The Effect of Paid Leave of Absence Legislation on Living Kidney Donation in the United States

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Abstract

This article investigates the causal impact of paid leave of absence legislation to serve as an organ donor on living related and unrelated kidney donation rates in the U.S. We create a synthetic control group that mimics the pre-legislation living kidney donation rates of the states that enacted paid leave of absence legislation. We then compare the living kidney donation rates of the synthetic states with no legislation to the paid leave-enacted states for the period 1988-2010. Of the 16 states analyzed that passed the legislation, the paid leave was found to be effective only in California; the passage of paid leave of absence legislation increased living unrelated kidney donation rates by about 2 percent in California relative to a comparable synthetic California in the absence of legislation. However, our analysis was unable to identify a causal effect on living related kidney donation rates in any of the states that passed the legislation.

JEL Classification: I18; K32; C15

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1 INTRODUCTION

Rising prevalence of diabetes and hypertension together with the advances in transplant technology led to an ever-growing demand for transplantable organs. As of November 2013, there are about 99,000 patients waiting for a kidney transplant in the US. However during 2013, about 11,000 kidneys are recovered of which only about 3,700 were from living donors. Despite the efforts, the yearly additional number of patients registered for a transplant far outstrips the yearly number of removals from the waiting list, resulting in an increasing shortage of transplantable organs in the U.S. In an effort to alleviate organ shortages and increase living donation rates, several states in the U.S. passed paid leave of absence and tax incentive legislations to compensate living donors.

This study contributes to the empirical literature by investigating the causal impact of paid leave of absence legislation for public employees in the U.S states for the period of 1988-2010 using a novel and superior methodology compared to the difference-in-differences (DiD) estimator employed in the literature. In the context of potential outcomes framework for causal inference, we ask the following research question: how the living kidney donation rates would have evolved in a particular paid leave-enacted state if the legislation had not been enacted. To answer this question, we invoke the synthetic control method developed by Abadie and Gardeazabal (2003) and extended by Abadie et al. (2010). We create a synthetic control group that mimics the pre-legislation living kidney donation rates of the states that enacted paid leave of absence legislation by using the convex combination of other states that have not enacted any legislation. Causal inference is carried out by comparing the living kidney donation rate of the synthetic states (absence of legislation) against that of the paid leave-enacted states in the post-legislation period. We find that the passage of paid leave of absence legislation increased living unrelated kidney donation rates by about 2 percent in California relative to a comparable synthetic California without paid leave of absence legislation. However, our analysis was unable to identify a causal effect on living related kidney donation rates in any of the states that passed the legislation. This suggests that the legislation may help removing barriers to living kidney donation for unrelated persons but only in California. On the other hand, paid leave legislation is found to be ineffective in Colorado, New York and Kansas and unidentifiable in the remaining twelve states in which the legislation is in effect. Potential explanations of the latter finding are that the scale of the legislation may be too narrow to detect the causal effect; some states may have extremely high/low donation rates leading to the failure to satisfy the convex-hull criteria; and the donor pool may not consist of states with similar observable characteristics to that of the treated state's in order to yield a good approximation due to the fact that a very large number of states have passed such legislations and greatly reduced the number of comparable control states.

Section 2 discusses the background on state legislation, section 3 discusses the synthetic control method and its advantages over the traditional panel regression analysis, sections 3.2 and 3.3 respectively present the results and the placebo studies, section 3.4 performs a robustness check and section 4 discusses the findings of the analysis and its policy implications.

2 BACKGROUND ON STATE LEGISLATION

It is known that individuals willing to become living donors are exposed to serious financial and medical risks and that these risks may generate disincentives or barriers to donation. In the face of insurmountable shortage and widely acknowledged repugnance for explicit monetary incentives for donation, an increasing number of proposals emphasizes the urgent need to introduce reimbursement for living donors for non-medical costs incurred throughout the process of donation (Delmonico et al., 2002; Gaston et al., 2006; Klarenbach et al., 2006a; Matas, 2007; Abouna, 2008). The idea is to provide some monetary compensation that is ethically and politically acceptable while retaining the giftlike features of the exchange (Healy, 2006). Avoiding the language of the market also allows both the transplant community and the public to regard reimbursement or compensation as a slight modification of the current system without resorting to drastic changes (Mahoney, 2009). Reimbursement schemes of this kind include compensation for the costs of travel, lodging, forgone earnings, social security in the form of life insurance or long-term health insurance. A survey by the Gallup (2005) showed that 52 percent of Americans strongly supports compensation of living donors for the costs of travel, child care and forgone earnings and 73 percent stresses that living donors should not be denied health or life insurance after donation.

A second line of proposals discusses tax deduction schemes to exhort individuals to serve as an organ or bone marrow donor (Calandrillo, 2004; Milot, 2008). Tax deduction that may be claimed for organ donation is subtracted from the gross income at the time of filing tax returns. As a result this lowers the overall taxable income and the amount of tax paid. Tax deductions are peculiarly regressive because they depend on the tax bracket (Calandrillo, 2004). A tax credit might correct this inequity problem because it is independent of tax bracket and it reduces the tax owed rather than reducing taxable income. Milot (2008) stresses that tax deduction, albeit it prima facie appears to be a sound legislative approach, turns an otherwise non-tax event into a tax item that increases complexity of the tax system and provides differential tax returns to those who become living organ donors because it depends on the tax bracket. At the national level, a study by Boulware et al. (2006) reveals that of those 845 participants surveyed, 91 percent were in favor of reimbursement of medical costs, 84 percent were in favor of paid leave but only 35 percent were in favor of tax deduction/credit.

Within the last two decades, several states in the U.S. enacted legislation that allows individuals to take paid leave of absence or to receive tax deduction should they decide to become living donors. In 1998, Colorado became the first state that allows paid leave of absence for prospective donors followed by Wisconsin and Maryland in 2000[1]. A number of states further enacted legislation that allows a $10,000 tax deduction or to receive tax credit to serve as an organ or bone marrow donor which may be claimed for lost wages, travel, lodging and medical expenses. Tax deduction was first introduced in the states of Wisconsin and Georgia in 2004 followed by Arkansas in 2005. Since 1998, 35 states in the U.S enacted some type of compensation legislation for living donors. Figure 1 shows the yearly number of states by enactment year of paid leave of absence and tax incentive legislations.

The first study that investigates the impact of state legislation and federal policies on living kidney donation rates in the U.S. was carried out by Boulware et al. (2008). The findings show that the state legislation and the federal policies were not associated with sustained improvements in the larger number of living related donations and therefore overall living donation rates. On the other hand, state and federal policies were positively associated with living unrelated kidney donations. This indicates that legislation related to compensation of living donors may selectively decrease barriers to living kidney donation from unrelated persons and does not provide additional incentives for related donors. Boulware et al. (2008) emphasize that most of the states that enacted legislation to compensate living donors allow compensation of public employees only for becoming an organ donor and/or address only employed persons with sufficiently high levels of income to benefit from a $10,000 tax deduction or credit, indicating that those who could

[1] See Boulware et al. (2008) and Lacetera et al. (2012) for a detailed description of state legislation and federal initiatives for the compensation of living organ donors in the US. A concise and updated version of state legislation can be found in table 2. For a detailed description of international legislation on reimbursement of living donors consult Pattinson (2003) and Klarenbach et al. (2006b).

potentially benefit from the legislation may be a very small group. Wellington and Sayre (2011) examines the association between financial incentives that either allow for tax deduction or 30 days of paid leave of absence and organ donations in the U.S. The results, in line with the findings of Boulware et al. (2008), show that state legislation is not associated with overall living donations. A recent study by Lacetera et al. (2012) employs a DiD methodology to assess the impact of paid leave of absence legislation on organ and bone marrow donation and concludes that the legislation had no overall effect on the number of organ donations but exerts a positive effect on bone marrow donation.

Albeit the existing strand of empirical research reaches a consensus, they share two common methodological shortfalls. First, they are unable, by design, to assess the individual heterogenous causal effects. By employing the synthetic control method, this paper accounts for unobservable and time-varying state-level heterogeneity, reduces other potential biases that might persist in a traditional regression analysis, and most importantly identifies the state-specific causal effect of paid leave of absence legislation. As we show in the following sections, in a treatment sample of sixteen states that passed the legislation, the paid leave of absence legislation has a causal effect on living unrelated kidney donation rates only in California. By employing a DiD methodology, one is likely to find no effect of the legislation due to pooling of states and the fact that a very small causal effect in a treatment unit may be clouded by zero causal effects in other treated units and that the average treatment effect on the treated is too small to detect. This problem sheds light on the importance of identifying heterogenous causal effects in assessing the impact of the legislation when only very few states are causally affected by the legislation.

A second potential problem is that the introduction of paid leave of absence legislation may not be exogenous to kidney donations for the changes may be more likely related to lobbying in organ transplantation. When state policies respond, given enough constituents care about organ donation, we might expect benefits for organ donors to arise in years after a large number of living donors have made donations. Mean reversion would push the estimates of the effects of the legislation down as the states that have the policies had unusually high-donation rates in the pre-legislation period. As a result, a positive increase in donation could look insignificant if these effects are at play. The synthetic control method is robust to this type of endogeneity if the identifying assumptions are met. In the following section, we seek to answer, along the lines of Abadie et al. (2010), how the living kidney donation rates at the state level would have evolved in the absence of paid leave of absence legislation.

3 SYNTHETIC CONTROL METHOD

The synthetic control method, developed by Abadie and Gardeazabal (2003) and extended by Abadie et al. (2010) and Abadie et al. (2012), has a number of advantages over the traditional panel data regression framework. In traditional regression analysis, the choice of the comparison units is left at the discretion of the researcher, based on the degree of similarity between the control and the treated units. In the synthetic control method, the choice of the appropriate counterfactual is selected from a pool of unaffected units (the *donor pool*) to compare to the treated state. The appropriate comparison unit is based on observable and quantifiable characteristics which reduce subjectivity and allows for better causal inference (Abadie et al., 2010).

Unlike traditional regression analysis which relies on extrapolation outside the support of the data, the synthetic control method constructs a linear combination of states that have not enacted paid leave of absence legislation with positive weights that sum to one. The traditional regression also, but rather in an implicit way, computes these weights which can be positive or negative but not necessarily sum to one. This may lead to severe extrapolation biases. In synthetic control method, the weights are assigned to states in the donor pool in such a way that the pre-legislation living kidney donation rates and the covariates that are thought to influence living kidney donation rates are comparable to those of the treated state before the passage of legislation[2]. This comparability is determined by the minimization of root mean square prediction error (hence RMSPE) in the pre-legislation period, which measures the lack of fit between the trajectory of the outcome variable and its synthetic counterpart (Abadie et al., 2012)[3].

The synthetic control method allows the effect of unobservable state heterogeneity to vary over time. In the fixed effects (FE) model and the DiD estimator, the effect of unobservable heterogeneity is assumed to be fixed over time. Hence, the synthetic control method provides an improvement over FE and DiD methods and deals better with endogeneity caused by the presence of time-varying unobservable confounders, all of which are presumed to have been accounted in the traditional regression framework. Throughout our analysis we maintain the Stable Unit Treatment Value Assumption (SUTVA) such that the potential outcome for any state do not vary with the legislation enacted in other states. Specifically, the paid leave of absence legislation should not have an effect on states that did not pass the legislation (absence of geographical spillover). Hypothetically speaking, if the legislation in California had positive spillover effects on kidney donations in the weight-assigned control states, for example Nevada and Arizona (i.e. states that did not pass such legislation), then the synthetic control states would underestimate the counterfactual kidney donation rate trajectory of the treated state in the absence of legislation and the synthetic control estimates would be biased downward. On the other hand, spillover effects on states not included in the synthetic control do not affect synthetic control estimates (Abadie et al., 2012).

3.1 Data and Sample

We use state-level panel data for the period 1988-2010. Until 2010, 31 states in the U.S enacted paid leave of absence legislation for state employees and 15 states did not enact any type of legislations. Of the 31 states that enacted paid leave of absence, one state (Idaho) was discarded due to lack of data on the number of living kidney donors and 14 states (Arkansas, Georgia, Iowa, Mississippi, Utah, Illinois, Pennsylvania, Minnesota, New Mexico, Connecticut, Maine, South Carolina, Wisconsin, North Dakota) were discarded because they have further enacted tax deduction/credit or paid leave for private employees on the same or near period of the passage of paid of leave of absence legislation (i.e. one cannot isolate the effect of paid leave and the effect of tax incentive legislation). This leaves 16 states or treated units to be used in the synthetic control. On the other hand, of the 15 states that enacted none of the above laws, 2 states (Montana, Wyoming) were discarded due to lack of data on the number of living kidney donors. This leaves a total of 13 states to be considered in the donor pool for each of the 16 treated states. Table 1 displays the assignment of treated and donor states.

The set of characteristics we consider to synthetize our outcome variables, the living related and unrelated kidney donation rates per million adult population (pmap) for 16 states that enacted paid leave of absence are

[2]For technical details, consult section C of the appendix and Abadie et al. (2010) p:494-496.

[3]The pre-legislation RMSPE is $\left(\frac{1}{T_0}\sum_{t=1}^{T_0}\left(Y_{1t}-\sum_{j=2}^{P+1} w_j^* Y_{jt}\right)^2\right)^{1/2}$ and the post-legislation RMSPE is $\left(\frac{1}{T_1}\sum_{t=T_0+1}^{T_1}\left(Y_{1t}-\sum_{j=2}^{P+1} w_j^* Y_{jt}\right)^2\right)^{1/2}$ where T_0 and T_1 are the pre-legislation and post-legislation number of periods respectively, $w_j^* Y_{jt}$ is the synthetic living kidney donation rates using the j^{th} state, absent legislation, with weight w^* and Y_{1t} is the actual living kidney donation rate of the paid leave-enacted state.

the living related and unrelated kidney donation rates of the control states without any such legislation for every year in the pre-legislation period, kidney waiting list additions per million population (pmp), deceased kidney donations pmap, the number of transplant centers pmap, the prevalence of end-stage renal disease (ESRD) pmp, traffic fatalities pmp, cerebrovascular deaths pmp and real GDP per capita for the period 1988-2010. The living related and unrelated kidney donation rates were available for each year under consideration, hence used for every year in the pre-legislation period. The remaining covariates are used when available at least for one year in the pre-legislation period.

Our pre-legislation period ranges from 10 to 17 years depending on the year in which a particular state enacted the paid leave of absence legislation[4]. With a large number of pre-legislation periods, matching on pre-legislation outcomes allows to control for heterogenous responses to multiple unobserved factors. The intuition is that only states that are alike in both observed and unobserved determinants as well as in the effect of those determinants on kidney donation rates should produce similar trajectories of the kidney donation rates over extended periods of time (Abadie et al., 2012).

State data on the number of living related and unrelated living adult kidney donors, the number of deceased kidney donations, kidney waiting list additions (candidates) and the number of transplant centers are retrieved from the Organ Procurement and Transplantation Network (OPTN)[5]. Total state population and the population over the age of 18 are obtained from the US Census Bureau[6]. The number of living related and unrelated kidney donors, the number of deceased kidney donations and the number of transplant centers are divided by the respective adult population and multiplied by million to obtain the per million adult population (pmap) rates. Kidney waiting list additions are divided by the respective total state population and multiplied by million to obtain the per million population (pmp) rates. The prevalence of ESRD pmp is retrieved from the US Renal Data System (USRDS) 2010 Annual Report[7]. The real GDP per capita (in 2005 US dollars) is obtained from the US Department of Commerce, Bureau of Economic Analysis[8]. The number of traffic fatalities is retrieved from the National Highway Traffic Safety Administration, Fatality Analysis Reporting System (FARS)[9]. The number of cerebrovascular deaths is retrieved from CDC-WONDER[10]. Both variables are expressed in pmp rates. The state legislation is collected from the National Kidney Foundation[11], TransplantLiving[12], National Conference of State Legislatures[13], Boulware et al. (2008) and Lacetera et al. (2012).

In the final specification for which we report our results, we dropped all the covariates except the living kidney donation rates of the unexposed control states for every year in the pre-legislation period because

[4]The earliest year of passage of legislation is 1998 and the latest is 2005.

[5]In the U.S., most of the kidney transplants from living donors are associated with kidneys donated by relatives, spouses or partners, known as *living related donation*. In our sample, related donation is composed of donations by blood related child, full sibling, half sibling, identical twin, other relative, parent, spouse and life partner. In order to increase the number of living donor organ transplants, individuals are further allowed to direct their donation to a specific but unrelated (sometimes unknown) individual, known as *living unrelated donation*. This type of living donors are those who do not have any biological, romantic or legal ties with the organ recipient. In our sample, unrelated donation is composed of non-biological anonymous donations and other unrelated directed donations only. Pairwise kidney exchanges (PKEs) have been excluded from unrelated donations although they are coded as such by OPTN. The reason for excluding PKEs is that the decision to donate by a relative to a biologically unrelated person under pairwise kidney exchange is conditioned upon his/her recipient receiving a kidney from another biologically unrelated person. Thus the motivation under pairwise exchanges is not based upon the same reasons to donate under typical unrelated donations. See http://optn.transplant.hrsa.gov/.

[6]http://www.census.gov/popest/states.html

[7]http://www.usrds.org/reference.htm

[8]http://www.bea.gov/regional/gsp/

[9]www.-fars.nhtsa.dot.gov/States/StatesFatalitiesFatalityRates.aspx.

[10]http://wonder.cdc.gov/controller/datarequest/D72;jsessionid=2D458CFDCFCF17D2F52A54B2A78D71FF

[11]http://www.kidney.org/transplantation/LivingDonors/pdf/LDTaxDed_Leave.pdf

[12]http://www.transplantliving.org/livingdonation/financialaspects/legislation.aspx

[13]http://www.ncsl.org/default.aspx?tabid=13383

neither did other covariates improve the pre-legislation fit of the trajectory of the living kidney donation rates nor yield accurate donation rate trajectories when all the covariates except the living kidney donation rates of the unexposed control states were used. This implies that the pre-legislation actual donation trajectory is best reproduced by some linear combination of the donation rates of the unexposed states.

3.2 Results

With two types of outcome variables (related and unrelated donation rates) for 16 treated states, we obtain 32 distinct synthetic controls, 32 placebo runs and 32 placebo distributions for a total of 96 figures. For space concerns, we omit the results of the synthetic controls and the results of the subsequent placebo studies for all states for which the synthetic control method was unable to reproduce the pre-legislation donation rate trajectory. As a result, the synthetic control method was unable to reproduce the pre-legislation living related kidney donation rate trajectory for any of the states that passed the legislation. For living unrelated donation rates, of the 16 states which enacted paid leave of absence legislation, the synthetic control method was able to reproduce the pre-legislation unrelated donation rate trajectory only for four states (California, Colorado, New York and Kansas)[14]. We therefore report here and in the following sections the results of the analysis for these four states' unrelated donation rates only. The entire analysis for the remaining twelve treated states is available from the author upon request.

Figure 2 plots the trends in living unrelated kidney donation rates for California, Colorado, New York and Kansas and their synthetic counterparts over the period 1988-2010[15]. The synthetic living unrelated kidney donation rate trajectory is constructed by using the convex combination of states in the donor pool that closely resembled the treated state before the passage of paid leave of absence[16]. In California, the synthetic unrelated donation rate trajectory almost perfectly reproduces the actual unrelated donation rate trajectory in the pre-legislation period. In the next four years following the passage of paid leave, the synthetic unrelated donation rate keeps rising while the actual unrelated donation rate in California rises until 2004 and begins to fall which lasts until 2006, and then rises again.

The estimate of the impact of paid leave of absence legislation for a treatment state is given by the difference between the actual and the synthetic unrelated kidney donation rates in the post-legislation period. Our findings suggest that in the post-legislation period, the living unrelated kidney donation rates in California increased on average by 2 percent relative to synthetic California in the absence of legislation[17]. The paid leave of absence legislation increased living unrelated kidney donation rates by about 37 percent in Colorado but decreased by 1.74 percent in New York and 43.2 percent in Kansas relative to synthetic Colorado, synthetic New York and synthetic Kansas respectively.

[14] When the synthetic control method fails to reproduce the actual outcome trajectory for a number of states in the pre-legislation period, an alternative and potentially promising strategy is to aggregate the living kidney donation rates and the predictors that are thought to influence living kidney donation rates of the paid-leave-enacted states by the enactment year. State aggregation could result in an increase in the power of the synthetic control but poses problems in the discussion of the results with respect to policy implications since the treated units are no longer states but a collection of states. We avoid state aggregation because the correct unit of observation at which the laws and the policy decisions are made is the state level.

[15] The actual and the synthetic trends for New York end in 2005 because New York further passed tax deduction legislation in 2006.

[16] We use the `synth` command in STATA, which can be found at http://www.mit.edu/~jhainm/synthpage.html

[17] This causal effect is calculated by taking the ratio of the difference between the average unrelated donation rate of California and the average unrelated donation rate of the synthetic California to the average unrelated donation rate of the synthetic California in the post-legislation period.

3.3 Inference about the impact of state legislation

In order to ensure that a particular synthetic control estimate reflects the impact of the legislation (i.e. the synthetic controls provide good predictors of the trajectory of living kidney donation rate in the pre-legislation periods), we perform a series of falsification tests known as *in-space placebo* test, in which we artificially reassign the legislation period to each of the 13 states which did not enact paid leave of absence legislation and shift the treated state into the donor pool. If a particular state enacted the legislation and other states did not, our expectation is that the control states that are subject to the synthetic control method should not be affected by the legislation, thus should not yield a diverging actual and synthetic living kidney donation rate akin to that of the treated state in the post-legislation period. Therefore, our confidence that a sizeable synthetic control estimate reflects the effect of legislation would be severely undermined if similar or larger estimated kidney donation rate gaps are obtained when the legislation is artificially assigned to states that did not enact such legislation (Abadie et al., 2010)[18].

Figures 3 and 4 report the results of the placebo experiments for living unrelated kidney donation rates in California, Colorado, New York and Kansas. The black line represents the estimated living unrelated kidney donation rate gap and the gray lines represent the estimated living unrelated kidney donation rate gap for states that did not enact the legislation. In order to infer that the paid leave of absence legislation imposes a significant positive effect on kidney donation rate, the estimated gap (synthetic - actual) of the treated state should be very close to zero in the pre-legislation period (i.e. should yield a good fit) but should also stand out in the post-legislation period (i.e. the gap should be as negative as possible) relative to the estimated gaps for the states in the donor pool. Otherwise, we would either infer that the post-legislation donation gap is artificially created by a lack of fit (Abadie et al., 2010), or the legislation does not have any sizeable impact on donation rates of the treated state. The former may happen when the pre-legislation fit is poor and the latter may happen when the synthetic living kidney donation rate closely tracks the actual over the entire sample period. In addition to the placebo results for all 13 control states, we report two sets of the placebo results for a restricted number of placebo runs based on the mean square prediction error (MSPE) cut-off level.

The left panel in figures 3 and 4 shows the living unrelated kidney donation rate gaps in treatment states and placebo gaps in all 13 control states. In the mid panel of figures 3 and 4, we discard all the states with a MSPE five times higher than that of treated state's. At this cutoff level, 8, 1, 4 and 4 states are discarded from the donor pool for California, Colorado, New York and Kansas respectively. In the right panel of figure 3 and 4, we discard all the states with a MSPE two times higher than that of the treated state's. At this cut-off level, a total of 13, 5, 7 and 8 states are discarded for California, Colorado, New York and Kansas respectively. In figure 3, the synthetic living unrelated kidney donation rate yields a good fit for California in the pre-legislation period. In the post-legislation period, the estimated gap widens (in absolute value) and clearly stands out. As shown below, not only the post-legislation living unrelated kidney donation rate gap in California is among the largest of all placebo gaps but also the effect of the paid leave of absence legislation on living unrelated kidney donation rates in California is causal rather than a random effect.

In order to assess whether the estimated effect is causal, we apply the synthetic control method to estimate *in-space* placebo kidney donation gaps for every potential control state in order to create a distribution of placebo effects. This distribution enables us to identify the exact significance level of the estimated effect of the legislation. Our confidence that a sizeable synthetic control estimate reflects the effect of the legislation

[18] A second array of falsification tests is known as *in-time placebo* tests, in which the passage of paid leave of absence legislation is artificially reassigned to dates earlier than the actual date. We did not perform in-time placebos because our sample period is not sufficiently long to assign an artificial date of passage of legislation.

would be severely undermined if the estimated gap fell well inside the distribution of placebo gaps (Abadie et al., 2012). This would imply that our results are driven by randomness rather than causality. In other words, a significant causal effect of the legislation in the treated state requires that the estimated effect should be unusually large relative to the distribution of placebo effects. The estimated effect of the legislation for the treated state is evaluated by calculating the ratio of post-legislation RMSPE to pre-legislation RMSPE that are equal to or greater than the one for the treated state. This ratio is the p-value that can be interpreted as the probability of obtaining a post/pre-legislation RMSPE that is at least as large as the one obtained for the treated state when the legislation is artificially and randomly reassigned to a state that did not enact such legislation (Abadie et al., 2012).

Figure 5 plots the distribution of placebo effects for California, Colorado, New York and Kansas and for every 13 states in the donor pool for unrelated donations. The estimated living unrelated kidney donation rate gap fell well outside the distribution of placebo gaps only for California. This means that, if a state would have been randomly selected from the sample, the probability of obtaining a post/pre-legislation RMSPE ratio as high as that of California would be $2/14 = 0.1429$. Only one control state in the sample (South Dakota) achieves a ratio higher than that of California's. On the other hand, the estimated unrelated kidney donation rate gaps fell inside the distribution of placebo gaps for Colorado, New York and Kansas, indicating that the estimated effect is not causal and therefore the paid leave of absence legislation in these three states is not effective in raising donation rates.

3.4 Robustness Test

In this section, we perform a robustness check to test the sensitivity of our results to the changes in the synthetic control state weights induced by the exclusion of any particular state from the sample. From table 3, the synthetic California is constructed by the weighted average of six states, namely Alabama, Florida, Michigan, North Carolina, Nevada and South Dakota. We iteratively re-estimate our model to construct a synthetic California omitting in each iteration one of the states that was assigned a weight in table 3. Our aim is to assess the extent to which our results are driven by any particular state (Abadie et al., 2012). Figure 6 displays the results in which the black solid line is the actual living unrelated donation rate, the black dashed line is the synthetic living unrelated donation rate of California with all six weight-assigned states and the gray lines are the leave-one-out estimates.

The average of all six leave-one-out estimates of the synthetic control (gray lines) are on average 0.94 percent higher than the actual living unrelated donation rate in California (black solid line) in the pre-legislation period and 1.8 percent higher than the original synthetic California (black dashed line) in the pre-legislation period. In the post-legislation period, the average of all six leave-one-out estimates of the synthetic control are on average 4 percent higher than the original synthetic California. The leave-one-out estimate of the synthetic control is sensitive to the exclusion of Nevada and Alabama. This is shown by the upper two gray lines in the post-legislation period in figure 6.

The pre-legislation RMSPE of the leave-Alabama-out estimate of the synthetic California is 23 percent higher than the RMSPE of the original synthetic California whereas the pre-legislation RMSPE of the leave-Nevada-out estimate is more than twice the RMSPE of the original synthetic California. The leave-one-out distribution of synthetic California is not robust to the exclusion of Nevada and Alabama but the pre-legislation fit leave-Alabama-out and leave-Nevada-out estimates are also poor. This implies that if Nevada or Alabama was excluded from the donor pool, the post-legislation living unrelated kidney donation rate gap would be artificially created by a lack of fit and would show a lower-than-otherwise estimated effect of the legislation.

4 DISCUSSION

We used state-level panel data for the period 1988-2010 in order to reveal the causal impact of paid leave of absence legislation on living kidney donation rates in the U.S states. We employed the synthetic control method which is based on estimating the counterfactual: how the state living related and unrelated kidney donation rates would have evolved in the absence of legislation. Extended over a period of 8 years (from 2002 to 2010), we find an average gap of about 2 percent between the actual living unrelated kidney donation rates in California and the living unrelated kidney donation rates in a comparable synthetic California in the absence of legislation. Albeit very small, this positive effect is attributed to the passage of paid leave of absence legislation. However our results are sensitive to the exclusion of two control states; Nevada and Alabama, indicating that these states are vital comparison units for California in order to yield a good linear approximation. On the other hand, our analysis was unable to identify a causal effect on living related kidney donation rates in any of the states that passed the legislation.

It is important to discuss why a similar causal effect cannot be identified for the remaining fifteen paid-leave-enacted states' unrelated donation rates. First, the scale of the legislation may be too narrow to identify the causal effect of the legislation. The intervention has to be significant in the sense that it has the potential to exert a large-scale impact and should sustain for a period of time. The legislation for which we attempt to identify this effect affects only public employees. If the share of public employees in the state employment is too low, that is those who could potentially benefit from the legislation are a very small group, then the synthetic control may not be able to pick up any effect of the legislation in that state. To assess whether there are significant differences in the share of public employees between California and other treated states, we collected data on the share of public employees in the total state employment for the period of 2001-2010 for our 16 treated states, which we use as a proxy for the scale of legislation[19]. Our aim is to discuss whether the failure to identify a causal effect of the legislation in all states but California could be attributed to the variations in the scale of the legislation. Although a mere inspection of this distribution cannot identify whether this is true, it gives us hints about the validity of the argument. Figure 7 shows the distribution of the scale of the legislation. The average share of public employees in the total state employment ranges from 12.8 percent in Massachusetts to 20.8 percent in Oklahoma with an average share of 16.94 percent for all 16 treated states as shown by the vertical red line. Among our four treated states for which we were able to obtain good pre-legislation fits, the share of public employees for California and Colorado is below the average whereas this share is above the average of all 16 treated states for New York and Kansas. If the variations in the scale of the state legislation is what determines or causes whether the legislation is effective or not, then we would either expect to observe an unusually large share of public employees in California relative to other treated states or would have failed to identify a causal effect of the legislation in California on the grounds that the scale of the legislation is too small to identify this effect. Neither of these cases prevails in our analysis. Further, the share of public employees in total employment for any of the sixteen states is fairly large indicating that the failure to meet the requirement of minimum efficient scale in order for the legislation to function as intended may not be so severe. Other arguments may be more likely to explain our findings.

[19] Bureau of Labor Statistics, Quarterly Census of Employment and Wages (QCEW): http://data.bls.gov/pdq/querytool.jsp?survey=en

Second, the method constructs a synthetic version of the paid leave-enacted (treated) state by using some linear weighted combination of states that did not enact the legislation (convex-hull criteria). This means that the synthetic control would not yield accurate trajectories of the states with extreme values of the variable of interest and other observable characteristics. The synthetic control method would fail to reproduce the pre-legislation living kidney donation rates of the most “egotistic” and the most “altruistic” treated state in the sample because the linear weighted combination of the living kidney donation rates of the donor states cannot yield a synthetic donation rate as low as the donation rates of the most “egotistic” and as high as the donation rates of the most “altruistic” treated state. This may explain the failure to reproduce the pre-legislation outcome trajectory for states like Hawaii, West Virginia and Delaware where the living kidney donation rates are extremely low and for states like Maryland where the living kidney donation rates are extremely high (results not reported).

Third, for other states, a potential explanation of this failure is that the donor pool may not consist of states with similar observable characteristics in order to yield a good approximation. Until 2010, 70 percent of all U.S states have passed some sort of compensation legislation, leaving the remaining 30 percent to be used as control units. Even though the synthetic control method does not require a large number of control units and in fact may yield good approximations even with a number of control units as few as six, the control states in our analysis may not be good comparison units for those twelve paid-leave-enacted states for which the pre-legislation fit was poor, and we were unable to identify the causal effect of the legislation in those states.

Finally, most of the states that enacted paid leave of absence legislation only allow compensation of public employees for becoming an organ donor. Failure to allow compensation for the unemployed, uninsured or low-income individuals who are a major proportion of the population and who are more likely to experience financial hardship as a result of donation hinders the effectiveness of the legislation. If the aim is to lower the barriers to living donation by offering modest monetary incentives that are ethically and politically acceptable it is suggestsed that the current legislation should be amended in order to cover a broader group of beneciaries of the legislation to ensure that the barriers to living donation are lowered and that the provisions of the amendment should be independent of whether beneciaries of the legislation are employed or not. However this might be a difficult task considering the indistinct line between offering modest rewards and outright payments prohibited by the National Organ Transplant Act of 1984.

A Tables

Table 1: Synthetic Control States

Treated States		Donor States	
California	Maryland	Alabama	New Hampshire
Colorado	New York	Arizona	New Jersey
Delaware	Ohio	Florida	Nevada
Hawaii	Oklahoma	Kentucky	South Dakota
Indiana	Texas	Michigan	Tennessee
Kansas	Virginia	North Carolina	Vermont
Massachusetts	West Virginia	Nebraska	
Missouri	Washington		

Table 2: State Legislation for Kidney Donors

State	Paid leave of absence (up to 30 days)		Tax deduction	Tax credit
	Public employees	Private employess	($10,000)	($10,000)
Alaska	2008 (HB.252)			
Arkansas	2003 (HB.1289)	2005 (ACA §11-3-205)	2005 (HB.1393)	2005 (Act No.2235)
California	2002 (AB.1825)			
Colorado	1998 (CRS 24-50-104)			
Connecticut	2007 (SB.1447)	2004 (SB.327)		
Delaware	2001 (SB.45)			
Georgia	2002 (HB.1049)		2004 (HB.1410)	
Hawaii	2005 (HRS §78-23.6)			
Idaho	2006 (SB.1373)			2006 (SB.1373)
Illinois	2002 (HB.0411)	2005 (HB.324)		
Indiana	2002 (HB.1030)			
Iowa	2003 (HB.381)		2005 (HF.801)	
Kansas	2001 (Exec. Order 2001-02)			
Lousiana			2005 (SB.26)	
Maine	2002 (26 MRSA §843)	2002 (26 MRSA §843)		
Maryland	2000 (SB.17)			
Massachusetts	2005 (149:33E)			
Minnesota	2006 (MN Stat §181.9456)		2005 (HF.785)	
Mississippi	2004 (SB.2639)	2004 (SB.2639)	2006 (HB.1512)	
Missouri	2001 (HB.679)			
New Mexico	2007 (NM Stat §24-28-3)		2005 (HB.105)	
New York	2001 (AB.4138)		2006 (AB.372)	
North Dakota	2005 (SB.2298)		2005 (HB.1474)	
Ohio	2001 (HB.326)		2007 (HB.119)	
Oklahoma	2002 (SB.1628)		2007 (SB.806)	
Oregon				1991
Pennsylvania		2006 (35 PS §6120.3)		2006 (35 PS §6120.3)
Rhode Island			2009 (SB.76)	
South Carolina	2002 (SB.830)	2006 (SC Code §8-11-65)		
Texas	2003 (HB.89)			
Utah	2002 (SB.125)		2005 (SB.164)	
Virginia	2001 (HB.1642)		2005 (VA Code §58.1-322)	
Washington	2002 (Exec. Order 02-01)			
West Virginia	2005 (SB.240)			
Wisconsin	2000 (AB.545)		2004 (AB.477)	

Note: The following fifteen states passed none of the above laws: Alabama, Arizona, Florida, Kentucky, Michigan, Montana, North Carolina, Nebraska, New Hampshire, New Jersey, Nevada, South Dakota, Tennessee, Vermont, Wyoming.
Source: National Kidney Foundation, TransplantLiving, National Conference of State Legislatures, Boulware et al. (2008) and Lacetera et al. (2012). See text.

Table 3: Synthetic Control State Weights, Living Unrelated Kidney Donation

		Selected Treated States			
		CA	CO	NY	KS
Donor States	AL	0.074		0.121	0.150
	AZ		0.153	0.062	
	FL	0.08			
	KY			0.049	0.216
	MI	0.431			
	NC	0.065	0.668	0.211	
	NE			0.076	0.066
	NH		0.082		
	NJ				0.412
	NV	0.297	0.049	0.22	0.100
	SD	0.052	0.049		
	TN			0.23	
	VT			0.032	0.056

Source: Author's own calculations.

B Figures

Figure 1: Number of U.S states by enactment year, 1998-2009

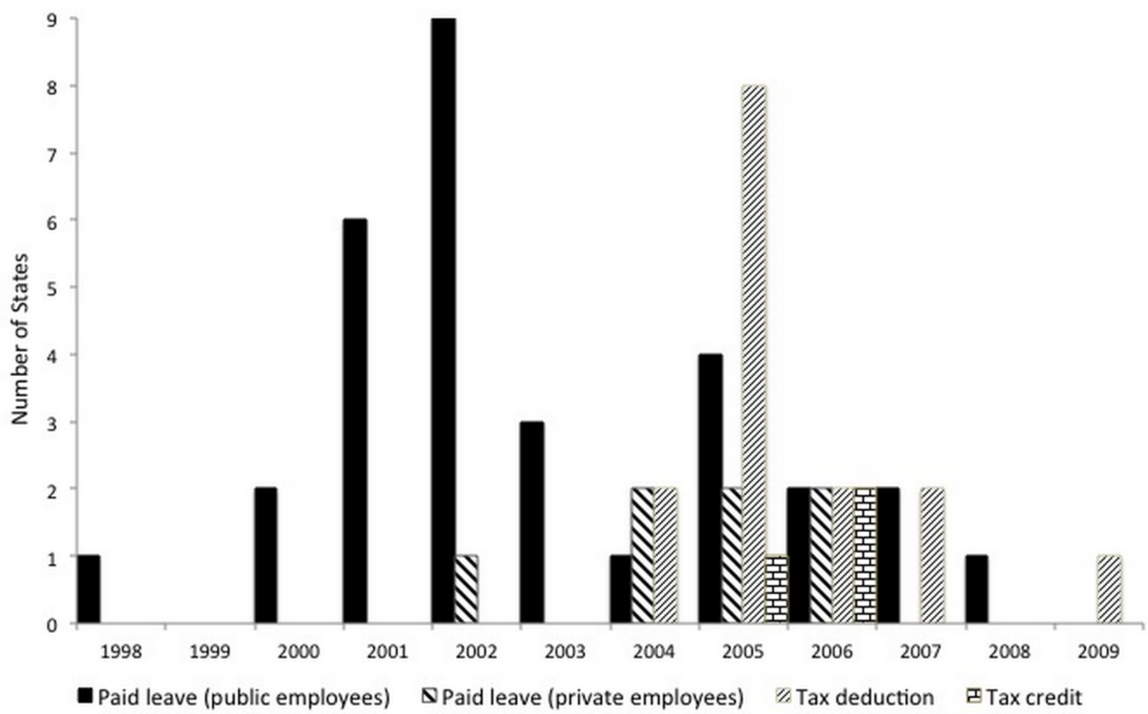
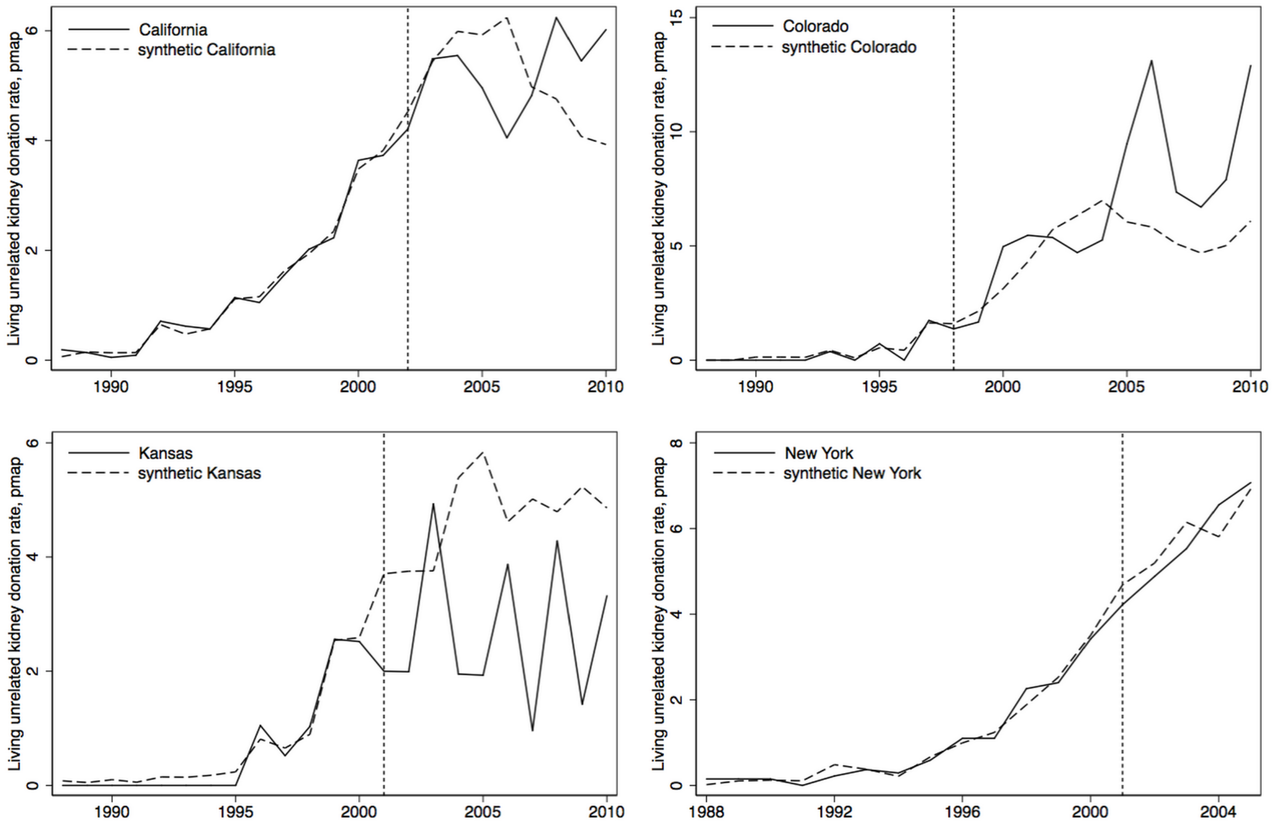
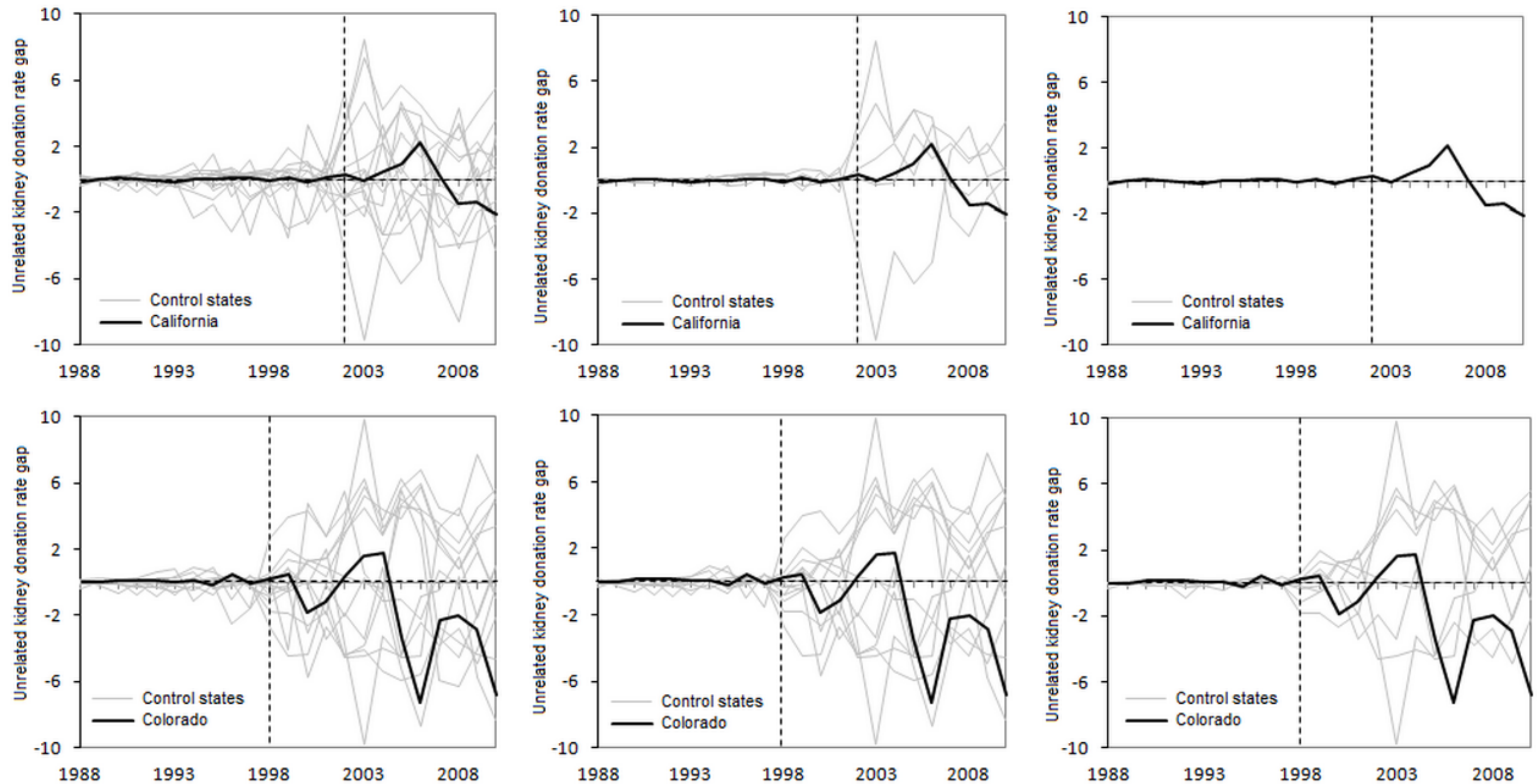


Figure 2: Trends in living unrelated kidney donation rates: treated vs. synthetic states



Notes: The actual and synthetic trends for New York end in 2005 because New York further passed tax deduction legislation in 2006.

Figure 3: In-space placebo experiments, living unrelated kidney donation (Panel I)

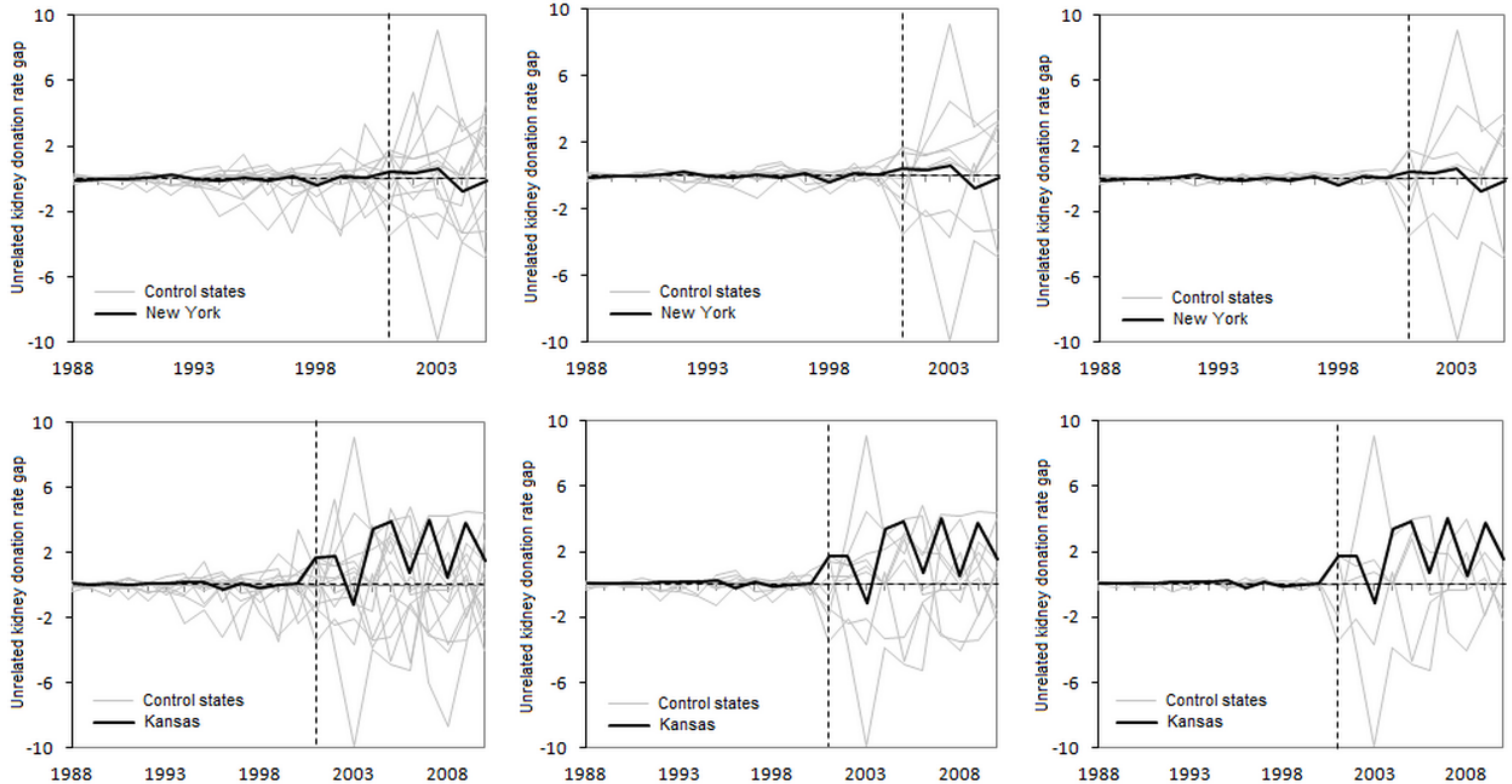


Unrelated kidney donation rate gaps in treatment states and placebo gaps in control states

Unrelated kidney donation rate gaps in treatment states and placebo gaps in control states (discards states with pre-legislation MSPE five times higher than the treated state's)

Unrelated kidney donation rate gaps in treatment states and placebo gaps in control states (discards states with pre-legislation MSPE two times higher than the treated state's)

Figure 4: In-space placebo experiments, living unrelated kidney donation (Panel II)

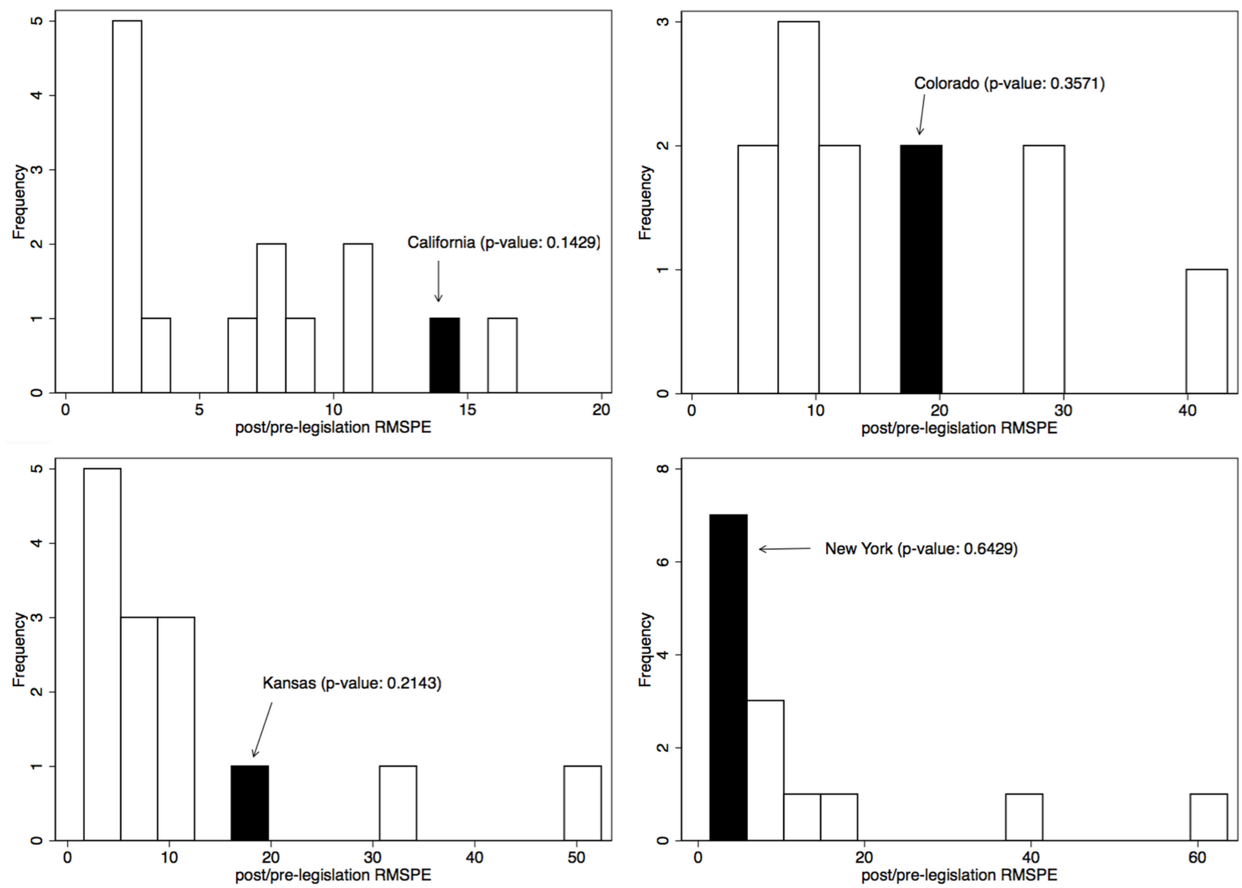


Unrelated kidney donation rate gaps in treatment states and placebo gaps in control states

Unrelated kidney donation rate gaps in treatment states and placebo gaps in control states (discards states with pre-legislation MSPE five times higher than the treated state's)

Unrelated kidney donation rate gaps in treatment states and placebo gaps in control states (discards states with pre-legislation MSPE two times higher than the treated state's)

Figure 5: In-space placebo distributions, living unrelated kidney donation



Notes: The pre-legislation RMSPE for two conrol states in the in-space placebo distributions for Colorado were zero, therefore we were unable to calculate the post/pre-legislation RMSPE for these two control states and excluded them.

Figure 6: Leave-one-out distribution of the synthetic control, living unrelated kidney donation

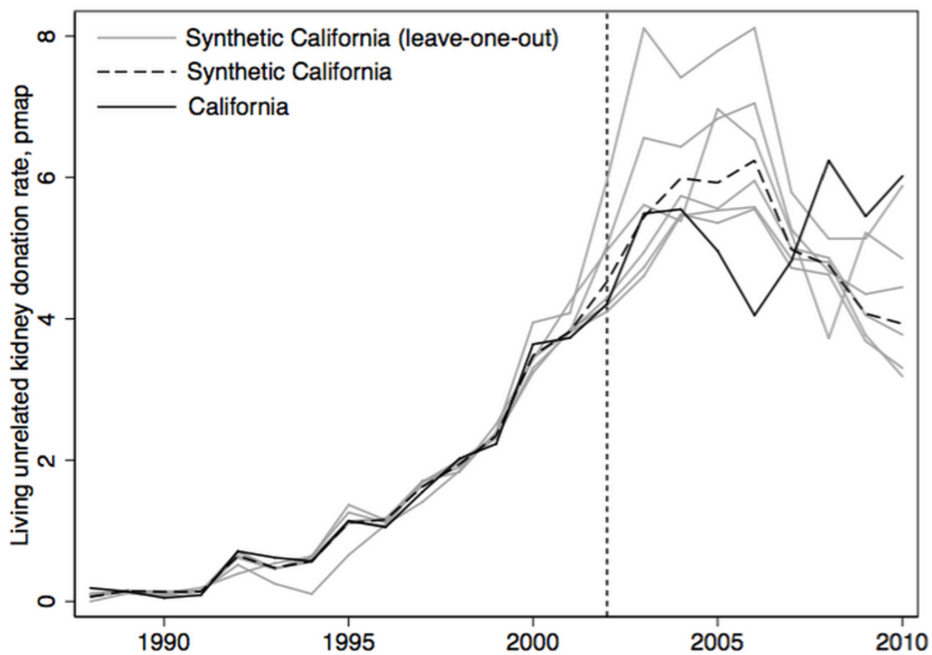
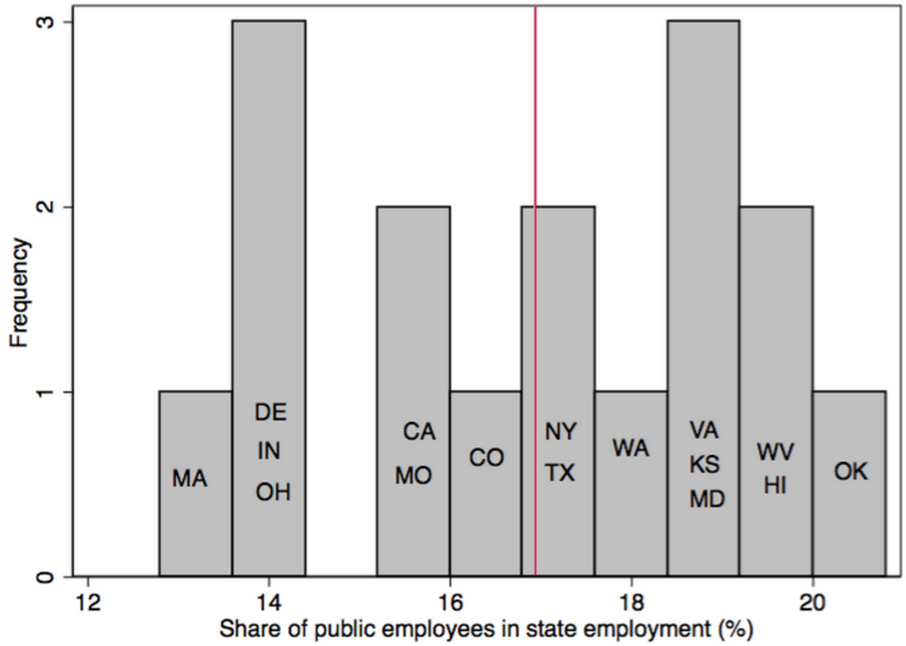


Figure 7: Distribution of the scale of legislation



Source: Bureau of Labor Statistics, Quarterly Census of Employment and Wages (QCEW)

C Technical Details

The synthetic control method is developed by Abadie and Gardeazabal (2003) and expanded by Abadie et al. (2010) and Abadie et al. (2012). In this section, we overview the technical details of the methodology and discuss its advantages over the traditional regression framework.

Suppose there are $P+1$ states in the sample, indexed by $i = 1, 2, \ldots, P+1$ over T periods, $t = 1, 2, \ldots, T$. Only state $i = 1$ enacted the legislation and the remaining P states are the potential control states not enacted the legislation, called the *donor pool*. There are T_0 number of pre-legislation periods and T_1 number of post-legislation periods so that $T_0 + T_1 = T$. The effect of paid leave of absence for unit i at time t is given by $\alpha_{it} = Y_{it}^I - Y_{it}^N$ where Y_{it}^I is the living kidney donation rate of unit i if enacted the legislation in $T_0 + 1$ to T and Y_{it}^N is the living kidney donation rate in the absence of legislation. Since only unit $i = 1$ enacted the legislation, we need to estimate $(\alpha_{1T_0+1}, \ldots, \alpha_{1T})$. We first estimate Y_{it}^N by the following factor model:

$$Y_{it}^N = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \varepsilon_{it} \tag{1}$$

where δ_t is an unknown common factor invariant across units, Z_i is the covariate vector not affected by the legislation, θ_t is a vector of unknown time-specific parameters, λ_t is a vector of unknown common factors, μ_i is the state-specific unobservable and the error term ε_{it} are the zero-mean transitory shocks. The presence of anticipatory effects are irrelevant in our case, implying that all the elements in Z_i that belong to pre-legislation period are unaffected by the law.

Equation (1) allows the effect of unobservable state heterogeneity to vary over time. In the fixed effects (FE) model and the difference-in-differences (DiD) estimator, the effect of unobservable heterogeneity, λ_t, is assumed to be fixed over time. Hence, the synthetic control method provides an improvement over FE and DiD methods and deals better with endogeneity caused by the presence of time-varying unobservable confounders, all of which were presumed to have been accounted in the traditional regression framework.

The method aims to construct the missing counterfactual, Y_{it}^N, from states not enacted the legislation. Let $W = (w_2, \ldots, w_{P+1})^{'}$ be $(P \times 1)$ vector of weights such that $0 \leq w_j \leq 1$ for $j = 2, 3, \ldots, P+1$ and $\sum_{j=2}^{P+1} w_j = 1$. Define the linear combination of pre-legislation values of kidney donation rate as $\bar{Y}_j^k = \sum_{m=1}^{T_0} k_m Y_{jm}$. Abadie et al. (2010) show that if the following conditions hold, then the estimate of the effect of paid leave of absence legislation for the affected/exposed unit, $\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{P+1} w_j^* Y_{jt}$, is an unbiased estimator of α_{1t}:

$$\textstyle\sum_{j=2}^{P+1} w_j^* Z_j = Z_1 \quad \wedge \quad \sum_{j=2}^{P+1} w_j^* \bar{Y}_j^k = \bar{Y}_1^k \tag{2}$$

where w_j^* is the weight assigned to the j^{th} unexposed state.

Equation (2) can hold exactly only if $\left(\bar{Y}_1^k, Z_1\right)$ belongs to the convex hull of $\left[\left(\bar{Y}_2^k, Z_2\right), \ldots, \left(\bar{Y}_{P+1}^k, Z_{P+1}\right)\right]$. This means that the living kidney donation rate of some of the states that passed the legislation may not be synthetized accurately using the pre-legislation characteristics of the states that did not pass any legislation.

The vector W^* is chosen to minimize the distance between the vector of pre-legislation characteristics for the exposed state (X_1) and the weighted matrix that contains the pre-legislation characteristics of unexposed states (X_0): $\| X_1 - X_0 W \| = \sqrt{(X_1 - X_0 W)^{'} V (X_1 - X_0 W)}$ where V is a symmetric and positive semidefinite matrix. This minimization procedure is subject to the constraints that the weight assigned to each unexposed state should lie between zero and one and that the sum of the weights is bounded by one.

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