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Essays in Health Economics

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by

Hannah Bae

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The Dissertation of Hannah Bae is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

University of California San Diego

2024

DEDICATION

To my parents, Jae-Hak Bae and Young-Mee Lyu.

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Chapter 1, "Dependent Insurance Coverage and Parental Job Retention: Evidence from the Affordable Care Act," coauthored with Katherine Meckel and Maggie Shi, is currently being prepared for submission for publication of the material. The dissertation author was a joint primary investigator of this paper.

Chapter 2, "Can Redrawing Boundaries Save Lives? Evidence from a Reform of the Kidney Allocation System," is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the paper.

Chapter 3, "Quality Labeling and Allocation Decisions of Scarce Organs," is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the paper.

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ABSTRACT OF THE DISSERTATION

Essays in Health Economics

by

Hannah Bae

Doctor of Philosophy in Economics

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Professor Katherine Meckel, Chair

This dissertation consists of three chapters that pertain to a core theme in health economics: the role of government policies on individuals, families, and markets. In Chapter 1, we study how employer-sponsored insurance coverage for dependents affects their parents' labor supply decisions. Coverage for dependents is a common feature of employer-sponsored insurance. While prior work shows that employees trade off job mobility for their own coverage, there is less evidence on the intra-family spillovers of dependent coverage onto parental labor supply. We study this using a panel of insurance claims that links dependent insurance enrollment with a proxy for parental job tenure. We develop a regression discontinuity design that exploits variation in coverage eligibility by dependent birth date from the Affordable Care Act, and find

that a one percent increase in the dependent enrollment likelihood increases parental job retention by 0.20 percent.

In Chapter 2, I study a reform of the kidney allocation system that involved re-drawing the areas within which donated organs are matched to patients from fixed regions to circles around each donor hospital. To estimate the causal effects of the reform, I exploit the sharp timing of its implementation as well as variation in predicted treatment intensity across transplant centers. I create a novel dataset consisting of detailed administrative information on the universe of donated organs, transplant candidates, and transplant centers using the universe of transplant records in the United States. I find that the reform increased efficiency in organ allocation by reducing the share of donated kidneys that were discarded and mortality among transplant candidates. I document important distributional effects – kidney recipients after the reform were more likely to have extended dialysis history and reside in counties with higher marginalized populations.

In Chapter 3, I explore the impact of a "high risk" designation on organ quality assessment on the efficiency of the organ allocation system. As all kidneys from deceased donors aged 60 or above are classified as expanded criteria donor (ECD) regardless of their health conditions between 2002 and 2014, the dichotomous kidney classification may have increased confusion about the kidney quality of older donors. Combining administrative data on the universe of organ donors and transplant recipients in the U.S., I employ a regression discontinuity design in the donor's age to study the impact of ECD designation on the use of donated kidneys for transplant. I find that the ECD scheme increases the kidney discard rate at the cut-off. Exploiting the timing of the policy change that replaced the ECD scheme with a continuous measure for organ quality in December 2014, I provide evidence that the discard rate decreases for kidneys no longer classified as marginal kidneys under the new scheme compared to those that remain classified in that category.

Chapter 1

Dependent Insurance Coverage and Parental Job Retention: Evidence from the Affordable Care Act

1 Introduction

Nearly half of Americans rely on employer-sponsored health insurance for insurance coverage (Kaiser Family Foundation, 2022). This tight linkage between insurance and employment in the U.S. has been shown to generate "job lock" in the labor market: that is, employer-sponsored health insurance availability can distort labor supply decisions and reduce job mobility (Madrian, 1994; Gruber and Madrian, 1995, 1997; Garthwaite et al., 2014; Dave et al., 2015). This literature primarily focuses on the effects of an individual's *own* coverage on their employment. Yet a common feature of employer-sponsored health insurance is that coverage can also extend to an employee's children and spouse – their "dependents." 96 percent of employers offering health benefits to their employees also provide coverage to their dependents, and 50 percent of children under 19 in the U.S. are covered under employer-sponsored plans (Kaiser Family Foundation, 2020, 2023).

However despite its prevalence, relatively little is known about whether dependent coverage affects parental labor supply decisions, or the extent of these distortions. On the one hand, dependent coverage is a form of non-wage compensation similar to own coverage, and thus, by increasing the value of employment, should lead to greater job lock. On the other hand, dependent coverage may have a more limited effect because dependents are younger and healthier, because planholders are already "job locked" by their own coverage, or because employers reduce other forms of compensation to offset its cost. Understanding the extent to which dependent insurance causes parental job lock is of critical importance when considering policies that affect coverage for children, such as the Children's Health Insurance Program (CHIP) or insurance coverage mandates.

One factor that has limited prior work on the intra-family spillovers of dependent coverage is a lack of data on both insurance take-up and employment outcomes for different family members. While these outcomes are reported in some survey data, sample sizes are often too small to support well-powered analyses. An important contribution of our paper is our use of a large panel of private health insurance enrollment data from employer-sponsored plans. We leverage three key features of this dataset: (1) a measure of job tenure for the planholder; (2) monthly dependent enrollment information; and (3) the linkage between planholders and dependents. We proxy for job tenure with the number of months a planholder retains coverage from any plan offered by their employer – including those from different insurers – and provide supporting evidence from survey data that this measure is a reliable proxy of job tenure.[1] Future work using this proxy for job mobility may provide valuable insights into the connection between health, insurance, and employment outcomes, as well as potential spillovers within the household.

Using these data yields two key advantages for our analysis. First, the size of the dataset and ability to link family members together allow us to study heterogeneity in job lock across different subgroups – for example, we can test whether parents of relatively sicker dependents are more job locked. Exploring this heterogeneity across subgroups is useful for understanding the mechanisms underlying the job lock response. Second, observing both dependent and parental

[1] In complementary work, Aouad (2023) uses claims data from one insurer to study intra-family spillovers from dependent coverage to parents. Our data, which include claims for all insurers provided by an employer, allows us to follow employees even if they switch insurers. This results in a measure which is well-suited for measuring job lock.

outcomes in the same data allows us to scale each parties' responses with each other. This is informative for drawing policy conclusions from our findings, as our results can be used to calculate how much parental job lock one might expect given the size of an expansion in dependent coverage.

In particular, we use our data to study the effects of a dependent coverage expansion that occurred as part of the Affordable Care Act (ACA). The so-called "dependent mandate" requires private insurers to extend coverage to adult children up to 26, whereas previously coverage was provided through age 19, or 23 for full-time students. Recent work has found sizable increases in insurance coverage among young adults following the dependent mandate (e.g., Akosa Antwi et al., 2013; Sommers et al., 2013; Barbaresco et al., 2015; Carpenter et al., 2021; Kim, 2022) and documented various health and financial impacts on dependents (Sommers et al., 2013; Hernandez-Boussard et al., 2014; Barbaresco et al., 2015; Daw and Sommers, 2018; Blascak and Mikhed, 2023).

To identify the effects of the dependent coverage expansion, we develop a regression discontinuity (RD) design which exploits the fact that, on average, adult dependents born in January became eligible for more months of coverage than those born in December. This difference arises because some plans cover dependents through December of the year in which they turn 26, whereas others only cover dependents through their birth month. Using this RD approach allows us to avoid issues associated with difference-in-differences models in the setting of the ACA dependent mandate, as noted by Slusky (2017).

Our analysis sample includes dependents born from January 1985 to December 1986 — these cohorts turn 26 by the end of our data in 2012 and thus all coverage added under the mandate is included in our sample period. We find that dependents eligible for more coverage are more likely to enroll and are enrolled for a longer period of time once the mandate is in effect, in line with prior work on the dependent mandate. Dependent enrollment increases by 1.4 percentage points at the birth date cut-off, an increase of 7.4 percent over the enrollment rate for dependents born in December 1985. In addition, the enrollment duration increases by 12.3 days

(18.7 percent). Turning to parents, we find that parental job retention likelihood increases by 1 percentage point (1.8 percent) and job duration increases by 5.8 days (1.6 percent).

These results are consistent with the increased insurance eligibility for adult dependents making parents' current jobs more valuable, thus leading to greater job retention. The dependent coverage mandate is not employer-specific, meaning parents could in principle switch employers and re-enroll their dependents in employer-sponsored health insurance at their next employer. But despite its portability across employers, dependent mandate could still reduce job mobility if parents would have to switch providers under their potential future firm's insurance network (Sabety, 2023), if insurance generosity, coverage, or prices differ between their current and potential future firm, or if their outside option does not have dependent insurance (e.g., they are switching to Medicare or to a period of non-employment between employers).

Combining these estimates with the effects on dependent coverage, we estimate a 1 percent increase in the share of dependents covered is associated with an increase in the parental job retention rate by 0.20 percent. For job duration, a 1 percent increase in the share of dependents covered is associated with an increase of 0.11. Applying our results to the effect of the overall ACA dependent mandate, which was estimated to have increased dependent coverage by 30 percent, implies that about 400,000 parents were "job locked" by the mandate (Akosa Antwi et al., 2013).

Our estimates remain similar under a variety of robustness checks, including dropping controls, excluding weights, clustering on the running variable, using alternate bandwidths, and replacing our linear control function with a local linear specification. We assess potential threats to our identification assumption that factors other than coverage eligibility do not change at the discontinuity by conducting a placebo analysis on the 1983-1984 and 1995-1996 cohorts. These cohorts were either too old or too young to be eligible for the dependent mandate. Reassuringly, we find no effects on dependent enrollment or parental job retention in these placebo cohorts.[2]

[2] Note that while these cohorts were too old to qualify for the ACA dependent mandate, some may have been covered by state-level dependent mandates that extend past 26. For example, some states had exceptions for veterans or disabled dependents.

We then use heterogeneity analyses to explore mechanisms. We find evidence of greater job lock among parents who may have otherwise been more likely to leave their jobs: those eligible for retirement benefits, and those who do not provide coverage for their spouse or other children. We also find greater job lock for parents who may value coverage more: those with dependents with prior inpatient care, and those who are on fee-for-service (FFS), as opposed to health maintenance organization (HMO), plans pre-ACA. Finally, we find greater job lock in firms that offer a greater diversity of plan types. Taken together, these results demonstrate that job lock is stronger among parents who are more likely to be on the margin of a job exit, parents who may value their coverage more, and parents in firms with a wider ranger of insurance options.

2 Policy Context

2 .1 The Dependent Coverage Mandate

Under the dependent coverage mandate, private health insurers were required to extend coverage to adult children through the age of 26 (Cantor et al., 2011).[3] Prior to the mandate, most plans provided dependent coverage through age 19 if the dependent was not a full-time student or through age 23 if the dependent was a full-time student. In addition, some states had laws that extended coverage past age 23 for certain categories of dependents (e.g., full-time students or those claimed as dependents on their parents' tax returns). The state mandates did not apply to self-insured plans, which cover more than half of private sector workers with employer-sponsored health insurance and, as such, were limited in scope (Levine et al., 2011; Monheit et al., 2011; Akosa Antwi et al., 2013).

The ACA mandate applied to all insurance plans after September 23, 2010. Dependents must be born on or after January 1985, and therefore turn 26 on or after January 2011, to receive additional coverage under the ACA mandate. Plans could not charge different premiums or offer

[3]For more information on the dependent mandate, see: https://obamawhitehouse.archives.gov/sites/default/files/rss_viewer/qa_young_adults_may.pdf (accessed on May 22, 2022).

different benefit packages, and the premiums receive the same tax-favored status as those paid for other dependents. The dependent mandate was a highly salient and largely popular component of the ACA: over 70 percent of the public was aware of the dependent mandate within a month of enactment (Kaiser Family Foundation, 2010). The other major provisions of the ACA, including the establishment of healthcare exchanges and the coverage mandates for mid and large-sized firms, were implemented later in 2013 and 2014. As our data end in 2012, these policies should not be a source of confounding in our analyses.[4]

While the dependent mandate only requires plans to insure dependents through the month in which they turn 26, some plans choose to provide coverage through the end of the year in which they turn 26.[5] We refer to these plans as "birth month" vs. "end of year" plans, respectively.

The number of additional months of coverage implied by the ACA dependent mandate depended on the beneficiary's plan type and their birth month, as illustrated in gray in Figure 1.1a. In particular, we plot the number of additional coverage months in 2011-2012 by dependent birth month, separately for birth month plans and end of year plans. For dependents in birth month plans, the number of additional months increases linearly in birth month. For example, individuals born December 1985 are eligible for 12 months of coverage as they would lose coverage when they turn 26 in December 2011, whereas individuals born January 1986 are eligible for 13 months of coverage as they would lose coverage in January 2012.

In contrast, for those on end of year plans, the number of additional months is the same within a birth year cohort and then jumps discontinuously between the December 1985

[4]For a full timeline of the implementation of ACA provisions, see: https://www.ncbi.nlm.nih.gov/books/NBK241401/.

[5]Healthinsurance.org, an online consumer resource site, explains: "young adults can remain on a parent's health plan until age 26. Some plans will keep the young adult insured until the end of the plan year (which often corresponds to the calendar year) in which they turn 26, although others will drop them from the plan the month they turn 26." (Source: https://www.healthinsurance.org/faqs/under-the-aca-can-young-adults-still-remain-on-their-parents-health-plans-until-age-26/). As an example, Kaiser Permanente provides the following explanation in response to the question "Will I lose my coverage at age 26?": "if you're a dependent on your parent's plan, you may lose coverage under that plan either at the end of your birth month or end of the calendar year." (Source: https://continuecoverage.kaiserpermanente.org/losing-parents-plan/).

and January 1986 cohorts. Dependents born in January 1986 turn 26 in December 2012, and thus become eligible for 24 months of coverage, whereas dependents born one month earlier in December 1985 are eligible for only 12 months of coverage. These dependents should be otherwise similar, which motivates our use of a regression discontinuity design by birth month.

With both plan types in the sample, we would expect the discontinuity at January 1986 to be a weighted average of the 12 additional months for dependents on end of year plans and the one additional month for those on birth month plans. The blue points in Figure 1.1a shows an illustrative example of the average discontinuity under the assumption that half of dependents are on each type of plan. While we cannot directly observe whether a dependent is on a birth month or end of year plan, we find evidence of both types of plans in our data, as discussed in Section 4 .2.

3 Data

3 .1 Data Description

Our main source of data is the Truven Health MarketScan CCE Database ("MarketScan Data"), a large panel of employer-sponsored health insurance claims. The data combine detailed information on individual claims, monthly enrollment records, and basic demographic information. The data cover 2000 to 2012 and includes 143,969,922 enrollees, of which 69,227,012 are planholders (i.e., the employee) and 74,742,910 are dependents (i.e., their spouse and children). The data include employees between the age of 18 and 64. While the sample disproportionately covers the South, it has wide geographic coverage (Baker et al., 2014; Blewett et al., 2018).

The data were provided to MarketScan by 246 large employers and health insurers ("data contributors"). Most of these employers are Fortune 500 firms, and medium and small firms are relatively underrepresented in the data (Adamson et al., 2008). We limit our sample to data provided by employers (212 out of the 246 data contributors). Doing so ensures we can track employees over time as long as they remain with the same employer and do not drop health insurance altogether. Importantly, this means we can track employees across plans offered by the same employer (Adamson et al., 2008). This unique feature of our data allows us to use it as a source of information on job tenure.

Our sample is a monthly panel of enrollees — each observation represents an enrollee and enrollment month. For each individual, we observe an enrollee ID, which allows us to follow them over time, and a family ID, which allows us to link planholders with their covered dependents (spouses and children). Note that we can only track dependents while they remain covered by the same employee. For example, if a child disenrolls from one parent's plan and re-enrolls on another parent's plan, we would not be able to follow them.

We impose several additional sample restrictions. First, we limit the sample to plans that include at most one dependent born between January 1985 to December 1986. Second, to ensure that the relationship between the planholder and dependent is that of a parent-child, we require at least a 16-year age gap between the two. Third, we limit the sample to plans with planholders who are under 65 throughout the sample period, or those born after 1947. As our data do not include employees older than 65, we might otherwise confuse exits from the data with exits from one's employer. Fourth, we require that the planholder and dependent are first observed in the data prior to 2010 (the "pre-period"). This step ensures that we avoid endogenous selection into the sample due to enrollment incentives created by the dependent mandate.[6] Fifth, we require that dependents are enrolled for at least one month in the pre-ACA period while younger than 23, to avoid any issues of selection due to the pre-existing state-level mandates that provided coverage beyond 23. In robustness exercises, we show that requiring that dependents are observed under the age of 19, rather than 23, does not alter our main findings, although it does reduce sample size (and, as a result, the power to examine heterogeneous treatment effects). We also require that time-varying control variables (i.e., family size, marriage, inpatient care,

[6]Although the ACA mandate was officially implemented in 2011, some plans elected to start providing coverage earlier in 2010 to graduating college students, to avoid a summer coverage gap. While our sample cohorts are generally too old to be in college in 2010 (as they are 24-25), we exclude all data from 2010 from our analysis for this reason.

and full-time status) are observed prior to 2010 to avoid confusing changes in these variables with endogenous responses to the dependent mandate.

We then limit the sample to the subset of data contributors that participate continuously from 2008-2012. New data contributors are added to the MarketScan sample each year in January, as shown in Appendix Figure 1.A.2. Thus, this step ensures that we avoid selection into the sample by dependent birth date that could arise as a result.[7]

The key independent variable in our analyses is dependent birth month. Dependent birth date is not directly reported in the MarketScan data — instead, we back it out using the fact that enrollee age is reported on a monthly basis. Specifically, age is reported as of the 1st of the given enrollment month. Thus, an enrollee's birth month is the month before the one in which their age increases. In order to ensure we observe birth month for each dependent, it is necessary to limit the sample to plans in which dependents are enrolled for at least 12 months continuously in the pre-period. Imposing this final sample restriction leaves us with an analysis sample of 393,791 planholder-dependent pairs. Henceforth, we refer to the planholder as the "parent."

Our outcomes of interest measure whether and for how long the parent and dependent are covered by the parent's pre-ACA employer in the post-mandate period. Specifically, our outcomes are enrollment for at least one month ("enrollment likelihood") and total enrollment days ("enrollment duration") in 2011-2012. These outcomes are our measures of post-mandate insurance coverage for the dependent and job retention for their parent.

It is important to consider what we can measure with regard to dependent coverage. Because we require that all dependents are covered by their parent's plan in the pre-ACA period, our measure of "any enrollment" is in fact an indicator for whether the dependent is still enrolled (or re-enrolled) on any insurance plan provided by their parent's pre-mandate employer. Thus,

[7] Appendix Table 1.A.1 lists, for each birth cohort in our sample (January 1985-December 1986), the range of enrollment months during which we could conceivably observe them enrolled on their parent's plan while under the age of 23. The range starts in January 2000 because that is the first month of our MarketScan sample. Our goal is to avoid differential selection into the sample between December and January birth months. Adding new data contributors in January of each calendar year would result in new sets of dependents with January birth months (as compared to December birth months). Imposing this initial enrollment age restriction limits the sample to plan holders whose data contributors continuously participate in MarketScan from 2008 to 2012.

we do not count adult dependents who enroll in their parent's plan as a result of the ACA mandate but who were not previously covered by the same parent. In addition, we cannot observe coverage provided by that parent if they move to a different employer after 2010. Similarly, we do not observe coverage provided through other sources, such as the parent's spouse or the adult dependent's employer.

Summary Statistics

Table 1.1 presents summary statistics for our analysis sample, where each observation reflects a parent-child pair. We report means of our outcome variables and control variables for both the full sample (Column 1) as well as by dependent birth year (Columns 2-3). Of the 393,791 parents in our sample, 46 percent have dependent children born in 1985 and 54 percent have dependent children born in 1986.

Comparing dependents in the 1985 and 1986 birth cohorts, the share enrolled for at least one month during 2011-2012 increases from 0.14 to 0.26, or 86 percent. Similarly, there is a large increase in the total number of coverage days during 2011-2012, from 35.91 to 127.70, or 256 percent. These increases reflect the fact that the 1985 cohort is only eligible for coverage under the dependent mandate in 2011 (when they turn 26), whereas the 1986 cohort is eligible in both 2011 and 2012.

As for parents, those with dependents born in 1986 vs. 1985 are slightly more likely to remain with their pre-ACA employer for at least one month in 2011 to 2022 (3.7 percent increase). Similarly, total job days during 2011-2012 increases by 3.5 percent. The fact that parents' job retention is higher for the 1986 cohort provides initial evidence in favor of the "job lock" hypothesis.

Table 1.1 also reports means of our control variables for the 1985 and 1986 birth cohorts. All time-varying controls are measured with respect to the pre-period, before 2010. There is little difference across these cohorts in the following: female dependent (50 percent), female parent (40 percent), whether a spouse was added to the plan prior to 2010 (78 to 79 percent), number of dependent children added to the plan prior to 2010 (2.3 to 2.4 percent), and whether the dependent received inpatient care prior to 2010 (0.07 to 0.08 percent). As for parental birth month, dependents born in 1985 tend to have older parents than dependents born in 1986, as would be expected. Since younger parents will tend to retire later, increased job retention for those with dependents in the 1985 vs. 1985 cohort may reflect the effects of age, rather than job lock. This point emphasizes the importance of controlling for parental age in our analyses.

The last set of control variables measure the generosity and flexibility of the parent's pre-period insurance coverage options. The construction of these variables is described in Appendix 11 . The first is an indicator for whether the parent's pre-period plan is a health maintenance organization ("HMO"), which tend to provide less generous coverage than other common plans (e.g., a preferred provider organization, or "PPO") at lower rates. The second variable is an indicator for whether the parent's pre-period employer offers both HMO and fee-for-service ("FFS") plans. This measure is meant to capture the diversity of plan options offered by an employer, which, by giving employees more choice, should increase the value of health benefits. There is no difference in the means of these measures across dependent birth cohorts: 0.23 for HMO coverage and 0.74 to 0.75 for offering both types of plans.

3 .2 Insurance Dis-enrollment as a Proxy for Job Exit

In this subsection, we discuss and provide evidence on the validity of our measure of parental job retention. We proxy for job retention using an indicator for whether parents continue coverage from any plan offered by their pre-mandate employer. If a parent remains with the same employer but elects to forego health insurance coverage, then our proxy would incorrectly code them as having left their job.

To assess the importance of measurement error in our proxy measure, we use 2011-2013 data from the Panel Study for Income Dynamics (PSID) to look at how often employees forgo insurance but stay at their job. Appendix Section 11 .1 describes the sample construction and analysis in further detail. Using individuals with similar profiles as our sample who do not leave their job by 2013, we construct an indicator for whether the individual is no longer covered by their employer in 2013. Appendix Table 1.A.2 shows the tabulation of these indicators for heads and spouses in our sample. Only one percent of this sample drops their employer-sponsored insurance. Thus, it appears that dropping health insurance while remaining with the same employer is highly unusual for this sample. This suggests that it is reasonable to infer that the end of a planholder's coverage from their employer coincides with the end of their employment with them.

An additional concern with this proxy for parental job retention is related to the way it interacts with our sample restrictions. We restrict our sample to families with a dependent who we observe having coverage before the month in which they turn 23. This sample restriction requires families of older cohorts to stay with their employer for longer than families of younger cohorts – we require that dependents born in a given month in 1985 be observed before that same month in 2009, and, likewise, those born in a given month in 1986 to be observed before the same month in 2010. Through job churn and attrition over time, we should expect our job retention measure to be lower among older cohorts, leading to a mechanical difference between different cohorts that is unrelated to the mandate. It should not, however, generate a discontinuity between December 1985 and January 1986. We address this concern in several ways. First, we include a linear birth month control to account for any changes which are linear in birth month. Second, we repeat our sample restriction and analysis with a placebo cohort: dependents who were too old for the mandate to be relevant in 2011. We expect that attrition and job churn patterns to be similar among these parents, so if the estimates are due to the sample restriction, they should appear in these as well. We confirm that we only find a discontinuity among cohorts for whom the mandate is relevant.

4 When do Dependents Exit Parental Coverage?

In this section, we examine the age at which dependents exit parental coverage. Doing so allows us to provide additional evidence that the dependent mandate shifted patterns of coverage across birth cohorts. We also provide evidence on the prevalence of end-of-year vs. birth month plans in our sample.

4 .1 Effect of the Dependent Mandate on Age of Disenrollment

Appendix Figure 1.A.1 plots the age at which dependents exit coverage (under their parents) in the post-ACA period (2011-2012). In particular, for a given dependent, we calculate their age in months when they last appear on their parent's plan ("exit age"). For the 1983 and 1984 birth cohorts, who were too old to qualify for the dependent mandate, the most common exit age is 23 years and 1 month (15.3 percent and 11.7 percent of the cohorts, respectively). Since full-time students could stay enrolled until they turned 23 under the pre-ACA mandates, this pattern suggests dependents in our sample tend to attend college. Virtually no exits occur in the 26th birthday month (or afterwards). Smaller exit spikes appear in the 24th and 25th birthday months, reflecting state-sponsored mandates that extended coverage through these ages.

In contrast, the distributions for the 1985 and 1986 cohorts, who were younger than 26 when the mandate passed, are consistent with the policy increasing parental coverage. A spike emerges at exactly the 26th birthday month, and for the later cohort it becomes by far the most common exit month.[8]

4 .2 Evidence of Birth Month and End-of-Year Plans

As discussed in Section 2 , the number of additional months implied by the mandate depends on a dependent's birth date as well as whether they were on a birth month or end of year plan. For birth month plans, which cover a dependent until the month they turn 26, the number

[8]The spike at the 23rd birth month for the 1987 cohort reflects dependents who exited after college prior to the ACA (and never subsequently re-enrolled).

of additional months is linear in birth month. For end of year plans, which cover a dependent until the end of the year they turn 26, there is a discontinuity in additional months between dependents who turn 26 in 2011 or 2012. Thus in order to estimate our empirical design, we need a sufficiently large share of dependents to be on end-of-year plans.

While there is qualitative evidence from insurer manuals and policy documents that both of these plan types exist, we cannot directly observe this plan characteristic in our data. However, we can use the timing of exits from parental coverage to provide evidence on the prevalence of end-of-year plans. While dependents on birth month plans must exit on or before their birth month, dependents on end-of-year plans can remain enrolled until December. Figure 1.1b plots the distribution of exit months for dependents *not* born in December who disenroll in the year they turn 26. Over a quarter of these dependents disenroll in December, consistent with a sizable share of end-of-year plans. Note also that Appendix Figure 1.A.1 also provides evidence of birth month plans, as the spikes in the distribution show that many dependents exit at exactly 19, 23, and 26. Finally, as an additional check of this policy variation, in Section 6 .4, we construct a proxy for the whether an employer has a high share of end of year plans and confirm that job lock is stronger among those employers.

5 Empirical Method

Our empirical strategy is a regression discontinuity (RD) design in which dependent birth date serves as the running variable. We expect dependent coverage eligibility to jump discontinuously from December 1985 to January 1986. We focus on the 1985 and 1986 cohorts around this particular cut-off because our study period of 2011-2012 includes all of their new months of coverage eligibility. The 1985 cohort turns 26 in 2011 and the 1986 cohort turns 26 in 2012. Older cohorts did not qualify for coverage under the mandate, whereas younger cohorts turn 26 after our sample ends.

For a given family, we use i to refer to the parent and j to refer to the dependent. Define

B_j as the birth date (year-month) for dependent j and c as the cut-off value ($c =$ December 1985). We define the outcome variable, Y_{ij}, as a measure of either dependent enrollment or parental job retention. Then, we model Y_{ij} as follows:

$$Y_{ij} = \alpha + \beta 1[B_j > c] + 1[B_j > c] \cdot f(B_j - c) + f(B_j - c) + X_{ij}\gamma + \varepsilon_{ij}, \tag{1.1}$$

where $f(\cdot)$ is a control function based on dependent birth date. In our baseline regressions, $f(\cdot)$ is linear. This choice is motivated by the policy variation depicted in Figure 1.1a, which indicates that outside of the discontinuity from December 1985 to January 1986, the additional months of insurance coverage provided by the ACA should increase linearly by dependent birth date. The term $1[B_{jt} > c] \cdot f(B_j - c)$ allows the slope of the outcome variable in birth month to vary on either side of the cut-off c, should account for any linear trends by birth month that could arise from our sample restriction, as discussed in Section 3 .2 . X_{ij} is a set of controls: gender of the parent and dependent; parental birth date (year-month); number of dependents added to the parent's plan before 2010 (the pre-period); whether a spouse was ever added to the plan in the pre-period; whether the dependent ever received inpatient care in the pre-period; the share of end-of-year plans offered by the parent's pre-period employer; whether the parent's pre-period plan was an HMO; and whether the employer offered both HMO and FFS plans to their employees during the pre-period. We weight each observation using triangular weights, which decrease linearly in distance from the cut-off month and cluster standard errors at the individual-level.

The coefficient of interest is β, which measures the effect of additional dependent coverage eligibility on dependent enrollment and parental job retention outcomes in 2011-2012. A positive β on dependent enrollment would indicate that dependents to the right of the cut-off are more likely to be enrolled or are enrolled for longer during these years. Likewise, a positive β on parental job retention indicates that the parents of dependents to the right of the cut-off are more likely to remain at the pre-mandate employer or work there for longer.

We estimate a number of variations of our main specification to test the robustness of our results. These include dropping the triangular weights, assigning $f(\cdot)$ to be a local linear function, alternative bandwidth choices, excluding the control variables X_{ij}, and clustering standard errors by the running variable.

Lastly, we perform placebo tests by re-estimating Eq. 1.1 using two alternative cut-off dates: December 1983, for dependents too old to be eligible for additional coverage under the mandate, and December 1995, for dependents who were too young to be affected during our study period of 2011-2012.

Tests of Identification Assumptions

The RD design estimates causal effects by identifying treatment and control groups that are eligible for different amounts of dependent coverage, but are otherwise "seemingly identical." In our case, the treatment group consists of families with dependents born right after the start of 1986, while the control group consists of families with dependents born right before. The identification assumption is that absent the effects of the dependent mandate, our outcomes would evolve smoothly around the end-of-year cut-off in dependent birth date. Two common ways to test this assumption are to evaluate whether the density of the running variable is smooth through the cut-off value and to test whether observable characteristics evolve smoothly through the cut-off.

Examining the density of the running variable and the smoothness of observable characteristics sheds light on whether there may be manipulation or misreporting around the cut-off, and also probes for any other reasons for systematic differences that could affect our outcomes. This could occur, for example, if parents with a dependent born in December falsely report a January birth date to receive extra coverage for their child, resulting in more January birth months than December birth months.[9] Another possibility is that birth month is misreported. If a

[9]This particular scenario seems unlikely in our sample because we define birth month based on enrollment data collected prior to the ACA dependent mandate – thus, parents would have to anticipate the reform years in advance.

data provider had a practice of replacing all missing birth months with "January," for example, that would violate our identification assumption.

We assess this by first examining the smoothness of the distribution of dependent birth month around the cutoff. Appendix Figure 1.A.3 plots the density of dependents by birth month. The distribution appears to be smooth through the end of year. We fail to reject the null hypothesis of a smooth density around both cut-offs – the discontinuity estimate is -0.01803 with a p-value of 0.17.

Next, we examine whether the observable characteristics of dependents, parents, and employers evolve smoothly through the cut-off. For observable characteristics, we use the 8 control variables shown in Table 1.1: gender of the parent and dependent; whether the parent covered a spouse prior to 2010; birth date of the parent; the number of dependents added prior to 2010; and whether the dependent received any inpatient care prior to 2010.

Appendix Figure 1.A.4 plots the unadjusted means of these variables by dependent birth month. Visually, these graphs appear quite smooth through the birth date cut-offs. All are relatively flat except for parent's birth date, which is linearly increasing. This reflects the fact that younger children will tend to have younger parents.

We formally test for discontinuities in these characteristics by re-estimating our RD specification (Eq. 1.1) with the outcome variable Y_{ij} equal to the indicated control variable and omitting the vector of control variables. Estimates of β are reported in Table 1.2. The magnitudes of the 8 estimates are uniformly small and 7 of them are statistically insignificant. Thus, the combination of results in Appendix Figure 1.A.4 and Table 1.2 provide strong support for our causal design.

6 Results

6 .1 Main Results

We first estimate the effects of additional months of dependent coverage on dependent enrollment and parental job retention. For each of our outcomes, we present graphical evidence ("RD graphs") as well as estimates of β from Eq. 1.1. The RD graphs plot residualized means of our outcome variables that are adjust for our vector of control variables (X_{ij} in Eq. 1.1). One important reason we do so is to control for parental birth date, which increases linearly in the running variable (as shown in Appendix Figure 1.A.4). Because parental job retention decreases in parental age, the raw trend in parental job retention slopes upward in a way that is unrelated to variation in dependent coverage eligibility.

Figures 1.2a-1.2b display RD graphs for dependent enrollment likelihood and duration during 2011-2012. On each section of the graph, we include a linear fit line. In column (1) of Table 1.3, we report corresponding estimates of β along with their standard errors. We also report the mean of the outcome variables for dependents in the December 1985 (control) cohort, which we use to convert our estimates into percent changes.

We hypothesize that expanded dependent coverage should increase the likelihood a dependent is enrolled on their parent's plans as well as the duration of their enrollment. Accordingly, Figures 1.2a-1.2b reveal a discontinuous jump in both enrollment likelihood and duration for dependents at the birth date cut-off. The corresponding regression estimates, along with standard errors, are reported in Table 1.3. Enrollment likelihood increases 1.8 percentage points (9.0 percent of the December 1985 mean) and the duration of enrollment increases by 9.7 days at the cut-off (14.6 percent). Each of these estimates is statistically significant at the 1 percent level.

We then turn to the effects of expanded dependent coverage eligibility on parental job retention. Figures 1.2c-1.2d show RD graphs for parental job retention likelihood and duration during 2011-2012. Table 1.3 reports that the likelihood a parent retains their job increases by 1.0 percentage points (1.8 percent). Correspondingly, our measure of job duration increases by

5.8 days (1.6 percent). These estimates are statistically significant at the 1% level and 5% level, respectively.

6 .2 Robustness and Placebo Checks

We next investigate the robustness of our results to changes in our specification and sample. First, we re-estimate our main effects on dependent enrollment and parental job retention, making the following changes one-by-one: excluding controls; excluding regression weights; clustering the standard errors at the level of birth month, the running variable; employing different bandwidths around the cut-off months; and replacing our linear control function with a local linear specification. The results are reported in Appendix Table 1.A.6, which includes the baseline results in Column (1) for comparison. Reassuringly, there is very little change in the magnitude or precision of our estimates across the columns.

As an additional robustness check, we restrict our sample of dependents to those are enrolled in the pre-period while under the age of 19 rather than 23 (70% of dependents in our current sample). Recall that prior to the ACA mandate, dependent coverage was provided to all dependents through age 19, whereas only students could remain covered through age 23. While our analysis suggests that dependents in our data tended to exit on their 23rd birthday in the pre-period, adding this requirement provides a check that there are no confounding policies at the December/January cut-offs from age 20-22.

A drawback of this approach is that limiting the sample to dependents are observed in the pre-ACA period while under age 19 requires us to restrict the set of data contributors to those that continually provided data from from 2004 to 2012 (rather than 2008-2012). This step cuts our sample size from 393,791 to 266,855, as many data contributors joined after 2004 (see Appendix Figure 1.A.2). Column (7) in Appendix Table 1.A.6 re-creates our main results using the subset of dependents first observed prior to 19. Reassuringly, the point estimates are very similar (and in percent terms are nearly identical). In addition, each estimate is statistically significant at the 5 percent level.

Next, we conduct placebo exercises with different cohorts of dependents who were either too old or too young to be affected by the mandate. First, we use dependents born from January 1983 to December 1984 and set the cut-off value to be $c =$ December 1983. These cohorts were too old to be eligible for coverage under the dependent mandate when the ACA passed, but are similar in age to those in our main sample. This placebo test provides further evidence that there are no other factors besides dependent coverage eligibility that change discontinuously at the December vs. January cut-off, either due to non-linearities in characteristics by birth month or due to our sample selection criteria.

Appendix Figure 1.A.5 display the RD graphs for $c = December 1983$ and Appendix Table 1.A.7 reports the corresponding estimates and standard errors. The graphs appear smooth through the cut-offs and the estimated coefficients are small and imprecise.

Second, we construct a sample of dependents born from January 1995 to December 1996 and set the cut-off value to be $c =$December 1995. We restrict the sample to dependents with parents in the same birth cohorts as the main sample. The dependents in this sample are 10 years younger than those in our main sample and are covered under pre-existing, nationwide mandates in 2011-2012 (when they were 16-17). Thus, we again expect to find no changes in dependent enrollment or parental job retention at these placebo cut-offs.

Appendix Figure 1.A.6 display the RD graphs and Appendix Table 1.A.8 reports the corresponding estimates and standard errors. We find no evidence of discontinuous changes at the cut-offs, despite a much higher level of dependent enrollment than in the previous placebo sample.

6 .3 Scaling the Job Lock Response by the Change in Dependent Coverage

A unique advantage of our setting and data is that we can observe both parental and dependent outcomes. This allows us to scale the change in parental job retention to the change in dependent coverage in the sample. In particular we convert the effects on dependent coverage and parental job retention in Table 1.3 to percent changes relative to the average for the December 1985 cohort and then calculate the ratio between the percent change in job retention and insurance takeup.[10]

Scaling the two in this way provides a relative measure of how much parents value the additional insurance – among those who take up coverage, how many valued it enough to change their labor supply decisions to gain coverage? A large ratio between the two implies that the additional coverage was highly valued, while a smaller ratio implies that while parents valued the coverage enough to incur the cost to sign up their dependent, they did not value it enough for it to meaningfully change their labor supply decisions.

This is useful in two ways. First, we can use our estimates to extrapolate what the parental job retention effects would be of policies for which we only know the change in the dependent coverage rate. Additionally, we can use the ratios to make informative comparisons of labor supply responses across groups, after adjusting for differential take-up of dependent coverage across groups. We use these comparisons to shed light on potential mechanisms underlying the parental job responses in Section 6 .4 below.

For the full sample, the ratio of the percent change in job retention likelihood with respect to the percent change in dependent coverage likelihood (henceforth, the "job retention likelihood ratio") is 0.20, and the ratio of the percent change in job duration to the percent change in dependent coverage duration (henceforth, the "job retention duration ratio") is 0.11. Since the ACA dependent mandate was estimated to increase coverage by 30 percent, a back-of-the-envelope calculation implies that 400,000 parents were "job locked" by mandate (Akosa Antwi et al., 2013).[11] This calculation assumes that we can extrapolate our results nationally. However,

[10]We are, in essence, calculating an elasticity. However, we call it a "ratio" to highlight that it should not be interpreted as an elasticity of within-person responses, but rather of the relative sizes of the takeup and labor supply responses in the population.

[11]We calculate the number of affected parents, 9.7 million, using the SIPP and Census. We arrive at this number by calculating the share of adults aged 44-63 with children aged 19-25 in the 2008 wave of the SIPP, and then extrapolating using the total number of adults from the 2010 Census. The percentage point change in job retention, 1.8, divided by percent change in take-up, 9.0, is 0.20. Multiplying this by 30 implies that 6 percent of affected parents, or about 588,000, were "job locked."

the firms and employees in the Marketscan data may not be a fully representative sample of the overall affected population, as they are more likely to be large employers and disproportionately located in the South.

We also note that there may be other concerns about the external validity of the magnitudes of our estimates. On the one hand, the ACA dependent mandate was highly salient so our results may be an overestimate. On the other hand, our sample consists of relatively older parents and dependents, so it may be an underestimate of job lock responses for families with younger dependents who are be eligible for more years of insurance coverage. Nonetheless, our results are of policy relevance for two reasons. First, we are the first to show that dependent coverage expansions can lead to an increase in parental job lock. Second, our exploration of mechanisms in the following section is relevant for uncovering why job lock arises and which individuals are most sensitive to it.

6 .4 Heterogeneity Analysis and Mechanisms

Next, we compare our scaled estimates for different subsets of the data by characteristics of parents, dependents, and plans in order shed light on the mechanisms behind our results. In a simple model of job-to-job transitions, a reform that applies equally across employers would not be expected affect the likelihood of job transitions, and thus the dependent mandate should not result in job lock. However, we might expect job lock to arise if some employees are choosing between staying in the labor force or exiting (e.g., retiring), if there is heterogeneity in the types of plans employers offer and the extent to which employees value them, or if there are frictions associated with leaving one's health insurance plan or re-establishing care under a new plan.

We next discuss and test for these mechanisms by conducting heterogeneity analyses on subgroups. Figure 1.3 plots the retention ratios for each subgroup, and Appendix Figures 1.A.7 and 1.A.8 plot the separate effects in percent terms. Appendix Tables 1.A.3, 1.A.4, and 1.A.5 report the corresponding coefficients and standard errors.

First, we look for evidence that parents nearing retirement are more likely to be "job locked" by the mandate. Parents approaching retirement age may have a larger job lock response for two reasons. They are more likely to be on the margin of exiting the labor force, and thus may be more responsive to to job retention incentives. Additionally, their outside option is less likely to offer insurance, and even if they can insure themselves through retiree insurance or Medicare, they will not be able to obtain coverage for their dependents. We split parents by whether they are over or under 55, as individuals who retire at age 55 or older can withdraw from their 401(k) without penalty and thus it is a popular early retirement age.[12]

Dependents of parents eligible for early retirement are less likely to take up coverage, but the job retention effect is larger for these parents. This translates to a job retention ratio of 0.30 for parents over 55 compared to 0.13 for parents under 55; the job duration ratio is also somewhat higher for retirement-age parents than for younger parents. This implies that the dependent mandate is more likely to induce job lock among parents nearing retirement age.

Second, we hypothesize that parents who also provide coverage to their spouse or other children will be less responsive to a marginal change in an individual child's eligibility, as they may already be "job locked" by the other family members. These parents would face a greater cost of exiting to non-employment or re-establishing care at a new job. Thus, we estimate heterogeneous effects by whether the parent also provides coverage to their spouse or also provides coverage to their child.

We find that parents who cover their spouse or other children are more likely to take up dependent coverage, and the magnitude of the job retention effect is larger as well. However, once the two effects are scaled relative to each other, job retention ratios are smaller for parents who cover their spouse or other children versus those who do not (Figure 1.3). This example highlights the importance of scaling the labor supply effect by the take-up effect – comparing just the magnitudes of the labor supply effects alone would lead to the opposite conclusion. The magnitude of the job retention effect is larger for parents who cover a spouse or other dependents

[12] Indeed, unreported analysis of our MarketScan sample reveals 55 is the first age at which a significant share of employees leave their jobs.

simply because they are more likely to take up coverage. But the job lock they face is actually *smaller* – that is, the ACA dependent mandate did not distort their labor supply decisions as much as it did for parents who were not covering other family members.

While it is difficult to assess how much a parent or dependent "values" the additional coverage with our data, a reasonable assumption would be that the value of coverage, and therefore the extent of job lock, should be greater for parents of dependents in worse health. Thus, we consider heterogeneity by a proxy for dependent health: whether we observe the dependent receiving inpatient care in the pre-ACA period. We leverage the fact that we can observe claims and utilization in the Marketscan data to identify dependents who had at least one inpatient stay from 2000 to 2009. Figure 1.3 shows that parents of children with prior inpatient care have higher job retention ratios: the ratio for likelihood is 0.40 for these parents, compared to 0.19 for parents of children without prior inpatient care.

We would expect that families with access to more "valuable" employer-sponsored insurance should be more likely to be job locked. An employee may value their employer-sponsored insurance because of the generosity of the coverage or the flexibility in provider or plan choice. To measure generosity, we consider whether a family is on a health maintenance organization (HMO) plan or a fee-for-service (FFS) plan before the ACA. HMO plans limit coverage to doctors within their network, and typically have limited or no coverage out of network. In contrast, fee-for-service plans such as preferred provider organizations (PPO) are less restrictive. While dependent takeup is similar across the two types of plans, only FFS plans generate parental job lock (Table 1.A.3). Thus, the job retention ratios are higher for families who were previously on FFS plans (Figure 1.3).

One potential concern with looking at each individual family's plan type is that plans also differ in their premia and cost-sharing, which we cannot directly observe. This motivates our third, employer-level measure: the number of plans offered by the parent's employer (i.e., contributor). We expect that employees value having more choice in their insurance plan, and thus expect job lock to be stronger when their firm offers them more options. We split by employers by whether they offer only FFS or both HMO and FFS.[13] We find that insurance takeup is higher when there are more plans available (Table 1.A.5), perhaps because families are more likely to have access to a plan which fits their needs. Correspondingly, the parental labor supply response is stronger among these employers as well. The job retention ratios are higher among firms that offer both HMO and FFS plans. Taken together, the heterogeneity analyses on patient-level prior utilization, plan-level generosity, and plan-level as well as firm-level flexibility are consistent with job lock arising when families value the insurance at their employer more.

Finally, to confirm the policy variation underlying our RD analysis, we consider differences across firms that appear to offer a greater share of "end of year" or "birth month" plans. We do not directly observe the type of plan that families are enrolled in in our data. Instead, we construct a proxy for the prevalence of "end of year" plans provided by each employer, as discussed in Appendix Section 11 . We divide the sample into employers with an above-average and below-average share, where we expect that employers with an above-average share should have more dependents on "end of year" plans. Thus, there should be a larger discontinuity between December and January in terms of months of dependent coverage eligibility for employees of these firms. For job retention likelihood, Figure 1.3 shows that the ratio is higher for employers with an above-average end of year share. For job duration, the job lock responses are relatively close in magnitude. While parents are more likely to stay if more dependent coverage is available, conditional on staying the value of *the additional month* of dependent coverage does not appear to vary.

7 Conclusion

In this paper, we study the effect of increased coverage for adult dependents under the Affordable Care Act on parental "job lock." While prior research provides evidence of job lock due to own coverage, less is known about the effects of dependent coverage, despite the fact that it is a widely provided benefit. We compare dependent insurance take-up and parental job

[13] In our sample, all employers offer at least one type of FFS plan.

retention outcomes in families with adult children who, depending on whether they were born in January vs. December, gained access to different amounts of insurance coverage on average.

Our dataset is a large panel of employer-sponsored insurance claims and enrollment records. By linking together parents and their adult children, we can observe both dependent coverage and a proxy for parental job retention. This novel linkage is key to understanding the extent to which insurance coverage for one family member distorts job mobility for others. Scaling the job retention effect by dependent coverage take-up allows us to assess the degree to which labor supply is distorted by job lock, both in the overall sample and across different subgroups.

Leveraging the discontinuous increase in months of dependent coverage eligibility at the January vs. December cut-off, we first show that adult dependents are more likely to take up coverage when they are eligible for more months, and they also remain enrolled for longer. We then find that parents of dependents eligible for more coverage are more likely to remain with their employer, and remain for a longer period of time.

We combine the reduced form estimates to calculate the elasticity of parental job retention with respect to dependent coverage take-up, and find an average elasticity of 0.20. There is evidence of substantial heterogeneity: parents nearing retirement age, those who do not also cover their spouse's insurance, those with a dependent who is an only child, and those with a dependent in worse health all face more job lock from the additional dependent coverage. These scenarios correspond to cases in which a job exit would be more probable or dependent insurance is more valuable.

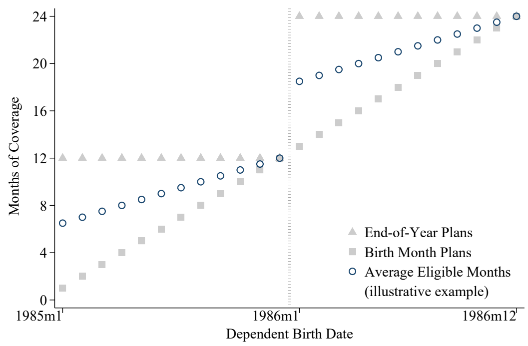
Our results suggest that the entire package of employer-sponsored health insurance, covering both employees and their family, plays a prominent role in determining labor supply. Thus, policies aimed at expanding dependent health insurance coverage, say through public insurance expansions or private insurance mandates, may have important within-family spillover effects on labor supply.

8 Acknowledgements

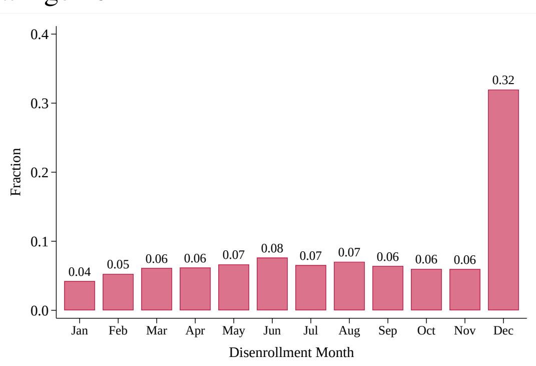
This chapter is coauthored with Katherine Meckel and Maggie Shi and is currently being prepared for submission for publication of the material. The dissertation author was a joint primary investigator of this paper.

9 Figures and Tables

(a) Potential Additional Coverage, by Plan Type



(b) Exit Timing for Dependents Who Disenroll at Age 26



(c) Dependent Age in Months at Exit

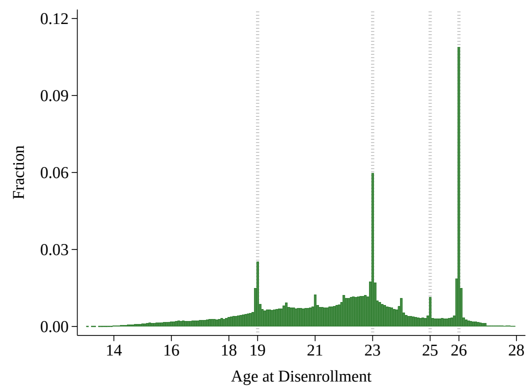


Figure 1.1. Variation in Additional Months of Coverage

Notes: Subfigure 1.1a shows the number of months of dependent coverage that cohorts born from January 1985-December 1986 became eligible for under the dependent mandate of the Affordable Care Act. "Birth Month Plans" are those that provide coverage through the month in which the dependent turns 26. "End of Year Plans" are those that provide coverage through December of the year in which the dependent turns 26. The "average eligible months" is constructed under the hypothetical assumption that half of dependents are on "Birth Month Plans" and half are on "End of Year Plans". The vertical line at December 1985 corresponds to the cut-off value used in our regression discontinuity design. We assume that dependents are not eligible for other sources of coverage past age 23 and that plan years start on January 1, as is the case for all plans in our data. Subfigure 1.1b displays the share of exits by calendar month for the subset of dependents born in 1985 and 1986 who exit during their 26th year (i.e., post-ACA) but *not* in their birthday month. The sample used to create this figure includes dependents from the 1985 and 1986 birth cohort who (1) are not born in December, (2) disenroll from their parent's plan at age 26, and (3) disenroll in a month other than their birth month. Subfigure 1.1c displays the distribution of dependents' age in months when they disenroll from coverage provided by their parents' pre-ACA employer. If dependents dis-enroll multiple times, we consider only the last disenrollment.

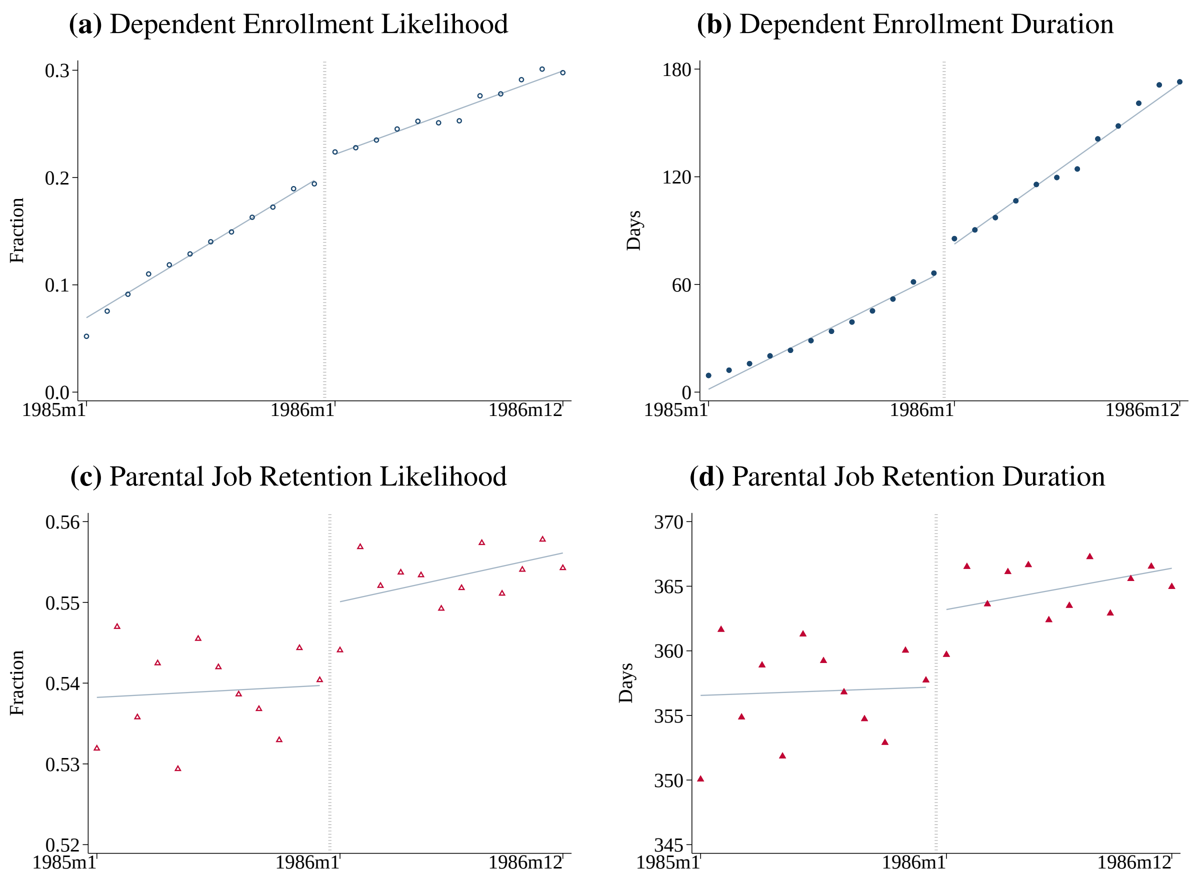


Figure 1.2. Effects of Dependent Coverage on Enrollment and Parental Job Retention

Notes: This figure displays regression-adjusted means of the dependent enrollment and parental job retention outcomes by dependent birth date. The outcome variable in Figure 1.2a is an indicator for whether a dependent is enrolled on a plan provided by their parent's pre-ACA employer at any point during 2011-2012. In Figure 1.2b, the outcome is total days of enrollment during 2011-2012. The outcome variable in Figure 1.2c is an indicator for whether the parent is employed by their pre-ACA employer at any point during 2011-2012. In Figure 1.2d, the outcome is total days of employment with that employer during 2011-2012. To calculate the regression-adjusted means, we regress these outcomes on our control variables (X_{ij} from Eq. 1.1), and then calculate the residual means by birth month. See the notes to Appendix Table 1.1 for more information on the data source, sample construction, and variable descriptions.

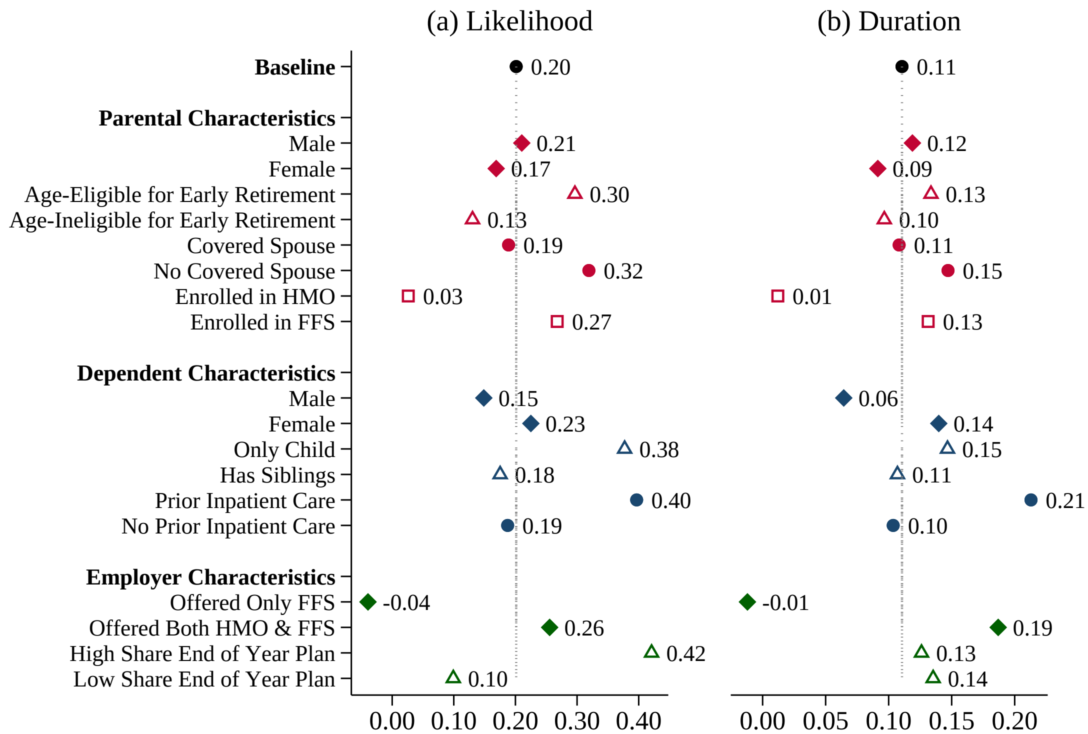


Figure 1.3. Ratio of Parental Job Retention Response to Dependent Enrollment Response

Notes: The figures above display our estimates of the ratio between the change in parental job retention and the change in dependent enrollment take-up. In particular, the left panel (a) depicts the percent change in parental job retention likelihood associated with a 1 percent increase in dependent enrollment likelihood. The right panel (b) depicts the percent change in parental job retention duration associated with a 1 percent increase in dependent enrollment duration. We report estimates for both the overall sample ("Baseline") and subsamples by characteristics of the dependent and parent. All characteristics are measured prior to 2010, in the pre-ACA period. See the notes to Table 1.1 for more information on the data source, sample construction, and variable definitions.

Table 1.1. Summary Statistics

	(1)	(2)	(3)
	Full Sample	By Dependent Birth Cohort	
		1985	1986
1) Dependent Enrollment, 2011-2012			
Likelihood	0.20	0.14	0.26
Duration (days)	85.40	35.91	127.70
2) Parental Job Retention, 2011-2012			
Likelihood	0.55	0.54	0.56
Duration (days)	361.02	354.30	366.77
3) Parental Characteristics			
Female	0.40	0.40	0.40
Parent's Birth Date	9/1957	4/1957	2/1958
Spousal Coverage	0.78	0.79	0.78
Enrolled in HMO	0.23	0.23	0.23
4) Dependent Characteristics			
Female	0.50	0.50	0.50
Number of Dependents	2.34	2.33	2.35
Prior Inpatient Care	0.07	0.08	0.07
5) Employer Characteristics			
Offered Both HMO and FFS	0.74	0.74	0.75
Observations	393791	181470	212321

Notes: The data source is the Truven Health MarketScan Commercial Claims and Encounters Database, a large panel of employer-sponsored health insurance claims and enrollment records. Our sample spans 2000-2012 and is restricted to a subset of employers that continuously provided data to MarketScan from 2008 to 2012. Each observation represents a dependent-parent pair. To be included in the sample, dependents must: (1) be born from January 1985 to December 1986; (2) be covered on their parent's plan for at least 12 months prior to 2010 (i.e., the "pre-ACA period"); and (3) be covered on their parent's plan while under the age of 23 in the pre-ACA period. Panel 1 and 2 provide summary statistics for our main outcome variables. "Dependent Enrollment" refers to coverage provided by the parent's pre-ACA employer. "Likelihood" indicates that the dependent was covered for at least one month during 2011-2012 ("post-ACA period"). "Duration" measures the total days of coverage in the post-ACA period. "Parental Job Retention" refers to whether (and for how many days) the parent remained with their pre-ACA employer during the post-ACA period. Panel 3 provides summary statistics for control variables used in our regression. "Spousal coverage" is an indicator for whether the planholder parent provided coverage to a spouse in the pre-ACA period. "Number of Dependents" indicates the total dependents covered by the planholder parent in the pre-ACA period. "Prior Inpatient Care" indicates whether the dependent received inpatient care in the pre-ACA period.

Table 1.2. Tests for Covariate Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Parental Characteristics				Dependent Characteristics			Employer Characteristics
	Female	Birth Date	Spousal Coverage	Enrolled in HMO	Female	Number of Dependents	Prior Inpatient Care	Offer Both HMO&FFS
RD estimate	-0.0035	0.0257	-0.0031	-0.0028	0.0009	0.0139*	-0.0019	0.0012
	(0.0034)	(0.3959)	(0.0028)	(0.0029)	(0.0034)	(0.0078)	(0.0018)	(0.0030)
Mean, left of cut-off	0.41	-28.66	0.79	0.23	0.50	2.36	0.07	0.74
Observations	393791	393791	393791	393791	393791	393791	393791	393791
Controls	No	No	No	No	No	No	No	No
Weighting scheme	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Bandwidth	±12 mo	±12 mo	±12 mo	±12 mo	±12 mo	±12 mo	±12 mo	±12 mo
Degree of polynomial	1	1	1	1	1	1	1	1

Notes: This table reports estimates of β from a version of Eq. 1.1 that excludes the vector of control variables (X_{ij}). Each column represents a separate regression in which one of the control variables, as indicated in the column headings, is the dependent variable Y_{ij}. "Parent's Birth Date" is enumerated in months relative to January 1960, so the average value of -29 indicates August 1957. Robust standard errors are reported in parentheses. "Mean, control cohort" is the average value of the outcome variable for dependents born in December 1985. See the notes to Table 1.1 and Table 1.3 for more information on the sample, variable definitions, and RD specification. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 1.3. Effects of Dependent Coverage on Enrollment and Parental Job Retention

	(1)
	RD Estimate
(a) Dependent Enrollment, 2011-2012	
(1) Likelihood	0.0175***
	(0.0028)
Mean, left of cut-off	0.19
(2) Duration (days)	9.6811***
	(1.1164)
Mean, left of cut-off	66.48
(b) Parental Job Retention, 2011-2012	
(1) Likelihood	0.0098***
	(0.0034)
Mean, left of cut-off	0.54
(2) Duration (days)	5.7603**
	(2.3791)
Mean, left of cut-off	357.63
Observations	393791
Controls	Yes
Weighting scheme	Triangular
Bandwidth	±12 mo
Degree of polynomial	1

Notes: The table above reports estimates of β from Eq. 1.1. Robust standard errors are reported in parentheses. Each coefficient and standard error pair are from a separate regression in which the outcome Y_{ij} is labeled in the first column. "Mean, control cohort" is the average value of the outcome variable for dependents born in December 1985. See the notes to Table 1.1 and Table 1.3 for more information on the sample, variable definitions, and RD specification. The outcome variable, Y_{ij} is reported in the first column. * p<0.10, ** p<0.05, *** p<0.01.

10 Appendix Figures and Tables

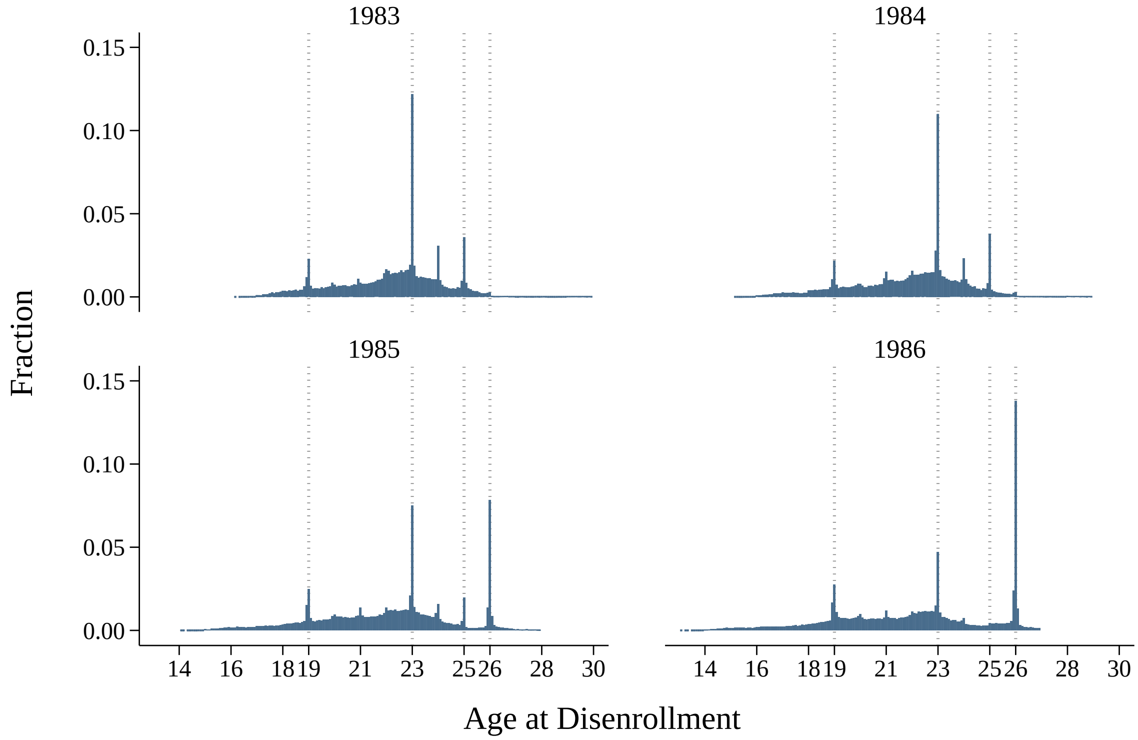


Figure 1.A.1. Distribution of Age in Months at Dis-enrollment by Birth Cohort

Notes: The figure displays the distribution of dependents' age in months when they disenroll from coverage provided by their parents' pre-ACA employer, separately by birth cohort. If dependents dis-enroll multiple times, we consider only the last disenrollment. The sample is restricted to dependents who are first covered on their parent's plan prior to the ACA (before 2010). The sample constructed similarly to that used in our main analysis sample with one exception. Because we include the 1983 and 1984 cohorts in this analysis, we limit data contributors to those that participate continuously from 2006 to 2012, rather than 2008-2012. Dependents born in 1983 or 1984 were more likely to disenroll from their family plan during the month they turn age 23 than those born in or after 1985: 12.7 percent for 1983 birth cohort, 11.3 percent for the 1984 cohort, whereas it is 8.4 percent for the 1985 cohort, and 4.9 percent for the 1986 cohort.

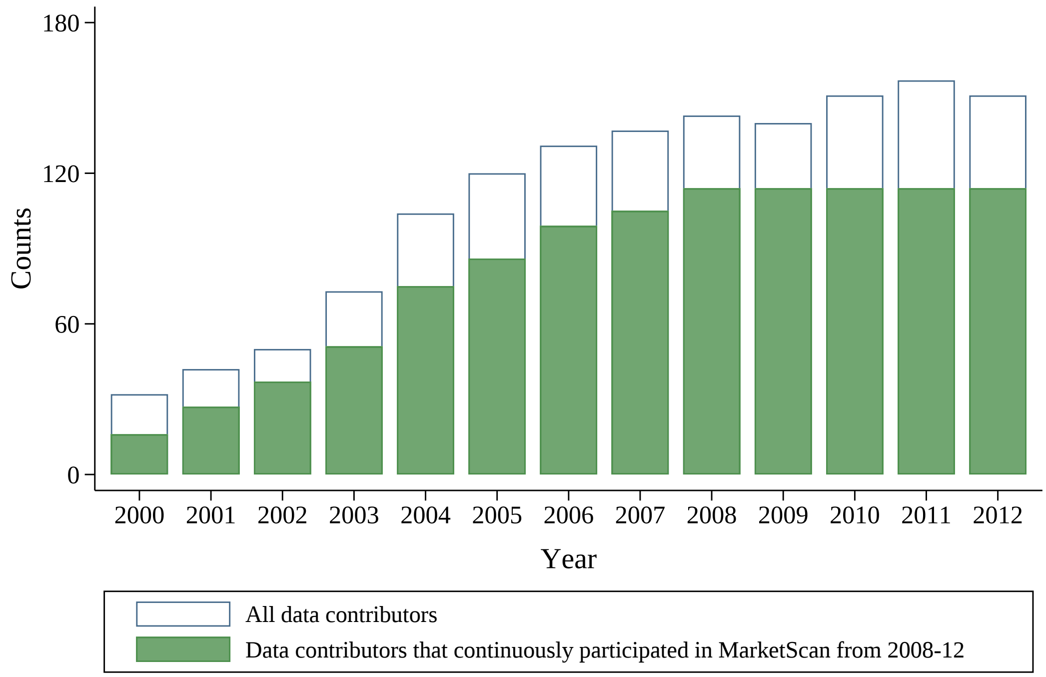


Figure 1.A.2. Employers that Contribute Data, Truven MarketScan Panel

Notes: Ths figure plots the number of employers who contribute in each year of the Truven Marketscan panel from 2000-2012. Of these employers, 114 continuously provided data from 2008-2012 and are thus included in our main sample.

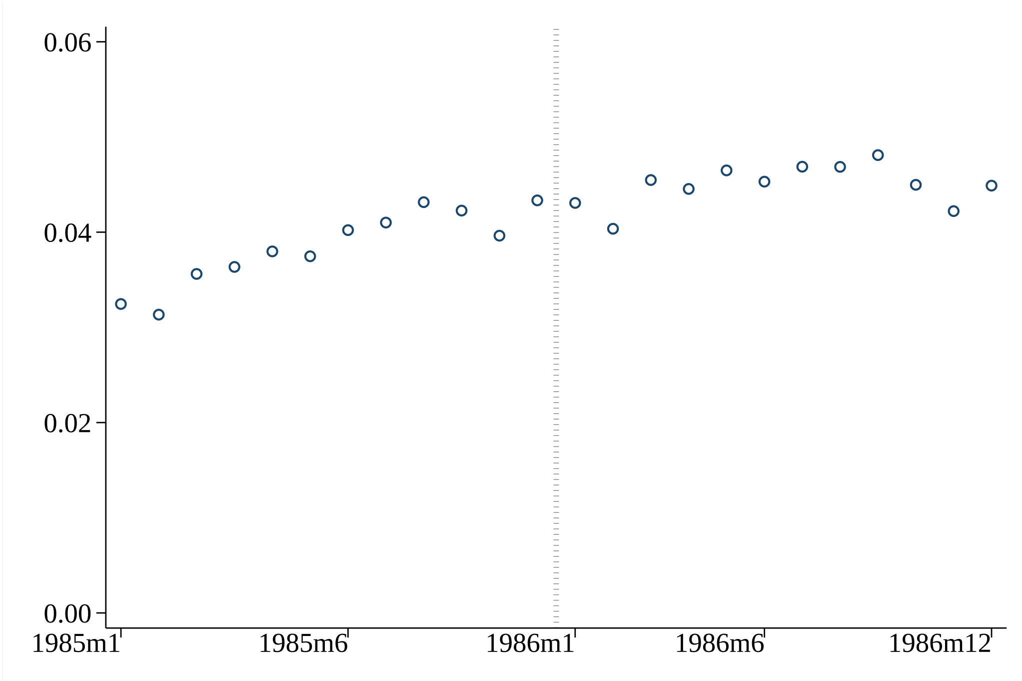


Figure 1.A.3. McCrary Density Test

Notes: This figure displays the density of dependents in our analysis sample by their birth month. We conduct a McCrary density test in Stata by using DCDensity.ado, written by Justin McCrary and Brian Kovak. The discontinuity estimates from the McCrary density test are -0.01803 (standard error=0.01191, p-value=0.16848). See the notes to Table 1.1 for more information on the data source and sample construction.

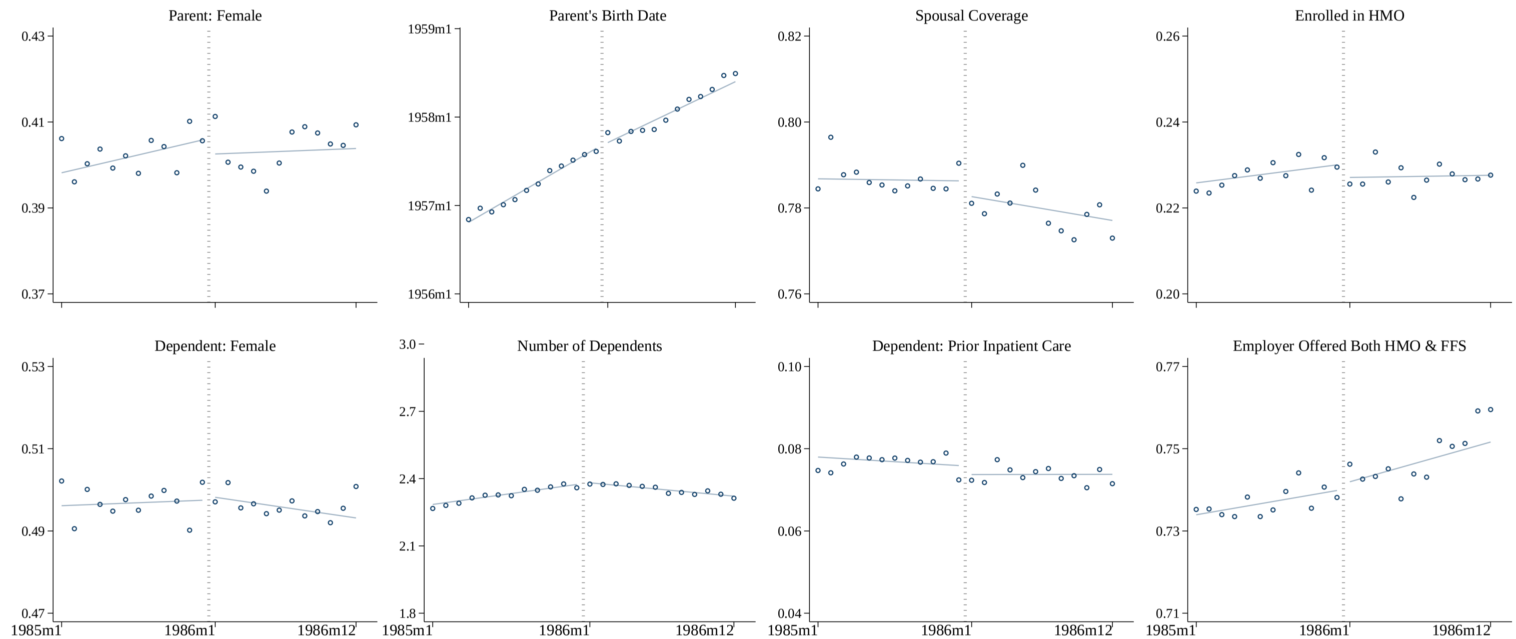


Figure 1.A.4. Characteristics by Birth Month

Notes: This figure displays unadjusted means of our control variables by dependent birth cohort. Table 1.2 reports corresponding regression discontinuity estimates. See the notes to Table 1.1 for more information on the data source, sample construction, and variable definitions.

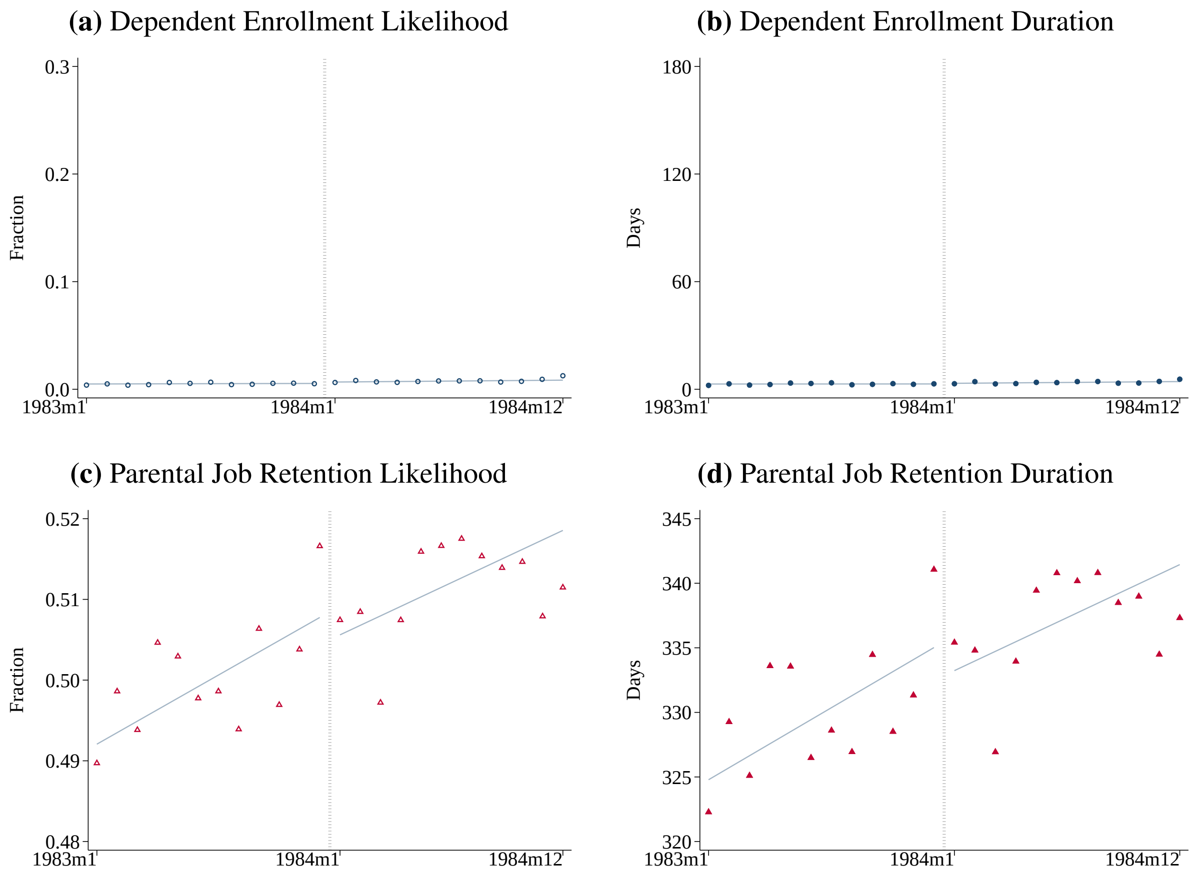


Figure 1.A.5. 1983-1984 Cohort Placebo Test

Notes: This figure displays regression-adjusted means of dependent enrollment outcomes by birth month. The sample consists of dependents born between January 1983 and December 1984. The RD cut-off value is December 1983. The outcome variable in Figure 1.A.5a is an indicator for whether a dependent is enrolled on a plan provided by their parent's pre-ACA employer at any point during 2011-2012. In Figure 1.A.5b, the outcome is total days of enrollment during 2011-2012. The outcome variable in Figure 1.A.5c is an indicator for whether the parent is employed by their pre-ACA employer at any point during 2011-2012. In Figure 1.A.5d, the outcome is total days of employment with that employer during 2011-2012. The corresponding RD estimates are reported in Appendix Table 1.A.7.

Figure 1.A.6. 1995-1996 Cohort Placebo Test

Notes: This figure displays regression-adjusted means of dependent enrollment outcomes by birth month. The sample consists of dependents born between January 1995 and December 1996. The RD cut-off value is December 1995. The outcome variable in Figure 1.A.6a is an indicator for whether a dependent is enrolled on a plan provided by their parent's pre-ACA employer at any point during 2011-2012. In Figure 1.A.6b, the outcome is total days of enrollment during 2011-2012. The outcome variable in Figure 1.A.6c is an indicator for whether the parent is employed by their pre-ACA employer at any point during 2011-2012. In Figure 1.A.6d, the outcome is total days of employment with that employer during 2011-2012. The corresponding RD estimates are reported in Appendix Table 1.A.8.

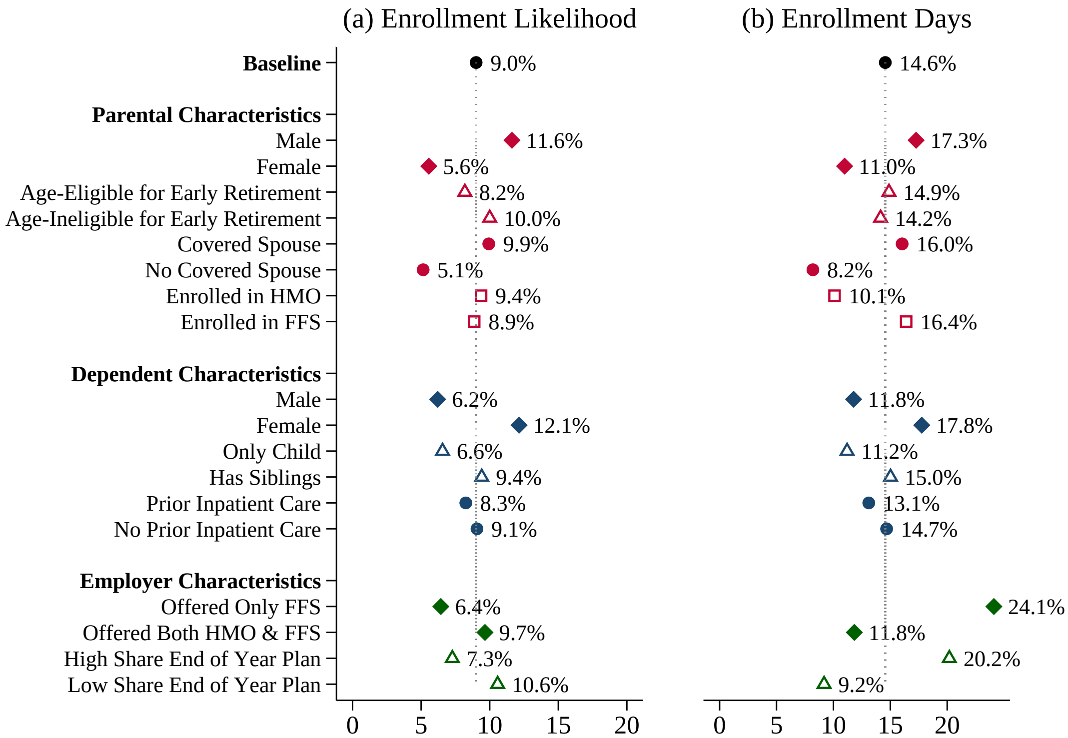


Figure 1.A.7. Percent Change from Baseline: Dependent Enrollment

Notes: The figures above display RD estimates (β from a version of Eq. 1.1), expressed as a percent of the control mean (i.e., the mean for cohort December 1985). The outcomes are dependent enrollment likelihood and length (days) during 2011-2012. We report effects for both the overall sample ("Baseline") and subsamples by characteristics of the dependent and parent. See the notes to Table 1.1 for more information on the data source, sample construction, and variable definitions.

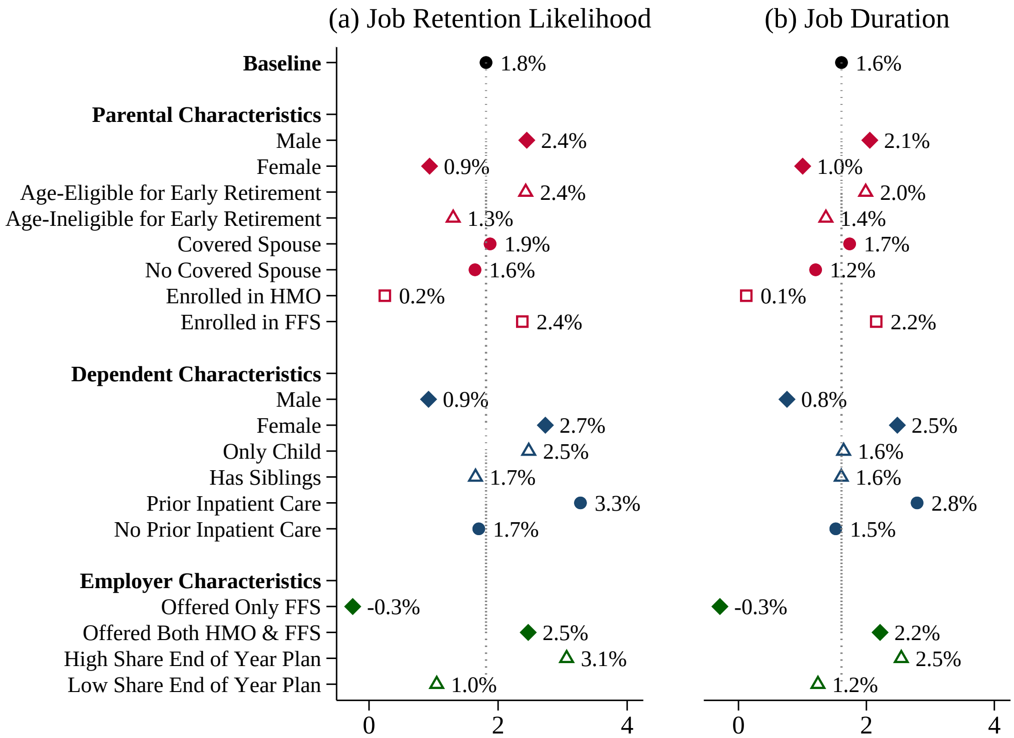


Figure 1.A.8. Percent Change from Baseline: Parental Job Retention

Notes: The figures above display RD estimates (β from a version of Eq. 1.1), expressed as a percent of the control mean (i.e., for parents of children born December 1985). The outcomes are parental job retention likelihood and length (days) during 2011-2012. We report effects for both the overall sample ("Baseline") and subsamples by characteristics of the dependent and parent. See the notes to Table 1.1 for more information on the data source, sample construction, and variable definitions.

Table 1.A.1. Time Range in Our Sample During which Dependent Cohorts are Under 23

Dependent Birth Date While Under 23	In-Sample Dates (Month/Year)
1/1985	1/2000-1/2008
2/1985	1/2000-2/2008
3/1985	1/2000-3/2008
4/1985	1/2000-4/2008
5/1985	1/2000-5/2008
6/1985	1/2000-6/2008
7/1985	1/2000-7/2008
8/1985	1/2000-8/2008
9/1985	1/2000-9/2008
10/1985	1/2000-10/2008
11/1985	1/2000-11/2008
12/1985	1/2000-12/2008
1/1986	1/2000-1/2009
2/1986	1/2000-2/2009
3/1986	1/2000-3/2009
4/1986	1/2000-4/2009
5/1986	1/2000-5/2009
6/1986	1/2000-6/2009
7/1986	1/2000-7/2009
8/1986	1/2000-8/2009
9/1986	1/2000-9/2009
10/1986	1/2000-10/2009
11/1986	1/2000-11/2009
12/1986	1/2000-12/2009

Notes: The table above shows, for each dependent birth month, the range of months during which they could be observed in our sample while under the age of 23. New data contributors are added to the MarketScan sample every January. These annual changes in contributors would result in additional under-23 dependents with January birth months (as compared to December birth months), as illustrated by the above table. To avoid selection into the sample by dependent birth date, we thus restrict our main sample to data contributors that continuously participate in MarketScan from 2008 to 2012.

Table 1.A.2. PSID: Share of Employees Who Remain Employed but Drop Insurance within 2 Years

	Drops Insurance		Total
	Yes	No	
N	84,420	8,001,158	8,008,578
Share	0.01	0.99	1.00

Notes: The source of data is the Panel Study of Income Dynamics, Waves 2011-2013. The sample is limited to heads of household born between 1948 and 1970, who are planholders of an employer-sponsored plan in 2011 and who remain at the same employer by 2013. "Drops Insurance by 2013 " is an indicator for whether the individual is no longer covered by their employer by 2013. Sample counts reflect the use of 2013 PSID cross-sectional individual-level weights. See Appendix Section 11 .1 for more information on sample and outcome construction.

Table 1.A.3. Heterogeneity by Parental Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Gender		Early Retirement		Spousal Coverage		Enrolled in	
		Male	Female	Age-Eligible	Age-Ineligible	Yes	No	HMO	FFS
(a) Dependent Enrollment, 2011-2012									
(1) Likelihood	0.0175***	0.0214***	0.0117***	0.0162***	0.0190***	0.0198***	0.0091	0.0228***	0.0160***
	(0.0028)	(0.0035)	(0.0045)	(0.0038)	(0.0040)	(0.0032)	(0.0056)	(0.0062)	(0.0031)
Mean, left of cut-off	0.19	0.18	0.21	0.20	0.19	0.20	0.18	0.24	0.18
(2) Duration (days)	0.0098***	0.0130***	0.0052	0.0122***	0.0076	0.0103***	0.0085	0.0015	0.0122***
	(0.0034)	(0.0044)	(0.0053)	(0.0047)	(0.0049)	(0.0038)	(0.0073)	(0.0069)	(0.0039)
Mean, left of cut-off	0.54	0.53	0.56	0.50	0.58	0.55	0.52	0.63	0.51
(b) Parental Job Retention, 2011-2012									
(1) Likelihood	9.6811***	10.8855***	7.8602***	10.0577***	9.2287***	10.9316***	4.9358**	8.6487***	9.9660***
	(1.1164)	(1.4225)	(1.7952)	(1.5551)	(1.6045)	(1.2859)	(2.2220)	(2.4941)	(1.2442)
Mean, left of cut-off	66.48	63.02	71.56	67.56	65.21	68.15	60.20	85.64	60.77
(2) Duration (days)	5.7603**	7.2349**	3.6696	6.5533**	5.3409	6.3029**	4.0824	0.5155	7.2840***
	(2.3791)	(3.0801)	(3.7307)	(3.2464)	(3.5025)	(2.6891)	(5.0995)	(4.9401)	(2.7140)
Mean, left of cut-off	357.63	352.31	365.42	329.47	390.79	362.72	338.45	422.86	338.20
Observations	393,791	234,968	158,823	211,907	181,884	308,284	85,507	89,616	304,175
Weights	Triangular								
Controls	Yes								
Bandwidth	$\pm$ 12 mo								
Degree of polynomial	1								

Notes: This table reports estimates of β from Eq. 1.1, separately for subsamples by parental characteristics. See the notes to Table 1.1 for more information on the data source, sample construction, and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

Table 1.A.4. Heterogeneity by Dependent Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Gender		Number of Dependents		Prior Inpatient Care	
		Male	Female	Only Child	Has Siblings	Yes	No
(a) Dependent Enrollment, 2011-2012							
(1) Likelihood	0.0175***	0.0126***	0.0225***	0.0112*	0.0189***	0.0189*	0.0174***
	(0.0028)	(0.0040)	(0.0039)	(0.0057)	(0.0032)	(0.0108)	(0.0029)
Mean, left of cut-off	0.19	0.20	0.19	0.17	0.20	0.23	0.19
(2) Duration (days)	0.0098***	0.0050	0.0147***	0.0130*	0.0090**	0.0185	0.0092***
	(0.0034)	(0.0048)	(0.0048)	(0.0073)	(0.0038)	(0.0122)	(0.0035)
Mean, left of cut-off	0.54	0.54	0.54	0.52	0.54	0.56	0.54
(b) Parental Job Retention, 2011-2012							
(1) Likelihood	9.6811***	8.3204***	11.0817***	6.4587***	10.3574***	10.6651**	9.5896***
	(1.1164)	(1.6042)	(1.5521)	(2.3005)	(1.2739)	(4.5653)	(1.1488)
Mean, left of cut-off	66.48	70.63	62.36	57.65	68.93	81.34	65.32
(2) Duration (days)	5.7603**	2.7139	8.8768***	5.6716	5.8044**	10.4637	5.4199**
	(2.3791)	(3.3559)	(3.3735)	(5.1293)	(2.6848)	(8.6158)	(2.4748)
Mean, left of cut-off	357.63	357.87	357.39	345.09	361.11	374.73	356.29
Observations	393,791	198,240	195,551	84,920	308,871	29,499	364,292
Weights				Triangular			
Controls				Yes			
Bandwidth				± 12 mo			
Degree of polynomial				1			

Notes: This table reports estimates of β from Eq. 1.1, separately for subsamples by dependent characteristics. See the notes to Table 1.1 for more information on the data source, sample construction, and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

Table 1.A.5. Heterogeneity by Employer Characteristics

	(1)	(2)	(3)	(4)	(5)
	All	Offered		Share of End-of-Year Plan	
		FFS Only	Both HMO & FFS	Above Average	Below Average
(a) Dependent Enrollment, 2011-2012					
(1) Likelihood	0.0175***	0.0109**	0.0197***	0.0142***	0.0212***
	(0.0028)	(0.0052)	(0.0033)	(0.0040)	(0.0040)
Mean, left of cut-off	0.19	0.17	0.20	0.19	0.20
(2) Duration (days)	0.0098***	-0.0013	0.0136***	0.0163***	0.0059
	(0.0034)	(0.0067)	(0.0039)	(0.0049)	(0.0047)
Mean, left of cut-off	0.54	0.51	0.55	0.53	0.56
(b) Parental Job Retention, 2011-2012					
(1) Likelihood	9.6811***	13.6299***	8.2904***	13.4764***	6.2901***
	(1.1164)	(2.2198)	(1.2910)	(1.6944)	(1.5148)
Mean, left of cut-off	66.48	56.54	70.01	66.74	68.50
(2) Duration (days)	5.7603**	-0.9670	8.1041***	8.9029***	4.6273
	(2.3791)	(4.6702)	(2.7629)	(3.4379)	(3.3442)
Mean, left of cut-off	357.63	333.75	366.10	349.74	372.40
Observations	393,791	101,246	292,545	187,985	197,568
Weights	Triangular				
Controls	Yes				
Bandwidth	± 12 mo				
Degree of polynomial	1				

Notes: This table reports estimates of β from Eq. 1.1, separately for subsamples of employers based on whether they likely have a high or low share of "end of year plans." We calculate the share of dependents born in January-March 1986 that dis-enroll during April-December 2012 (i.e., after they turn 26). Employers with higher shares will have more dependents on "end of year" plans. We divide employers into above average ($>= 0.05$) and below average (< 0.05) shares and construct subsamples of dependents and parents on these plans. See the notes to Table 1.1 for more information on the data source, sample construction, and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

Table 1.A.6. Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(a) Dependent Enrollment, 2011-2012								
(1) Likelihood	0.0175***	0.0171***	0.0138***	0.0175***	0.0208***	0.0197***	0.0158***	0.0150***
	(0.0028)	(0.0028)	(0.0026)	(0.0022)	(0.0034)	(0.0030)	(0.0032)	(0.0031)
(b) Duration (days)	9.6811***	9.4946***	9.7020***	9.6811***	10.7675***	10.1435***	6.9478***	11.4530***
	(1.1164)	(1.1212)	(1.0874)	(0.9597)	(1.3556)	(1.2144)	(1.2524)	(1.1994)
(b) Parental Job Retention, 2011-2012								
(1) Likelihood	0.0098***	0.0092***	0.0101***	0.0098***	0.0085**	0.0094**	0.0093**	0.0093**
	(0.0034)	(0.0034)	(0.0031)	(0.0028)	(0.0041)	(0.0037)	(0.0041)	(0.0038)
(b) Duration (days)	5.7603**	5.3384**	5.8330***	5.7603***	4.7457	5.4837**	6.0359**	5.3881**
	(2.3791)	(2.4009)	(2.2114)	(1.9401)	(2.8991)	(2.5947)	(2.8882)	(2.7035)
Observations	393,791	393,791	393,791	393,791	269,378	334,369	266,855	393,791
Sample	age < 23	age < 23	age < 23	age < 23	age < 23	age < 23	age < 19	age < 23
Controls	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Weighting Scheme	Triangular	Triangular	None	Triangular	Triangular	Triangular	Triangular	Triangular
Linear f()	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Local linear
Bandwidth	±12 mo	±12 mo	±12 mo	±12 mo	±8	±10	±12 mo	±12 mo
Std Error	Robust	Robust	Robust	Cluster(birth month)	Robust	Robust	Robust	Robust

Notes: This table examines the robustness of our estimates to modifications in Eq. 1.1. Column (1) reports our baseline estimates in Table 1.3, whereas Columns (2)-(8) report the results of the variations as the following: excluding the control variables; excluding the triangular weights; clustering the standard errors at the level of birth month (the running variable); employing different bandwidths around the cut-off months; restricting the main sample to dependents who were covered at least one month on their parent's plan in the pre-period prior to the age of 19; and replacing our linear control function with a local linear specification. Across all of these specifications, the RD estimates remain highly similar, providing strong evidence in favor of the robustness of our findings. See the notes to Table 1.1 and Table 1.3 for more information on the data source and baseline RD specification. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 1.A.7. Placebo Test: Dependents Born in 1983-1984

	(1)
	RD Estimate
(a) Dependent Enrollment, 2011-2012	
(1) Likelihood	0.0012*
	(0.0007)
Mean, left of cut-off	0.01
(2) Duration (days)	0.2512
	(0.3711)
Mean, left of cut-off	3.00
(b) Parental Job Retention, 2011-2012	
(1) Likelihood	-0.0037
	(0.0041)
Mean, left of cut-off	0.52
(2) Duration (days)	-2.8011
	(2.8925)
Mean, left of cut-off	341.12
Observations	265752
Controls	Yes
Weighting scheme	Triangular
Bandwidth	±12 mo
Degree of polynomial	1

Notes: In this table, we report estimates of β from RD specifications that are similar to our main estimating strategy but use the placebo sample of dependents born between January 1983 and December 1984. We modify Eq. 1.1 so that the cut-off is December 1983 (rather than December 1985). Dependents in the placebo sample are over 26 during 2011-2012 and therefore were ineligible for coverage on their parent's plan in most cases. The corresponding RD graphs are shown in Appendix Figure 1.A.5. Standard errors are adjusted for individual-level heteroskedasticity. "Mean, control cohort" is the average value of the outcome variable for dependents born in December 1983.* $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 1.A.8. Placebo Test: Dependents Born in 1995-1996

	(1)
	RD Estimate
(a) Dependent Enrollment, 2011-2012	
(1) Likelihood	0.0042
	(0.0032)
Mean, left of cut-off	0.57
(2) Duration (days)	3.3618
	(2.2786)
Mean, left of cut-off	375.37
(b) Parental Job Retention, 2011-2012	
(1) Likelihood	0.0031
	(0.0032)
Mean, left of cut-off	0.60
(2) Duration (days)	1.8820
	(2.2762)
Mean, left of cut-off	396.79
Observations	438435
Controls	Yes
Weighting scheme	Triangular
Bandwidth	±12 mo
Degree of polynomial	1

Notes: In this table, we report estimates of β from RD specifications that are similar to our main estimating strategy but use the placebo sample of dependents born between January 1995 and December 1996. We modify Eq. 1.1 so that the cut-off is December 1995, rather than December 1985. Dependents in the placebo sample were under 19 during 2011-2012 and therefore were eligible for parental coverage under the pre-ACA rules. The corresponding RD graphs are shown in Appendix Figure 1.A.6. Standard errors are adjusted for individual-level heteroskedasticity. "Mean, control cohort" is the average value of the outcome variable for dependents born in December 1995. See the notes to Table 1.1 and Table 1.3 for more information on the data source and baseline RD specification. * p<0.10, ** p<0.05, *** p<0.01.

11 Appendix: Measures of Employer Plan Offerings

Our data do not report parameters of each insurance plan offered by employers. Instead, we create proxies for the characteristics of plans offered to parents by their pre-mandate employer. First, we construct two different measures of the generosity and flexibility of insurance coverage. Our first measure of insurance generosity is an indicator for whether the parent's pre-period plan is an health maintenance organization (HMO) plan or a fee-for-service (FFS) plan. HMO plans limit coverage to doctors within their network, and typically have limited or no coverage out of network. In contrast, fee-for-service plans such as preferred provider organizations (PPO), which make up nearly all other plans in our data, are less restrictive. In particular, we use the PLANTYP variable in the Marketscan data, and assign plan types Comprehensive, EPO, POS, PPO, POS with capitation, CDHP, and HDH as FFS. For the 1.7% of individuals in the sample with a missing value, we assign them as 0 for the indicator for HMO coverage. The findings are robust to whether we classify them as HMO or FFS as the share of planholders with the missing plan type information is smooth around the cut-off. If parents are enrolled in multiple types of plans in the pre-period, we use their earliest plan.

One potential concern with measuring generosity or flexibility through plan characteristics is that plans also differ in their premiums, which we cannot observe. This motivates our second measure: an indicator for employers offering both HMO and FFS plans during the pre-period. In contrast to the previous measure, which was at the individual-level, this measure is constructed at the employer-level. In particular, we calculate the annual number of plan holders who maintained their plans for 12 months by employer between 2000 and 2009. We also count the number of plan holders enrolled in HMOs each year. Using these two numbers, we calculate the average share of annual HMO enrollees in a given employer. Plan holders with missing plan type information in a given year are also included in the denominator when calculating the share of HMO enrollees. Employers with a zero annual share of HMO plans are categorized as those who did not offer any HMO plans during the pre-ACA period.

Lastly, we create a proxy for the share of plans offered by each employer that provide "end of year" coverage to dependents. Our proxy is defined as the share of dependents born in January-March who we still observe as being enrolled *past* March of the year they turn 26. We expect that among "birth month" plans this share should be 0, and for "end of year" plans it should be close to 1. Most employers have a share that is far from both 0 and 1, which suggests that they offer a mix of plans with "birth month" and "end of year" policies. We divide the sample into employers with an above-average and below-average share, where we expect that employers with an above-average share should have more dependents on "end of year" plans.

11 .1 PSID

The PSID is a longitudinal survey with information on both employment and health insurance. We use survey years 2011 and 2013 because it approximately overlaps with our sample and includes insurance information. The PSID is administered every other year during this time period, so our sample combines 3 waves. Observation counts reflect sampling weights provided by the PSID. We then limit the sample to heads of households that participated in the survey in 2011 and 2013 – doing so allows us to observe their employment and health insurance outcomes in both years. We then require that individuals are born from 1948 to 1970, the range of birth cohorts of primary beneficiaries in our MarketScan sample, and that they are observed to have a dependent in 2011. We keep individuals who are employed at the same employer in both 2011 and 2013 and who served as the planholder of an employer-sponsored plan in the 2011.

Our outcome is an indicator for whether the individual is no longer covered by their employer by 2013. Specifically, we code this as either: 1) no one in the household is covered by health insurance (H61D3), or 2) the individual is not covered by employer-sponsored insurance (H61E), or 3) the individual is covered by employer-sponsored insurance but they are no longer the planholder (H61F).

Chapter 2

Can Redrawing Boundaries Save Lives? Evidence from a Reform of the Kidney Allocation System

1 Introduction

In 2019, chronic kidney disease afflicted one in seven Americans and ranked as the eighth-leading cause of death in the United States (Xu et al., 2021). Among those with kidney failure, treatment options include dialysis, which serves as disease management, or a kidney transplant, which can provide a full recovery. Demand for donated kidneys is substantial – in recent years, over 100,000 Americans were on the centralized transplant waitlist for kidneys from deceased donors, who represent the major source of donated kidneys.[1]

However, evidence suggests that there is room for improvement in how donated kidneys are allocated. In 2019, 4,163 donated kidneys intended for transplant were discarded, and 46 percent of them were not used because they became unviable before they could be successfully matched to a recipient. In the same year, 7,703 candidates either died or became too sick for a transplant while waiting for an offer. Besides saving lives, improving the efficacy of use of donated kidneys could generate important savings for Medicare, as patients with permanent kidney failure constituted over 6 percent of total Medicare spending and their program eligibility

[1]Kidneys from deceased donors account for 77 percent of all kidney transplants in 2021 (Lentine et al., 2023).

expires three years after a kidney transplant (Centers for Medicare & Medicaid Services, 2022; United States Renal Data System, 2022).

The centralized kidney allocation system in the U.S. prioritizes transplant candidates based on location relative to the donor hospital that recovered the donated kidney, with some minor exceptions. Prior to March 15, 2021, kidneys were first allocated to candidates registered at transplant centers in the same county group ("service area") as a donor hospital. Previous studies document that this system is associated with substantial geographical disparities in donated organ usage and transplant access across service areas (Adler et al., 2016; King et al., 2019; Zhou et al., 2018). However, there has been limited causal evidence on the role of geographic boundaries in the allocation system.

Starting on March 15, 2021, the county-group service areas were replaced with circles drawn around each donor hospital with a radius of 250 nautical miles. By leveraging temporal and spatial variation associated with the reform, this study examines the effects of this policy on allocative efficiency and equity outcomes and provides evidence on the role of geography in organ allocation. While the reform was expected to address disparities in kidney access across the country, this improvement could be undermined if the reform led to behavioral changes, such as changes in organ acceptance behavior, organ donation decisions, and newly waitlist enrollment choices.

To examine this, I use the administrative data from the Scientific Registry of Transplant Recipients (SRTR) that include detailed information on all donated organs, donors, and transplant candidates. I construct a sample consisting of all donated kidneys entering the allocation system within 28 weeks of the reform. Using these data, I estimate a regression discontinuity design that leverages the sharp timing of the policy change with respect to the date each kidney was recovered for transplant. The identifying assumption is that factors other than the policy change evolve smoothly through the cut-off date when kidneys enter the allocation system. In support of this assumption, I find no evidence of discontinuous changes in the density of donated kidneys or donor characteristics around the cut-off.

I first show that kidneys recovered for transplant after the policy change are substantially less likely to be allocated within the pre-reform service areas. In particular, the likelihood of kidneys being matched successfully with transplant recipients within these county groups decreases by 23.4 percentage points (43 percent). I also find evidence that the policy change increases allocative efficiency. The likelihood of kidneys being discarded decreases by 3.8 percentage points (17 percent), driven by a 3.3 percentage point (26 percent) reduction in discards due to waiting time. I find that the additional kidneys tend to be of lower quality and are more likely to be recovered in major metropolitan areas and near transplant centers that are predicted to experience a decrease in access to kidneys after the reform.

Lastly, I assess whether the reform changes the composition of kidney recipients, including their location relative to the donor hospital. As expected, given the increase in the size of the service areas, I find evidence that the recipients are located further away from the donor hospitals and kidneys spend more time in transport after the reform. In addition, the results suggest that the reform increased equity – candidates living in counties with higher marginalized populations are more likely to receive kidney transplants after the reform.

Next, I estimate a difference-in-differences model estimated with a monthly panel of transplant centers. While all transplant candidates are subject to the reform, the extent of its impact varies across transplant centers depending on changes in available kidneys. Importantly, the administrative data include the exact locations of all transplant centers and donor hospitals, allowing me to assign each to their pre- and post-reform service areas. Using the pre-reform data, I calculate predicted changes in access to kidneys by transplant center and use this as a proxy for treatment intensity to examine the effects on mortality of transplant candidates.

I find that the number of deaths on the waitlist decreases by 1.7 percent after the reform for a unit increase in predicted kidney access. I show that the decrease in pre-transplant deaths is not driven by changes in living donor transplants or changes in which transplant-center waitlist candidates join but by an increase in deceased-donor kidney transplants. I find no changes in the number of transplant recipients experiencing adverse health outcomes within 12 months of transplant before and after the reform. It is reassuring that increases in predicted kidney access cause important improvement in health outcomes of transplant candidates, as expected. Based on the estimates from the main findings, a back-of-envelope calculation suggests that the reform increased the number of deceased donor kidney recipients by 816 annually and $119 million from taxpayers otherwise spent on subsidizing dialysis treatment.

This paper makes several contributions to the literature. First, to my knowledge, this is the first work to provide causal estimates for the role of geographic boundaries in the allocation system. Findings of this paper contribute to existing studies showing a strong association between the geographic boundaries of service areas and disparities in access to organ transplants among transplant candidates (Adler et al., 2016; Goldberg et al., 2015; Cron et al., 2022). Most closely related to my work, OPTN Report (2022) and Rausch et al. (2022) provide descriptive evidence but show conflicting findings on the use of kidneys associated with the reform.[2] Findings based on causal frameworks allow me to evaluate the policy while avoiding issues such as time-varying confounding factors.

Second, this paper is related to previous studies on the design of the organ allocation system. Many of these papers are part of mechanism design literature: waitlist design for deceased donor kidney transplants (Kessler and Roth, 2012; Agarwal et al., 2020) and kidney-exchange program for living donor kidney transplants (Roth et al., 2004; Agarwal et al., 2019). Other papers on organ allocation focus on determinants of demand for deceased donor kidneys (Dickert-Conlin et al., 2019, Forthcoming).[3] In particular, Choi (2021) examines the impact of increasing waitlist priorities of transplant candidates with higher medical urgency on access to deceased donor kidney transplants and post-transplant health outcomes. My paper provides an important contribution to our understanding of policies that can improve both the efficiency and

[2] OPTN Report (2022) compares the discard rates between kidneys recovered 1-12 months before the reform and those recovered 0-11 months after the reform and reports that the discard rate increased from 22 to 25 percent. Rausch et al. (2022) compares the discard rate between kidneys recovered 3.5 months before and those recovered 3.5 months after the reform and documents that the discard rate decreased from 24 to 22 percent.

[3] More broadly other papers consider that affects other aspects of organ donation in the U.S.: determinants of supply in laboratory experiments (Kessler and Roth, 2014a,b) as well as in quasi-experimental settings (Dickert-Conlin et al., 2011; Li et al., 2013; Dickert-Conlin et al., Forthcoming).

equity of the allocation system.

Third, this paper relates to the literature on how to allocate goods and services involving perishable items or substantial travel costs, including studies that explore variation arising from the definition of a local catchment area to decide which households are eligible, such as in the context of school assignment in public education (Cullen et al., 2005; Angrist et al., 2022), health care providers (Garthwaite et al., 2018; Dillender, 2022; Agha et al., 2023; Cullen et al., 2023), and food assistance programs (Marcus and Yewell, 2022; Prendergast, 2022). This paper contributes to the literature by examining these questions in the context of kidney transplants, where the stakes of preventing inefficient allocation are particularly high, given that a single kidney can save the life of a transplant candidate.

The rest of this paper is structured as follows. Section 2 provides a brief overview of the institutional setting. Section 3 describes the data. Section 4 lays out my two empirical methods. Section 5 presents my analysis of main outcomes and examines heterogeneous treatment effects. Section 6 concludes.

2 Institutional Background

2 .1 Deceased Donor Kidneys

Organs may be procured for transplant from either living or deceased individuals ("donors"). While those from living donors are generally given to relatives or spouses[4], deceased donor organs enter a centralized allocation system and are matched with an anonymous recipient. This study focuses exclusively on organs from deceased donors, which account for 77 percent of all transplants in the U.S. in 2021 (Lentine et al., 2023).

Individuals can elect to have their organs donated after death by registering on their state's donor registry via the Department of Motor Vehicles, enrolling on the national Donate

[4]Between January 2010 and June 2015, 3.1 percent of all living kidney donations were contributed by 'non-directed donors' who generously donated their kidneys to strangers (Organ Procurement and Transplantation Network, 2015).

Life registry online, and including organ donation preferences in their will.[5] In addition, a deceased patient can be put into the donor registry if the next of kin agrees to donate the organs.

When an organ donor is declared deceased in a hospital (henceforth, "donor hospital"), physicians first conduct a medical evaluation to determine whether the donated organs are suitable for transplant.[6] Multiple organs can be recovered from a single deceased donor and each organ type has its own allocation system. This study focuses on transplants using kidneys, which are the most common deceased donor organ transplant, accounting for 19,636 of 36,421 (54 percent) transplant cases in 2022 (Organ Procurement and Transplantation Network, 2023).

2 .2 Transplant Candidates

Patients with serious renal disease may be recommended for a transplant by their physicians. End-stage renal disease (ESRD) is characterized by advanced-stage, chronic kidney failure that requires regular dialysis or a kidney transplant for patient survival (Centers for Disease Control and Prevention, 2023).

Compared to dialysis, opting for a kidney transplant offers higher life quality and improved health outcomes as well as generates cost savings for patients. Quality of life for dialysis patients increases 44 percent after a kidney transplant (Whiting, 2000), while the annual cost of dialysis and related treatments is $121,000 per patient (Held et al., 2016). To address the high medical costs linked with ESRD treatment, Medicare eligibility is extended to ESRD patients under 65.[7]

To register their name on the waitlist for a kidney transplant, candidates must obtain

[5] Age and definition of eligible death may vary by state and local laws. For instance, individuals below 18 cannot be a registered organ donor upon death nor a living donor unless state or local law allows them to do so.

[6] Appendix Figure 2.B.1a is a map of the locations of all donor hospitals for kidneys from 2019-2022.

[7] Medicare coverage for individuals with ESRD who are under 65 starts either 1) after the third month of dialysis treatment or 2) upon admission to a Medicare-certified hospital for a kidney transplant or necessary pre-transplant healthcare, as long as the transplant occurs within the following two months. For ESRD patients with group health plans from their own or spouses' current employer, Medicare serves as a "secondary payer" that only covers costs that the group health plan does not cover due to coverage limits. After the 30th month of eligibility, Medicare becomes the primary payer of benefits.

a referral and then select a transplant center.[8] Once a potential transplant candidate submits their application to a transplant center, a patient assessment is conducted. This assessment involves gathering relevant information for the transplant process. For instance, Estimated post transplant survival (EPTS) score is a measure of the expected post-transplant survival of candidates calculated based on the following four factors: candidate's age, duration on dialysis in years, current diabetes status, and prior organ transplant.

Once a candidate is registered in the system, they may continue to receive dialysis or any other necessary treatments they received prior to joining the waitlist. Transplant candidates have the option to register their name at different transplant centers, a practice known as multi-listing, although this is uncommon.[9] However, it is important to note that registering for more than two transplant centers within the same service area is not permitted.

Patients will then wait for a suitable transplant offer to become available. Upon receiving a transplant, candidates are recommended to attend follow-up visits and adhere to the prescribed immunosuppressant medications to ensure transplant success.

2 .3 Allocation System for Deceased Donor Kidneys

There are no other channels for obtaining deceased donor organs outside of the allocation system. Two major pieces of legislation – the Uniform Anatomical Gift Act of 1968 (UAGA) and the National Organ Transplantation Act of 1984 (NOTA) – created much of the regulations governing the organ allocation system that exist in the United States today. The UAGA specifies the circumstances through which an individual can become an organ donor, either through their own choices or post-mortem by their relatives. The NOTA of 1984 prohibited commercial transactions of human organs nationwide and established the Organ Procurement and Transplantation Network (OPTN). OPTN administers organ recovery, maintains a national

[8] Appendix Figure 2.B.1b is a map of transplant centers in the U.S.

[9] This occurs among 4 percent of candidates per transplant center (Ardekani and Orlowski, 2010). It is important to note that registering for more than two transplant centers within the same service area, defined in the next section, is not permitted.

organ matching registry, and develops organ allocation policies.

In addition, the NOTA of 1984 introduced the concept of a "service area," which is a geographical boundary within which donor hospitals are matched to transplant centers and later referred to as donor service area (DSA). DSAs consist of sets of counties defined to ensure an adequate supply of organs to registered candidates while limiting the distance organs must travel. There were 57 DSAs across the nation in 2022. Figure 2.1 shows a map of DSA in the U.S.

If the donor's organ is eligible for transplant, the donor hospital notifies the local Organ Procurement Organization (OPO). The OPO oversees the allocation of the organs recovered within the DSA. Next, the donated kidney is evaluated for quality based on donor health and demographic information. The system calculates a Kidney Donor Profile Index (KDPI) score calculated based on the following ten donor characteristics to evaluate the expected life span of kidneys: age, height, weight, ethnicity, hypertension history, diabetes, cerebrovascular accident death, serum creatinine, Hepatitis C virus, and circulatory death. A higher index means a kidney has a lower expected likelihood of graft failure compared to other kidneys recovered in the last year. The allocation system classifies them into four subgroups: (1) 0–20%, (b) 21–34%, (c) 35–85%, and (d) 86–100%.

Finally, a computer system called DonorNet generates an ordered list of transplant candidates – candidates are restricted to those with compatible blood types.[10] Candidates are also assessed on the similarity of tissue typing with a prospective donor based on HLA-ABDR typing. Fewer type mismatches are linked to a decreased likelihood of triggering an immune response and better post-transplant outcomes, including lower chances of graft rejection and graft failure (Held et al., 1994; Opelz and Döhler, 2012).[11] Except for cases in which candidates have the same tissue typing as the donor[12], priority is given to those who are registered at a transplant

[10]Kidneys from donors with blood types of AB, B, and O are offered to transplant candidates with the same blood types. Exceptions include 1) kidneys with blood types O or B can be transplanted to candidates with different blood types for offers with zero HLA mismatch and 2) kidneys with blood type A are offered for transplant candidates with blood type AB.

[11]The number of mismatches ranges from zero ("zero mismatch") to six, and a lower number indicates that donors and transplant recipients share similar tissue type.

[12]Candidates with "zero mismatch" with the donor are prioritized over candidates within the same DSA as the

center within the same service area as the donor hospital. That is, candidates in the same DSA as the donor hospital are prioritized for a kidney offer over those enrolled in transplant centers outside the DSA but within the same OPTN region[13] and those who are outside the OPTN region (that is, elsewhere in the nation). Appendix Table 2.B.1 provides a table for kidney allocation point calculation used to rank each candidate within candidate category.[14]

After receiving an offer, candidates are given a specific amount of time to accept or reject the offer, given that kidneys lose viability within 72 hours after the donor's death. Non-utilization rate and pre-transplant mortality deaths are two measures often used to evaluate the efficiency of the allocation system. First, to evaluate the use of donated kidneys, non-utilization rate (often called the discard rate) is the share of kidneys recovered for transplant but not utilized (OPTN, 2023; Israni, et al., 2020). Second, pre-transplant mortality is the number of transplant candidates that die on the waitlist prior to getting a kidney transplant and is often used to assess performance of service areas and transplant centers.[15]

2 .4 Donor Service Areas and the 2021 Reform

Although the DSA boundaries were originally defined to ensure equal access to kidney transplant candidates, in recent decades, supply and demand for deceased donor kidney transplants vary greatly across different geographical areas (Adler et al., 2016). Figure 2.2 illustrates geographical variation in use of donated kidneys (Panel A) and access to kidney transplants (Panel B) in years prior to the reform. To examine the relationship between kidney access and the use of donated kidneys, Appendix Figure 2.B.3 provides a scatterplot illustrating the relationship

donor hospital, regardless of the location of their transplant centers.

[13]Each OPTN region consists of a group of DSAs. There have been 11 OPTN regions in the U.S. since 1984. Appendix Figure 2.B.2 shows the map of OPTN regions in the country.

[14]In Appendix Table 2.B.2, calculation of the kidney points considers the distance between donor hospital and transplant center (0-2 points) starting from March 21, 2021. If candidates have same points within the category, they are ranked based on the duration of registration. Table 1 of Israni et al. (2014) describes how candidates are ranked in the national waitlist based on their health and demographic characteristics when kidney(s) becomes available from a deceased donor.

[15]For instance, the SRTR publishes pre-transplant mortality rates for the last two years for each transplant center in their annual program-specific reports.

between the number of available kidneys per candidate and the discard rate during the pre-period by "pre-reform" service area. The scatterplot shows a positive correlation (p-value=0.016) between these two measures, suggesting that the discard rate tended to be lower in places with lower kidney access during the pre-reform period.

A new policy was introduced on March 15, 2021, with the goal of addressing efficiency and equity in access to kidney transplants by providing more equal access to donated organs across geographical areas.[16] Under this reform, the allocation of deceased donor kidneys is based on the proximity between donor hospitals and transplant candidates rather than county-based service areas. Transplant candidates registered in transplant centers located within 250 nautical miles (i.e., 287.695 miles) from the donor hospital are prioritized for kidney offers, regardless of whether they are located within the same DSA as the donor hospital.[17]

Using the pre-reform data, I assess whether the policy change is expected to reduce disparities in access to kidneys across geographic areas. Appendix Figure 2.B.4 provides the distribution of the monthly number of recovered kidneys per candidate across transplant centers before and after the reform. This figure illustrates that the dispersion in kidney access is predicted to be smaller after the reform, suggesting that the reform may have contributed to equalizing kidney access across different areas.

The timing of policy implementation was hard to predict as it was originally planned to be implemented on December 15, 2020, but was pushed back due to concerns about the increased role of socioeconomic factors on transplant access and the COVID-19 pandemic. The OPTN announced in late February that the policy would be implemented on March 15, 2021 barring any further court intervention. The OPTN website posted an announcement on March 6, 2021, that the policy would go into effect on March 15, 2021.

[16]The reform also took place in the pancreas allocation system. However, the size of the system is considerably smaller in scale compared to that of kidneys, with 19,762 kidney transplants in contrast to just 964 pancreas transplants in 2021.

[17]The only exception was kidneys recovered from donor hospitals in Alaska (which takes less than 0.3% of kidneys recovered for transplant from 2021-2022) for which the concentric circle is drawn around Seattle-Tacoma International Airport. This measure ensures that kidneys recovered in Alaska can find suitable recipients promptly, given that there are no transplant centers located within the state.

While the change in geographic boundaries was the main policy change, three additional policy changes were introduced on March 15, 2021. These policies change the priority ranking of three rare types of candidates that comprise a small share of transplant recipients (<2% in both pre- and post-policy period) and they that had been prioritized over most other candidates prior to the reform. Specifically, these categories are: (1) candidates with exhausted and imminent failure of access to dialysis ("medical urgency")[18]; (2) candidates who are prior living donors; and (3) pediatric candidates with kidney offers with non-zero HLA mismatch and KDPI below 35%. In robustness exercises, I show that my results are highly similar in a subsample of candidates whose ranking would not have been affected.[19]

3 Data

My source of data is the restricted Scientific Registry of Transplant Recipients (SRTR), which stores information on the universe of deceased and living transplant donors, transplant candidates, and transplant recipients in the U.S. I combine detailed demographics and health conditions of all transplant donors, candidates, and recipients; comprehensive information on all offers made for a given kidney; precise geographical information on donor hospitals and transplant centers; and mortality outcomes from Social Security records for all recipients. Using these data, I construct two analysis samples – one that includes all donated kidneys and spans January 2019–April 2023, and a monthly panel of transplant centers that spans the same time period.

3 .1 Donated Kidney Sample

I construct a dataset covering all kidneys offered to transplant recipients from 1/2019 to 3/2023. Each observation is a donated kidney. To create these data, I first link together four

[18]Transplant candidates ever reported with medical urgency are rare – only 14 registrations were ever waiting in medical urgency status and 4 received a deceased donor transplant between the policy implementation date and June 30, 2021 (OPTN Report, 2022).

[19]This is consistent with Rausch et al. (2022), which discusses that the reform is unlikely affected access to kidney transplants for pediatric candidates.

datasets on donated kidneys using a unique ID for the donor. The first includes donor health measures, which are used to assess kidney quality. The second, called the Potential Transplant Recipient (PTR) dataset, contains the initial offer date for each kidney (approximately, the donor's date of death). The data also include information on any subsequent offers and the results of each offer (i.e., whether it was accepted or refused and the reasons for any refusals).

Third, I use data on the outcome ("disposition") for each kidney, including whether it was ultimately transplanted or discarded and the reasons for these outcomes. In particular, kidneys may be discarded for three reasons: (a) waited too long on the waitlist[20], (b) low quality[21], or (c) other factors.

Fourth, I link the donor hospital that oversaw organ recovery with detailed geographic information (GPS coordinates, zip code, and institution name, OPO affiliation). During the period between January 2019 and April 2023, a total of 2,220 donor hospitals recovered at least one deceased donor kidney for transplant. As the distance between donor hospital and transplant center determines allocation after the policy implementation, I calculate the distance for all possible pairs of donor hospitals and transplant centers using their GPS coordinates.[22] To do so, I extract longitude and latitude information based on the addresses of transplant centers from Google Maps as the SRTR data does not provide information on their GPS coordinates.[23]

Finally, for the subset of kidneys that are transplanted, I merge in detailed information on each transplant recipient. I use this information to shed light on changes in access to kidney transplants across subgroups as a response to the policy change. Specifically, the information includes demographics (age, race, ethnicity, education attainment, insurance coverage) and health conditions (history of dialysis, diabetes, prior organ transplant).

To examine whether the reform changes access to kidneys among sociodemographic

[20] Too old on pump; too old on ice; warm ischemic time too long; no recipient located – list exhausted.

[21] Donor medical history; positive CMV; positive HIV; positive Hepatitis; biopsy findings; diseased organ; poor organ function; organ trauma; diseased organ; anatomical abnormalities; inadequate urine output.

[22] I use the "geosphere" package in R, which includes the "distm" function to compute the shortest distance between two locations.

[23] I use the "ggmap" package in R to extract GPS coordinates based on the addresses.

groups, I merge recipients' zip code of residence with the CDC Social Vulnerability Index (SVI). SVI ranks counties based on 15 sociodemographic factors[24] to assess the area's relative vulnerability to public health emergencies, and it is known to be associated with disparities in access to health care and health outcomes among residents (Khazanchi et al., 2020; Phelos et al., 2021; Bauer et al., 2022). I use the SVI 2018 for the analysis (Centers for Disease Control and Prevention, 2020). As SVI is a composite measure of social determinants of health, I merge the poverty rates from the 2019 5-year ACS estimates with the candidates' county of residence information to check the robustness of results.

My analytic sample includes adult deceased donors whose kidneys were recovered for transplant[25] with non-missing information for donor characteristics, including information used to calculate KDPI score for each kidney. The sample is further limited to deceased donor kidneys recovered from donor hospitals outside Alaska and recovered in DSAs that did not experience any change in OPO affiliation through the sample period. Two OPOs, LifeChoice Donor Services (CTOP) and New England Donor Bank (MAOB), merged on January 1, 2021. The sample includes 23,466 kidneys recovered for transplant within 28 weeks of the policy change from 1,749 donor hospitals.

Panels A-B of Table 2.1 present descriptive statistics for the kidney-level data. In Panel A, 23 percent of kidneys recovered for transplant are discarded (henceforth, "discard rate"). Of kidneys that were discarded, 60.9 percent of them were not used for transplant as they waited too long the waitlist, 7 percent of them were discarded due to organ or donor health concerns, and 2.4 percent were discarded due to other reasons. 17 percent of kidneys have KDPI above 85%, which are considered marginal kidneys. Panel B provides descriptive statistics of transplant candidates who received kidneys. Average distance between transplant donor hospital and

[24]SVI is calculated using 15 factors including poverty rate, unemployment rate, educational attainment, elderly and child population, disability status, single-parent households, ethnic diversity, English proficiency, housing type, and vehicle ownership. CDC obtains these variables from the American Community Survey (ACS).

[25]Categories of deceased donor organs not recovered are (a) authorization was not requested, (b) authorization not obtained, (c) not recovered, (d) recovered not for transplant (e.g., education/research purposes). Deceased donor kidneys with one of these disposition decision codes rarely enter the allocation system– a total of 248 non-recovered kidneys appeared in the PTR dataset within 26 weeks of the policy implementation date.

transplant recipient is 195 miles and average length of pre-transplant dialysis per transplant recipient is 4.4 years.

3 .2 Transplant Center Sample

My second analysis dataset is a monthly panel of transplant centers that includes measures of health for all candidates registered at a given center, including those that do not receive a transplant. To create this sample, I use microdata on candidates that include health conditions (e.g., history of dialysis, diabetes, previous transplant records), transplant registration records, and transplant records (e.g., date of transplant, type of donor, characteristics of transplanted kidney). In addition, the data provide the exact date of death of all transplant candidates from the Social Security Death Master File.

My sample consists of transplant centers with active transplant candidates prior to the policy change. To proxy for the size of each center, I calculate the average waitlist enrollment per month during 1-15 months prior to the reform. I exclude transplant centers located in DSAs that experienced any change in OPO affiliation through the sample period. Panel C of Table 2.1 presents descriptive statistics for transplant centers. The sample covers 222 transplant centers, with 423 candidates on the waitlist on average prior to the reform.

I consider two measures of candidate mortality: whether the candidate died while waiting for a transplant and whether the candidate died within 5 years after registering, regardless of whether they received a transplant. While the first measures deaths that may have been avoidable with a kidney transplant, the latter encompasses deaths that occur after waitlist removal due to deteriorated health and after transplants. For each outcome, I calculate total death counts by transplant center and month. To examine whether the policy change affected candidates' choice of transplant centers, I calculate the monthly number of newly added transplant candidates per transplant center.

Finally, I consider a few outcomes that are observed only for the subset of candidates that receive a transplant. These include whether the candidate experienced death or graft failure, or went back to dialysis within 3, 6, 9, and 12 months after their transplant. 3,125 candidates who received transplants experienced these outcomes within 12 months of kidney transplant.

Treatment Intensity

While all transplant candidates are subject to the policy change, the extent of its impact varies across transplant centers depending on changes in the number of available kidneys. I develop a measure of the predicted change in available kidneys at each transplant center, based on pre-reform data on kidney supply and demand at that center. I then use this proxy for treatment intensity in a differences-in-differences identification strategy, described in the next section.

Changes in kidney availability for a given transplant center can vary based on the number of donor hospitals for which it lies in the service area, as well as the size of other transplant centers in the same service area. For example, consider a scenario in the pre-reform era in which a DSA includes only one donor hospital and one transplant center. Suppose that there are several nearby donor hospitals within 250 nautical miles of this transplant center but positioned across the DSA border. Then, the transplant center would experience an increase in the number of available kidneys. This increase occurs assuming no changes in the set of transplant centers also receiving kidneys from these donor hospitals.

In Eq. 2.1, $\Delta Access_h$ is the predicted change in the number of available deceased donor kidneys for transplant center h, which is calculated as follows:

$$\Delta Access_h = \sum_{d \in circle(h)} k_d * \frac{n_h}{n_d} - \sum_{d \in DSA(h)} k_d * \frac{n_h}{n_{DSA(h)}} \tag{2.1}$$

where *DSA(h)* is a set of donor hospitals located in the same DSA as transplant center *h*. *circle(h)* is a set of donor hospitals located within 250 nautical miles of transplant center *h*. n_h denotes the average waitlist enrollment per month in transplant center *h* during the 28 months prior to the policy change (i.e., January 2019-Feburary 2021). Then I calculate average monthly kidneys recovered within the same timeframe as a proxy for kidney supply at donor hospital d, k_d. Next, I calculate (1) total transplant candidates registered at centers within the same DSA as transplant center h, $n_{DSA(h)}$, and (2) total transplant candidates within 250 nautical miles of donor hospital d, n_d.

Based on this information, I calculate the predicted number of available kidneys to transplant center h in the pre-period by summing the average number of deceased donor kidneys recovered from donor hospitals within the same DSA as transplant center h, weighted by the share of transplant candidates registered in h in the DSA. For the predicted number of available kidneys in the post-period, I sum the monthly number of kidneys recovered from all donor hospitals located within the 250 nautical miles of transplant center h, weighted by the share of transplant candidates in h among transplant candidates within 250 nautical miles from donor hospital d.[26]

Of 222 transplant centers included in the sample, 46 percent of them are predicted to have an increase in the number of available kidneys after the policy change (Panel C of Table 2.1).[27] To examine whether the treatment intensity is a strong proxy for changes in access to kidney transplants, I calculate a correlation between the predicted and actual changes in the monthly number of kidneys per transplant center before and after the reform.[28] The correlation between the predicted and actual changes, weighted by the size of the transplant center, is 0.37 (p-value$<$0.001), providing a suggestive evidence that the predicted treatment intensity is a good proxy for the actual changes. In addition, Appendix Figure 2.B.7 plots the predicted changes in access to kidneys against the number of available kidneys by transplant center during the pre-reform period. The scatterplot shows a negative correlation (-0.23 with p-value$<$0.001) between these two measures, indicating that the reform may mitigate disparities in access to kidneys by reallocating donated kidneys to places with lower access during the pre-reform period.

[26] Appendix Figure 2.B.5 presents a numerical example of how the predicted changes in the number of available kidneys for each transplant center are calculated.

[27] Appendix Figure 2.B.6 presents a histogram of $\Delta Access_h$, and Appendix Figure 2.B.8 illustrates the average treatment intensity mapped by DSA.

[28] Appendix Figure 2.B.9 provides a scatterplot that compares the proxy for treatment intensity with the actual shifts in average recipients of deceased donor kidneys within 15 months of the reform.

Furthermore, I use this measure to calculate the predicted treatment intensity for transplant centers to examine how use of kidneys depends on overall changes in access to kidneys among nearby transplant centers. For instance, the decrease in the discard rate at the cut-off is likely to come from kidneys recovered from donor hospitals whose transplant centers within 250 nautical miles are predicted to experience lowered access to kidneys after the policy change. In Eq. 2.2, $\overline{\Delta Access}_d$ is calculated as the average treatment intensity measure for transplant centers located within 250 nautical miles from a donor hospital d, weighted by their relative size to all transplant centers within the concentric circle drawn from the donor hospital.

$$\overline{\Delta Access}_d = \sum_{h \in circle(d)} \Delta Access_h * \frac{n_h}{n_d} \tag{2.2}$$

4 Empirical Strategies

My empirical strategies are (1) a regression discontinuity (RD) design with the initial offer date as a running variable and (2) a difference-in-differences (DD) design exploiting variation in the timing of policy change and treatment intensity across transplant centers. I use the kidney-level data to estimate the RD design and the transplant-center dataset for the DD design.[29]

4 .1 Regression Discontinuity Design

My RD design leverages the sharp timing of the reform to identify its causal effects. Using this design, I investigate how the use of kidneys and the composition of transplant recipients respond when the service areas used for kidney allocation were changed from fixed geographical boundaries to flexible concentric circles. My analysis sample includes deceased donor kidneys recovered for transplant and first offered to transplant candidates within 28 weeks from the policy

[29]This study used data from the SRTR. The SRTR data system includes data on all donor, wait-listed candidates, and transplant recipients in the US, submitted by the members of the OPTN. The Health Resources and Services Administration (HRSA), U.S. Department of Health and Human Services provides oversight to the activities of the OPTN and SRTR contractors.

implementation date, March 15th, 2021.[30] Eq. 2.3 presents the estimating equation for my RD specification:

$$Y_i = \alpha + \beta 1(\mathrm{D}_i \geq c) + 1(\mathrm{D}_i \geq c) f(\mathrm{D}_i - c) + f(\mathrm{D}_i - c) + X_i\rho + \varepsilon_i \tag{2.3}$$

where f() is a control function based on the initial offer date. D_i denotes the initial date which deceased donor kidney i first appeared in the allocation system. $1(\mathrm{D}_i \geq c)$ is an indicator for deceased donor kidneys offered to kidney transplant candidates for the first time on or after March 15, 2021, which serves as the cut-off value c. Triangular weights are used to linearly weigh each observation based on distance from the cut-off. Standard errors are clustered at the level of initial offer date. X_i denotes control variables, which include a set of donor characteristics (gender, race/ethnicity, blood type, history of hypertension). Triangular weights are used to linearly weight each observation based on distance from the cut-off. Standard errors are clustered at the level of initial offer date.

The identifying assumption is that the assignment of kidneys into the allocation system on either side of the cut-off date is as good as random so that the outcomes of interests would have evolved smoothly in the absence of the policy change (Lee, 2008). Under this assumption, β captures the effects on use of deceased donor kidneys and the composition of transplant recipients. I employ two tests of the identifying assumption. First, I check the smoothness of the distribution of the initial offer date using the McCrary density test. This test helps examine possible manipulation or deliberate delay in registering certain kidneys in the allocation system under the new system. The results of the McCrary density test do not reject the null hypothesis that the density of my running variable is smooth around the cut-off point (p-value=0.16).[31]

Second, I examine whether deceased donor kidneys on either side of the cut-off have similar observable characteristics. Table 2.2 and Appendix Tables 2.B.3-2.B.6 provide the

[30] I employ the method developed by Calonico et al. (2014) to calculate the data-driven bandwidth for my main outcome variable, an indicator of whether kidneys were discarded. The optimal bandwidth is 196 days (28 weeks).
[31] Appendix Figure 2.B.10 presents the density of deceased donor kidneys based on their initial offer date.

RD estimates when Eq. 2.3 is estimated by setting each of the following kidney (i.e., donor) characteristics as the the dependent variable: a) demographics, b) health conditions, c) donor consent mechanisms, and d) circumstances of death. provide the RD estimates when these characteristics are used as the dependent variable to estimate Eq. 2.3. There are no significant systematic differences in these observable characteristics around the cut-off, which supports the validity of my RD design.

For robustness exercises, I estimate Eq. 2.3 using a number of alternatives to my baseline specification: (1) excluding triangular weights, (2) excluding X_i, (3) setting f() as a local linear function form, and (4) using alternative choices of bandwidth length.

Furthermore, I perform placebo exercises by estimating Eq. 2.3 with two samples on livers and hearts recovered from deceased donors in the kidney-level data. As there are separate allocation systems based on organ type, the reform should not affect the allocation of deceased-donor organs that have not been affected by the policy change. Livers and hearts are two most commonly transplanted organs other than kidney.[32] Similarly to the kidney-level data, I restrict the sample to nonkidney organs that were recovered for transplant and were recovered as a single organ (that is, I drop double-lung from the sample) to keep the unit of the observation at the organ level.

4 .2 Difference-in-Differences Design

Next, I use the monthly panel of transplant centers to examine how the reform affected access to transplant and health outcomes of transplant candidates. With these data, I employ a difference-in-differences design leveraging within-transplant center variation in predicted access to deceased donor kidney transplants over time. A fixed-effect Poisson model is used to estimate Eq. 2.4 to address the existence of zero pre-transplant deaths in certain transplant centers during specific months:

[32]In 2021, 3,861 hearts, 2,443 lungs, and 8,595 livers from deceased donors were used for transplant.

$$E[Y_{ht}|\Delta Access_h * Post_t, \theta_h, \lambda_t] = exp[\gamma \Delta Access_h * Post_t + \theta_h + \lambda_t] \tag{2.4}$$

where Y_{ht} denotes an outcome for transplant hospital h in month t. $Post_t$ is an indicator for months on or after March 2021. $\Delta Access_h$ is the predicted change in the available deceased donor kidneys for h, as defined in Eq. 2.1. θ_h is a transplant center fixed effects to take unobserved time-invariant transplant hospital characteristics into account. λ_t is a year-month fixed effect to capture aggregate time trends affecting all transplant centers in the U.S. I control for the natural log of the average number of candidates on the waitlist during the pre-period for each transplant center and constrain the coefficient to be one. γ is the key parameter of interest, capturing the effect of changing the service area boundaries on transplant center outcomes. Standard errors are clustered by transplant hospital.

To interpret γ as the causal effect of the policy change, the transplant centers predicted with higher treatment intensity would have had to have otherwise evolved on the same trajectory during the post-period compared to those with lower treatment intensity (Callaway et al., 2021). I estimate the following event-study specification to test for differential pre-trends in outcomes across transplant hospitals and to illustrate dynamic treatment effects:

$$E[Y_{ht}|X] = exp[\sum_{\tau=-15(\neq -1)}^{14} \delta_\tau 1(t=\tau) * \Delta Access_h + \theta_h + \lambda_t] \tag{2.5}$$

where event-time τ is the difference in months between a given month t and March 2021. δ_τ captures changes in transplant center outcomes in month τ, relative to February 2021 ($\tau = -1$). Standard errors are clustered at transplant center level.

5 Results

I estimate the RD specification using the kidney-level data to study the effect of the reform of service areas on the use of deceased-donor kidneys and the demographics of transplant recipients. Then, I estimate the difference-in-differences strategy using the transplant-center-level data to examine the policy's impact on health outcomes of transplant candidates.

5 .1 Kidney Outcomes: Utilization and Characteristics of Transplant Recipients

Use of Deceased Donor Kidneys

I first assess whether the reform shifts the geographic allocation of kidneys—in particular, whether kidneys are less likely to find the recipients at centers in the same DSA as the donor hospital, as would be expected. Figure 2.3 plots the share of kidneys transplanted within the DSA by initial offer date. Table 2.3 reports estimates and standard errors corresponding to Y_i from Eq. 2.3. In Column 1, the likelihood of kidneys finding the recipients within the DSA drops by 23.4 percentage points (43 percent) at the cut-off, suggesting that the policy change has an immediate and important effect on where kidneys are allocated.[33]

Next, I explore how the policy change affects the use of these kidneys. Figure 2.4 plots the overall discard rate against the initial offer date. In Column 2 of Table 2.3, the likelihood of kidneys being discarded drops by 3.8 percentage points (17 percent) at the cut-off. In particular, the likelihood of kidneys being discarded because they are on the waitlist too long discontinuously drops by 3.3 percentage point (26 percent; Column 3). In contrast, there are no discontinuous changes in the likelihood of kidneys being discarded because of organ-quality issues and other reasons at the cut-off (Columns 4–5). Thus, these results suggest that the reform increased the usage of donated kidneys and did so through an improvement in allocative efficiency, rather than a change in the composition of donors or other factors.

(a) Kidney Quality: To explore whether certain types of kidneys are more likely to be allocated after the reform, I examine how the effects on the discard rate vary by kidney quality.[34] In Panel

[33]Similarly, in Column 2 of Appendix Table 2.B.7, the share of kidneys transplanted within the OPTN region drops by 11.9 percentage points (18 percent). Appendix Figure 2.B.11 plots the share of kidneys transplanted within the OPTN region by initial offer date.

[34]As explained earlier, kidney quality decreases (equivalently, health risk increases) with KDPI and these four subgroups are (a) 0–20 percent, (b) 21–35 percent, (c) 35–85 percent, and (d) 86–100 percent. Appendix Figure 2.B.12 plots the discard rate by four subgroups based on the KDPI score used by the allocation system.

A of Figure 2.5, the discard rate for kidneys with KDPIs above 85 percent decreases by 7.7 percentage points (12 percent) at the cut-off. In contrast, the coefficients for kidneys with KDPI below 85 percent are not statistically different from zero at 10 percent. These lower-quality kidneys must be used in a shorter period and can only be matched to transplant recipients who gave informed consent in advance to express their interest in these kidney offers. Reassuringly, the findings are similar when an alternative measure of kidney quality is used to estimate Eq. 2.3.[35] These findings indicate that the policy particularly increases the use of marginal kidneys.

(b) Donor Hospital Characteristics: Next, I turn to explore heterogeneous effects by donor hospitals. First, Panel B of Figure 2.5 explores how the effects on use of donated kidneys vary by the location of donor hospital. Kidneys recovered from donor hospitals located in core metropolitan areas are less likely to be discarded by 5.8 percentage points (25 percent) at the cut-off. By comparison, the decrease in the discard rate from kidneys with donor hospitals located in less urbanized/populated areas is somewhat smaller (1.1 percentage points or 5 percent).[36]

Second, Panel C of Figure 2.5 explores how use of kidneys depends on overall changes in access to kidneys among nearby transplant centers.[37] The discard rate for kidneys recovered from donor hospitals where nearby transplant centers are predicted to experience lower kidney access on average drops by 5.4 percentage points (24 percent) at the cut-off. In comparison, the discard rate for kidneys from donor hospitals where nearby transplant centers are predicted to have higher kidney access on average is 3.1 percentage points (14 percent).[38] Still, this comparison is suggestive as the coefficients are not statistically distinguishable.

To assess the robustness of my findings, I estimate Eq. 2.3 using the dependent variables in Table 2.3 but with a number of different specifications: (1) dropping the control variables, (2)

[35]Prior to 2014, kidneys were classified into one of two categories: expanded criteria donor (ECD) or standard criteria donor (SCD) kidneys, with the latter indicating higher quality than the former. ECD kidneys are those recovered from donors (1) 60 or older or (2) aged 50–59 with two or three of the following conditions: high blood pressure, creatinine levels of 1.5 or higher, or death due to stroke (Ojo, 2005). ECD kidneys were only offered to candidates who showed interest in them in advance. In Columns 5–6 of Appendix Table 2.B.8, ECD kidneys show a 7.9 percentage point (13.9 percent) decrease in the discard rate at the cut-off.

[36]Appendix Table 2.B.9 provides the RD estimates.

[37]As mentioned earlier, it is based on donor hospital's predicted treatment intensity in Eq. 2.2.

[38]Appendix Table 2.B.10 provides the RD estimates.

excluding weights, (3) setting f() as a local linear function form, and (4) using different bandwidth choices. In Table 2.4, the RD estimates remain very similar across different specifications, which suggests that my findings are not sensitive to changes to my baseline specification.

Moreover, I perform placebo tests by using samples on hearts and livers. Table 2.5 provides the RD estimates when dependent variables for Eq. 2.3 are an indicator of discarded organs and indicators of three organ-discard reasons.[39] All estimates in Table 2.5 are statistically indistinguishable from 0, supporting the validity of my study design.

Characteristics of Transplanted Kidneys

Next, I examine how the policy change affects travel distance, match quality, and the time it takes for kidneys to reach transplant centers where their recipients are enrolled. To do so, I limit the kidney-level data to kidneys used for transplant.

Table 2.6 reports the RD estimates when the dependent variables are the distance between donor hospitals and transplant centers. In Column 1, the travel distance increases by 19.8 nautical miles at the cut-off, but the estimate is not statistically distinguishable from zero. This could be attributable to the distributional effects – the likelihood of kidneys finding the recipients within 250 nautical miles of donor hospitals increases by 3.5 percentage points (4 percent; Column 3) but the likelihood of kidneys finding the recipients within 50 nautical miles decreases by 18.2 percentage points (40 percent; Column 2).[40] Similar to the effects on travel distance, the number of hours kidneys are in cold storage (i.e. cold ischemia hours) increases by 1.6 hours (9 percent) at the cut-off (Column 4).

To examine the impact on match quality, I estimate Eq. 2.3 by setting the dependent variable equal to outcomes related to the number of HLA mismatches. In Appendix Table 2.B.11, all RD estimates in the table are close to zero and are statistically insignificant at the 10 percent level. These results indicate that the policy change does not change the match quality but

[39]Appendix Figure 2.B.13 provides the share of nonkidney organs that are being discarded by initial offer date.

[40]These results are consistent with an increased likelihood of kidneys finding the recipients *outside* the DSA (Column 1 of Table 2.6) but *within* 250 nautical miles after the reform (Column 3 of Table 2.6).

increases the efficiency of the allocation system by allowing kidneys to find a recipient more flexibly and within a timely manner.

Characteristics of Transplant Recipients

I shift my focus to examining how the reform changes who receives a kidney transplant. Limiting the sample to kidneys used for transplants, I estimate the RD model in Eq. 2.3 using the dependent variable as an indicator of candidate subgroups to test whether the composition of transplant recipients changes at the cut-off. Figure 2.6 plots the RD estimates.[41]

(a) Recipient Health Status: I examine whether the reform affects access to transplants for transplant candidates with higher health risks. I focus on three factors used to calculate the Estimated Post Transplant Survival score: number of years on dialysis, previous transplant history, and diabetes history.[42] To assess any distributional effects, I estimate Eq. 2.3 using the dependent variable as an indicator of four subgroups categorized by dialysis duration of the transplant candidate prior to the transplant: (1) zero years, (2) one to two years, (3) three to four years, and (4) five years or more.

In Panel A of Figure 2.6, the likelihood of candidates with five or more years of dialysis history receiving kidney transplant increases by 5.8 percentage points (17 percent) at the cut-off. Meanwhile, the changes in the likelihood of recipient subgroups with less than five years of dialysis history at the cut-off are not statistically distinguishable from zero. But there are no discontinuous changes in the likelihood of candidates with previous transplant history or diabetes receiving kidney transplant at the cut-off.

(b) Sociodemographic Characteristics: Next, I examine whether the policy change has varying

[41]These RD estimates are reported in Appendix Tables 2.B.12-2.B.14. To explore how the changes in kidney access associated with the reform map to changes in the composition of transplant recipients, Appendix Figure 2.B.14 plots the average treatment intensity across transplant candidate subgroups. To do so, I use the data of transplant candidates on the waitlist prior to the reform. In Figure 2.6, candidate subgroups with higher average treatment intensity tend to have an improved access to kidney transplant after the reform.

[42]As mentioned earlier, these factors are used to calculate EPTS score. As the life expectancy for transplant candidates on dialysis is 12.3 years (Held et al., 2016), those who undergo an extended period of dialysis face not only a shorter expected life span but an increased risk of being removed from the transplant waitlist because of factors such as mortality or other medical complications.

impacts on access to kidney transplants based on sociodemographic characteristics. I estimate Eq. 2.3 by setting dependent variables as indicators for quartile groups based on SVI of counties where candidates live at the time of the transplant. In Panel B of Figure 2.6, the likelihood of candidates living in counties with higher social vulnerability at time of the transplant increases discontinuously at the cut-off: a 3.2 percentage point (13 percent) increase for the fourth quartile, whereas a 4.7 percentage point (20 percent) decrease for the first quartile. Similarly, average SVI discontinuously increases by 5 percent at the cut-off.[43]

Given that SVI is a composite measure of social determinants of health, I check the robustness of results by re-estimating Eq. 2.3 by setting dependent variables as county poverty rates and indicators for racial/ethnic subgroups. Panel C of Figure 2.6 shows that the change in the likelihood of candidates from counties with higher poverty rates receiving kidney transplants at the cut-off. I observe a 3.5 percentage point increase (15 percent) for the candidates from the third quartile but a 3 percentage point decrease (12 percent) for the first quartile at the cut-off. In addition, Panel D of Figure 2.6 explores how changes in access to kidney transplants vary by racial/ethnic group.[44] The likelihood of having a Hispanic/Latino transplant recipient increases by 2.1 percentage points at the cut-off. In contrast, changes in the likelihood of non-Hispanic white, black, and AAPI recipients at the cut-off are close to zero and statistically indistinguishable at 10 percent, suggesting that the reform increases access to kidneys to marginalized populations.

5 .2 Transplant Center Outcomes

Given that the results of the RD design suggest that the use of deceased donor kidneys increased as a response to the policy change, the policy change may have affected transplant candidates. In light of this, I turn to the transplant center-level identification strategy to examine the impact of the reform on health outcomes of transplant candidates. In addition, I explore its potential channels of change by examining the effects on monthly deceased kidney transplants,

[43]Column 1 of Appendix Table 2.B.13 provides the corresponding RD estimates.

[44]ESRD incidence rates are 3.8 times higher for Blacks and twice as high for Hispanics compared to non-Hispanic Whites (United States Renal Data System, 2022).

living donor kidney transplants, and candidates newly joining the waitlist.

Effects on Kidney Transplant Candidates

I estimate the event-study design in Eq. 2.5 to examine the effects on the deaths of transplant candidates. Figure 2.7a plots the event-study coefficients on the number of transplant candidates who passed away while on the waitlist in a month for a given transplant center. Figure 2.7a supports the claim that the transplant centers predicted to have higher treatment intensity evolved along the same trajectory in the number of transplant-candidate deaths during the pre-period as those predicted to have lower treatment intensity. Table 2.7 reports the estimated value for γ in Eq. 2.4, which captures the average effects of a one-unit increase in the number of kidneys available to transplant centers after the reform. In Column 1, monthly pre-transplant deaths decrease by 1.7 percent when access to deceased-donor kidneys increases by one unit.

Results are similar when an alternative death measure for transplant candidates is used. Figure 2.7b plots the event-study coefficients on the monthly transplant candidates' death counts within five years of waitlist registration per transplant center. This measure includes all deaths of transplant candidates who passed away on the waitlist and those who received kidney transplants but died within five years of entering the waitlist. The monthly death counts of candidates who passed away within five years of listing decreases by 1.6 percent when access to kidneys increases by one unit after the reform (Column 2 of Table 2.7).

Consistent with the RD result that the use of deceased-donor kidneys increases at the cut-off, monthly deceased donor kidney transplant recipients increase by 1.4 percent when access to kidneys increases by one unit after the reform (Column 3 of Table 2.7). However, the decrease in monthly pre-transplant deaths is unlikely to be attributable to changes in living donor kidney transplants (Column 4 of Table 2.7). As transplant candidates need to look out for those willing to become living donors, these findings suggest that there is no crowding out in response to the reform. This may be linked to the relatively low substitutability between deceased and living donor kidneys, given that living donor kidney transplants are expected to last longer.[45]

Furthermore, the change in monthly pre-transplant deaths is also unlikely to be attributable to changes in the number of candidates newly joining the waitlist. Appendix Figure 2.B.15b plots the coefficients from the event study when the outcome is monthly number of newly added candidates on the waitlist. In Column 5 of Table 2.7, the effect on the number of candidates joining the waitlist for deceased donor kidneys is close to zero and is statistically insignificant at the 10 percent level.

To check the robustness of the findings, I estimate Eq. 3 with OLS specification. Appendix Table 2.B.15 provides the results from the OLS model on the transplant candidate outcomes in Tables 4, respectively. The results are similar to those from the Poisson model in Table 2.7.[46]

Effects on Kidney Transplant Recipients

The results of the RD design suggest that the decrease in the discard rate is largely driven by increased use of marginal kidneys and that kidneys are remaining in cold storage until transplanted to recipients. As these types of kidneys are expected to have higher graft failure, I examine whether the policy change affects the health outcomes of transplant recipients in the short run. I define post-transplant adverse health outcomes as (1) deaths, (2) graft failure, and (3) resuming maintenance dialysis. Table 2.8 reports the estimates of γ in Eq. 2.4. Figure 2.8 plots the event-study coefficients on monthly number of transplant recipients who ever experience adverse health outcomes within 3, 6, 9, and 12 months of transplant.

In Figure 2.8, transplant centers predicted to have higher treatment intensity move on the same trajectory in terms of the number of transplant recipients experiencing adverse health

[45] As most living donors for kidney transplant are family members or relatives of the recipients, a living donor kidney transplant is expected to have greater compatibility for the recipient compared to a deceased donor kidney transplant. The estimated graft failure for a living donor kidney was 19.2 years but 12.1 years for a deceased donor kidney transplant from 2014-2017 (Poggio et al., 2021).

[46] For instance, I find that the number of pre-transplant deaths per 1,000 candidates decreases by 1.8 percent in the OLS model (Column 2 of Appendix Table 2.B.15) for one unit increase in available kidneys to transplant centers in the post-reform period, which is very similar to the 1.7 percent increase found in the Poisson model in Eq. 2.4.

outcomes during the pre-period compared to those predicted to have lower treatment intensity. All estimates in Table 2.8 are close to zero and statistically insignificant at the 10 percent level, which are also found in the OLS model.[47] The results suggest that it is unlikely that the reform affects the mortality of transplant recipients in the short run.

6 Conclusion

This paper studies the impact of redefining boundaries to reallocate perishable goods across local catchment areas and its implications for efficiency and equity. I examine these questions in the context of kidney transplants, where the stakes of preventing inefficient allocation are particularly high. I employ detailed administrative microdata on organs, transplant candidates, and transplant centers to estimate two empirical designs that exploit the sharp timing of the reform and variation in treatment intensity across transplant centers based on their precise location.

Employing a regression discontinuity design, I find that the reform resulted in an immediate decline in the role of pre-reform geographic boundaries in the kidney allocation system by 43 percent. In addition, the reform led to a 17 percent decrease in the kidney discard rate. I also document distributional effects – kidney recipients after the reform were more likely to have extended dialysis history and live in counties with higher marginalized populations. Using a difference-in-differences design, I find that monthly pre-transplant deaths decrease by 1.7 percent but no changes in the adverse health outcomes of transplant recipients when access to deceased donor kidneys increases by one unit in response to the reform.

By combining the estimates from the main findings, a back-of-envelope calculation suggests that the reform led to an additional 816 kidneys[48] being used for transplant annually,

[47] Appendix Table 2.B.16 provides the results from the OLS model on the post-transplant health outcomes.

[48] As 23,466 kidneys were recovered for transplant within 196 days of the reform, 1,822 kidneys were recovered for transplant each month on average. Given that the discard rate was 22 percent before the reform, 401 kidneys ($= 1,822$ kidneys$\times 0.22$) were discarded each month on average. As results from the RD design show that the discard rate was decreased by 17 percent after the reform, additional 68 kidneys ($= 401 \times 0.17$) were used for transplant each month, which is 816 transplant recipients annually.

which would otherwise have been discarded. As transplant candidates are expected to have an additional 7 years compared to receiving dialysis while on the waiting list, and this also leads to taxpayer savings of $146,000 due to Medicare covering substantial costs of dialysis (Held et al., 2016), the back-of-the-envelope calculation indicates that the reform may have saved 5,712 years of life and $119 million (816 transplant recipients $\times$ $146,000) for taxpayers who would otherwise have borne the burden of subsidizing dialysis treatment. These results suggest that policies aimed at reallocating scarce resources by redefining allocation boundaries for perishable or time-sensitive goods have important implications for the efficiency and equity of the allocation system.

7 Acknowledgements

This chapter is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the paper. The data reported here have been supplied by the Hennepin Healthcare Research Institute as the contractor for the SRTR. The interpretation and reporting of these data are the responsibility of the author and in no way should be seen as an official policy of or interpretation by the SRTR or the U.S. Government.

8 Figures and Tables

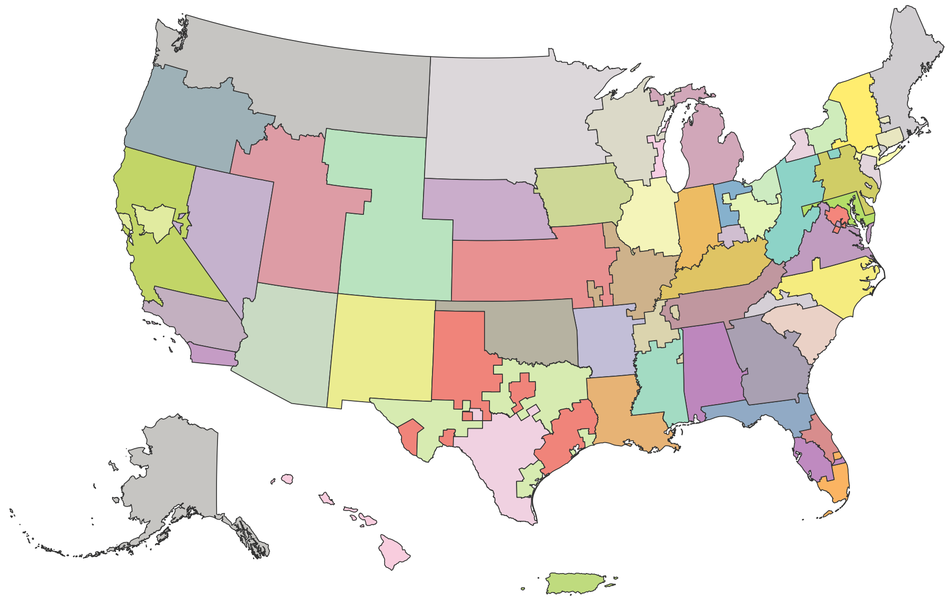
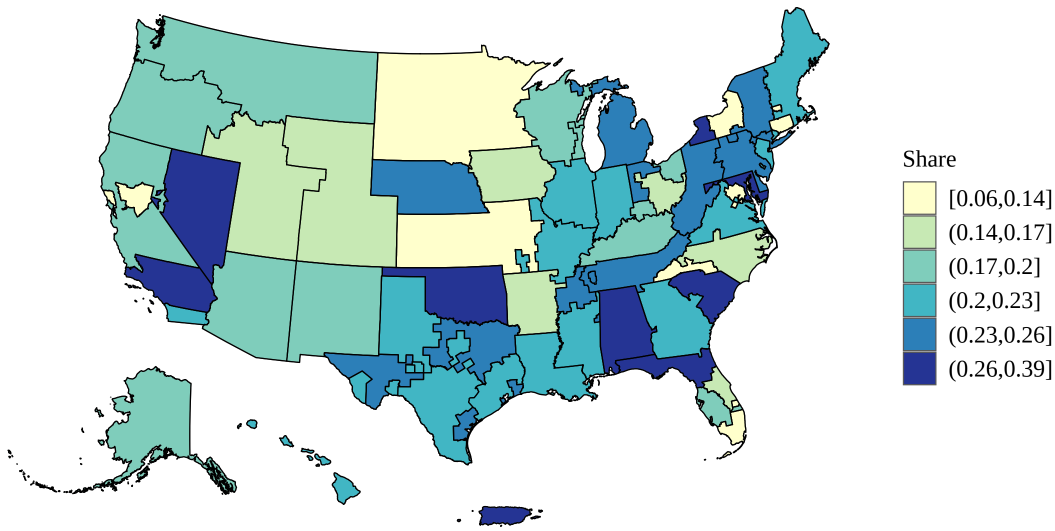


Figure 2.1. Map of Donor Service Area

Notes: In 2022, there are 57 donor service areas (DSAs) across the United States (Panel A). Each DSA consists of a group of counties defined to ensure an adequate supply of organs to registered candidates while limiting the distance organs must travel. Under the 2021 reform, the allocation of deceased donor kidneys is based on the proximity between donor hospitals and transplant candidates rather than county-based service areas. For more information about DSAs, see section 2 .

(a) Share by Kidneys Discarded by Donor Location



(b) Share of Candidates Receiving Kidney Transplants by Location

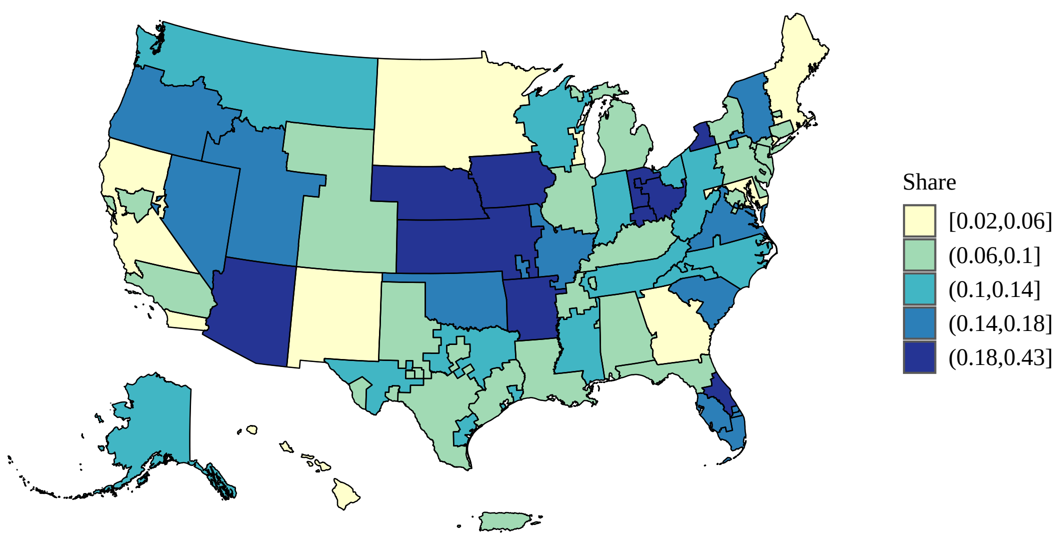


Figure 2.2. Geographic Disparities in Kidney Allocation Prior to the 2021 Reform

Notes: This figure illustrates the geographical variation in the use of donated kidneys (Panel A) and access to kidney transplants (Panel B) in the years prior to the reform. Panel A maps the share of deceased donor kidneys procured but discarded in a given donor service area (DSA). Panel B maps the share of transplant candidates who received kidneys from the donor registered in a given DSA. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets from January 2019 to February 2021, which include the universe of deceased donor kidneys and transplant candidates in the U.S.

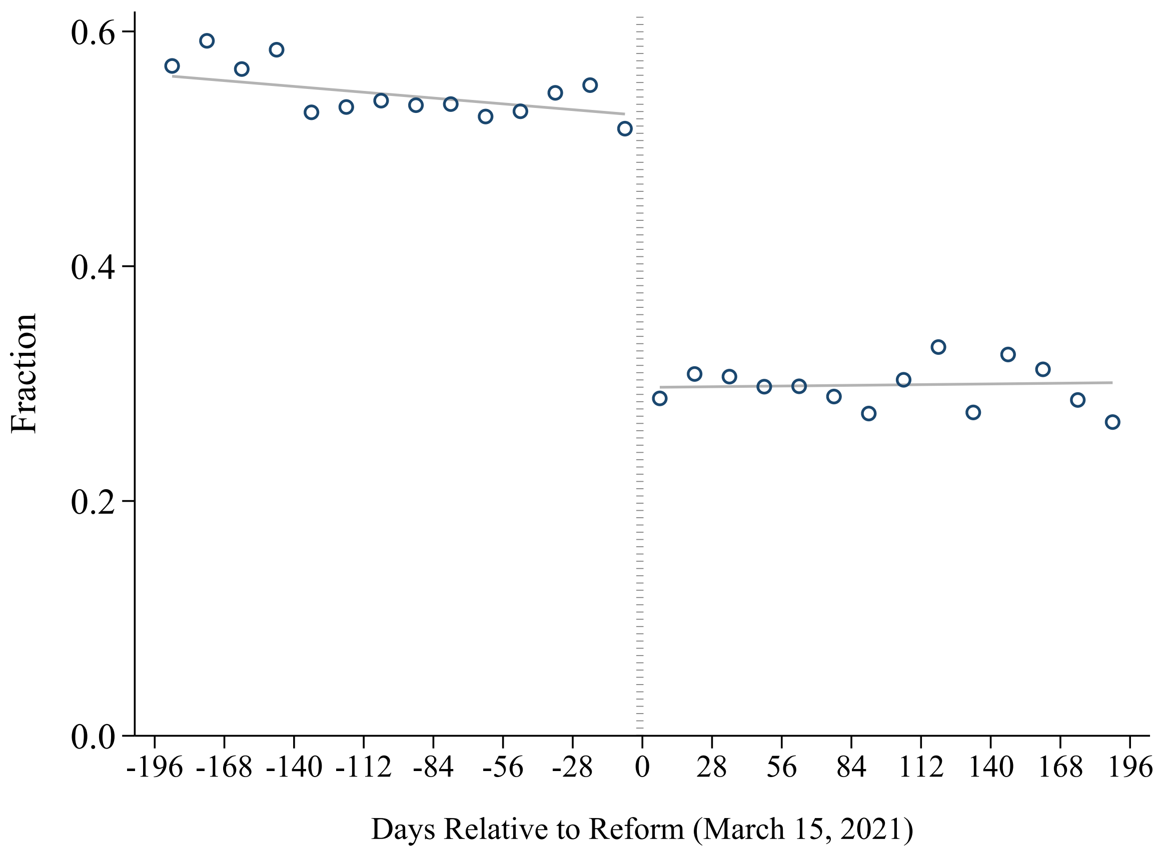


Figure 2.3. Share of Donated Kidneys Allocated Within "Pre-Reform Service Areas"

Notes: This figure shows the share of kidneys allocated to candidates at transplant centers within Donor Service Areas, which is the allocation boundaries in use prior to March 15, 2021. The date on the x-axis is the initial offer date. Each dot represents two weeks. Sample is drawn from the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. For more information on the sample, see the notes to Table 2.1.

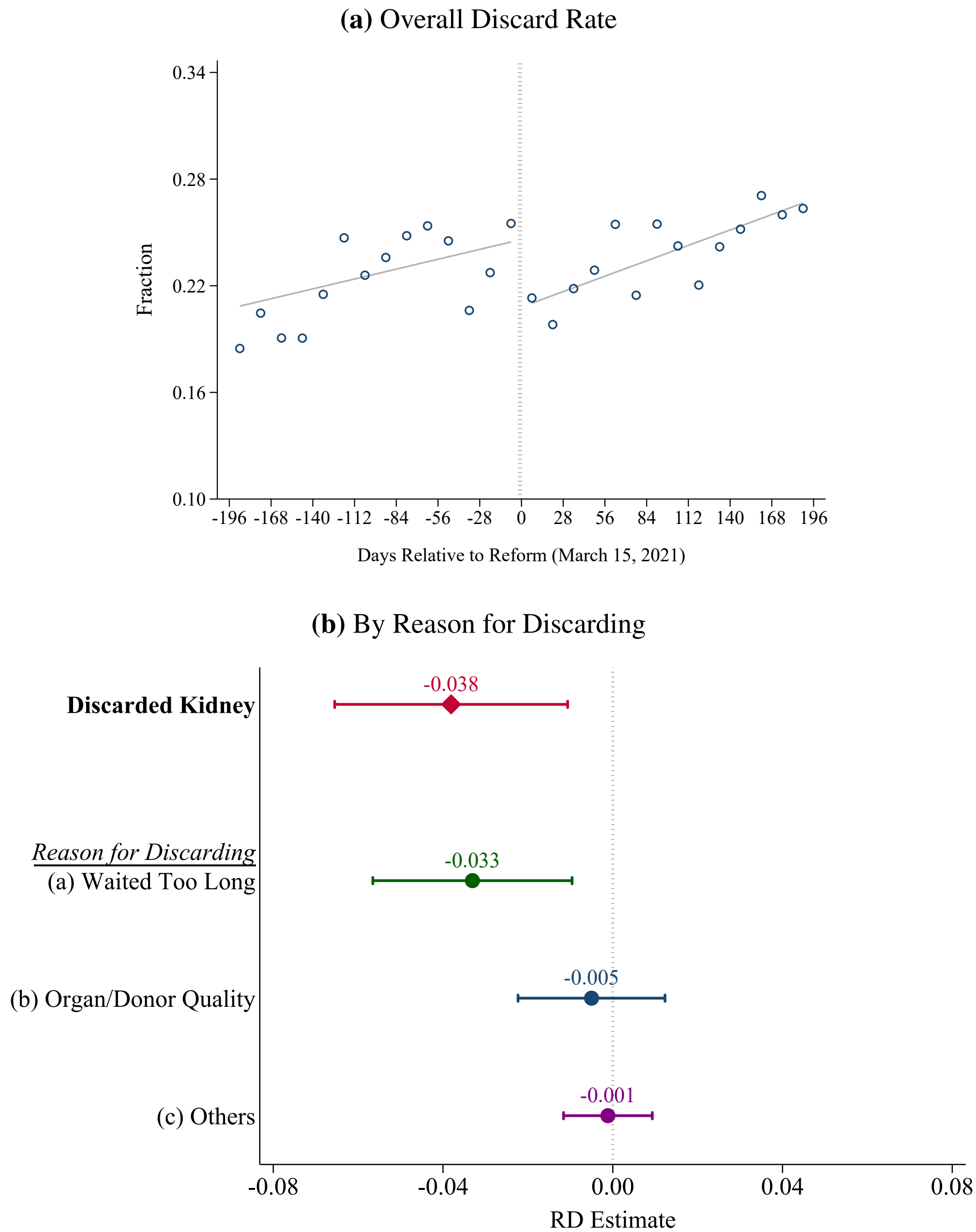


Figure 2.4. Share of Donated Kidneys that are Discarded

Notes: Figure 2.4 displays the share of deceased donor kidneys that are discarded by initial offer date relative to the allocation reform on March 15, 2021. Each dot represents two weeks. Figure 2.4b plots β from Eq. 2.3 and corresponding 95% confidence intervals. The outcomes are (1) an indicator of kidneys discarded and (2) indicators of kidneys discarded a) as they waited too long on the waitlist ("discarded due to list exhaustion"), b) due to organ quality concerns ("discarded due to donor quality"), and c) due to other factors ("others"). The date on the x-axis is the initial offer date. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. For more information on sample, see the notes to Table 2.1.

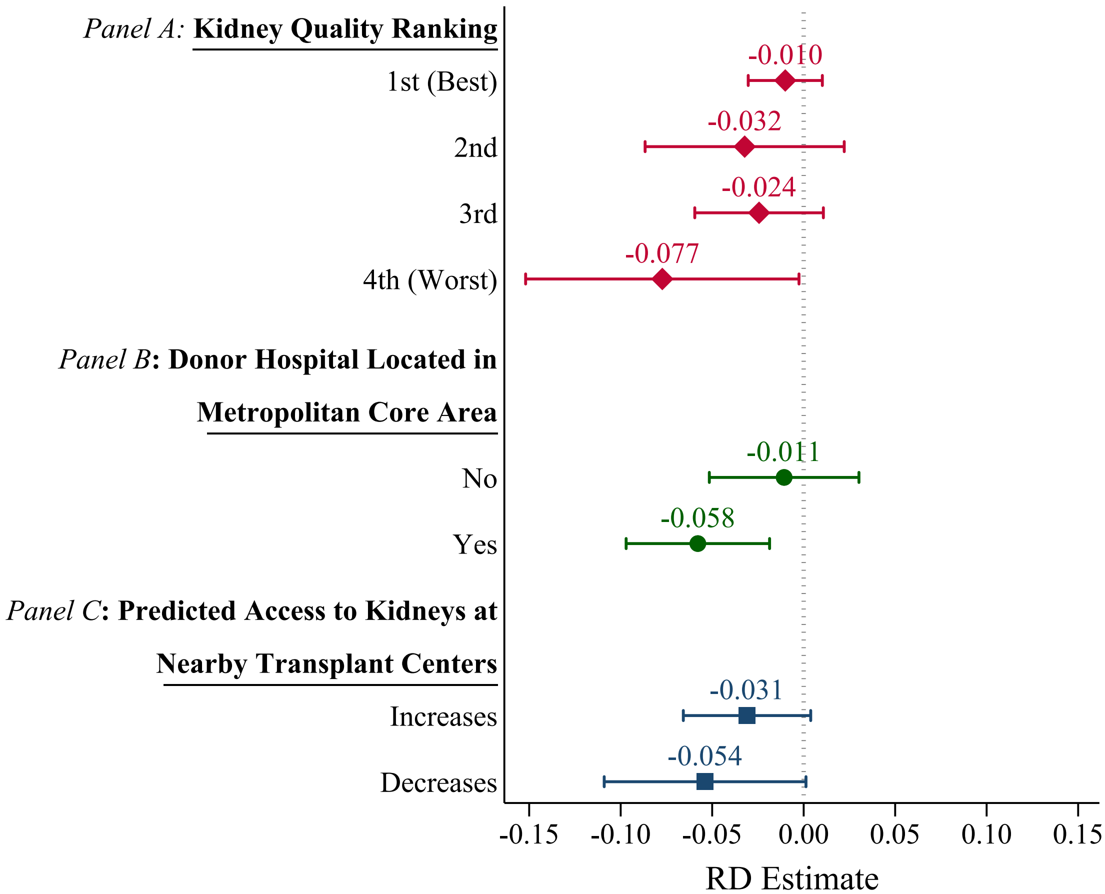


Figure 2.5. Heterogeneous Effects on the Likelihood a Kidney is Discarded

Notes: This figure plots β from Eq. 2.3 and corresponding 95% confidence intervals when an indicator of discarded kidneys is used as an outcome variable. Tables 2.B.8-2.B.10 provide the corresponding RD estimates. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. For more information on sample, see the notes to Table 2.1.

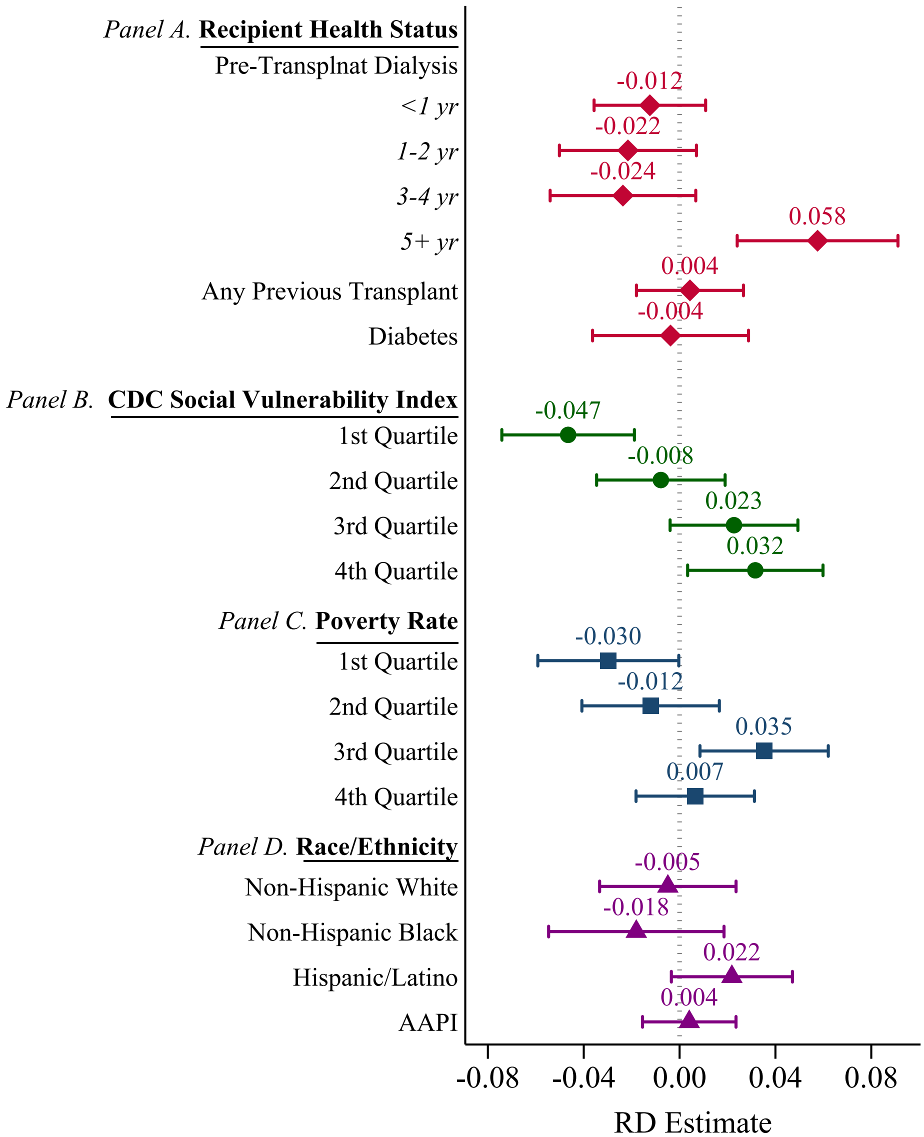
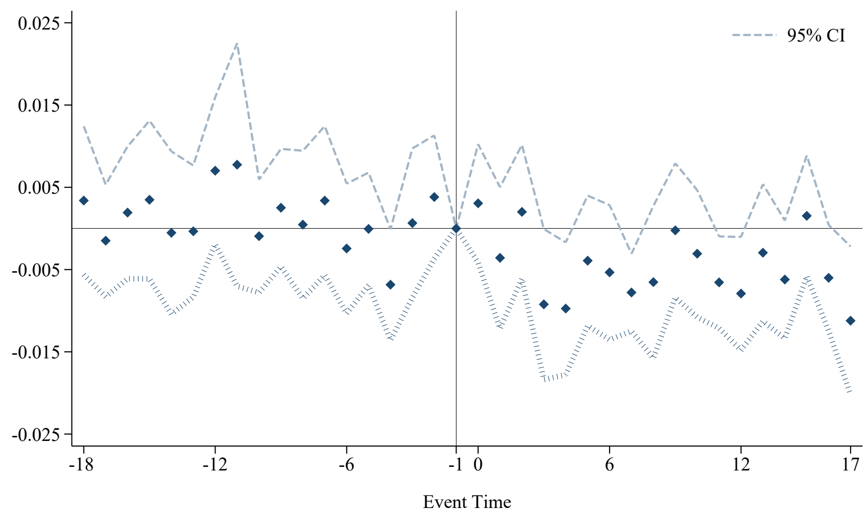


Figure 2.6. Effects on the Composition of Kidney Transplant Recipients

Notes: This figure presents β from Eq. 2.3 and corresponding 95% confidence intervals. Panel A presents the RD estimates when an indicator of four subgroups categorized by pre-transplant dialysis duration is used as the dependent variable: (1) zero years, (2) one to two years, (3) three to four years, and (4) five years or more. Panel B provides the RD estimates when an indicator for quartile groups based on the CDC social vulnerability index of counties where recipients live at the time of their transplant as the dependent variable. Panel C reports the RD estimates when an indicator for quartile groups based on the poverty rate of counties where recipients live at the time of their transplant as the dependent variable. Panel D provides the RD estimates when Eq. 2.3 is estimated by setting dependent variables as indicators for the race and ethnicity of transplant recipients. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. The sample consists of deceased donor kidneys in Table 2.3 used for transplant. Appendix Tables 2.B.12-2.B.14 provide the corresponding regression tables.

(a) Deaths Among Candidates on the Waitlist



(b) All Candidate Deaths Within 5 Years of Entering Waitlist

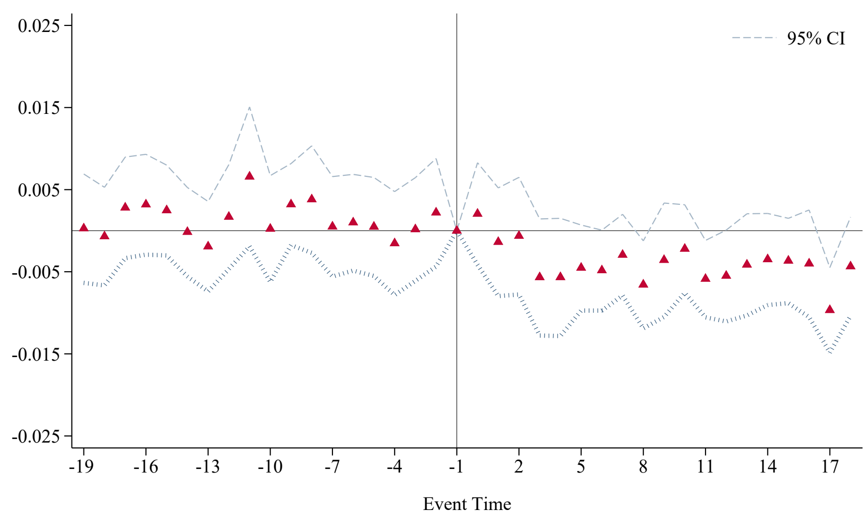
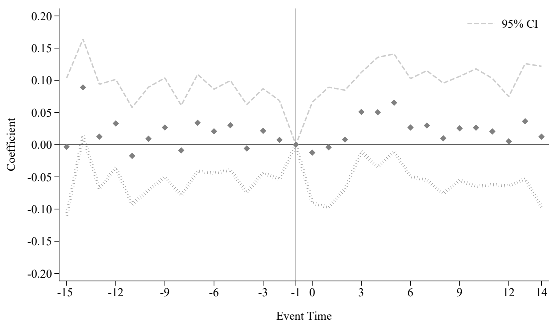


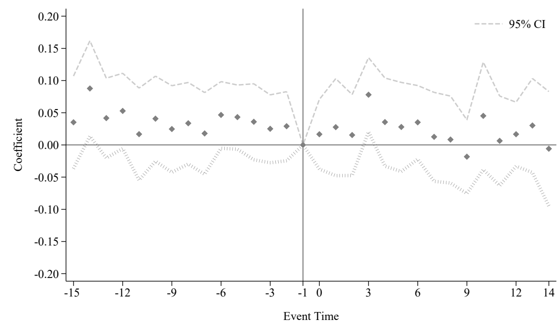
Figure 2.7. Effects on Deaths Among Transplant Candidates

Notes: This figure displays the coefficients and their corresponding 95% confidence intervals obtained from the event-study design specified in Eq. 2.5. The outcomes are the monthly number of deaths on the waitlist (Panel A) and the number of transplant candidates' deaths within five years of waitlist registration per transplant center (Panel B). Event time is defined as the number of months since February 2021, a month before the policy change. The sample is limited to transplant centers with active transplant candidates prior to the policy change and not located in DSAs that did not experience any change in OPO affiliation through the sample period.

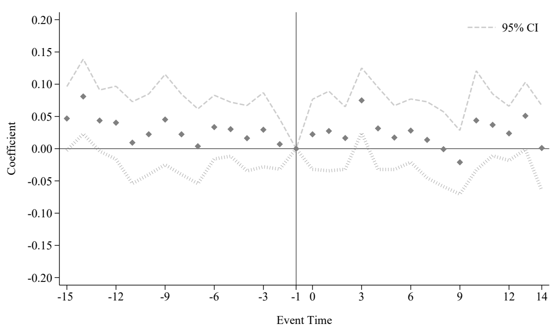
(a) within 3 months of transplant



(b) within 6 months of transplant



(c) Within 9 months of transplant



(d) Within 12 months of transplant

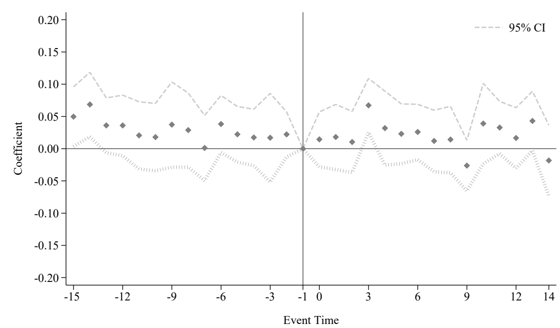


Figure 2.8. Event Study Figures on Post-Transplant Adverse Health Outcomes

Notes: This figure presents the coefficients and 95% confidence intervals from regressions estimation in Eq. 2.5. I define post-transplant adverse health outcomes as 1) deaths, 2) graft failure, and 3) resuming maintenance dialysis. The dependent variables used to construct each sub-figure are the number of transplant recipients ever experienced adverse health outcomes within 3, 6, 9, and 12 months of transplant, respectively. Event time refers to the number of months between transplant date and February 2021, a month prior to the policy change. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S.

Table 2.1. Descriptive Statistics

	(1) Mean
Panel A: Deceased Donor Kidneys *(N=23,466)*	
Share of discarded kidneys	0.230
a) Discarded due to list exhaustion	0.140
b) Discarded due to kidney/donor quality	0.065
c) Others	0.024
Blood O Type	0.480
KDPI 86-100%	0.165
Donor Hospital Characteristics	
Located in metropolitan core	0.576
Predicted decrease in access to kidneys at nearby transplant centers	0.451
Panel B: Transplant Recipients *(N=17,911)*	
Non-Hispanic white	0.375
Non-Hispanic black	0.344
Hispanic/Latino	0.197
Wait time (in days)	1580.372
County of residence	
CDC Social vulnerability index	0.561
Poverty rate	14.157
Panel C: Transplant Centers *(N=222)*	
Average active candidates per quarter	423.336
Centers with predicted increase in access to kidneys after the reform	0.478

Notes: The sample is drawn from the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. In Panel A, each observation is a donated kidney from a deceased donor. The sample is limited to (i) adult donors (18 or above), (ii) initial offer was made $\pm$ 196 days (28 weeks) from the cut-off, (iii) donors whose kidney biopsy information is complete, (iv) donor hospital located outside Alaska, (v) donor hospitals which did not experience any change in OPO affiliation through the sample period. "Share of kidneys discarded" is calculated by dividing the number of kidneys not used for transplant by the number of kidneys recovered for transplant. I use the reason for discard code for organ disposition to calculate the share of kidneys discarded: a) as they waited too long on the waitlist ("discarded due to list exhaustion"), b) due to organ quality concerns ("discarded due to donor quality"), and c) due to other factors ("others"). In Panel B, the sample covers 17,911 transplant recipients who (1) received transplants using kidneys included in the sample for Panel A (2) with non-missing candidate ID. In Panel C, descriptive statistics for transplant centers included in the transplant-center sample are provided. These transplant centers have at least one candidate waiting for a deceased donor kidney transplant prior to the policy change.

Table 2.2. Validity Test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Kidney Quality Ranking (KDPI)				Donor Blood Type				Donor Hospital Characteristics	
	1st (Best)	2nd	3rd	4th (Worst)	A	B	AB	O	Metropolitan Core Area	Nearby Transplant Centers Reduced Access to Kidneys
1(OfferDate$\geq$ c)	-0.0034	0.0183	-0.0043	-0.0106	0.0005	0.0102	0.0168	-0.0275	0.0074	-0.0119
	(0.0172)	(0.0135)	(0.0181)	(0.0153)	(0.0175)	(0.0073)	(0.0124)	(0.0207)	(0.0208)	(0.0212)
Mean, Pre-policy	0.186	0.134	0.519	0.161	0.378	0.031	0.104	0.486	0.571	0.478
Observations	23,466	23,466	23,466	23,466	23,466	23,466	23,466	23,466	23,466	23,466
Controls	No	No	No	No	No	No	No	No	No	No
Bandwidth	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days
Degree of polynomial	1	1	1	1	1	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3. Each column represents a separate regression in which the dependent variable is one of donor characteristics, as reported in the column headings. The sample includes deceased donor kidneys for which the initial offer was made $\pm$ 196 days (28 weeks) from the cut-off, March 15, 2021. The cut-off date is March 15, 2021. As the allocation system classifies kidneys into four subgroups based on Kidney Donor Profile Index (KDPI) to generate a nationwide waitlist, Columns 1-4 provide the RD estimates when the dependent variable is an indicator for (a) kidneys with KDPI 0-20% ("1st best"), (b) 21-34%, (c) 35-85%, and (d) 86-100% ("4th worst"). "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. For more information on the sample, see the notes to Table 2.1. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3. Effects on Use of Deceased Donor Kidneys

	(1)	(2)	(3)	(4)	(5)
	1(Transplanted Within DSA)	1(Discarded Kidney)	Reason Code for Being Discarded		
			1(Waited Too Long)	1(Organ quality)	1(Others)
1(OfferDate≥c)	-0.2341***	-0.0381***	-0.0331***	-0.0050	-0.0011
	(0.0157)	(0.0140)	(0.0119)	(0.0088)	(0.0053)
Mean, Pre-policy	0.548	0.223	0.126	0.070	0.025
Observations	23,466	23,466	23,466	23,466	23,466
Controls	Yes	Yes	Yes	Yes	Yes
Bandwidth	± 196 days	± 196 days	± 196 days	± 196 days	± 196 days
Degree of polynomial	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3. Column (1) provides the impact on an indicator of kidneys finding recipient within the donor service area of donor hospital. Column (2) provides the impact on an indicator of discarded kidneys. Columns (3)-(5) report the impact on indicators of kidneys (a) discarded as they waited too long on the waitlist, (b) discarded due to organ quality concerns, and (c) discarded due to other factors, respectively. The sample includes deceased donor kidneys for which the initial offer was made ± 196 days (28 weeks) from the cut-off, March 15, 2021. The running variable is the initial offer date of kidneys. The cut-off date is March 15, 2021. Control variables include (a) gender of donor, (b) race/ethnicity of donor, (c) blood type of donor, and (d) donor's hypertension status. 'Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. For more information on the sample, see the notes to Table 2.1. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4. Robustness Check

	(1)	(2)	(3)	(4)	(5)	(6)
	1(Transplanted	1(Transplanted	1(Discarded	Reason Code for Being Discarded		
	Within DSA)	Within OPTN Region)	Kidney)	1(Waited Too Long)	1(Organ quality)	1(Others)
Panel A. Main Specification						
1(OfferDate≥c)	-0.2349***	-0.1157***	-0.0384***	-0.0334***	-0.0051	-0.0011
	(0.0156)	(0.0151)	(0.0140)	(0.0120)	(0.0089)	(0.0053)
Panel B. No Covariates						
1(OfferDate≥c)	-0.2351***	-0.1154***	-0.0379***	-0.0325***	-0.0054	-0.0011
	(0.0154)	(0.0153)	(0.0144)	(0.0120)	(0.0090)	(0.0053)
Panel C. No Weighting						
1(OfferDate≥c)	-0.2245***	-0.1028***	-0.0409***	-0.0284**	-0.0113	-0.0021
	(0.0145)	(0.0145)	(0.0131)	(0.0111)	(0.0080)	(0.0050)
Panel D. Local Linear Specification						
1(OfferDate≥c)	-0.2371***	-0.1184***	-0.0378***	-0.0343***	-0.0039	-0.0009
	(0.0163)	(0.0156)	(0.0145)	(0.0124)	(0.0092)	(0.0055)
Panel E. Bandwith ± 22 weeks						
1(OfferDate≥c)	-0.2386***	-0.1223***	-0.0357**	-0.0358***	-0.0014	-0.0001
	(0.0174)	(0.0165)	(0.0156)	(0.0134)	(0.0099)	(0.0059)
Panel F. Bandwith ± 24 weeks						
1(OfferDate≥c)	-0.2378***	-0.1201***	-0.0369**	-0.0348***	-0.0029	-0.0007
	(0.0168)	(0.0160)	(0.0150)	(0.0129)	(0.0095)	(0.0057)
Panel G. Bandwith ± 26 weeks						
1(OfferDate≥c)	-0.2369***	-0.1183***	-0.0379***	-0.0343***	-0.0040	-0.0009
	(0.0162)	(0.0155)	(0.0145)	(0.0124)	(0.0092)	(0.0055)
Panel G. Bandwith ± 30 weeks						
1(OfferDate≥c)	-0.2329***	-0.1130***	-0.0390***	-0.0326***	-0.0063	-0.0012
	(0.0151)	(0.0147)	(0.0135)	(0.0116)	(0.0086)	(0.0052)
Panel H. Bandwith ± 32 weeks						
1(OfferDate≥c)	-0.2306***	-0.1108***	-0.0397***	-0.0323***	-0.0069	-0.0014
	(0.0147)	(0.0143)	(0.0131)	(0.0112)	(0.0083)	(0.0050)
Panel I. Bandwith ± 34 weeks						
1(OfferDate≥c)	-0.2298***	-0.1097***	-0.0398***	-0.0312***	-0.0076	-0.0017
	(0.0143)	(0.0140)	(0.0128)	(0.0109)	(0.0081)	(0.0049)

Notes: This table provides the RD estimates of β in Eq. 2.3 using a number of alternatives to my baseline specification (Panel A): 1) excluding X_i (Panel B), 2) without the triangular weights (Panel C), 3) setting f() as a local linear function form (Panel D), and 4) using alternative choices of bandwidth length (Panels E-I). The sample consists of deceased donor kidneys recovered for transplant. The cut-off date is March 15, 2021. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. For more information on the sample, see the notes to Table 2.1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.5. Placebo Test: Non-Kidney Samples

	(1)	(2)	(3)	(4)
	1(Discarded)	Reason Code for Being Discarded		
		1(Waited Too Long)	1(Organ quality)	1(Others)
Panel A. Non-Kidney Organs (Heart, Lung)				
1(OfferDate≥c)	0.0059	-0.0054	0.0072	0.0015
	(0.0104)	(0.0046)	(0.0078)	(0.0057)
Observations	11,615	11,615	11,615	11,615
Panel B. Liver				
1(OfferDate≥c)	0.0119	-0.0063	0.0133	0.0015
	(0.0142)	(0.0062)	(0.0107)	(0.0076)
Observations	8,258	8,258	8,258	8,258
Panel C. Heart				
1(OfferDate≥c)	-0.0097	-0.0035	-0.0082	0.0016
	(0.0103)	(0.0034)	(0.0058)	(0.0071)
Observations	3,357	3,357	3,357	3,357

Notes: This table provides the RD estimates of β in Eq. 2.3 using the data on the placebo sample. The sample consists of livers, hearts, and lungs recovered for transplant from the donors included in the kidney-level data. The cut-off date is March 15, 2021. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. For more information on sample, see the notes to Table 2.1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6. Transplanted Kidney Characteristics – Travel Distance/Hours

	(1)	(2)	(3)	(4)
	Distance Between Donor Hospital and Transplant Center			Cold Storage
	Travel Distance (nm)	1(Within < 50 nm)	1(Within < 250 nm)	Hours
1(OfferDate≥c)	19.8353	-0.1819***	0.0351***	1.5598***
	(12.2534)	(0.0183)	(0.0127)	(0.2838)
Mean, Pre-policy	195.166	0.468	0.820	17.624
Observations	17,911	17,911	17,911	17,911
Controls	Yes	Yes	Yes	Yes
Bandwidth	± 196 days	± 196 days	± 196 days	± 196 days
Degree of polynomial	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3. The sample consists of deceased donor kidneys in Table 2.3 used for transplant. The running variable is the initial offer date of kidneys. Column (1) shows the effect on travel distance in nautical miles (nm; 1 nm = 1.15078 miles) between donor hospital and transplant center. Columns (2)-(3) present the effects on whether the transplant center is located within 50 and 250 nm of donor hospital, respectively. Column (4) reports the impact on cold ischemic hours, which measures the number of hours the kidney was in cold or chilled status (i.e., between being removed from the donor and the cold storage solution). Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. The cut-off date is March 15, 2021. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7. Effects on Transplant Candidates

	(1)	(2)	(3)	(4)	(5)
	Monthly Number of Transplant Candidates				
	Died on Waitlist	All Deaths (Within 5 years Joining Waitlist)	Received Deceased Donor Kidney Transplant	Received Living Donor Kidney Transplant	Newly Joined Waitlist
$\Delta Access_h * Post_t$	-0.01675***	-0.01551***	0.01428**	0.00176	0.00208
	(0.00614)	(0.00310)	(0.00680)	(0.00513)	(0.00400)
Mean of Dep. Var (Pre)	1.959	2.950	5.812	1.901	12.077
Observation	6,660	6,660	6,660	6,660	6,660

Notes: This table reports the estimates of γ in Eq. 2.4. The transplant center-month data includes transplant centers with active transplant candidates prior to the policy change, not located in donor service areas that did not experience any change DSA affiliation through the sample period. Column 1 presents the impact on the number of deceased kidney transplants. Columns 2 and 3 reports the impact on the number of transplant candidates died on the waitlist and transplant candidates died within 5 years of entering the waitlist, respectively. Column 4 reports the impact on the number of living donor transplants. Column 5 reports the impact on the number of newly added candidates. "Mean of Dep-Var" is the average of outcome variable between -1 and -15 months prior to the policy change. Standard errors clustered at transplant center level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

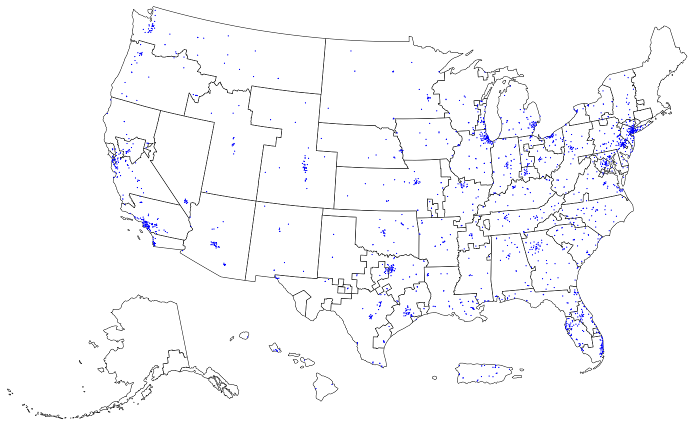
Table 2.8. Effects on Transplant Recipients

	(1)	(2)	(3)	(4)
	Transplant Recipient Adverse Health Outcomes			
	$\leq$3 months	$\leq$6 months	$\leq$9 months	$\leq$12 months
$\Delta Access_h * Post_t$	0.00406	-0.00659	-0.00344	-0.00662
	(0.01482)	(0.01404)	(0.00978)	(0.00818)
Mean of Dep. Var (Pre)	0.175	0.272	0.339	0.410
Observation	6,660	5,328	6,660	6,660

Notes: This table reports the estimates of γ in Eq. 2.4. I define post-transplant adverse health outcomes as 1) deaths, 2) graft failure, 3) resuming maintenance dialysis within 3, 6, 9, and 12 months of deceased donor kidney transplant. Columns (1)-(4) report the effects on the number of deaths of recipients within 3, 6, 9, 12 months of transplant date. "Mean of Dep-Var" is the average of outcome variable between -1 and -15 months prior to the policy change. Figure 2.8 plots the estimates from Eq. 2.5 when monthly number of transplant recipients who experienced these adverse health outcomes within 3, 6, 9, and 12 months is used as a dependent variable in Eq. 2.5. Standard errors clustered at transplant center level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

9 Appendix Figures and Tables

(a) Donor Hospitals



(b) Transplant Centers

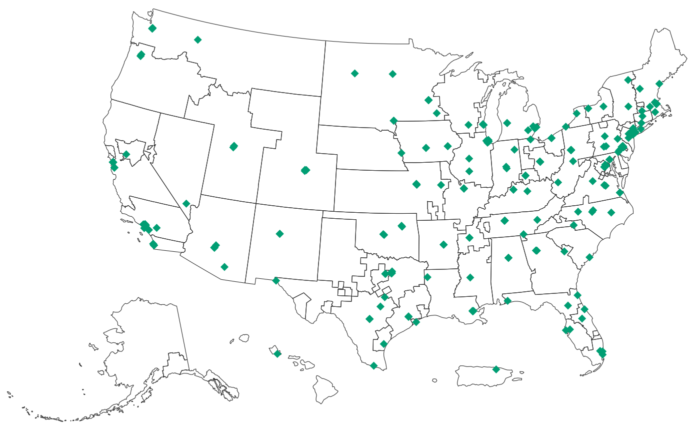


Figure 2.B.1. Map of Donor Hospital and Transplant Center

Notes: This figure shows geographic locations of 1,749 donor hospitals (Panel A) and 222 transplant centers (Panel B). Each dot shows the geographic coordinates of donor hospitals and transplant centers.

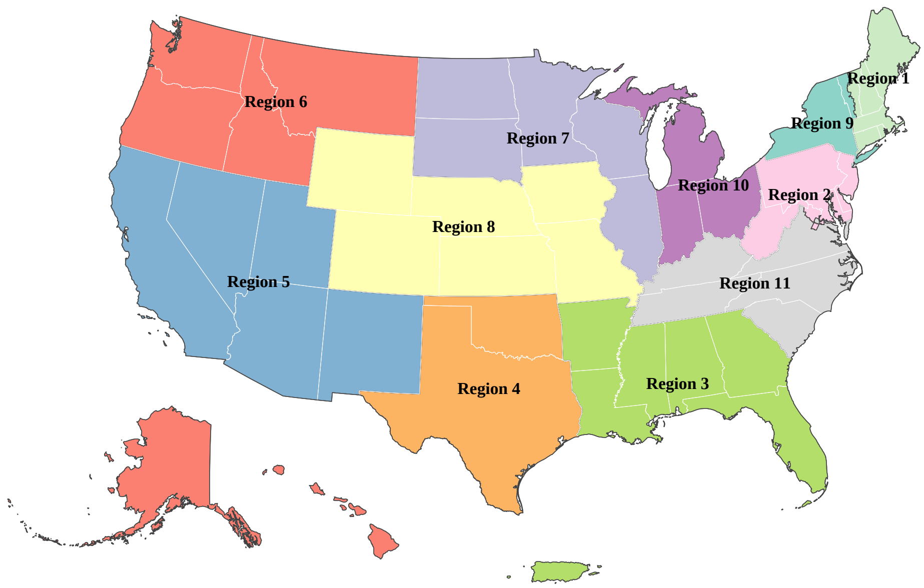


Figure 2.B.2. Map of OPTN Region

Notes: 11 OPTN regions were established in 1986 and there were no changes since then (Source: https://optn.transplant.hrsa.gov/about/regions/). For more information about OPTN regions, see section 2 .

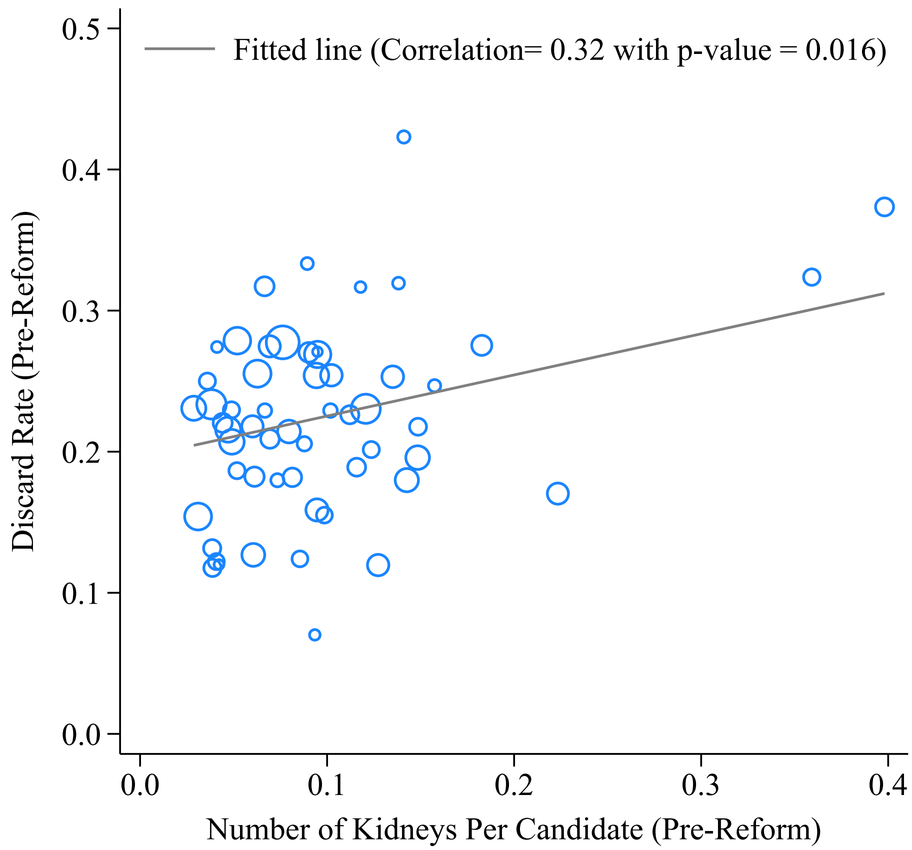


Figure 2.B.3. Kidney Discard Rate and Access to Kidneys Prior to the Reform

Notes: This figure displays a scatterplot illustrating the relationship between the discard rate and average access to kidneys by "pre-reform" service area. Access to kidneys is defined as the quantity of available kidneys per candidate in a given "pre-reform" service area during the pre-reform period (i.e., January 2019-February 2021). Each data point on the scatterplot represents a "pre-reform" service area, with the size of the data point being proportionally weighted by the monthly number of kidneys recovered for transplant during the pre-reform period (i.e., January 2019-February 2021). Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S.

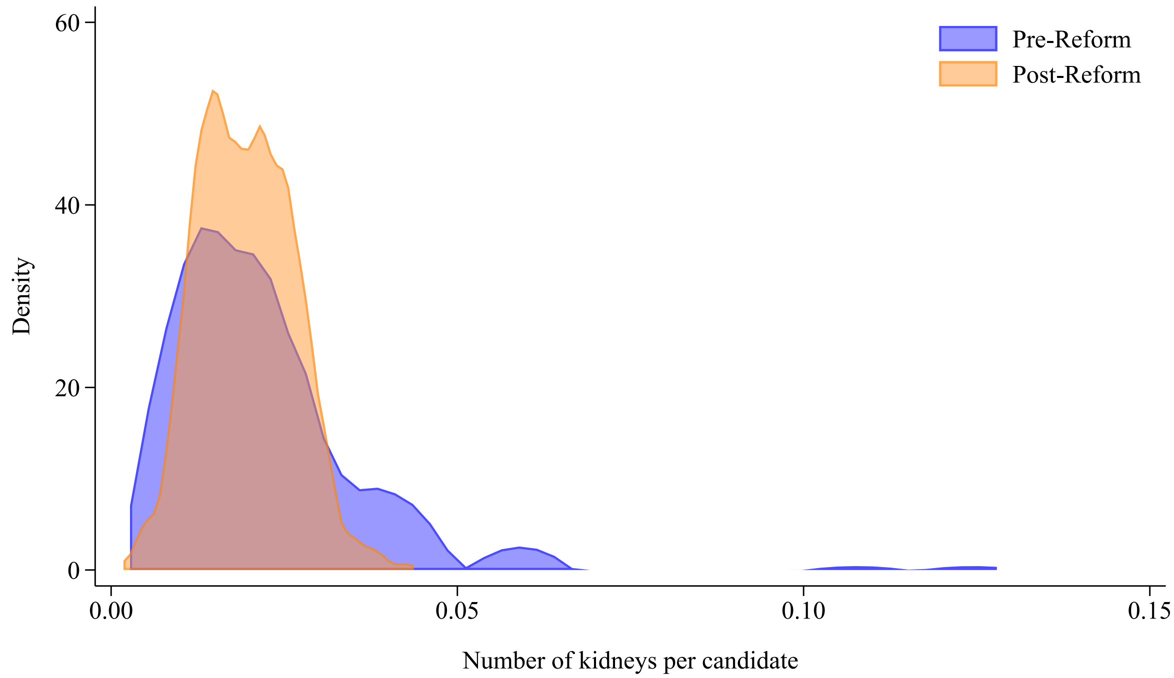
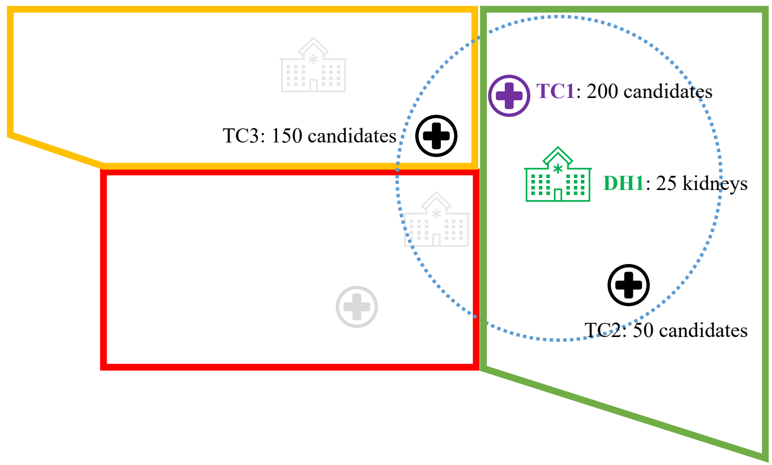


Figure 2.B.4. Distribution of Predicted Access to Kidneys

Notes: The figure presents the kernel density of predicted access to kidneys across transplant centers before and after the reform. Predicted access to kidneys is defined as the quantity of available kidneys per candidate in a given transplant center during the pre-reform period (i.e., January 2019-February 2021). Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S.

(a) Panel A: Donor Hospital in Green DSA



(b) Panel B: Donor Hospital in Red DSA

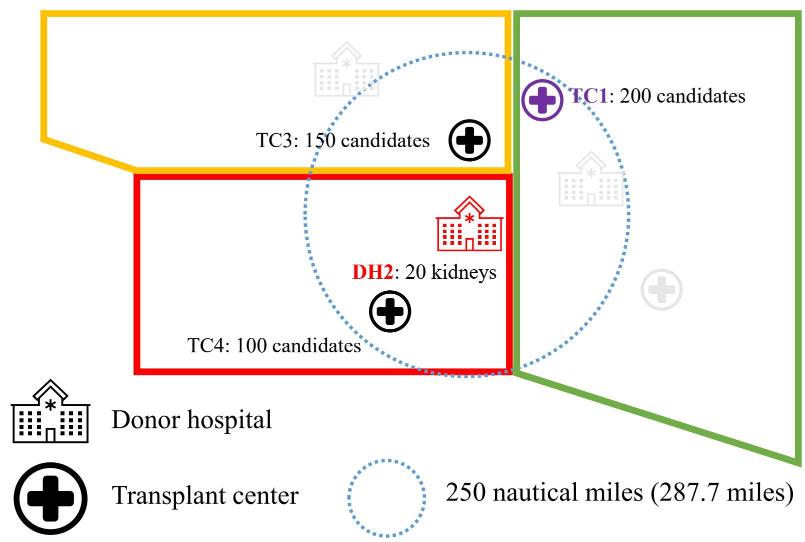


Figure 2.B.5. Numerical Example of Treatment Intensity Calculation

Notes: Consider an example where there are three DSA areas in a county that consists of three DSAs and there are two transplant centers in the green DSA, TC1 and TC2. Within this green DSA, a single donor hospital named DH1 recovers an average of 25 deceased donor kidneys per month. TC1 has approximately 200 active transplant candidates, while TC2 has around 50 active candidates. Given that there is only one donor hospital in the green DSA, transplant candidates listed at TC1 or TC2 receive priority access to kidneys recovered from DH1 compared to candidates listed at transplant centers outside the green DSA. Prior to the policy change, TC1 has access to 20 kidneys from DH1, which was calculated as $(= 25 * (200/(200+50)))$. In the post-period, TC1 now belongs to the initial offer group of two donor hospitals: DHI, located within the same DSA, and DH2, located in the red DSA, which recovers 20 kidneys monthly. In Panel A, DH1 is located within a 250nm radius of three transplant centers (TC1, TC2, TC3), with a total of 400 registered candidates across these centers. Similarly, DH2 is located within 250nm of three transplant centers (TC1, TC3, TC4), with a total of 450 registered candidates across these centers, in Panel B. As a result, TC1 is predicted to receive 12.5$(=25 * (200/400))$ kidneys from DH1 and 8.9 $(\approx 20 * (200/450))$ kidneys from DH2 in the post-period. Therefore, the predicted change in available deceased donor kidneys for TC1 is 1.4 kidneys.

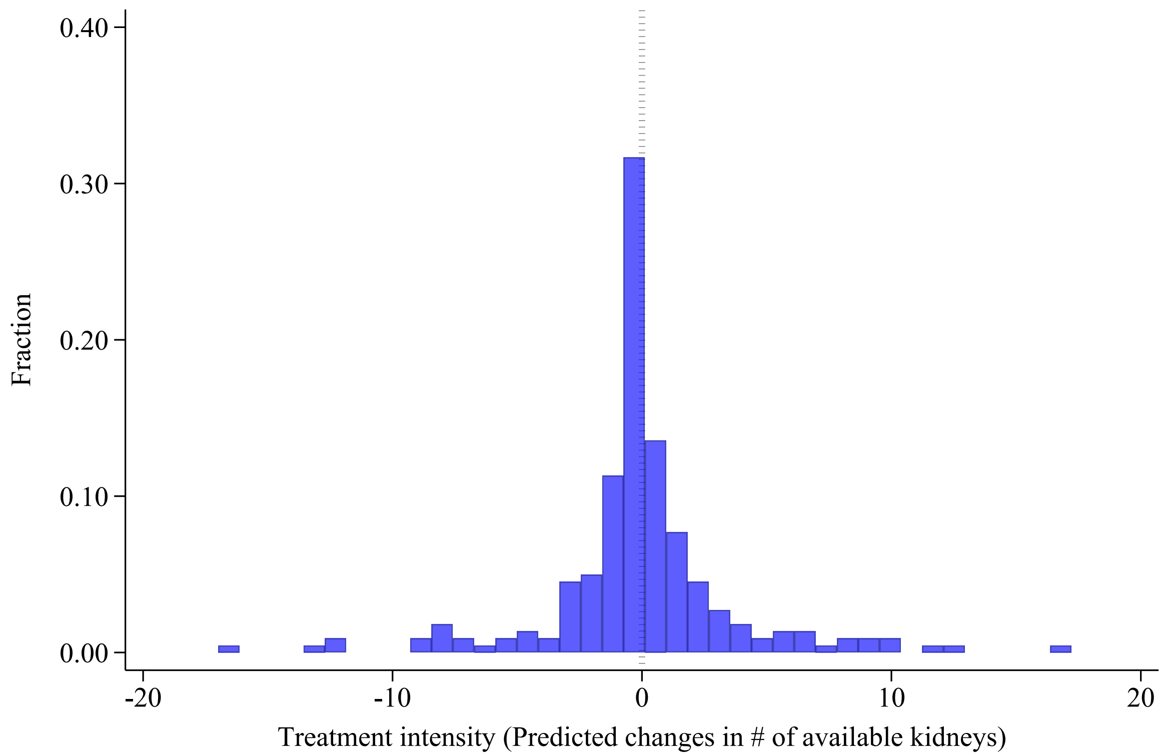


Figure 2.B.6. Histogram of Predicted Changes in Kidney Access by Transplant Center

Notes: This figure provides a histogram of treatment intensity measure with a bin size 40. Eq. 2.1 is used to calculate this measure. Appendix Figure 2.B.5 presents a numerical example of calculating the predicted changes in available deceased donor kidneys for each transplant center. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S.

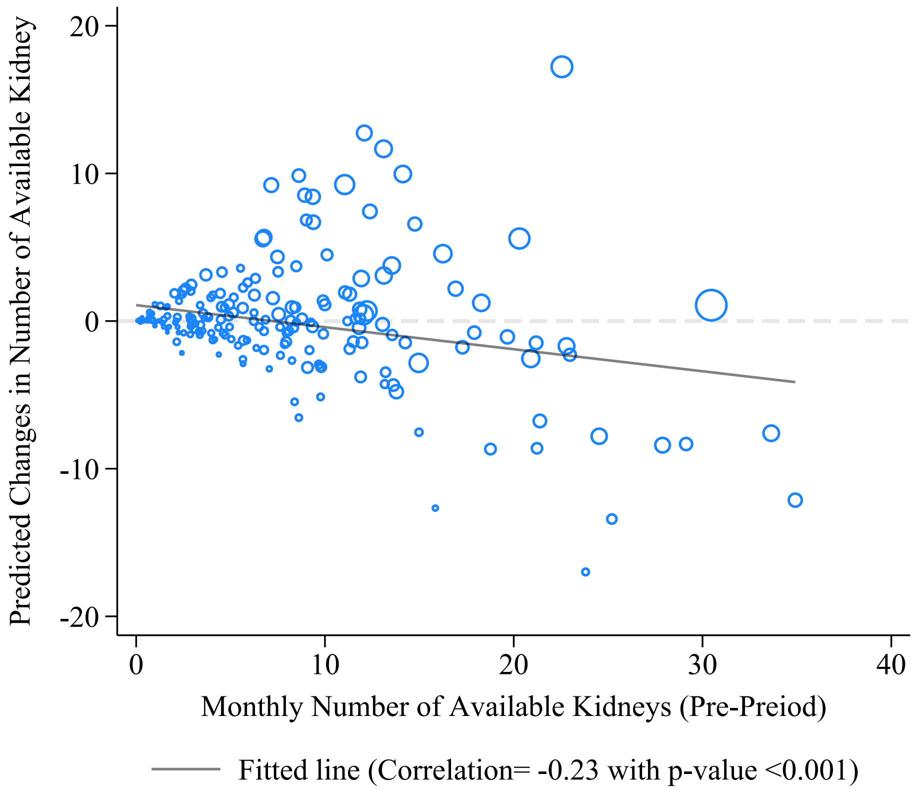
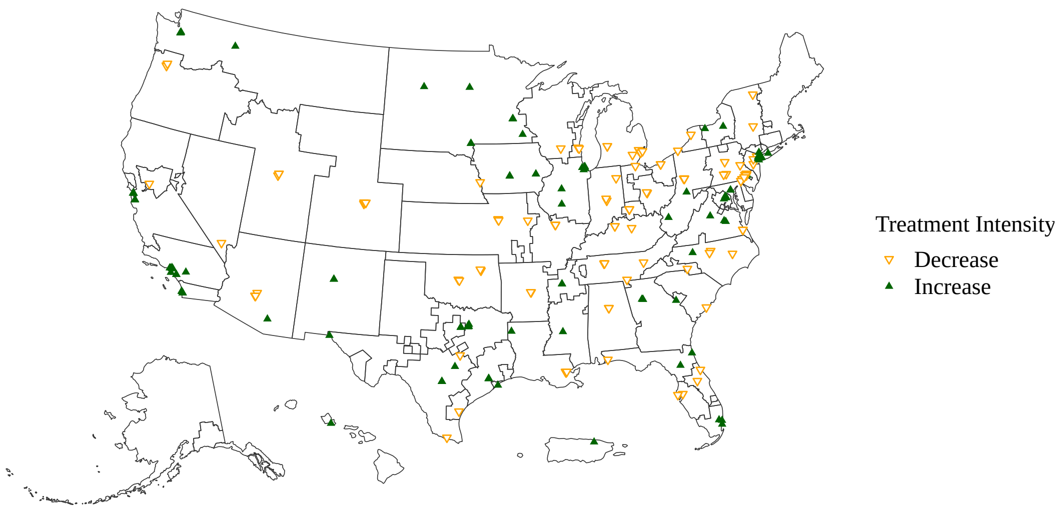


Figure 2.B.7. Predicted Changes in Kidney Access and Pre-Reform Kidney Access

Notes: This figure displays a scatterplot illustrating the relationship between the monthly number of avaiable kidneys during pre-reform period and the predicted changes in kidney access by transplant center. Each data point on the scatterplot represents a transplant center. The correlation between these two measures, weighted by the size of the transplant center, is - 0.23 (p-value$<$0.001). Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S.

(a) By Transplant Center



(b) By "Pre-Reform Service Area"

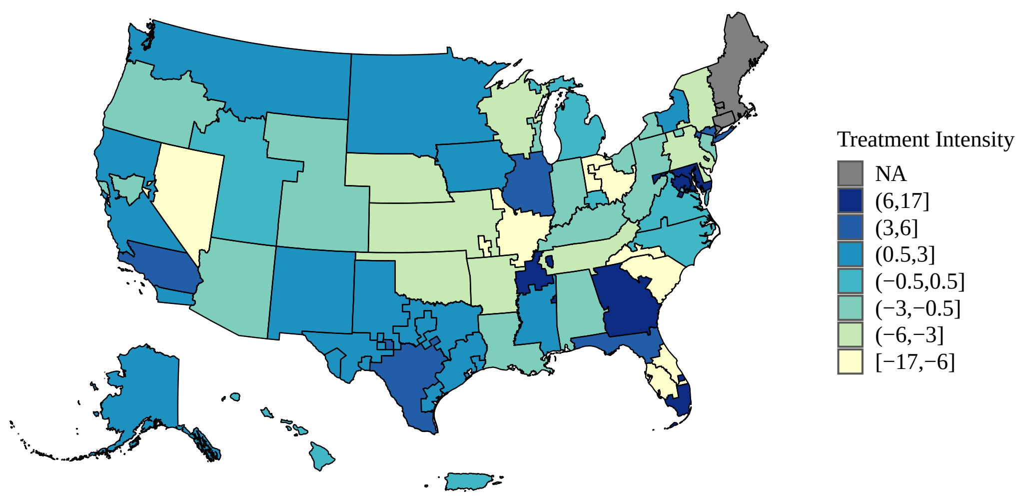


Figure 2.B.8. Map of Predicted Change in Kidney Access

Notes: Eq. 2.1 calculates the predicted change in the number of available deceased donor kidneys for each transplant center based on pre-reform data. Appendix Figure 2.8d illustrates the average treatment intensity mapped by transplant center. Appendix Figure 2.B.8b depicts the average treatment intensity by Donor Service Area (DSA). Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys, transplant candidates, and transplant centers in the U.S.

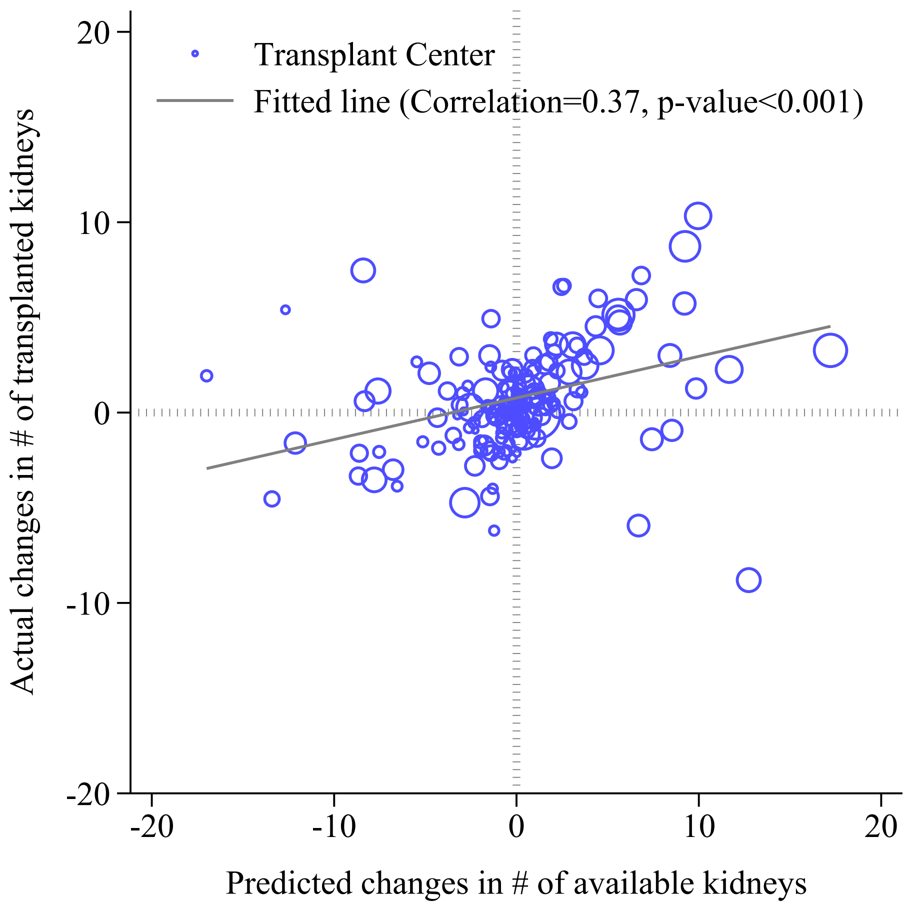


Figure 2.B.9. Predicted and Actual Changes in Kidney Access

Notes: A scatterplot compares the predicted changes in available kidneys ("treatment intensity") against the actual changes in average deceased donor kidney transplant recipients within 15 months of the policy change. Predicted changes in the number of available deceased donor kidneys for transplant center h is calculated based on Eq. 2.1. Appendix Figure 2.B.5 presents a numerical example of calculating the predicted changes in available deceased donor kidneys for each transplant center. The correlation between the predicted and actual changes, weighted by the size of the transplant center, is 0.37 (p-value<0.001).

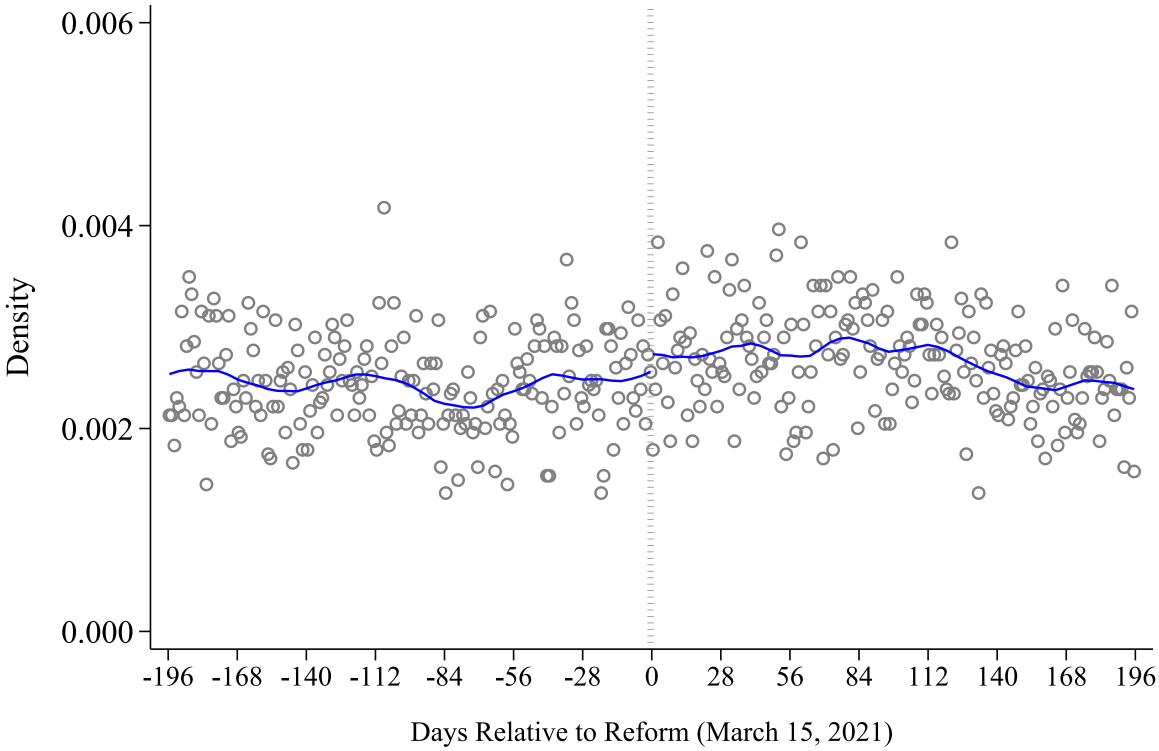


Figure 2.B.10. McCrary Density Test

Notes: The figure presents the density of deceased donor kidneys by their initial offer date. Each dot represents a single day. I use DCDensity.ado, which was written by Justin McCrary and Brian Kovak, to estimate a McCrary density test in Stata. The p-value of the McCrary density test is 0.1603. For more information on the sample, see the notes to Table 2.1.

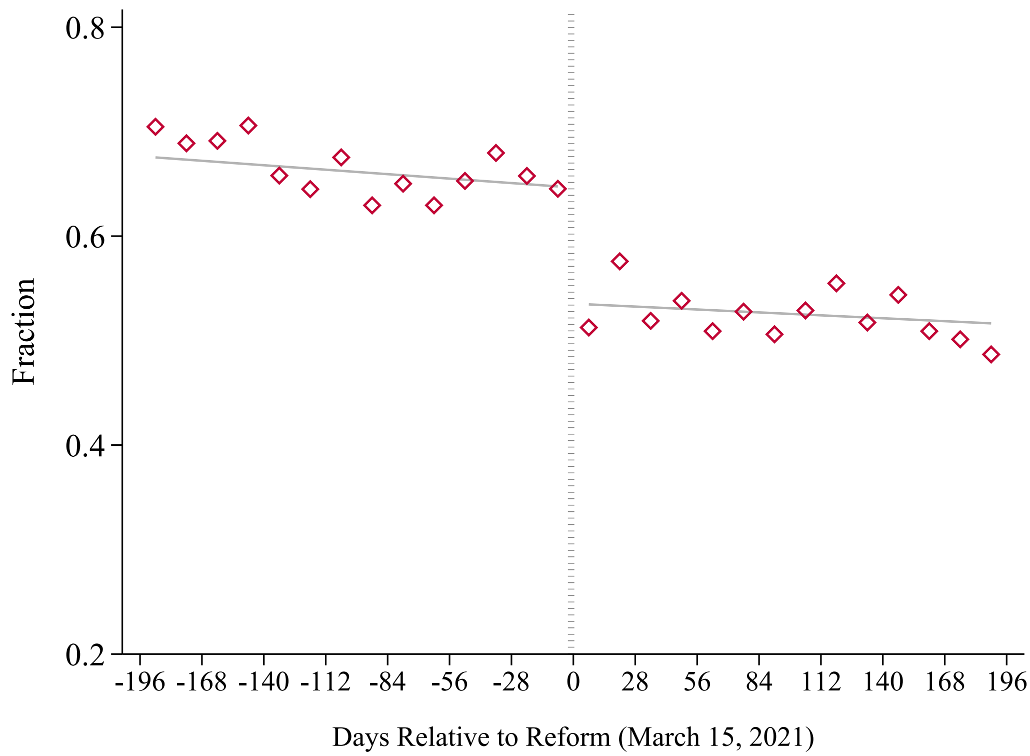


Figure 2.B.11. Kidneys Finding the Recipients Within the Same OPTN Region

Notes: This figure shows the share of kidneys finding to the recipients within OPTN region by initial offer date relative to the allocation reform on March 15, 2021. Each dot represents two weeks. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. For more information on sample, see the notes to Table 2.1.

Figure 2.B.12. Heterogeneity by Kidney Quality

Notes: This figure shows the share of deceased donor kidneys that are discarded by initial offer date relative to the allocation reform on March 15, 2021 by kidney quality measure. Kidney Donor Profile Index (KDPI) is a kidney quality measure used to evaluate the expected likelihood of graft failure for a given for deceased donor kidney relative to other kidneys recovered in the last year. The sample is divided into four based on four KDPI categories used in the allocation system. Each dot represents two weeks. The date on the x-axis is the initial offer date. Each dot represents two weeks. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. For more information on sample, see the notes to Table 2.1.

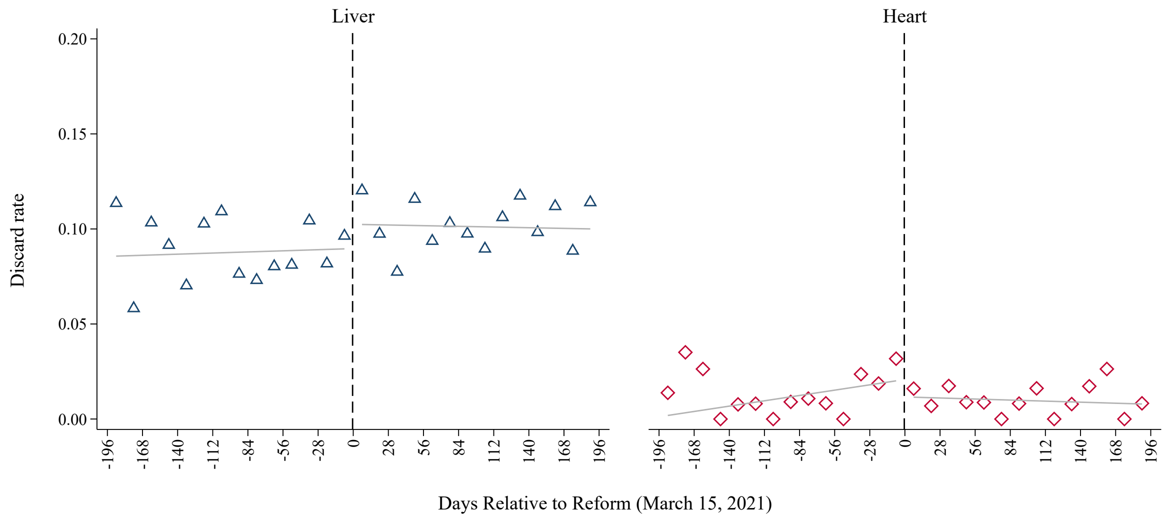


Figure 2.B.13. Placebo Test: Discard Rates for Liver and Heart

Notes: This table provides the RD estimates of β in Eq. 2.3 using the data on the placebo sample. The sample consists of livers, hearts, and lungs recovered for transplant from the donors included in the kidney-level data. The cut-off date is March 15, 2021. “Mean, Pre-policy” is the average of outcome variable between -1 and -28 weeks prior to the policy change. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S.

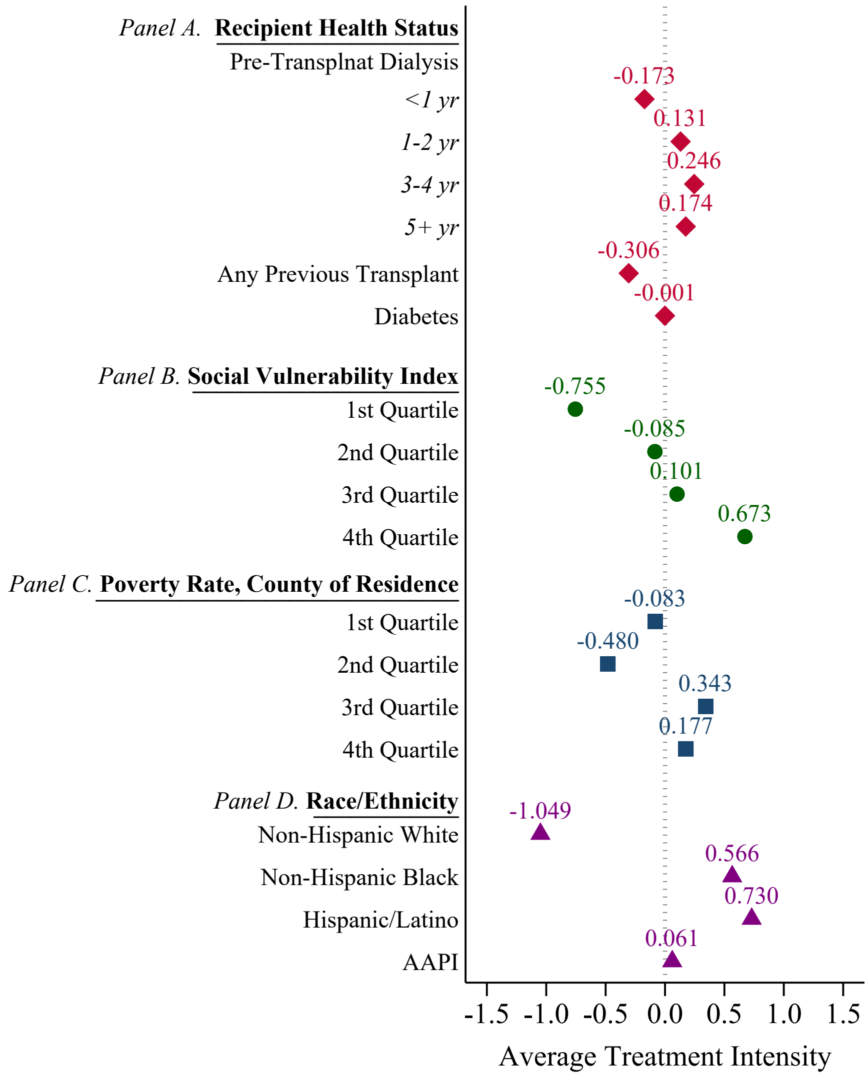
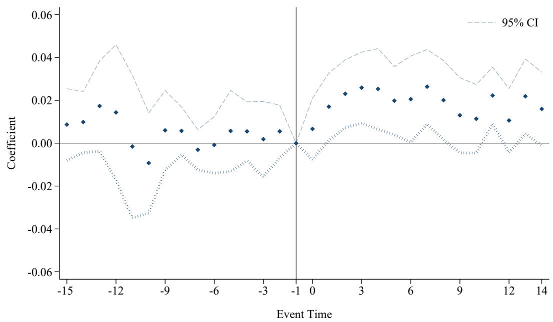


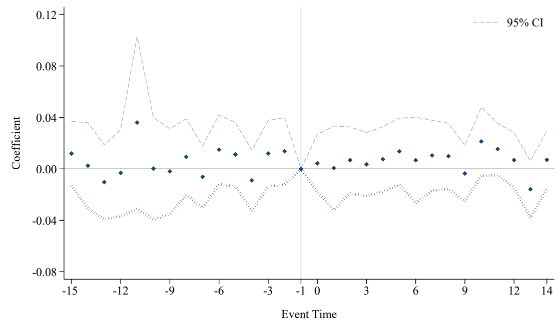
Figure 2.B.14. Average Treatment Intensity across Transplant Candidate Subgroups

Notes: This figure presents the average treatment intensity for different transplant candidate subgroups: pre-dialysis duration/pre-transplant history/diabetes status (Panel A), CDC social vulnerability index (Panel B), poverty rate in 2019 (Panel C), and racial/ethnic groups of candidates (Panel D). The average treatment intensity for different subgroups is calculated based on transplant candidates who remain on the waitlist prior to the policy implantation date, March 15, 2021. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S.

(a) Deceased Donor Kidney Transplant



(b) Living Donor Transplant



(c) Newly Joined to Waitlist

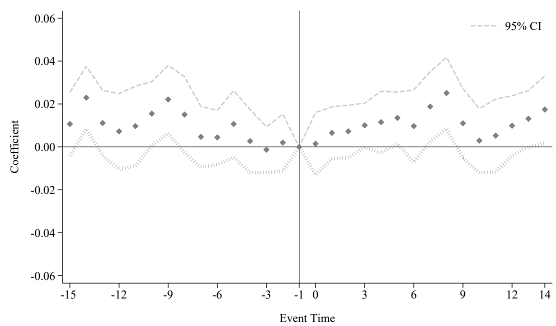


Figure 2.B.15. Event Study Figures on Outcomes of Transplant Centers

Notes: This figure displays the coefficients and 95% confidence intervals from regressions estimation in Eq. 2.5. Event time refers to the number of months from February 2021, a month before the policy change. The sample is limited to transplant centers with active transplant candidates at least 19 months prior to the policy change and not located in DSAs that did not experience any change in OPO affiliation through the sample period. Figure 2.B.15a plots the event study coefficients and 95% confidence intervals when the outcome is the number of decease donor kidney transplant recipients. Figure 2.B.15b plots the event study coefficients when the number of living donor kidney transplant recipients is used as a dependent variable in Eq. 2.5. Figures 2.7a plots the estimates when the number of transplant candidate deaths who died on the waitlist is used as a dependent variable in Eq. 2.5. Figure 2.B.15c plots the event study estimates when the number of newly added candidates on the waitlist is used as a dependent variable in Eq. 2.5. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S.

Table 2.B.1. Kidney Allocation Point Calculation

(a) Before March 15, 2021

Transplant Candidate	Points Awarded
1) Waiting Time (in days)	$\frac{1}{365}$ points
2) Age 0-10, zero-HLA mismatch	4 points
3) Age 11-17, zero-HLA mismatch	3 points
4) Age 0-10, KDPI 0-35% kidney	1 point
5) Prior living donor	4 points
6) CPRA 20-100%	Table 2.B.2
7) Single HLA-DR mismatch	1 point
8) Zero HLA-DR mismatch	2 points

(b) On/After March 15, 2021

Transplant Candidate	Points Awarded
1) Waiting Time (in days)	$\frac{1}{365}$ points
2) Age 0-10, zero-HLA mismatch	4 points
3) Age 11-17, zero-HLA mismatch	3 points
4) Age 0-10, KDPI 0-35% kidney	1 point
5) Prior living donor	4 points
6) CPRA 20-100%	Table 2.B.2
7) Single HLA-DR mismatch	1 point
8) Zero HLA-DR mismatch	2 points
9) Distance between donor hospital and transplant center	
(i) $<$ 250nm	$2-\left(\frac{2}{250}\times \text{distance}\right)$
(ii) 250nm -2499 nm	$4-\left(\frac{4}{2500-250}\times (\text{distance}-250)\right)$

Notes: This table shows the kidney allocation point calculation used to rank each candidate within the candidate category before and after the policy change.

Table 2.B.2. Kidney Points Based on Transplant Candidate's CPRA Score

CPRA Score of Transplant Candidate	Points Awarded
0-19	0
20-29	0.08
30-39	0.21
40-49	0.34
50-59	0.48
60-69	0.81
70-74	1.09
75-79	1.58
80-84	2.46
85-89	4.05
90-94	6.71
95	10.82
96	12.17
97	17.3
98	24.4
99	50.09
100	202.1

Notes: This table shows the calculation of the kidney points considers the distance between the donor hospital and transplant center entered the kidney points (0-2 points) starting from March 21, 2021.

Table 2.B.3. Validity Test: (1) Donor Demographics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Female	Age	Non-Hispanic White	Non-Hispanic Black	Hispanic /Latino	Body Mass Index	Recovered in Different State
1(OfferDate$\geq$ c)	0.0153	-0.8364	0.0100	-0.0080	-0.0006	-0.2623	-0.0065
	(0.0210)	(0.6239)	(0.0158)	(0.0142)	(0.0143)	(0.2987)	(0.0100)
Mean, Pre-policy	0.389	43.352	0.726	0.143	0.132	28.892	0.082
Observations	23,466	23,466	23,466	23,466	23,466	23,466	23,466
Controls	No	No	No	No	No	No	No
Bandwidth	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days
Degree of polynomial	1	1	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3. Each column represents a separate regression in which the dependent variable is one of donor characteristics, as reported in the column headings. The sample includes deceased donor kidneys for which the initial offer was made $\pm$ 196 days (28 weeks) from the cut-off, March 15, 2021. The cut-off date is March 15, 2021. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. For more information on sample, see the notes to Table 2.1. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.4. Validity Test: (2) Donor's Consent Mechanism

	(1)	(2)	(3)
	Written Consent	Driver license	Donor Registry
1(OfferDate$\geq$ c)	-0.0074	0.0044	-0.0072
	(0.0211)	(0.0161)	(0.0215)
Mean, Pre-policy	0.600	0.191	0.497
Observations	23,466	23,466	23,466
Controls	No	No	No
Bandwidth	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days
Degree of polynomial	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3. Each column represents a separate regression in which the dependent variable is one of donor characteristics, as reported in the column headings. The sample includes deceased donor kidneys for which the initial offer was made $\pm$ 196 days (28 weeks) from the cut-off, March 15, 2021. The cut-off date is March 15, 2021. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. For more information on sample, see the notes to Table 2.1. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.5. Validity Test: (3) Donor's Cause of Death

	(1)	(2)	(3)	(4)	(5)
	Brain	Circumstances of Death			
	Death	Natural Causes	MVA	Suicide/Homicide	Non-MVA/Others
1(OfferDate$\geq$ c)	0.0022	-0.0269	0.0172	-0.0120	0.0217
	(0.0154)	(0.0211)	(0.0122)	(0.0131)	(0.0167)
Mean, Pre-policy	0.715	0.464	0.114	0.137	0.285
Observations	23,466	23,466	23,466	23,466	23,466
Controls	No	No	No	No	No
Bandwidth	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days	$\pm$ 196 days
Degree of polynomial	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3. Each column represents a separate regression in which the dependent variable is one of donor characteristics, as reported in the column headings. The sample includes deceased donor kidneys for which the initial offer was made $\pm$ 196 days (28 weeks) from the cut-off, March 15, 2021. The cut-off date is March 15, 2021. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. For more information on sample, see the notes to Table 2.1. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.6. Validity Test: (4) Donor's Health Status

	(1)	(2)	(3)	(4)	(5)
	Donation after Circulatory Death	Kidney Donor Risk Index	Expanded Criteria Donor Kidney	Cancer History	HCV Positive
1(OfferDate≥ c)	-0.0102	-0.0224	-0.0225	0.0089	0.0054
	(0.0166)	(0.0167)	(0.0166)	(0.0074)	(0.0127)
Mean, Pre-policy	0.337	1.100	0.235	0.033	0.114
Observations	23,466	23,466	23,466	23,466	23,466
Controls	No	No	No	No	No
Bandwidth	± 196 days	± 196 days	± 196 days	± 196 days	± 196 days
Degree of polynomial	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3. Each column represents a separate regression in which the dependent variable is one of donor characteristics, as reported in the column headings. The sample includes deceased donor kidneys for which the initial offer was made ± 196 days (28 weeks) from the cut-off, March 15, 2021. Standard errors are clustered at the running variable The cut-off date is March 15, 2021. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. For more information on sample, see the notes to Table 2.1. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.7. Results on the Likelihood a Kidney finds a Recipient within OPTN Region/State

	(1)	(2)
	Kidney Found Transplant Recipient	
	Within OPTN Region	Within State
1(OfferDate$\geq$c)	-0.1195***	-0.1437***
	(0.0147)	(0.0145)
Mean, Pre-policy	0.666	0.517
Observations	23,466	23,466
Controls	Yes	Yes
Bandwidth	$\pm$ 196 days	$\pm$ 196 days
Degree of polynomial	1	1
Weighting Scheme	Triangular	Triangular
Std error	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3. Column (1) reports the impact on an indicator of kidneys finding recipient within the OPTN region of donor hospital. Column (2) reports the impact on an indicator of kidneys finding recipient within the state of donor hospital. The running variable is the initial offer date of kidneys. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. The cut-off date is March 15, 2021. For more information on sample, see the notes to Table 2.1. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.8. Heterogeneity by Kidney Quality

	(1)	(2)	(3)	(4)	(5)	(6)
	Kidney Donor Profile Index (KDPI)				Expanded Criteria Donor	
	0-20	21-34	35-85	86-100	No (SCD)	Yes (ECD)
1(OfferDate≥c)	-0.0100	-0.0322	-0.0244	-0.0773**	-0.0118	-0.0832**
	(0.0103)	(0.0277)	(0.0179)	(0.0380)	(0.0130)	(0.0342)
Mean, Pre-policy	0.030	0.066	0.202	0.646	0.128	0.531
Share, Subgroup	0.179	0.135	0.521	0.165	0.768	0.232
Observations	4,209	3,161	12,231	3,865	18,033	5,433
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	± 196 days	± 196 days	± 196 days	± 196 days	± 196 days	± 196 days
Degree of polynomial	1	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3, when the outcome is an indicator of discarded deceased donor kidneys. The cut-off date is March 15, 2021. Columns (1)-(4) use the sample of kidneys with KDPI (1) 0-20%, (2) 21-34%, (3) 35-85%, and (4) 86-100%, respectively. Columns (5)-(6) use the sample of kidneys classified as "standard criteria donor" (SCD) and "expanded criteria donor" (ECD), respectively. Prior to 2014, the kidney allocation system classified kidneys into two types: ECD or SCD kidneys. ECD kidneys are those recovered from donors (1) 60 or older or (2) aged 50–59 with two or three of the following conditions: high blood pressure, creatinine levels of 1.5 or higher, or death due to stroke. SCD kidneys refer to kidneys that are not categorized as ECD kidneys. The running variable is the initial offer date of kidneys. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. For more information on sample, see the notes to Table 2.1. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.9. Heterogeneity by Donor Hospital Location

	(1)	(2)
	Donor Hospital Location: Metropolitan, Core	
	Yes	No
1(OfferDate≥c)	-0.0578***	-0.0107
	(0.0199)	(0.0208)
Mean, Pre-policy	0.229	0.215
Observations	13,510	9,956
Controls	Yes	Yes
Bandwidth	± 196 days	± 196 days
Degree of polynomial	1	1
Weighting Scheme	Triangular	Triangular
Std error	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3, when the outcome is an indicator of discarded deceased donor kidneys. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. For more information on sample, see the notes to Table 2.1. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.10. Heterogeneity by Nearby Transplant Centers

	(1)	(2)
	Donor Hospital Characteristics: Nearby Transplant Centers Predicted to Increase in Kidney Access after the Reform	
	No	Yes
1(OfferDate$\geq$c)	-0.0539*	-0.0310*
	(0.0280)	(0.0177)
Mean, Pre-policy	0.226	0.221
Share, Subgroup	0.346	0.654
Observations	8,115	15,351
Controls	Yes	Yes
Bandwidth	$\pm$ 196 days	$\pm$ 196 days
Degree of polynomial	1	1
Weighting Scheme	Triangular	Triangular
Std error	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3, when the outcome is an indicator of deceased donor kidneys that are discarded. Column (1) reports the impact on the share of discarded kidneys from donor hospitals whose nearby transplant centers are predicted to decrease on average after the policy change. Column (2) provides the impact on the share of discarded kidneys from donor hospitals whose nearby transplant centers are predicted to increase on average after the reform. The running variable is the initial offer date of kidneys. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. The cut-off date is March 15, 2021. For more information on the sample, see the notes to Table 2.1."Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Standard errors are clustered at the initial offer date and are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.11. Transplant Recipient Characteristics – Number of HLA Typing Mismatch

	(1)	(2)	(3)	(4)	(5)
		Number of HLA Typing Mismatchs			
	Raw count	0	1 or 2	3 or 4	5 or 6
1(OfferDate≥c)	-0.0380	-0.0002	0.0113	-0.0026	-0.0085
	(0.0498)	(0.0071)	(0.0074)	(0.0160)	(0.0173)
Mean, Pre-policy	4.209	0.044	0.056	0.410	0.489
Observations	17,878	17,878	17,878	17,878	17,878
Controls	Yes	Yes	Yes	Yes	Yes
Bandwidth	± 196 days	± 196 days	± 196 days	± 196 days	± 196 days
Degree of polynomial	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3 using the sample of deceased donor kidney transplant recipients. Column (1) reports the impact on average number of HLA mismatches. Columns 2-5 report the RD estimated when the dependent variable is an indicator of zero HLA mismatch (Column 2), one or two HLA mismatches (Column 3), three or four HLA mismatches (Column 4), and five or six HLA mismatches(Column 5), respectively. The sample consists of deceased donor kidneys in Table 2.3 used for transplant. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. The cut-off date is March 15, 2021. For more information on the sample, see the notes to Table 2.1. "Mean, Pre-policy" is the average outcome variable between -1 and -28 weeks before the policy change. Standard errors are clustered at the running variable and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.12. Transplant Recipient Characteristics – Health Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Num. of Years	By Num. of Years				Previous	Diabetes
	on Dialysis	< 1	1-2	3-4	5+	Transplant	History
1(OfferDate≥c)	0.3526***	-0.0124	-0.0216	-0.0236	0.0576***	0.0043	-0.0038
	(0.1259)	(0.0118)	(0.0145)	(0.0155)	(0.0171)	(0.0114)	(0.0166)
Mean, Pre-policy	3.683	0.207	0.226	0.218	0.349	0.125	0.411
Observations	17,911	17,911	17,911	17,911	17,911	17,911	17,911
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	± 196 days	± 196 days	± 196 days	± 196 days	± 196 days	± 196 days	± 196 days
Degree of polynomial	1	1	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3. The sample consists of deceased donor kidneys in Table 2.3 used for transplant. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. The cut-off date is March 15, 2021. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change.Standard errors are clustered at the running variable and in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.B.13. Transplant Recipient Characteristics – SVI and Poverty Rate

	(1)	(2)	(3)	(4)	(5)
	All	By Quartile			
	(Raw value)	1st	2nd	3rd	4th
Panel A. CDC Social Vulnerability Index					
1(OfferDate≥c)	0.0255***	-0.0465***	-0.0078	0.0227*	0.0316**
	(0.0079)	(0.0141)	(0.0136)	(0.0136)	(0.0144)
Mean, Pre-policy	0.555	0.260	0.253	0.244	0.242
Average Social Vulnerability Index		0.226	0.487	0.677	0.859
Panel B. Poverty Rate					
1(OfferDate≥c)	0.1717	-0.0303**	-0.0122	0.0356***	0.0068
	(0.1776)	(0.0150)	(0.0145)	(0.0135)	(0.0126)
Mean, Pre-policy	14.040	0.254	0.257	0.238	0.251
Average Poverty Rate		8.053	12.424	15.093	20.765
Controls	Yes	Yes	Yes	Yes	Yes
Bandwidth	± 196 days	± 196 days	± 196 days	± 196 days	± 196 days
Degree of polynomial	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3. The sample consists of deceased donor kidneys in Table 2.3 used for transplant. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys in the U.S. Poverty rate refers to the percentage of county residents living under the federal poverty line in 2019. Column (1) reports the impact on average county characteristics that recipient lived at the time of transplant. Columns (2)-(5) report the RD estimates when the dependent variable is an indicator of transplant recipient living in counties with in the first, second, third, or forth quartile group, respectively. “Mean, Pre-policy” is the average of outcome variable between -1 and -28 weeks prior to the policy change. Standard errors are clustered at the running variable and in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.14. Transplant Recipient Characteristics – Race/Ethnicity

	(1)	(2)	(3)	(4)
	Transplant Recipient			
	Non-Hispanic White	Non-Hispanic Black	Hispanic/Latino	Non-Hispanic AAPI
1(OfferDate≥c)	-0.0049	-0.0181	0.0218*	0.0040
	(0.0145)	(0.0186)	(0.0129)	(0.0099)
Mean, Pre-policy	0.392	0.341	0.185	0.091
Observations	17,911	17,911	17,911	17,911
Controls	Yes	Yes	Yes	Yes
Bandwidth	± 196 days	± 196 days	± 196 days	± 196 days
Degree of polynomial	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular
Std error	Cluster	Cluster	Cluster	Cluster

Notes: This table provides the RD estimates of β in Eq. 2.3 using the sample that consists of deceased donor kidneys used for transplant. The cut-off date is March 15, 2021. "Mean, Pre-policy" is the average of outcome variable between -1 and -28 weeks prior to the policy change. Source of data is the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which include the universe of deceased donor kidneys and transplant candidates in the U.S. Standard errors are clustered at the running variable and in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.B.15. Robustness Check – Effects on Transplant Candidates

	(1)	(2)	(3)	(4)	(5)
	Number of Transplant Candidates per 1,000 Candidates				
	Received Deceased Donor Kidney Transplant	Died on Waitlist	All Deaths Within 5 years Joining Waitlist	Received Living Donor Kidney Transplant	Newly Joined Waitlist
$\Delta Access_h * Post_t$	0.19040**	-0.08235***	-0.12255***	-0.00067	-0.06834
	(0.09551)	(0.02666)	(0.02504)	(0.02207)	(0.13072)
Mean of Dep. Var (Pre)	13.728	4.628	6.967	4.490	28.529
Observation	6,660	6,660	6,660	6,660	6,660

Notes: This table reports the estimates of γ in Eq. 2.4 but with the OLS specification. The transplant center-month data includes transplant centers with active transplant candidates prior to the policy change, not located in donor service areas that did not experience any change DSA affiliation through the sample period. Results in Columns 1–5 are weighted by the monthly average waitlist enrollment during the pre-preiod. Column 1 presents the impact on the number of deceased kidney transplants per 1,000 candidate. Columns 2 and 3 reports the impact on the number of transplant candidates died on the waitlist per 1,000 candidate and transplant candidates died within 5 years of entering the waitlist per 1,000 candidate, respectively. Column 4 reports the impact on the number of living donor transplants per 1,000 candidate. Column 5 reports the impact on the number of newly added candidates per 1,000 candidate. "Mean of Dep-Var" is the average of outcome variable between -1 and -15 months prior to the policy change, weighted by the average number of transplant candidates on the waitlist. Standard errors clustered at transplant center level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.B.16. Robustness Check – Effects on Transplant Recipients

	(1)	(2)	(3)	(4)
	Transplant Recipient Adverse Health Outcomes per 1,000 Candidates			
	$\leq$3 months	$\leq$6 months	$\leq$9 months	$\leq$12 months
$\Delta Access_h * Post_t$	0.00161	-0.00919	-0.00342	-0.00716
	(0.00665)	(0.00905)	(0.00941)	(0.00986)
Mean of Dep. Var (Pre)	0.414	0.625	0.800	0.969
Observation	6,660	6,660	6,660	6,660

Notes: This table reports the estimates of γ in Eq. 2.4. I define post-transplant adverse health outcomes as 1) deaths, 2) graft failure, 3) resuming maintenance dialysis within 3, 6, 9, and 12 months of deceased donor kidney transplant. Columns (1)-(4) report the effects on the number of deaths of recipients within 3, 6, 9, 12 months of transplant date per 1000 candidate. Results in Columns 1–5 are weighted by the average number of transplant candidates on the waitlist per transplant center during the pre-preiod. "Mean of Dep-Var" is the average of outcome variable between -1 and -15 months prior to the policy change, weighted by the average number of transplant candidates on the waitlist. Standard errors clustered at transplant center level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 3

Quality Labeling and Allocation of Scarce Organs

1 Introduction

In the U.S., on average, 13 Americans on the kidney waitlist pass away each day. Receiving a deceased donor kidney transplant, including kidneys considered to be "low quality", is associated with lower mortality, better quality of life, and more cost-effective than remaining in dialysis (Axelrod et al., 2018; Senanayake et al., 2020). However, one in five deceased donor kidneys are discarded in spite of being recovered for transplant. The kidney discard rate is especially high among those donated from older donors, which may be linked to the fact that kidneys with lower quality are associated with worse post-transplant outcomes and an increased risk of patient return to maintenance dialysis.

This paper explores the impact of a "high risk" designation for organ quality assessment on the allocative efficiency of donated organs. One way to increase kidney transplant cases is to alleviate perceived barriers and encourage the broader use of kidneys of lower quality. Beginning in October 2002, the quality of cadaveric kidneys was classified based on the age and health condition of donors: (i) expanded criteria donor (ECD) or (ii) standard criteria donor (SCD). In particular, all kidneys from deceased donors aged 60 or above were classified as ECD regardless of the donor's health condition. The ECD scheme aimed to inform potential recipients that donated kidneys recovered from older donors are viable for transplant and place ECD offers only to candidates who showed their interest beforehand, thereby aiming to reduce the discard rate.

Organ quality information provision under the ECD scheme can be particularly valuable in determining whether to accept the offer, particularly given the short time frames necessary to make an allocation decision. By contrast, disclosing quality information in coarse measure could obscure the quality of products, especially those classified as relatively lower tier due to a narrow margin from the threshold. Therefore, dichotomous kidney classification may have increased confusion about the kidney quality of older donors and increased the discard rate.

To estimate these trade-offs, I employ a regression discontinuity (RD) design to study the causal effects of ECD designation on the efficiency of organ allocation. My RD design leverages the change in kidney quality assessment with respect to the donor's age. The identification strategy relies on the assumption that factors other than the ECD designation are similar on either side of the cutoff. In support of this assumption, I provide evidence that the density of deceased donor kidneys and the characteristics of these kidneys evolve smoothly around the cutoff. In complementary analyses, I employ a difference-in-differences (DD) design to explore how the use of donated kidneys changes after the age-based ECD designation is removed in December 2014. I leverage the timing of the policy change that replaced the ECD scheme with a continuous measure that incorporates ten donor characteristics to define "marginal kidneys."

My data source is the Scientific Registry of Transplant Recipients (SRTR) datasets, which provide rich information on the universe of donated organs, deceased donors, and transplant candidates in the U.S. I estimate my RD design by constructing a kidney-level dataset that consists of all deceased donor kidneys recovered within 32 months from their donors reaching age 60. For the DD design, I use the data on donated kidneys recovered from deceased donors aged 60 or above, which are classified as ECD kidneys regardless of any underlying health conditions prior to the policy change.

I find that the use of donated kidneys substantially changes at the cutoff while the underlying kidney quality across deceased donors around age 60 are similar. The likelihood of deceased donor kidneys classified as ECD kidneys jumps by 47 percent at the age cutoff of ECD designation. I also show that the likelihood of kidneys being discarded increases by 5.4 percentage points (18 percent) at this cutoff. These additional discarded kidneys tend to be recovered from donor hospitals located in areas with higher kidney access prior to the ECD scheme.

To further explore whether an increase in the discard rate is linked to a change in organ acceptance decisions, I employ my RD design using the data on top-ranked offers of donated kidneys. Given that a transplant candidate can only receive a kidney transplant if others with higher priority decline the offer, a decrease in acceptance rate among top-ranked offers may be indicative of an increase in discard rate due to ECD designation. I find that an increase in kidney discard rate is linked to a decrease in the likelihood of kidneys being accepted by transplant candidates with the top-ranked offer at the cutoff. The drop in acceptance rate at the threshold tends to be larger among top-ranked offers of kidneys recovered from healthier donors.

Using the DD design, I find that the use of donated kidneys no longer classified as "marginal kidneys" improves by 17 percent under the continuous kidney quality assessment scheme compared to those kept classified as "marginal kidneys" under the new scheme. These findings suggest that the dichotomous age-based measure affected the adoption of donated kidneys around the threshold.

This paper relates to previous studies that explore factors to increase the efficiency of organ allocation. In particular, this paper is closely linked to medical literature that aims to examine the association between ECD designation and use of donated kidneys (Ojo, 2005; Hirth et al., 2010; Sung et al., 2005; Freeman and Klintmalm, 2006). To the best of my knowledge, this paper offers the first causal evidence on the impact of ECD designation on the use of donated kidneys and its implications on organ acceptance behavior. I provide evidence that underlying health risks are unlikely to jump at the threshold by taking advantage of continuous measures that replaced the ECD scheme in 2014. These findings highlight possible unintended consequences of introducing dichotomous health risk measures on the efficiency of the allocation system.

This paper also speaks to previous literature examining the role of information provision about product quality through categorical measures in various applications, such as school performance ratings (Figlio and Lucas, 2004; Figlio and Rouse, 2006), environmental quality (Jin and Leslie, 2003; Powers et al., 2011; Lee and Nakazawa, 2022), consumer ratings (Luca, 2016), health care services (Chen et al., 1999). In particular, this paper is closely related to previous works on the use of the "high risk" designation of patient health status in health care settings (Almond et al., 2010; Bharadwaj et al., 2013). To the best of my knowledge, the current study contributes to the existing literature by exploring this question in the context of the organ allocation system.

The remainder of this paper proceeds as follows. Section 2 discusses a brief overview of the kidney allocation system and measures of organ quality. Section 3 describes the main data source. Section 4 presents the empirical method. Section 5 provides the results. Section 6 concludes.

2 Institutional Background

Human organs procured for transplant are from either deceased or living individuals (henceforth "donors"). Organs recovered from deceased donors are allocated based on the centralized allocation system and are matched with an anonymous recipient. In contrast, relatives or spouses are the primary sources of living kidney donations.[1] This study focuses on deceased donor organs, which are the main source of organ transplant in the U.S(Lentine et al., 2023).[2]

2 .1 Deceased Donor Kidneys

In the U.S., individuals can choose to decide to donate their organs after death through the following ways: (1) registering with the state's donor registry or enrolling in the national

[1]The kidney exchange program enables the matching of two living donors whose kidneys are incompatible with their intended recipients so that they can donate their kidneys to another pair's recipient. By doing so, both recipients can receive more compatible kidneys. 3.1 percent of living kidney transplants that took place between January 2010 and June 2015 were operated based on kidneys from strangers or "non-directed donors" (Organ Procurement and Transplantation Network, 2015).

[2]In 2021, 77 percent of 41,354 organ transplants were performed using organs from deceased donors.

Donate Life registry, (2) stating their intention to donate their organs in their will, or (3) the next of kin consents to add the deceased to the organ donation registry.

When an organ donor is declared deceased in a hospital (hereafter, "donor hospital"), physicians conduct a medical evaluation to determine whether the donated organs are suitable for transplant. Up to two kidneys can be recovered from a single deceased donor and non-kidney organs. Other organs, including the heart and lung, can also be recovered from a deceased donor and each organ type is distributed according to the rules of its allocation system. The kidney is the most commonly used for organ transplant in the U.S. – the kidney accounts for 54 percent (19,636 of 36,421 cases in 2022) of all transplant cases in the US (Organ Procurement and Transplantation Network, 2023). Then the donor hospital notifies the local Organ Procurement Organization (OPO) to inform the donor's organ(s) is considered appropriate for transplant.

"Marginal Kidney" Definitions

The allocation system uses the deceased donor's health and demographic information collected from the medical evaluation to determine how to distribute kidneys. Two measures have been used to inform the relative risk of post-transplant graft failure in the kidney allocation system.

(a) Expanded Criteria Donor

Starting from October 30, 2002 the kidney allocation system implemented a new classification scheme that classifies donated kidneys into two groups: a standard criteria donor (SCD) or an expanded criteria donor (ECD).[3] The goal of this policy was to improve the use of donated kidneys recovered from elderly donors.

Four donor characteristics were used to determine whether kidneys are classified as from ECD donors: donor's age, high blood pressure status, creatinine levels, and cause of death due to stroke. These factors are known to be related to inferior post-transplant outcomes, including graft

[3]The term ECD was introduced in the early 1990s by Kauffman et al. (1997), which found that ECD kidneys are associated with worse post-transplant outcomes (Ojo, 2005).

failure. If kidneys are recovered for transplant from deceased donors aged 60 or above, kidneys are considered as recovered from ECD regardless of the donor's health condition or circumstances of death. In contrast, if kidneys are recovered from donors aged between 50 and 59, they are considered ECD kidneys if two or three of the following conditions hold: high blood pressure, creatinine levels of 1.5 or higher, or death due to stroke (Ojo, 2005). Figure 3.1a presents the share of kidneys classified as ECD by deceased donor's age in months between October 2002 and November 2014. Due to the age-based discontinuity in organ quality assessment built into the ECD definition, the share of kidneys classified as ECD discontinuously increases at age 60.

The allocation system only offered ECD kidneys only to transplant candidates who mentioned their interest in them beforehand. Transplant candidates do not face any penalty to their waitlist priority when they decline an ECD kidney offer (Rosengard et al., 2002).[4] In addition, candidates who express their interest in ECD kidneys at listing may opt-out afterwards. 49.3 percent of adult transplant candidates on the waitlist for donated kidneys between October 2002 and November 2014 reported their interest in an ECD kidney.

(b) Kidney Donor Profile Index

Starting from December 4th, 2014, the allocation system replaced the ECD scheme with the Kidney Donor Profile Index (KDPI) to account for wide variations of expected kidney function within these two broad categories. Compared to the ECD scheme, KDPI provides more granularity in how each kidney is expected to function relative to other donated kidneys.

KDPI is on a cumulative percentage scale ranging from 0 to 100 percent. For example, donated kidneys with a KDPI of 85 percent are predicted to have a higher risk of graft failure compared to 85 percent of those recovered for transplant in the previous year. KDPI is derived based on the Kidney Donor Risk Index (KDRI), which is computed based on the following ten donor characteristics: age, height, weight, ethnicity, hypertension history, diabetes, cerebrovas-

[4]Appendix Figure 3.C.1 illustrates the distribution of age of transplant candidates by their interest in ECD kidney offers at listing. On average, transplant candidates who express interest in ECD kidney offers are older than those who do not.

cular accident death, serum creatinine, Hepatitis C virus, and circulatory death.[5] The OPTN releases a KDRI-to-KDPI Mapping Table to inform which KDPI values correspond to ranges of KDRI values.

The kidney allocation system uses KDPI to classify kidneys into four groups: (a) 0–20 percent, (b) 21–35 percent, (c) 35–85 percent, and (d) 86–100 percent. Similar to the ECD scheme, deceased donor kidneys with KDPI above 85 percent are offered only to transplant candidates who indicated their interest in them in advance.

2 .2 Transplant Candidates

Individuals with renal diseases have kidneys that cannot adequately filter and regulate blood pressure. Two common causes of kidney failures are diabetes and high blood pressure.[6] Chronic kidney disease is classified into five stages based on the severity, and end-stage renal disease (ESRD) is a medical condition categorized as the most advanced stage. Patients diagnosed with ESRD need to undergo regular dialysis to maintain daily life or they may opt for a kidney transplant (Centers for Disease Control and Prevention, 2023).

Receiving a kidney transplant is associated with higher life quality and greater cost savings for ESRD patients than undergoing dialysis (Whiting, 2000). ESRD patients qualify for Medicare even if they are under 65 due to high annual medical expenses linked to ESRD

[5] Equation 3.1 is a formula used to calculate the KDRI using the 10 donor characteristics.

$$
\begin{aligned}
\text{KDRI} = \frac{1}{\text{KDRI}_{median}} * \exp(\sum & 0.0128 * (Age - 40) - 0.0194 * 1(Age < 18) * (Age - 18) \\
& + 0.0107 * 1(Age > 50) * (Age - 50) - 0.0464 * (Height - 170cm)/10 \\
& - 0.0199 * 1(Weight < 80kg) * (Weight - 80kg)/5 \\
& + 0.1790 * 1(\text{African American ethnicity}) + 0.1260 * 1(\text{hypertension}) \\
& + 0.1300 * 1(\text{diabetes}) + 0.0881 * 1(\text{cause of death: CVA}) \\
& + 0.2200 * (\text{creatinine} - 1) - 0.2090 * (\text{creatinine} - 1) * 1(\text{creatinine} - 1.5mg/dL) \\
& + 0.2400 * 1(HCV\, positive) + 0.1330 * 1(\text{cardiac death}))
\end{aligned} \tag{3.1}
$$

KDRI_{median} denotes the median KDRI value among recovered donated kidneys from the previous year. For detailed information on how to compute KDRI and KDPI using these ten donor characteristics, see https://optn.transplant.hrsa.gov/media/j34dm4mv/kdpi_guide.pdf.

[6] For more information, refer to www.cdc.gov/kidneydisease/basics.html.

treatments, such as dialysis (Held et al., 2016).[7]

When at least one kidney is deemed viable for transplant from a deceased donor, the allocation system uses a computer system called DonorNet to generate an ordered list of transplant candidates with compatible blood types with the donor. Transplant candidates listed at a transplant center within the same donor service area as the donor hospital are prioritized over those registered at transplant centers outside this area, except for cases in which the donor and transplant candidates share very similar tissue typing (or "zero mismatch" under HLA-ABDR typing).[8] There are no additional priority points assigned for showing their interest in ECD kidneys (Ojo, 2005).

As donated kidneys have a limited window of viability for transplant, candidates need to decide whether to accept the offer within a specified timeframe.[9] The exceptions are bypassed offers, which are uncommon and do not involve the behavior decisions of candidates as they often arise from unexpected occurrences, including natural disasters and donor medical urgency Choi et al. (2020). Declining an offer imposes no penalty on transplant candidates on the waitlist. However, this may increase their risk of waitlist mortality as predicting the timing of the next offer's availability is challenging.

3 Data and Sample

The restricted Scientific Registry of Transplant Recipients (SRTR) dataset is the main data source for my analysis. The data contains information on detailed demographics and health conditions of the universe of donors, transplant candidates, and transplant recipients, as well as

[7]ESRD patients under 65 are eligible for Medicare if 1) they have undergone more than after the third month of dialysis or 2) they are admitted to a hospital accredited by Medicare to receive a kidney transplant or required pre-transplant care needed to receive the organ transplant within the next two months.

[8]HLA-ABDR typing is used to evaluate tissue typing similarity between a prospective candidate and the donor. Fewer type mismatches are associated with better post-transplant outcomes, including a lower risk of graft failure. For more information on how the kidney allocation point calculation works to rank each candidate within the candidate category, see Israni et al. (2014).

[9]Transplant centers can select either "provisional yes" or "no" when they receive an offer. Offers initially marked with "provisional yes" can later be turned down without accountability. For more information, see https://www.srtr.org/faqs/for-transplant-center-professionals/.

precise geographical information on donor hospitals and transplant centers.

I link three datasets on donated kidneys using a unique ID for deceased donors to construct the kidney-level data for analysis. First, I use detailed records of the universe of deceased donors. This dataset includes detailed information on deceased donors in the US, including whether donors meet ECD criteria and ten characteristics needed to construct the KDPI measure.

Second, I use the donor ID to link data that covers information on each kidney's outcome ("disposition"). Using this information, I construct an indicator for donated organs that were recovered for transplant but discarded in the end.[10]

Third, I merge data on donor hospitals that performed organ recovery with detailed geographic information, including state location and OPO affiliation. 2,205 donor hospitals procured at least one deceased donor kidney for transplant between 2002 and 2014 in the U.S.

Next, I construct an initial kidney offer dataset to assess whether the ECD scheme affects the organ acceptance behavior at the cut-off. The Potential Transplant Recipient (PTR) dataset includes information on which candidates received offers, when the offers were made, and whether the candidate accepted the offer. As up to two kidneys are recovered from a deceased donor, I define initial kidney offers as those presented to transplant candidates first or second in line. After limiting the data to initial offers, I merge in the donor-level using Donor ID.

Using the transplant candidate identifier, I merge in the microdata that includes rich information on demographics, health conditions, and transplant registration records of transplant candidates. Then I limit the sample to transplant candidates who mentioned they are willing to get ECD kidney offers. I limit the sample to kidney offers either accepted or declined as the bypassed offers are uncommon and do not involve acceptance decisions by transplant candidates

[10]The data classifies the reason for deceased donor organs not recovered for transplant are categorized as follows: (a) authorization was not requested, (b) authorization not obtained, (c) not recovered, (d) recovered not for transplant (e.g., education/research purposes). If donated organs were discarded, the data provides information on the reason for their disposal: (a) waited too long on the waitlist (too old on pump; too old on ice; warm ischemic time too long; no recipient located – list exhausted), (b) low quality (donor quality: Donor medical history; positive CMV; positive HIV; positive Hepatitis; biopsy findings; diseased organ; poor organ function; organ trauma; diseased organ; anatomical abnormalities; inadequate urine output), or (c) other factors.

(Choi et al., 2020).[11] Consistent with previous works, declined offers are defined as those initially marked “no” or “provisional yes” but eventually turned down.[12]

4 Empirical Method

I employ a regression discontinuity (RD) design based on the age-based discontinuous organ quality assessment for deceased donor kidneys. I use the kidney-level data to estimate the RD design in Eq. 3.2.[13]

$$Y_i = \alpha + \beta 1(A_i \geq c) + 1(A_i \geq c) g(A_i - c) + g(A_i - c) + X_i \gamma + \varepsilon_i \tag{3.2}$$

where A_i is the age in months of the deceased donor of kidney i. The cut-off value, c, is set to be 60 as donated kidneys are classified as ECD kidneys once the donor's age is 60 or above, regardless of their health condition. $1(\mathrm{A}_i \geq c)$ is an indicator for deceased donor kidneys recovered from donor age 60 or above. g() is a control function based on the donor's age. X_i is a set of control variables. β is the main coefficient of interest that captures the effects of ECD designation on kidney-level outcomes if the identifying assumption holds.

I estimate Eq. 3.2 using the analytic sample of the deceased donor kidneys recovered for transplant and the donor's age at recovery is within 32 months from age 60. My baseline regression specification uses triangular weights to linearly weigh each kidney based on its distance from the cut-off. I use the method developed by Calonico et al. (2014) to calculate the data-driven bandwidth for my main outcome of interest, which is an indicator for donated kidneys recovered for transplant but discarded. The optimal bandwidth is 32 months from their donors reaching age 60. Heteroskedasticity-robust standard errors are used as the running variable is

[11] For more information on the SRTR's risk adjustment model, see https://www.srtr.org/tools/offer-acceptance.

[12] For more information, see https://www.srtr.org/faqs/for-transplant-center-professionals/.

[13] This study used data from the Scientific Registry of Transplant Recipients (SRTR). The SRTR data system includes data on all donor, wait-listed candidates, and transplant recipients in the US, submitted by the members of the Organ Procurement and Transplantation Network (OPTN). The Health Resources and Services Administration (HRSA), U.S. Department of Health and Human Services provides oversight to the activities of the OPTN and SRTR contractors.

more discrete in nature (Kolesár and Rothe, 2018).

To explore the sensitivity of my findings, I estimate Eq. 3.2 with various alternative regression specifications: (1) excluding X_i, (2) excluding triangular weights, and (3) using alternative bandwidth choice. To estimate this, I use data that consists of deceased donor kidneys recovered from transplant when the allocation system used the ECD scheme.

My analytic sample consists of kidneys procured from deceased donors aged 50 or above at the organ recovery, which are subject to ECD classification. I also limit the sample to kidneys from deceased donors with non-missing information for donor characteristics, including blood type and information necessary to determine ECD status and compute the KDPI score for each kidney. The sample is further restricted to deceased donor kidneys recovered in DSAs that did not experience any change in OPO affiliation between 2002 and 2014. The sample consists of 12,835 kidneys that were recovered for transplant from deceased donors whose ages were within 32 months from age 60.

Table 3.1 presents the means of outcome measures and controls for the full sample (Column 1) and by donor's age (Columns 2 and 3). In Column 1, 41.2 percent of kidneys recovered for transplant were discarded in the end (hereafter, "discard rate"). Donated kidneys from deceased donors over 60 are more likely to be discarded than those from donors under 60 (38.2 percent vs 44.8 percent). On the other hand, kidney characteristics used to define ECD status or KDPI score are similar between the two groups, which indicates that the underlying health characteristics are similar.

4 .1 Validity of Study Design

The validity of my RD design relies on the assumption that the assignment of kidneys on either side of the threshold is as good as random so that the outcomes of interests would have evolved smoothly without the ECD designation (Lee, 2008). That is, the cut-off seems to reflect agreement on organ quality, but unlikely the underlying health conditions or biologic criteria.

I employ two approaches to examine whether the identifying assumption seems to hold.

First, I employ the McCrary density test to examine the smoothness of the distribution of donated kidneys. The McCrary density test results support the null hypothesis that the density of the running variable is smooth around the cut-off (p-value=0.1454).[14] This suggests that the donor hospital is unlikely to delay kidney procurement for transplant across donor ages deliberately. This is also in line with the institutional context that donated kidneys are only allocated through the national allocation system, and thus, transplant candidates are unlikely to predict the details of the kidney offers before receiving them.

Next, I test whether donated kidneys on either side of the cut-off share similar observable characteristics. I estimate Eq. 3.2 by setting characteristics of donated kidneys as the dependent variables: a) demographics, b) health conditions, c) donor consent mechanisms, and d) circumstances of death. In Appendix Table 3.C.1, all of the RD estimates are close to zero and are statistically indistinguishable at 10 percent, which supports the validity of my RD design.

4 .2 Placebo Exercises

I conduct several exercises to address concerns on whether the effects are driven by factors other than the ECD scheme. First, I construct samples of hearts and livers that were procured for transplant from the donors whose kidneys are observed in the main analysis sample to estimate Eq. 3.2 for placebo exercises. As each donated organ is allocated based on its own allocation system, the policy for the kidney allocation system only does not apply to how other types of donated organs are allocated. If the study design is valid, the outcomes of interest should evolve smoothly around the cut-off with the placebo samples.

Second, I estimate Eq. 3.2 with different cut-off values (age 52, 55, 62, 65) to explore the validity of my findings. Since the ECD designation is not determined based on any of these age cut-offs, I expect to observe no discontinuous changes in the outcome measures at these placebo cut-offs.

Furthermore, I use the data on the donated kidneys recovered prior to the ECD scheme

[14] Appendix Figure 3.C.2 illustrates the density of donated kidneys across the donor's age in months.

adoption (i.e., from January 2000 to October 2002). Since there were no kidney quality measures using the age-based discontinuity during this period, the outcome measures are predicted to be smooth around the cut-off.

4 .3 Impact of ECD Classification Removal

To complement my findings from RD design, I employ a difference-in-difference (DD) design to explore whether the use of donated kidneys classified as "marginal kidneys" improves after the age-based discontinuity in organ quality assessment is removed. My DD design in Eq. 3.3 leverages the timing of the policy change that replaced ECD with KDPI in December 2014. As the age-based discontinuity is replaced under the KDPI scheme, the use of kidneys no longer classified as "marginal kidneys" may improve after the reform compared to those whose classification remains unchanged even after the policy change.

$$Y_{igt} = \alpha + \gamma 1(Change.Label_g)1(Post_t) + \lambda_t + 1(Change.Label_g) + X_i\theta + \varepsilon_{igt} \tag{3.3}$$

where $1(Change.Label_g)$ is an indicator of deceased donor kidneys i with KDPI above 85% in 2014. $1(Post_t)$ is an indicator of kidneys recovered in or after December 2014, which is the month that marginal kidneys are defined using KDPI, not ECD scheme. X_i is a set of control variables, which include donor's age in months, donor's blood types, donor's race, and month fixed effects. My coefficient of interest is γ, which captures the effects on kidney-level outcomes if the identifying assumption holds. I estimate Eq. 3.3 using kidney-level data that consists of kidneys recovered from donor's age 6o or above. I cluster standard errors at donor hospital level.

The identifying assumption is that kidneys from whose deceased donor with KDPI below 85 percent would have had similar outcomes to those from deceased donors with KDPI above 85 percent if there was no policy change in December 2014. I estimate the event-study specification in Eq. 3.4 to illustrate whether there are any differential pre-trends in outcomes between kidneys with different KDPI scores in the sample and to examine dynamic treatment effects.

$$Y_{igt} = \sum_{\tau=-10(\neq -1)}^{5} \delta_\tau 1(t=\tau) * 1(Change.Label_g) + \lambda_t + 1(Change.Label_g) + X_i\theta + \varepsilon_{igt} \quad (3.4)$$

where τ denotes event-time, difference in years between a given month t and December 2014. λ_t denotes event-time fixed effects. δ_τ identifies changes in kidney-level outcomes between kidneys with below and above KDPI 85% in event time τ, relative to the 12 months prior to the policy change ($\tau = -1$ or December 2013-November 2014).

5 Results

5 .1 Kidney Utilization

I estimate the RD specification using the kidney-level data to study the effects of age-based organ quality designation on the use of donated kidneys. To explore the change in the discard rate at the cut-off, I use the initial offer-level data to explore whether organ acceptance behavior changes at the threshold.

Due to the age-based discontinuity in organ quality assessment built into the ECD definition, the share of "marginal kidneys" increases at the threshold. Figure 3.1a presents the share of donated kidneys classified as those recovered from ECD donors. In Column 1 of Table 3.2, the likelihood of kidneys classified as ECD kidneys increases by 48.4 percentage points (or 94 percent).

On the other hand, Figure 3.1b illustrates that the average KDRI score steadily increases with the donor's age.[15] I estimate Eq. 3.2 by setting the continuous kidney quality measure used since December 2014 as the dependent variable. In Column 3 of Table 3.2, the RD estimate is

[15]Since the KDPI is a measure on a cumulative percentage scale where each KDPI value corresponds to a range of KDRI values, I use the KDRI score to examine the changes in the underlying health risk of kidney quality at the threshold. Appendix Figure 3.C.3 presents the average KDRI score with the donor's age fixed at 59. Compared to Figure 3.1b, the upward trend in the average KDRI score with respect to the donor's age becomes flatter in Appendix Figure 3.C.3. This suggests that this trend is largely attributable to the age component.

close to zero and statistically indistinguishable at 10 percent, which indicates that the underlying kidney quality measured by the continuous score evolves smoothly around the cut-off.

Next, I explore how the age-based discontinuity in kidney quality assessment affects the use of donated kidneys. Figure 3.2 plots the share of kidneys discarded by donor's age. In Column 2 of Table 3.2, the likelihood of kidneys being discarded increases at the cut-off by 5.8 percentage points (19 percent).

(a) Donor Hospital Characteristics

I examine whether there are any differential effects by the location of the donor hospital. Using the data covering information on deceased donor kidneys and waitlisted candidates between January 2000 and September 2002, I calculate the number of transplant candidates per kidney to construct a proxy for market tightness. Using this measure, I split the sample into donor hospitals located in donor service areas with "tight" (above median) or "loose" (below median) kidney access.

In Appendix Table 3.C.2, the likelihood of donated kidneys being discarded drops by 9.6 percentage points (30 percent) at the cut-off for those recovered from donor hospitals located in high kidney access. In contrast, the increase in the discard rate from kidneys recovered from low kidney access areas is somewhat smaller (2.4 percentage points or 4 percent).

(b) Donor's Health

I turn to explore differential effects by donor health condition. In Appendix Table 3.C.3, I present the share of the kidney being discarded by the number of comorbidities used to calculate ECD status. Kidneys from deceased donors with 0-1 comorbidity condition determining ECD status are more likely to be discarded by 18.7 percent at the cut-off. In contrast, the increase in discard rate from kidneys from deceased donors with 2-3 comorbidities at the cut-off is similar (19 percent).

Robustness Checks

To check the sensitivity of findings to the choice of main specification, I estimate Eq. 3.2 with various specifications: (1) excluding control variables, (2) dropping triangular weights, and (3) using a local linear function form for f(), and (4) employing different bandwidth choices. In Appendix Table 3.C.4, the RD estimates reported are very alike in terms of magnitude and statistical significance across different specifications, implying that the findings are robust to changes in the baseline specification.

Placebo Checks

I conduct several placebo exercises to examine the validity of my research design. First, I estimate Eq. 3.2 using the samples on hearts and livers that were recovered from the donors in the main sample. In Appendix Table 3.C.5, the changes in the likelihood of hearts and livers being discarded at the cut-off are close to zero and are statistically indistinguishable from zero at the 10 percent level. This supports the validity of my study design that changes in the discard rate at the cut-off for the kidney sample are unlikely to be attributable to factors unrelated to the ECD scheme.

Next, I use the sample of donated hearts and livers procured from the donors whose kidneys are observed in the main analysis sample to estimate Eq. 3.2. In Appendix Table 3.C.6, the RD estimates are close to zero and statistically indistinguishable from zero when different cut-off values are used to estimate Eq. 3.2. This aligns with my prediction that there are no changes in the use of non-kidneys at the cut-off as each donated organ has its own allocation system and the policy for kidney allocation system only is not applicable on how other types of donated organs are allocated.

Moreover, I estimate Eq. 3.2 by using data on kidneys recovered prior to ECD was adopted in the allocation system (i.e., Jan 2000 and October 2002). As kidneys recovered before the ECD scheme was adopted in the allocation system, the discard rate is predicted to be smooth around the cut-off. In Appendix Table 3.C.7, the RD estimates are not statistically distinguishable from zero at the 10 percent level, which supports the validity of my RD design.

Changes in Kidney Offer Acceptance

To explore whether the discontinuous increase in the kidney discard rate is linked to changes in the organ acceptance behavior, I estimate Eq. 3.2 by using the data on transplant candidates with top-ranked kidney offers. Focusing on top-ranked offers allows me to examine changes in organ acceptance behavior given that the chance of receiving a kidney offer depends on the decisions of those ranked higher in priority. As ECD kidney offers are only extended to transplant candidates who have expressed their interest in such offers in advance, I restrict the sample to top-ranked offers presented to candidates who opt in for ECD kidneys at listing.

Figure 3.3 presents the share of initial offers declined by donor's age. In Column 1 of Table 3.C.8, the likelihood of top-ranked offers getting declined increases by 3.9 percentage points at the cut-off.[16]

The drop in the acceptance rate is greater among kidneys from healthier donors.[17] In Appendix Table 3.C.9, the likelihood of the top-ranked offer accepted drops by 6.3 percentage points (or 8 percent) if donated kidneys were recovered from the donors with zero or one more comorbidity. In contrast, the drop in acceptance rate for those recovered from the donors with two or three comorbidities is smaller (2.4 percentage points or 3 percent) but is not statistically distinguishable from zero.

In addition, kidneys are more likely to be declined at the initial offer stage if the pre-ECD organ acceptance rate is already lower before the ECD scheme was adopted. To examine this, I use the PTR data covering the initial offers made between January 2000 and September 2002 and calculate the share of initial offers accepted at a transplant center prior to the allocation system adopted the ECD scheme. Using this measure, I split the data on initial offers into two groups – initial offers made to transplant centers with "high" (above median) or "low" (below

[16]In Columns 2-5, the RD estimates are similar across alternative bandwidth choices, suggesting the results are not robust to the choice of bandwidth.

[17]Appendix Figure 3.C.4 presents the share of initial offers declined by the number of comorbidities of deceased donors.

median) acceptance rates. In Column 1 of Appendix Table 3.C.10, the likelihood of initial offer acceptance rate drops by 6.1 percentage points (8 percent) at the cut-off for the initial offers presented to transplant candidates with high pre-ECD acceptance rate. In Column 2, the drop in organ acceptance rate for initial offers made to transplant cents with "low" pre-ECD acceptance rate is small (2.2 percentage points) and statistically insignificant at 10 percent.

I explore whether the discontinuous drop in the offer acceptance rate is linked to changes in the composition of transplant candidates receiving the initial offer around the cut-off. To test this, I estimate Eq. 3.2 by setting transplant candidates' demographic and health conditions presented with the initial offer, including the number of wait days on the waitlist, age at listing, gender, previous non-kidney transplant history, and blood type. In Appendix Table 3.C.11, the RD estimates are not statistically distinguishable from zero at 10 percent. This suggests that the drop in the acceptance behavior at the cut-off is likely to be driven by the age-based discontinuous quality measure rather than the changes in the characteristics of the offer recipients.

5 .2 Impact of ECD Classification Removal

The findings of the RD design suggest that the discard rate increases at the cut-off due to age-based discontinuity in organ quality assessment in the ECD scheme. As a complementary analysis, I estimate a difference-in-difference design in Eq. 3.3 to explore the impact of removing the age-based discontinuity in organ quality assessment on the use of donated kidneys.

Consistent with my RD findings, the use of donated kidneys improves among those no longer classified as "marginal kidneys" compared to those that remain classified as marginal kidneys after KDPI is used as a new risk designation measure. In Figure 3.4, while these two groups share a similar trajectory during the pre-period, the event study estimates become negative once the ECD scheme is replaced with KDPI to define "marginal kidneys". In Table 3.3, the discard rate among donated kidneys in the treated group decreases by 5.9 percentage points (12 percent) relative to the control group during the post-period. These findings provide further evidence that the age-based discontinuity in ECD designation is linked to the drop in the use of donated kidneys around the threshold.

6 Conclusion

This paper explores the impact of discontinuous quality measures and its implications for efficiency. I examine these questions in the context of the organ allocation system, where donated kidneys must match to a recipient promptly before losing transplant viability and information on organ quality can be valuable for transplant candidates in determining whether to accept the offer. To answer this, I employ the RD design that exploits the discontinuity in organ quality classification across the donor's age.

Using administrative microdata on the universe of donated organs and transplant candidates, I find unintended consequences of this policy change – ECD designation led to a 19 percent increase in the likelihood of kidney discards at the cutoff. This is in spite of fact that other observable health characteristics, such as the continuous organ quality measure adopted in 2014, are smooth around the cutoff. A back-of-envelope calculation using estimates from my main findings suggests that 1,071 additional donated kidneys that were viable for transplant were discarded under the ECD scheme.[18]

This paper provides evidence that disclosing quality using a discontinuity measure could obscure the quality of products and affect the use of goods classified as relatively lower quality around the threshold. The findings are relevant to a recent policy change implemented in the kidney allocation system. Beginning April 2022, the allocation system adopted a rule that specifies which deceased donor kidneys are required to undergo kidney biopsy. Similar to the ECD scheme, donated kidneys recovered from deceased donors aged 60 or above are required to undergo a biopsy regardless of their underlying health conditions prior to death.[19] While the ECD scheme is no longer in use in the allocation system and there have been mixed findings on

[18] 18,982 kidneys recovered for transplant from deceased donors aged 60 or above between October 2002 and November 2014 * 29.7 (discard rate at age 59) * 19 percent (increase in discard rate in percent).

[19] For more information, see policy 2.11.A which illustrates the criteria specified in the policy document at the following URL, https://optn.transplant.hrsa.gov/media/eavh5bf3/optn_policies.pdf.

the association between undergoing biopsy and the use of donated kidney, the findings of this paper suggest the need of assessing whether the age-based discontinuity in biopsies could affect the use of donated kidneys.

7 Acknowledgements

This chapter is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the paper. The data reported here have been supplied by the Hennepin Healthcare Research Institute (HHRI) as the contractor for the Scientific Registry of Transplant Recipients (SRTR). The interpretation and reporting of these data are the responsibility of the author and in no way should be seen as an official policy of or interpretation by the SRTR or the U.S. Government.

8 Figures and Tables

(a) Share of ECD Kidneys

(b) Kidney Donor Risk Index (KDRI)

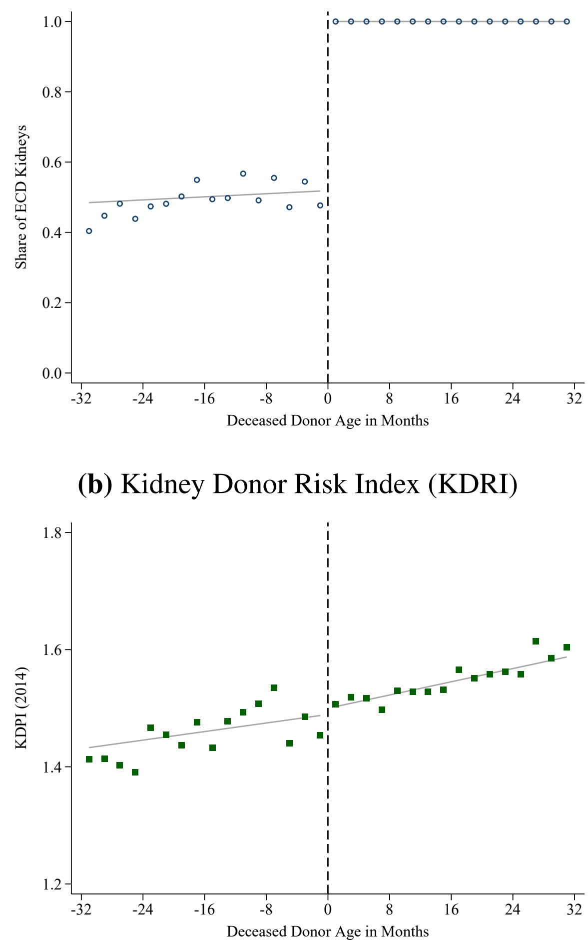


Figure 3.1. ECD Designation and Kidney Quality by Age of Deceased Donors

Notes: This figure presents the share of donated kidneys classified as ECD kidneys (Panel A) and the average Kidney Donor Risk Index (KDRI) (Panel B) by the age of deceased donors. Each dot represents a two-month bin. The x-axis is the age of deceased donor in months at organ recovery. Sample is sourced from the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which provide the universe of kidneys recovered from deceased donors in the U.S. See the notes to Table 3.1 for further details regarding the analysis sample.

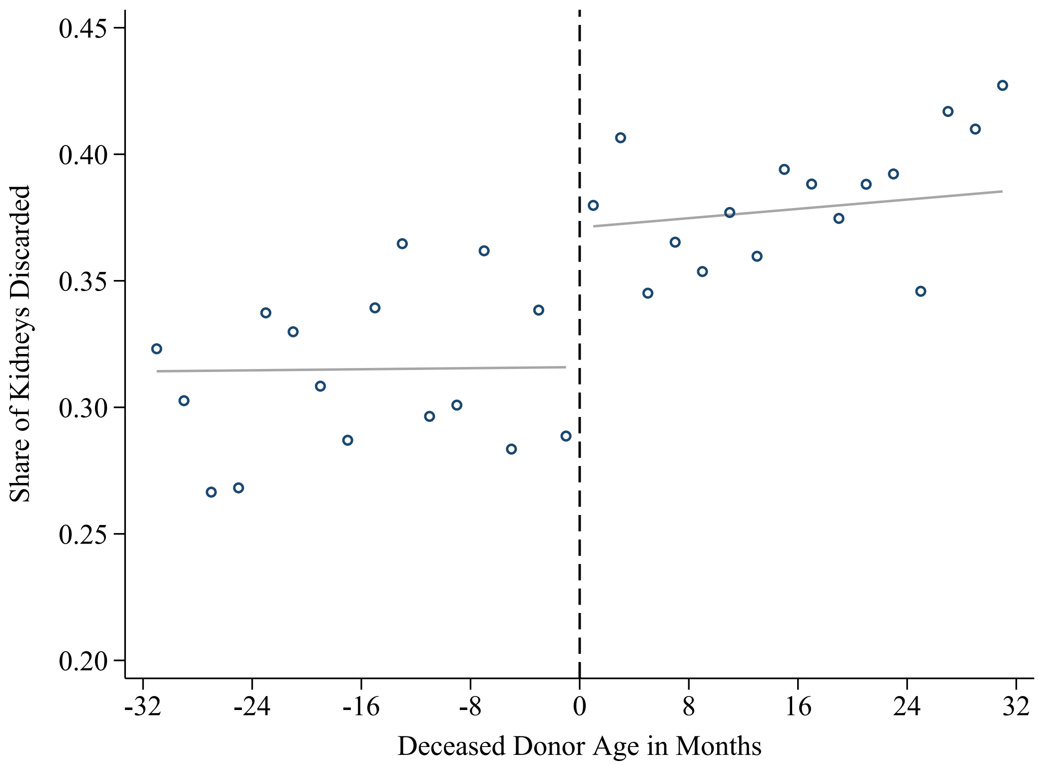


Figure 3.2. Kidney Utilization by Age of Deceased Donors

Notes: This figure presents the share of donated kidneys recovered for transplant but discarded in the end. The x-axis is the age of deceased donor in months at organ recovery. Each dot represents a two-month bin. Sample is sourced from the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which provide the universe of kidneys recovered from deceased donors in the U.S. See the notes to Table 3.1 for further details regarding the analysis sample.

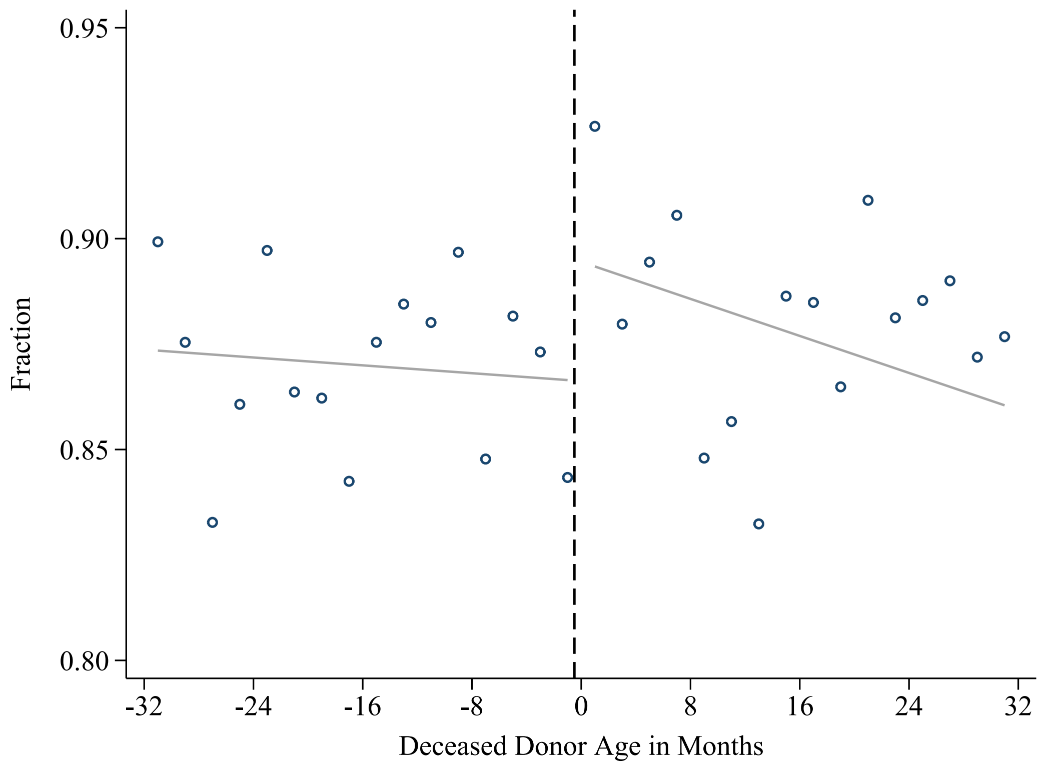


Figure 3.3. Kidney Offer Acceptance by Age of Deceased Donors

Notes: This figure presents the share of kidneys declined by candidates receiving the initial offer. The sample consists of transplant candidates who are willing to receive ECD kidney offers and receive initial offers of donated kidneys included in the kidney-level data. Each dot represents a two-month bin. Sample is sourced from the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which provide the universe of kidneys recovered from deceased donors and transplant candidates who ever enrolled the kidney waitlist in the U.S.

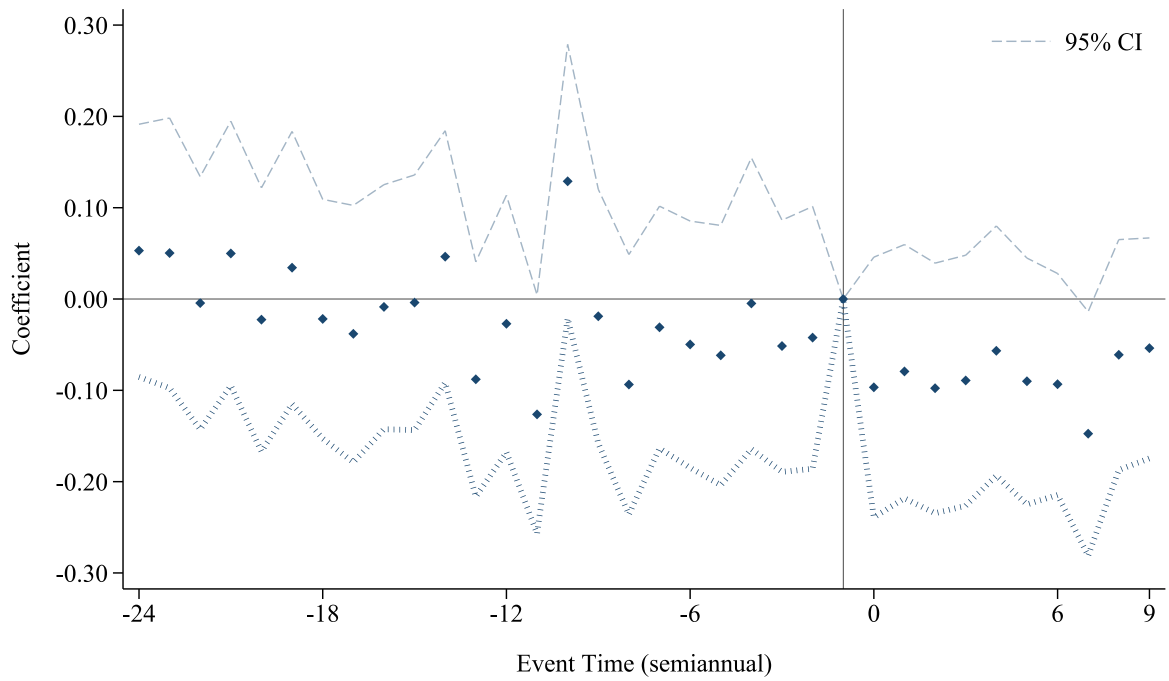


Figure 3.4. Event Study: Effects on Kidney Utilization

Notes: This figure plots the event-study coefficients and 95% confidence intervals on an indicator for kidneys that were recovered for transplant but discarded in the end. I estimate Eq. 3.4 by using the sample constructed using the restricted Scientific Registry of Transplant Recipients (SRTR) datasets. The sample consists of deceased donor kidneys recovered from donor age 60 or above from 2002 to 2019. As the age-based discontinuity is replaced under the KDPI scheme in December 2014, the event time $\tau = -1$ is May 2014-November 2014. Standard errors are clustered at donor hospital's level.

Table 3.1. Descriptive Statistics – Deceased Donor Kidney

	(1)	(2)	(3)
	Full sample	By Donor Age	
		Age < 60	Age ≥ 60
Outcomes			
1(ECD kidney)	0.714	0.492	1.000
1(Discarded)	0.343	0.313	0.381
Kidney/Donor Characteristics			
KDRI 2014	1.495	1.455	1.546
KDRI (at age 59)	1.483	1.485	1.480
Female	0.473	0.477	0.468
White	0.236	0.243	0.226
Blood Type			
O	0.465	0.468	0.461
A	0.381	0.376	0.386
B	0.118	0.116	0.119
AB	0.037	0.040	0.033
BMI	28.372	28.481	28.231
Diabetes	0.182	0.182	0.183
History of Hypertension	0.595	0.594	0.595
Serum Creatinine	1.161	1.157	1.167
Cause of Death: Cerebral Vascular Accident	0.631	0.626	0.637
Observations	12535	7060	5475

Notes: The sample is constructed using the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which cover information on the universe of kidneys recovered from deceased donors in the U.S. Each observation is a deceased donor kidney. The sample is restricted to donated kidneys (i) recovered within 32 months from their donors reaching age 60, (ii) recovered from deceased donors whose kidney biopsy information is complete, (ii) recovered from donor hospitals which did not experience any change in OPO affiliation between October 2002 and November 2014. Kidney discard rate is computed by dividing the number of kidneys not used for transplant by the number of kidneys recovered for transplant.

Table 3.2. Effects on Kidney-Level Outcomes

	(1)	(2)	(3)
	1(ECD)	1(Discarded)	KDRI (2014)
1(Age$\geq$ 60)	0.4766***	0.0558***	0.0058
	(0.0138)	(0.0185)	(0.0088)
Controls	Yes	Yes	Yes
Bandwidth	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo
Degree of polynomial	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular
Std error	Robust	Robust	Robust
Mean of Dep. Var	0.517	0.311	1.485
Observation	12,535	12,535	12,535

Notes: This table presents the RD estimates of β in Eq. 3.2. Each column heading indicates the kidney characteristic used as the dependant variable to estimate Eq. 3.2. The sample consists of deceased donor kidneys recovered within 32 months from their donors reaching age 60, which is the cut-off. Column 1 provides the RD estimates when the dependent variable is an indicator of ECD kidneys, and Column 2 provides the RD estimates when the dependent variable is an indicator of donated kidneys being discarded. Column 3 presents the RD estimates when the dependent variable is the Kidney Donor Risk Index (KDRI) score. "Mean, Pre-policy" refers to the average of dependent variable from -1 to -12 months prior to age 60, which is the cut-off value. See the notes to Table 3.1 for further details regarding the analysis sample. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.3. Effects of ECD Classification Removal on Kidney Utilization

	(1)	(2)
$1(Change.Label_g)1(Post_t)$	-0.0577***	-0.0590***
	(0.0213)	(0.0212)
Controls	No	Yes
Std error	Cluster	Cluster
Mean of Dep. Var	0.509	0.509
Observation	17,548	17,548

Notes: This table presents the DD estimates of γ in Eq. 3.3. The sample is constructed using the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which cover information on the universe of kidneys recovered from deceased donors in the U.S. I estimate Eq. 3.3 using kidney-level data that consists of kidneys recovered from donor's age 6o or above from 2002 to 2019. "Mean, Pre-policy" refers to the average of dependent variable 1 year prior to December 2014 (i.e., December 2013-November 2014). * p<0.1, ** p<0.05, *** p<0.01. Standard errors are clustered at donor hospital's level.

9 Appendix Figures and Tables

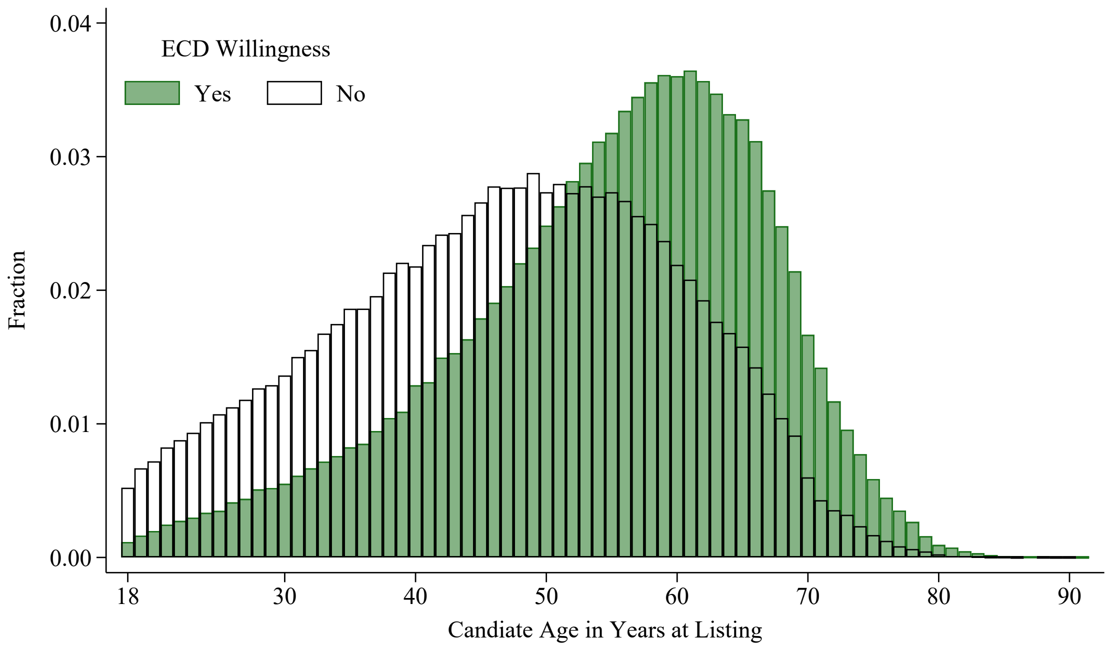


Figure 3.C.1. Age Distribution of Transplant Candidates by Interest in ECD Kidney Offers

Notes: This figure presents the distribution of age of transplant candidates by their interest in ECD kidney offers at listing. The sample consists of transplant candidates aged 18 or above who were active on the waitlist for deceased donor kidney transplants when the ECD scheme was used in the allocation system (i.e., between October 30, 2002 and December 3, 2014). The sample is sourced from the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which provide the universe of transplant candidates waitlisted for deceased donor kidney transplants in the U.S.

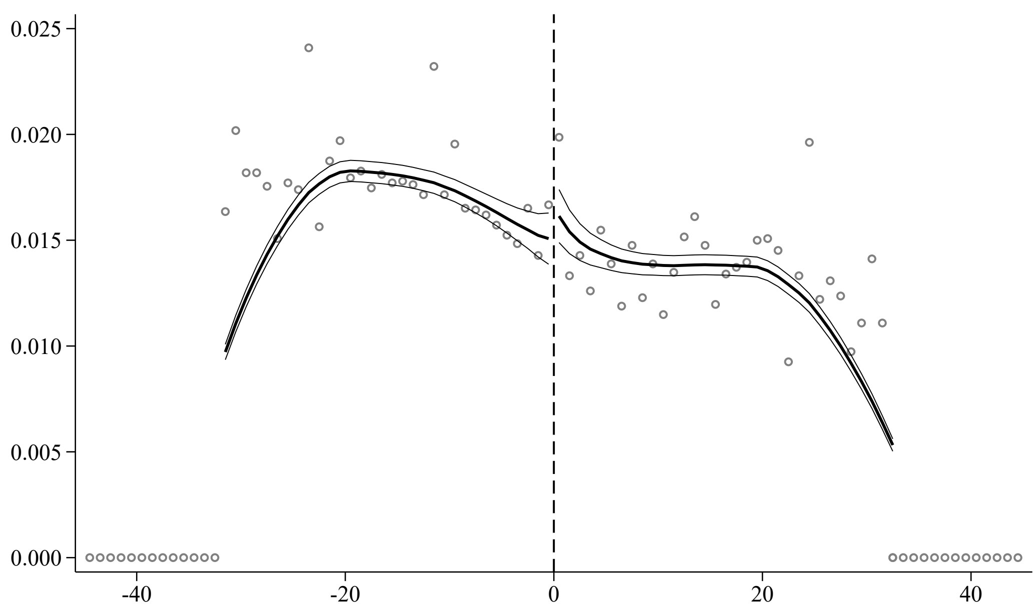


Figure 3.C.2. McCrary Density Test

Notes: The figure presents the density of deceased donor kidneys by age of deceased donors. To conduct the McCrary density test in Stata, I use DCDensity.ado written by Justin McCrary and Brian Kovak. The p-value of the McCrary density test is 0.1454. See the notes to Table 3.1 for further details regarding the analysis sample.

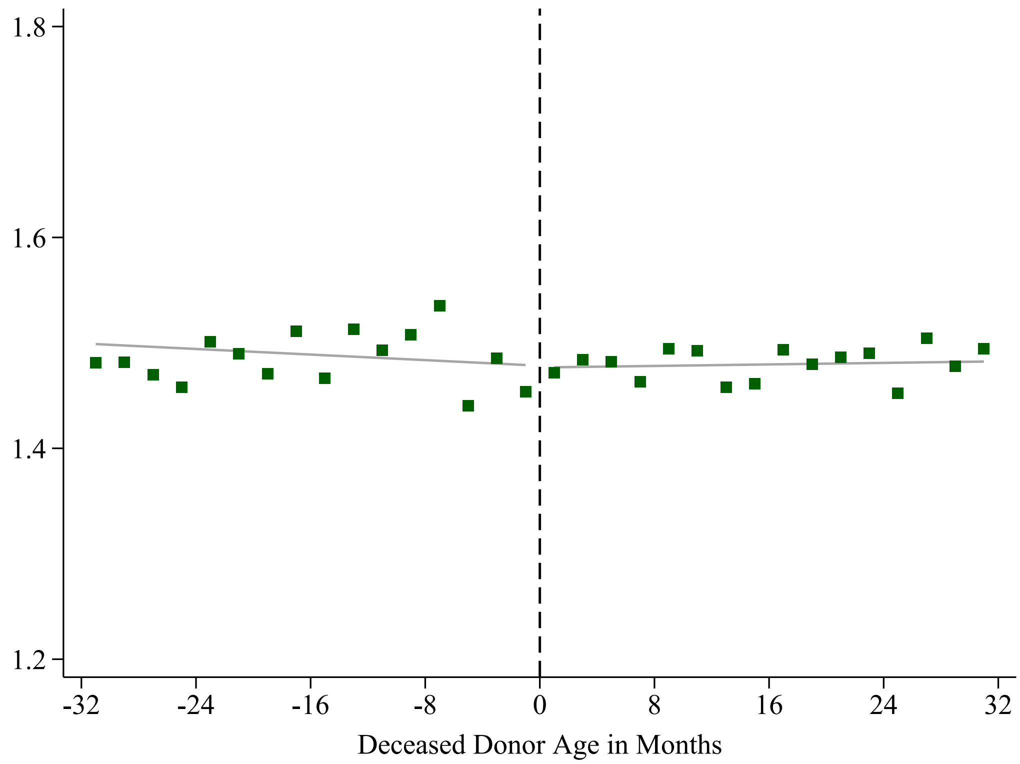


Figure 3.C.3. Average KDRI Calculated Fixing Donor's Age to Be 59

Notes: This figure presents the average Kidney Donor Risk Index (KDRI) with the donor's age set at 59. Compared to Panel B of Figure 3.1a, the upward trend in the average KDRI score with respect to the donor's age becomes flatter in Appendix Figure 3.C.3. The x-axis is the age of deceased donor in months at organ recovery. Each dot represents a two-month bin. See the notes to Table 3.1 for further details regarding the analysis sample.

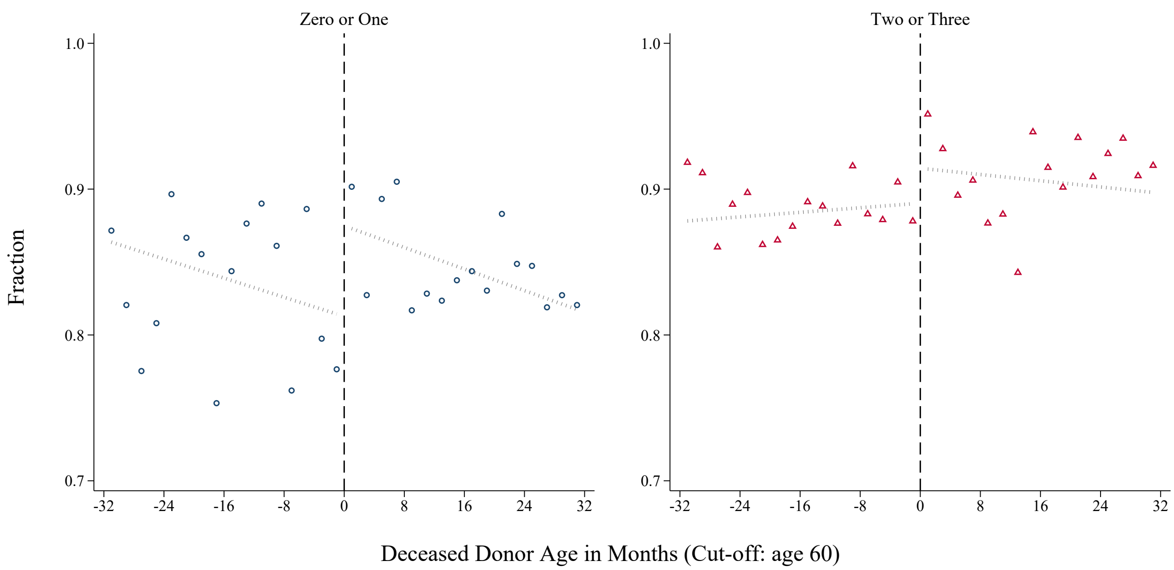


Figure 3.C.4. Initial Offers Declined by Number of Comorbidities of Deceased Donors

Notes: This figure presents the share of top-ranked offers declined by the donor's age in months. The sample consists of deceased donor kidneys whose top-ranked offers were presented to candidates who opted for ECD kidneys at listing. The sample is further restricted to deceased donor kidneys recovered within 32 months from their donors reaching age 60. Each dot represents a two-month bin.

Table 3.C.1. Characteristics of Kidneys

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Number of Comorbidities	1(Stroke)	1(Creat)	1(Hypertension)	Blood Type				Race/Ethnicity		Female	Consent Mechanism
					O	A	B	AB	Black	Hispanic/Latino		Driver License
1(Age$\geq$ 60)	-0.0117	-0.0288	-0.0230	-0.0091	-0.0051	0.0006	0.0054	-0.0009	0.0044	0.0072	-0.0377*	0.0122
	(0.0336)	(0.0186)	(0.0288)	(0.0191)	(0.0196)	(0.0192)	(0.0122)	(0.0072)	(0.0128)	(0.0119)	(0.0196)	(0.0169)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo
Degree of polynomial	1	1	1	1	1	1	1	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust
Mean of Dep. Var	1.483	0.672	1.187	0.621	0.454	0.400	0.110	0.036	0.124	0.102	0.481	0.249
Observation	12,535	12,535	12,529	12,535	12,535	12,535	12,535	12,535	12,535	12,535	12,535	12,535

Notes: This table presents the RD estimates of β in Eq. 3.2. Each column heading indicates the kidney characteristic used as the dependant variable to estimate Eq. 3.2. The sample consists of deceased donor kidneys recovered within 32 months from donor age 60. “Mean, Pre-policy” refers to the average of dependent variable -1 month prior to age 60, which is the cut-off value. See the notes to Table 3.1 for further details regarding the analysis sample. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.C.2. Kidney Discard Rate by Market Tightness Prior to Implementation of ECD Scheme

	(1)	(2)
	Market Tightness (Pre-ECD)	
	Loose	Tight
1(Age$\geq$ 60)	0.0957***	0.0239
	(0.0265)	(0.0233)
Controls	Yes	Yes
Bandwidth	$\pm$32 mo	$\pm$32 mo
Degree of polynomial	1	1
Weighting Scheme	Triangular	Triangular
Std error	Robust	Robust
Mean of Dep. Var	0.314	0.308
Observation	5,920	6,615

Notes: This table presents the RD estimates of β in Eq. 3.2. The sample consists of deceased donor kidneys recovered within 32 months from the donor age 60. “Mean, Pre-policy” refers to the average of dependent variable from -1 to -12 month prior to age 60, which is the cut-off value. See the notes to Table 3.1 for further details regarding the analysis sample. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.C.3. Kidney Discard Rate by Donor's Health

	(1)	(2)
	Number of Comorbidities	
	Zero or One	Two or Three
1(Age$\geq$ 60)	0.0468*	0.0702***
	(0.0247)	(0.0269)
Controls	Yes	Yes
Bandwidth	$\pm$32 mo	$\pm$32 mo
Degree of polynomial	1	1
Weighting Scheme	Triangular	Triangular
Std error	Robust	Robust
Mean of Dep. Var	0.250	0.368
Observation	6,203	6,332

Notes: This table presents the RD estimates of β in Eq. 3.2. The sample consists of deceased donor kidneys recovered within 32 months from the donor age 60. "Mean, Pre-policy" refers to the average of dependent variable from -1 to -12 month prior to age 60, which is the cut-off value. See the notes to Table 3.1 for further details regarding the analysis sample. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.C.4. Robustness Check

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Baseline	No Controls	No Triangular Weights	Bandwidth Choice							
				16	20	24	28	36	40	44	48
Panel A. Classified as ECD Kidney											
1(Age$\geq$ 60)	0.4773***	0.4773***	0.4864***	0.5035***	0.5007***	0.4924***	0.4842***	0.4723***	0.4708***	0.4694***	0.4700***
	(0.0138)	(0.0138)	(0.0091)	(0.0201)	(0.0179)	(0.0161)	(0.0148)	(0.0129)	(0.0121)	(0.0115)	(0.0109)
Panel B. Whether Donated Kidney was Discarded											
1(Age$\geq$ 60)	0.0561***	0.0561***	0.0502***	0.0818***	0.0730***	0.0664***	0.0614***	0.0511***	0.0483***	0.0473***	0.0464***
	(0.0186)	(0.0186)	(0.0169)	(0.0258)	(0.0233)	(0.0213)	(0.0198)	(0.0176)	(0.0166)	(0.0158)	(0.0152)
Controls	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$16 mo	$\pm$20 mo	$\pm$24 mo	$\pm$28 mo	$\pm$36 mo	$\pm$40 mo	$\pm$44 mo	$\pm$48 mo
Degree of polynomial	1	1	1	1	1	1	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	No	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust
Observation	12,535	12,535	12,535	6,240	7,843	9,477	11,044	14,118	15,873	17,425	19,088

Notes: This table presents the RD estimates of β in Eq. 3.2. Column (1) provides the baseline estimates in Table 3.2. Columns (2)-(11) provides the results across different specifications: excluding X_i (Column (2)), excluding triangular weights (Column (3)), and using alternative bandwidth choice (Columns (4)-(11)). "Mean, Pre-policy" refers to the average of dependent variable from -1 to -12 month prior to age 60, which is the cut-off value. See the notes to Table 3.1 for further details regarding the analysis sample. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.C.5. Placebo Exercise: Donated Livers and Hearts

	(1)	(2)
	Organ Type	
	Liver	Heart
1(Age≥ 60)	-0.0140	0.0039
	(0.0219)	(0.0129)
Controls	Yes	Yes
Bandwidth	±32 mo	±32 mo
Degree of polynomial	1	1
Weighting Scheme	Triangular	Triangular
Std error	Robust	Robust
Mean of Dep. Var	0.136	0.000
Observation	4,904	302

Notes: This table presents the RD estimates of β in Eq. 3.2. The sample consists of deceased donor lungs and hearts recovered for transplant from the donors whose kidneys are observed in the main analysis sample. "'Mean, Pre-policy" refers to the average of dependent variable from -1 to -12 month prior to age 60, which is the cut-off value. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.C.6. Placebo Exercise: Different Age Cut-Offs

	(1)	(2)	(3)	(4)	(5)
Age 52	-0.0170				
	(0.0130)				
Age 55		-0.0089			
		(0.0139)			
Age 57			-0.0198		
			(0.0153)		
Age 63				-0.0009	
				(0.0211)	
Age 65					0.0130
					(0.0238)
Controls	Yes	Yes	Yes	Yes	Yes
Bandwidth	±32 mo	±32 mo	±32 mo	±32 mo	±32 mo
Degree of polynomial	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Robust	Robust	Robust	Robust	Robust
Mean of Dep. Var	0.235	0.258	0.293	0.397	0.435
Observation	18,470	17,200	15,834	9,622	8,039

Notes: This table presents the RD estimates of β in Eq. 3.2. Each column provides the RD estimates when placebo cut-off values are used to estimate Eq. 3.2. "Mean, Pre-policy" refers to the average of dependent variable from -1 to -12 month prior to age 60, which is the cut-off value. See the notes to Table 3.1 for further details regarding the analysis sample. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.C.7. Placebo Exercise: Prior to Implementation of ECD Scheme

	(1)	(2)
	Sample: Pre-ECD Sample	
	1(Discarded)	KDRI (2014)
1(Age$\geq$ 60)	0.0138	0.0082
	(0.0408)	(0.0145)
Controls	Yes	Yes
Bandwidth	$\pm$32 mo	$\pm$32 mo
Degree of polynomial	1	1
Weighting Scheme	Triangular	Triangular
Std error	Robust	Robust
Mean of Dep. Var	0.272	1.424
Observation	1,904	1,904

Notes: This table presents the RD estimates of β in Eq. 3.2. The sample is constructed using the restricted Scientific Registry of Transplant Recipients (SRTR) datasets, which cover information on the universe of kidneys recovered from deceased donors in the U.S. Each observation is a deceased donor kidney. The sample is restricted to donated kidneys (i) recovered from donors within 32 months from age 60, (ii) recovered from deceased donors whose kidney biopsy information is complete, (ii) recovered from donor hospitals which did not experience any change in OPO affiliation between January 2000 and September 2002. “Kidney discard rate” is computed by dividing the number of kidneys not used for transplant by the number of kidneys recovered for transplant. “Mean, Pre-policy” refers to the average of dependent variable from -1 to -12 month prior to age 60, which is the cut-off value. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.C.8. Effects on Kidney Offer Acceptance

	(1)	(2)	(3)	(4)	(5)
	Baseline	Bandwidth Choice			
		24	28	36	40
1(Age$\geq$ 60)	0.0383***	0.0483***	0.0418***	0.0350**	0.0314**
	(0.0146)	(0.0168)	(0.0156)	(0.0138)	(0.0131)
Controls	Yes	Yes	Yes	Yes	Yes
Bandwidth	$\pm$32 mo	$\pm$24 mo	$\pm$28 mo	$\pm$36 mo	$\pm$40 mo
Degree of polynomial	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Robust	Robust	Robust	Robust	Robust
Mean of Dep. Var	0.871	0.871	0.871	0.871	0.871
Observation	9,591	7,270	8,500	10,707	11,961

Notes: This table presents the RD estimates of β in Eq. 3.2. The dependent variable is an indicator for transplant candidates accepting the initial offer. The sample consists of transplant candidates who are willing to receive ECD kidney offers and receive initial offers of donated kidneys included in the kidney-level data. The baseline sample consists of the initial offers of donated kidneys recovered within 32 months from their donors reaching age 60. "Mean, Pre-policy" refers to the average of dependent variable from -1 to -12 month prior to age 60, which is the cut-off value. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.C.9. Kidney Offer Acceptance by Donor's Health

	(1)	(2)
	Number of Comorbidities	
	Zero or One	Two or Three
1(Age$\geq$ 60)	0.0625**	0.0242
	(0.0269)	(0.0169)
Controls	Yes	Yes
Bandwidth	$\pm$32 mo	$\pm$32 mo
Degree of polynomial	1	1
Weighting Scheme	Triangular	Triangular
Std error	Robust	Robust
Mean of Dep. Var	0.832	0.890
Observation	3,933	5,658

Notes: This table presents the RD estimates of β in Eq. 3.2. The dependent variable is an indicator for transplant candidates declining the initial offer. The sample consists of deceased donor kidneys whose top-ranked offers were presented to candidates who opted for ECD kidneys at listing. The sample is further restricted to deceased donor kidneys recovered within 32 months from their donors reaching age 60. "Mean, Pre-policy" refers to the average of dependent variable from -1 to -12 month prior to age 60, which is the cut-off value. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.C.10. Kidney Offer Acceptance by Transplant Center

	(1)	(2)
	Decline Rate Prior to ECD Scheme was adopted	
	Low	High
$1(\text{Age} \geq 60)$	0.0212	0.0609***
	(0.0220)	(0.0197)
Controls	Yes	Yes
Bandwidth	±32 mo	±32 mo
Degree of polynomial	1	1
Weighting Scheme	Triangular	Triangular
Std error	Robust	Robust
Mean of Dep. Var	0.865	0.877
Observation	4,627	4,583

Notes: This table presents the RD estimates of β in Eq. 3.2. The dependent variable is an indicator for transplant candidates declining the initial offer. The sample consists of deceased donor kidneys whose top-ranked offers were presented to candidates who opted for ECD kidneys at listing. The sample is further restricted to deceased donor kidneys recovered within 32 months from their donors reaching age 60. "Mean, Pre-policy" refers to the average of dependent variable from -1 to -12 month prior to age 60, which is the cut-off value. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

Table 3.C.11. Characteristics of Transplant Candidates with Top Offers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Wait time (days)	Age	Female	Race: White	Previous TX	Blood O	Blood A	Blood B	Blood AB
1(Age$\geq$ 60)	20.8225	0.8232	-0.0118	0.0187	-0.0425**	0.0096	-0.0193	0.0031	0.0066
	(62.0982)	(0.5991)	(0.0221)	(0.0224)	(0.0205)	(0.0222)	(0.0219)	(0.0148)	(0.0106)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo	$\pm$32 mo
Degree of polynomial	1	1	1	1	1	1	1	1	1
Weighting Scheme	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Std error	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust
Mean of Dep. Var	1452.486	50.587	0.417	0.490	0.311	0.415	0.400	0.123	0.062
Observation	9,591	9,591	9,591	9,591	9,591	9,591	9,591	9,591	9,591

Notes: This table presents the RD estimates of β in Eq. 3.2. Each column represents a separate regression in which the dependent variable is one of characteristics of transplant candidates with initial offers. Each column heading indicates the kidney characteristic used as the dependant variable to estimate Eq. 3.2. The sample consists of deceased donor kidneys recovered within 32 months from donor age 60. “Mean, Pre-policy” refers to the average of dependent variable from -1 to -12 month prior to age 60, which is the cut-off value. * p<0.1, ** p<0.05, *** p<0.01. Robust standard errors in parentheses.

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