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Challenging Assumptions of Outcomes and Costs Comparing Peritoneal and Hemodialysis

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Abstract

Objectives—Policymakers have suggested increasing peritoneal dialysis (PD) would improve end-stage kidney disease (ESKD) outcomes and reduce Medicare spending compared to hemodialysis (HD). We compared mortality, hospitalizations, and Medicare spending between PD and HD among uninsured adults with incident ESKD.

Methods—Using an instrumental variable design, we exploited a natural experiment encouraging PD among the uninsured. Uninsured patients usually receive Medicare at dialysis month four. For

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those initiating with PD, Medicare covers the first three dialysis months, including *pre-dialysis* services in the calendar month of dialysis start. Starting dialysis later in a calendar month increases *pre-dialysis* coverage essential for PD catheter placements. The policy incrementally encourages PD when developing ESKD later in the month. Dialysis start day appears unrelated to patient characteristics and effectively "randomizes patients" to dialysis modality, mitigating selection bias.

Results—Starting dialysis later in the month was associated with increased PD uptake: every week later in the month was associated with an *absolute* increase of 0.8% (95% CI: 0.6%, 0.9%) at dialysis day 1 and 0.5% (95% CI: 0.3%, 0.7%) at dialysis month 12. We observed no significant *absolute* difference between PD and HD for 12-month mortality (–0.9%, 95% CI: −3.3%, 0.8%), hospitalizations during months 7–12 (–0.05, 95% CI: −0.20, 0.07), and Medicare spending during months 7–12 (–$702, 95% CI: −$4,004, $2,909).

Conclusions—In an instrumental variable analysis, PD did not result in improved outcomes or lower costs compared to HD.

Precis:

Despite an impetus by policymakers aimed at increasing home dialysis use, policies that promote home dialysis are unlikely to improve outcomes or reduce spending.

INTRODUCTION

The end-stage kidney disease (ESKD) program costs Medicare $36 billion per year or 7% of Medicare's budget.[1,2] Kidney disease experts[3–5] and policy makers[6] have suggested that home dialysis, which is primarily peritoneal dialysis (PD), improves ESKD outcomes and reduces Medicare spending relative to center-based hemodialysis (HD). On July 10, 2019, the President signed the "Advancing American Kidney Health" Executive Order, which aims to increase home dialysis use and kidney transplantation to 80% of the incident ESKD population by 2025.[7] Over 85% of incident patients use center-based HD, so this policy would radically reverse decades of practice.[1]

Home dialysis momentum continues despite the absence of randomized evidence.[8] The Centers for Medicare and Medicaid Services (CMS) has already finalized a mandatory payment model penalizing providers who do not successfully increase PD use in their practice.[9] Although observational studies suggest that home dialysis offers similar to improved survival,[10–13] selection bias remains a major shortcoming. Some experts espouse potential savings of $10,000–$15,000 per patient when switching from center-based HD to PD,[1,3,5,14] but these estimates do not adequately adjust for patient characteristics. Ideally, a randomized-controlled trial (RCT) could compare PD and center-based HD. However, randomizing alternative dialysis modalities is difficult, and past trials have failed to recruit enough patients.[15]

We compared PD to HD provision by exploiting an idiosyncratic policy encouraging asymmetric PD use in the uninsured based on the calendar day of dialysis start. Medicare eligibility normally begins in the fourth calendar month of dialysis.[16] Uninsured patients starting with PD receive Medicare at day one and retroactive *pre-dialysis* coverage to the beginning of the calendar month. Since PD catheters require two to four weeks of surgical site healing *before* commencing dialysis,[17] initiating dialysis later in the month increases the likelihood of receiving sufficient pre-dialysis coverage for the catheter and in turn increases the likelihood of PD use at dialysis start and thereafter.[18] We therefore used the calendar day of dialysis start as a natural experiment to compare mortality, hospitalizations, and costs in uninsured patients starting with PD versus HD.

METHODS

Analytic Strategy

We conducted an instrumental variable (IV) analysis, a quasi-experimental method commonly used to reduce observational study bias (Appendix).[19–23] Medicare coverage policies encourage patients starting dialysis at the end of the month to start with PD (Appendix Figure 1). If the calendar day of dialysis start were randomly distributed, we would expect patients have similar characteristics irrespective of start day. Additionally, if starting dialysis later in the calendar month encourages PD use, we may use dialysis start day as an instrument that *randomly* "assigns" patients to PD versus HD (as a coin flip assigns treatment in an RCT). A caveat of our analysis (and IV analyses in general) is that we estimated the local average treatment effect of uninsured patients who were encouraged to switch from HD to PD due to the Medicare coverage policy based on the calendar day of the month.

Although we cannot definitively prove that dialysis start day is random, we provide corroborating evidence. Specifically, we find no evidence of bunching of dialysis starts or substantive differences in observable patient characteristics based on dialysis start day (Figure 1, Table 1, Appendix Figure 2, Appendix Table 1).

These empirical findings argue against the possibility that nephrologists might "game" the start date of dialysis by delaying dialysis starts for patients interested in PD until the end of the month to maximize retroactive Medicare coverage. Widespread gaming of the coverage policy would mean that dialysis start date is not random and thus not suitable as an instrumental variable. Gaming of dialysis starts would result in bunching of dialysis starts at the end of the calendar month. Additionally, we would expect to observe patients starting dialysis later in the calendar month reflecting characteristics associated with PD: younger in age, healthier, and socioeconomically more advantaged.[24] We observe neither result.

Instead, our findings are consistent with dialysis start day being random and suggest that patients starting dialysis at the beginning of the calendar month but interested in PD likely initiate HD with the intention of switching to PD. For patients starting with HD and subsequently receiving a PD catheter, as long as the patient switches to PD before the fourth month of dialysis, the patient will receive retroactive Medicare coverage through the beginning of dialysis, *including* the PD catheter. Patients at the margin, and their nephrologists, likely have weaker modality preferences and are probably willing to initiate HD with the eventual plan of switching to PD. Simultaneously, because dialysis initiation (PD and HD) is complex and involves multiple steps (e.g., referral to a surgeon, identifying an accepting facility, and coordinating patient and dialysis facility schedules), providers are less able to precisely predict the start date, making gaming of dialysis initiation more difficult.

In addition to providing evidence that dialysis start day is random, we also test whether dialysis start day is associated with differences in PD use. Consistent with prior work[18] and what we would predict from the coverage policy, dialysis start day was associated with differences in PD use at dialysis start. Interestingly, differences in modality at dialysis start translated into long-term differences in PD use. These findings suggest that having patients start center-based HD, even with the eventual goal of switching to PD, leads to lower long-term PD uptake. A prior study extensively analyzed the IV's properties and the mechanism by which it affects long-term PD uptake.[18] Briefly, the study observed that patients rarely switch to PD after initiating HD. This inertia explains how dialysis start day, which influences PD use at dialysis start, results in long-term differences in PD use. We therefore exploit differences in dialysis start day to test whether sustained PD uptake yields superior outcomes to HD.

Inferring causality between dialysis modality and outcomes requires that the only mechanism through which the instrument (dialysis start day) affects outcomes is through dialysis modality. Although one can never prove this assumption, we provide two points of corroborative evidence. First, the most plausible alternate mechanism is through long-term differences in Medicare coverage. Patients starting in the end of the month are more likely to choose home dialysis and thus more likely to obtain Medicare at dialysis start. However, coverage differences dissipate by month six (Appendix Table 2). Second, we observe no difference in short-term (6-month) mortality between patients starting dialysis in the first versus second half of the month (Appendix Table 3), suggesting that short-term differences in Medicare coverage are not associated with health.

We initially conducted an "intention-to-treat" (ITT) analysis, comparing patients starting dialysis at the beginning of the month to those starting at the end. Afterwards, we conducted an IV regression to directly compare outcomes between PD and HD.

Data Sources, Population, and Follow-up

From the United States Renal Data System (USRDS),[1] a registry of all US patients with ESKD linked to Medicare fee-for-service claims, we identified uninsured adults starting dialysis between 6/1/2005 and 12/31/2014. We used the CMS-2728 Form, submitted by dialysis facilities for all incident patients irrespective of insurance coverage, to identify dialysis start date and patient characteristics at start: insurance, employment status, comorbidities, and laboratory data. We identified facility characteristics from the annual dialysis facility survey (CMS-2744) and sociodemographic characteristics of each facility's zip code from the 2010 Census and the 2012 American Community Survey. We used Medicare Parts A and B claims to obtain total Medicare spending, acute hospitalizations, outpatient dialysis services, and physician dialysis visits. Medicare payments were inflation adjusted to 2015 dollars.

We excluded patients with more than 31 days of retroactive Medicare before ESKD onset according to the enrollment database, who received a kidney transplant prior to dialysis, or who started dialysis on a federal holiday or weekend because dialysis initiation on these days is usually emergent, and PD is not usually initiated under emergent circumstances. We followed patients for 12 months and did not censor for death, kidney transplantation, or modality switch. We defined one month of follow-up as four weeks (28 days) to standardize months.

Variables

Our main outcomes were 12-month mortality, number of hospitalizations (identified using a previously described algorithm[25]) per patient during dialysis months 7–12, and Medicare spending per patient during dialysis months 7–12. We modeled hospitalizations and costs as continuous variables. Because costs were right-skewed and to minimize the effect of outliers, we modeled the natural logarithm of cost. In a sensitivity analysis, we modeled non-logarithm transformed costs.

To study mortality, we used the entire uninsured population irrespective of downstream Medicare coverage. We could only observe hospitalizations and spending in patients with Medicare Parts A and B as primary payer. For these outcomes, we required Medicare coverage while alive from months 7–12. We started at month seven because coverage differences dissipate by then and including earlier months would bias results against PD. Patients who died between months 7–12 remained in the sample and contributed zero hospitalizations and zero spending in our base analysis. In a sensitivity analysis, we assessed 24-month outcomes, which required excluding patients starting dialysis in 2014.

The independent variables of interest were dialysis start day (instrument), which we modeled as a continuous variable, and whether the patient started with PD (intervention), which we modeled as a binary variable. We controlled for patient characteristics (age, sex, race, ethnicity, employment status, comorbidities, primary cause of ESKD, pre-ESKD nephrology care, body mass index, serum albumin, and serum hemoglobin), facility characteristics (profit status, whether it was hospital-based, number of patients, and patient to staff ratio), geographic characteristics (population, proportion of residents with high school diploma, median income, and median rent within the facility's zip code), and temporal characteristics (month and year) at dialysis start (full list in Table 1).

Statistical Analyses

All analyses used robust standard errors. Before conducting the IV regression, we tested the instrument's properties. First, we assessed whether the dialysis start day bunched near the end of the month. Using a Kolmogorov-Smirnov test, we compared the dialysis start day distribution among the *entire* uninsured population to a theoretical distribution from pure random chance, the proportion of times a numbered day appeared during the study period. We opted for a Kolmogorov-Smirnov test because other more powerful distributional tests (e.g., Shapiro-Wilk's or Lilliefors) assume a normal distribution. Second, we compared observable characteristics between patients starting in the first half and second half of the calendar month, using Pearson's chi-square test. Third, we assessed the instrument's strength, or its correlation with PD use. Conceptually, the analysis estimates adherence to the "assigned" treatment after randomization. We used a linear probability model to regress the probability of starting with PD on the calendar day. We plotted predicted probabilities as a function of each calendar day of dialysis start. We computed the regression's F-statistic; in the econometrics literature, an F-statistic of 10 typically indicates that the instrument has sufficient strength.[26] Finally, we confirmed that the instrument was associated with sustained differences in 12-month PD use.

The ITT analysis regressed mortality, number of hospitalizations, and costs on whether the patient started dialysis in the first versus second half of the month (i.e., we reclassified the instrument as a binary coin flip). We used logistic regression for mortality and ordinary least squares for hospitalizations and the natural logarithm of costs.

When comparing PD to HD, we first employed "traditional" multivariable regression models, logistic for mortality and ordinary least squares for hospitalization and costs. To demonstrate the effect of selection bias, we successively included covariates in a stepwise manner. Subsequently, we conducted an IV regression using two-stage residual inclusion (2SRI), which is commonly employed with a non-linear first-stage regression and has improved precision over two-stage least squares.[27] Unlike maximum likelihood estimation, 2SRI relaxes distributional assumptions. The first-stage probit regression estimated the probability of starting with PD as a function of the day of dialysis start (instrument) and controlling covariates. The second-stage used ordinary least squares (OLS) to regress the outcomes on starting with PD, controlling covariates, and the first-stage generalized residual.[28,29] We excluded the day of dialysis start (instrument) from the second-stage regressions. We chose OLS for all outcomes, including death and logarithm-transformed costs, because non-linear second-stage models do not necessarily preserve expectations when incorporating the residual.[28] Conversely, our base-case specification is robust to misspecifications of the first-stage probit model with asymptotically valid standard errors. We computed the absolute difference in marginal effects of dialysis modality. For costs, we estimated the average predicted cost for PD and HD by exponentiating regression estimates, then took the absolute difference. We used a non-parametric bootstrap (250 samples) to estimate 95% confidence intervals.

Because a large fraction (~25%) of our population had missing serum albumin and hemoglobin, we used multiple imputation (10 imputations) to account for missing covariates. We nested imputations within each bootstrap draw.[30,31]

Sensitivity Analyses

We performed sensitivity analyses on complete cases given the computational overhead of the multiple imputation model: we (1) performed a complete case analysis; (2) included patients with missing covariates and omitted those covariates; (3) used a 24-month follow-up window among patients initiating dialysis 6/1/2005 to 12/31/2013; (4) modeled costs directly (not using the natural logarithm); (5) modeled costs using a negative binomial second-stage; (6) modeled daily spending while patients were alive; (7) varied the IV regression's functional form; (8) modeled mortality using a Cox proportional hazards regression in the second stage; (9) repeated the mortality analysis in patients obtaining Medicare by month 7; (10) repeated the hospitalization and spending analyses in patients surviving through 12 months; (11) excluded patients who received a transplant in the study period; (12) excluded patients who changed dialysis modality; (13) included patients starting dialysis on a weekend or holiday; and (14) omitted facility characteristics in case modality choice drives facility choice (i.e., if facility characteristics are endogenous).

We used SAS version 9.4 (SAS Institute, Cary, NC) to construct our analytical dataset and Stata version 14.0, MP edition (StataCorp, College Station, TX) for all statistical analyses.

IRB Approval

The study was approved by the University of Southern California Institutional Review Board (IRB).

RESULTS

Baseline Characteristics and Randomness of Dialysis Start Day

Of 58,539 uninsured adults meeting inclusion criteria (Appendix Figure 3), 8% started with PD (Table 1). Compared to patients starting with HD, patients starting with PD were younger; more likely female, white, and employed; had fewer comorbidities; and had higher serum albumin and hemoglobin. They were more likely to receive pre-dialysis nephrology care and dialyze at for-profit, free-standing, and urban facilities with large PD populations.

The dialysis start day distribution in the entire population was statistically no different from pure random chance, the proportion of times a numbered day appeared during the study period (Kolmogorov-Smirnov test, $p>0.2$, Figure 1, Appendix Figure 2).[32] Critically, we observed no bunching of dialysis starts at the end of the month, which is what we would observe with delayed PD starts. Additionally, we observed no statistical difference in most observable characteristics between those starting in the first versus second half of the month in the entire study cohort (Table 1) and when restricting to the 42,932 adults obtaining Medicare by the end of month six (Appendix Table 1). Together, these findings support the hypothesis that dialysis start day is random and suggest that providers are not delaying dialysis starts for patients interested in PD.

Instrumental Variable's Properties

Starting dialysis later in the calendar month was associated with increased PD use after adjusting for confounders: every week later in the month was associated with an *absolute* increase of 0.8% (95% CI: 0.6%, 0.9%) at dialysis day 1 and 0.5% (95% CI: 0.3%, 0.7%) at dialysis month 12 (Figure 2, Appendix Figure 4). In adjusted analysis, 10.2% (95% CI: 9.7%, 10.6%) of patients starting on the first of the month used PD by dialysis month 12, while 12.3% (95% CI: 11.9%, 12.8%) of patients starting on the last day of the month used PD by dialysis month 12, an *absolute* increase of 2.2% (95% CI: 1.4%, 3.0%) and a *relative* increase of 22%. The F-statistic was 62, suggesting more than sufficient strength as an IV.

Intention to Treat Analysis

Patients starting dialysis in the first versus second half of the month had no significant difference in outcomes in unadjusted analysis (Figure 3, Appendix Figure 5). After adjusting for confounders, the odds ratio for mortality was 0.98 (95% CI: 0.92, 1.04), the coefficient for hospitalizations −0.01 (95% CI: −0.03, 0.02), and the risk ratio for total Medicare spending 1.01 (95% CI: 0.96, 1.06) (Appendix Table 4).

Comparing Peritoneal Dialysis and Hemodialysis

Unadjusted, patients starting with PD had significantly higher survival at one year (96% versus 92%, p<0.001), fewer hospitalizations during months 7–12 (0.61 versus 0.69, p<0.001), but higher log-transformed spending during months 7–12 ($15,917 versus $10,622, p<0.001) (Appendix Figure 6). PD was less expensive than HD during months 7–12 when modeling costs directly ($27,386 versus $30,875, p<0.001).

When adjusting for confounders, the "traditional" analysis showed decreased mortality and increased spending between PD and HD (Table 2). Conversely, the IV analysis showed no significant *absolute* difference between PD and HD for 12-month mortality (−0.9%, 95% CI: −3.3%, 0.8%), hospitalizations during months 7–12 (−0.05, 95% CI: −0.20, 0.07), and Medicare spending during months 7–12 (−$702 (95% CI: −$4,004, $2,909) per patient.

To explore the effect of selection bias, we successively added covariates to the traditional model in a complete case analysis. Differences in 12-month mortality and total Medicare spending declined in magnitude but remained significant (Appendix Table 5). When modeling costs directly (i.e., without log-transforming), PD was cheaper than HD in unadjusted analysis but not the adjusted traditional or IV analyses (Appendix Table 6).

Sensitivity Analyses

Results were robust to all sensitivity analyses except when using Pearson residuals (death, costs), when excluding patients who changed dialysis modality (inpatient costs), and when omitting facility characteristics or covariates with missing values (hospitalizations, inpatient costs) (Appendix Table 7).

DISCUSSION

After accounting for selection bias, we found that initiating PD (rather than HD) did not result in statistically significant differences in mortality, hospitalizations, or Medicare spending in uninsured adults with ESKD. Unlike other observational studies, ours used an ostensibly random event, the day of dialysis start, to mitigate bias from *unobserved* characteristics. We found mortality and cost differences when using traditional observational methods that dissipated with IV regression.

Unlike previous studies associating similar[10,12] or improved survival[11,13] and decreased costs with PD,[3–5,14] our study did not show statistically significant differences in mortality, hospitalization, and costs between PD and HD among uninsured patients.[33] In addition to population differences between our analysis and prior studies, there are other key differences. Our log-transformed model showed *increased* unadjusted costs in PD over HD, while our non-transformed model showed *decreased* costs (consistent with prior studies). One can reconcile these results by recognizing that the log-transformed model down-weights outliers. That is, the median patient on PD is more expensive than the median patient on HD, but average per patient spending on PD is lower than on HD. To control for confounders, prior studies have used multivariable regression and matching techniques, with annual savings estimates of over $10,000 per patient attributable to PD.[3,10–14] However, prior findings are subject to selection bias.

Our study demonstrates the pitfalls of standard observational methods. Selection bias when comparing dialysis modalities is unsurprising, since patients starting with PD are generally younger, healthier, and less socioeconomically disadvantaged than those starting with HD.

Although an RCT could address these shortcomings, none have been successful. In 2003, investigators in the Netherlands designed an RCT to compare PD and HD. They recruited 773 patients, but only 38 patients (fewer than 5%) agreed to participate.[15] IV regression mitigates these selection bias concerns.

Generalizability of IV estimates, however, is limited to local average treatment effects[20] of uninsured patients who were encouraged by the Medicare coverage policy to switch from PD to HD. We make two observations that suggest our findings may be more widely applicable to the dialysis population. First, the failures of previous RCTs suggest patients have strong preferences for dialysis modality, and patients more likely to switch dialysis modalities due to insurance coverage may be more likely to switch modalities in response to real-world policy. Second, although uninsured patients *without ESKD* are sicker than the rest of the population, uninsured patients *with ESKD* are younger and healthier than their Medicare or Medicaid counterparts and are *more likely* to use peritoneal dialysis than patients with Medicaid.[18] Our findings support expert opinion that patients should be allowed to choose between two roughly equivalent dialysis modalities based on personal preferences.

Despite little evidence demonstrating its superiority (at least vis-à-vis conventional outcome metrics), the Administration has made home dialysis the centerpiece of its ESKD reform effort.[7] Persuading patients to start with or switch to home dialysis, though, may be challenging. Past randomization failures suggest that many patients have strong dialysis modality preferences by the start of dialysis. CMS implemented financial incentives for home dialysis in 2011, but uptake remains sluggish.[24,34] Even in other countries prioritizing home dialysis use, patients using home dialysis usually constitute less than half the ESKD population.[35]

To further encourage home dialysis use, two new incentives for home dialysis have already taken effect this year: a policy exacting large financial penalties from facilities and nephrologists with low home dialysis uptake and an expanded dialysis transitional add-on payment for home dialysis capital equipment.[9,36] Without evidence of improved outcomes or downstream savings, these policies seem likely to penalize providers with little material benefit while potentially adding to already ballooning ESKD costs. Additionally, some experts have suggested that CMS increase reimbursements for pre-dialysis education[8,37] or pay for dedicated home dialysis caregivers ("assisted peritoneal dialysis") with the hope of eventual savings.[38,39] Our results suggest that additional funding of large-scale home dialysis efforts would not yield substantial benefits to patients or savings to the Medicare program, and certainly not at the magnitude suggested by policymakers.

Even if PD does not significantly reduce mortality, hospitalizations, or spending, it could yield improved quality of life. Unfortunately, studies investigating quality of life are also fraught with selection bias.[40,41] Patients using PD are more likely to have stable housing, more likely employed, and more likely to have strong social supports, characteristics that also contribute to a higher quality of life. Because we could not observe quality of life, we could not formally study this question. Still, based on our findings, we suspect that quality of life studies would see large attenuations in effect sizes if subjected to randomization or quasi-experimental designs.

Our findings have important limitations that may diminish their applicability to the general ESKD population. First, despite using an IV, there may be residual confounding by unobserved characteristics. Still, given the difficulties of past RCTs—the gold-standard for addressing unobserved confounding—, IV regression may be the best alternative to mitigate bias. Although limitations of the CMS-2728 Form may contribute to residual confounding,[42] the IV model mitigates these unobserved biases. Second, we cannot prove the assumptions of the IV model. However, we provide evidence that the dialysis start day is randomly distributed and also show that dialysis start day does not affect long-term Medicare enrollment, the most likely alternative mechanism through which dialysis start day could affect outcomes. Third, we could not assess differences in short-term costs between months 1–6 given data limitations. However, our sensitivity analyses demonstrate no differences in extended long-term costs, from month 7 through 12 (in our primary analysis) and month 24 (in our secondary analysis), which is likely the more relevant policy metric. Fourth, our findings may not generalize beyond the uninsured. Because IV estimates are of local average treatment effects, they apply only to patients who switched dialysis modality due to retroactive Medicare coverage. However, as previously discussed, the uninsured population is younger and more likely to use PD and thus may be more representative of patients willing to consider PD.

CONCLUSIONS

In summary, we find no evidence that increasing PD use for patients with incident ESKD would offer reductions in mortality, hospitalizations, or cost. Policy makers eager to promote home dialysis should temper expectations of improved outcomes and reduced spending.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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HIGHLIGHTS

- While observational studies suggest that peritoneal dialysis offers improved outcomes and lower costs than hemodialysis, these studies are fraught with selection bias. Researchers have attempted randomized controlled trials to study the initial choice of dialysis modality (peritoneal dialysis or in-center hemodialysis) but have failed to enroll enough patients.
- We conducted an instrumental variable analysis, exploiting an idiosyncratic policy encouraging PD use in 58,539 uninsured adults with end-stage kidney disease. The initial choice of dialysis modality (peritoneal dialysis or in-center hemodialysis) did not result in statistically significant differences in mortality, hospitalizations, or costs.
- Policy makers have already implemented policies financially incentivizing peritoneal dialysis over in-center hemodialysis. Even if these policies increase peritoneal dialysis use, they should temper expectations of improved outcomes or reduced spending.

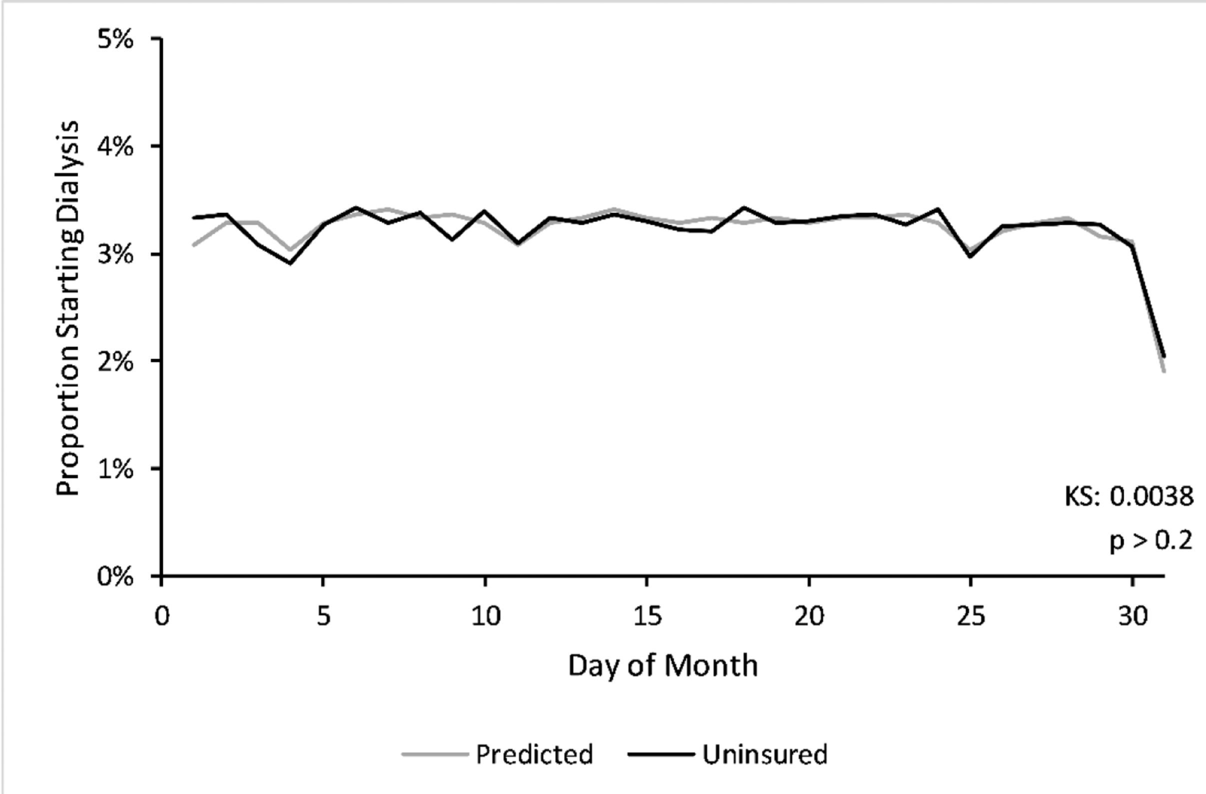

Figure 1: Comparing observed days of the month of dialysis start and predicted.

We show the proportion of uninsured patients starting dialysis at each numbered day of the month (in black) against the predicted distribution or the proportion of times the numbered day appears between 1/1/2006 and 12/31/2015 (in gray). Distributions exclude weekends and federal holidays. We show the cumulative distribution in Appendix Figure 2. The one-sample Kolmogorov-Smirnov statistic is the maximum distance between the predicted and observed cumulative distributions ($p > 0.2$). Abbreviations: KS = Kolmogorov-Smirnov statistic.

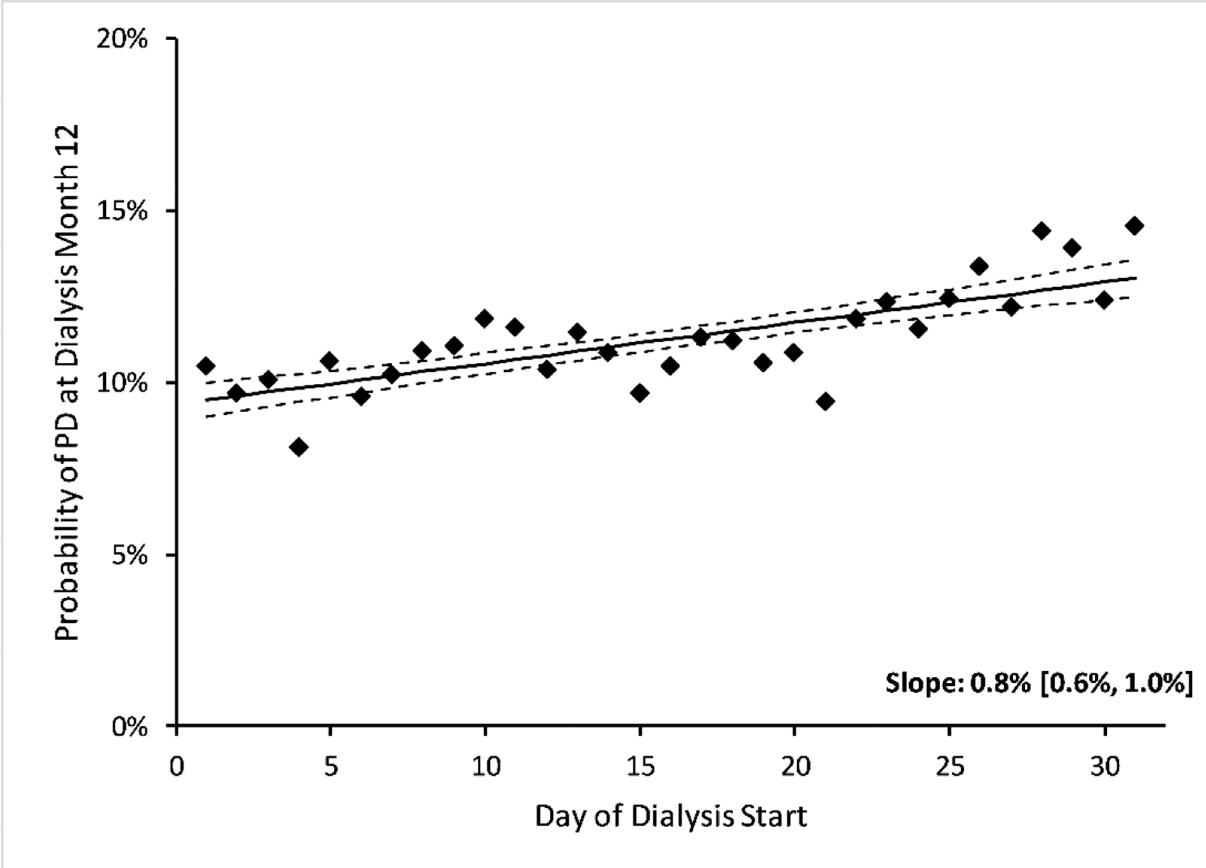

Figure 2: Unadjusted Probability of Peritoneal Dialysis Use at Month 12.

We show the unadjusted and adjusted probabilities of peritoneal dialysis at dialysis start and the adjusted probability of peritoneal dialysis at month 12 in Appendix Figure 4. Abbreviations: PD = peritoneal dialysis.

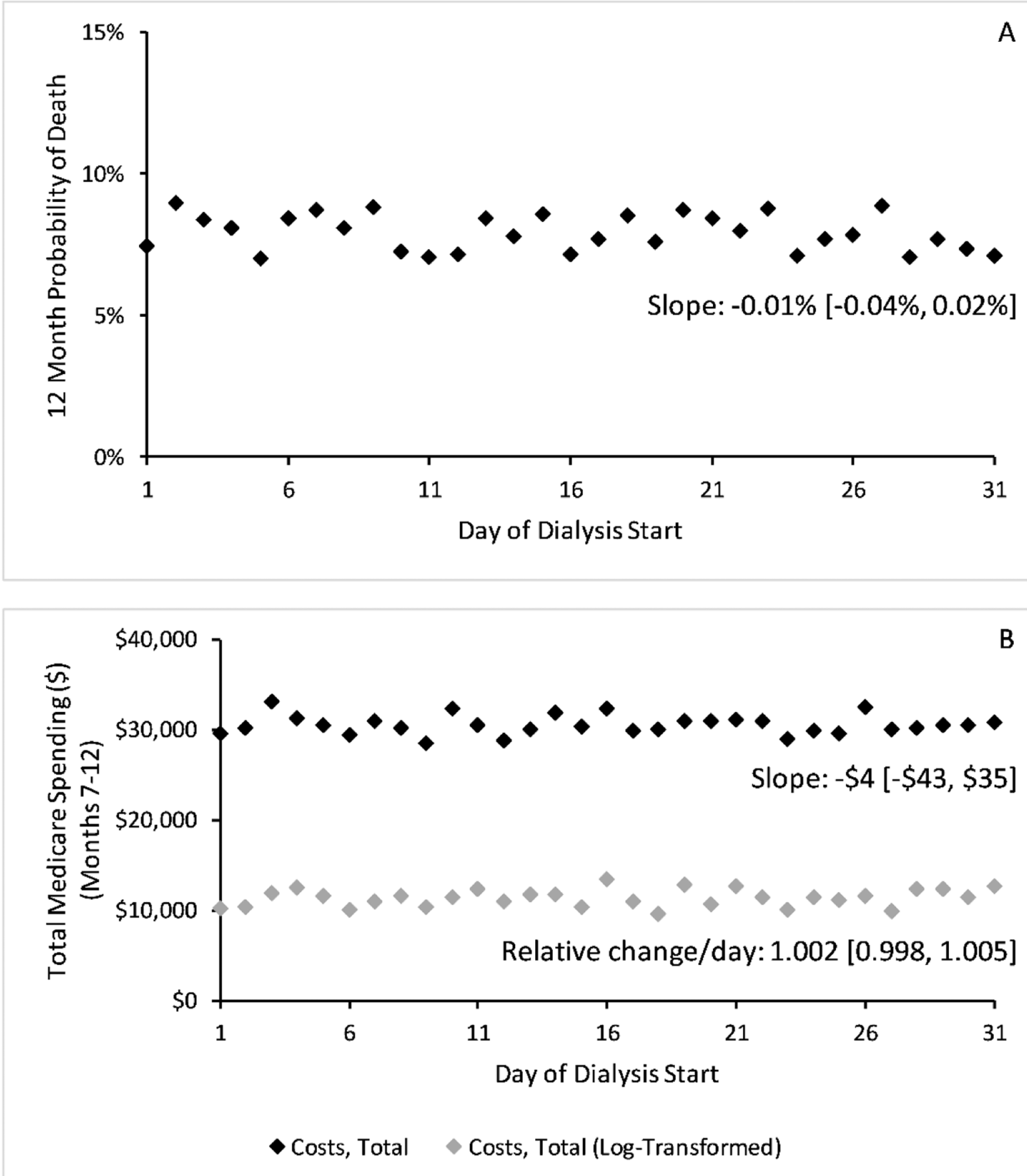

Figure 3: Unadjusted changes in mortality and spending by day of dialysis start.

We show unadjusted 12-month probability of death (Panel A) and total Medicare spending per patient in months 7–12 of dialysis (Panel B) by the day of dialysis start. Costs (Panel B) are modeled using log costs (grey dots) and without log transforming (block dots). Trends and slopes were computed using ordinary least squares without adjusting for other covariates. We show differences in hospitalization rates by day of dialysis start in Appendix Figure 5. Abbreviations: HD = hemodialysis, PD = peritoneal dialysis.

Table 1:

Baseline Characteristics of Uninsured Adults, Stratified by Starting Dialysis Modality and by Day of Dialysis Start

CHARACTERISTIC	Dialysis Modality at Start				Half of Calendar Month at Start			
	HD		PD		First Half		Second Half	
	N	%	N	%	N	%	N	%
Patient								
Age at Day 1 of Dialysis ‡								
18–45	19,440	36%	1,914	40%	10,447	37%	10,907	36%
46–55	17,272	32%	1,494	31%	9,067	32%	9,699	32%
56–65	15,165	28%	1,366	28%	8,070	28%	8,461	28%
>65	1,823	3%	65	1%	929	3%	959	3%
Sex ‡								
Male	34,182	64%	2,688	56%	18,030	63%	18,840	63%
Female	19,518	36%	2,151	45%	10,483	37%	11,186	37%
Race ‡								
White	28,274	53%	3,146	65%	15,170	53%	16,250	54%
Black	22,548	42%	1,450	30%	11,842	42%	12,156	41%
Asian	1,640	3%	154	3%	860	3%	934	3%
Other	1,238	2%	89	2%	641	2%	686	2%
Hispanic Ethnicity ‡	12,481	23%	1,039	22%	6,572	23%	6,948	23%
Employment Status ‡								
Unemployed	32,809	61%	2,516	52%	17,204	60%	18,121	60%
Employed	5,729	11%	981	20%	3,224	11%	3,486	12%
Retired-Age	2,479	5%	215	4%	1,295	5%	1,399	5%
Disabled (Short- and Long-term)	10,826	20%	886	18%	5,727	20%	5,985	20%
Other	1,857	4%	241	5%	1,063	4%	1,035	3%

CHARACTERISTIC	Dialysis Modality at Start				Half of Calendar Month at Start			
	HD		PD		First Half		Second Half	
	N	%	N	%	N	%	N	%
Primary Cause of ESRD ‡								
Diabetes	21,723	41%	1,912	40%	11,485	40%	12,150	41%
Hypertension	18,054	34%	1,428	30%	9,595	34%	9,887	33%
Other	13,923	26%	1,499	31%	7,433	26%	7,989	27%
Prior Nephrology Care ‡ §								
Yes	18,513	35%	3,285	68%	10,478	37%	11,320	38%
No	28,238	53%	1,281	27%	14,442	51%	15,077	50%
Unknown	6,949	13%	273	6%	3,593	13%	3,629	12%
Comorbid Conditions								
Alcohol Dependence ‡	1,969	4%	47	1%	982	3%	1,034	3%
Amputation †	1,109	2%	68	1%	571	2%	606	2%
Atherosclerotic Heart Disease ‡	4,446	8%	305	6%	2,339	8%	2,412	8%
Cancer ‡	1,341	3%	76	2%	688	2%	729	2%
Congestive Heart Failure ‡	11,574	22%	658	14%	5,951	21%	6,281	21%
Chronic Obstructive Pulmondary Disease ‡	1,958	4%	126	3%	1,022	4%	1,062	4%
Prior CVA/TIA *	2,676	5%	208	4%	1,408	5%	1,476	5%
Diabetes: Insulin Dependent	17,102	32%	1,484	31%	9,041	32%	9,545	32%
Diabetes: Without Medications	2,914	5%	248	5%	1,547	5%	1,615	5%
Diabetes: Oral Medications *	5,845	11%	477	10%	3,049	11%	3,273	11%
Diabetes: Retinopathy ‡	4,277	8%	470	10%	2,286	8%	2,461	8%
Drug Dependence ‡	2,336	4%	48	1%	1,192	4%	1,192	4%
Hypertension ‡	47,192	88%	4,347	90%	25,063	88%	26,476	88%
Disability ‡	3,786	7%	120	3%	1,895	7%	2,011	7%
Other Cardiac Disease ‡	4,838	9%	331	7%	2,479	9%	2,690	9%

CHARACTERISTIC	Dialysis Modality at Start				Half of Calendar Month at Start			
	HD		PD		First Half		Second Half	
	N	%	N	%	N	%	N	%
Peripheral Vascular Disease ‡	3,195	6%	216	5%	1,693	6%	1,718	6%
Smoker ‡	6,028	11%	458	10%	3,157	11%	3,329	11%
BMI, kg/m2 ‡								
<18.5	1,770	3%	96	2%	902	3%	964	3%
18.5 – <30	31,364	59%	2,712	57%	16,549	59%	17,527	59%
30 – <40	14,838	28%	1,577	33%	8,029	28%	8,386	28%
40+	5,245	10%	400	8%	2,784	10%	2,861	10%
Albumin, g/dl ‡								
<2.5	9,097	23%	360	10%	4,594	22%	4,863	22%
2.5 – <3	9,236	23%	476	13%	4,743	22%	4,969	22%
3 – <3.5	10,229	26%	867	24%	5,465	26%	5,631	25%
3.5+	11,587	29%	1,989	54%	6,577	31%	6,999	31%
Hemoglobin, g/dl ‡								
<8	9,741	20%	471	11%	5,034	20%	5,178	19%
8 – <9	10,280	21%	691	16%	5,336	21%	5,635	21%
9 – <11	19,630	41%	1,877	44%	10,410	41%	11,097	41%
11+	8,782	18%	1,197	28%	4,857	19%	5,122	19%
Facility								
Hospital-Based ‡ ‖	7,909	15%	374	8%	4,165	15%	4,118	14%
For-Profit Status ‡ §								
For-Profit	42,236	79%	4,094	85%	22,439	79%	23,891	80%
Non-Profit	10,263	19%	681	14%	5,433	19%	5,511	18%
Unknown	1,201	2%	64	1%	641	2%	624	2%

CHARACTERISTIC	Dialysis Modality at Start				Half of Calendar Month at Start			
	HD		PD		First Half		Second Half	
	N	%	N	%	N	%	N	%
Total facility patients per year ‡								
1–50	10,004	19%	667	14%	5,307	19%	5,364	18%
51–100	21,954	41%	1,583	33%	11,385	40%	12,152	41%
101–150	13,307	25%	1,292	27%	7,062	25%	7,537	25%
151+	8,435	16%	1,297	27%	4,759	17%	4,973	17%
Total PD patients per year ‡ ¶								
0	31,086	58%	69	1%	15,359	54%	15,796	53%
1–10	9,355	17%	671	14%	4,930	17%	5,096	17%
11–25	7,687	14%	1,448	30%	4,385	15%	4,750	16%
26–50	4,246	8%	1,463	30%	2,707	10%	3,002	10%
50+	1,326	3%	1,188	25%	1,132	4%	1,382	5%
Patient : nurse Ratio ‡								
>0–10	12,340	23%	990	21%	6,546	23%	6,784	23%
>10–15	17,648	33%	1,857	39%	9,468	34%	10,037	34%
>15–20	13,283	25%	1,145	24%	7,007	25%	7,421	25%
20+	9,550	18%	753	16%	5,019	18%	5,284	18%
Patient : Staff Ratio ‡ §								
>0–4	5,009	10%	191	4%	2,576	9%	2,624	9%
>4–5	7,313	14%	345	7%	3,796	14%	3,862	13%
>5–6	13,410	25%	789	17%	6,941	25%	7,258	25%
>6–7	13,355	25%	918	19%	7,008	25%	7,265	25%
>7–8	8,004	15%	864	18%	4,204	15%	4,664	16%
8+	5,757	11%	1,639	35%	3,527	13%	3,869	13%
Geographic								
Total population in zipcode ‡								

CHARACTERISTIC	Dialysis Modality at Start				Half of Calendar Month at Start			
	HD		PD		First Half		Second Half	
	N	%	N	%	N	%	N	%
0 – <5,000	1,976	4%	198	4%	1,059	4%	1,115	4%
5,000 – <15,000	6,722	13%	661	14%	3,601	13%	3,782	13%
15,000 – <25,000	12,435	23%	1,263	26%	6,655	23%	7,043	24%
25,000 – <50,000	24,996	47%	2,285	47%	13,236	47%	14,045	47%
50,000+	7,512	14%	428	9%	3,930	14%	4,010	13%
Median income of zipcode ($) ‡								
0 – <25,000	3,850	7%	369	8%	2,065	7%	2,154	7%
25,000 – <50,000	32,845	62%	2,733	57%	17,365	61%	18,213	61%
50,000 – <75,000	12,883	24%	1,370	29%	6,904	24%	7,349	25%
75,000 – <100,000	2,976	6%	252	5%	1,578	6%	1,650	6%
100,000+	779	2%	75	2%	407	1%	447	2%
Portion of zipcode below poverty line ‡								
0 – <15	16,979	32%	1,768	37%	9,085	32%	9,662	32%
15 – <25	18,930	35%	1,640	34%	10,109	36%	10,461	35%
25 +	17,540	33%	1,396	29%	9,174	32%	9,762	33%
Portion of zipcode unemployed (%) ‡								
0 – <5	13,946	26%	1,528	32%	7,489	26%	7,985	27%
5 – <10	33,894	63%	2,917	60%	17,974	63%	18,837	63%
10 – <15	5,343	10%	314	7%	2,761	10%	2,896	10%
15 +	420	1%	71	2%	236	1%	255	1%
Portion of zipcode without high school diploma ‡								
0 – 15	22,282	42%	2,579	53%	12,085	43%	12,776	43%
15 – 30	24,137	45%	1,877	39%	12,657	45%	13,357	45%
30 +	7,184	13%	374	8%	3,718	13%	3,840	13%

CHARACTERISTIC	Dialysis Modality at Start				Half of Calendar Month at Start			
	HD		PD		First Half		Second Half	
	N	%	N	%	N	%	N	%
Median rent of zipcode ($) ‡								
0 – <750	22,265	42%	1,957	41%	11,765	42%	12,457	42%
750 – <1000	19,667	37%	1,893	40%	10,539	37%	11,021	37%
1000 – <1250	7,361	14%	662	14%	3,894	14%	4,129	14%
1250 +	3,991	8%	279	6%	2,097	7%	2,173	7%
Urban (versus Rural) ‡	44,915	84%	4,286	89%	23,966	84%	25,235	84%
Temporal								
Year of Dialysis Start ‡								
2005	3,032	6%	190	4%	1,547	5%	1,675	6%
2006	5,744	11%	373	8%	2,995	11%	3,122	10%
2007	5,803	11%	374	8%	3,043	11%	3,134	10%
2008	5,860	11%	407	8%	3,064	11%	3,203	11%
2009	6,098	11%	453	9%	3,219	11%	3,332	11%
2010	6,101	11%	526	11%	3,241	11%	3,386	11%
2011	5,900	11%	653	14%	3,191	11%	3,362	11%
2012	5,221	10%	587	12%	2,789	10%	3,019	10%
2013	5,607	10%	711	15%	3,027	11%	3,291	11%
2014	4,334	8%	565	12%	2,397	8%	2,502	8%

Notes:

Abbreviations: ESKD = end-stage kidney disease, HD = hemodialysis, PD = peritoneal dialysis p-values computed using Pearson's chi-square test

* PD versus HD: $p < 0.05$

† PD versus HD: $p < 0.01$

‡ PD versus HD: $p < 0.001$

§ 1st half versus 2nd half: $p < 0.05$

// 1st half versus 2nd half: $p < 0.01$

¶ 1st half versus 2nd half: $p < 0.001$

Table 2:

Comparing Mortality, Hospitalizations, and Spending in Patients Starting with Hemodialysis and Peritoneal Dialysis (Instrumental Variable Regression)

OUTCOME	Traditional Regression			Instrumental Variable Regression		
	HD	**PD**	**Difference**	**HD**	**PD**	**Difference**
Mortality *						
Months 1–12	8.2%	6.3%	−1.9%	8.1%	7.2%	−0.9%
	(8.0%, 8.4%)	(5.6%, 6.9%)	(−2.6%, −1.2%)	(7.8%, 8.4%)	(5.2%, 8.8%)	(−3.3%, 0.8%)
# Hospitalizations / Patient †						
Months 7–12	0.70	0.72	0.02	0.71	0.66	−0.05
	(0.69, 0.71)	(0.68, 0.76)	(−0.02, 0.07)	(0.69, 0.72)	(0.52, 0.77)	(−0.20, 0.07)
Total Spending ($) / Patient (Log Model) †						
Months 7–12	$13,847	$16,508	$2,661	$14,208	$13,506	−$702
	($13,594, $14,309)	($15,615, $17,904)	($1,527, $4,110)	($13,838, $14,932)	($10,987, $16,906)	(−$4,004, $2,909)
Inpatient Spending ($) / Patient (Log Model) †						
Months 7–12	$47	$58	$11	$49	$35	−$14
	($45, $51)	($50, $70)	($1, $23)	($47, $57)	($25, $56)	(−$31, $7)
Dialysis Spending ($) / Patient (Log Model) †						
Months 7–12	$6,990	$8,623	$1,632	$7,032	$8,221	$1,189
	($6,857, $7,236)	($8,104, $9,439)	($1,027, $2,452)	($6,862, $7,406)	($6,582, $10,282)	(−$822, $3,418)

Notes: We show marginal effects when conditioning the population on starting with hemodialysis and peritoneal dialysis. The difference is the *absolute* change in estimated outcomes between modalities. All estimates adjusted for patient, facility, geographic, and temporal characteristics. Multiple imputation (10 imputations nested within each bootstrap sample) conducted for missing covariates.

Abbreviations: HD = hemodialysis, PD = peritoneal dialysis

* Models estimated in the entire uninsured population irrespective of Medicare coverage. The traditional regression used logistic regression; the instrumental variable regression used probit regression for the first-stage and ordinary least squares for the second-stage with generalized residuals from the first-stage. 95% confidence intervals for both models estimated using non-parametric bootstrap.

† Models estimated in uninsured patients with Medicare coverage months 7–12 while alive. The traditional regression used ordinary least squares; the instrumental variable regression used probit regression for the first-stage and ordinary least squares for the second-stage with generalized residuals from the first-stage. 95% confidence intervals for the standard model estimated using the delta method and for the instrumental variable model using non-parametric bootstrap.