Effects of Medicare Coverage for the Chronically Ill on Health Insurance, Utilization, and Mortality: Evidence from a Coverage Expansion for End-Stage Renal Disease*

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Abstract

I study the effect of the 1973 expansion of Medicare coverage to individuals with end-stage renal disease (ESRD) on insurance coverage, health care utilization, and mortality. The ESRD expansion had no effect on the probability that an individual with ESRD had any insurance coverage, but there was a large increase in Medicare coverage that complemented existing private insurance coverage. In some specifications, I also find evidence of an increase in physician visits. The expansion also reduced mortality from kidney disease by between 0.5 and 1.0 deaths per 100,000. Lastly, I provide evidence for two mechanisms that affected mortality: an increase in access to and use of treatment, which is plausibly driven by changes in insurance coverage; and an increase in entry of dialysis clinics in areas with a greater burden of kidney disease. Based on changes in the ages at which people died from kidney disease and all other causes, the ESRD program cost between $29000 and $245000 per life year saved.

Keywords: Insurance, Mortality, Kidney disease, Health, Health Insurance
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1 Introduction

Until 2010, and the passage of the Affordable Care Act, the United States has typically expanded public insurance programs by providing coverage to distinct demographic groups. For example, the introduction of Medicare and Medicaid in 1966 provided insurance coverage to people who were 65 and older or had low income. However, several expansions of these programs have defined eligibility based in part on the presence of medical conditions (e.g. long-term disabled, people with end-stage renal disease, pregnant women, and women diagnosed with breast or cervical cancer). By selecting on ill-health, the effects of a disease-specific insurance expansion on insurance coverage, health care utilization, and health outcomes may differ considerably from the effects of more broad-based expansions.

Previous studies of the Medicare and Medicaid programs have demonstrated that Medicare may reduce mortality (Card et al., 2009; Chay et al., 2017), increase health care utilization (Card et al., 2008), and improve financial risk protection (Barcellos and Jacobson, 2015; Engelhardt and Gruber, 2011), while the introduction of state Medicaid programs reduced infant mortality (Goodman-Bacon, Forthcoming). More recent evidence from an expansion of Medicaid for pregnant women demonstrates improvements in infant health outcomes (Currie and Gruber, 1996b) and a related expansion affecting children improved their health and increased health care utilization (Currie and Gruber, 1996a). A recent randomized study of the Oregon Medicaid program (Finkelstein et al., 2012) also demonstrated greater health care utilization and better self-rated physical and mental health among people randomized to receive Medicaid coverage, although there were no statistically significant differences in mortality.

There is, to my knowledge, no empirical evidence of the effects of three other disease-specific insurance expansions that provide insurance coverage for women with breast or cervical cancer, the long-term disabled, and people with end-stage renal disease (ESRD). In this paper, I examine the effect of extending Medicare coverage to people with end-stage renal disease in 1973 on insurance coverage, health care utilization, and mortality. The ESRD program is an attractive area to study for several reasons. First, the introduction of the program was, for the most part, unanticipated so that there is unlikely to be any significant anticipatory effects (Ball, 1973). Second, the population I am studying, people with ESRD, tends to be in extremely poor health, which means that insurance that facilitates access to care is likely to have unusually large effects on health (which also increases my statistical power). Third, because for most people treatment was unaffordable prior to the expansion and insurance typically did not cover treatment (Congressional Research Service, 1971; Rettig, 2011), these results provide some insight into the welfare consequences of moral hazard induced spending.

The ESRD program is also worthy of study simply on the basis of the size of the program. In 2015 the United States spent over $30 billion to treat 500,000 Medicare beneficiaries with ESRD, which represents 1% of all Medicare beneficiaries and 5% of Medicare spending, and ESRD enrollment has been increasing since the inception of the program in 1973. These numbers include people who were eligible for Medicare coverage due solely to having ESRD as well as those who had a long-term disability. In other words, the ESRD program is almost as large as the entire Medicaid program in the state of Texas, which is the third largest Medicaid program (by spending) in the country.

I estimate triple-difference models that compare outcomes for people over 65, who were always eligible for Medicare coverage, versus those under 65, before versus after the expansion took effect, with versus without ESRD. However, due to a contemporaneous expansion of Medicare coverage to the long-term disabled, the triple difference estimate is biased. Hence, I also estimate difference-in-differences models that condition on having ESRD. In these models, there is no bias due to the SSDI expansion since everyone in the ESRD group became eligible for Medicare coverage following the expansion. These two estimators will yield similar results as long as either the treatment effect of Medicare eligibility is small in the non-ESRD group or the share of people eligible for Medicare coverage in that group is small. This research design identifies the effect of Medicare eligibility among people with ESRD, which differs from those treated by the ESRD-specific expansion since the ESRD-specific expansion. The latter group excludes people who have also received long-term disability payments. However, for purposes of this paper, I focus on the population with ESRD, regardless of the source of the expansion.

In this paper I document three main facts about the ESRD program. First, I demonstrate that the ESRD expansion significantly increased insurance coverage among people under 65 years of age with kidney disease. Close to the traditional Medicare eligibility threshold of 65, I find a 19.6 percentage point increase in the probability of any insurance coverage in my triple-difference models and a 26.3 percentage point increase in models that restrict to people with kidney disease. The increase in Medicare coverage was even larger than the increase in insurance coverage, indicating that some people would have had insurance coverage in the absence of the expansion. Notably, the Medicare expansion was not associated with a significant reduction in private insurance coverage, indicating that people were "doubling-up" with both private and Medicare coverage. At the time, this kind of doubling-up represented a significant improvement in coverage for people with kidney disease since Medicare covered dialysis and kidney transplantation, the only treatments available for kidney failure, while many private plans did not (Congressional Research Service, 1971).

Second, I find that the ESRD expansion increased physician visits by 25 to 30 percent for younger, but not older, people with kidney disease below 65 years of age. The increase in physician visits is consistent with my results on health insurance coverage and implies that a ten percent increase in the share of the population with insurance increases physician visits by about five percent. Because of the wording of the survey question that I use to assess physician visits, it is also possible that the increase in physician visits represents an increase in visits to, among other things, dialysis clinics.

Third, I document a significant reduction in mortality due to kidney disease which, by 1978, implies that the program is averting between 170 and and 300 deaths per year for whites between 45 and 64 years of age (my estimation sample) and extrapolated to the entire population under 65 years of age is responsible for preventing between 150 and 540 deaths per year. Assuming that all of the mortality reduction arises from people enrolled in the ESRD program, then these estimates correspond to a reduction of 0.3 to 1.6 percentage points in the probability of dying from kidney disease in the coming year. I document a similar mortality reduction when I compare the United States with other developed countries around the world, suggesting that the mortality reduction was, in fact, causal.

I am also able to provide evidence in support of two mechanisms by which the ESRD expansion affected health. First, the state-specific effect of the ESRD expansion on kidney disease mortality was larger in states that had more treatment facilities per capita in 1971. One interpretation of this result is that the presence of treatment facilities reduced mortality by increasing access to treatment. This interpretation is also consistent with the increase in physician visits. Second, I document an increase in the number of dialysis clinics per capita from 1971 to 1975 in states that had a higher under 65 mortality rate due to kidney disease, which is consistent with a demand side shock encouraging entry of new treatment facilities.

By studying the changes in the mortality rate due to kidney and non-kidney disease by age, I am also able to show, subject to some important assumptions, that the ESRD expansion added an additional 4000 to 25000 life years for people between 45 and 65 years of age. Based on spending on the program in 1978, a lower bound on the implied cost per life year is between $30000 and 150000. These estimates indicate that the ESRD expansion may have been welfare improving based on the increase in the length of time that people survived and are a lower bound on the welfare implications since the increased entry of dialysis clinics and transplant programs were also likely to have been welfare improving.

The remainder of the paper proceeds as follows. Section 2 provides background information on end-stage renal disease, discusses the role that the federal government has played in the treatment of ESRD, and describes the 1973 Medicare expansions that I study. Sections 3 describes the data that I use for my analyses and the empirical approach that I take, while sections 4 present my main results from the Medicare expansion in 1973. Section 5 presents potential mechanisms behind my results. Section 6 discusses the welfare implications of my results. Section 7 concludes.

2 Background

2.1 Kidney Function and End-Stage Renal Disease

End-stage renal disease is the end of a progressive decline in kidney function to the point that one's kidneys are no longer able to function. As a result of the loss of kidney function, there is a buildup of toxins in the blood that, left unchecked, leads to death.

The leading causes of chronic kidney disease include diabetes, hypertension, glomerulonephritis, polycystic kidney disease, kidney stones, urinary tract infections, and various congenital defects (National Kidney Foundation, 2009). Appendix table A lists the ICDA-8 and ICD-9 codes that I use to identify deaths with these underlying cause of death codes.

Treatment for end-stage renal disease emphasizes replacing lost kidney function through "renal replacement therapy." These treatments consist of various forms of hemodialysis, in which blood is passed over a porous membrane so that waste products, excess fluids, and salts cross the membrane leaving relatively clean blood, and kidney transplantation, in which a donor kidney is transplanted into the patient so that the new kidney filters the blood. Hemodialysis for chronic kidney disease is a relatively recent innovation, with the first dialysis clinic opening in 1960. Furthermore, at its inception, dialysis was extremely costly leading to rationing at the first dialysis clinic in the United States (Alexander, 1962). In July of 1972, there were 5786 living dialysis patients in the United States (Rettig, 1976, p. 200) and the Congressional Research Service (1971) estimated that the annual cost of dialysis was $15,000 in 1971 (nominal dollars, $85,000 in 2015 using the CPI-U).

Like dialysis, kidney transplantation was a relatively new development in the early 1960s, with the first successful transplant being performed in 1956. Subsequent development of kidney transplantation was characterized by slow improvement (Congressional Research Service, 1971). By 1967, there were 428 kidney transplants performed ni the United States and that number more than doubled to 1172 transplants by 1971 (Rettig, 1976). Throughout this period, kidney transplantation was a costly procedure with the Congressional Research Service (1971) estimated that kidney transplantation had a nominal one-time cost of $10,000 to $20,000 ($59,000 to $117,000 in 2015) and maintenance costs of $1,000 per year ($5,900 in 2015).

2.2 The Federal Government and Treatment of ESRD

Following the development of an effective kidney dialysis program in the early 1960s, the federal government began to take an active role in the diffusion of treatments for end-stage renal disease. In 1963, the Veteran's Administration announced that it would open 30 dialysis clinics in its hospitals; in 1964 and 1965 the National Institutes of Health began programs studying transplant immunology, which would make kidney transplantation easier; and in 1965 the Public Health Service started the Kidney Disease Control Program, which provided start-up grants to open a dozen dialysis centers (Rettig, 1991). However, these programs were insufficient with costs outstripping initial estimates.

Simultaneously, the Bureau of the Budget convened a committee to advise the federal government on kidney disease and the increasing financial obligations that the government was undertaking. The committee's report recommended the development of a federal program to finance treatment for ESRD and declared that dialysis and kidney transplantation were not experimental but rather established, therapies. Despite the clear recommendations from the committee report, it did not appear to have influenced federal policymaking in this area (for further discussion see Rettig, 1991).

2.3 The 1973 Medicare Expansions

The Medicare expansion authorized in 1972 (Public Law 92-603) that took effect in July of 1973 expanded Medicare eligibility to two groups of people. First, the law provided Medicare coverage to people who had been eligible for Social Security Disability Insurance (SSDI) benefits. Second, the law provided Medicare coverage to people who have received three, or more, months of renal dialysis and extended for up to twelve months after a person received a kidney transplant. Neither expansion is truly universal since in both cases, only individuals who are eligible for insurance under the Social Security program are eligible. Collectively, these two programs increased Medicare enrollment by 1.7 million people, of whom 6,371 were eligible solely due to the ESRD in the first year of the program. By 1978, there were almost 44,000 Medicare beneficiaries with ESRD, of whom almost 35,000 were under 65 years of age, with per capita spending of almost $65,000 (in 2015 dollars).

The ESRD component of the expansion, which was initially expected to enroll 35,000 people and cost $1 billion (nominal) per year, rapidly ballooned in size, covering more than 50,000 people and costing over $1 billion per year in 1979 (table 1). In 2013, the ESRD program covered almost half a million people at a cost of $30 billion, which represents approximately 1% of Medicare enrollees and 5% of Medicare spending.

3 Data and Empirical Framework

3.1 Data

I use data from a variety of sources to measure insurance coverage, health care utilization, and mortality in my main analyses as well as data on potential mechanisms and confounding factors. In this subsection, I describe each of these data sources.

Table 1: Enrollment, Spending, and Utilization in the ESRD Program

	Enrollment		Kidney Deaths		Spending		Utilization	
Year	Total	Under 65	Under 65	65 and Over	Total	Per enrollee	Transplants	Dialysis
1971			5335	7534				
1974	15993		4633	8949	1050.3	65673		
1975	22674	12702[a]	4540	9491	1545.6	68164		
1976	28941	14721[a]	4532	10597	2086.9	72110		
1977	35889	16514[a]	4345	11008	2449.2	68243		
1978	43482	34828	4498	11973	2804.6	64500		
1979	52636	43031	3761	11966	3126.4	59397	4189	45565
1981	61930	47520	3761	13703	3723.7	60127	4898	58924
1986	93197	59570	3914	17851	6786.7	63646	8948	90886
1991	142510	83443	3395	17963	9704.2	56844	10037	144175
1996	255578		3433	20869	14141.8	55333	12219	215557

Source—Greenbook (various years), Annual Statistical Supplement to the Social Security Bulletin (various years), Multiple Cause of Death files, 1971-1996.
[a] Enrollees eligible solely due to ESRD.
Notes—Enrollment based on enrollment in Medicare Part A, expenditures are for Medicare Parts A and B. Spending data have been inflated to 2015 using the CPI for urban workers. Utilization data are the number of transplants and number of enrollees dialyzed, respectively. Kidney deaths are based on chronic coding only, see appendix table A; the coding of kidney deaths changed between 1978 and 1979.

3.1.1 Insurance Coverage and Health Care Utilization

The National Health Interview Survey asked respondents about insurance coverage in even numbered years beginning in 1968, although the specific wording and universe for various questions has changed over time. In 1968 the NHIS inquired about health insurance generically and did not differentiate between public and private coverage and it was not until 1978 that the NHIS inquired about Medicare coverage for people under 65 years of age. In the 1974 and 1976 waves of the survey individuals with only Medicare coverage were instructed to respond that they were uninsured. As a result, I present results using data from 1968, 1970, 1972, 1978, and 1980 for most insurance outcomes (I include data on private insurance coverage in 1974 and 1976). I define an individual as having private insurance coverage based on whether or not an individual reported having private hospital coverage (as in Finkelstein, 2007) and define Medicare coverage in a comparable manner.

The NHIS also included questions on the number of doctor visits in the prior year beginning in 1969, which I use to measure health care utilization. Because the NHIS questions refer to treatment received over the prior year, I omit people 65 years of age from the utilization analysis and all data from July 1973 through June 1974, the twelve months following the implementation of the Medicare expansion.

I also use the condition inventory and the list of conditions that caused the interview to miss days from work or access health care services to construct indicators for the presence of kidney disease. The coding is based on an adaptation of the codes listed in Appendix Table A. In total, out of 371,181 people in the NHIS, I identified 1644 people between 45 and 84 years of age with kidney disease using the broad definition. Despite the small sample size, the ESRD expansion is likely to have led to large changes in insurance coverage, hence I remain sufficiently powered to identify effects of the ESRD expansion on insurance coverage. For the utilization analyses, it is possible that I will be underpowered to detect effects if the increase in physician visits from the expansion is small.

3.1.2 Mortality

I use the Multiple Cause of Death files from the National Center for Health Statistics' (NCHS) for the years from 1968 through 1978 (United States Department of Health and Human Services. National Center for Health Statistics, 2007a,b). These data provide the state and county of residence, race, gender, age, underlying cause of death and all other diagnoses listed on the death certificate for all deaths in the United States, except in 1972, when the NCHS was only able to process half of the submitted death certificates.[1] Preliminary analyses of the distribution of deaths by age indicated significant excess mass at five-year intervals of age for non-white individuals, which was also reported in Honoré and Lleras-Muney (2006), so I omit non-whites from my mortality analyses. I also drop deaths to non-U.S. residents since they were not eligible for the ESRD program.

I code each death as being a kidney disease death, or not, based on either the underlying cause of death, which the World Health Organization defines as "the disease or injury that initiated the train of events leading directly to death, or the circumstances of the accident or violence which produced the fatal injury," or using any of the diagnosis codes listed on the death certificate. For each source of cause of death diagnosis codes, I defined a death as due to kidney disease using three sets of diagnosis codes. First, I defined a "narrow" definition of kidney disease, which did not restrict to only chronic disease, but is generally based on the "renal failure" codes in the ICDA-8. Second, I created a "chronic" definition by restricting the narrow definition to deaths due to chronic causes. Lastly, I created a "broad" definition, which was based on the codes used by the Kidney Disease Program in tracking kidney disease mortality (Kidney Disease Program, 1971). Appendix Table A lists the ICDA-8 diagnosis codes for the three cause of death groupings that I use.

I combine the mortality data with population data from the SEER program and the U.S. Census Bureau in order to adjust for changes in the size of the population over time, which also affects the expected number of deaths due to kidney disease. Because these data do not break out population figures for individuals 85 and over, I restrict my analysis to deaths to individuals who are 84 or younger.

[1]Since I use functions of the count of deaths in a given demographic-time cell as my dependent variable, I multiply the count of deaths in 1972 by 2.

3.1.3 Mechanisms and Confounders

In my discussion of mechanisms and potential confounders, below, I rely on data from three other datasets. I collected data on the geographic distribution of treatment facilities in 1971 from the publication "Kidney disease services, facilities, and programs in the United States" (Kidney Disease Program, 1971), which lists treatment facilities by state. Based on the name of the facility, I also classified these facilities into Veteran's Administration/Military vs. civilian categories since access to the former may be restricted. Data on treatment facilities in 1975 came from the 1977 Annual Statistical Supplement to the Social Security Bulletin, which lists the number of hospital transplant programs, hospital-based dialysis clinics, and freestanding dialysis clinics by state.

I collected data on the share of people in an age-gender-state cell who receive income from either Social Security or the Supplemental Security Income program from the March CPS supplements for 1977 to 1979 (spanning 1976 to 1978).

3.2 Empirical Approach

3.2.1 Identification

My data includes three sources of variation that I could use to identify the effect of the Medicare ESRD program on insurance coverage, health care utilization, and kidney disease mortality. First, there are differences over time in Medicare eligibility for individuals of the same age and disease status. Second, there are differences by age in eligibility for Medicare for individuals in the same year and disease status. Third, there are differences by disease status in eligibility for Medicare coverage for individuals in the same year and of the same age. In principle, these three sources of variation would justify a triple difference estimator assuming that potential outcomes between these groups satisfy a "parallel trends" assumption (Lee and Kang, 2006). However, in my setting the parallel trends assumption is unlikely to hold because the SSDI expansion means that there is partial takeup of Medicare coverage in one of the comparison groups. The structure of the problem, allows me to identify the source of any bias from these comparisons and identify a solution that leads to unbiased estimates of the intent-to-treat effect of the Medicare expansion on people with kidney disease.

To demonstrate the bias and identify situations in which it does not affect my results, let Y^0_{akt} denote the potential outcome for someone in age group a ($a = 1$ for people under 65) who has kidney disease if $k = 1$ when she is not eligible for Medicare, in time period t, while Y^1_{akt} denotes the corresponding potential outcome when she is eligible for Medicare coverage. Assume that there is a share α_{akt} who are eligible for Medicare in each akt cell and define $Y_{akt} = \alpha_{akt}Y^1_{akt} + (1 - \alpha_{akt})Y^0_{akt}$. Ignoring the fact that some people 65 and older are not eligible for Medicare, Medicare program rules imply that $\alpha_{0kt} = 1$ for all $k, t \in \{0, 1\}$ and $\alpha_{1k0} = 0$ for $k \in \{0, 1\}$. Finally, because (almost) everyone with kidney disease is automatically eligible for

Medicare coverage, but only some people without kidney disease are eligible for Medicare coverage, we also have $\alpha_{111} > \alpha_{101}$.

Then the triple-difference estimator can be written as:

$$\begin{aligned} DDD = &\alpha_{111}\left(Y^1_{111} - Y^0_{111}\right) - \alpha_{101}\left(Y^1_{101} - Y^0_{101}\right) \\ &+ \left(Y^0_{111} - Y^0_{110}\right) - \left(Y^1_{011} - Y^1_{010}\right) \\ &- \left(Y^0_{101} - Y^0_{100}\right) + \left(Y^1_{001} - Y^1_{000}\right) \end{aligned}$$

Even assuming that the "parallel trends" held with respect to potential outcomes so that the second and third lines vanish, DDD would still be biased by partial takeup of treatment ($\alpha_{111} < 1$) and the fact that some people without kidney disease are also treated ($\alpha_{101} > 0$). However, the bias can be signed if one assumes that the sign of the treatment effect is the same regardless of kidney disease status, in which case the triple-difference estimate will be biased towards zero unless the treatment effect of eligibility for people without kidney disease is significantly greater than the treatment for people with kidney disease.

In the difference-in-difference estimate that restricts to people with kidney disease, there is no bias from the fact that people without kidney disease are partially treated. One can write this estimator as:

$$DD_k = \alpha_{111}\left(Y^1_{111} - Y^0_{111}\right) + \left(Y^0_{111} - Y^0_{110}\right) - \left(Y^1_{011} - Y^1_{010}\right)$$

Assuming that the parallel trends assumption holds, then DD_k provides a scaled estimate of the causal effect of Medicare eligibility for people with kidney disease. In the DD_k estimator, the parallel trends assumption implies that in the absence of the ESRD expansion, trends in mortality would have progressed along similar paths following the expansion for people over and under 65 years of age. While there are reasons to doubt this assumption, due to the fact that renal replacement therapy was generally more suited to younger, rather than older, people, it is unclear why there would be a sudden change at age 65, as would be required to bias my estimates.

3.2.2 Event Study and Difference-in-Difference Models

I first consider analyses that use age and time variation separately. To do so, I estimate event-study models of the form:

$$\begin{aligned} y_{iatgd} = &\beta_1^{65} Kidney_d + \beta_2^{65} Post_t + \beta_3^{65} K_d \times Post_t \\ &+ \sum_{a' \neq 65} \left(\beta_1^{a'} Kidney_d + \beta_2^{a'} Post_t + \beta_3^{a'} K_d \times Post_t\right) \mathbf{1}_{[a=a']} \\ &+ X_{ig}\Gamma_1 + \tau_t + \alpha_a + \varepsilon \end{aligned} \tag{1a}$$

and

$$
\begin{aligned}
y_{iatgd} &= \beta_1^{1971} Kidney_d + \beta_2^{1971} Under65_a + \beta_3^{1971} K_d \times Under65_a \\
&+ \sum_{t' \neq 1971} \left(\beta_1^{t'} Kidney_d + \beta_2^{t'} Under65_a + \beta_3^{t'} K_d \times Under65_a \right) \mathbf{1}_{[t=t']} \\
&+ X_{ig}\Gamma_1 + \tau_t + \alpha_a + \varepsilon
\end{aligned} \tag{1b}
$$

Where y_{iatgd} is the outcome—type of insurance coverage, amount of health care utilization, or deaths per 100,000 people—for person i (I only have person-level data on insurance coverage), who belongs to age group a in time period t, where time is measured in half-year increments, gender g, and cause of death d, $Kidney_d$ is a dummy for deaths due to kidney disease, $Post_t$ is a dummy for the ESRD period, which takes the value of 1 for time periods after July 1, 1973, $Under65_a$ is an indicator that a is less than 65, X_{ig} is a vector of controls including fixed effects for each demographic group, τ_t and σ_a are year and age fixed effects, respectively. The coefficients β_i^a and β_i^t are the age or period-specific coefficients on kidney disease, the post period (or being under 65), and their interaction.

I then summarize the results of these event studies using a triple-difference estimator, which is subject to bias from the SSDI program, and a difference-in-differences estimator that is unbiased, but may also be less precise. The triple-difference model can be written as:

$$
\begin{aligned}
y_{iatgd} &= \beta_1 Kidney_d + \beta_2 Post_t + \beta_3 Under65_a + \beta_4 Kidney_d \times Post_t \\
&+ \beta_5 Kidney_d \times Under65_a + \beta_6 Post_t \times Under65_a \\
&+ \beta_7 Kidney_d \times Post_t \times Under65_a + X_{ig}\Gamma_1 + \tau_t + \alpha_a + \varepsilon
\end{aligned} \tag{2}
$$

And the corresponding difference-in-differences estimator is:

$$
y_{iatgd} = \alpha_1 Post_t + \alpha_2 Under65_a + \alpha_3 Post_t \times Under65_a + X_{ig}\Gamma_1 + \tau_t + \alpha_a + \varepsilon \tag{3}
$$

The previous discussion of identification in this setting implies that $|\alpha_3| \geq |\beta_7|$, assuming that treatment effects in the ESRD expansion are comparable in size, or larger, than treatment effects of the SSDI expansion. Standard errors for all models are clustered on age and time, unless otherwise specified, using clus_nway.ado (Cameron et al., 2011; Kleinbaum et al., 2013).

4 Effect of the Medicare Expansion

4.1 Health Insurance

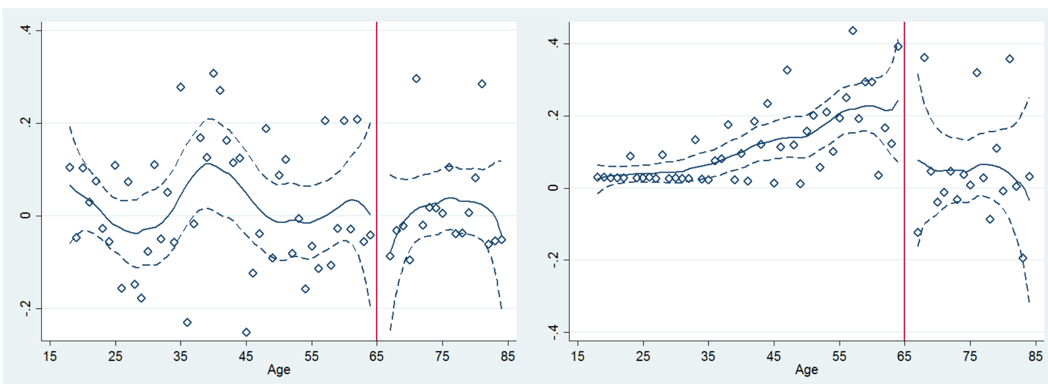
I first consider the effect of the ESRD expansion on health insurance coverage. Figure 1 presents an event study of the change in any insurance coverage (panel A) and Medicare coverage (panel B) using the triple-difference version of equation (1b).[2]

[2]The figure using the difference-in-difference is similar, but less precisely estimated.

Figure 1: Changes in Insurance Coverage from the Medicare Expansion

A: Any Insurance B: Medicare

C: Any Insurance D: Medicare



Source—National Health Interview Survey, even number years from 1970-1980
Notes—Sample restricted to people between 45 and 84 years of age. Points are year-by-under 65 years of age coefficients from a linear regression of an indicator for either any insurance or Medicare on year fixed effects (omitted 1970), an under 65 indicator, kidney disease status (using the "broad" definition), and their interaction. Confidence intervals are clustered on age.

Prior to the Medicare expansion, the probability that an individual with kidney disease had any form of insurance coverage was increasing. However following the expansion there was no appreciable increase in insurance coverage, on average, for people with kidney disease. Medicare coverage, by contrast, increased significantly by 1978 with the bulk of the increase in Medicare coverage happening at older ages (panel D). Conversely, the ESRD expansion appears to have increased coverage somewhat for people close to the age 65 cutoff, but there was also a noticeable increase in insurance coverage for people around 40 years of age (panel C).

The graphical results in figure 1 provide support for the "parallel trends" assumption, which means that trends among people over 65 are similar to trends among the younger population.

Table 2: Effect of the ESRD Program on Health Insurance and Health Care Utilization

	Any Insurance		Medicare		Any Private		Only Private		Doctor Visits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Difference-in-Difference-in-Difference										
DDD	-0.009	0.061	0.148**	0.220+	-0.073	-0.028	-0.219*	-0.206	0.216**	0.090
	(0.052)	(0.096)	(0.042)	(0.115)	(0.110)	(0.161)	(0.085)	(0.131)	(0.067)	(0.126)
Age Trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	147669	147669	118375	118375	188071	188071	118375	118375	371181	371181
B: Difference-in-Difference										
DD	0.037	0.125	0.191**	0.313*	-0.139	-0.049	-0.214*	-0.246	0.281**	0.169
	(0.056)	(0.105)	(0.052)	(0.136)	(0.099)	(0.161)	(0.083)	(0.142)	(0.085)	(0.134)
Age Trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	1354	1354	800	800	1359	1359	800	800	2456	2456
Means										
65-84, Without Kidney Disease										
Pre	0.96		0.93		0.55		0.04		5.05	
Post	0.98		0.93		0.66		0.04		4.88	
65-84, With Kidney Disease										
Pre	0.94		0.93		0.50		0.04		10.41	
Post	0.98		0.94		0.55		0.02		9.34	
45-64, Without Kidney Disease										
Pre	0.82		0.00		0.82		0.82		3.87	
Post	0.89		0.04		0.84		0.81		3.88	
45-64, With Kidney Disease										
Pre	0.79		0.00		0.79		0.79		9.38	
Post	0.86		0.21		0.68		0.55		10.77	

Source—Author's analysis of the National Health Interview Survey from 1968-1980.
Notes—Dependent variable is indicated by the column group title. Kidney disease is defined using the definition of kidney disease (see Appendix Table A). DDD is the triple-difference coefficient from the interaction of a dummy for being under 65 years of age, a dummy for the second half of 1973 or later, and a dummy for having kidney disease; DD is the difference-in-differnece coefficient from a sample with kidney disease. Models include year, age, gender, and race fixed effects along with all one-, two-, and, if appropriate, three-way interactions of under 65, post, and kidney disease; models with age trends also include additional interactions with age-65. Sample restricted to individuals between 45 and 84 years of age; columns (9) and (10) exclude data on people 65 years of age and from July 1 1973 to June 30 1974. Estimates are from OLS regressions in columns (1)-(8) and Poisson in columns (9) and (10). Standard errors clustered on age in round brackets.
+ p<0.1, * p<0.05, ** p<0.01

Consistent with the event study in figure 1, I find no evidence that the ESRD expansion increased insurance coverage among people with kidney disease (table 2, column 1). However, there was a 20 to 26 percentage point increase in coverage for people close to the age 65 cutoff (column 2). The increase in insurance coverage in models with age trends is slightly smaller than the increase in Medicare coverage (columns 3 and 4), which is consistent with either a degree of crowd-out or "doubling-up" of private and public insurance coverage. I find only modest evidence of a decrease in private insurance coverage associated with the ESRD expansion, but a large decrease in the share of people who reported only private insurance coverage. This final change–the reduction in reports of only private insurance–is indicative of people using both Medicare and private insurance coverage simultaneously. This kind of doubling up of insurance coverage provided additional benefits to people with ESRD since private insurance plans at the time typically did not cover dialysis or renal transplantation, hence adding Medicare coverage represented a significant improvement in insurance coverage for people with kidney disease.

As a specification check, online appendix table B1 presents results from "donut" regressions that exclude people within five years of turning 65. These donut estimates are, in general, consistent with my main specifications, particularly for models without age trends.

4.2 Health Care Utilization

Figure 2: Event Study Estimates of Changes in Health Care Utilization

A: # Doctor Visits—Year×Under 65×Kidney B: # Doctor Visits—Age×Post×Kidney

Source—National Health Interview Survey, 1970-1977.
Notes—Sample restricted to people between 45 and 84 years of age. Points in panel A and C are year-by-under 65 years of age-by-kidney disease coefficients from a regression of the number of doctor visits (panel A) or an indicator for any hospital stay (panel C) on year fixed effects (omitted 1972), an under 65 indicator, an indicator for kidney disease, and all two- and three-way interactions. Panels B and D present point estimates for pairs of years of age interacted with a post dummy (after 1973) and kidney disease from a regressions of the number of doctor visits (panel B) or an indicator for any hospital stay (panel D) on age fixed effects, post, kidney disease, and all two- and three-way interactions; smoothed line is local polynomial estimate where estimates are weighted by the inverse of their standard errors. Estimates in panels A and B from Poisson regressions while panels C and D use OLS. Confidence intervals in panels A and C based on covariance matrix that is clustered on age, while panels B and D use heteroskedasticity robust standard errors. Panels A and C report results using both the "narrow" and "broad" definitions, while panels B and D use the "broad" definition only (see appendix table A).

Figure 2 presents triple difference estimates for the number of physician visits.

There is some evidence in panel A of an increase in physician visits after the expansion, relative to before the expansion. Panel B demonstrates that any increase in physician visits affected virtually all ages below 65 years of age and the increase in physician visits was smaller for individuals close to the age 65 eligibility threshold for Medicare coverage.

Consistent with the event studies, column 9 of table 2 demonstrates that the ESRD expansion was associated with a 25 to 30 percent increase in physician visits in models that do not control for age trends. Models that do control for age trends (column 10) yield substantially smaller and statistically insignificant estimates, which is consistent with the age profile of the change in physician visits from panel B of figure 1. These results are essentially unchanged in donut regressions (online appendix table B1, columns 9 and 10).

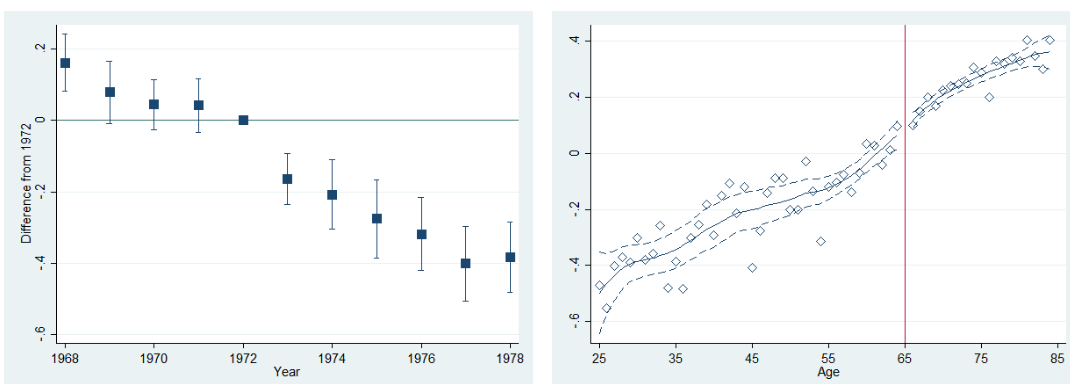
4.3 Mortality Effects

4.3.1 Comparisons within the United States

Figure 3: Event Study Estimates of the ESRD Program and Mortality

A: Underlying Cause of Death by Year (1972 reference)

B: Underlying Cause of Death by Age (65 reference)



Source—Author's analysis of Multiple Cause of Death Files, 1968-1998.
Notes—Points are coefficients on time periods from a Poisson regressions of the number of deaths per 100,000 in an age-gender-time period cell on time fixed effects (panels A and B) or age fixed effects (panels C and D) interacted with a dummy for kidney disease and either a dummy for being under 65 or after the second half of 1973. Kidney disease deaths classified using the underlying cause of death only in panels A and C and, in all panels, the "narrow" definition of kidney disease (see appendix table A). 95% confidence intervals are clustered on age and time.

Figure 3 plots event studies for kidney disease mortality using the underlying cause of death, where the event studies are based on triple-difference estimates. Panel A indicates that there was a reduction in mortality due to kidney disease in 1973 and visually, this reduction was larger than the potential downward trend in kidney disease mortality prior to 1973. Panel B demonstrates that there was a relative increase in kidney disease mortality among people 65 years of age and older following the ESRD expansion and indicates that there was a strong age trend in the mortality change following the ESRD expansion, which justifies focusing on specifications that include age trends.

Table 3: Poisson Estimates of the Effect of the ESRD Program on Mortality

	Narrow Definition				Broad Definition		Chronic Only	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Ages 45 to 84								
DDD	-0.373**	-0.073*	-0.059**	-0.021*	-0.067**	-0.010	-0.057*	-0.020+
	(0.049)	(0.029)	(0.007)	(0.010)	(0.011)	(0.009)	(0.027)	(0.012)
DD	-0.363**	-0.079*	-0.048**	-0.025+	-0.073**	-0.015	-0.064+	-0.024+
	(0.053)	(0.035)	(0.014)	(0.014)	(0.022)	(0.011)	(0.034)	(0.014)
B: Ages 45 to 60 and 70 to 84								
DDD	-0.446**	-0.148**	-0.080**	-0.066**	-0.144**	-0.040**	-0.134*	-0.062**
	(0.052)	(0.049)	(0.006)	(0.017)	(0.019)	(0.011)	(0.053)	(0.021)
DD	-0.437**	-0.150*	-0.068**	-0.065*	-0.145**	-0.040+	-0.136*	-0.062*
	(0.056)	(0.066)	(0.015)	(0.026)	(0.043)	(0.022)	(0.068)	(0.026)
Age trends	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Underlying only?	Yes	Yes	No	No	Yes	No	Yes	No
Mean Annual Kidney Disease Mortality Rates (per 100,000)								
Ages 45-64								
1968-1973H1	6.3	6.3	38.2	38.2	13.1	49.6	5.3	34.8
1973H2-1978	5.1	5.1	35.6	35.6	8.6	43.4	4.4	31.2
Ages 65-84								
1968-1973H1	26.9	26.9	240.1	240.1	95.6	351.6	23.4	227.7
1973H2-1978	30.7	30.7	230.4	230.4	73.8	310.1	25.9	209.2

Source—Author's analysis of Multiple Cause Mortality Files for 1968-1978.
Notes—Dependent variable is the mortality rate in the age-time-gender-cause of death cells, where time is measured in half-year increments. Definitions of kidney disease based on codes in table A. DDD is the triple difference coefficient for being under 65, in the post expansion period, with kidney disease; DD is the difference-in-differences coefficient for being under 65 and in the post expansion period using a sample that is restricted to deaths due to kidney disease. Models that do not restrict to underlying causes of deaths also define a death as due to kidney disease if kidney disease is either an underlying or a contributing cause of death. All models include time fixed effects (measured in six month increments), age fixed effects, an indicator for female, and all possible interactions of an indicator for being under 65, a post period dummy, and, where appropriate, an indicator for deaths due to kidney disease. Models with age trends also include interactions with age minus 65 in addition to the under 65, post, and kidney disease interactions. Sample is restricted to deaths to whites between 45 and 84 years of age in panels A; panel B excludes deaths to people between 61 and 69 years of age. Estimates are from Poisson regressions, standard errors two-way clustered on age and time in round brackets.
\+ p<0.1, * p<0.05, ** p<0.01

In triple difference models based on equation (2) and difference-in-differences estimates based on equation (3) I find that the ESRD program reduced mortality from kidney disease by 36.3 to 37.3 log points in a model that does not include age trends. This estimate is, at first blush, implausibly large and reflects the age trends seen in panel B of figure 3. Including age trends (column 2) yields smaller estimates of a 7.3 to 7.9 log point reduction in mortality. In models with age trends, I also find that the DD estimate is larger in magnitude (although not significantly so) than the DDD estimate, which is what one would expect from the lower level of Medicare eligibility among people without kidney disease. Columns (3) and (4) repeat the preceding analysis, but define deaths based on both the underlying and contributing cause of death codes. Using the broader definition of mortality yields smaller mortality reductions in log points, but the aggregate reduction in deaths is larger using both underlying and contributing causes of death.[3]

Because there are many potential diagnoses that may indicate a death due to kidney disease, in columns (5) and (6) I present results using the "broad" definition of kidney disease. The broad results are qualitatively similar and also indicate a reduction in mortality based on kidney disease as the underlying cause of death, but not when kidney disease is merely a contributing cause of death.

Lastly, the ESRD program, in particular, was targeted at people with chronic kidney disease, so in columns (7) and (8) I restrict my definition of kidney disease to people who died of chronic kidney disease, based on the codings in appendix table A. The chronic estimates indicate that the Medicare expansion was associated with a reduction in deaths due to chronic kidney disease, although three of the estimates are only significant at the ten percent level.

Collectively, the results in panel A of table 3 demonstrate that expanding Medicare coverage to people with kidney disease (through either the ESRD or SSDI expansions) reduced mortality due to kidney disease. The reductions are, except in models that do not include age trends and use only the underlying cause of death, plausible in magnitude. The implied effect size, relative to the number of people enrolled in the Medicare program with ESRD, is on the order of a one to three percentage point reduction in mortality. Such a reduction in mortality is more than an order of magnitude larger than the (non-significant) mortality reduction from the Oregon Health Insurance Study (Finkelstein et al., 2012), but is plausible because of the high baseline mortality rate of kidney disease, so that the relative reduction is small.

There are two main threats to the validity of my results that are unique to mortality data. First, there is a reverse "harvesting" effect, in which people who would have died of kidney disease in the absence of the program are able to survive until they turn 65 after the program. The implication of this kind of harvesting is that the mortality rate among people 65 and older will be overstated. I am able to test for this possibility by re-running my underlying models while excluding people between 60 and 70 years of age (panel B). In these donut regressions, my results are essentially unchanged and, in fact, my estimated mortality reductions become

[3]The DDD results in columns (1) and (3) imply a reduction of 1.96 ($= (\exp(-0.373) - 1) \times 6.3$) or 2.19 ($= (\exp(-0.059) - 1) \times 38.2$) deaths per 100,000, respectively. The corresponding estimates in columns (2) and (4) are 0.44 and 0.79 fewer deaths per 100,000.

larger. This is inconsistent with reverse harvesting, which would predict that the mortality reductions would be smaller in magnitude when I exclude people between 60 adn 70 years of age.

The second threat is that people who do not die of kidney disease will die of something else. This "competing risk" effect is well known in economics and epidemiology and cannot be resolved without imposing assumptions on the processes that determine mortality (Honoré and Lleras-Muney, 2006). The bias due to competing risks is similar to the bias from harvesting, but now it is the mortality rate due to non-kidney causes that is inflated. Notably, competing risks can only bias my estimates if there is, in fact, an effect of the Medicare expansion on kidney disease mortality. In the absence of such a reduction, there is no reason to expect to find a competing risk bias. I can address the bias from competing risks by restricting my data to deaths due to kidney disease, in other words the DD results are not subject to competing risks. My DD results demonstrate that any bias from competing risks is small since my DD estimates are, in general, larger in magnitude than the DDD estimates (which is also the relationship one would expect to hold if the treatment effect of Medicare eligibility was of the same sign for people with and without kidney disease).

Table 4: Impact of the ESRD Program on Kidney Disease Mortality

	Narrow Definition		Broad Definition		Chronic Only	
	(2)	(4)	(5)	(6)	(7)	(8)
A: White, 45-64 years old						
# Deaths in 1978	2132	14453	3276	17201	1817	12557
# Deaths Averted	167.7	311.2	236.4	172.9	108.9	252.2
95% CI	(27 - 309)	(5 - 617)	(155 - 318)	(-128 - 474)	(1 - 216)	(-43 - 548)
B: White, 0-64 years old						
# Deaths in 1978	2890	18529	4235	21808	2448	16037
# Deaths Averted	227.4	399.0	305.6	219.2	146.8	322.0
95% CI	(36 - 418)	(7 - 791)	(200 - 411)	(-162 - 601)	(2 - 292)	(-55 - 699)
C: All, 45-64 years old						
# Deaths in 1978	3283	19349	4959	22902	2862	16924
# Deaths Averted	258.3	416.7	357.8	230.2	171.6	339.9
95% CI	(41 - 475)	(7 - 826)	(234 - 482)	(-170 - 631)	(2 - 341)	(-58 - 738)
D: All, 0-64 years old						
# Deaths in 1978	4498	25149	6474	29444	3890	21887
# Deaths Averted	353.9	541.6	467.1	295.9	233.2	439.5
95% CI	(57 - 651)	(9 - 1074)	(306 - 629)	(-219 - 811)	(3 - 463)	(-75 - 954)
Underlying only?	Yes	No	Yes	No	Yes	No

Source—Author's analysis of Multiple Cause Mortality Files for 1978.
Notes—Deaths averted based on #Deaths Averted = $-\frac{\beta}{1+\beta} \times$ # Deaths in 1978, where β is the triple-difference coefficient from the corresponding column of table 3

Table 4 presents the implied effect of the Medicare expansions on kidney disease mortality in 1978. Each column uses the DDD coefficient in panel A from the identically numbered column in table 3. The first row in each panel is the number of deaths in the relevant demographic group and the second row in each panel is the implied change in deaths due to the program, which I define as the number of deaths in 1978 multiplied by $\frac{-\beta}{1+\beta}$, where β is the triple difference coefficient from table 3. The confidence interval was derived using the delta method. In panel A, which uses the estimation sample (whites between 45 and 84 years of age), the results indicate that the program averted between 100 and 300 deaths, depending upon the definition of kidney disease being used. For all whites under 65 years of age (panel B), the number of deaths averted was larger—ranging from 150 to 400. Including nonwhites in the sample, who were excluded because of concerns about age-heaping, yields a mortality reduction of anywhere from 175 deaths (45-64 year olds, underlying chronic kidney disease) to almost 600 averted deaths (0-64 year olds, underlying broader definition of kidney disease).

Assuming that the mortality reduction came solely from a change in mortality due to the ESRD program, so that the correct denominator is under 65 Medicare enrollment among people with kidney disease in 1978, my results imply that there was a 0.3 to 1.6 percentage point reduction in mortality due to kidney disease. While this estimate may seem large, it is not implausibly so for a disease that is amenable to treatment and has a high mortality rate in the absence of treatment. Currently, individuals with ESRD who decline dialysis treatment are expected to die within a few days or weeks (National Kidney Foundation, 2008).

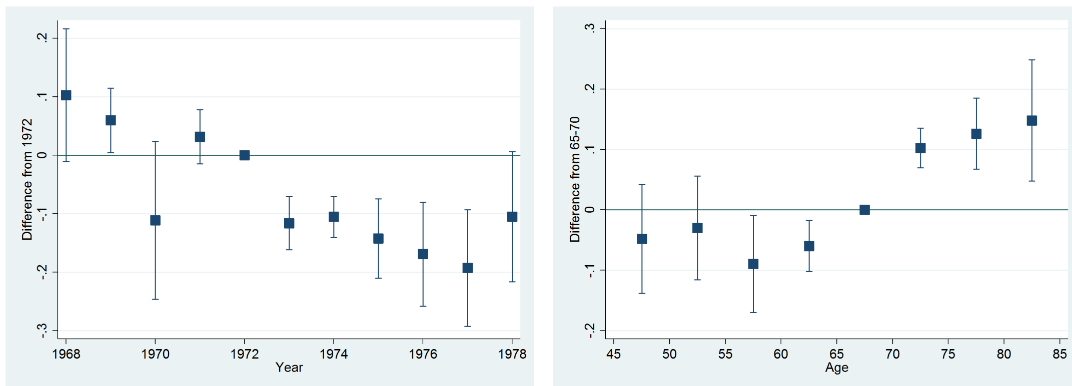
The online appendix presents results from a log-linear OLS specification, which are qualitatively similar (table B2). The online appendix also provides robustness tests of the triple and double-difference results by varying the range of ages included (online appendix figure A1) and varying the age and time controls that are included (table B3).

In the online appendix (table B4), I also consider the potential confounding effect of the introduction of the Supplemental Security Income (SSI) program in 1974. The SSI program provides cash transfers to low-income people who are aged, blind, or disabled and, in most states, also provides access to Medicaid coverage. To test if the SSI program is confounding my estimate of the effect of the SSDI program, I interacted the triple difference coefficients with (demeaned) shares of people in an age-gender-state cell who reported either Social Security or SSI income in the March CPS from 1977 to 1979 (covering years 1976 to 1978). In a separate specification, I interacted the triple difference coefficients with indicators for two factors that states can use to discourage enrollment in Medicaid–using more stringent eligibility criteria and requiring a separate Medicaid application. I find no evidence that any of these interactions are statistically significant, indicating that the SSI program is not driving the differential mortality reduction for kidney disease, relative to other cuases of death.

4.3.2 Comparisons with Other OECD Countries

Figure 4: Cross-Country Event Study Estimates of the ESRD Program and Mortality

A: Mortality Differences by Year B: Mortality Differences by 5-Year Age Group



Source—Author's analysis of the World Health Organization Mortality Database for 1968 through 1978.
Notes—Points in panel A are quadruple-difference estimates by year comparing mortality from kidney disease to all other deaths in the United States versus all other OECD member states that joined prior to 1969, relative to 1971 for the indicated age groups. Panel B reports quadruple-difference estimates by age group comparing before, versus after, the Medicare expansion, with versus without kidney disease, in the United States versus all other OECD member states. Standard errors are clustered on country.

Figure 4 plots event-study estimates of the change in kidney disease mortality in the United States, relative to other OECD countries by either year (panel A) or age (panel B). Over time, there is a pronounced reduction in kidney disease mortality for people under 65 in the United States in 1973 that was not observed in other countries. However, there is also some evidence of a trend in kidney disease mortality in the United States towards fewer people under 65 dying from kidney disease, although with one exception, all of the confidence intervals before 1972 include 0. Despite the possible violation of the parallel trends assumption, there is still evidence of a substantial reduction in kidney disease mortality beginning in 1973. Results by age (panel B) are also suggestive of a reduction in kidney disease mortality, although there appears to be a reduction in mortality among 65-70 year olds, relative to people 70 and older, in the data as well.

Going from the event-study estimates in figure 4 to triple and quadruple difference results, I find that the ESRD program was associated with a four to eight log point reduction in mortality from kidney disease, depending upon the specification and sample (table 5). This reduction in mortality is robust to including country fixed effects, interacting country fixed effects with either kidney disease or an indicator for 1974 or later (the post dummy takes the value 0.5 in 1973), and including year-by-kidney disease indicators, which accounts for innovations in the treatment of kidney disease. These results are also similar in magnitude to my results using

Table 5: Cross-Country Estimates of the Effect of the ESRD Program on Kidney Mortality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DDDD	-0.064**	-0.077**	-0.064**	-0.076**	-0.057**	-0.064**	-0.058**	-0.065**
	(0.023)	(0.024)	(0.024)	(0.024)	(0.020)	(0.023)	(0.020)	(0.023)
DDD	-0.048*	-0.060**	-0.045*	-0.048*	-0.039*	-0.040*		
	(0.023)	(0.022)	(0.020)	(0.021)	(0.017)	(0.018)		
Only Members Before 1969	No	Yes	No	Yes	No	Yes	No	Yes
Country FE			X	X	X	X	X	X
Country Interactions					X	X	X	X
Year-by-Kidney							X	X

Source—Authors' analysis of the World Health Organization Mortality Database for 1968 through 1978, covering the United States and OECD Member States at any point in time.

Notes—Coefficients are point estimates from Poisson regressions using the mortality rate per 100,000 in each country-year-gender-age group-cause of death cell as the dependent variable. DDDD is the coefficent on the four-way interaction of a dummy for the United States, an indicator for the post period, a dummy for deaths due to kidney disease, and a dummy for being 45-64 years of age; DDD is the corresponding coefficient in models that restrict to deaths due to nephritis. All models include year, age, and gender fixed effects and trends in age-65, where age in each cell was recentered by 2.5 years. County Interactions are two-way interactions of country fixed effects with dummies for kidney disease and post. Sample restricted to individuals between the ages of 45 and 84 and years in which the country used the ICD-8 coding regime. Estimates are from Poisson regressions, cells weighted by population, standard errors clustered on country in parentheses.

+ $p<0.1$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$

the narrow definition of kidney disease and underlying cause of death codes in the US mortality data, which is the most comparable specification.

5 Mechanisms

The ESRD expansion may have affected health through two classes of mechanisms. First, by lowering the cost of accessing treatment, health insurance may have increased demand for renal replacement services (dialysis and kidney transplantation), which would have been otherwise unaffordable. This mechanism implies that there may be an "access motive" to purchase health insurance in the sense of Nyman (2003, 1999a,b) and, in essence, reflects the fact that the ESRD program provided a large in-kind transfer from healthy people to those with ESRD.

The second class of mechanisms relate to changes in the supply of renal replacement services. The ESRD program did not merely shift the demand curve outward, but it also guaranteed payment for treatment services, which reduced the risk of investing in renal replacement services. In much the same way that the original introduction of Medicare stimulated entry by hospitals and increased technology adoption (Finkelstein, 2007), the ESRD expansion may have increased adoption and entry of renal replacement services across the country.

5.1 Access to Care

Table 6: In-state Treatment Capacity and Mortality Reduction

	Narrow Definition			Chronic Only		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Base Model						
DDD	-0.077**	-0.076*	-0.078*	-0.060*	-0.058+	-0.061+
	(0.028)	(0.031)	(0.033)	(0.028)	(0.031)	(0.032)
×Log Dialysis Clinics						
Per Capita in 1971	-0.065**		-0.093**	-0.062*		-0.086*
	(0.023)		(0.033)	(0.025)		(0.037)
×Log Transplant Programs						
Per Capita in 1971		-0.008	0.061		-0.009	0.057
		(0.060)	(0.063)		(0.058)	(0.065)
B: Including Indicators for VA Treatment Facilities						
DDD	-0.080**	-0.077*	-0.081*	-0.063*	-0.059*	-0.064*
	(0.029)	(0.031)	(0.032)	(0.029)	(0.029)	(0.031)
×Log Dialysis Clinics						
Per Capita in 1971	-0.067**		-0.082*	-0.065*		-0.076*
	(0.025)		(0.037)	(0.027)		(0.037)
×Log Transplant Programs						
Per Capita in 1971		-0.016	0.044		-0.017	0.037
		(0.058)	(0.065)		(0.057)	(0.064)

Source—Author's analysis of Multiple Cause Mortality Files for 1968-1978 and the publication "Kidney Disease Services, Facilities, and Programs in the United States" (Kidney Disease Program, 1971).
Notes—Dependent variable is the mortality rate in the state-age-time-gender-cause of death cells, where time is measured in half-year increments. Definitions of kidney disease based on codes in table A. DDD is the triple difference coefficient for being under 65, in the post expansion period, with kidney disease; models with interactions of DDD with either dialysis clinics or transplant programs also include all two- and three- way interactions of dialysis clinics or transplant programs with under 65, post, and the kidney disease indicator. Models also include indicators for having no dialysis clinics or transplant programs in a state; panel B also includes indicators for the presence of VA dialysis clinics and transplant programs (also interacted with DDD). All measures of dialysis clinics and transplant programs have been demeaned. All models include state, time, and age fixed effects, an indicator for female, and age trends interacted with under 65, post, and kidney disease. Sample is restricted to deaths to whites between 45 and 84 years of age. Estimates are from Poisson regressions, standard errors three-way clustered on state, age, and time in round brackets; each state weighted by its total population.
+ $p<0.1$, * $p<0.05$, ** $p<0.01$

In order to test if access to care was an important contributor to the reduction in mortality associated with the ESRD expansion, I augmented equation (2) with interactions between the triple-difference variables and measures of the number of dialysis clinics and transplant programs per capita. Table 6 presents the results of this analysis. Panel A demonstrates that living in a state with more dialysis clinics in 1971 was associated with a significantly larger decline in kidney disease mortality. This result (panel B) persists, even after I include indicators for the presence of Veteran's Administration dialysis clinics and transplant programs.

The lack of evidence that transplant programs affect local mortality is not surprising since transplant programs require fewer visits than dialysis clinics. Therefore patients may be willing to travel long distances in order to get a kidney transplant, meaning that the number of programs in a state is not the most relevant metric affecting their survival.

The implication of these results is that either the ESRD program increased the number of dialysis clinics in states that already had a large number of clinics, relative to population, or that the program facilitated access to the existing clinic network. In the next subsection, I test if the number number of dialysis clinics per capita after the expansion increased more in areas with more dialysis clinics per capita in 1971, or if there was greater entry in areas with fewer dialysis clinics per capita.

5.2 Entry of Treatment Facilities

In order to test the entry hypothesis, table 7 presents regression coefficients from the following model:

$$\ln E\left[\frac{y_s^{1975}}{pop_s^{1975}}\right] = \alpha_0 + \alpha_1 \ln\left(\frac{y_s^{1971}}{pop_s^{1971}}\right) + \alpha_2 \ln Mort_{s,<65}^{1971} + \alpha_3 \ln Mort_{s,\geq 65}^{1971} + \alpha_4 \mathbf{1}\left[y_s^{1971} = 0\right] \quad (4)$$

Where the model is estimated as a Poisson regression, s denotes the state, superscripts refer to the year to which the data refer, y_s^t is either the number of dialysis clinics or transplant programs in state s at time t, $Mort_{s,g}^t$ is the kidney disease mortality rate using the "narrow" definition with deaths to attributed kidney disease based on the underlying cause of death codes in state s, year t, for age group g (either under 65 or 65 and older), and pop_s^t is the population in state s and year t. α_1 tests if the measure of treatment programs in a state is converging across the country depending on whether or not the elasticity of 1975 treatment capacity with respect to 1971 treatment capacity is greater than, less than, or equal to one. α_2 and α_3 test if treatment capacity is responsive to the burden of disease in the area since areas with a greater burden of disease will have a higher mortality rate due to kidney disease. *A priori* one would expect $\alpha_2 > 0$ and $\alpha_3 \approxeq 0$ as indicators that the Medicare expansion, since it affected people under 65 years of age, encouraged entry.

Table 7 demonstrates that the number of dialysis clinics (columns 1-3) was converging over time since the coefficient on 1971 treatment capacity is less than one. In other words, states with comparatively few dialysis clinics, relative to population, in 1971 experienced a more rapid rate of increase than did states with more dialysis clinics per capita in 1971. Furthermore, there is evidence that mortality among people under 65 served to increase the number of clinics in a state in 1975, which is consistent with the Medicare expansion encouraging entry of new dialysis clinics. Column 4-6 demonstrate that this pattern was weaker for transplant programs, with the elasticity of transplant programs per capita being significantly closer to 1 than was the case for dialysis clinics. There is also no evidence that entry of transplant

Table 7: Entry of Dialysis and Transplant Facilities

	Per 100,000 in 1975					
	Dialysis Clinics			Transplant Programs		
	(1)	(2)	(3)	(4)	(5)	(6)
Log Per Capita						
Dialysis Clinics	0.316*	0.362**	0.388**			
	(0.127)	(0.082)	(0.079)			
Transplant Programs				0.710**	0.610**	0.751**
				(0.154)	(0.134)	(0.146)
Log Kidney Disease Mortality Rate						
Under 65		0.648**	0.462+		0.627	-0.470
		(0.209)	(0.275)		(0.477)	(0.739)
65 and Over			0.521			2.448*
			(0.523)			(1.127)
Constant	-0.381	-1.988**	-4.436+	-0.500	-2.441	-13.232**
	(0.260)	(0.628)	(2.566)	(0.437)	(1.562)	(4.947)

Source—Author's analysis of Multiple Cause Mortality Files for 1971, the publication "Kidney Disease Services, Facilities, and Programs in the United States" (Kidney Disease Program, 1971) and the 1977 Social Security Bulletin. See text for details.

Notes—Independent variables are measured in 1971. Kidney disease mortality defined using the "narrow" definition and underlying causes of death. Estimates from Poisson models, robust standard errors in parentheses.

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

programs was correlated with the 1971 burden of disease among people under 65 years of age, but rather kidney disease mortality among people 65 and over was more strongly correlated with the number of transplant programs in a state in 1975.

The fact that the Medicare expansion promoted greater entry of dialysis clinics than transplant programs is consistent with differences in how these two forms of treatment are used. Dialysis clinics require that patients return frequently for treatment since the typical treatment regimen may include as many as five treatments per week, as a result proximity to a dialysis clinic is important, hence one would expect to see a large increase in dialysis clinics. On the other hand, kidney transplant programs require fewer visits so that patients may be willing to travel long distances in order to get a kidney transplant, meaning that there is a weaker incentive for new transplant programs to enter in response to patient demand.

6 Welfare Implications

In order to assess the welfare implications of the ESRD program, I estimated the number of life years saved by the program for 0 and 45 year olds through age 65 (when the person would otherwise enter Medicare) and through age 84, where I assumed that everyone died at the end of that year. I estimated the mortality rate due to kidney disease and all other causes using a Poisson regression of the mortality probability (deaths divided by population) on year fixed effects, a dummy for gender, and a full set of age fixed effects interacted with an indicator for kidney disease and an indicator for the post period (equation (1a)). I then considered two counterfactuals, one in which people cannot die from kidney disease, so the only factor determining mortality is the non-kidney disease mortality rate. In the second counterfactual people could die of kidney disease but the time of death from kidney disease was independent of the time of death from all other causes, so that probability of dying at age a due to either kidney disease or some other cause was simply the sum of the two probabilities.[4] I then calculated the survival of a cohort of 0 year olds to either age 65 or age 85 and 45 year olds to age 65 under the two counterfactuals and before and after the ESRD expansion and computed the difference and difference-in-difference estimates to capture changes in treatment of all other causes of death and changes in kidney disease mortality. In order to scale these results, I then converted each estimate into the number of life years saved based on the number of 0 or 45 year olds in 1978 and calculated the cost per life year saved using 1978 per capita spending ($64,500 per year) and assuming that there were 45000, 35000, or 11500 beneficiaries for the 0 to 85, 0 to 65, and 45 to 65 calculations, respectively.[5]

Table 8 presents the results of this analysis. Given how rare kidney disease is, it is not surprising that columns 1 and 3, which measure expected survival with and without deaths due to kidney disease, are so close. In all but one case, I also find that expected survival increased more in the case when there are deaths due to kidney disease than when there are no deaths due to kidney disease. Finally, under more than half of the scenarios that I considered, the ESRD program spent less than $100,000 per life year saved, suggesting that the ESRD program may have been welfare improving.

7 Conclusions

In this paper, I estimated the causal effect of the 1973 Medicare expansions affected people with kidney disease. In aggregate the expansion increased insurance coverage for people with kidney disease and may have increased physician visits. I also documented a significant decrease in mortality due to kidney disease that was robust to a variety of specification checks and alternative definitions of kidney disease.

I identify two mechanisms for my results. The first mechanism is that the increase in insurance coverage provided access to treatment that was otherwise un-

[4] Assuming that survival durations are independent is a strong assumption, but some assumption is needed in order to evaluate treatment effects in this kind of a competing risks framework (Honoré and Lleras-Muney, 2006).

[5] I assumed that there were 11500 beneficiaries between 45 and 65 years of age based on the age distribution of people who were eligible for Medicare only because of ESRD, so people who did not also have a long-term disability.

Table 8: Life Years Gained from the ESRD Program

	W/o Kidney Disease		W/Kidney Disease				
	(1) Post	(2) Post-Pre	(3) Post	(4) Post-Pre	(5) DD	(6) # Life Years	(7) $000s/LY
Narrow Definition, Underlying Cause							
0-84	71.54	1.50	71.48	1.51	0.004	13535	207.3
0-65	61.80	0.70	61.78	0.70	0.007	22807	99.0
45-65	19.41	0.21	19.40	0.21	0.002	5056	146.7
Narrow Definition, Underlying and Contributing Cause							
0-84	71.88	1.51	71.48	1.51	-0.005	-16689	-168.1
0-65	61.89	0.69	61.78	0.70	0.010	33657	67.1
45-65	19.46	0.21	19.40	0.21	0.005	11673	63.5
Chronic Definition, Underlying Cause							
0-84	71.53	1.50	71.48	1.51	0.003	11428	245.5
0-65	61.79	0.70	61.78	0.70	0.005	18024	125.2
45-65	19.41	0.21	19.40	0.21	0.002	4081	181.8
Chronic Definition, Underlying and Contributing Cause							
0-84	71.83	1.50	71.48	1.51	0.010	33380	84.1
0-65	61.87	0.69	61.78	0.70	0.013	42009	53.7
45-65	19.45	0.20	19.40	0.21	0.007	14992	49.5
Broad Definition, Underlying Cause							
0-84	71.59	1.47	71.48	1.51	0.036	119600	23.5
0-65	61.81	0.69	61.78	0.70	0.016	53746	42.0
45-65	19.42	0.20	19.40	0.21	0.008	17433	42.5
Broad Definition, Underlying and Contributing Cause							
0-84	71.98	1.47	71.48	1.51	0.032	106648	26.3
0-65	61.91	0.68	61.78	0.70	0.021	71434	31.6
45-65	19.47	0.20	19.40	0.21	0.012	25431	29.2

Source—Author's analysis of Multiple Cause Mortality Files for 1968-1978.
Notes—Life years calculated based on coefficients from a poisson regression of the cause-specific mortality rate on year fixed effects, an indicator for female, and a full set of age-by-post-by-kidney disease fixed effects. Predicted mortality applied to the 1978 population to calculate change in life years from the ESRD expansion, see text for details. Column 6 is the total number of life-years saved over the lifetime of a newborn (rows 0-84 and 0-65) or for a 45 year old. Column 7 is the cost per life-year saved based on a cost of $64,500 per beneficiary and either 43,500 (0-84), 35,000 (0-65) or 11,500 (45-65) beneficiaries.

available (Nyman, 2003, 1999a,b). Consistent with this mechanism, I find larger reductions in kidney disease mortality for people under 65 in areas that had more dialysis facilities in 1971. An implication of this mechanism is that there is a large liquidity effect in the demand for medical care.

I also find evidence in support of a second, supply-side, mechanism by which the Medicare expansion lead to increased entry of dialysis clinics. In support of such a response, I found evidence of an increase in the number of dialysis clinics by 1975 in states with a higher kidney disease mortality among people under 65 years of age in 1971. I do not find a comparable effect for transplant programs, which is consistent with transplant programs competing across larger geographic areas, while dialysis clinics compete in more local markets.

My results contribute to a large literature on the effects of public insurance programs (e.g. Currie and Gruber, 1996a,a; Cutler and Gruber, 1996; Finkelstein, 2007; Finkelstein and McKnight, 2008; Finkelstein et al., 2012; Goodman-Bacon, Forthcoming; Gruber and Simon, 2008). However, a distinctive feature of my results, relative to others in the literature, is that the program that I study conditions coverage on being in poor health. As a result, the benchmark for evaluating this program is somewhat different than for other insurance expansions since an effect on mortality that may seem large among a population that was not selected on the basis of ill health, may be much more plausible in the context of a program that explicitly conditioned eligibility on people having an expected survival of days or weeks following diagnosis with ESRD.

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Appendix Table A: ICD Codes for Kidney Disease, by ICD Revision

	ICD-7 (1968 NHIS)	ICDA-8 (1968-1978)	ICD-9 (1979-1980 NHIS)
Narrow Definition:			
Chronic Kidney Disease	592-594,792	582-584, 593.2, 792	582-589
Acute Kidney Disease	590-591	580-581, 593.1	580-581, 584
Broad Definition (additions):			
Other Diseases of Urinary System	600-609	590-599	590-599
Hypertension	442,446	403-404	403-404

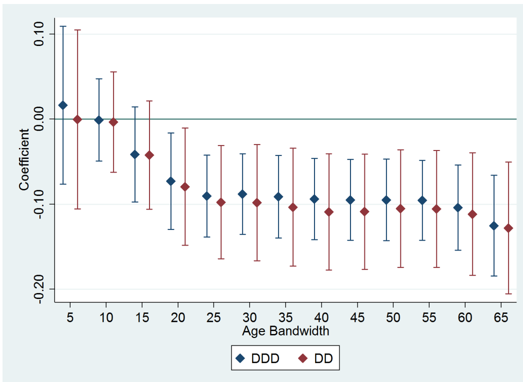
Online Appendix—Not for publication

Online Appendix Figures

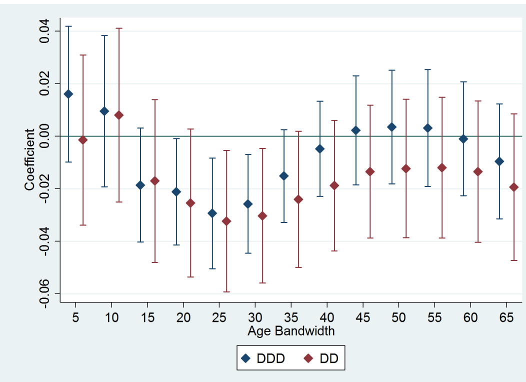
Online Appendix Tables

Figure A1: Robustness to Different Bandwidths

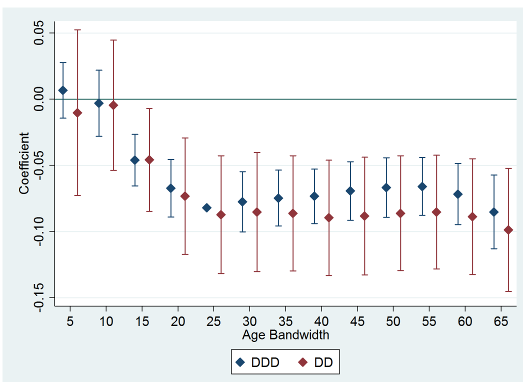
A: Narrow, Underlying Cause



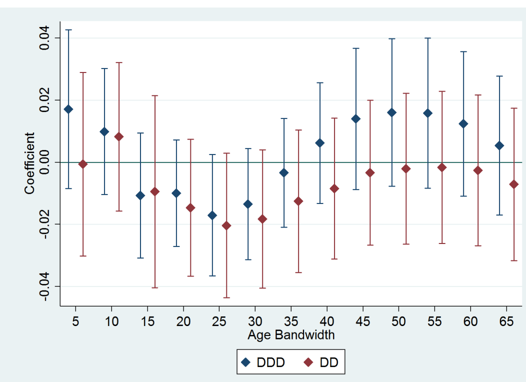
B: Narrow, Underlying or Contributing Cause



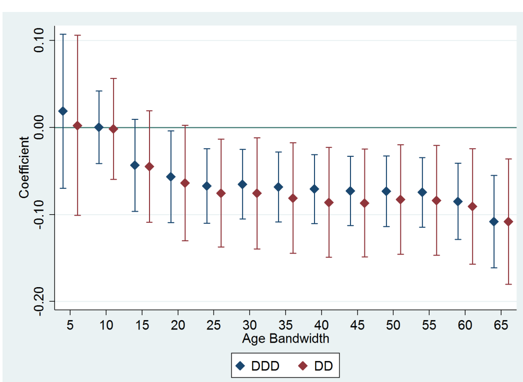
C: Broad, Underlying Cause



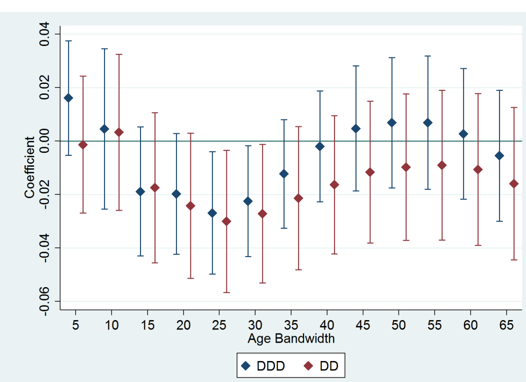
D: Broad, Underlying or Contributing Cause



E: Chronic, Underlying Cause



F: Chronic, Underlying or Contributing Cause



Source—Author's analysis of Multiple Cause of Death Files, 1968-1978.
Notes—See notes to table 3.

Table B1: Effect of the ESRD Program on Health Insurance and Health Care Utilization (Donut Regressions)

	Any Insurance		Medicare		Any Private		Only Private		Doctor Visits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Difference-in-Difference-in-Difference										
DDD	-0.029	0.043	0.153**	0.362**	-0.058	0.087	-0.241*	-0.326	0.241**	0.131
	(0.062)	(0.154)	(0.033)	(0.089)	(0.129)	(0.310)	(0.089)	(0.229)	(0.089)	(0.293)
Age Trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	112784	112784	90091	90091	143358	143358	90091	90091	291674	291674
B: Difference-in-Difference										
DD	0.018	0.117	0.196**	0.438**	-0.142	0.068	-0.221*	-0.359	0.313**	0.226
	(0.065)	(0.178)	(0.045)	(0.113)	(0.117)	(0.325)	(0.084)	(0.247)	(0.103)	(0.313)
Age Trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	1013	1013	586	586	1000	1000	586	586	1843	1843

Source—Author's analysis of the National Health Interview Survey from 1968-1980.
Notes—Dependent variable is indicated by the column group title. Kidney disease is defined using the definition of kidney disease (see Appendix Table A). DDD is the triple-difference coefficient from the interaction of a dummy for being under 65 years of age, a dummy for the second half of 1973 or later, and a dummy for having kidney disease; DD is the difference-in-differnece coefficient from a sample with kidney disease. Models include year, age, gender, and race fixed effects along with all one-, two-, and, if appropriate, three-way interactions of under 65, post, and kidney disease; models with age trends also include additional interactions with age-65. Sample restricted to individuals between 45 and 59 and 70 and 84 years of age. Estimates are from OLS regressions in columns (1)-(8) and Poisson in columns (9) and (10). Standard errors clustered on age in round brackets.
+ $p<0.1$, * $p<0.05$, ** $p<0.01$

Table B2: OLS Estimates of the Effect of the ESRD Program on Mortality

	Narrow Definition				Broad Definition		Chronic Only	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Ages 45 to 84								
DDD	-0.372**	-0.060	-0.078**	-0.030*	-0.068**	-0.020*	-0.028	-0.028*
	(0.052)	(0.036)	(0.008)	(0.012)	(0.020)	(0.009)	(0.033)	(0.013)
DD	-0.359**	-0.049	-0.063**	-0.018	-0.057*	-0.009	-0.018	-0.016
	(0.054)	(0.041)	(0.014)	(0.015)	(0.023)	(0.012)	(0.039)	(0.015)
B: Ages 45 to 60 and 70 to 84								
DDD	-0.453**	-0.155**	-0.095**	-0.079**	-0.161**	-0.056**	-0.105+	-0.073**
	(0.053)	(0.047)	(0.009)	(0.018)	(0.033)	(0.012)	(0.053)	(0.023)
DD	-0.437**	-0.140*	-0.077**	-0.060*	-0.144**	-0.038+	-0.090	-0.056+
	(0.057)	(0.064)	(0.015)	(0.027)	(0.048)	(0.021)	(0.068)	(0.028)
Age trends	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Underlying only?	Yes	Yes	No	No	Yes	No	Yes	No
Mean Annual Kidney Disease Mortality Rates (per 100,000)								
Ages 45-64								
1968-1973H1	6.3	6.3	38.2	38.2	13.1	49.6	5.3	34.8
1973H2-1978	5.1	5.1	35.6	35.6	8.6	43.4	4.4	31.2
Ages 65-84								
1968-1973H1	26.9	26.9	240.1	240.1	95.6	351.6	23.4	227.7
1973H2-1978	30.7	30.7	230.4	230.4	73.8	310.1	25.9	209.2

Source—Author's analysis of Multiple Cause Mortality Files for 1968-1978.

Notes—Dependent variable is the log mortality rate in the age-time-gender-cause of death cells, where time is measured in half-year increments. Definitions of kidney disease based on codes in table A. DDD is the triple difference coefficient for being under 65, in the post expansion period, with kidney disease; DD is the difference-in-differences coefficient for being under 65 and in the post expansion period using a sample that is restricted to deaths due to kidney disease. Models that do not restrict to underlying causes of deaths also define a death as due to kidney disease if kidney disease is either an underlying or a contributing cause of death. All models include time fixed effects (measured in six month increments), age fixed effects, an indicator for female, and all possible interactions of an indicator for being under 65, a post period dummy, and, where appropriate, an indicator for deaths due to kidney disease. Models with age trends also include interactions with age minus 65 in addition to the under 65, post, and kidney disease interactions. Sample is restricted to deaths to whites between 45 and 84 years of age in panels A; panel B excludes deaths to people between 61 and 69 years of age. Estimates are from OLS regressions, standard errors two-way clustered on age and time in round brackets.

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

Table B3: Robustness of the Effect of the ESRD Program on Mortality

	Underlying Cause of Death					Underlying and Contributing Cause of Death				
		Age Trends		Time Trends			Age Trends		Time Trends	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Power	N/A	Linear	Quadratic	Linear	Quadratic	N/A	Linear	Quadratic	Linear	Quadratic
A: Narrow Definition										
DDD	-0.373**	-0.073*	-0.059*	-0.076	-0.075	-0.059**	-0.021*	-0.016	-0.017*	-0.020**
	(0.049)	(0.029)	(0.029)	(0.056)	(0.066)	(0.007)	(0.010)	(0.010)	(0.007)	(0.007)
DD	-0.363**	-0.079*	-0.064*	-0.067	-0.062	-0.048**	-0.025+	-0.018	-0.009	-0.007
	(0.053)	(0.035)	(0.031)	(0.062)	(0.071)	(0.014)	(0.014)	(0.013)	(0.018)	(0.018)
B: Broad Definition										
DDD	-0.197**	-0.067**	-0.048**	-0.073**	-0.070**	-0.042**	-0.010	-0.007	-0.030**	-0.030**
	(0.019)	(0.011)	(0.012)	(0.010)	(0.013)	(0.006)	(0.009)	(0.008)	(0.006)	(0.007)
DD	-0.189**	-0.073**	-0.053**	-0.066*	-0.058+	-0.032**	-0.015	-0.010	-0.021	-0.017
	(0.025)	(0.022)	(0.017)	(0.028)	(0.033)	(0.010)	(0.011)	(0.010)	(0.015)	(0.014)
C: Chronic Definition										
DDD	-0.340**	-0.057*	-0.042	-0.062	-0.063	-0.056**	-0.020+	-0.014	-0.018*	-0.022**
	(0.046)	(0.027)	(0.026)	(0.058)	(0.067)	(0.007)	(0.012)	(0.011)	(0.007)	(0.007)
DD	-0.331**	-0.064+	-0.047	-0.053	-0.050	-0.045**	-0.024+	-0.017	-0.010	-0.009
	(0.051)	(0.034)	(0.029)	(0.064)	(0.074)	(0.013)	(0.014)	(0.012)	(0.019)	(0.020)

Source—Author's analysis of Multiple Cause Mortality Files for 1968-1978.
Notes—See Note to table 3.
+ p<0.1, * p<0.05, ** p<0.01

Table B4: The Confounding Effect of the SSI Program on Mortality

	Narrow Definition		Broad Definition		Chronic Only	
	(1)	(2)	(3)	(4)	(5)	(6)
DDD	-0.0572	-0.0814**	-0.0654*	-0.0809**	-0.0356	-0.0562+
	(0.039)	(0.030)	(0.030)	(0.017)	(0.035)	(0.029)
×% with Soc. Security Income	0.114		0.0145		0.0509	
	(0.125)		(0.081)		(0.130)	
×% with SSI Income	-0.00173		-0.580		0.447	
	(0.783)		(0.563)		(0.769)	
×More Stringent Criteria		0.0491		0.0357+		0.0327
		(0.039)		(0.019)		(0.040)
×Separate Medicaid Application		-0.0124		0.0440*		-0.0279
		(0.035)		(0.019)		(0.051)

Source—Author's analysis of Multiple Cause Mortality Files for 1968-1978 and the Current Population Survey.

Notes—Dependent variable is the mortality rate in the state-age-time-gender-cause of death cells, where time is measured in half-year increments. Definitions of kidney disease based on codes in table A. DDD is the triple difference coefficient for being under 65, in the post expansion period, with kidney disease. % with Soc. Security Income and % with SSI income are the demeaned percentages of people in a state-age-gender cell who report receiving either Social Security or SSI income, respectively, in the 1977 to 1979 March CPS, More Stringent Criteria indicates that a state has adopted a more stringent asst or income test for Medicaid eligibility than used by the SSI program, and Separate Medicaid Application indicates that a state requires individuals to separately apply for SSI and Medicaid. Interactions with DDD also include interactions with under 65, post, and kidney disease. All models include state, time, and age fixed effects, an indicator for female, and age trends interacted with under 65, post, and kidney disease. Sample is restricted to deaths to whites between 45 and 84 years of age. Estimates are from Poisson regressions, standard errors three-way clustered on state, age, and time in round brackets; each state weighted by its total population.

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