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Essays in Prosocial Behavior

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in

Economics

by

Naveen Nagesh Basavanhally

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2019

The dissertation of Naveen Nagesh Basavanhally is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

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University of California San Diego

2019

DEDICATION

To my parents, whose Herculean efforts and sacrifices afford me the privileged life I enjoy today.

EPIGRAPH

When nothing seems to help, I go and look at a stonecutter hammering away at his rock, perhaps a hundred times without as much as a crack showing in it. Yet at the hundred and first blow it will split in two, and I know it was not that last blow that did it, but all that had gone before.

—Jacob Riis

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ABSTRACT OF THE DISSERTATION

Essays in Prosocial Behavior

by

Naveen Nagesh Basavanhally

Doctor of Philosophy in Economics

University of California, San Diego, 2019

Professor Viswanathan Krishnan, Co-Chair
Professor Joel Watson, Co-Chair

This dissertation examines empirical questions related to charitable giving and prosocial behavior. Chapter 1 examines the effect of mandated choice on organ donation registration rates. Chapter 2 examines the association between intra-household bargaining and charitable giving. Chapter 3 takes a machine learning approach to predict the likelihood of households engaging in intra-household bargaining.

Chapter 1

The Effects of Mandated Choice on Organ Donation: Evidence from a Natural Experiment

1.1 Introduction

The United States, along with many other developed countries around the world, has had chronic shortages in organ supply for the purposes of transplantation. According to the United States Department of Health and Human Services[1], in 2017, 59,645 people were added to the waiting list for an organ transplant, but only 34,770 transplants were conducted. Excess demand for organ transplants is not unique to 2017. Figure 1.1 shows the gap between waiting list additions and transplants conducted since 2001.

This gap results in an average of 22 people dying each day due to a shortage of organ donations.[2] How can we bridge this gap between organ supply and demand? Standard economic theory dictates that such a gap can be resolved with market-clearing prices. If the demand

[1] https://optn.transplant.hrsa.gov/

[2] http://www.organdonor.gov/about/data.html

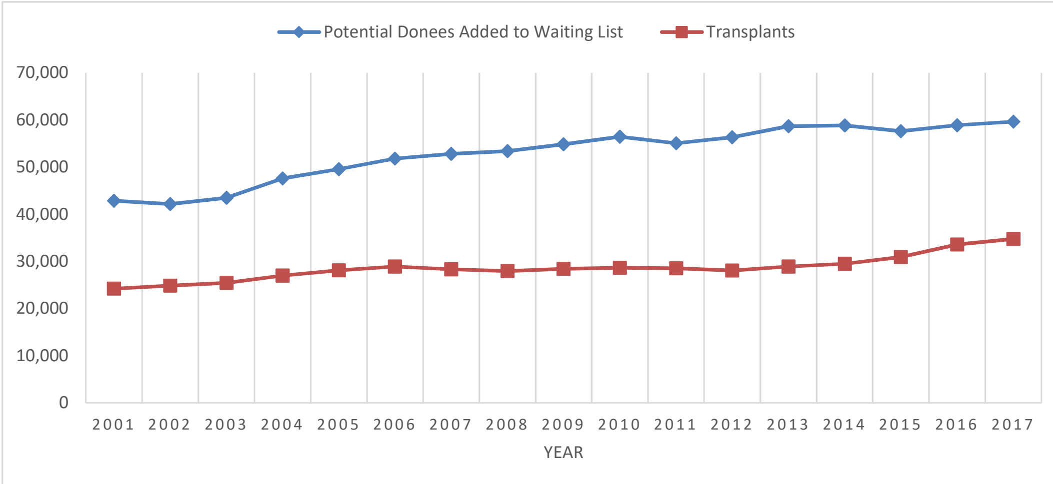


Figure 1.1: The Gap Between Potential Donee Additions and Transplants.

outweighs the supply, then an increase in the market price can reduce excess demand. In the context of organ transplants, however, increasing the supply of available organs through prices for the ever-increasing demand leads to an ethical quagmire.[3] Even if selling organs were legal, there is evidence that providing monetary incentives can possibly crowd-out intrinsic motivations and ultimately reduce prosocial behavior (see Titmuss (1970), Mellstrm and Johannesson (2008), Bruno S. Frey (1997), Uri Gneezy (2011), Uri Gneezy (2000), and Gneezy (2000)). Due to these constraints, policy-makers have looked to alternate methods and policies to increase donor registration rates. For example, Singapore enacted the Human Organ Transplant Act in 1987, which automatically enrolled its citizens to be organ donors (commonly referred to as "presumed consent").[4] If Singaporeans do not wish to donate their organs, they must opt-out. Although such a system yields higher donation rates, it is somewhat controversial, since the Singaporean government assumes that its citizens should be organ donors, unless one specifies otherwise. In addition, doctors in countries with presumed consent laws typically cannot use the organs of deceased individuals if next-of-kin have strong objections.[5] Israel has cleverly used incentives

[3]Although the purchase of organs is legal is some countries (most notably Iran), it has been outlawed in the United States by the National Organ Transplant Act of 1984.

[4]Additional European countries, such as Spain, Belgium, France, Austria, and Wales have since also adopted presumed consent policies.

[5]The preferences of next-of-kin can determine the final outcome of one's organs, especially in situations where

to implement a policy where those on a waiting list to receive organs are given higher priority, all else equal, if they are also registered organ donors.[6] Their policy is aptly nicknamed "don't give-don't get." The literature lacks empirical studies of these policies; the closest is a lab experiment by Kessler and Roth (2012), which demonstrates that such a priority system can yield much higher rates of donor registration.

Another method to increase donor registration rates is to change how people are asked to become organ donors. Data from the United States Department of Health and Human Services show that 95% adults support organ donation, but only 54% are registered.[7] This suggests that there exists a latent demand to give organs, but many people will not do so unless explicitly asked. Recent literature (Andreoni et al. (2011), Andreoni and Rao (2011), and Exley and Petrie (2016)) suggests that the people's decision to give is largely dependent on the way and the context in which they are asked. If policy-makers wish to increase donor registration rates, they must pay special attention to how individuals are asked to become donors. In the United States, the most common way to register as an organ donor is during one's driver's license application or renewal.[8] In every state, the driver's license application and renewal forms contain a question asking if the individual would like to register as an organ donor. Policy makers have considered changing the framing of this question in order to generate higher donor registration rates. This paper focuses on measuring the causal effect of one such framing change, mandated choice, on donor registration rates. We take advantage of a natural experiment that took place in the state of California due to a policy misimplmentation to examine the effect of framing on organ donation rates and find that the presence of a mandated choice requirement can increase donor registration rates by 2-2.5 percentage points. In addition, we find this increase to be largely driven by males, younger individuals, and those from higher socioeconomic classes. We offer

one's preferences are unclear. Mandated choice helps to resolve this issue.

[6]This policy originally had some loopholes, which have since been closed (see Kessler and Roth (2014b)).

[7]https://www.organdonor.gov/statistics-stories/statistics.html

[8]These registrations only affect one's status as a deceased donor; registering to become a living donor is a separate and independent process.

some possible explanations for these results in Section 1.6.

The rest of the paper is organized in the following manner. Section 1.2 reviews some of the relevant literature on framing and its uses in organ donation. Section 1.3 details the policy changes that took place in California. Section 1.4 describes the data. Section 1.5 discusses the estimation strategy and presents the results. Section 1.6 discusses how our findings fit in with the charitable giving literature, the policy implications, and possible areas of future research. Section 1.7 concludes.

1.2 Organ Donation and Choice Architecture

Choice architecture and framing has been one of the cornerstones of behavioral economics and psychology. There has been a growing trend of late in using insights from behavioral economics and psychology to inform public policy. For example, it is well-established that defaults can have an enormous impact on people's choice behavior. One of the seminal uses of default choices was in the context of retirement savings (see Thaler and Benartzi (2004), Benartzi and Thaler (2007), and Beshears et al. (2009)), where individuals were enrolled in retirement savings plans by default. Despite the fact that this default enrollment did not change the enrollment choices available to them, enrollment rates skyrocketed. The main attraction of these insights from behavioral economics and psychology is that they typically involve relatively small and inexpensive interventions, but can yield significant welfare-improving outcomes.

How can defaults be used when individuals are asked to register as organ donors? Defaults can take two possible forms in the context of organ donations: opt-out and opt-in. Under opt-out (also known as presumed consent), individuals are registered as donors by default, whereas opt-in policies require individuals to register if they wish to be an organ donor. One would expect opt-out policies to have higher donor registration rates than opt-in policies; Johnson and Goldstein (2003), with a laboratory experiment, argue that opt-out policies can significantly increase organ donor registration rates relative to opt-in policies. However, in many countries (such as the United States), opt-out policies are not allowed and enforcing a default where individuals are not registered as organ donors would lead to lower donor registration rates. In these cases, one option is to remove the default altogether and require individuals to answer whether or not they wish to register as organ donors. We call such a policy *mandated choice*.

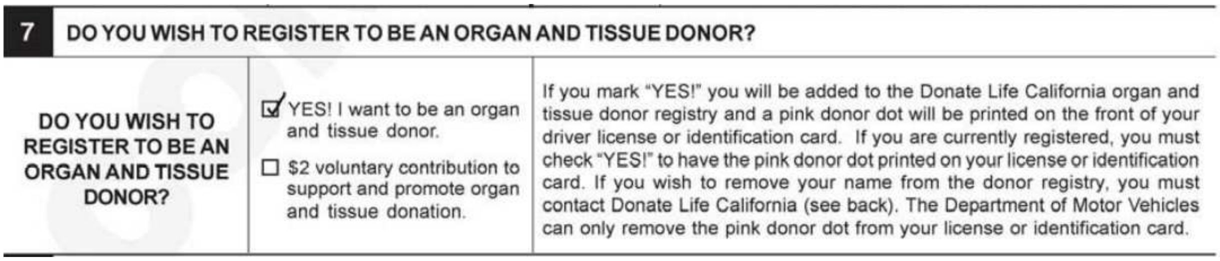
1.2.1 Mandated Choice

Mandated choice requires one to answer the question whether or not they wish to register to be an organ donor and removes a default option. There are several reasons why one would expect the implementation of mandated choice to increase donor registration rates. The first is the power of defaults. In countries with opt-in policies, the lack of mandated choice implies that by default, individuals will not be registered as organ donors. As highlighted earlier, defaults can have very powerful impacts on individuals' decisions; mandated choice removes this default. Therefore, we would expect the mandated choice to increase donor registration rates. The second reason is omission bias. Omission bias dictates that people are more likely to judge harmful actions as worse than equally harmful acts of omission, because actions tend to make the decision and consequences more salient. Psychologists have explored the consequences of omission bias in detail (see Ritov and Baron (1990), Spranca et al. (1991), and Baron and Ritov (1994)); economists have also begun to examine the effects of omission bias (Hallsworth et al. (2015)). In the context of organ donation and mandated choice, one can argue that if declining to register as an organ donor yields disutility, then choosing to answer "no" to the organ donation question is more costly than simply omitting to answer it. Thus, the theory of omission bias predicts that enforcing mandated choice should yield higher donation rates. The last reason is that mandated choice increases the cost of declining to be an organ donor. While the introduction of mandated choice is unlikely to sway the decision of individuals with strong priors, it can affect the choices of those on the margin. In particular, those who, in the absence of mandated choice, were on the margin and chose not to register as a donor may be willing to do so when the cost of saying no increases. Besides the possibility of increasing donor registration rates, policy makers and administrators have called for the use of mandated choice for decades (see Herz (1999), Spellman (2005), Spital (1992), Spital (1995), and Spital (1996)) for ethical reasons. Mandated choice removes the ambiguity surrounding one's intention to be an organ donor; this removes the decision-making burden from the next-of-kin.

Formal research on the effect of framing on organ donation registration rates has been limited. Most empirical work has only measured correlations and studies that claim causality have been limited to lab experiments. For example, Johnson and Goldstein (2003) and Johnson and Goldstein (2004) use an online experiment to show that opt-out frames yield higher donor registration rates than opt-in and neutral (no default) frames. Their experiments involve hypothetical choices in an artificial environment, limiting the external validity of the results.[9] Johnson and Goldstein (2003) and Johnson and Goldstein (2004) also show that countries with opt-out policies have significantly higher donor registration rates than those with opt-in policies. However, without any exogeneous changes in policies, these differences in average donor registration rates are only correlations and have no causal interpretation. Countries with opt-in policies may be systematically different than those with opt-out policies in ways that are correlated with their residents' propensity to register as an organ donor (cultural norms, population health, etc.), confounding the measured difference in mean donation rates.

The ideal experiment to examine the effect of mandated choice is to have a treatment group where individuals have yes/no options and are required to choose one, and a control group where individuals have yes/no options, but individuals are not required to choose one of them. For the control group, failing to answer the question results in the individual not being registered as an organ donor (i.e. there is an implicit "no" default), given that presumed consent is not

[9] Even experiments that involve real organ donor decisions, such as Kessler and Roth (2014a), have limited external validity since the process of participating in a laboratory experiment for the purposes of answering questions on organ donation is vastly different from how most people register as an organ donor (during a driver's license application or renewal, where registering as an organ donor is typically not the main priority.)



7	DO YOU WISH TO REGISTER TO BE AN ORGAN AND TISSUE DONOR?	
DO YOU WISH TO REGISTER TO BE AN ORGAN AND TISSUE DONOR?	☑ YES! I want to be an organ and tissue donor. ☐ $2 voluntary contribution to support and promote organ and tissue donation.	If you mark "YES!" you will be added to the Donate Life California organ and tissue donor registry and a pink donor dot will be printed on the front of your driver license or identification card. If you are currently registered, you must check "YES!" to have the pink donor dot printed on your license or identification card. If you wish to remove your name from the donor registry, you must contact Donate Life California (see back). The Department of Motor Vehicles can only remove the pink donor dot from your license or identification card.

Figure 1.2: The organ donation question on DMV applications prior to July 2011 (opt-in frame).

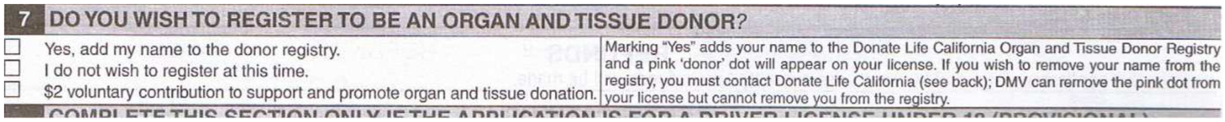
an option in the United States. However, such experiments in the field are generally not feasible, as changes in the framing on DMV applications typically requires legislative action at the state level. Our approach bridges the gap between previous empirical and laboratory studies by exploiting a natural experiment that we use to measure the effect of mandated choice of donor registration rates using actual DMV data to establish causality. To the best of our knowledge, our study is the first of its kind to use field data, providing results that are much more externally valid than prior studies.

1.3 California's Policy Change

The State of California recently turned to the effects of framing in order to boost donor registration rates. In July 2011, the California state legislature passed legislation that changed the language used regarding organ donation on drivers license applications and renewal forms.[10] The framing of the question was changed from an opt-in to an active choice framework (as coined by Kessler and Roth (2014a)). Prior to July 1, 2011, those who wished to donate their organs had to opt-in to the donor registry.

Failing to answer the question meant one would not be registered as an organ donor. Since July 1, 2011, the drivers license application and renewal forms have had two separate answers: yes and no. In addition, drivers were now required to answer the question; the application

[10] http://leginfo.ca.gov/pub/09-10/bill/sen/sb_1351-1400/sb_1395_bill_20100902_chaptered.html



7 DO YOU WISH TO REGISTER TO BE AN ORGAN AND TISSUE DONOR?

- ☐ Yes, add my name to the donor registry.
- ☐ I do not wish to register at this time.
- ☐ \$2 voluntary contribution to support and promote organ and tissue donation.

Marking "Yes" adds your name to the Donate Life California Organ and Tissue Donor Registry and a pink 'donor' dot will appear on your license. If you wish to remove your name from the registry, you must contact Donate Life California (see back); DMV can remove the pink dot from your license but cannot remove you from the registry.

Figure 1.3: The organ donation question on DMV applications after July 2011 (active choice frame).

would not be processed without an answer.

1.3.1 Issues with Policy Implementation

Drivers license applications can be submitted through three different channels: the field office, online, and via email.[11] However, not all three methods of application were exposed to the same framing after July 2011. First, a default answer of "no" was inadvertently selected for online renewals.[12] Second, the mail-in renewals never required drivers to answer the question on organ donation. Those that left it blank were recorded as not wanting to donate their organs. As a result, the requirement to have a mandated choice was never enforced for mail-in applications. Applications and renewals completed in a field office were the only instances of the policy changes being properly enforced. Table 1.1 summarizes the policy changes by application type.

1.3.2 Natural Experiment

The policy changes detailed above form a natural experiment that allows us to directly examine framing effects on organ donation registration rates. In particular, we focus on the removal of the default option for online renewals. Prior to July 16, 2015, due to the presence of a "no" default, those who renewed their driver's license online were not required to answer the organ donation question; those who skipped the question were automatically considered to have answered "no." After July 16, 2015, those who renewed their driver's license online were

[11] First-time applications can only be done in a field office. Renewal applications can be done in a field office, online, or by mail (although all three options may not be available to all drivers looking to renew).

[12] This default was corrected on July 16, 2015.

Table 1.1: Summary of Policy Changes

	Before July 1, 2011	July 1, 2011 - July 16, 2015	July 16, 2015-present
Field Office	opt-in no mandated choice no default	active choice mandated choice no default	active choice mandated choice no default
Online	opt-in no mandated choice no default	active choice mandated choice default	active choice mandated choice no default
Mail	opt-in no mandated choice no default	active choice no mandated choice no default	active choice no mandated choice no default

required to answer the organ donation question; the renewal would not be processed without an answer. The removal of this default option is equivalent to the implementation of the mandated choice requirement.[13] Comparing donor registration rates immediately before and after the default removal will allow us to measure the causal effect of the mandated choice requirement on donor registration rates. Our estimation strategy is discussed in more detail in Section 1.5.

1.4 Data

The California data used in this analysis is a repeated cross-section[14] that are provided by Donate Life California.[15] The data contain individual responses to the organ donation question (as seen in Figures 1.2 and 1.3).

The data are daily, range from 2012 to 2015, and also include information regarding age, gender, zip code, and field office information (if the application/renewal took place at a field office). Our dataset contains an average of 5,716,207 individuals per year. The overwhelming

[13]The implementation of mandated choice through the default removal is slightly different than the ideal experiment described in Section 1.2.1. In the ideal experiment, there is an implicit default where an individual will not be registered as an organ donor if they do not answer the question. In our natural experiment, the "no" default is explicitly shown.

[14]California state laws require driver's licenses to be renewed every five years. For the purposes of our analysis, this implies that there are no individuals that appear on multiple occasions in the data.

[15]Special thanks go to Charlene Zetter and Brad Makaiau for their invaluable support.

majority (65.5%) of applicants apply for or renew their driver's license at a field office. 19.5% of applicants do so through mail and the remaining 15.02% renew their driver's license online. Approximately 28.21% of applicants in our data have chosen to register as organ donors; 50.47% are male and the average age is 44.7 years. Figure 1.4 shows the registration rate at the quarterly level by application method. In addition, females register as organ donors at higher rates than males. Figure 1.5 shows the registration rates by gender and application method. Additional summary statistics can be found in Table A.1.

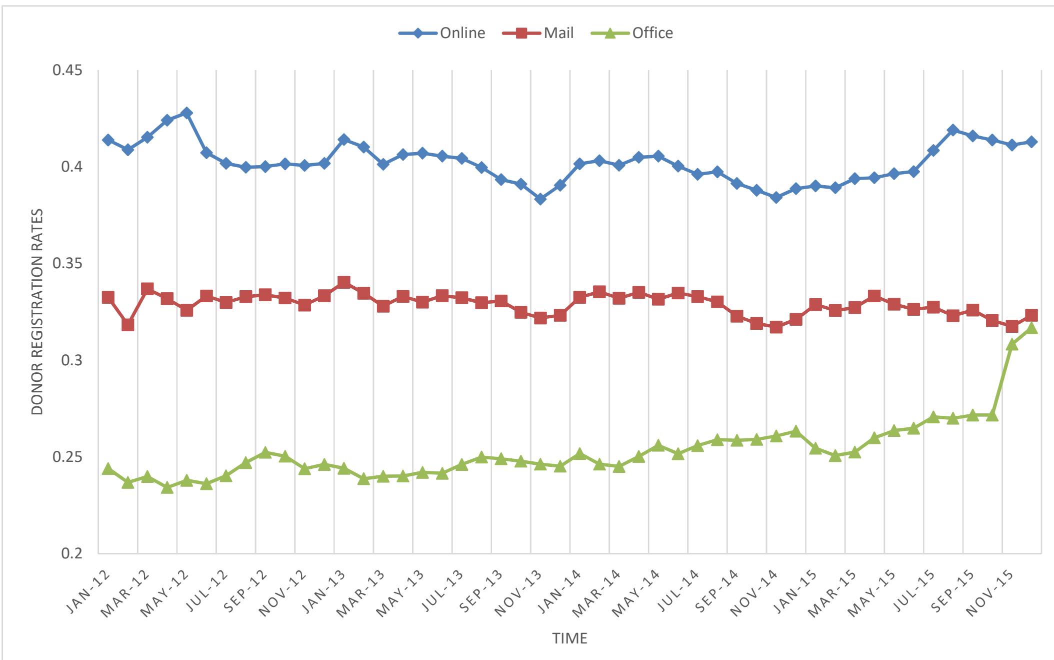


Figure 1.4: Donor Registration Rates by Application Methods.

1.5 Estimation Strategy and Results

The California DMV removed the default of "no" on online renewals on July 16, 2015, which enforced the mandated choice requirement of the active-choice frame. This mandated choice enforcement was implemented as a server-side change and was not previously announced;

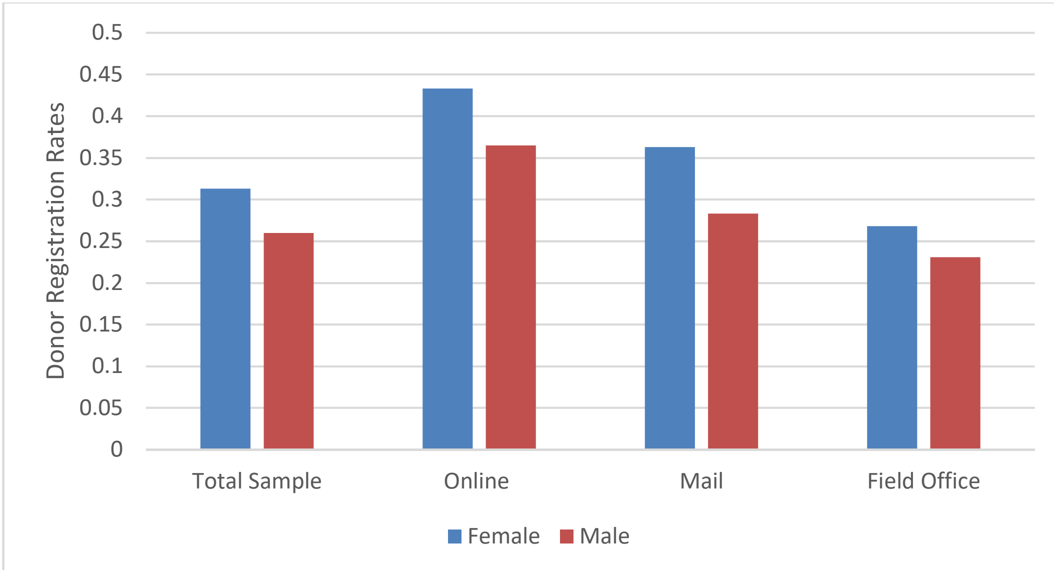


Figure 1.5: Donor Registrations by Gender and Application Type.

we consider it to be exogenous. Therefore, the treatment of mandated choice can be viewed as a local randomized experiment, where individuals who renew their driver license online just after the default removal are considered to be the treated group and individuals who renew their driver license online just before the removal are considered to be the control group. We only use individuals who renewed online to measure the effect of mandated choice, so our estimation does not suffer from a sample selection problem. We use a regression discontinuity (RD) framework, with time as the forcing variable, to capture the causal effect of the default removal on donor registration rates. The p-th order regression discontinuity specification we estimate is of the following form:

$$y_i = \alpha + \tau D_i + \sum_{j=1}^{p} \beta_j \widetilde{T}_i^{\,j} + \sum_{k=1}^{p} \gamma_k \widetilde{T}_i^{\,k} D_i + X_i' \theta + \varepsilon_i \tag{1.1}$$

where $y_i \equiv \mathbb{1}\{\text{individual } i \text{ registers as an organ donor}\}$,
$D_i \equiv \mathbb{1}\{\text{individual } i \text{ renews on or after July 16, 2015}\}$, X_i is a vector of covariates, and $\widetilde{T}_i$ is time (in days) centered around July 16, 2015.

This framework allows us to incorporate different slopes on each side of the discontinuity.

The parameter of interest is τ, which measures the discontinuity in the probability of registering when an individual renews immediately prior to the default removal to immediately after the default removal.

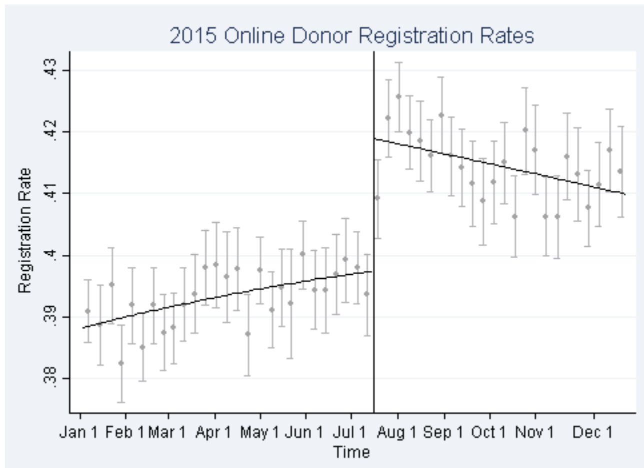


Figure 1.6: Online Registration Rates for 2015

Binned online registration rates for 2015 plotted by week. Error bars represent 95% confidence intervals.

Figure 1.6 shows the weekly donor registration rates for online renewals in 2015. The vertical cutoff line represents the the date of July 16, 2015, when the default was removed. It is clear from Figure 1.6 that there is a discontinuity in the donor registration rates at the time of the policy change, resulting in a sizable increase in registration rates.[16]

Following the recommendations of Lee and Lemieux (2010), we estimate several different specifications of the RD with different polynomial orders and control variables. To prevent overfitting the data, we restrict the estimation of Equation 1.1 to linear and quadratic specifications (see Gelman and Imbens (2014)). One robustness check, as advocated by Lee and Lemieux (2010), is to observe the impact of control variables on the estimated treatment effect. Treatment assignment is only a function of the running variable; as such, the inclusion of additional covariates should not significantly affect estimated treatment effects. Additionally, we consider

[16]The different slopes on either side of the cutoff is most likely due to the seasonality in online donor registration rates (see Figure 1.4). This is further discussed in Section 1.6.

the possibility that the implementation of mandated choice being confounded by the day of the week. For instance, if we believe that people tend to be more generous during the end of the week than the beginning of the week, then it is plausible that these systemic differences are confounding our estimates. We include day-of-week dummies to address this possible confound.

Table 1.2: Parametric (OLS) Regression Discontinuity (Full Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Mandated Choice	0.021***	0.022***	0.022***	0.023***	0.021***	0.021***
	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)
Age		-0.001***	-0.001***		-0.001***	-0.001***
		(0.000)	(0.000)		(0.000)	(0.000)
Female		0.066***	0.066***		0.066***	0.066***
		(0.001)	(0.001)		(0.001)	(0.001)
Constant	0.398***	0.418***	0.403***	0.397***	0.420***	0.405***
	(0.001)	(0.002)	(0.041)	(0.002)	(0.002)	(0.041)
Observations	1142960	1142960	1142960	1142960	1142960	1142960
Adjusted R^2	0.001	0.052	0.052	0.001	0.052	0.052
Additional Controls	No	Yes	Yes	No	Yes	Yes
Polynomial Order	1	1	1	2	2	2
DOW Dummies	No	No	Yes	No	No	Yes

Standard errors in parentheses
Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.
Additional controls include controls for age, gender, and zip code.
DOW dummies control for the day-of-week.
Standard errors are clustered at the daily level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

We also present the estimates of a fully-flexible version of Equation 1.1 in Tables 1.3 (linear specification) and 1.4 (quadratic specification) for various bandwidths around the July 16, 2015 threshold. These specifications address potential confounds by allowing for covariates to have heterogeneous effects on the outcome variable for each side of the treatment cutoff. For all bandwidths, the quadratic specification generally estimates weakly smaller treatment effects compared to those of the linear specification, while remaining statistically significant. Therefore,

Table 1.3: Fully-Flexible Regression Discontinuity (Linear)

	(1)	(2)	(3)	(4)	(5)
Mandated Choice	0.028***	0.024***	0.027***	0.029***	0.031***
	(0.009)	(0.008)	(0.006)	(0.005)	(0.004)
Age	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	0.071***	0.070***	0.070***	0.070***	0.069***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)
Age Interaction	-0.000**	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female Interaction	-0.005	-0.006*	-0.006**	-0.005**	-0.006***
	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)
Constant	0.008	0.540***	0.484***	0.476***	0.383***
	(0.067)	(0.084)	(0.084)	(0.067)	(0.045)
Observations	211053	388657	576755	750479	1142960
Adjusted R^2	0.054	0.052	0.052	0.052	0.052
Sample	60 days	120 days	180 days	240 days	Full Sample

Standard errors in parentheses
Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.
The sample indicates the number of days restricted for the analysis
For example, 60 days restricts observations to those that occur 30 days before and after July 16, 2015.
Standard errors are clustered at the daily level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4: Fully-Flexible Regression Discontinuity (Quadratic)

	(1)	(2)	(3)	(4)	(5)
Mandated Choice	0.022**	0.020**	0.025***	0.025***	0.031***
	(0.010)	(0.009)	(0.007)	(0.006)	(0.005)
Age	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	0.071***	0.070***	0.070***	0.070***	0.069***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)
Age Interaction	-0.000**	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female Interaction	-0.005	-0.006*	-0.006**	-0.005**	-0.006***
	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)
Constant	0.003	0.539***	0.482***	0.478***	0.385***
	(0.067)	(0.084)	(0.083)	(0.067)	(0.046)
Observations	211053	388657	576755	750479	1142960
Adjusted R^2	0.054	0.052	0.052	0.052	0.052
Sample	60 days	120 days	180 days	240 days	Full Sample

Standard errors in parentheses
Dependent variable is a dummy variable indicating whether or not the
individual registered as an organ donor.
The sample indicates the number of days restricted for the analysis
For example, 60 days restricts observations to those that occur 30 days
before and after July 16, 2015.
Standard errors are clustered at the daily level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

our preferred specification for the remainder of the paper is the quadratic, fully-flexible RD. Although we present results for a wide range of bandwidths, we focus on the results from 60-day and 120-day bandwidths in order to reduce the likelihood of unobservable, time-varying events confounding the estimated treatment effect.[17]

Our main results are presented in columns 1 and 2 in Tables 1.2 and 1.4. They show that the implementation of mandated choice increased donor registration rates by about 2%. Next, we show that the identification assumptions behind our regression discontinuity specifications hold and measure heterogeneous treatment effects.

1.5.1 Identification Tests and Robustness Checks

In order to rule out the presence of an unknown or unobserved seasonal event that may be confounding the removal of the default, we perform a placebo test by estimating similar regression discontinuity models from previous years, assuming the existence of a treatment on July 16 in each of the previous years. If there exist unobserved events that would lead to an increase in donation rates in July of every year, then these regression discontinuities estimations should capture such factors. Figures A.1 and A.2 show the average daily registration rates in 2014 and 2013, respectively [18]. The vertical lines in each of these figures represent July 16 of the corresponding year. The falsification test shows that the discontinuity observed in the 2015 data does not exist in any of the previous years. This evidence suggests that the increase in the donor registration rate after July 16, 2015 is due to the implementation of mandated choice; Table A.2 confirms this empirically.

In addition, we show the results of several tests, as advocated by Lee and Lemieux

[17]As a robustness check to account for the seasonality in online donor registration rates, we estimate fully-flexible quadratic regression discontinuity specifications use all 2012-2015 online data with monthly fixed-effects; the results are robust to these specifications.

[18]We omit 2012 from the placebo test, as there appears to be a discontinuous decrease in donor registrations rates in the beginning of 2012 for an unknown reason. Despite that anomaly, there appear to be no significant jumps in the donation rates around July 16, 2012.

(2010), that show the identification assumptions behind the RD are satisfied. The first test is to show that the inclusion of control variables have no significant impact on the parameter of interest, since the treatment variable is solely determined by the forcing variable. Table 1.2 shows that controlling for age, gender, zip codes, and day-of-week fixed-effects do not have a noticeable impact on our estimates.

The biggest threat to the validity of a regression discontinuity design, as with any causal exercise, is the lack of balance between treatment and control groups. This lack of balance can manifest in two forms. The first is individuals' ability to perfectly manipulate their status around the threshold; manipulation around the threshold suggests that the pre-treatment sample is not an appropriate control group. Perfect manipulation is not a concern in our context, given the policy change was implemented without prior notice. The second is jumps in covariates and pre-determined characteristics at the threshold. If there are discontinuous jumps in pre-determined characteristics (such as age or gender) at the threshold that are correlated with one's propensity to register as a donor, then the pre-treatment sample may not serve as an appropriate control group. To show that there are no discontinuities around the threshold in individuals' pre-determined characteristics, we estimate an RD with age and gender as the dependent variable. Figures A.3 and A.4 show that there exist no noticeable discontinuities in the proportion of females applying for renewals and age of individuals, respectively, at the time of the implementation of mandated choice.[19] Table A.3 confirms this empirically.

We also re-examine what effect day-of-week had on the change in donor registration rates. To complement the inclusion of day-of-week dummies in earlier regressions, we repeat the RD analysis, but restrict the data to only Thursdays [20]. Figure A.5 plots the donor registration rates by time for only Thursdays. We see that there still exists a noticeable jump in the donor registration rate at the threshold. Table A.4 shows the estimates from Equation 1.1 for the

[19] In the state of California, individuals' driver's licenses typically expire on their birthday (https://www.dmv.org/ca-california/renew-license.php). This explains the downward trend observed in the age plot.

[20] July 16, 2015 was a Thursday.

Thursday subsample. The treatment effect is robust to this specification. This evidence allows us to be confident that the RD specifications presented earlier are not confounded by unobserved, time-varying factors that may have been correlated with one's propensity to give.

1.5.2 Heterogeneous Treatment Effects

In this section, we look for heterogeneous treatment effects.[21] From a practical standpoint, uncovering heterogeneous treatment effects can allow policy-makers to carefully focus their effort to increase donor registration rates. More importantly, heterogeneous treatment effects can also help us to understand the underlying mechanisms driving the increases in donor registration rates. First, we look at heterogeneity by gender and age, two of the most important determinants of charitable giving and altruism. If we believe the estimates presented in Section 1.5 to be causal due to changes in the price of giving, then we should expect to see heterogeneous treatment effects by gender.

Prior research has documented the differences in tastes for charitable giving between genders. In particular, Andreoni and Vesterlund (2001) document that males tend to be more price-elastic than females on average. If mandated choice affects donor registration rates through increasing the cost of declining to register as an organ donor, then we should expect to see larger treatment effects for males than for females. Figure A.6 shows the RD plots for the female and male subsamples, respectively. While there are discontinuous jumps in the donor registration rates for both males and females, the jump for males appears to be larger. Table A.5 presents the estimates for fully-flexible specifications for males and females for a variety of bandwidths around the cutoff. The results show that increases in donor registration rates due to mandated choice is driven disproportionately by males.

We repeat a similar analysis to look for heterogeneous treatment effects by age. We split

[21] In regression discontinuity frameworks, heterogeneous treatment effects are estimated with split-sample estimations, rather than including interactions in the main model (see Hsu et al. (2016)).

the sample into two groups based on the median age (41.29 years of age). Figure A.7 show the RD plots for individuals below and above the median. The results of the fully-flexible RD are presented in Table A.6. Here, we find the treatment effects are driven almost entirely by individuals younger than the median age.

Lastly, we look for heterogeneous treatment effects by various socioeconomic factors that are well-known to be determinants of charitable giving, such as income (Andreoni and Scholz (1998), Gittell and Tebaldi (2006)), education (Brown and Ferris (2007), minority status (Rooney et al. (2005)), and household size. In order to do this, we supplement our data with US Census data at the zip-code level for median household income, median household size, educational attainment, and minority representation. For each determinant, we split the sample at the median and estimate split-sample regressions. Figures A.8, A.9, A.10, and A.11 show the RD plots for income, minority representation, education, and household size, respectively. Tables A.7, A.8, A.9, and A.10 show the corresponding results. A noticeable pattern emerges from the analysis; individuals living in lower socioeconomic zip codes (lower incomes, smaller households, and larger minority representation) have smaller treatment effects than those living in higher socioeconomic zip codes.

1.6 Discussion

Broadly, our results support the idea that potential organ donors are sensitive to the price of giving. Our results are consistent with many of the previously established stylized facts regarding charitable giving and also contribute new findings to the existing literature. Our main result, that mandated choice has a significant impact on donor registration rates, reinforces the power of the ask, as outlined by Andreoni et al. (2011) and Andreoni and Rao (2011). Our results also reinforce a main point by Andreoni and Vesterlund (2001), namely that on average, males are more price-elastic than females. The implementation of mandated choice increases the cost of declining to become an organ donor. Therefore, the relative cost to register as an organ donor decreases. Our results from Table A.5 show that the increase in donor registration rates in response to the implementation of mandated choice is driven disproportionately by males.

As discussed previously, mandated choice is more likely to affect those on the margin. Given that younger individuals have been asked fewer times to register as organ donors, it is plausible that younger individuals are more likely to be on the margin, and therefore, are more likely to be swayed by the implementation of mandated choice. Our results from Table A.6 provide evidence that younger individuals are indeed more likely to be on the margin. This mechanism is further reinforced when examining the connection between organ donation and socioeconomic status. We find that individuals living in zip-codes that, on average, are of a lower socioeconomic class tend to have smaller estimated treatment effects than those living in higher socioeconomic zip-codes. To help understand why this is the case, it is useful to first look at baseline donor registration rates across socioeconomic classes; Figure 1.7 shows organ donation rates by socioeconomic class quartiles.

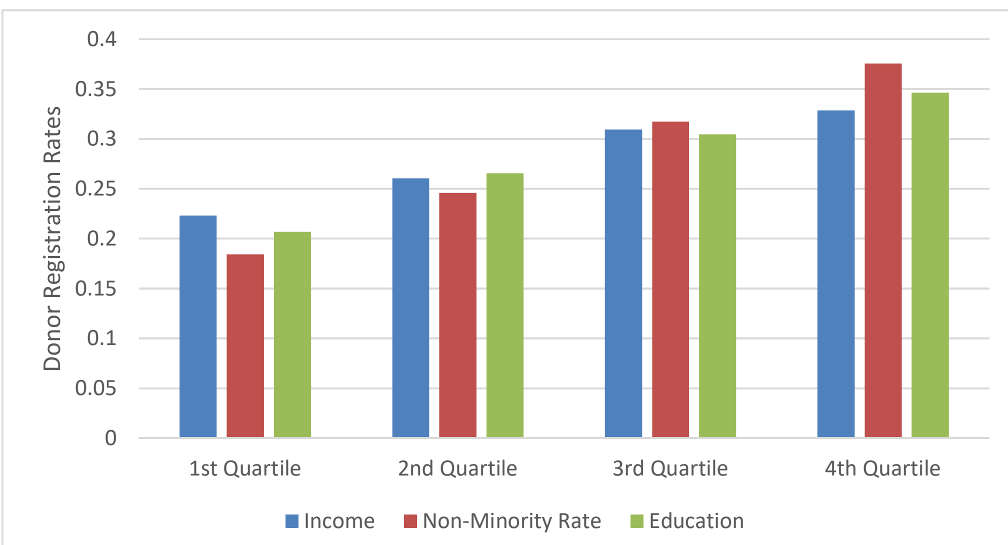


Figure 1.7: Organ Donation Rates by Socioeconomic Class Quartiles.

Doing so reveals another clear pattern: individuals in lower socioeconomic regions are less likely to register as organ donors. This is in line with previous medical and social psychology literature (Goldberg et al. (2013) and René et al. (1994)). Reasons for lower donor registration rates include lack of knowledge regarding organ transplantation (McNamara et al. (1999) and Spigner et al. (2002)), religious/cultural norms Callender and Miles (2001), and a distrust of the medical system (Siminoff and Arnold (1999)). Cultural norms and a distrust of the medical system may create strong priors on preferences towards organ donation. Given the strong correlations between minority status and socioeconomic classes, it is not surprising that individuals from lower socioeconomic classes have smaller treatment effects than those from higher socioeconomic classes.

1.6.1 Limitations

As outlined in Section 1.2, we believe our approach has stronger external validity than previous studies. Nonetheless, the nature of the regression discontinuity yields some limitations to our analysis. First, the regression discontinuity only identifies a treatment effect at the threshold for the sample being analyzed. In our application, this implies that the causal effects are only internally valid for measuring how the implementation of mandated choice through a default removal affects those who renew online in July 2015. In California, one's driver's license typically expires on their birthday, so we should expect the treated sample to comprise mostly of individuals born in July and August (out of the 2,961 individuals that renewed their driver's license online on July 16, 2015, 2,596 were born in either July or August). Previous evidence suggests that there is seasonality in conception and birth: children born in the winter are disproportionately born to women of lower socioeconomic status (see Buckles and Hungerman (2013)). Therefore, we may expect the effect of mandated choice on donor registration rates to be larger during the summer months than the winter months. In addition, if we believe that individuals that choose to process their driver's license applications in a field office or through the mail are systematically different than those that choose to do so online, and that these differences are correlated with their propensity to register as an organ donor, then it is not clear how mandated choice would impact the donor registration rates for these subgroups.[22] [23] For example, older people tend to renew by mail [24] and younger people tend to use the online application or go to a field office[25]. Moreover, those who renew by mail are more likely to be female, older, have lower incomes, and be less educated than those who renew online. As a result, we might expect mandated choice to have a smaller treatment effect for those that renew by mail. Similarly, given that first-time applicants must go to the field office and are typically younger, we might expect mandated choice to have a larger treatment effect for office renewals (although individuals who go to field offices typically come from zip-codes with higher minority representation and smaller median income levels, which may decrease the expected treatment effects.).

The regression discontinuity specification also does not allow us to examine long-term effects of the default removal.[26] Should we expect the observed increase in donor registration rates to be persistent over time? How does the affect of mandated choice on donor registration rates change by time of year? How are future answers to the organ donation question affected by mandated choice? These are questions that our study cannot answer.

1.6.2 Policy Implications

The policy ramifications of the increase in registration rates are difficult to estimate due to the reasons outlined in Section 1.6.1 and factors that determine whether or not a registered donor can actually provide organs for transplant. Nevertheless, we attempt to provide a simple "back-of-the-envelope" calculation to quantify the policy impact. While every donor has the

[22]We are generally comfortable with extrapolating the direction of the treatment effect, but not the magnitude.

[23]The requirements for being able to renew by Internet can be found at https://www.dmv.ca.gov/portal/dmv/detail/online/dlrbi/dlfaqsmain.

[24]The average for mail, online, and field office applications are 51.2, 43.7, and 42.3 years of age, respectively.

[25]First-time applications must be done at a field office. Subsequent renewals can typically be done either online, by mail, or at a field office.

[26]While a difference-in-difference specification would have been our preferred specification, the field office and mail subsamples did not allow us to satisfy the parallel trends assumption.

potential to provide eight organs for transplantation, it is very rare for people to die in a manner that allows them to do so. According to Donate Life California and the US Department of Health and Human Services, only one percent of deaths qualify for an organ donation [27] and each donor provides three to four organs per transplant on average [28]. Our data show there are, on average, 1,072,500 individuals who renew their driver's license online each year. A 2% increase in the donor registration rate leads to an average of 21,450 additional donors per year, which translates to roughly 215 qualified donors per year. Assuming that people do not change their decisions in the future to be organ donors, this leads to an average of 640-865 additional organs being donated every year.[29] We consider this to be a very large impact given how seemingly minor the enforcement of mandated choice is. Moreover, this is a conservative estimate, given that online applications are the least popular of all three application methods, but are seeing a steady increase year-after-year.[30]

1.7 Conclusion

Our study is the first of its kind to measure the causal impact of framing changes on organ donor registration rates using actual donor data and a natural experiment. We take advantage of an incorrectly implemented policy to change the organ donation question on driver's license applications from a passive-choice frame to an active-choice frame where the "no" option for online renewals was inadvertently selected as a default option. This was subsequently corrected on July 16, 2015, which serves as an exogenous implementation of mandated choice. To measure the causal impact of mandated choice on donor registration rates, we compare registration rates immediately before and after the policy shift using a regression discontinuity design. We show

[27] http://donatelifecalifornia.org/10-million-californians-say-yes-to-organ-donation/

[28] https://srtr.transplant.hrsa.gov/annual_reports/2010/pdf/07_decd_donation_11.pdf

[29] This does not include the wide range of bone and connective tissue that donors can also provide upon death.

[30] There were 641,071 online renewals in 2012, 917,523 online renewals in 2013, 1,026,298 online renewals in 2014, and 1,150,165 online renewals in 2015.

that mandated choice increased registration rates by 2-2.5%. This reinforces the findings of the power of the ask (Andreoni et al. (2011) and Andreoni and Rao (2011)). Moreover, we find the increase in donor registration rates to be disproportionately driven by males, younger individuals, and those from higher socioeconomic classes. This heterogeneity provides several key insights on the underlying mechanisms by which mandated choice affects donor registration rates. The first is that mandated choice increases donor registration rates by decreasing the relative cost of registering to be a donor. The second is that this change in cost will only affect those on the margin; those who already have strong beliefs about organ donation are unlikely to be swayed in their decision. This is important to understand when considering the external validity of mandated choice. Our results make a broader point that potential organ donors are sensitive to the price of giving and reducing barriers to giving may be an important lever for policy-makers to leverage in order to increase donor registration rates. Further research involving carefully planned interventions should be conducted to better understand how framing and the price of giving affects propensities to register as an organ donor.

Chapter 1, in part is currently being prepared for submission for publication of the material. Basavanhally, Naveen; Krishnan, Viswanathan. The dissertation author was the primary investigator and author of this material.

Chapter 2

Bargaining and Charitable Contributions by Married Couples

2.1 Introduction

Gender differences in economic settings have been well-documented. For example, men are found to be more competitive, (Gneezy et al. (2003), Gneezy et al. (2009), and Niederle and Vesterlund (2010)), deceptive (Dreber and Johannesson (2008)), and risk-prone (Croson and Gneezy (2009), Holt and Laury (2002), Fehr-Duda et al. (2006), and Charness and Gneezy (2012)) than women. In the context of charitable giving, the evidence of gender differences is substantial, albeit somewhat fractured. Eckel and Grossman (2008) show that when risk is abstracted, women tend to be more generous than men. Andreoni and Vesterlund (2001) offers a more nuanced perspective. They show that men tend to be more price-elastic than women. Also, when altruism is expensive, women are kinder, and the opposite is true when altruism is cheaper.

How do these differences manifest in households with married couples that make charitable decisions? First, it is important to understand how households make such decisions. Does one individual typically make decisions for the entire household, or do various family members collectively come to an agreement? Or perhaps family members simply make decisions independently? Samuelson (1956) and Becker (1981) argued for a unitary household model, where household decisions are made by a benevolent head. In this model, households maximize the sum of the spouses' utilities subject to a pooled budget constraint. It can easily be shown that the household's demand functions only depend on the total income rather than the individual sources of income. This leads to the central testable prediction of the unitary household model: income pooling. Income pooling implies that changes in the distribution of the household's income, while keeping total income fixed, should have no effect on the household's demand. There have been multiple studies showing that household behavior is not consistent with income pooling (see Lundberg et al. (1997) and Attanasio and Lechene (2002) for two such examples). Non-unitary models of household bargaining have been developed in response to the evidence against unitary household models. Non-unitary models can broadly fall into one of two categories: cooperative and non-cooperative bargaining. These models have the common trait that each member of the household has their own preferences, but differ in the solution concepts used to characterize the equilibria (see Donni and Chiappori (2011) for an in-depth survey). Given that spouses may have different tastes for charitable giving and also bargain to make household decisions, the natural question to ask is how these two (possibly contradictory) forces interact. We do not expect couples to marry at random; charitable preferences may be one of the many factors that endogenously select partners into marriage. If this is the case, will married couples who choose to make decisions jointly donate more than the total contribution spouses would have made had they not been making decisions jointly? Alternatively, if spouses do not have the same preferences for charitable giving, do the costs of bargaining depress total giving by the household? For example, if one spouse cares deeply about cultural organizations and the other spouse prefers to support international affairs, then the cost of bargaining may make it prohibitively expensive for the household to make charitable contributions at the margin. Instead, the household would have a higher level of charitable contributions if either the husband or wife were the sole decision-maker, or if the spouses made decisions separately.

How should we think about measuring the difference in giving associated with bargaining? In order to answer this question, we must carefully think about the counterfactual. That is, how should we expect households to behave if they are not able to come to an agreement? In the context of bargaining theory, we are interested in the disagreement point, whose outcome is realized in the event negotiations break down. One possibility is to assume that in the absence of bargaining, one of the spouses will have sole discretion over the household's giving decisions. Andreoni et al. (2003) and Yörük (2010) look at households where either the husband or wife has sole discretion over the household's giving and compares them with households where decisions over household giving are made jointly between spouses. Andreoni et al. (2003) find that bargaining is associated with a 4% reduction in giving, whereas Yörük (2010) find that bargaining is associated with a 6%-7% increase in giving. However, assuming either the husband or wife may have sole discretion over giving may not be the most appropriate counterfactual to consider when couples are unable to come to an agreement. In fact, it could be argued that ceding decision-making authority to one spouse is a particular type of bargaining outcome rather than the realization of a disagreement point.

In standard cooperative and non-cooperative models of household bargaining, the threat point is typically divorce. However, divorce may not be a realistic outcome when small disagreements (such as disagreements over how charitable contributions should be spent) arise. Instead, family members may simply choose to make separate decisions rather than bargain. An attractive alternative to the divorce threat-point bargaining model is the separate-spheres model proposed by Lundberg and Pollak (1993). As the name suggests, in the separate-spheres model, spouses who fail to reach a bargaining solution will instead make decisions separately and independently rather than choosing to divorce; the threat point is characterized by a noncooperative Nash equilibrium. The separate-spheres model not only provides a more natural explanation to how households may behave if they fail to reach an agreement, but can also provide insight on how household bargaining can affect overall giving. The effect of bargaining on overall giving crucially depends on spouses' preferences towards giving and the presence of bargaining frictions. For example, if spouses' preferences for giving align, then charitable contributions are a household public good. If the spouses fail to come to an agreement, they will make separate choices based on their individual financial resources. Their outcome will be the threat point (which is characterized by a Cournot Nash equilibrium), where giving is inefficient and under-allocated (with respect to the Pareto efficient allocation when there are no bargaining frictions). Therefore, the effect of bargaining on overall giving depends on how costly bargaining is. If bargaining is costless, then bargaining will increase total giving compared to the level of giving at the threat point. However, if bargaining is sufficiently costly, then bargaining will be depressed compared to the level of giving at the threat point. Here, we may expect spouses to reach an agreement that centralizes the decision in order for each spouse to internalize the other spouse's benefit of consuming the household public good, rather than the spouses making separate decisions or one spouse ceding decision-making authority to the other spouse. Similarly, suppose spouses' preferences for giving do not align. In this case, giving is simply a private good for each spouse. Assuming costly bargaining, bargaining will decrease overall giving compared to the level of giving at the threat point. In the extreme case, where each spouses' giving creates a negative externality for other spouse, bargaining will eliminate overall giving. Fortunately, the PSID allows me to identify couples that make decisions household decisions separately; these households can be used as a more natural counterfactual to those who make joint decisions to better understand the power of bargaining. Previous literature, such as Andreoni et al. (2003), use the 1992 and 1994 waves of the Current Population Survey (CPS), which does not report separate-deciding households; and although Yörük (2010) uses the 2003 wave of the PSID, he does not use separate-deciding households in his analysis.

In this paper, I replicate the methods of Andreoni et al. (2003) (where husband- and wife-deciding households are used as counterfactuals for joint-deciding households) using the

2003 and 2005 waves of PSID data, where I find strikingly different results than those found in Andreoni et al. (2003) and Yörük (2010). Andreoni et al. (2003) find bargaining to be associated with 4% lower levels of giving and husbands having much more bargaining power than wives. Yörük (2010) finds bargaining to be associated with 6% higher levels of giving (although some methodological concerns cloud the validity of this result, which I expand upon in Section 2.6). However, my results indicate that bargaining is associated with 40%-60% lower levels of giving. They also indicate that husband's and wives' bargaining powers are not significantly different from each other. I then use separately-deciding households to re-examine the effect of bargaining on household giving, and find similar results to my replication of Andreoni et al. (2003) and Yörük (2010), namely that household bargaining is associated with drastically smaller levels of giving. Lastly, I also examine households that switched between joint-deciding and separate-deciding regimes to see how giving changes within household when the decision-making regime changes. Once again, I find that bargaining is associated with lower levels of giving. One possible explanation for the narrowing of the gap between spouses' bargaining power is the relative gains made by women in the labor market over the past two decades. This, combined with imperfect alignment in preferences for charitable giving between spouses and the presence of bargaining frictions can help explain the reduced levels of giving associated with bargaining. The rest of the paper is as follows: Section 2.2 describes the PSID data, Section 2.3 seeks to provide empirical evidence of differences in propensities to give based on gender and the household's decision-making regime, Section 2.4 presents the empirical strategy and main results, Sections 2.5, 2.6, and 2.7 discuss the results and briefly concludes.

2.2 Data

I use the 2003 and 2005 waves of the Center of Philanthropy Panel Study (COPPS) (the philanthropy module of the PSID). These two waves yield an unbalanced panel with 6,526 households for a total of 10,476 observations. The key feature of the PSID is that it asks households which member is responsible for making charitable giving decisions. In addition, I use NBER's TAXSIM program (Feenberg and Coutts (1993)) to back out the marginal tax rate for each household. Combining this with the decision to itemize, I construct a measure for the price of donating one dollar to charity (1 for non-itemizers, $1-t$ for itemizers, where t is the household's marginal tax rate).[1] Another advantage of the PSID dataset is access to many variables that are considered to be determinants of giving, such as income (Andreoni and Scholz (1998), Gittell and Tebaldi (2006)), age (Feldstein and Clotfelter (1976), Andreoni and Scholz (1998), Gittell and Tebaldi (2006)), education (Brown and Ferris (2007), minority status (Rooney et al. (2005)), household size, and religiosity (Regnerus et al. (1998)). These measures are also determinants of each spouse's threat point.

2.2.1 Summary Statistics

Pooling the 2003 and 2005 waves of the COPPS data and removing observations with missing key variables yields a sample of 9,352 households; 68.16% of these households reported having donated to charity with an average donation of $1,481. Excluding married couples yields a subsample of 7,171 single households (2,910 male and 4,261 female). Among singles individuals, 47.78% of males and 55.77% of females are donors. Among single individuals who made charitable contributions, the average contribution is $1,270 for males and $1,202 for females. Married couples that have reported non-zero charitable contributions fall into four distinct categories, depending on who made the decisions regarding charitable contributions: households where the wife has sole discretion over giving (wife-deciding households), households where the husband has sole discretion over giving (husband-deciding households), households where giving is decided jointly by spouses (joint-deciding households), and households where spouses

[1] If one is concerned with the endogeneity of the after-tax price of giving, one can use the "first-dollar" price of giving (the price of giving had no charitable contributions been made) as an instrument for the price of giving.

make separate decisions over giving (separate-deciding households). The average charitable contribution for married households is $3,088.57. The complete set of summary statistics for single and married households can be found in Tables B.1 and B.2, respectively.

2.3 Gender Differences

2.3.1 Single Individuals

As previously mentioned in Section 2.1, there is extensive evidence of gender differences with regards to social preferences; I seek to validate this in the data. Using the PSID data, I look for empirical evidence of gender differences in propensities to give for single individuals. Table 2.1 examines the differences in how determinants of giving are correlated with one's tendency to give. Columns 1 and 2 look at the extensive margin; Columns 3 and 4 look at the intensive margin (inverse hyperbolic sine of total contributions). Not surprisingly, lower prices of giving and higher levels of income are strongly associated with more giving. Age, education and religion are also associated with significantly higher levels of giving. As expected, testing for gender differences in giving behavior leads me to reject the null hypothesis for both the extensive margin ($p = 0.0238$) and the intensive margin ($p = 0.0188$) at conventional significance levels, providing evidence that single women and men display different tendencies towards giving.

2.3.2 Married Couples

Do the differences between genders in propensities to give carry over to married couples? To answer this, I look for evidence of difference in propensities to give for married couples by the household's decision-maker. The 2003 and 2005 waves of the PSID contain the following question to ascertain the household's decision-maker for charitable contributions: "When you and [NAME OF SPOUSE] made decisions about supporting charities, did one of you make

Table 2.1: Propensity to Give Charitably for Single Individuals

	Probit		Tobit	
	Male	Female	Male	Female
Price	-0.655***	-1.111***	-10.02***	-12.85***
	(0.0863)	(0.0955)	(1.140)	(0.884)
Log Income	0.0110***	0.0132***	0.199***	0.214***
	(0.00283)	(0.00213)	(0.0454)	(0.0297)
Age	0.00695***	0.00632***	0.120***	0.0974***
	(0.000682)	(0.000507)	(0.0105)	(0.00692)
High School Graduate	0.0909***	0.119***	1.716***	1.911***
	(0.0248)	(0.0199)	(0.424)	(0.296)
Attended College	0.254***	0.232***	4.192***	3.493***
	(0.0323)	(0.0281)	(0.498)	(0.370)
Attended Graduate School	0.177***	0.226***	2.909***	3.352***
	(0.0643)	(0.0558)	(0.804)	(0.534)
Household Size	-0.00309	-0.00422	-0.0381	-0.0505
	(0.00706)	(0.00584)	(0.118)	(0.0833)
Religion	0.236***	0.208***	4.057***	3.253***
	(0.0169)	(0.0174)	(0.296)	(0.250)
2002 Dummy	-0.0280*	-0.0400***	-0.493**	-0.542***
	(0.0147)	(0.0117)	(0.224)	(0.150)
Minority	-0.0801***	-0.101***	-1.262***	-1.207***
	(0.0190)	(0.0169)	(0.308)	(0.227)
Observations	2787	4180	2787	4180

Standard errors in parentheses are clustered by household.
The dependent variable in the probit regressions is an indicator that takes the value of one if the household made charitable contributions.
The dependent variable in the tobit regressions is the inverse hyperbolic sine of total contributions.
Estimates are weighted by probability weights.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

most of the decisions about how much to give each charity, did you mostly decide together, or did you each make your own separate decisions?" It is important to note that this question was only asked to households that made non-zero charitable contributions, so I am only able to look for differences in giving patterns between household decision-makers in the intensive margin, whereas Andreoni et al. (2003) were able to look for differences in both the extensive and intensive margins with CPS data.

It is not unreasonable to expect to see differences in the propensities to give between households of various decision-making regimes. The separate-spheres model would suggest that there must be husband-deciding, wife-deciding, and joint-deciding households to justify their decision to bargain. It also suggests that the disagreement point is located on the frontier for separate-deciding households (i.e. there is no surplus to divide). These fundamental differences suggest different propensities to give between bargaining and non-bargaining households. Table 2.2 shows the results of regressing the log of total contributions on determinants of giving. Again, the price of giving is significantly associated with lower levels of giving across most decision-making regimes. Income is associated with higher levels of giving, although this is only significant for separate-deciders. Just as before, I test for differences in the propensities to give between the decision-making regimes. Not surprisingly, testing for these differences again leads me to reject the null hypothesis ($p < 0.00001$). I also conduct pairwise tests between pairs of decision-making regimes; all tests lead me to reject the null hypothesis of similar propensities to give (this is contrast to Andreoni et al. (2003), who found no statistically significant difference between husband-deciding and joint-deciding couples). This provides evidence that households with different decision-making regimes behave differently; these differences may be partially attributed to the effect of bargaining.

Figure 2.1 shows the average level of giving for a representative household for each decision-making regime at various price points. There are two main takeaways from Figure 2.1. The first is that wife-deciding households donate more than husband-deciding households when the price of giving is low and less when the price of giving is high. The second is that husband-deciding households tend to be more reactive to price changes than wife-households. These two points are in line with the findings of Andreoni et al. (2003) and Yörük (2010), and are consistent with the main results of Andreoni and Vesterlund (2001). More importantly, they show that the decision-making regime can have a large impact on the household's choices.

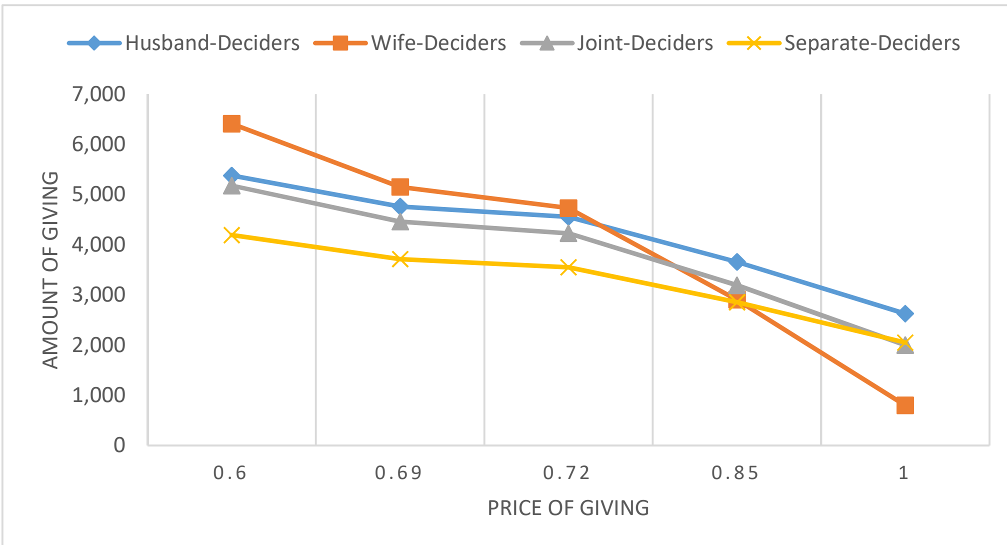


Figure 2.1: The average level of giving for a representative household at various price points.

In addition to examining differences in giving patterns between households with different decision-makers, I follow Andreoni et al. (2003) by examining how gifts are distributed across different types of charities by constructing the Herfindahl index of giving for each household. The PSID asks households to report the amount donated to each of the following 11 types of organizations: religious, combination organizations, needy, health, education, youth, arts/cultural, neighborhood/communities, environmental, world peace, international, and miscellaneous. For a given household-year observation, the Herfindahl index is defined as $HI = \sum_{c=1}^{11} (\frac{s_c}{C})^2$, where s_c is the level of charitable contributions made to organizations of type c and C is the total level of charitable contributions. Note the two extremes of the Herfindahl index: if a household chooses to donate to only one type of organization, its Herfindahl index will be one; if a household instead chooses to donate to all types of organizations in equal proportion, its Herfindahl index will be about 0.091. That is, higher levels of the Herfindahl index indicate a higher concentration of giving. The Herfindahl index is 0.597 for couples where the husband is the decider, 0.599 for couples where the wife is the decider, 0.699 for couples that make decisions jointly, and 0.578 for couples that make separate decisions. These indices are significantly different between the various decision-making regimes and more importantly, the Herfindahl index for couples that make decisions jointly is significantly larger than those who do not, implying that couples that make decisions together tend to concentrate their giving around fewer domains than those that do not. Interestingly, the difference in the Herfindahl index between husband-deciding, wife-deciding, and separately-deciding couples is not significantly different. One possible explanation for this is that spouses' preferences towards giving do not perfectly align and bargaining frictions force couples to only contribute to causes that both spouses support, whereas when household giving decisions are made by only one spouse or separately, giving is more likely to reflect one or both spouses' preferences more broadly. For example, suppose the husband prefers to spend charitable dollars on religious and youth causes and the wife prefers to spend charitable dollars on youth and environmental causes. If they jointly-decide household giving, then they will certainly contribute towards youth causes, but bargaining frictions may prevent the household from contributing to religious and environmental causes. However, if negotiations break down and they make separate decisions, then we may expect each spouse to donate to their preferred causes; the household will contribute to religious, youth, and environmental causes. Instead, suppose one spouse has decision-making authority for the household. For example, suppose the husband cedes decision-making authority to the wife. Then, we might expect the wife to make decisions on behalf of the household based on her own preferences and donate to youth and environmental causes. These types of interactions can explain why joint-deciding households tend to concentrate their giving relative to households where decisions are made separately or by one spouse. These patterns in the Herfindahl index differ from the results found by Andreoni et al. (2003), who find that male decision-makers concentrate their giving moreso than female decision-makers and that jointly-deciding couples do not have significantly different levels of concentration than male decision-makers.

2.4 Bargaining Costs/Benefits

2.4.1 Husband- and Wife-Deciders

In the previous sections, I have so far documented the differences in giving behavior for both single individuals and married couples. While this may not be surprising, the more interesting question is to understand in which direction these differences manifest themselves. That is, when couples make joint decisions on charitable giving, do their tastes align such that their total contributions are larger than what they would have been if the couple failed to reach an agreement, or do their preferences misalign in such a way that depresses total giving? In my first approach to measure the bargaining costs/benefits of married couples, I follow the methodology of Andreoni et al. (2003) and compare giving by couples that decide jointly to their predicted levels had either of the spouses had sole discretion over household giving. Formally, this involves estimating the following set of equations using pooled ordinary least squares:

$$G_{ht} = \phi + H'_{ht}\beta_m + C'_{ht}\beta_c + \eta_{ht} \tag{2.1}$$

$$G_{ht} = \omega + W'_{ht}\theta_f + C'_{ht}\theta_c + \mu_{ht} \tag{2.2}$$

$$G_{ht} = \rho + \alpha_m \widehat{G}^m_{ht} + \alpha_f \widehat{G}^f_{ht} + C'_{ht}\gamma + \varepsilon_{ht} \tag{2.3}$$

G_{ht} is household h's level of giving in period t. C_{ht}, W_{ht}, and H_{ht} are vectors of common (i.e. household), wife, and husband characteristics, respectively. Equations 2.1 and 2.2 are estimated on husband- and wife-deciding households, respectively. Their respective fitted values, $\widehat{G}^m$ and $\widehat{G}^f$, are the predicted levels of giving for joint-deciding couples if either spouse had sole discretion over household giving. These predicted levels are then used as regressors of interest in Equation 2.3, which is estimated only on joint-deciding couples. α_m (α_f) represents the amount by which a couple's charitable contributions will increase by, on average, if the amount the cou-

Table 2.2: Propensity to Give Charitably for Married Couples

	Husband Deciders	Wife Deciders	Joint Deciders	Separate Deciders
Price	-1.209	-3.520***	-1.977***	-1.427***
	(0.741)	(0.685)	(0.229)	(0.433)
Log Income	0.0391	0.0553	0.0141	0.0661***
	(0.0391)	(0.0426)	(0.0135)	(0.0252)
Age	0.0361	0.0279	0.0135*	0.0277*
	(0.0272)	(0.0242)	(0.00783)	(0.0146)
Wife Age	-0.000738	0.0242	0.0120	0.000920
	(0.0278)	(0.0253)	(0.00796)	(0.0157)
High School Graduate	0.472	-0.0536	0.165	-0.341
	(0.815)	(0.286)	(0.128)	(0.323)
Attended College	0.466	-0.394	0.262**	-0.132
	(0.846)	(0.341)	(0.134)	(0.339)
Attended Graduate School	1.087	-0.341	0.552***	0.224
	(0.870)	(0.443)	(0.173)	(0.351)
Wife High School Graduate	0.395	-0.318	0.191	0.304
	(0.467)	(0.299)	(0.124)	(0.270)
Wife Attended College	0.831	0.154	0.319**	0.564**
	(0.507)	(0.335)	(0.137)	(0.278)
Wife Attended Graduate School	1.049*	-0.0424	0.221	0.691**
	(0.538)	(0.413)	(0.174)	(0.319)
Minority	0.0713	0.355	-0.396***	0.459*
	(0.308)	(0.323)	(0.107)	(0.258)
Household Size	0.250**	0.164*	0.0753**	0.0297
	(0.118)	(0.0991)	(0.0296)	(0.0690)
Religion	0.485	0.718***	1.033***	0.841***
	(0.408)	(0.208)	(0.0915)	(0.170)
2002 Dummy	-0.0383	0.174	-0.0989**	-0.369***
	(0.198)	(0.169)	(0.0434)	(0.125)
Constant	2.780**	2.366**	3.965***	3.697***
	(1.269)	(1.125)	(0.327)	(0.684)
Observations	129	218	2609	512

Standard errors in parentheses are clustered by household
The dependent variable is the log of total contributions.
Sample is restricted to households who reported positive charitable contributions.
Estimates are weighted by probability weights
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

ple would give had the husband (wife) been in charge were to increase by \$1. For example, if α_m is one, then a \$1 increase in the charitable contributions the couple would have made had the husband been in charge of charitable contributions would be associated with a \$1 increase in the charitable contributions of jointly-deciding couples, providing evidence of high bargaining power. On the other hand, if α_m is zero, then the same \$1 increase in the charitable contributions the couple would have made had the husband been in charge of charitable contributions would be associated with no change in the charitable contributions of jointly-deciding couples, providing evidence of no bargaining power. These parameters can be thought of as the extent to which each spouse's preferences are represented in the bargained outcome. Thus, larger values of α_m and α_f represent higher levels of bargaining power for the husband and wife, respectively. The estimated coefficients (and standard errors) for $\widehat{\alpha}_m$ and $\widehat{\alpha}_f$ are 0.344 (0.203) and 0.239 (0.186), respectively. $\widehat{\alpha}_m + \widehat{\alpha}_f = 0.584$, with a standard error of 0.209[2] and it is significantly different from one (p = 0.0461).[3] This suggests that, on average, bargaining is associated with a 41.6% decrease in giving. This result starkly contrasts those made by Andreoni et al. (2003) and Yörük (2010). In particular, Andreoni et al. (2003), using CPS data, finds bargaining associated with only a 4% decrease in giving. Yörük (2010), using only the 2003 wave of the PSID, finds bargaining associated with a 6% increase in giving. However, it should also be noted that Yörük (2010) estimated Equations 2.1, 2.2, and 2.3 on jointly-deciding couples (that is, no information from husband- and wife-deciding couples were used), so it is not clear if those estimations accurately measure bargaining power.

In order to construct the counterfactual for jointly married couples, I use regression esti-

[2]The estimation of $\widehat{\alpha}_m$ and $\widehat{\alpha}_f$ involves a two-step procedure, where the second stage estimations use regressors generated from the first stage. As such, the point estimates in the second stage are consistent, but the standard errors calculated in the second stage are no longer correct (see Pagan (1984) and Murphy and Topel (1985)). To construct the correct standard errors, I use a pairwise bootstrap and do not resample for the second stage. In particular, the first stage involves randomly choosing H households with replacement (this is to allow for correlations in error terms within the same household) to construct $\widehat{G}^m$ and $\widehat{G}^f$. Then, using the same random sample, I use $\widehat{G}^m$ and $\widehat{G}^f$ from the first stage to estimate α_m and α_f. This procedure is repeated 1000 times. The standard deviation from the 1000 estimates of α_h and α_w from the second stage regressions is the bootstrap standard error for these estimated coefficients.

[3]The 95% confidence interval for $\alpha_m + \alpha_f$ is [0.175, 0.993].

mates from husband-deciding married couples and wife-deciding married couples, so the counterfactual measures of giving are or not biased by the endogeneous selection into marriage (that is, the analysis is restricted to within married couples). However, there may still be endogenous selection in determining who the decision-maker is for married couples.

2.4.2 Separate-Deciders

In order to measure how household giving is associated with bargaining, Andreoni et al. (2003) and Yörük (2010) use husband-deciding and wife-deciding couples as counterfactuals to measure the giving behavior of households in the absence of bargaining. However, this counterfactual may not be the most appropriate one. If a household chooses not to engage in cooperative bargaining, conditional on not undergoing divorce, we should expect spouses to separately decide their own levels of giving (as proposed by Lundberg and Pollak (1993)). Fortunately, the PSID allows us to identify couples that are separate-deciders.[4] Analyzing these households through the lens of the separate-spheres bargaining model implies that spouses of households that identify as separate-deciding failed to come to an agreement; their charitable contributions reflect a noncoopereative equilibrium that is their threat point.

There are two ways to use separate-deciders as a counterfactual. The first method is to use a similar methodology by Andreoni et al. (2003) and Yörük (2010). This involves predicting how much a jointly-deciding couple would giving if they were separate-deciders and comparing the predicted levels of giving to their actual levels of giving. Formally, this corresponds to estimating the following pair of regressions:

$$G_{ht} = \alpha + C_{ht}'\gamma + W_{ht}'\kappa + H_{ht}'\delta + \nu_{ht} \tag{2.4}$$

$$G_{ht} = \rho + \tau\widehat{S_{ht}} + C_{ht}'\xi + \varepsilon_{ht} \tag{2.5}$$

[4]The CPS does not ask couples if they are separate-deciders, so this information was not available to Andreoni et al. (2003). Yörük (2010) did not use separate-deciders in his analysis.

Equation 2.4 is estimated only for separate-deciding households. G_{ht} is the total giving by the household. C_{ht}, W_{ht}, and H_{ht} are vectors of common (household), wife, and husband characteristics, respectively. The predicted value from Equation 2.4 is then used as a covariate in Equation 2.5, which is estimated only for joint-deciding households. The fitted value $\widehat{S_{ht}}$ can be interpreted as the imputed level of household giving at the threat point. The parameter of interest is τ from Equation 2.5, which aims to measure the relationship between joint-deciding couples actual giving with their imputed level of giving at the threat point. Similar to how we are interested if the bargaining power for each spouse as measured by Andreoni et al. (2003) and Yörük (2010) are smaller than or greater than one, we are interested in if $\widehat{\tau}$ is smaller or greater than one. For example, if we believe that there is no association between household bargaining and giving, then we should expect τ to be one. That is, if the threat point changes such that the noncooperative outcome changes and we believe no association exists between bargaining and giving, then the same change should be reflected in the bargaining outcome. Similarly, if $\widehat{\tau}$ is 0.75, then each additional dollar made by a separate-deciding couple is associated with only a 75 cent increase in giving by a similar joint-deciding couple. This would lead us to conclude that bargaining is associated with lower levels of giving.

The estimated $\widehat{\tau}$ is 0.372 with a standard error of 0.147 [5]. It is significantly smaller than one. [6] This suggests an even more drastic result than what was estimated earlier in Equation 2.3: intra-household bargaining is associated with a 62.8% decrease in household giving relative to households that make decisions separately.

The obvious issue with using households from other decision-making regimes as counterfactuals to impute the behavior of joint-deciding couples were they to fail to come to an agreement is that there may be unobservable differences between these decision-making regimes that correlated with a couple's propensity to give. For example, the overlap in charitable causes that both spouses support may be substantially different between joint-deciding couples and

[5]The standard error is computed the same way as described for Equation 2.3.

[6]The 95% confidence interval for τ is [0.083, 0.661].

separate-deciding couples. Or perhaps the bargaining frictions are fundamentally different between couples of different decision-making regimes. Such differences would lead to endogeneous selection of the household's decision-making regime. The second method to use separate-deciders attempts to control for some of these unobservables. It involves restricting the sample to joint- and separate-deciding households and regressing total giving on a dummy for being a joint-decider with household fixed-effects, as represented by the following Equation:

$$G_{ht} = \alpha + \beta J_{ht} + \lambda_h + X_{ht}'\theta + \varepsilon_{ht} \tag{2.6}$$

where J_{ht} takes a value of one if household h at time t self-reports as a jointly-deciding household, λ_h are household fixed-effects, X_{ht} is a vector of covariates, and ε_{ht} is an error term. Here, β represents the marginal effect of being a jointly-deciding household (relative to a separately-deciding household) on total giving, all else equal. The presence of household fixed-effects allows me to identify this marginal effect only on within-household variation. That is, this effect is only identified using households that switch between joint-deciding and separate-deciding regime, thus allowing me to control for unobservable household-level, time-invariant confounders. This can arguably allow me to obtain a better measure of the household's threat point. The results for Equation 2.6 are presented below in Table 2.3.

The estimate for β shows there to be an average decrease of \$365-\$370 (12% - 14%) for couples who switch between joint-deciding and separate-deciding regimes. These results are weakly significant ($p = 0.11$ for Column 1, $p = 0.102$ for Column 2, $p = 0.198$ for Column 3, and $p = 0.124$ for Column 4). The main drawback to this approach is the limited number of households that switch in decision-making regimes, thus leading to low statistical power. As such, the results presented in Table 2.3 should be interpreted as weak evidence of reduced giving due to bargaining.

Table 2.3: Household Fixed-Effects Regression

	(1)	(2)	(3)	(4)
Joint-Decider Dummy	-365.029	-369.462	-0.124	-0.143
	(228.971)	(225.601)	(0.096)	(0.093)
Log Income		157.121**		0.026*
		(70.749)		(0.015)
Price		839.594		0.141
		(1256.000)		(0.263)
Household Size		51.334		-0.027
		(214.301)		(0.057)
Religion		-50.286		-0.074
		(216.480)		(0.190)
Observations	1878	1866	1878	1866
Adjusted R^2	0.762	0.764	0.707	0.735
Depdendent Variable	Level of Giving	Level of Giving	Log of Giving	Log of Giving

Standard errors in parentheses

Dependent variable is the amount of money donated by the household.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

2.5 Selection of the Decision-Maker

Although the results presented earlier is of great interest to charities and fundraisers, they cannot use these findings without knowing a household's decision-making regime. Since the analysis presented above is determined by the household's decision-maker, it is important to understand which factors determine the selection of the decision-maker. Following Andreoni et al. (2003) and Yörük (2010), I estimate a multinomial logit on various relevant predictors. Table B.3 shows the estimated marginal effects. Not surprisingly, the husband is more likely to make decisions when he is the primary earner. However, when he is not the primary earner, households are much more likely to make separate decisions. Differences in education are also play a role in determining the decision-maker. When one spouse has more education than the other, then that spouse is more likely to have decision-making authority. Household size is another important determinant; as household sizes increase, decision-making tends to go from spouses making separate decisions to one of the spouses being the sole decision-maker. Lastly, religion is the most important predictor of the decision-maker. Religious households tend to have charitable giving decisions made either by the husband or jointly by the spouses. While this model can provide interesting comparative statics, it has limited utility for charities and fundraisers. How should a charity or fundraiser classify a given household? Looking at predicted probabilities from the multinomial logit reveals a startling observation: all observations are predicted to be a joint-decider. Therefore, a more nuanced approach is required to better classify households by decision-making regimes. Basavanhally (2019b) attempts to predict the household's likelihood of bargaining using a machine learning approach. He finds that common machine learning algorithms outperform the multinomial logit in terms of predictive performance.

2.6 Discussion

My results consistently show that bargaining is associated with lower levels of giving and that husbands and wives have similar bargaining power. This starkly contrasts with the results of Andreoni et al. (2003) and Yörük (2010). While Andreoni et al. (2003) and I both find wives have bargaining power of about 25%, Andreoni et al. (2003) finds husbands have bargaining power of 62%, whereas I find they have bargaining power of about 34%. These differences are important in two ways. Firstly, the results of Andreoni et al. (2003) imply that husbands have more bargaining power than wives, whereas my results imply that husbands and wives have roughly equal bargaining power. Secondly, the results of Andreoni et al. (2003) also imply that bargaining is associated with approximately 4% lower levels of giving, whereas my results imply bargaining is associated with 50%-60% lower levels of giving. One possible explanation for these differences is that unlike the CPS used by Andreoni et al. (2003), the PSID data only provide decision-maker information for married couples that choose to make donations. There is evidence that households that choose not to donate are more likely to feature men as the decision-maker (Rooney et al. (2005)). Therefore, the sample observed in the PSID may simply be one that is under-represented by households whose decisions are made by husbands, thus leading to a lower estimate of husbands' bargaining power (although it should be noted that the result of bargaining being associated with lower levels of giving is robust to the counterfactual used). However, the more probable explanation is one of cohort effects; the contrasting results are more likely attributable to the advances women have made in the labor market. This is clearly evident from past studies examining trends in labor participation rates, the gender pay gap, own labor supply elasticities, and cross-wage labor elasticities. There is substantial evidence of a narrowing gender pay gap, a narrowing gender gap in labor force participation rates, as well as occupational shifts for women into careers that have typically been dominated by men leading up to the 1990s (see Blau and Kahn (2000) for a comprehensive review of the literature). These trends have continued after 1990, albeit at a slower rate. However, this evidence is not sufficient to shed light on the changing role of women in the household; examining the trends in own and cross labor supply elasticities helps to understand shifting family dynamics. Historically, there have been numerous studies (see Blundell and MaCurdy (1999), Jacobsen (1998), and Filler et al. (1996)) that find the median male labor supply elasticity have been very small (around 0.08) and the median female labor supply elasticity to be much larger (around 0.78). According to Devereux (2004), the cross-wage labor supply elasticies have historically been around -0.4 for women and between -0.001 and -0.06 for men. Thus, women have typically been much more sensitive to wages than men given that they traditionally had more options for substituting labor hours. The cross-wage labor supply elasticities help to highlight the division of labor within the household; men have traditionally been the primary earners, whereas women's wages have typically served as a secondary source of income. However, more recent evidence from Blau and Kahn (2007) and Heim (2007) shows a steady and dramatic reduction in both women own-wage and cross-wage labor-supply elasticities during 1980-2000. These long-term trends suggest that women's own-wage labor supply elasticity is approaching that of men's and women cross-price labor-supply elasticities to be approaching zero. It should also be noted that men's own-wage and cross-wage labor supply elasticities remained largely unchanged during the same time period. This indicates a shift in the labor supply behavior of married women and suggest that there have been substantial cross-cohort changes in preferences towards work. This, in turn, translates into increased relative bargaining power for married women in the household as a result of more powerful threat points. Combining this with the fact that bargaining may be costly at the margin and spouses' preferences for giving may not be perfectly aligned, it is not surprising to see a convergence in spouses' bargaining power and bargaining being associated with lower overall giving.

2.7 Conclusion

In this paper, I investigate the role of intra-household bargaining in household's decisions to donate. In addition to using the method proposed by Andreoni et al. (2003), where giving by joint-deciding couples is modeled as a linear combination of giving by husband- and wife-deciding couples, I use separate-deciding couples as a more natural counterfactual. My results show that bargaining is associated with 40%-60% lower levels of giving, which is much lower than the results reported by Andreoni et al. (2003). Moreover, my results also demonstrate that husbands and wives have approximately equal bargaining power, whereas previous results have shown husbands having significantly higher bargaining power than wives. I argue that the most probable reason for this is the significant advances made by women in the labor market, which has resulted in increased decision-making authority for women in the household and relatively similar bargaining power between spouses. Combining this with the theory that bargaining is costly at the margin and misalignment in preferences between spouses can explain why household bargaining is associated with decreased giving.

Chapter 2, in part is currently being prepared for submission for publication of the material. Basavanhally, Naveen. The dissertation author was the primary investigator and author of this material.

Chapter 3

Predicting Household Bargaining: A Machine Learning Approach

3.1 Introduction

Previous research (Andreoni et al. (2003) and Yörük (2010)) has documented the associations between intra-household bargaining and household charitable giving. More recent research from Basavanhally (2019a) has shown intra-household bargaining to be associated with sharp declines in household charitable giving. While these results are broadly interesting, they are of most interest to charitable organizations and fundraisers. Understanding the associations between bargaining and giving can allow them to strategically target households to maximize the returns on fundraising efforts. However, the decision-making regime of a household is typically not observable to the fundraiser. Fundamentally, the fundraiser must engage in a prediction exercise to predict the decision-making regime of the household based on observable characteristics of the household and its members. Advances in machine learning techniques now allow us to approach such a prediction problem more systematically and rigorously than in the past. In this paper, I attempt to extend the work of Basavanhally (2019a) by developing a prediction algorithm to predict whether or not a household engages in bargaining based on observable characteristics.

The standard methodology from Andreoni et al. (2003) and Yörük (2010) to better understand the selection of the decision-maker has been to estimate a standard logistic regression using age measures, education measures, whether or not the husband is the primary earner, minority status of the household, household size, and the household's religiosity. However, simply looking at the estimated coefficients and marginal effects from such a model provides limited insight. Table C.2 shows the marginal effects from the multinomial logistic regression using the 2003 and 2005 waves of the PSID data. This allows us to conduct some limited inference. For example, households where the husband is the primary earner are more likely to be husband-deciding households and less likely to be jointly-deciding households. Similarly, religion seems to play an important factor in predicting the household's decision-making regime. Households that report to be religious tend more likely to be husband-deciding or joint-deciding households. However, how can a charitable organization or fundraiser make use of these comparative statics? If they must decide whether or not to commit resources towards raising funds from a particular household, these marginal effects are of limited use. Perhaps if they face two households that are equal in all dimensions except one, such a model can help the fundraiser make a strategic decision. However, such a scenario is quite unlikely. Rather, they require a method which will allow them to predict the decision-making regime of the household. Formally, the goal is to develop a classification model that can be used to predict the decision-making regime of a new household not in the dataset. While a logistic regression is one type of classification model, simply examining its estimated coefficients does not inform us of its predictive capabilities. The rest of the paper is organized in the following manner: Section 3.2 discusses a popular approach to evaluate classification models, Section 3.3 outlines the predictive models used in this paper, Section 3.4 describes the data, Section 3.5 presents and discusses the results, and Section 3.6 concludes.

3.2 Evaluating Classification Models

When developing and testing predictive algorithms, preventing overfitting is very important. To prevent overfitting, the dataset is traditionally partitioned into a training set and a testing set. The training set is used to estimate the predictive model and the testing set is used to estimate the out-of-sample error. For binary classification problems, a predictive model typically outputs the predicted probabilities for each class for each observation in the test set. A decision rule is used to then assign a predicted classification to each observation in the test set. There are several metrics traditionally used in machine learning to evaluate the performance of a binary classifier.

The first, and most obvious one, is the raw accuracy. Given a predictive algorithm and a decision rule, the raw accuracy is simply the proportion of correct predictions. However, raw accuracy is not always a good measure. For example, in the case of class imbalance, simply predicting that all observations belong to the majority class can lead to a high accuracy rate; this is not a particularly informative classifier. Also, using predicted probabilities solely for the purpose of creating a class prediction leaves information on the table. For example, a household whose predicted probability of being a joint-decider is 0.51 and another whose predicted probability is 0.99 will have the same class prediction. However, confidence in the latter prediction will be much higher than that for the former prediction. To overcome this, there are alternative metrics traditionally used to evaluate classifiers.

Two popular metrics are the true positive rate and the false positive rate. These measure how well a classifier correctly identifies events and falsely identifies non-events, respectively. Formally, the true positive rate is the proportion of "true" cases that are correctly predicted as such. Similarly, the false positive rate is the proportion of "false" cases that are incorrectly predicted as being true. In the context of the PSID data and intra-household bargaining, suppose non-joint-deciding couples are "true" cases and joint-deciding couples are "false" cases. Then the true positive rate is the number of couples correctly predicted as a non-joint-deciding couple divided by the total number of actual non-joint-deciding couples. The false positive rate is the number of couples incorrectly predicted as a non-joint-deciding couple divided the total number of actual joint-deciding couples. A classifier typically faces a tradeoff between its true positive rate and its false positive rate. Intuitively, to increase a classifier's true positive rate requires more "true" predictions to be made, which also results in more non-events being incorrectly predicted. Thus, as the decision threshold decreases (and more observations are classified as "true"), both the true positive rate and false positive rate weakly increase. The receiving operating characteristic (ROC) curve is a popular tool to examine this tradeoff; it traces out the true positive rate on the vertical axis and the false positive rate on the horizontal axis in the $[1,1] \times [1,1]$ plane as the decision rule varies. The ROC curve for a random classifier will be the 45-degree line, where as the ROC curve for a perfect classifier is the top-left border of the $[1,1] \times [1,1]$ plane. Most classifiers will produce a ROC curve that is a concave function above the 45-degree line. The ROC curve can be used to compare multiple classifiers; those that are closer to the top-left border are typically better classifiers than those closer to the 45-degree line. However, since one ROC curve may not necessarily be uniformly above another ROC curve, the area under the curve (AUC) is typically used to compare classifiers. A random classifier will have an AUC of 0.5 and a perfect classifier will have an AUC of one; classifiers with higher AUC levels are preferred to those with lower AUC levels. For the remainder of this paper, the AUC will be used to characterize a classifier's efficacy.

3.3 Methods

I use a suite of popular classification algorithms, including penalized logistic regression, k-nearest neighbors, random forests, and multiple adaptive regression splines (MARS); these algorithms are generally good predictors for a wide range of predictive problems. Penalized logistic regressions simultaneously perform feature selection and prediction, and are generally quite interpretable. k-nearest neighbors are easy to implement and have good overall performance. Random forests serve as good predictors, as they are robust to noisy response, generate low variance predictions relative to other models, and by construction do not overfit (see Breiman (2001)). Lastly, MARS is an effective tool for capturing potential non-linearities and interactions in the data through piecewise linear models. In addition, an attractive feature of these algorithms (except for k-nearest neighbors) is that, by construction, most of them will only use the most relevant covariates for prediction. Thus, this approach can help our understanding of what factors drive selection into household bargaining.

To prevent overfitting, 70% of the data are used for building the model and the remaining 30% is held out in order to test the model's predictive ability. All of the models discussed above have tuning parameters. To find the optimal tuning parameter, I use a 10-fold cross-validation procedure, which is then repeated five times, on the training data. The optimal tuning parameter is the one that produces the largest AUC. The model is then re-run with the optimal tuning parameter on the entire training set and is tested on the hold-out data.

3.4 Data

The data used in this paper are from the 2003 and 2005 waves of the Center of Philanthropy Panel Study (COPPS) (the philanthropy module of the PSID). The sample is restricted to married couples that were reported to have made a charitable contribution. This restricted sample yields 3,379 households. In the PSID dataset, married couples who have made charitable contributions report their decision-making regime as one of the following options: husband-deciding, wife-deciding, joint-deciding, or separate-deciding. As mentioned in 3.3, the data are split into a training set (70%) and a testing set (30%) to prevent overfitting.

Table C.1 shows the summary statistics for relevant predictors of charitable giving for bargaining couples (i.e. joint-deciders) versus non-bargaining couples. Note that there are predictors that are highly correlated. For example, educational attainment in terms of degree earned as well as years of education. Both measures are included because it is not clear, *a priori*, which representation of education would serve as a better predictor for household bargaining. Instead, I take two popular approaches to understand which covariates serve as the best predictors. The first is using recursive feature elimination with random forests, where a random forest is built using all available covariates, and then the least informative covariate is iteratively removed until only the most important predictors remain. The second is to simply allow the models to pick the most relevant predictors. By design, penalized logistic regression, random forests, and MARS will only use the most informative covariates to construct predictions.

3.5 Results

Table C.2 presents the multinomial logistic regression from Basavanhally (2019a), which was originally proposed by Andreoni et al. (2003) as a way to better understand the household's selection of the decision-maker. Table C.3 presents the corresponding binomial logistic regression where the sample is split between bargaining and non-bargaining households.

Figure 3.1 shows the ROC curve for the binomial logistic regression. The AUC is 0.536 with a 95% confidence interval of $[0.495, 0.578]$ (confidence intervals for AUC measures are computed using the method developed by DeLong et al. (1988)); the specification used by Andreoni et al. (2003) and Yörük (2010) is no better than a random classifier.

Before turning to the classifiers outlined in Section 3.3, I first perform recursive feature elimination (RFE) using random forests to determine if any covariates are sufficiently uninformative and should not be used. Surprisingly, the RFE procedure shows that all covariates, including those that are correlated, have sufficient predictive power to justify their inclusion. Figure C.1 shows that ROC increases with the number of variables included. Therefore, I include all covariates for each predictive model. Figure 3.2 presents the main results for the penalized lo-

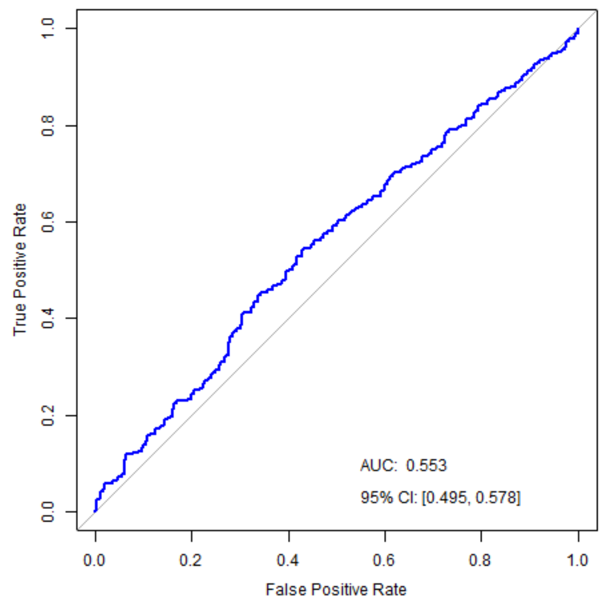


Figure 3.1: Andreoni et al. (2003) ROC Plot

gistic, k-nearest neighbors, random forest, and MARS models. The AUC ranges from 0.610 to 0.636. We see that the predictive power is robust across all four algorithms and is superior to the logistic specification estimated in Table C.3. Moreover, this robustness provides confidence that we are not overlooking possible nuances in the underlying data-generating process. For example, the penalized logistic regression does not contain non-linear terms, but given that its predictive power is on par with the MARS model, we can be reasonably sure that there are no meaningful interactions or non-linearities being overlooked.

Measures of variable importance are also constructed for each model. These measures serve two main purposes. The first is that while each model has a slightly different measure of variable importance, common patterns across models may be able to shed light on the underlying mechanics. However, it is very important that one not put too much weight on these variable importance measures, as they only measure the predictive strength of each covariate and have no causal interpretation. The second is that since each model performs relatively similarly, the fundraiser can choose which predictive model to use based on the data they have access to. Fig-

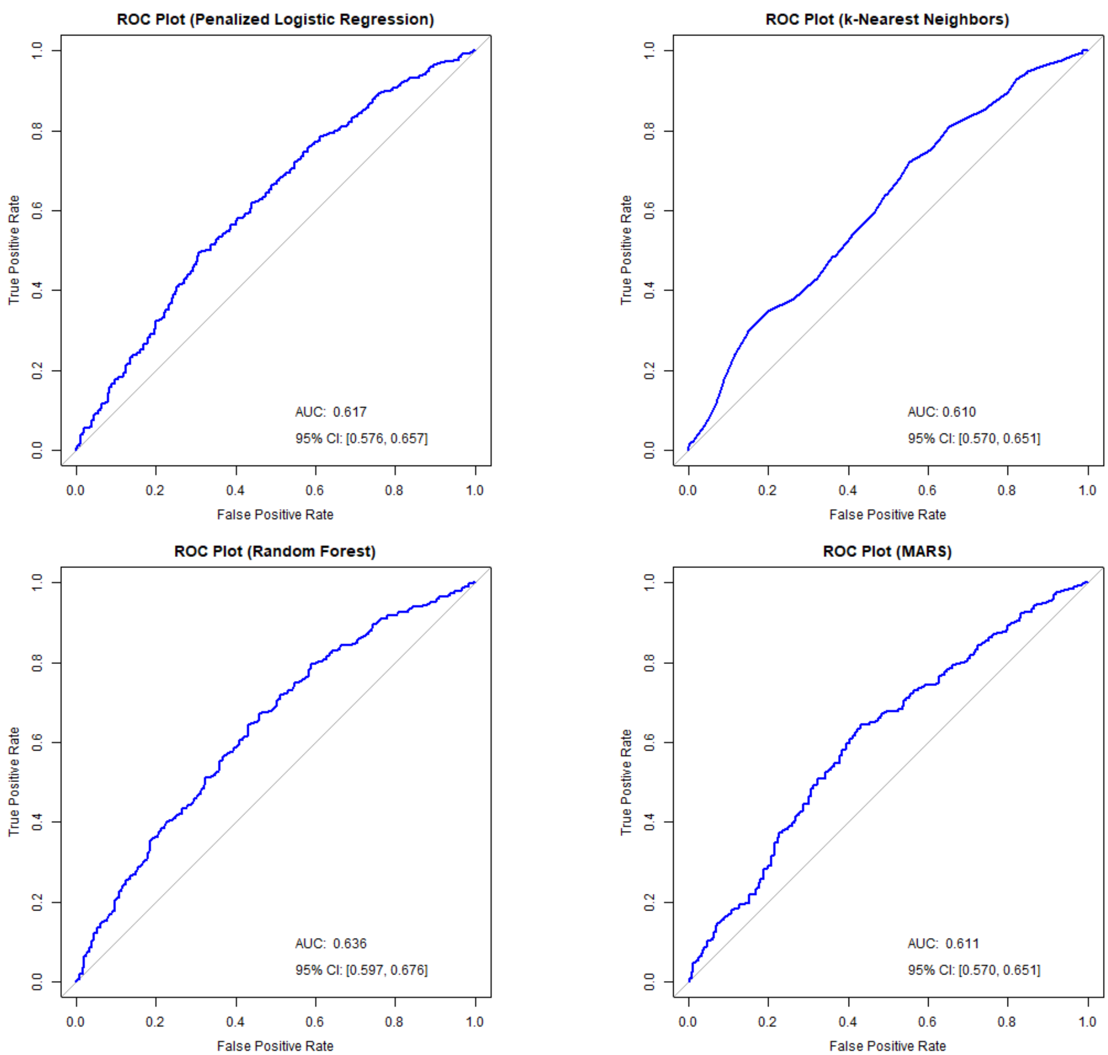


Figure 3.2: ROC Curves

ure C.4 shows the variable importance measures for each model. Not surprisingly, income is an important predictor for household bargaining. The age of spouses as well as the wife's education level also appear to be important predictors for household bargaining. While these patterns answer how households select into bargaining, they may be able to inspire future research to better understand the underlying mechanisms.

3.6 Conclusion

In this paper, I expand on the work of Andreoni et al. (2003), Yörük (2010), and Basavanhally (2019a) by taking a machine learning approach to predict the likelihood of a household engaging in intra-household bargaining based on observables. I first show that the standard logistic regressions offered by Andreoni et al. (2003) and Yörük (2010) perform no better than a random classifier. Then, I estimate a suite of classification models: penalized logistic regression, *k*-nearest neighbors, random forests, and MARS; these models serve as much better predictors. Variable importance measures suggest that household income (specifically the wife's wage), wife education, and age of spouses are good predictors of household bargaining. While these models serve as a baseline for predicting intra-household bargaining, understanding the underlying mechanisms behind selection into such decision-making regimes may be an interesting area for future research.

Chapter 3, in part is currently being prepared for submission for publication of the material. Basavanhally, Naveen. The dissertation author was the primary investigator and author of this material.

Appendix A

Chapter 1 Appendix

A.1 Summary Statistics

Table A.1: Summary Statistics

	Total Sample	Online Subsample	Mail Subsample	Field Office Subsample
Proportion of Females	0.492	0.505	0.534	0.480
Age	44.323	43.693	51.206	42.864
Median Income (%)	75,690.94	82,189.31	80,311.45	73,397.68
Minority Representation (%)	58.416	55.411	54.227	59.947
Black Representation (%)	6.130	5.491	5.252	6.453
Hispanic Representation (%)	37.227	33.396	33.663	38.772
Asian Representation (%)	15.058	16.524	15.312	14.772
Education (%)	38.746	42.389	41.312	37.466
Observations	31,402,799	4,199,678	5,075,767	22,127,354

Minority representation (including Black, Hispanic, and Asian representation) and education data are zip-code level US Census data.
Minority representation is the sum of Black, Hispanic, and Asian representation.
Education is a zip-code level measure of the percentage of individuals who have completed a higher education degree.

A.2 Robustness Checks

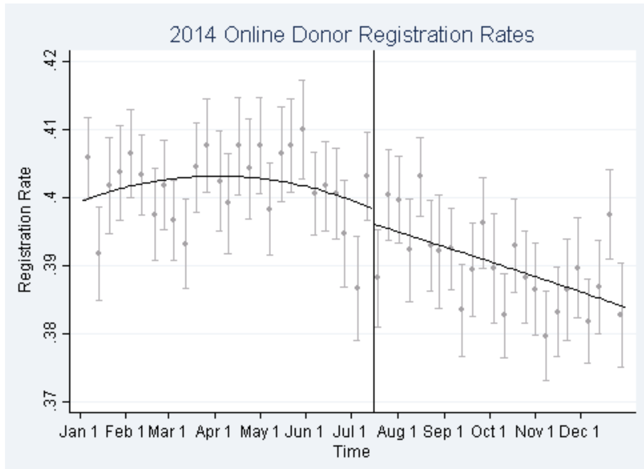


Figure A.1: Online Registration Rates for 2014

Binned online donor registration rates for 2014 plotted by week. Error bars represent 95% confidence intervals.

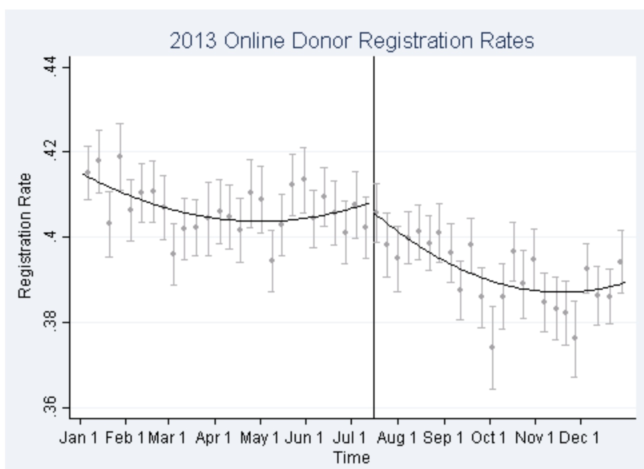


Figure A.2: Online Registration Rates for 2013

Binned online donor registration rates for 2013 plotted by week. Error bars represent 95% confidence intervals.

Table A.2: Placebo Test (Regression Discontinuity)

	2013		2014	
Mandated Choice	-0.00258	-0.00106	-0.00500	-0.00299
	(0.00296)	(0.00257)	(0.00314)	(0.00290)
Female		0.0739***		0.0701***
		(0.00105)		(0.000936)
Age		-0.00100***		-0.000985***
		(0.0000421)		(0.0000372)
Constant	0.403***	0.410***	0.401***	0.407***
	(0.00171)	(0.00233)	(0.00225)	(0.00258)
Observations	917498	917498	1025999	1025999
Adjusted R^2	0.000	0.049	0.000	0.050
Additional Controls	No	Yes	No	Yes

Standard errors in parentheses

Age is a continuous measure of an individual's age

Female is a dummy variable indicating whether or not the individual is female.

Additional controls include controls for age, gender, and zip code.

Standard errors are clustered at the daily level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

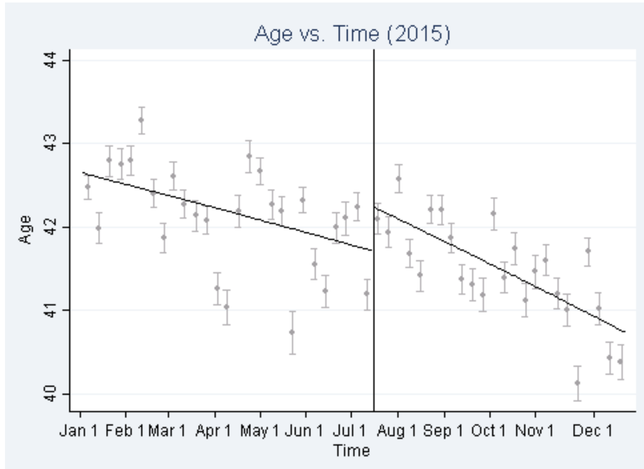


Figure A.3: RD Plot for Age

Binned age averages for 2015 are plotted by week (restricted to those who renewed online). Error bars represent 95% confidence intervals.

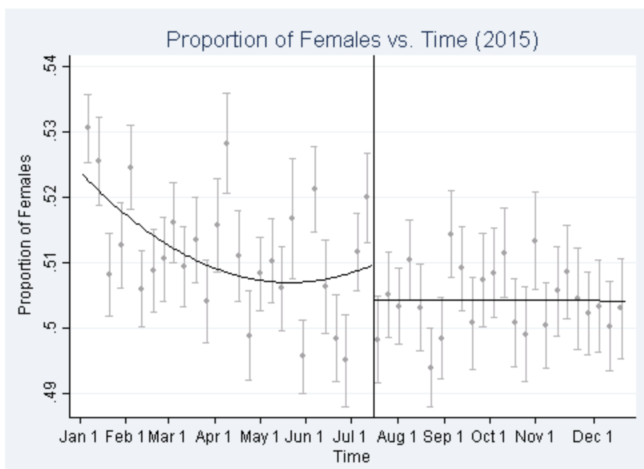


Figure A.4: RD Plot for Proportion of Females

Binned proportion of females for 2015 are plotted by week (restricted to those who renewed online). Error bars represent 95% confidence intervals.

Table A.3: Discontinuity in Baseline Covariates

	Age	Female
Mandated Choice	0.403	-0.00811
	(0.264)	(0.00588)
Constant	41.65***	0.510***
	(0.176)	(0.00474)
Observations	1128588	1128588
Adjusted R^2	0.001	0.000

Standard errors in parentheses
Age is a continuous measure of an individual's age
Female is a dummy variable indicating whether
or not the individual is female.
Standard errors are clustered at the daily level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

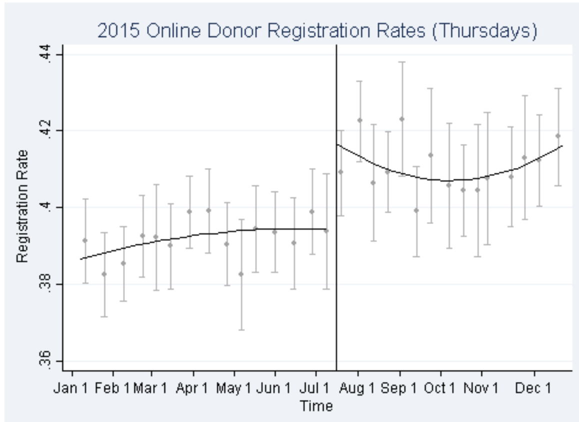


Figure A.5: 2015 Donor Registration Rates on Thursdays

Binned online donor registration rates for 2015 plotted by week (restricted to Thursdays and those who renewed online). Error bars represent 95% confidence intervals.

Table A.4: Parametric (OLS) Regression Discontinuity (Thursdays)

	(1)	(2)	(3)	(4)
Mandated Choice	0.014***	0.016***	0.022***	0.019**
	(0.004)	(0.005)	(0.007)	(0.009)
Age		-0.001***		-0.001***
		(0.000)		(0.000)
Female		0.067***		0.067***
		(0.002)		(0.002)
Constant	0.396***	0.424***	0.394***	0.425***
	(0.003)	(0.003)	(0.005)	(0.004)
Observations	188500	188500	188500	188500
Adjusted R^2	0.000	0.052	0.000	0.052
Additional Controls	No	Yes	No	Yes

Standard errors in parentheses clustered at the daily level.
Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.
Sample is restricted to Thursdays and those who renewed online.
Additional controls include controls for age, gender, and zip code.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3 Heterogeneous Treatment Effects

A.3.1 Gender Effects

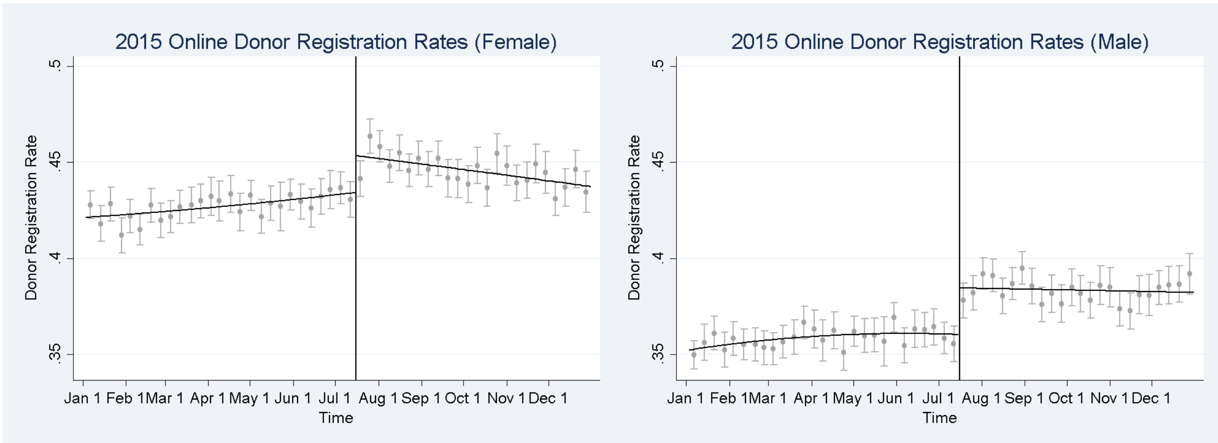


Figure A.6: RD Plots by Gender

Binned online donor registration rates for 2015 are plotted by week for females and males. Error bars represent 95% confidence intervals.

Table A.5: Fully-Flexible Quadratic Regression Discontinuity (Gender)

	60 days	120 days	180 days	240 days	Full Sample
Male	0.026*	0.021*	0.028***	0.029***	0.035***
	(0.014)	(0.012)	(0.009)	(0.008)	(0.007)
Female	0.015	0.015	0.015*	0.016**	0.021***
	(0.013)	(0.010)	(0.008)	(0.007)	(0.006)

Standard errors in parentheses clustered at the week level
Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.
Top row indicates bandwidth around the cutoff.
For example, 60 days restricts observations to those that occur 30 days before and after July 16, 2015.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.2 Age Effects

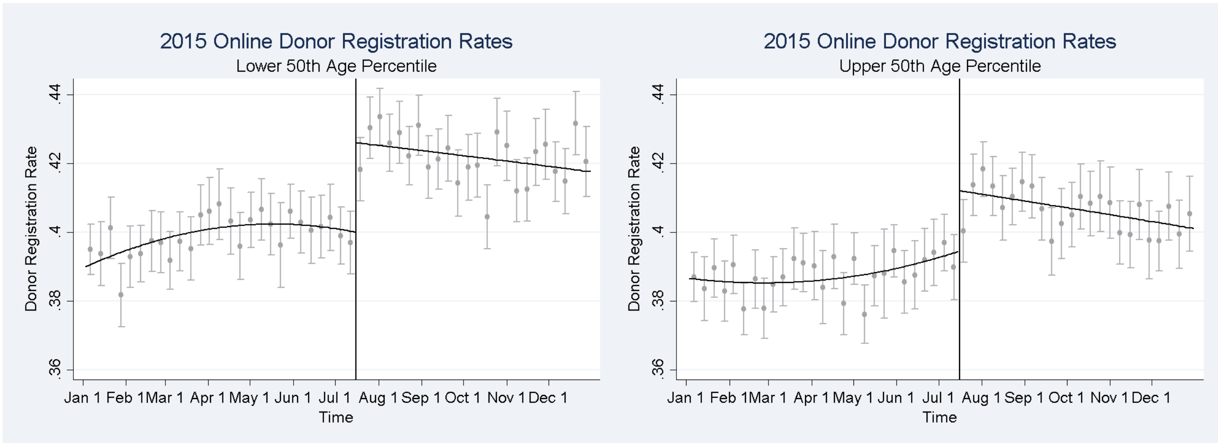


Figure A.7: RD Plots by Age

Binned online donor registration rates for 2015 are plotted by week for the lower and upper 50th age percentiles. Error bars represent 95% confidence intervals.

Table A.6: Fully-Flexible Quadratic Regression Discontinuity (Age)

	60 days	120 days	180 days	240 days	Full Sample
Lower 50th Percentile	0.013	0.020**	0.028***	0.030***	0.031***
	(0.010)	(0.008)	(0.006)	(0.006)	(0.005)
Upper 50th Percentile	0.004	0.010**	0.017***	0.018***	0.020***
	(0.009)	(0.007)	(0.006)	(0.006)	(0.005)

Standard errors in parentheses clustered at the daily level.
Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.
Top row indicates bandwidth around the cutoff.
For example, 60 days restricts observations to those that occur 30 days before and after July 16, 2015.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.3 Income Effects

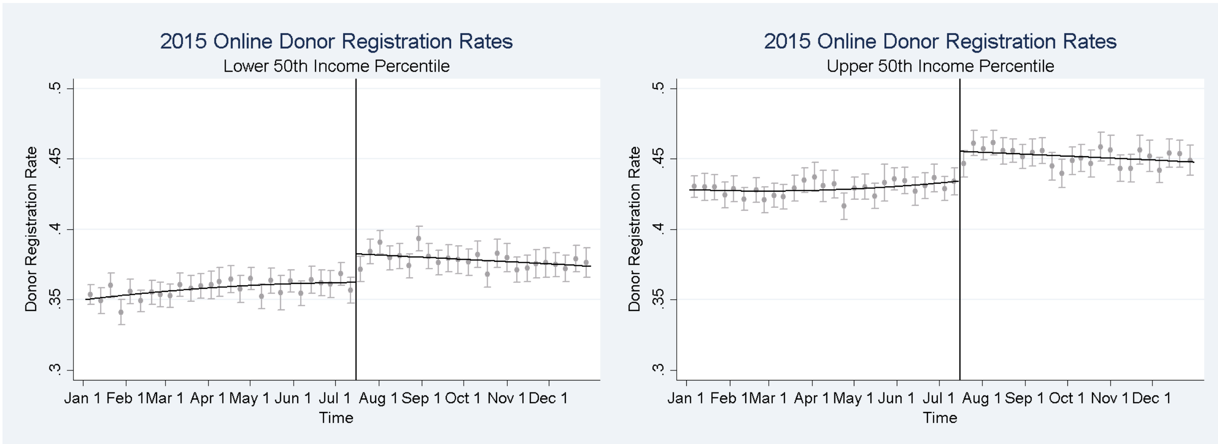


Figure A.8: RD Plots by Income

Binned online donor registration rates for 2015 are plotted by week by median zip-code income. The graph on the left is restricted to individuals living in zip-codes whose median income is in the lower 50th percentile. The graph on the right is restricted to individuals living in zip-codes whose median income is in the upper 50th percentile. Error bars represent 95% confidence intervals.

Table A.7: Fully-Flexible Quadratic Regression Discontinuity (Median Income)

	60 days	120 days	180 days	240 days	Full Sample
1st Half	0.004	0.008	0.010	0.013*	0.021***
	(0.011)	(0.010)	(0.009)	(0.008)	(0.006)
2nd Half	0.011	0.018	0.027***	0.027***	0.027***
	(0.009)	(0.007)	(0.006)	(0.005)	(0.005)

Standard errors in parentheses clustered at the daily level
Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.
Top row indicates bandwidth around the cutoff.
For example, 60 days restricts observations to those that occur 30 days before and after July 16, 2015.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.4 Minority Representation Effects

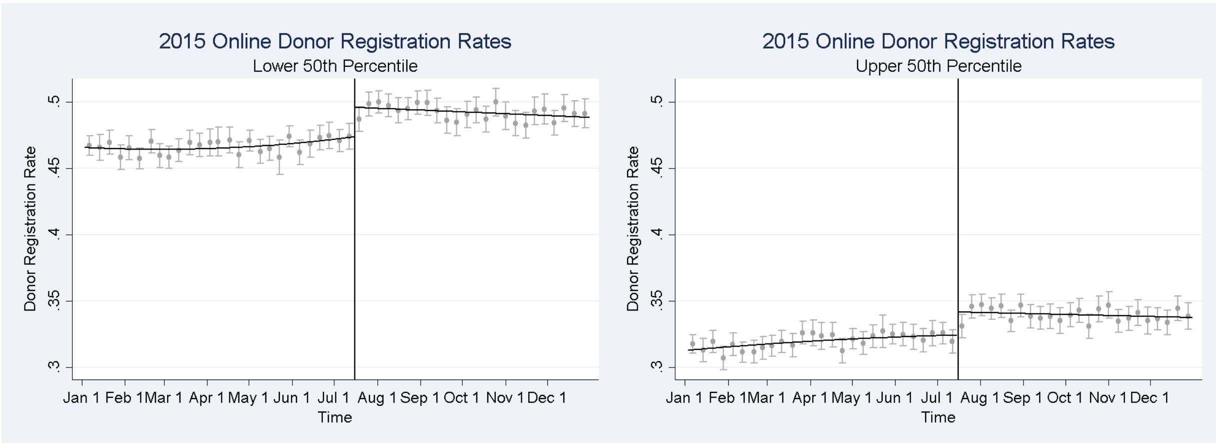


Figure A.9: RD Plots for Minority Representation

Binned online donor registration rates for 2015 are plotted by week by zip-code level minority representation. The graph on the left is restricted to individuals living in zip-codes whose minority percentage is in the lower 50th percentile. The graph on the right is restricted to individuals living in zip-codes whose minority percentage is in the upper 50th percentile. Error bars represent 95% confidence intervals.

Table A.8: Fully-Flexible Quadratic Regression Discontinuity (Minority Representation)

	60 days	120 days	180 days	240 days	Full Sample
1st Half	0.011	0.018**	0.024***	0.026***	0.029***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.005)
2nd Half	0.008	0.011	0.019***	0.019***	0.021***
	(0.009)	(0.007)	(0.006)	(0.006)	(0.005)

Standard errors in parentheses clustered at the daily level
Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.
Top row indicates bandwidth around the cutoff.
For example, 60 days restricts observations to those that occur 30 days before and after July 16, 2015.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.5 Education Effects

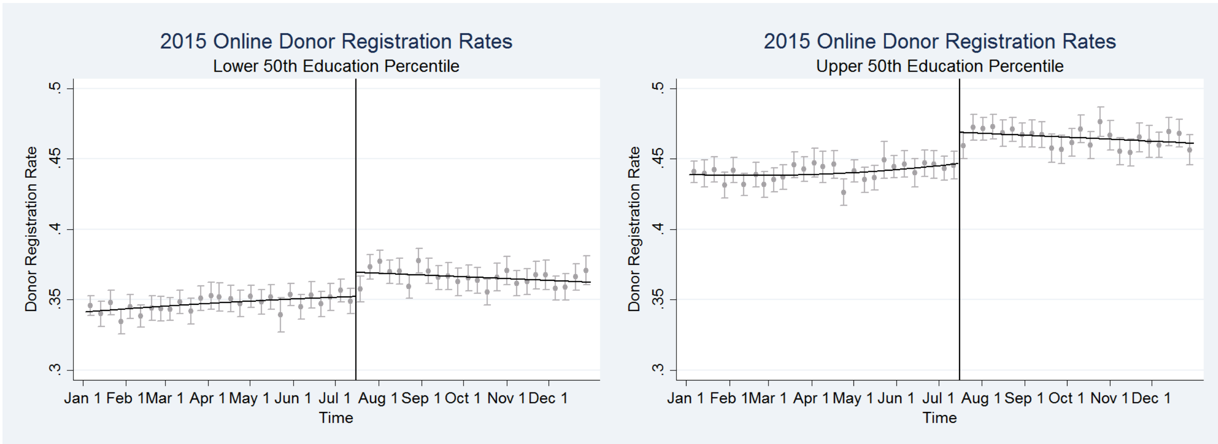


Figure A.10: RD Plots for Education

Binned online donor registration rates for 2015 are plotted by week by zip-code level educational attainment (the proportion of individuals in a zip-code who have completed a higher education degree). The graph on the left is restricted to individuals living in zip-codes whose average educational attainment is in the lower 50th percentile. The graph on the right is restricted to individuals living in zip-codes whose average educational attainment is in the upper 50th percentile. Error bars represent 95% confidence intervals.

Table A.9: Fully-Flexible Quadratic Regression Discontinuity (Education)

	60 days	120 days	180 days	240 days	Full Sample
Lower 50th Percentile	0.008	0.009	0.010	0.016**	0.021***
	(0.009)	(0.009)	(0.007)	(0.006)	(0.005)
Upper 50th Percentile	0.010	0.021**	0.033***	0.029***	0.029***
	(0.012)	(0.009)	(0.007)	(0.007)	(0.005)

Standard errors in parentheses clustered at the daily level
Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.
Top row indicates bandwidth around the cutoff.
For example, 60 days restricts observations to those that occur 30 days before and after July 16, 2015.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3.6 Household Size Effects

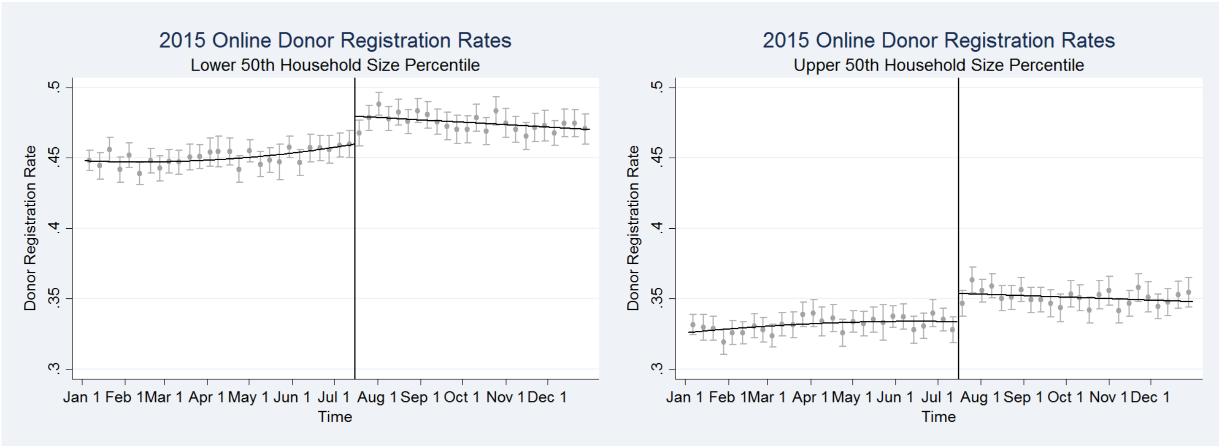


Figure A.11: RD Plots by Household Size

Binned online donor registration rates for 2015 are plotted by week by zip-code level median household size. The graph on the left is restricted to individuals living in zip-codes whose median household size is in the lower 50th percentile. The graph on the right is restricted to individuals living in zip-codes whose median household size is in the upper 50th percentile. Error bars represent 95% confidence intervals.

Table A.10: Fully-Flexible Quadratic Regression Discontinuity (Median Household Size)

	60 days	120 days	180 days	240 days	Full Sample
1st Half	0.000	0.013	0.021***	0.022***	0.023***
	(0.019)	(0.008)	(0.007)	(0.007)	(0.006)
2nd Half	0.019**	0.018**	0.022***	0.025***	0.027***
	(0.009)	(0.007)	(0.006)	(0.006)	(0.005)

Standard errors in parentheses clustered at the daily level
Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.
Top row indicates bandwidth around the cutoff.
For example, 60 days restricts observations to those that occur 30 days before and after July 16, 2015.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.4 Additional RD Specifications

Table A.11: Parametric (OLS) Regression Discontinuity (60-Day Bandwidth)

	(1)	(2)	(3)	(4)	(5)	(6)
Default Removal	0.019***	0.015***	0.015***	0.010	0.008	0.007
	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)
Age		-0.001***	-0.001***		-0.001***	-0.001***
		(0.000)	(0.000)		(0.000)	(0.000)
Female		0.068***	0.068***		0.068***	0.068***
		(0.002)	(0.002)		(0.002)	(0.002)
Constant	0.395***	0.415***	0.418***	0.390***	0.410***	0.413***
	(0.003)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Observations	212337	212337	212337	212337	212337	212337
Adjusted R^2	0.001	0.054	0.054	0.001	0.054	0.054
Additional Controls	No	Yes	Yes	No	Yes	Yes
Polynomial Order	1	1	1	2	2	2
DOW Dummies	No	No	Yes	No	No	Yes

Standard errors in parentheses
Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.
Additional controls include controls for age, gender, and zip code.
DOW dummies control for the day-of-week.
Observations are restricted to those that occur 30 days before and after July 16, 2015.
Robust standard errors are used for inference.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Parametric (OLS) Regression Discontinuity (120-Day Bandwidth)

	(1)	(2)	(3)	(4)	(5)	(6)
Default Removal	0.023***	0.020***	0.020***	0.017***	0.013***	0.014***
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)
Age		-0.001***	-0.001***		-0.001***	-0.001***
		(0.000)	(0.000)		(0.000)	(0.000)
Female		0.067***	0.067***		0.067***	0.067***
		(0.002)	(0.002)		(0.002)	(0.002)
Constant	0.396***	0.419***	0.423***	0.395***	0.418***	0.422***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	390977	390977	390977	390977	390977	390977
Adjusted R^2	0.000	0.052	0.052	0.000	0.052	0.052
Additional Controls	No	Yes	Yes	No	Yes	Yes
Polynomial Order	1	1	1	2	2	2
DOW Dummies	No	No	Yes	No	No	Yes

Standard errors in parentheses

Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.

Additional controls include controls for age, gender, and zip code.

DOW dummies control for the day-of-week.

Observations are restricted to those that occur 60 days before and after July 16, 2015.

Robust standard errors are used for inference.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: Parametric (OLS) Regression Discontinuity (180-Day Bandwidth)

	(1)	(2)	(3)	(4)	(5)	(6)
Default Removal	0.023***	0.021***	0.021***	0.022***	0.018***	0.019***
	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)
Age		-0.001***	-0.001***		-0.001***	-0.001***
		(0.000)	(0.000)		(0.000)	(0.000)
Female		0.067***	0.067***		0.067***	0.067***
		(0.001)	(0.001)		(0.001)	(0.001)
Constant	0.398***	0.420***	0.424***	0.395***	0.419***	0.422***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	580231	580231	580231	580231	580231	580231
Adjusted R^2	0.000	0.052	0.052	0.000	0.052	0.052
Additional Controls	No	Yes	Yes	No	Yes	Yes
Polynomial Order	1	1	1	2	2	2
DOW Dummies	No	No	Yes	No	No	Yes

Standard errors in parentheses

Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.

Additional controls include controls for age, gender, and zip code.

DOW dummies control for the day-of-week.

Observations are restricted to those that occur 90 days before and after July 16, 2015.

Robust standard errors are used for inference.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.14: Parametric (OLS) Regression Discontinuity (240-Day Bandwidth)

	(1)	(2)	(3)	(4)	(5)	(6)
Default Removal	0.025***	0.023***	0.023***	0.021***	0.018***	0.018***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Age		-0.001***	-0.001***		-0.001***	-0.001***
		(0.000)	(0.000)		(0.000)	(0.000)
Female		0.067***	0.067***		0.067***	0.067***
		(0.001)	(0.001)		(0.001)	(0.001)
Constant	0.396***	0.418***	0.422***	0.398***	0.420***	0.424***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	755025	755025	755025	755025	755025	755025
Adjusted R^2	0.000	0.052	0.052	0.000	0.052	0.052
Additional Controls	No	Yes	Yes	No	Yes	Yes
Polynomial Order	1	1	1	2	2	2
DOW Dummies	No	No	Yes	No	No	Yes

Standard errors in parentheses

Dependent variable is a dummy variable indicating whether or not the individual registered as an organ donor.

Additional controls include controls for age, gender, and zip code.

DOW dummies control for the day-of-week.

Observations are restricted to those that occur 120 days before and after July 16, 2015.

Robust standard errors are used for inference.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix B

Chapter 2 Appendix

B.1 Summary Statistics

Table B.1: Summary Statistics for Single Individuals

	Males	Females
Price	0.966	0.972
	(0.002)	(0.002)
Income	35,205.27	19,241.35
	(2830.293)	(742.266)
Age	43.167	52.237
	(0.556)	(0.538)
High School Graduate	0.575	0.570
	(0.016)	(0.014)
Attended College	0.204	0.180
	(0.014)	(0.011)
Attended Graduate School	0.053	0.041
	(0.008)	(0.006)
Minority	0.225	0.276
	(0.013)	(0.012)
Household Size	1.520	1.700
	(0.026)	(0.028)
Religion	0.558	0.706
	(0.015)	(0.012)
Observations	2910	4261

Standard deviations in parentheses.

Estimates are weighted by probability weights.

Table B.2: Summary Statistics for Married Couples

	Husband Deciders	Wife Deciders	Joint Deciders	Separate Deciders
Price	0.905	0.905	0.913	0.894
	(0.008)	(0.006)	(0.002)	(0.004)
Income	93,767.11	106,881.00	77,533.52	93,438.25
	(9777.121)	(23,662.15)	(3,632.306)	(4,972.78)
Contributions	4,183.60	3,036.963	3,138.895	3,048.3
	(602.68)	(512.955)	(169.310)	(353.079)
Husband Age	53.215	51.000	52.346	53.458
	(1.547)	(1.155)	(0.447)	(0.754)
Wife Age	50.592	49.159	50.128	51.389
	(1.474)	(1.149)	(0.444)	(0.730)
Husband High School Graduate	0.288	0.443	0.475	0.420
	(0.048)	(0.040)	(0.014)	(0.029)
Husband Attended College	0.501	0.378	0.318	0.319
	(0.055)	(0.0409)	(0.013)	(0.028)
Husband Attended Graduate School	0.174	0.106	0.102	0.195
	(0.045)	(0.025)	(0.009)	(0.025)
Wife High School Graduate	0.465	0.437	0.547	0.466
	(0.055)	(0.040)	(0.014)	(0.030)
Wife Attended College	0.320	0.385	0.277	0.342
	(0.050)	(0.040)	(0.012)	(0.028)
Wife Attended Graduate School	0.161	0.101	0.071	0.136
	(0.045)	(0.025)	(0.007)	(0.021)
Minority	0.072	0.076	0.088	0.096
	(0.026)	(0.021)	(0.007)	(0.017)
Household Size	3.193	3.150	2.981	2.828
	(0.129)	(0.091)	(0.032)	(0.057)
Religion	0.926	0.776	0.860	0.803
	(0.024)	(0.034)	(0.009)	(0.022)
Observations	122	212	2553	502

Standard deviations in parentheses.
Sample is restricted to households who reported positive charitable contributions.
Estimates are weighted by probability weights.

Table B.3: Determinants of the Household Decision-Maker (Multinomial Logit)

	(1) Husband-Deciders	(2) Wife-Deciders	(3) Joint-Deciders	(4) Separate-Deciders
Husband is Primary Earner	0.021**	-0.002	0.011	-0.030*
	(0.009)	(0.011)	(0.020)	(0.016)
Age Difference (Husband - Wife)	0.001	-0.001	0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)
Education Difference (Husband - Wife)	0.004*	-0.007***	0.004	-0.001
	(0.002)	(0.002)	(0.004)	(0.003)
Average Age	0.001**	0.000	-0.001*	0.001
	(0.000)	(0.000)	(0.001)	(0.001)
Average Education	0.007***	0.004*	-0.030***	0.019***
	(0.002)	(0.002)	(0.005)	(0.004)
Minority	0.003	-0.004	-0.056*	0.057**
	(0.016)	(0.018)	(0.031)	(0.026)
Household Size	0.008**	0.008**	-0.003	-0.013*
	(0.004)	(0.004)	(0.008)	(0.007)
Religion	0.027**	-0.035***	0.054**	-0.047**
	(0.014)	(0.012)	(0.024)	(0.018)
Observations	3389	3389	3389	3389

Standard errors in parentheses

Dependent variable is the amount of money donated by the household.

Average marginal effects are presented for each specification.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix C

Chapter 3 Appendix

C.1 Summary Statistics

Table C.1: Summary Statistics (Training Set)

	Non-Bargainers	Bargainers
Husband Age	48.933	48.050
	(12.327)	(14.274)
Wife Age	46.767	45.897
	(11.720)	(13.856)
Husband Wages	68,661.700	54,671.370
	(123,663.500)	(117,308.200)
Wife Wages	29,470.600	22,903.060
	(29,533.410)	(27,501.500)
Homeowner	0.909	0.885
	(0.288)	(0.319)

Husband High School Graduate	0.491	0.509
	(0.500)	(0.500)
Husband Attended College	0.320	0.313
	(0.467)	(0.464)
Husband Attended Graduate School	0.128	0.082
	(0.335)	(0.274)
Husband's Education (in years)	14.205	13.833
	(2.307)	(2.472)
Wife High School Graduate	0.493	0.547
	(0.500)	(0.498)
Wife Attended College	0.348	0.293
	(0.477)	(0.455)
Wife Attended Graduate School	0.110	0.064
	(0.313)	(0.246)
Wife's Education (in years)	14.216	13.679
	(2.152)	(2.308)
Total Income	98,132.300	77,574.430
	(130,055.000)	(121,822.200)
Household Size	3.214	3.190
	(1.163)	(1.251)
Black	0.199	0.132
	(0.399)	(0.338)
Hispanic	0.017	0.050

	(0.130)	(0.219)
Minority	0.216	0.181
	(0.412)	(0.385)
Religion	0.836	0.873
	(0.371)	(0.333)
Age Difference	2.166	2.153
	(4.244)	(4.186)
Husband is Primary Earner	0.702	0.701
	(0.458)	(0.458)
Average Age (of Spouses)	47.850	46.973
	(11.839)	(13.910)
Education Difference	−0.010	0.155
	(2.136)	(2.172)
Average Education (in years)	14.211	13.756
	(1.958)	(2.130)
Observations	586	1788

Standard deviations in parentheses.

Table C.2: Determinants of the Household Decision-Maker (Multinomial Logit)

	(1) Husband-Deciders	(2) Wife-Deciders	(3) Joint-Deciders	(4) Separate-Deciders
Husband is Primary Earner	0.021**	-0.002	0.011	-0.030*
	(0.009)	(0.011)	(0.020)	(0.016)
Age Difference (Husband - Wife)	0.001	-0.001	0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)
Education Difference (Husband - Wife)	0.004*	-0.007***	0.004	-0.001
	(0.002)	(0.002)	(0.004)	(0.003)
Average Age	0.001**	0.000	-0.001*	0.001
	(0.000)	(0.000)	(0.001)	(0.001)
Average Education	0.007***	0.004*	-0.030***	0.019***
	(0.002)	(0.002)	(0.005)	(0.004)
Minority	0.003	-0.004	-0.056*	0.057**
	(0.016)	(0.018)	(0.031)	(0.026)
Household Size	0.008**	0.008**	-0.003	-0.013*
	(0.004)	(0.004)	(0.008)	(0.007)
Religion	0.027**	-0.035***	0.054**	-0.047**
	(0.014)	(0.012)	(0.024)	(0.018)
Observations	3389	3389	3389	3389

Standard errors in parentheses

Dependent variable is the amount of money donated by the household.

Average marginal effects are presented for each specification.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Determinants of the Household Decision-Maker (Binomial Logit)

	(1) Joint-Deciders
Husband is Primary Earner	0.014
	(0.020)
Age Difference (Husband - Wife)	0.001
	(0.002)
Education Difference	0.004
	(0.004)
Average Age	-0.001*
	(0.001)
Average Education	-0.030***
	(0.005)
Minority	-0.057*
	(0.031)
Household Size	-0.005
	(0.008)
Religion	0.064***
	(0.023)
Observations	3389

Standard errors in parentheses are clustered by household.
Dependent variable is the amount of money donated by the household.
Average marginal effects are presented.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C.2 Recursive Feature Elimination

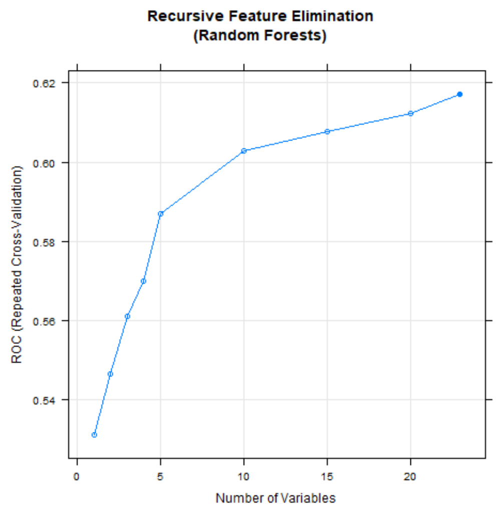


Figure C.1: Recursive Feature Elimination (Random Forests)

C.3 Cross-Validation Plots

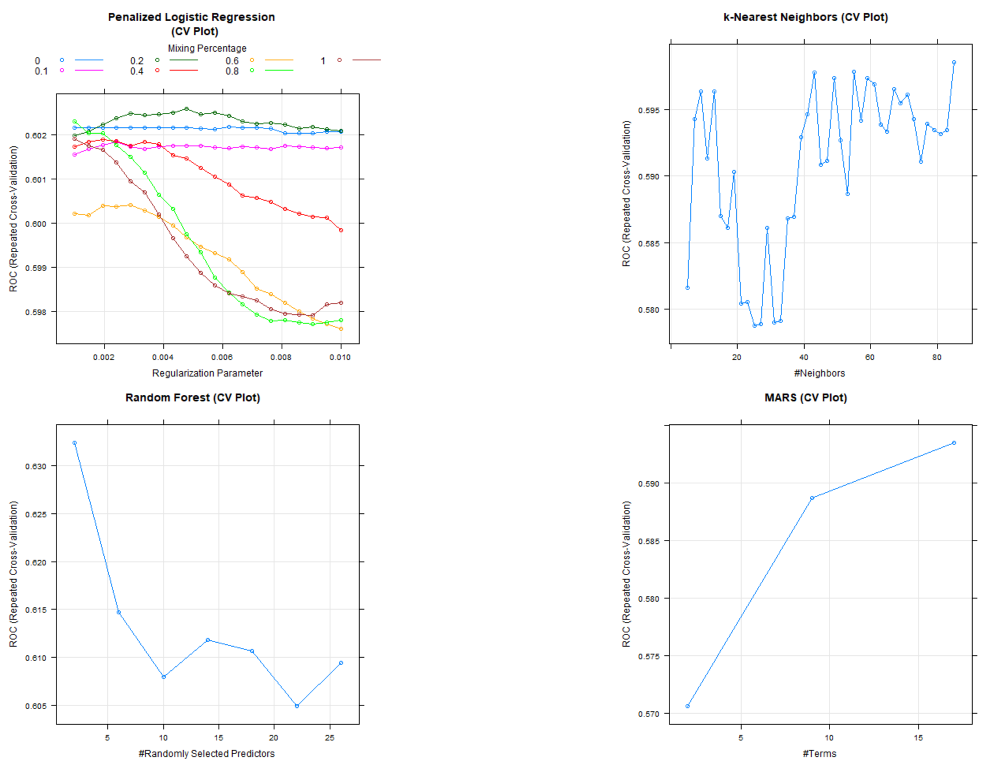


Figure C.2: Cross-Validation Plots for Finding the Optimal Tuning Parameters

C.4 Variable Importance

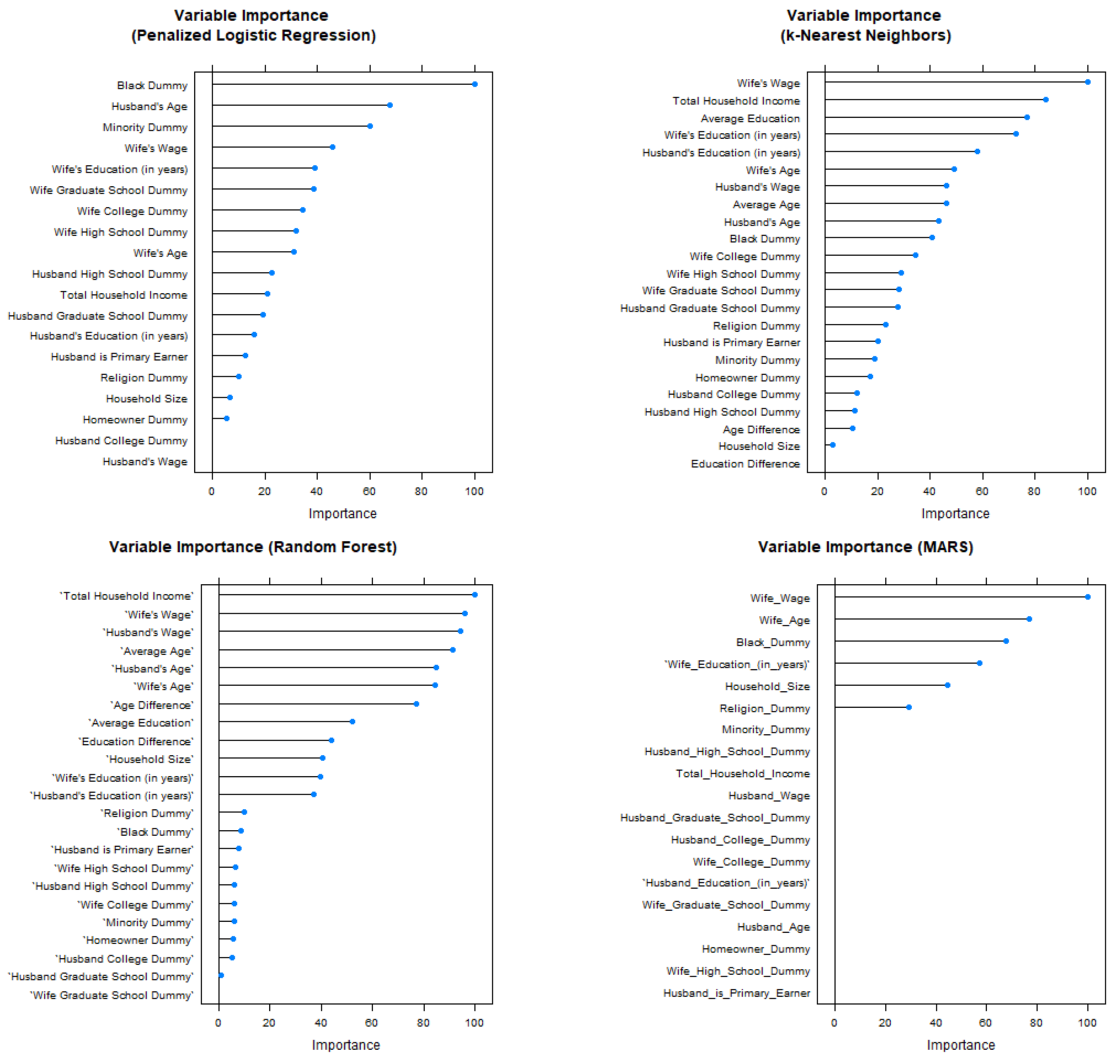


Figure C.4: Variable Importance Plots

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