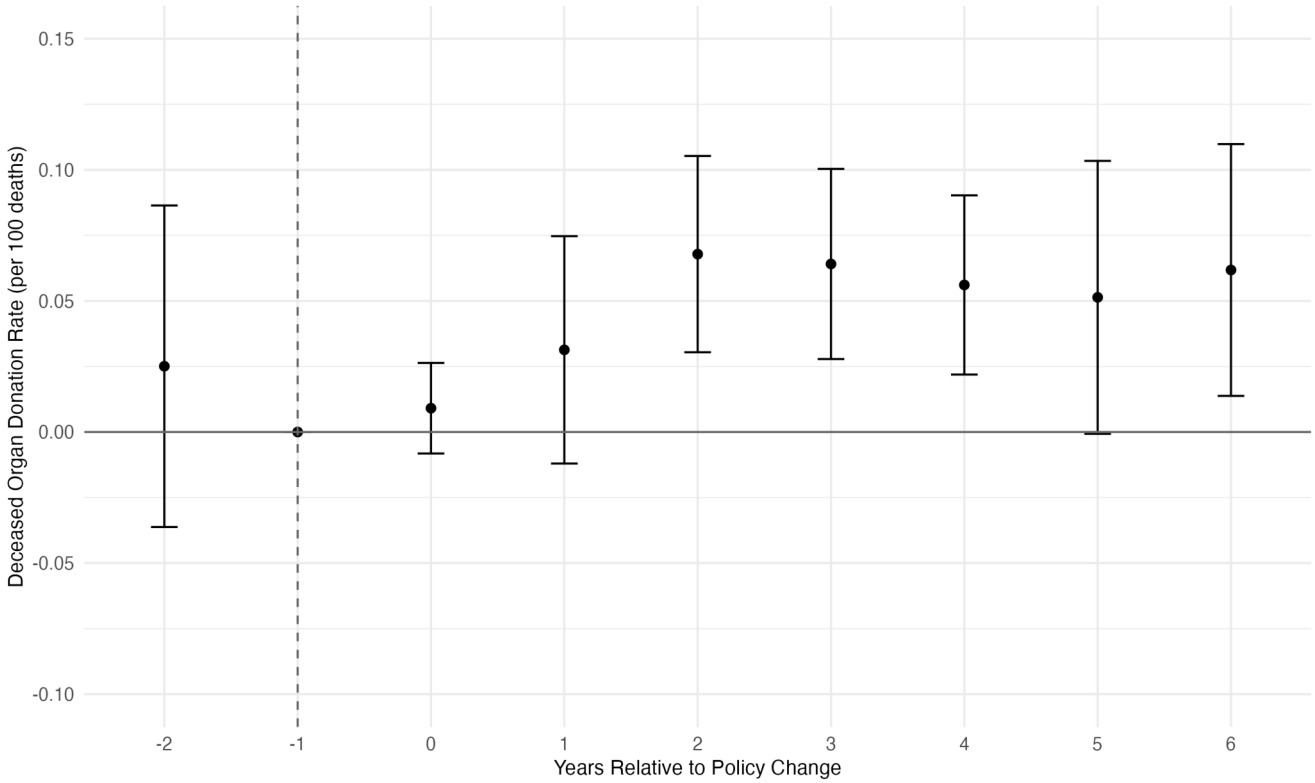
Notes: Estimated based on Equation 1 including panel data on Chile and Sweden that changed policy nationally from opt-in to opt-out which has at least 8 control countries (countries in the same continent) with data that extend at least 2 years before and 3 years after the event. The sample includes Sweden (1994-1999), Greece (1995-1999), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-1999), and Chile* (2008-2013), Bolivia (2008, 2010-2013), Ecuador (2009-2013), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2013). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals*

Figure A96: Event Study Graph for All Countries (6 post years)



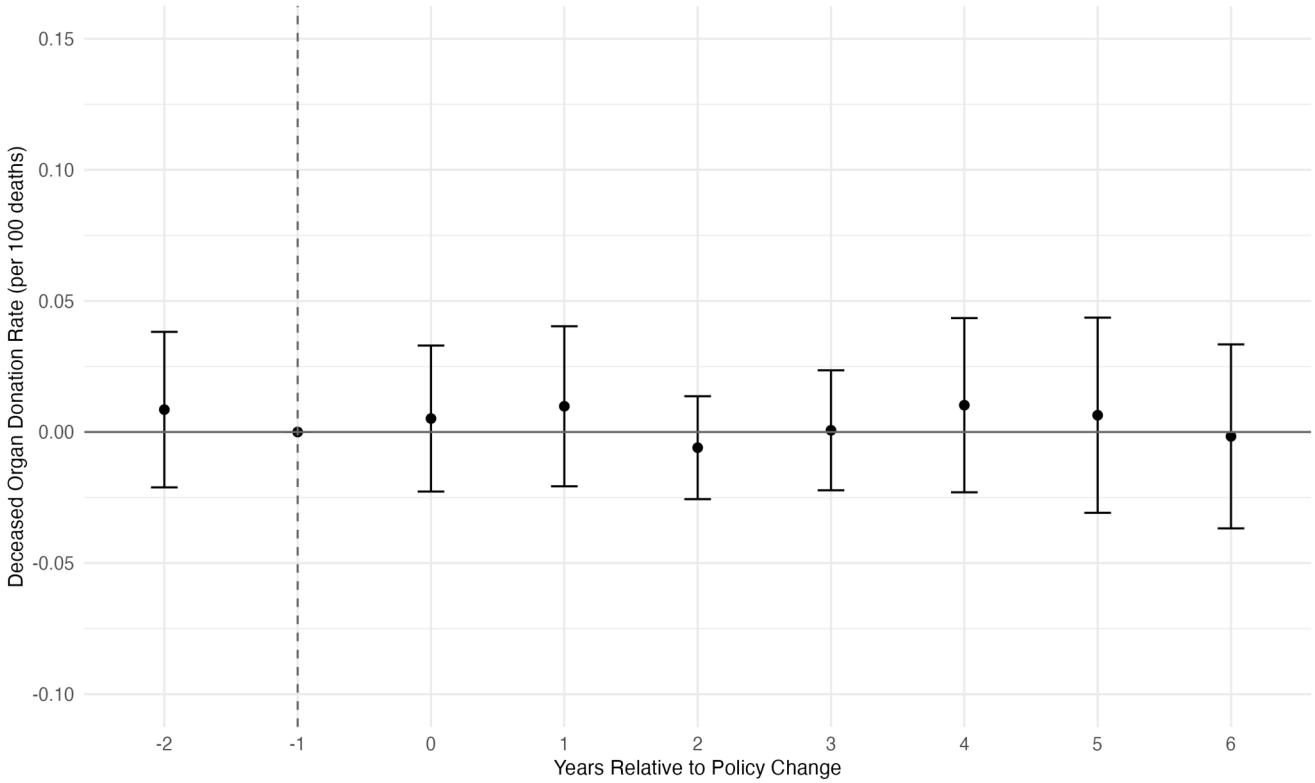
Notes: Estimated based on Equation 1 including panel data on all countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 6 years after the event. The sample includes Austria, Luxembourg* (1980-1988), Belgium* (1984-1992), Finland* (1983-1991), Norway (1981-1992), Germany, Netherlands, and UK (1980-1992). The sample also includes Sweden* (1994-2002), Greece (1995-2002), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-2002), and Chile* (2008-2016), Uruguay* (2011-2019), Bolivia (2008, 2010-2019), Ecuador (2009-2019), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2019). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals.*

Figure A97: Event Study Graph for Strict Opt Out Countries (6 post years)



Notes: Estimated based on Equation 1 including panel data on all strict opt out countries that changed policy nationally from opt-in to opt-out which has at least 3 control countries (countries in the same continent) with data that extend at least 2 years before and 6 years after the event. The sample includes Austria, Luxembourg* (1980-1988), Belgium* (1984-1992), Norway (1981-1992), Germany, Netherlands, and UK (1980-1992). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals.*

Figure A98: Event Study Graph for Weak Opt Out Countries (6 post years)



Notes: Estimated based on Equation 1 including panel data on all weak opt out countries that changed policy nationally from opt-in to opt-out which has at least 4 control countries (countries in the same continent) with data that extend at least 2 years before and 6 years after the event. The sample includes Finland, Germany, Netherlands, Norway, and UK (1983-1991). The sample also includes Sweden* (1994-2002), Greece (1995-2002), Austria, Belgium, Croatia, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Switzerland, UK (1994-2002), and Chile* (2008-2016), Uruguay* (2011-2019), Bolivia (2008, 2010-2019), Ecuador (2009-2019), Argentina, Brazil, Colombia, Cuba, Dominican Republic, Panama, Paraguay (2008-2019). Points show estimated coefficients; Countries marked with an asterisk * changed policy nationally from opt-in to opt-out; error bars indicate 95% confidence intervals.*

A.5 Supplementary Tables

We conducted a more systematic replication and robustness assessment of the treatment-effect estimates reported in Abadie and Gay (2006) and in Johnson and Goldstein (2003). Our analysis began by reproducing their baseline results using comparable observation windows, shifting the temporal frame slightly (three years forward), and employing the identical set of countries used in the original studies along with a largely similar empirical specification. We then broadened the sample to include all countries with available data for the relevant time periods, and we evaluated the sensitivity of the estimates to minor specification changes, such as the inclusion of overall death rates as an additional control. The findings reported by Abadie and Gay (2006) prove largely robust across these exercises. The principal result in Johnson and Goldstein (2003) (page 1339)— significant at the 2 percent level—loses statistical significance at all conventional levels when the regression sample is expanded beyond the 17 countries chosen in the original study. Moreover, in a resampling exercise in which we repeatedly draw 17 countries at random from the full set of countries with available data (for $1,000$ times), the originally reported 2 percent significance level is recovered in only about one-quarter of replications, and significance at the 5 percent level appears in roughly one-third of them. Finally, while adding the death rate control (since the outcome variable is donation after death per population) leaves the Abadie and Gay (2006) findings essentially unchanged, it renders the Johnson and Goldstein (2003) estimate non-significant at all conventional levels.

Table V: The effect of presumed consent legislation on cadaveric organ donation (pooled OLS, 1994-2005)

	Dependent variable: Log cadaveric organ donors per million population										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Presumed consent	0.161	0.106	0.285**	0.379***	0.250*	0.208	0.340***	0.271**	0.205	0.290**	0.238**
	(0.133)	(0.127)	(0.130)	(0.119)	(0.127)	(0.127)	(0.115)	(0.105)	(0.127)	(0.115)	(0.109)
Log GDP per capita			0.238*		0.289*	0.335**	0.264*	0.296**	0.353**	0.337**	0.345**
			(0.126)		(0.140)	(0.144)	(0.143)	(0.116)	(0.150)	(0.138)	(0.123)
Log of health expenditures per capita				0.161**							
				(0.070)							
Catholic country					0.199	0.221			0.196		
					(0.175)	(0.164)			(0.166)		
Common law					0.087	0.147	0.168	0.197	0.135	0.125	0.168
					(0.114)	(0.124)	(0.133)	(0.134)	(0.119)	(0.142)	(0.135)
Log of MVA and CVD deaths (per 1000 population)						0.245	0.176	0.292*			
						(0.175)	(0.202)	(0.167)			
Log of MVA deaths (per 1000 population)									0.098	0.219	0.169
									(0.217)	(0.206)	(0.203)
Log of CVD deaths (per 1000 population)									0.213	0.174	0.268*
									(0.156)	(0.163)	(0.135)
Include Spain	Yes	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	No
Num. Obs.	263	251	263	132	263	257	257	245	257	257	245
R^2	0.073	0.055	0.164	0.323	0.211	0.222	0.174	0.198	0.223	0.192	0.208

Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: This table reports OLS estimates of the effect of presumed consent legislation on cadaveric organ donation. The estimating equation is $Donation_{ct} = \alpha + \beta\,\text{OptOut}_{ct} + \mathbf{X}'_{ct}\theta + \delta_t + \varepsilon_{ct}$, where $Donation_{ct}$ is the log of cadaveric organ donors per million population in country c and year t, OptOut_{ct} is an indicator for whether presumed consent is in effect, $\mathbf{X}_{ct}$ includes time-varying controls such as GDP per capita, health expenditures, mortality rate from motor vehicle accidents (MVA) and cerebro-vascular disease (CVD) and country-level characteristics (Catholic, Common law), and δ_t are year fixed effects. Standard errors are clustered at the country level.