Labor Supply Responses to Government Provided Health Insurance: Evidence from Kidney Transplant Patients

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Abstract: I use data from the United States Renal Data Systems (USRDS) to estimate the labor supply effects of publicly provided health insurance. Between 1993 and 1995 Medicare increased the coverage of immunosuppression medication for kidney transplant recipients from one year post transplant to three years post transplant. This policy change provides an exogenous source of variation in publicly provided health insurance coverage that allows for empirical estimation of the program's labor supply effects. I apply a difference-in-differences methodology to this natural experiment to estimate the effects of this increased coverage on labor supply. This study adds further insight into the behavioral incentives created by publicly provided insurance coverage. Results indicate that a 10 percent increase in the value of the insurance benefit led to a 1 to 3 percentage point decline in labor force participation. These estimated semi-elasticities could then be used to estimate the possible labor supply responses to the introduction of a publicly funded health insurance plan that might cover the millions of uninsured Americans.

JEL Codes: I1, I3, J3

I. Introduction[1]

I use data from the United States Renal Data Systems (USRDS) to estimate the labor supply effects of publicly provided health insurance. Between 1993 and 1995 Medicare increased the coverage of immunosuppression medication for kidney transplant recipients from one year post transplant to three years post transplant. This policy change provides an exogenous source of variation in publicly provided health insurance coverage that allows for empirical estimation of the program's labor supply effects. I apply a difference-in-differences methodology to this natural experiment to estimate the effects of the increased coverage on labor supply. This study adds further insight into the behavioral incentives created by publicly provided insurance coverage.

The estimated semi-elasticities add insight into the possible labor supply responses and economics effects of the introduction of a large scale publicly funded health insurance plan that might insure the millions of otherwise uninsured Americans. There are many factors to consider when assessing the potential economic effects of a large scale public insurance expansion. While there would be income effects associated with the increases in insurance coverage that many would experience, these increases would somehow need to be funded with tax revenues. Therefore, the balanced budget incidence of the program expansions or general equilibrium effects that would also be present are other complicating factors that a comprehensive study of the subject would require. However, given that we have not observed, in the United States anyway, a policy change of the magnitude of a large scale public insurance expansion, we cannot directly estimate the effects the program would have.

[1] I would like to thank Professor Woodward, Professor Baughman, and Professor Conway for their valuable input and the USRDS and UNOS for their data.

Although we cannot directly estimate the potential economic effects of a large scale public insurance expansion, we can use existing policy changes to try to isolate the potential effects separately. In this paper, I attempt to study the potential income effects on labor force participation that such a policy change would cause.

The case of Medicare's increased medication coverage for kidney transplant recipients resembles the potential case of a large scale public health plan and avoids certain empirical weaknesses associated with recent studies of the mid 1990's Medicaid expansion. Every individual with end stage renal disease (ESRD) is eligible for Medicare coverage, and the immunosuppresion medications are critical to the long term success of kidney transplant patient outcomes. Thus, examining kidney transplant patients circumvents the problems of less than 100 percent take up rates among those without other forms of insurance and the endogenous nature of selection into other publicly provided health insurance programs. Recent studies of the behavioral effects associated with Medicaid expansions focus mainly on low income, single women with children. Focusing on individuals who receive kidney transplants provides a more representative cross section of the population, therefore the estimated effects should be more representative of what would occur if the United States introduced a large scale public insurance expansion[2].

Results indicate that the increase in Medicare's coverage of immunosuppresion medication was associated with significant decreases in labor force participation among low income patients and smaller decreases among high income patients. I find that the 10 percent increase in the value of Medicare's coverage of immunosuppression medications led to a 1 to 3 percentage point decline in labor force participation. Therefore, the potential costs and economic effects of a national publicly provided health insurance plan or a new

[2] I will discuss in more detail later the generalizability of the results from this population.

public program to insure the uninsured could extend far beyond the explicit dollar costs of the programs.[3]

Section II of the paper briefly reviews the relevant literature. Section III outlines the conceptual framework and methodology, section IV describes the data, and section V provides estimation results and policy implications. Section VI discusses the results. Section VII concludes and provides possible extensions for future research.

II. Brief Literature Review

A. Labor Supply Effects

There is an existing body of literature that examines the labor supply responses to health insurance provision. Moffitt and Wolfe (1992) examine the effect of Medicaid coverage on the labor supply of female heads of family. The authors find that Medicaid coverage has a negative and significant effect on female labor supply. Winkler (1990) estimates that a 10 percent increase in Medicaid coverage would lead to a 0.9 to 1.3 percentage point decline in labor force participation among female heads of household. Buchmueller and Valletta (1999) study the effect of employer provided health insurance coverage on female labor supply. Results from the study indicate that married women without spousal coverage work longer hours than women with spousal coverage.

Gruber and Hanratty (1995) examine the employment effects of Canada's introduction of a national health insurance plan. They find that there were no disemployment effects resulting from the introduction of the plan. Although this result

[3] Although economic theory predicts that the increases in Medicare's coverage of the immuno-suppression medications should cause decreases in labor force participation, one of the reasons behind increasing the coverage was to promote better outcomes among transplant recipients so they will live longer and therefore increase labor supply (and pay more taxes). While the increased coverage could promote better health that could lead to more work, the labor force participation discouraging income effects are still present and need to be considered.

goes against the conventional wisdom that government provided health insurance causes decreases in labor supply, the structure of the plan created labor supply promoting substitution effects that offset the income effects that should act to decrease labor supply. That is, the wage increases that resulted from the removal of employer sponsored health insurance were large enough to offset the income effects of government provided health insurance. Although shifting the financing from the employer to the government does not necessarily provide the individual with an increase in the total amount of compensation (wages and benefits), if individuals value an additional dollar in wages more than a dollar of health insurance, then a dollar for dollar replacement of wages for insurance could generate a labor supply promoting substitution effect.

Another potential behavioral effect of publicly provided insurance is the potential for publicly provided insurance to crowd out private insurance. Cutler and Gruber (1996) use data from the Current Population Survey (CPS) for the years 1987 to 1992 in order to estimate the effects of Medicaid expansions on private insurance coverage. The authors estimate that between 30 and 50 percent of the increase in Medicaid coverage resulted from individuals dropping private coverage and enrolling in the Medicaid program.[4] Therefore, this crowd out effect must be taken into account when considering the economic effects of a large scale public health insurance expansion.

<u>B. Kidney Graft Survival and Medicare's Coverage of Immunosuppression Medications</u>

This paper also requires a brief description of the End Stage Renal Disease (ESRD) literature. Woodward et. al. find evidence to suggest that the increase in the duration of Medicare's coverage of immunosuppression medication had a positive impact on the graft survival of low income cadaveric kidney transplant patients. The authors use the same

[4] It should be noted that the Cutler and Gruber estimate is quite a bit larger than other estimates in the literature, but this paper is widely cited and, for the most part, well done.

1993 to 1995 extension of Medicare's immunosuppression coverage as a natural experiment to identify the effects of extending the coverage of immuno-suppression medication on graft survival rates. The authors assign low income individuals to the treatment group (these individuals are the most likely to have been affected by the policy change) and high income individuals to the control group (high income individuals are more likely to have had sufficient income to pay for the immuno-suppression medications or other insurance coverage).

Woodward et. al. use USRDS data for the years 1992 to 1993 and 1995 to 1997 for their before and after approach. Results for the 1992 to 1993 cohort indicate that although there were no significant differences in graft survival rates of high income and low income patients at the end of year 1, the low income group had a higher graft failure rate in the second and third years post transplant after Medicare's immuno-suppression coverage ended. However, Medicare covered 3 years of medications for individuals transplanted between 1995 and 1997. Among this cohort, no significant differences in graft survival rates occurred in the second and third years post transplant. The methodology adopted for this research closely resembles that used in the Woodward et. al paper.

C. Background on ESRD and Kidney Transplantation

According to Danovitch's Handbook of Kidney Transplantation, in 2000 there were 275,000 individuals receiving dialysis for ESRD. This number is expected to reach 520,000 by 2010. Approximately half of the patients on dialysis are over age 65, and the average age of patients receiving kidney transplants has increased over time. 53 percent of patients with ESRD are male and 37 percent are black. In 2004, there were 60,000 patients on the kidney transplant waiting list. While the number of deceased donor kidneys has remained constant around 9,000 per year, the number of living donor transplants has been increasing.

Despite the critical shortage of donor kidneys, some patients with ESRD are fortunate enough to receive kidney transplants. Transplantation rates are lower for older individuals due to the risks associated with transplanting older patients, and transplantation rates are lower among blacks due to the availability of fewer donor organs. The first year following a kidney transplant is the most costly. Costs during the first post transplant year are $100,000, but after the first year the cost falls to $10,000 per year (this is the cost of the immuno-suppression medication).

Immunosuppression medications are necessary for the survival of the transplanted kidney. Schweizer et. al. (1990) discuss the factors that lead to transplant failure. The authors conclude that the leading cause of organ failure is non-compliance regarding post transplant medication. Results indicate that 91 percent of the non-compliant transplant recipients experience graft failure. The development of improved immunosuppression medications has led to improved graft survival rates. During the 1970's, graft survival rates at one year post transplant were only 50 percent. By the 1990's, graft survival rates at one year increased to 90 percent. Danovitch attributes this increase to the development of better immunosupression medications.

Transplanted patients that do not experience any surgical complications leave the hospital within one week. Almost all living donor recipients experience "excellent graft function" in the first week compared to 30 to 50 percent for deceased donor recipients.

First year post transplant mortality is about 5 percent, with most mortality occurring in the first three months[5].

III. Conceptual Framework and Methodology

Given that the immunosuppression medications are important for the survival of a transplanted kidney, it is possible to view the increase in Medicare's coverage as an increase in non-labor income[6]. We can analyze the labor supply incentives created by policy change within the standard labor-leisure model. Figure 1 depicts graphically the conceptual model that compares the situation between years 1 and 3 post transplant before and after the policy change. Before the policy change, in order to consume C* (which includes the immuno-suppresion medication), the individual would need to work some positive number of hours. However, after the policy change, C* is possible at zero hours of work. The individual represented in figure 1 would drop out of the labor force. Note however, that individuals with stronger preferences for work would not necessarily stop working all together. However, the scenario depicted in figure 1 is the hypothesis tested in this study. The policy change generates a pure income effect that should act to discourage labor force participation.

The policy's effect on hours of work conditioned on participation is less straightforward. The USRDS data do not contain an hours of work variable, but they do contain a variable for whether the individual works full time or part time. If hours data were available, the model would predict a reduction in hours worked as a result of the increased Medicare coverage, but if the marginal worker that drops out of the labor force

[5] This implies that the really sick newly transplanted patients will not contaminate the sample used in this study.
[6] This assumption is valid because most individuals without insurance are likely to be inframarginal. That is, even if the increase in coverage were given as a cash transfer, the amount of medication consumed would be equivalent.

had previously been employed part time, then it is reasonable to expect an increase in the percentage of individuals employed full time after the policy's implementation. This prediction assumes that full time workers are less likely to become part time workers than part time workers are to stop working.

The next issue is to determine which individuals are "treated" by the policy and which individuals are not. A logical approach is to consider those who use Medicare's coverage the treatment group, while those who have private insurance and do not use Medicare's coverage represent the control group. Unfortunately, the USRDS data set only contains claims data after 1995. Therefore, I am forced to use an alternative definition of the treatment and control groups. Given that about half of those eligible for the Medicare's coverage have other insurance, it seems reasonable to assume that individuals with other coverage are more likely to he higher income individuals. In fact, roughly 65 percent of low income individuals transplanted between 1995 and 2001 report Medicare as the primary payer for their care compared to only 35 percent of high income individuals. As Lewbel (2006) describes, given that I have the necessary information for a different time period, it is possible to use these numbers from the 1995 to 2001 data to weight the estimated policy effects found when using the imperfect treatment-control definitions. This method will be described in greater detail in the results section.

Figure 2 illustrates the nature of the policy change. Between 1990 and 1993, the duration of Medicare's coverage was one year post transplant. After 1995, the duration of coverage was three years. Therefore, the policy change lends itself nicely to the natural experiment "difference-in-differences" methodology used in Eissa (1995). Ideally, I would like to have a treatment group that was affected by the policy change and an equivalent control group that was not, but due to data limitations, I am forced to use a less than perfect treatment/control definition. It should be noted, however, that any mis-assignment of individuals into either the treatment or control group will bias the differences-in-differences coefficient towards zero, so if there is bias, my results will underestimate the true policy effect. However, I will apply the methodology found in Lewbel (2006) to provide unbiased estimates of the policy effect. In this setting, the difference-in-differences regression is identified by the assumption that non-health insurance factors causing labor supply changes by income group are not correlated with the timing of the Medicare coverage extension.

If the increased Medicare coverage affected the labor force participation of kidney transplant recipients, then we should observe a delayed re-entry into the labor force in the 1995 to 1997 period relative to the 1990 to 1992 period. If high income individuals are a valid control group, then we should observe a bigger effect among low income patients. In order to make these determinations, I estimate a Cox proportional hazard model. An advantage of the Cox proportional hazard model is that this function does not have to be estimated. Estimation of the model yields estimates of hazard ratios, which give the effect of model covariates on the timing of an event; in this case, the timing of re-entry into the labor force.

While the Cox proportional hazard model will be useful in determining whether the increase in Medicare's coverage of immunosuppression medications had a measurable effect on labor force participation, the results of these exercises will not be as useful in determining the actual magnitude of the policy effect. The time period of concern in this study is the time between one year and three years after transplantation. In order to focus on this post transplant time window, I use a difference-in-differences methodology.

The equation for the difference-in-differences estimator has the form:

$$P(LFP=1)=F(\alpha+\beta X+\gamma_1 LowY+\gamma_2 Post+\gamma_3(LowY*Post)) \quad (3)$$

In equation (1), LowY indicates that the individual is in the low income treatment group. Post denotes the post 1995 period and X is a vector of demographic controls. The γ_3 coefficient gives the difference-in-differences estimate designed to isolate the policy effect.

Conceptually, the change in the treatment group's labor force participation rate between the pre and post period contains the effect of the policy change (γ_3), the effect of anything else that might have changed between the pre and post periods that could affect labor force participation (γ_2), and any time invariant characteristics specific to low income individuals (γ_1). Therefore, in order to ascertain the effect of the policy, we subtract the change in labor force participation experienced by a control group that was not subjected to the policy change but was subjected to the same "anything else" that may have affected the treatment group's labor force participation rate. Subtracting the control group's difference from the treatment group's difference isolates the effect of the policy change, γ_3.

IV. Data

I use data from the United States Renal Data System (USRDS) for the years 1990 to 1997. The "before" period covers the years 1990, 1991, and 1992. The "after" period covers the years 1995, 1996, and 1997. 1993 and 1994 were omitted because they were years in which the policy was phased in. The unit of observation in this study is the patient follow-up visit[7]. Because the unit of observation is the patient follow-up visit, individuals appear multiple times in the data. However, due to both missing values of key variables and also missing follow up records, some individuals are represented only once.

[7] These observations are, of course, not independent. However, because some individuals appear only once, I could not include an individual fixed effect to remedy the problem. To determine the consequences of this non-independence, I restrict the sample to follow up visits at year 2. This point is discussed in more detail in the results section

Table 1 contains variable descriptions. Individuals are coded as participating in the labor force if they are listed as working full time, working part time, or seeking employment. Individuals listed as "retired" or "student" were dropped from the sample. Individuals are assigned to either the low income treatment group or the high income control group based on the median income of their residential zip code taken from the 2000 US Census.

The USRDS dataset does not contain a variable for the individuals' incomes. However, using the zip code level income information is actually preferable in this case. If I had used individual level income information to assign individuals into the treatment or control group, the individuals' employment statuses could determine into which group they were assigned[8]. Therefore, the zip code level median incomes provide a more desirable, exogenous measure of individual incomes.

The low income group consists of individuals in the first, second, and third income deciles. In alternative specifications used for robustness/sensitivity checks I restrict the low income group to the first and second zip code median income deciles. The high income group consists of individuals in the ninth and tenth income deciles. In alternative specifications designed to be sensitivity/robustness checks, I restrict the high income group to the tenth income decile. Given that the individual level variation in incomes decreases as we move into the tails of the zip code median income distribution, observations located in the tails provide a more accurate measure of predicted income than observations in the middle of the distribution. For this reason, I do not consider individuals in income deciles four through eight. The included deciles were chosen to provide somewhat equal numbers

[8] A basic requirement of the difference-in-differences strategy is that the treatment and control groups are stable and exogenously determined. Using zip code level incomes insures that individuals are not "group jumping" through changes in employment status, thus providing stable groups that are not endogenously determined *by* the individuals' employment statuses.

of observations in the low and high income groups while remaining in the tails of the zip code median income distribution.

Table 2 gives descriptive statistics for newly transplanted patients by year. Roughly 40 percent of individuals in the sample are female, and the percent of the sample that is black increased from 20 percent to 22.2 percent. The average age of transplanted patients increased between 1990 and 1997 from 39.75 to 43.50. Table 2 indicates that the population of transplant recipients got bigger, older, and more heavily black during the sample period. Variable columns in bold indicates that the differences between the 1990 and 1997 values were statistically significant ($p<0.001$). These figures are consistent with those given in Janovitch's <u>Handbook of Kidney Transplantation.</u>

Tables 3 and 4 give descriptive statistics of the treatment and control groups, respectively, for patient follow-up visits recorded at six months, one year, two years, three years, and four years post transplant. Although the relevant post transplant period for Medicare's coverage of immuno-suppression medications is years one to three, I include six month and four year visits to capture any effects of labor market rigidities that could force individuals to begin seeking employment before the end of the first post transplant year or to have difficulty finding employment immediately after the three year period ended. In the main model specification I restrict the analysis to follow-up visits recorded at one year, two years, and three years post transplant. In alternate specifications I vary the income deciles used for treatment/control definitions as well as the number of follow up periods considered. Variable rows in bold indicate statistically significant differences between to high income and low income cohorts ($p<0.001$).

Figure 3 depicts graphically the labor force participation rates of the treatment and control groups. Although both groups experienced a decline in labor force participation rates between 1993 and 1995, the drop was more drastic for the low income control group. Participation rates fell from roughly 80 percent before the policy change to between 60 and 65 percent after the policy change. Figure 4 gives the same information for the restricted sample that includes income deciles one, two, and ten. Labor force participation rates from the restricted sample are very similar to labor force participation from the full sample.

V. Estimation Results

A. Survival Estimates.

To estimate the Cox proportional hazard model, I use the sample criteria described in section IV. The data yielded observations for 25,033 individuals. Results from the Cox proportional hazard model are found in Table 5. Hazard ratio estimates indicate that being a low income patient in the "post" period reduces the probability of re-entry into the labor force by roughly 15 percent[9]. This hazard ratio estimate is not sensitive to covariates included in the model. Being female, black, or low income in either period also reduces the probability of re-entry into the labor force. Although the results of the Cox proportional hazard model provide valuable insights into the effect of the Medicare policy change on employment, they are not particularly useful in determining the actual magnitude of the policy effect. For example, due to missing data values, observing that an individual is working at time t does not guarantee that he/she re-entered the labor force at time t. While this problem is unlikely to be correlated with either of the income cohorts or time periods, it reduces the reliability and policy relevance of the hazard model estimates. Another issue is that the relevant time period of the policy change was between one and

[9] Hazard ratios less than 1 indicate a lower probability of event failure (defined in this study as re-entering the labor force). Ratios greater than 1 indicate a higher probability of event failure.

three years after transplantation. In order to focus only on that time period, I use a difference-in-differences methodology that considers only that time frame.

B. Difference-in-Difference Estimates

Tables 6 through 9 report results of the simple difference-in-differences calculations using four different subsamples. The first table represents the main sample, while the following three represent the samples used for sensitivity/robustness checks. Results from the main sample indicate a 10.23 relative percentage point decrease in labor force participation among low income transplant patients. Results from the three alternative specifications range from 8.57 to 10.72 relative percentage point decreases. These percentage point declines line up with those found in Winkler (1990). Measuring the value of the coverage increase over the midpoint of the values before and after the policy change, the percent increase in coverage is 100 percent. Thus, these simple difference-in-difference estimates indicate that a 10 percent increase in Medicare's coverage results in a 0.8 to 1.1 percentage point decrease in labor force participation. Recognizing that the observations are not independent, since individuals appear more than once in the data, I estimate the difference-in-difference models for only year two follow up visits. Results from these models were qualitatively similar to results from the full sample. This suggests that the consequences of the non-independence of the observations are minimal.

I conduct a similar difference-in-differences exercise with the full time/ part time measure of work intensity. Before the policy's introduction, 21 percent of high income workers were employed part time. This number fell to 8 percent during the post 1995 period. Before the policy's introduction, 34 percent of low income workers were employed part time, compared to 14 percent after 1995. These results suggest a net increase in the proportion of full time workers of 7 percentage points for the low income treatment group.

These results are consistent with the explanation that the marginal worker who exits the labor force is more likely to be employed part time.

Table 10 contains logit estimation results from the main sample. The first column contains only the simple difference-in-differences calculation. The second column includes controls for race and gender. Columns three and four include age controls. Column three includes a higher order age polynomial and column four contains categorical age dummies[10]. As expected, the coefficients on the female and black dummies are negative and statistically significant. The age dummies are significantly different from the omitted "adult" category in the expected way. That is, the probability of participation in the labor force declines with age. As with any non-linear model, the coefficient estimates from the logit models do not represent marginal effects. Table 10 gives marginal effects[11] (more specifically, the effect of a discrete change in the variable from a 0 to a 1) for each of the variables. To complicate matters further, the marginal effect of the interaction term is not calculated correctly by most statistical software packages. Therefore, I compute interaction effect (or policy effect) of the γ_3 coefficient using the method outlined in Ai and Norton (2003), where:

$$\frac{\Delta^2 E(Y \mid (X, LowY, Post)}{\Delta\gamma_1 \Delta\gamma_2} = \gamma_3 F'(\gamma_1 LowY + \gamma_2 Post + \gamma_3 LowY * Post + X\beta) + (\gamma_1 + \gamma_3 Post)(\gamma_2 + \gamma_3 LowY) F''(\gamma_1 LowY + \gamma_2 Post + \gamma_3 LowY * Post + X\beta) \quad (4)$$

[10] The impact of age on labor force participation in this setting is unclear. I include the quadratic and cubic terms because I did not want to impose a linear or quadratic relationship on the age variable.

[11] For simplicity, I use the term "marginal effect" even though this term is not entirely appropriate when discussing dichotomous variables. The "marginal effect" of a dummy variable refers to the change in the expected probability that the dependent variables takes on a value of 1 when the independent variable changes from 0 to 1.

The marginal effects at the sample mean imply that low income patients in the "post" period were approximately 9 to 10 percentage points less likely to participate in the labor force. These estimates are slightly lower than the estimates from the simple difference-in-differences estimates[12]. Given that the estimated interaction, or policy, effects vary with other model covariates, I compare the estimated policy effects (sometimes referred to as "average treatment effects", or ATE's) by race, age, and gender. The results of this exercise are found in Table 11. The policy effect estimate for men is -0.095 compared to -0.102 for women. This difference is significant at $p<0.001$. The policy effect estimate for whites is -0.097, compared to -0.102 for blacks. These differences are also significant at $p<0.001$. Breaking down the sample by both race and gender produces results consistent with the conventional wisdom of labor supply elasticities. The results suggest that black women are the most responsive (-0.105), while white men are the least responsive (-0.094). This difference is significant at $p<0.001$. The policy effect for black men and white women is identical at -0101. Comparing the different categorical age groups, the policy effect seems to be a non linear function of age. The teenage group is more responsive than the young adult group, with policy effects of -0.095 and -0.083, respectively. Beyond the young adult group, the policy effect increases with age. The estimated policy effects for both the middle age and elderly groups were -0.110. The overall differences among all groups were significant at $p<0.001$.

Results from the robustness/sensitivity check samples range from 9 to 11 relative percentage point decreases in labor force participation for low income individuals in the "post" period. However, as mentioned previously, there is some mis-assignment of

[12] For comparison purposes, the interaction effects estimated incorrectly by software packages are reported below the correct interaction effects in Table 8. Failure to correctly compute the interaction effects would significantly underestimate the true policy effect. Of 72 articles published between 1980 and 1999 that used a D-in-D methodology, Ai and Norton found that none of them computed the interaction effect correctly. As shown here, correct computation can make a significant difference in the resulting estimates.

individuals into the treatment and control groups. Lewbel (2006) shows that in this type of situation, an unbiased estimate can be obtained by dividing the biased estimate of the policy effect by the sum of the proportion of truly treated individuals in the treatment and the proportion of unaffected individuals in the control group, less 1. For example, using the biased estimate of a 10 percentage point relative decrease, I divide 10 by ((.65+.7)-1) to obtain an unbiased estimated policy effect of 28.57. This result suggests that a 10 percent increase in coverage led to a 2.8 percentage point relative decrease in labor force participation. Table 12 reports the full set of estimates adjusted in this way. These estimates range from 0.26 for white men to 0.30 for black females. The overall results indicate that a 10 percent increase in the value of insurance benefits led to a 1 to 3 relative percentage point decline in labor force participation.

The elephant in the room, so to speak, that I have ignored up to this point is the generalizability of these results. While the end stage renal disease population as a whole is pretty sickly, I focus only on transplanted individuals. Patients fortunate enough to receive transplants are those individuals whose quality of life would improve the most by receiving a transplant. Thus, the current kidney shortage insures that the sickliest patients are not receiving transplants and contaminating my sample. Further, the sample contains only individuals whose kidneys are still functioning at the time the follow ups were recorded. The USRDS follow up file contains a “functional status” variable, where possible responses include “no activity limitations”, “performs ADL’s with some assistance”, and “performs ADL’s with total assistance”. Among the sample used in the “post” period of this study, 90 percent of the low income patients report “no activity limitations”, while 92 percent of the high income patients report “no activity limitations”. Thus, while the generalizability of the results might be called into question, further investigation reveals that these individuals might be more "normal" than a casual observer would think.

Even if we believe that these patients are not representative of the general population, it is possible to describe the results of this study as a defensible upper or lower bound based on how the response of these patients would compare to the response from the general population. In a future version of this paper, I plan to explore this issue, although doing so will require exploring the valuation of health insurance benefits, which is easy for the kidney transplant patients because take up among the uninsured is likely to be 100 percent and the medications are important to their continued quality of life, but will be more difficult to ascertain for the general population. I would argue that because some of these patients might be working specifically to pay for their medications that the results found in this study are actually an upper bound estimate of the labor supply responses to government provided health insurance.

VII. Conclusion

Using data from the USRDS, I find evidence to suggest that a 10 percent increase in health insurance coverage among kidney transplant recipients led to a 1 to 3 percent relative decline in labor force participation. These results, if applied on a national scale in response to a potential national health insurance plan, are certainly non-trivial in terms of their economic impact. A key argument against the introduction of a national health insurance plan in the United States is the potential for labor supply discouraging effects. In this paper I find evidence to suggest that a program specifically targeted at uninsured individuals could produce such effects. I do not, however, explore the case of a full blown national health insurance program. A national health insurance program would have balanced budget and general equilibrium effects that would act to partially or fully offset the labor supply disincentives of publicly provided insurance. These effects are not likely to be found when considering a plan targeted at low income, uninsured individuals.

The labor supply reducing income effects associated with the provision of public insurance represent one piece of the puzzle that needs to be considered when examining the potential effects of the introduction of a large scale public health insurance plan. Although we have not observed the introduction of such a plan in the United States in recent times and therefore cannot estimate in a reduced form way the potential economic effects of such a program, we may have enough variation in other policies to piece together the possible economic effects in a more structural way. Using the variation in Medicare's coverage of immuno-suppression medication for kidney transplant patients is an example of this type of study.

Possible extensions to this research include investigating the extent to which the decline in labor force participation among low income kidney transplant recipients may have contributed to the improved graft survival rates found in Woodward et.al. (2000). This health production angle follows Ruhm's (2000) work investigating the impact of unemployment increases on health outcome measures. In addition, state Medicaid rules regarding ESRD may provide useful variation to study other behavioral effects related to public health insurance provision.

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Figure 1: The labor supply effect of a transfer

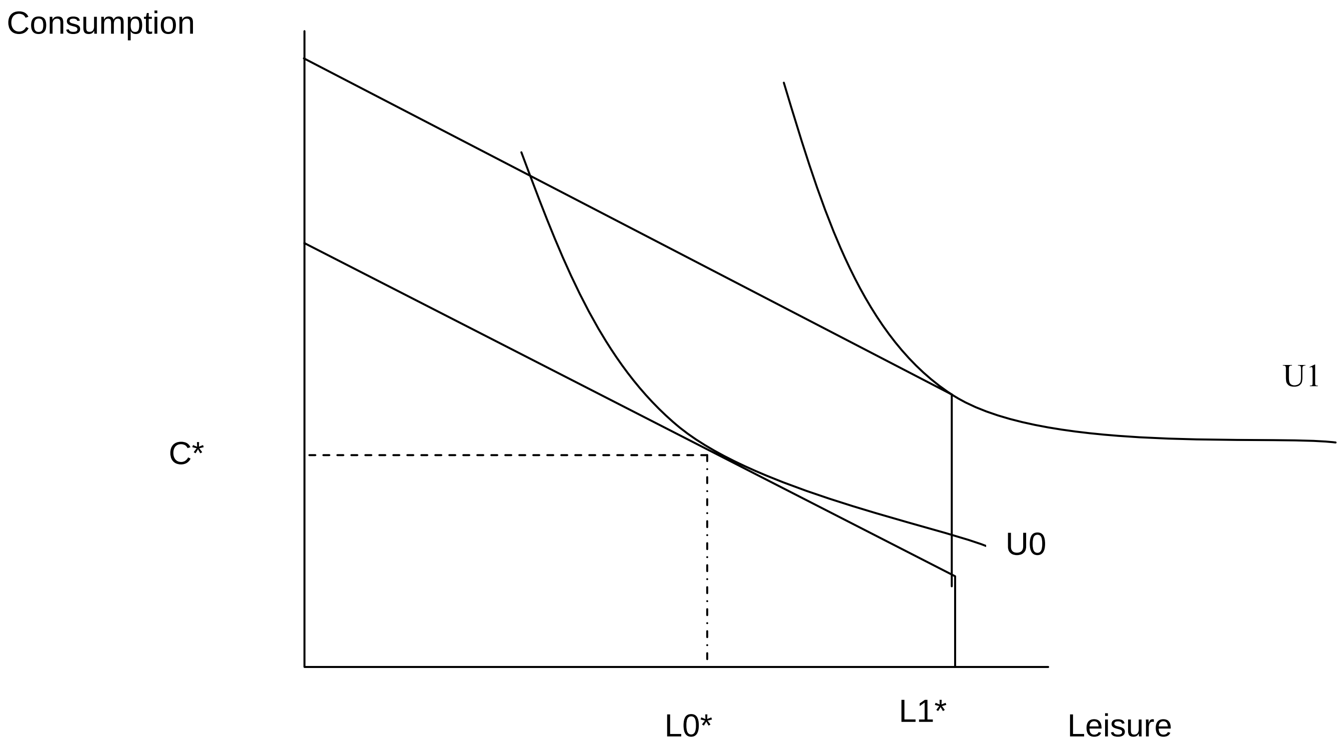


Figure 2: Months of Medicare Immuno-Suppression Coverage

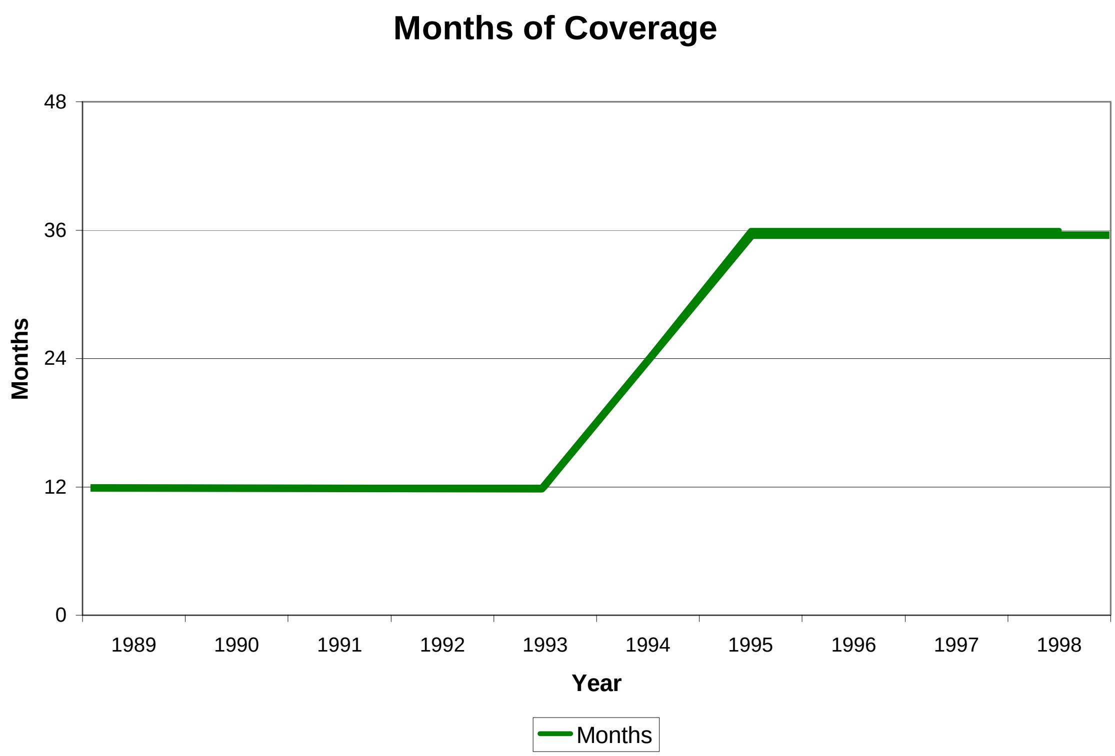
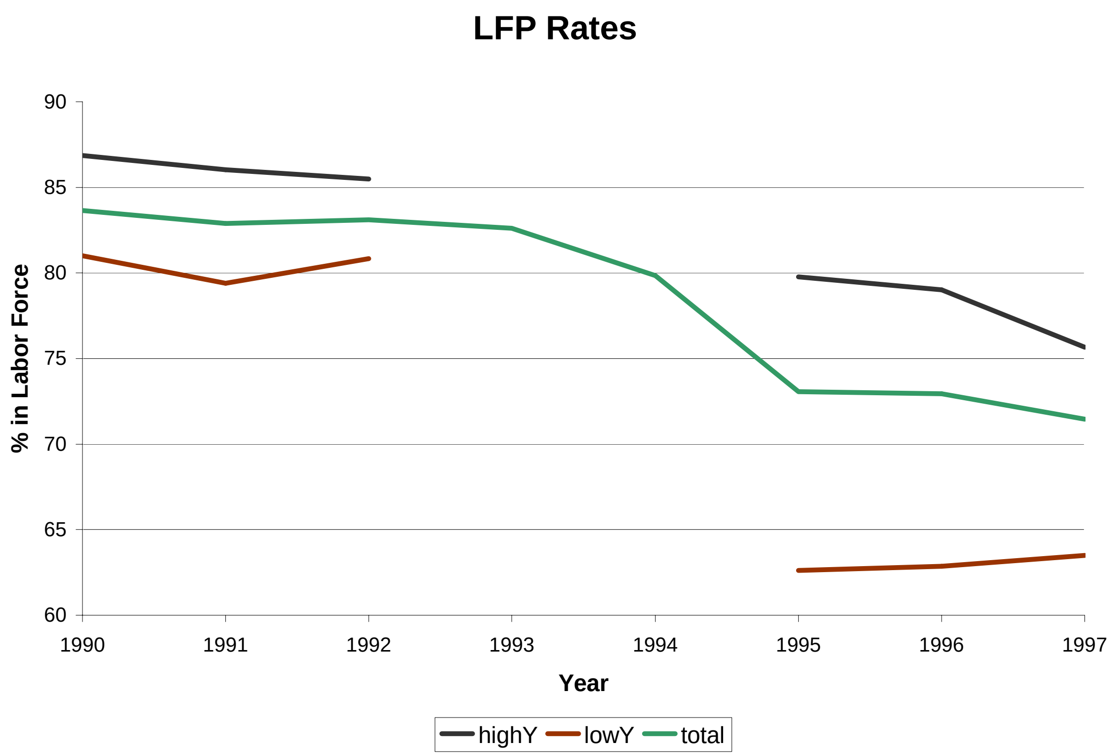
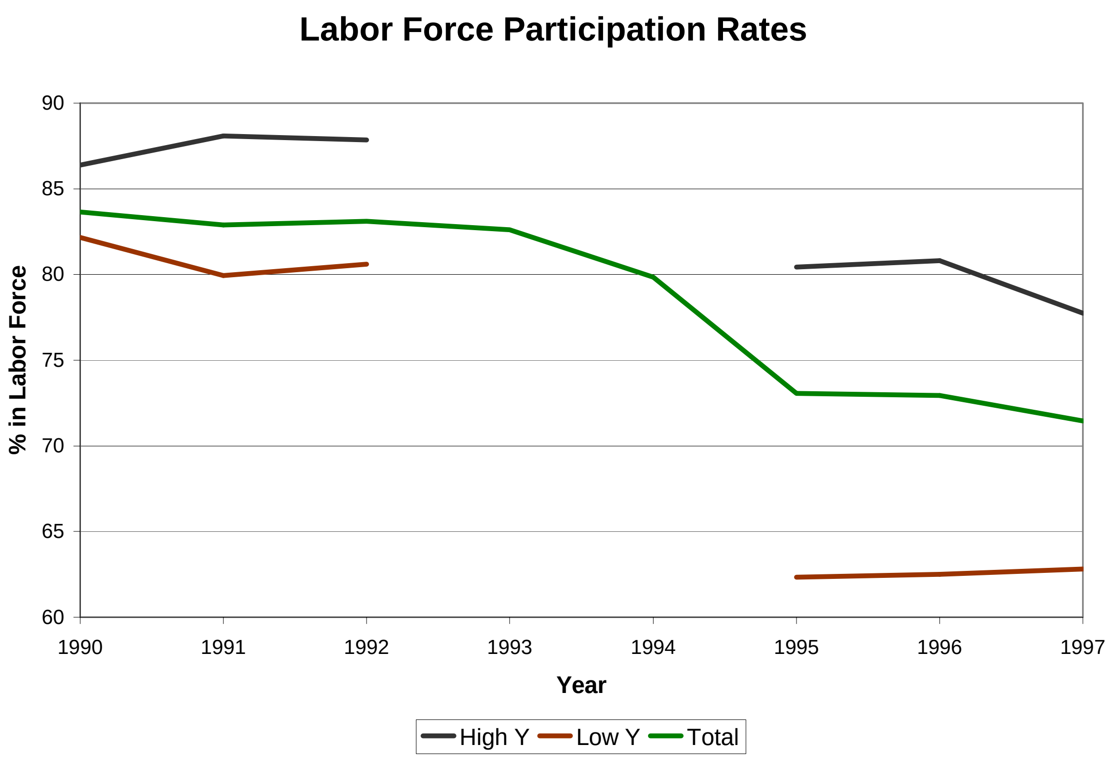


Figure 3: Labor Force Participation Rates



The high income control group includes individuals in income deciles 9 and 10. The low income treatment group includes individuals in income deciles 1, 2, and 3. Observations were recorded at 6 months, 1 year, 2 years, 3 years, and 4 years post transplant.

Figure 4: Labor Force Participation Rates



The high income control group includes individuals in income decile 10. The low income treatment group includes individuals in income deciles 1 and 2. Observations were recorded at 6 months, 1 year, 2 years, 3 years, and 4 years post transplant.

Table 1: Variable Definitions

Variable	Definition
LFP	LFP = 1 if the individual is coded as working full time, working part time, or seeking employment
Female	=1 if the individuals is female
Black	= 1 if the individual is black
Low Income (LowY)	Treatment group (median income deciles 1,2, and 3)
High Income	Control Group (median income deciles 9 and 10)
Before	Years 1990, 1991, and 1992
After (Post)	Years 1995, 1996, 1997
Age	Patient's age in years
Teen	=1 if the individual is between 16 and 20 years of age
Young Adult	=1 if the individual is between 20 and 30 years of age
Adult	=1 if the individual is between 30 and 45 years of age
Middle Aged	=1 if the individuals is between 45 and 60 years of age
Elderly	=1 if the individual is over age 60
Median Income	Median individual income in the zip code of the individual's residence

Table 2: Characteristics of Newly Transplanted Patients by Year

	% Female	% Black	Age	N
1990	0.402 (0.490)	**0.200** **(0.399)**	**39.75** **(14.17)**	9,922
1991	0.401 (0.490)	**0.198** **(0.399)**	**40.07** **(14.31)**	10,064
1992	0.411 (0.492)	**0.204** **(0.403)**	**40.92** **(14.36**	10,155
1993	0.401 (0.490)	**0.206** **(0.404)**	**41.13** **(14.47)**	11,008
1994	0.398 (0.490)	**0.224** **(0.417)**	**41.60** **(14.37)**	11,346
1995	0.394 (0.489)	**0.217** **(0.412)**	**42.12** **(14.56)**	11,911
1996	0.404 (0.491)	**0.216** **(0.411)**	**42.90** **(14.61)**	12,165
1997	0.402 (0.490)	**0.222** **(0.416)**	**43.50** **(14.56)**	12,493

Sample includes all kidney transplants that occurred in the given year that did not have any missing data values.

Table 3: Treatment Group Characteristics by Year

	1990	**1991**	**1992**	**1995**	**1996**	**1997**
LFP	**0.810 (0.392)**	**0.794 (0.405)**	**0.808 (0.394)**	**0.626 (0.484)**	**0.629 (0.483)**	**0.635 (0.481)**
% female	0.402 (0.491)	0.401 (0.490)	0.404 (0.491)	0.419 (0.494)	0.417 (0.493)	0.411 (0.492)
% Black	**0.423 (0.494)**	**0.384 (0.487)**	**0.385 (0.487)**	**0.368 (0.482)**	**0.382 (0.486)**	**0.373 (0.484)**
Age	**40.13 (12.64)**	**40.28 (12.42)**	**41.57 (12.74)**	**42.02 (12.59)**	**42.90 (12.41)**	**43.10 (12.45)**
Median Income	**$24,671 ($4249)**	**$24,642 ($4,374)**	**$24,787 ($4256)**	**$24,952 ($4229)**	**$24,886 ($4257)**	**$24,888 ($4252)**
N	1,377	2,644	4,064	5,028	5,111	5191

Sample includes follow up visits recorded at 6 months, 1 year, 2 years, 3 years, and 4 years post transplant from individuals in income deciles 1, 2, and 3. Observations with missing values were dropped from the sample.

Table 4: Control Group Characteristics by Year

	1990	1991	1992	1995	1996	1997
LFP	**0.869 (0.338)**	**0.860 (0.347)**	**0.855 (0.352)**	**0.798 (0.401)**	**0.790 (0.407)**	**0.756 (0.429)**
% female	.0435 (0.496)	0.406 (0.491)	0.402 (0.490)	0.419 (0.493)	0.411 (0.492)	0.404 (0.491)
% Black	**0.083 (0.275)**	**0.082 (0.275)**	**0.088 (0.284)**	**0.093 (0.291)**	**0.102 (0.303)**	**0.103 (0.304)**
Age	**39.83 (12.63**	**41.02 (12.67)**	**42.36 (12.55)**	**42.89 (11.98)**	**43.33 (12.05)**	**43.80 (11.96)**
Median Income	**$62,451 ($14,558)**	**$62,526 ($13,822)**	**$62,633 ($14,104)**	**$63,259 ($14,188)**	**$63,094 ($13,839)**	**$63,180 ($14,083)**
N	1635	3355	4962	7113	7651	8048

Sample includes follow up visits recorded at 6 months, 1 year, 2 years, 3 years, and 4 years post transplant from individuals in income deciles 9 and 10. Observations with missing values were dropped from the sample.

Table 5: Results from Cox Proportional Hazard Model (N=25,033)

Variable	Hazard Ratio	Standard Error	z-statistic	p-value
LowY	.9616805	.0206875	-1.82	0.069
Post	0.6160027	0.0111051	-26.88	<0.001
LowY*Post	0.8480146	0.0242263	-5.77	<0.001
Age	1.014746	0.005946	2.5	0.012
Age^2	0.9994495	0.0001632	-3.37	0.001
Age^3	1.000004	1.39E-06	2.71	0.007
Female	0.9188489	0.0130564	-5.96	<0.001
Black	0.9644733	0.0174119	-2	0.045

LR statistic significant at $p<0.001$

Table 6: Simple Difference-in-Differences Calculation (This is the main sample used in multivariate logit estimation)

	Before	After	Difference	Difference in Differences
Low Income	85.86%	64.45%	-21.41 pp	
High Income	90.44%	79.26%	-11.18 pp	**-10.23 pp**

N = 31,804. Includes income deciles 1, 2, and 3 in the low income group and income deciles 9 and 10 in the high income group. Observations were recorded at 1 year, 2 years, and 3 years post transplant.

Table 7: Simple Difference-in-Differences Calculation

	Before	After	Difference	Difference in Differences
Low Income	80.39%	63.00%	-17.39 pp	
High Income	85.89%	78.06%	-7.83 pp	**-9.56 pp**

N = 56,199. Includes income deciles 1, 2, and 3 in the low income group and income deciles 9 and 10 in the high income group. Observations were recorded at 6 months, 1 year, 2 years, 3 years, and 4 years post transplant.

Table 8: Simple Difference-in-Differences Calculation

	Before	After	Difference	Difference in Differences
Low Income	80.31%	62.56%	-18.87 pp	
High Income	87.70%	79.55%	-8.15 pp	**-10.72 pp**

N = 34,841. Includes income deciles 1 and 2 in the low income group and income decile 10 in the high income group. Observations were recorded at 6 months, 1 year, 2 years, 3 years, and 4 years post transplant.

Table 9: Simple Difference-in-Differences Calculation

	Before	After	Difference	Difference in Differences
Low Income	79.69%	64.17%	-15.52 pp	
High Income	87.74%	80.79%	-6.95 pp	**-8.57 pp**

N = 21,699. Includes income deciles 1 and 2 in the low income group and income decile 10 in the high income group. Observations were recorded at 1 year, 2 years, and 3 years post transplant.

Table 11: Estimated Policy Effects by Gender, Race, and Age

Group	Number of Observations	Policy Effect	Std. Deviation
Differences by Gender			
Group	**Number of Observations**	**Policy Effect**	**Std. Deviation**
Men	18,691	-0.095	0.010
Women	13,113	-0.102	0.007
Difference is significant at p<0.001			
Differences by Race			
Group	**Number of Observations**	**Policy Effect**	**Std. Deviation**
Whites	25,005	-0.097	0.009
Blacks	6,799	-0.102	0.007
Difference is significant at p<0.001			
Differences by Race and Gender			
Group	**Number of Observations**	**Policy Effect**	**Std. Deviation**
White Males	14,724	-0.094	0.010
White Females	10,281	-0.101	0.007
Black Males	3,967	-0.101	0.008
Black Females	2,832	-0.105	0.005
Differences between all groups were significant at p<0.001 except for white females compared to black males			
Differences by Age			
Group	**Number of Observations**	**Policy Effect**	**Std. Deviation**
Teen	1,036	-0.095	0.005
Young Adult	4,116	-0.083	0.006
Adult	13,084	-0.094	0.005
Middle Age	10,784	-0.110	0.003
Elderly	2,784	-0.110	0.001
Overall differences were significant at p<0.001			

Table 10: Logistic Regression Results

Variable	Estimate		Estimate		Estimate		Estimate	
Intercept	2.2470***	-	2.3543***	-	6.1495***	-	2.4350***	-
	(0.495)		(0.0512)		(0.4366)		(0.0540)	
Low Income	-0.4435***	-0.0758	-0.3729***	-0.0634	-0.3901***	-0.0649	-0.3840***	-0.0638
	(0.0680)		(0.0690)		(0.694)		(0.0694)	
Post	-0.9062***	-0.1340	-0.9020***	-0.1330	-0.8575***	-0.1243	-0.8641***	-0.1248
	(0.0538)		(0.0538)		(0.0542)		(0.0541)	
LowY*Post	-0.3021***	***-0.1022***	-0.3142***	***-0.0974***	-0.3341***	***-0.0983***	-0.3309***	***-0.0980***
	(0.0743)	*-0.0523*	(0.0744)	*-0.0544*	(0.0749)	*-0.0567*	(0.0749)	*-0.0561*
Female	-	-	-0.2082***	-0.0351	-0.2374***	-0.0392	-0.2317***	-0.0382
			(0.0277)		(0.0280)		(0.0280)	
Black	-	-	-0.2150***	-0.0372	-0.2058***	-0.0327	-0.2080***	-0.0351
			(0.0337)		(0.0341)		(0.0341)	
Age polynomial	No		No		Yes		No	
Categorical Age Dummies	No		No		No		Yes	
Significance of LR	P<0.001		P<0.001		P<0.001		P<0.001	
N	31,804	-	31,804	-	31,804	-	31,804	-

Individuals in income deciles 1, 2, and 3 constitute the "Low Income" treatment group. Individuals in income deciles 9 and 10 constitute the control group. Observations were recorded at 1 year, 2 years, and 3 years post transplant. *** indicates significance at the 1 percent level. Standard errors are in parentheses.

Table 12: Adjusted Policy Effects

Differences by Gender			
Group	**Number of Observations**	**Original Policy Effect**	**Adjusted Policy Effect**
Men	18,691	-0.095	-0.271
Women	13,113	-0.102	-0.291
Difference is significant at p<0.001			
Differences by Race			
Group	**Number of Observations**	**Original Policy Effect**	**Adjusted Policy Effect**
Whites	25,005	-0.097	-0.277
Blacks	6,799	-0.102	-0.291
Difference is significant at p<0.001			
Differences by Race and Gender			
Group	**Number of Observations**	**Original Policy Effect**	**Adjusted Policy Effect**
White Males	14,724	-0.094	-0.269
White Females	10,281	-0.101	-0.289
Black Males	3,967	-0.101	-0.289
Black Females	2,832	-0.105	-0.305
Differences between all groups were significant at p<0.001 except for white females compared to black males			
Differences by Age			
Group	**Number of Observations**	**Original Policy Effect**	**Adjusted Policy Effect**
Teen	1,036	-0.095	-0.271
Young Adult	4,116	-0.083	-0.237
Adult	13,084	-0.094	-0.269
Middle Age	10,784	-0.110	-0.314
Elderly	2,784	-0.110	-0.314
Overall differences were significant at p<0.001			