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Three Essays on Public Economics and Policy

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Abstract

This dissertation studies the effects of public programs that target at disadvantaged population. In particular, I examine the effects of these programs on individual well-being and whether these programs lead to behavior distortions. The dissertation consists of three essays that focus on different policy contexts and different populations.

In the first essay, I examine the effects of an active labor market program on the labor market outcomes of veterans. In the 1990s, the US Department of Defense implemented the Transition Assistance Program (TAP) to help military personnel transition to the civilian labor market. The program provides career counseling, employment workshops, and information briefing on educational and medical benefits through the Department of Veterans Affairs. The goal of this study is to measure the long-run effects of TAP on the labor market outcomes of veterans. To identify these effects, I exploit variation in program accessibility generated by its initial rollout process. Using data from the veterans supplement to the Current Population Survey, survey years 2001, 2003 and 2005, I find that TAP improves the labor market outcomes of veterans, measured approximately ten years after their separation from the military. The effects of TAP on labor market outcomes vary across veterans with different lengths of military experience, and these effects attenuate over time. One possible mechanism is that TAP encourages use of the GI Bill, a program that provides tuition reimbursement and a monthly allowance to attend institutions of higher education. Moreover, the effect of TAP is concentrated among participants who, based on self-reports, find TAP most helpful in career related services.

In the second essay, I examine linkages between Medicaid availability, insurance coverage for pregnant women, use of prenatal care and the health outcomes of newborns. Medicaid coverage for pregnant women would potentially improve health outcomes of newborns

by providing medical services at no cost. At the same time, Medicaid provides an incentive for eligible individuals to drop private insurance coverage. I exploit variation from two federal Medicaid expansions in mid-1980s, which eliminated the eligibility discontinuity across age groups. The main data come from the National Hospital Discharge Survey and the Vital Statistics Birth Records. I employ a difference-in-differences method. Findings suggest that the expansions led to a relative increase in Medicaid coverage for women from the immediately affected age group by a significant 6.5 percentage points. However, up to 60% of new beneficiaries would have been covered by private insurance, absent of the expansions. Findings do not reveal significant improvement in birth outcomes, or increase in prenatal care utilization.

In the third essay, I examine the effect of Medicaid on the asset holding behavior of the elderly. Medicaid provides coverage for nursing home care to elderly with limited resources. The eligibility rule potentially creates an incentive for the elderly to reduce their asset by making transfers to children. This study provides empirical evidence for the effect of Medicaid nursing home care policy on elderly's asset allocation. I exploit a policy change in 2006, the Deficit Reduction Act 2005, which imposed stricter penalty to asset transfer behavior when determining Medicaid eligibility. Using data from the Health and Retirement Survey, I employ a difference-in-differences method. I find that the elderly who anticipated nursing home entry significantly reduced asset transfer behavior by 3.2 to 4 percentage points, in response to the policy change. The result is robust across demographic groups and more pronounced for the elderly with the highest nursing home entry risk. The findings provide evidence of moral hazard in the public insurance market: the elderly would game with the system in anticipation of nursing home needs.

THREE ESSAYS ON PUBLIC ECONOMICS AND POLICY

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Chapter 1: Improving the Labor Market Outcomes of US Veterans: The Long-Run Effect of the Transition Assistance Program

1. Introduction

Each year, the Department of Defense must sustain a viable military force by recruiting qualified individuals. The attractiveness of a military career largely depends on the potential consequences of military service.[1] One consequence that is of particular policy interest is the subsequent labor market prospects of veterans. Anecdotally, the effect of military service on civilian labor market outcomes is unclear. On the one hand, military service may worsen labor market outcomes since military personnel forgo valuable experience in the civilian labor market. Military experience may also increase the prevalence of work-limiting disabilities, especially among personnel who serve in combat. On the other hand, veterans are eligible for a plethora of programs that promote labor market outcomes. These programs include direct job training programs, educational assistance programs (commonly known as the GI Bill), and medical services delivered through the Department of Veterans Affairs health care system.[2] On net, several empirical studies find that the overall effect of military experience on outcomes in the civilian labor market is negative.[3]

[1] See Hause (1973), Ash, Udis and McNown (1983), Schiller, Kroetch and Flacco (1991), and Borgschulte and Martorell (2013).

[2] See Goldberg and Warner (1987); Mangum and Ball (1989) for the tradeoffs between military service and civilian work experience; See Bendard and Deschênes (2006); Angrist, Chen and Frandsen (2010); van der Goes and Snyder (2012) for the health effect of military service. See Angrist (1993); Bound and Turner (2002) for how educational benefits effect labor supply. See Autor and Duggan (2007); Duggan, Rosenheck and Singleton (2010) for how disability compensation program affect labor supply.

[3] Angrist (1990), Angrist and Chen (2008) and Chaudhuri and Rose (2009) show that Vietnam veterans suffer a persistent earning loss compared to non-veteran counterparts. Angrist (1998) examines veterans who served in 1980s, and find military service reduced civilian earning of white veterans.

To mitigate the potential disadvantages of military service, the Department of Defense and the Department of Veterans Affairs gradually extended the scope of career assistance programs. One such program – and the focus of this study – is the Transition Assistance Program (TAP). TAP was established by Congress in the National Defense Authorization Act for Fiscal Year 1991, in anticipation of the military drawdown following the end of the Cold War. The program lasted three to four days, and was designed to help veterans establish a civilian mindset, increase job search efficiency, obtain interview skills, and improve job match quality. In addition, TAP provided information regarding benefits administered by the Department of Veterans Affairs. The empirical objective of this study is to estimate the effects of TAP on the labor market outcomes of veterans.

To identify the effect of TAP, this study exploits variation in TAP availability generated by its gradual rollout. Established as a national program in 1991, TAP expanded fully across the country over a couple of years. The program was rolled out to cover the greatest proportion of the military population in the shortest amount of time. This meant that states with large military populations and installations were covered first; states with small military populations and installations were covered later. Nonetheless, the program expanded quickly: by June 1991, 50 major installations established TAP. The number increased to 80 by September 1991, 178 by September 1992, and 226 by September 1993. The process created variation in program availability across veterans, which was mainly affected by their service location.

The main data come from the veterans supplement to the Current Population Survey, survey years 2001, 2003 and 2005.[4] The Current Population Survey covers demographic

[4] Survey years 2001, 2003 and 2005 are years that variables used for regression analysis are consistently constructed.

information, such as race, age, gender and education, as well as labor market outcomes, such as labor force participation, employment status, and family income. In addition, the veterans supplement asks questions regarding military experience. This information includes a veteran's year of separation, availability of TAP at the time of separation, and participation in TAP.

The effect of TAP is measured by the association between TAP participation at the time of separation and labor market outcomes at the time of the survey. Empirically, I follow a covariates-adjusted IV approach, using TAP availability as an instrument for TAP participation. The sample is restricted to male veterans who separated from the military in 1990 to 1993. The reason for this restriction is that the rollout of TAP, and thus variation in TAP availability, occurred predominantly during these years.

I find that TAP improved the labor market outcomes of veterans, measured approximately ten years after their separation from the military. In the preferred specification, TAP increased labor force participation by 7.29 percentage points and full-time work by 7.47 percentage points. With regard to earnings, TAP increased the likelihood of annual family income exceeding $40,000 by 12.01 percentage points.

The effect of TAP on labor market outcomes may be due to an increased awareness of, and thus participation in, the GI Bill. This bill provides tuition reimbursement and a monthly subsidy to qualified veterans. Depending on the era and length of service, veterans can receive benefits through the GI Bill for up to 36 months. Using the veterans supplement from the CPS, I show that TAP increased seeking education/training programs by 15.4 percentage points and some college education by 14.96 percentage points.

There are two concerns regarding identification. First, the likelihood of being offered TAP increased with years of military service. Thus, if senior service members outperform junior service members in the civilian labor market, independent of TAP, then the effect of TAP on labor market outcomes may be overstated. To address the concern, I control for demographic characteristics (age, race, high school education, state of residence) and military characteristics (length of service, deployment history). The preferred specification includes a full set of observed controls.

Second, TAP was first offered to veterans at large military installations, and installation size may affect employment prospects after separation. For example, military installations often hire recently separated veterans as civilian employees and contractors. This may generate a positive correlation between installation size and labor market outcomes, which in turn would generate a spurious, positive correlation between TAP availability and labor market outcomes. To address this concern, I use the American Community Survey of years 2003 through 2007, and look at the labor market outcomes of veterans separated within the past 12 months of the survey. I find that the labor market outcomes of newly separated veterans are uncorrelated with the military population of the states in which they separated.

I extend the main analysis to examine how the effects of TAP vary across different subgroups and over time. First, I investigate whether TAP has heterogeneous effects on veterans with varied military experience. Findings from subgroup analysis suggest that TAP improves labor market outcomes for veterans with different lengths of military service, with varied margins of behavior response. Among veterans who served for four years or less, TAP participants are significantly more likely to work full-time. Among veterans with longer than

four years of military experience, who are closer to retirement age at the time of the survey, TAP participants are significantly more likely to remain in the labor force.

Second, I estimate the short and long effects of TAP, rather than just ten years after separation in the main analysis. To do so, I use data from the veterans supplement for survey years 1995 to 2010 and focus on veterans separated from 1990 to 1993. According to the results, TAP has the largest effect on labor force participation immediately after separation, and this effect attenuates over time.

While the main analysis focuses on the objective measure of labor market outcomes, an equally important question is how participants themselves value the program.[5] I seek to answer this question by comparing one's subjective evaluation of TAP to their observed outcomes. Information regarding one's subjective evaluation is available in CPS, which directly asks TAP participants what aspect of the program they find most helpful. Using such information, I find that the effects of TAP on labor market outcomes are concentrated among participants who find TAP most helpful in career related services.

Although TAP is a large-scale national program, few studies examine its effects. A few studies attempt to evaluate TAP by surveying participants: Sadacca et al. (1995) conduct a survey of 3,000 veterans from all four military branches. Fauer et al. (2014) conduct a survey of 350 Army veterans. While both studies report participants' satisfaction with TAP, evidence from national representative population is lacking. One study most related to this one is by Heflin et al. (2014), who also uses information from CPS. The focus of their study is veterans'

[5] See DiNardo and Lee (2011) for discussion of objectives of program evaluation. For program evaluation that focuses on ex-post subjective evaluation, see Kristensen (2008); Eyal (2010) and Smith, Whalley and Wilcox (2013).

participation choice and how participants value the program. This study instead focuses on veterans' subsequent labor market outcomes.

This study also contributes to the broader active labor market program (ALMP) evaluation literature. Active labor market programs are government-funded programs targeted at promoting labor market outcomes for disadvantaged populations. Card et al. (2010) and Kluve (2010) provide comprehensive reviews of the literature. While substantial efforts have been made to assess the effectiveness of this type of programs, the focus of this study - the Department of Defense programs – has received less attention.

The rest of the paper proceeds as follows: section 2 introduces the history and institutional setting of TAP. Section 3 discusses the data source. Section 4 describes the methodology. Section 5 presents the results of TAP on labor market outcomes, educational attainment and disability condition. Section 6 provides supportive evidence for the key identification assumptions. Section 7 provides extensions. Finally, section 8 concludes.

2. Backgrounds

2.1 Military downsizing

The US military experienced a major drawdown after the conclusion of the Cold War in the early 1990s. Between 1989 and 1995, more than 110 installations were realigned, consolidated or closed. The total number of active service members decreased by more than 25%: from 2.1 million to 1.5 million (**Figure 1**). The Department of Defense (DOD) used various policy tools aimed at manpower reduction: the majority of the downsizing goal was reached by reducing the number newly recruited personnel, referred to as accessions. At the same time, DOD increased military discharges by restricting retention (a continued military

career after the end of the current contract) and providing monetary incentives to early separation (leaving the military before the end of the current contract).

The substantial downsizing of the active forces forced a large flow of service members to move from the military to the civilian labor market within a few years. In addition, the active forces were shaped towards seniority: as shown in **Figure 2**, over time, service members were on average older and tended to serve longer in the military. Such a pattern existed for both the enlistee and the officer population.

The increase in military separations raised concerns about the transition from military service to the civilian labor market. For some veterans, skills obtained from military service are not directly transferable to civilian labor market. For example, 15% of enlisted personnel work as members of combat units. Their training involves skills to "maneuver against enemy forces and positions and fire artillery, guns, mortars, or missiles to destroy those positions".[6] These military occupations do not have obvious counterparts in the civilian labor market. Even for veterans whose skills can be used in a civilian setting, lack of experience in the civilian labor market may impede a smooth transition.[7]

2.2 Transition Assistance Program

The Transition Assistance Program was established to help veterans with the transition to the civilian labor market. Congress passed legislation in December 1989 authorizing a pilot project in twelve military sites from six states (California, Florida, Georgia, Louisiana, Texas, and Virginia). These pilot programs began in May or June 1990. TAP was extended as a full-

[6] Occupational outlook handbook. Bureau of Labor Statistics. http://www.bls.gov/ooh/military/military-careers.htm#tab-2

[7] See GAO (1994) for a thorough discussion of the transition difficulties faced by veterans, particularly during the downsizing era.

scale national program in November 1990 by the National Defense Authorization Act for Fiscal Year 1991. The basic curriculum consists of three parts. First, near separation service members take a one to four hour pre-separation counseling session that helps them set a transition agenda and develop long-run personal and professional goals. The heart of TAP is a three-day employment workshop that provides job search assistance: introduction to job search resources, help with resume writing, and a mock interview. The final part of TAP is a four-hour briefing session regarding benefits through the Department of Veterans Affairs. These benefit programs include the GI Bill, which funds higher education and training; Disability Compensation, which provides monthly payments to veterans with disability condition caused or made worse by military service, referred to as service-connected disability; health care services, which is delivered through the VA health care system; and various career assistance programs in civilian settings.

In addition to the main modules, a half-day session (known as Disabled Transition Assistance Program or DTAP) is provided to veterans who have a service-connected disability, or who are believed to be leaving the military with a service-connected disability. DTAP covers information about Disability Compensation, Vocational Rehabilitation and Employment (VE&E) programs,[8] and additional health care services for disabled veterans.

Military branches have flexibility to modify or extend the original TAP program to meet the specific needs of their personnel. For example, the Army provides additional one-on-one

[8] The VE&E program provides comprehensive services to disabled veterans regarding job training, employment accommodations, resume development, and job seeking skills coaching.

counseling and interactive job training and assistance. The Navy adds a day to three-day employment workshop for detailed discussion of veteran benefits.[9]

TAP is jointly organized by multiple government agencies: the Department of Defense (DOD) is in charge of the pre-separation counseling and logistics control; the Department of Labor (DOL) is in charge of conducting the employment workshop; the Department of Veterans Affairs (VA) is responsible for the benefit briefing session and DTAP. In 1991, the three agencies entered a Memorandum of Understanding (MOU), which outlined the objectives and management arrangements. The MOU was revised in 1994, 2006 and 2013. The Department of Homeland Security, the Department of Education, and the Department of Small Business Administration also share the responsibility in later years.

To be eligible for TAP, one needs to be an honorably discharged service member who has been on active duty for longer than 180 days, or who served for less than 180 days but whose separation was due to a health condition. Eligible service members can seek TAP starting six months prior to separation and one year prior to retirement.[10]

DOD planned and directed the rollout of TAP to cover areas with large military populations first. The primary determinant for a military installation to become a TAP site was the number of active service members located in the particular state. DOD collected information on the total number of personnel on active duty located within each state and forwarded it to the DOL. Based on military population ranking, the corresponding officer from DOL, the Director for Veterans' Employment and Training (DVET), did a polling among state level military

[9] The Government Accountability Office report, 2002
[10] Veterans Education and Benefits Expansion Act of 2001 extends the time period from 6 months to 1 year for separatees and from 1 year to 2 years for retirees.

services to determine their desire to participate in TAP. Based on the initial polling, individual installations were selected to establish a TAP program on site.[11]

The rollout process generated variation in TAP availability at the installation level, which is presented in **Figure 3.** The rollout process follows two patterns. First, state level military populations positively correlate with TAP establishment timing. State level military populations are shown in **Appendix Table 2**. States that have high military population density, such as California, Georgia, and Texas, initiated TAP first. States with smaller military population, such as Tennessee, Minnesota, and Wisconsin, initiated TAP later. Second, the rollout timing was positively correlated with installation size. This is shown in **Figure 4,** which separately displays the rollout timing by installation size. Four out of the six largest installations (with greater than 30,000 personnel) had TAP by May 1991. Most installations with large populations (with greater than 6,000 personnel) established TAP in the first year of the program's initiation. Most smaller installations (with less than 1,500 personnel) established TAP in 1992 or 1993.

Overall, the rollout was rapid: As of June 30, 1991, TAP was operated at 50 military installations within the contiguous US, with 16,543 service members participated in 326 employment workshops. By the end of FY1991, TAP had expanded to 80 military installations, with 430 workshops serving 22,804 participants. By the end of FY1992, it had expanded to 178 major sites.[12] By the end of FY1993, all major installations in the contiguous US offered TAP.[13]

[11] Congressional hearing, 1991
[12] Congressional hearing, multiple years
[13] At the initial stage, TAP availability overseas was limited. To meet the needs of deployed personnel, DOD later expanded TAP program at oversea bases. For service members who are currently deployed, a condensed version of TAP was delivered by mobile instructors or through the form of videotape accompanied by the DOL workbook (GAO 2002). For service members returning from deployment, the 1994 MOU stated that they have priority in attending TAP in domestic installations.

Despite the fast rollout, the program was not offered uniformly to all military personnel within an installation. One issue was the attitude of commanding officers towards TAP. Commanding officers were responsible for identifying near separation soldiers, informing them of TAP, and freeing them from daily duty to participate. The Government Accountability Office (1994) reports that some commanding officers were reluctant in releasing TAP information because they did not want the solders to be distracted from daily duty. Another issue was the limited availability of TAP instructors. Before organizing seminars on site, TAP instructors were trained at Veterans Training Institution. Shortages of qualified instructors was a frequent issue. Finally, interagency collaboration inefficiency intensified the lack of resources. To organize a TAP workshop, the DOD needed to report to the DOL the number of eligible service members at each base for the latter to schedule workshops accordingly. However, data collecting and sharing was largely manually processed in the 1990s. This led to delay and inefficiency in DOL personnel allocation.[14] According to GAO (2002), after TAP's full implementation at installation level, 50% to 60% of all eligible personnel participate.

3. Data and sample

The major data source is the veterans supplement to the Current Population Survey. Current Population Survey is a monthly survey of non-institutionalized individuals conducted by the Bureau of Census on behalf of the Bureau of Labor Statistics. The veterans supplement to the CPS is conducted in August every other year. It includes additional questions for the veteran population.

[14] Logistics Management Institute report, 1997.

Two sets of information in the CPS facilitate the analysis of this study. First, CPS records the labor market outcomes and demographic characteristics at the time of the survey. This information includes labor force participation, hours worked, income, age, race, educational attainment, family structure, and state of residence.

Second, CPS records the historical military experience of veterans, which is asked retrospectively. This information includes year of separation, length of service, and receipt of medals for deployment overseas for military operation. Importantly, the key variables for analysis - TAP participation and TAP availability - are also recorded in CPS. CPS directly asks veterans whether they participated in TAP before separation. A follow up question is asked to veterans who did not participate in TAP regarding whether they were offered TAP.[15] Based on the answers to these two questions, I construct a dummy variable for TAP offering: equals one if the veteran participated or was offered TAP; equals zero if the veteran did not participate and was not offered TAP.[16]

The sample used in this study is obtained by pooling observations from survey years 2001, 2003 and 2005. These years are chosen to ensure that variables regarding TAP are consistently defined. The analysis sample is restricted to veterans separated in years 1990 to 1993. The time period covers one year before the national expansion of TAP to three years after. During these four years, DOD administrative records reports 1,425,386 separations. For

[15] One caveat with the data is that the questions regarding TAP participation and offering are only asked to veterans separated from the military after 1991. For 1990, when the TAP pilot program was in place, participants may be miscoded as non-participants. Such measurement error would lead to attenuation bias. However, the miscoded population is likely small. CPS from 1995 to 1999 also asked the question regarding TAP participation to veterans discharged prior to 1991. According to the responses, the TAP participation rate in 1990 was 0.52%. Additionally, conditional on offering, the take-up rate of TAP in 1991 was around 40%. If the take-up rate in 1990 was the same as in 1991, four observations from the analysis sample are potentially miscoded as not offered TAP in 1990.
[16] One implicit assumption is one has to be offered TAP to participate.

comparison, the corresponding population estimate from CPS is 1,142,844.[17] I further restrict the analysis sample to male veterans of age 26 to 64 at the time of survey. The restrictions retain 1347 observations.

Table 1 displays a summary of characteristics for the analysis sample, by years of separation. Over the sample period, veterans share overall similar characteristics. Two differences are worth noting. First, veterans separated in later years are on average slightly older. This is consistent with the observation from DOD records that the active forces trend towards seniority. Second, after 1990, the share of veterans that were ever deployed increased by about 20 percentage points, mostly due to gradual separation of Gulf War veterans after 1991.

4. Methodology

The empirical objective of this study is to estimate the causal effect of TAP on the labor market outcomes of veterans. The effect is measured by the association between TAP participation and labor market outcomes, as shown in equation (1).

$$(1)\quad Y_{ist} = \alpha_0 + \alpha_1 * TAP_{ist} + \epsilon_{ist}$$

Y_{ist} denotes the labor market outcomes of individual *i*, who resides in state *s* and is surveyed in year *t*. TAP_{ist} denotes whether the individual participated in TAP prior to separation. The error term is denoted as ϵ_{ist}.

The major difficulty with causally identifying α_1 is that individuals self-select into TAP participation. Participants and non-participants potentially differ on other dimensions that determine labor market outcomes. For example, individuals who have jobs lined up or have

[17] The CPS population estimate is lower than administrative records primarily because the year of separation information is missing for approximately 18.2% of the veteran sample.

educational plans prior to separation may find TAP not as helpful and thus choose not to participate. If so, the differences in labor market outcomes between participants and non-participants may understate the effects of TAP. On the other hand, non-participation may reflect one's unwillingness to invest in human capital. This would suggest an upward bias of the OLS. Overall, the relationship between the OLS estimate and the true causal effect is ambiguous.

To attempt to address the individual selection problem, this study utilizes variation in TAP availability generated by its gradual rollout: as discussed in the background section, from 1991 to 1993, TAP expanded at the installation level across the country. The rollout process generated differentiation in TAP accessibility, which was driven by veteran's location rather than latent individual characteristics.

Figure 5 graphically illustrates how TAP offering and participation rates change over time. In line with the national level program rollout process, both offering and participation rates increased rapidly in 1991 through 1993. After TAP's fully implementation at installation level in 1993, both offering and participation plateaued. After 1993, the overall offering rate stays at about 70%, and participation rate stays at about 50-60%.

One concern with using variation of TAP availability at individual level is that, as suggested by anecdotal evidence, TAP was not offered uniformly within installations. In particular, veterans who served longer were more likely offered TAP due to seniority. At the same time, longer time in the military is potentially associated with less civilian work experience, more complicated duties (for example, deployment), and an older age at separation. These attributes would directly affect subsequent civilian labor market outcomes.

Table 2 displays the noted differences in observed characteristics between the group offered TAP and the group not offered. Particularly, those who are offered TAP had longer military service: the offered group are less likely to serve for four years or less by 18.6 percentage points. They are more likely to serve for 5 to 19 years and more than 20 years by 9.8 percentage points and 8.9 percentage points, respectively. The differences in service lengths also contribute to significant differences in age: the offered group are on average 0.87 years older than the group not offered TAP at the time of survey. In addition, the offered group is more likely to have been deployed overseas by 11.3 percentage points.

To take into account the observed differences, I perform regression analysis to control for demographic characteristics (age, race, high school education, whether born native, and states of residence) and military characteristics (length of service, deployment). If these characteristics are sufficient controls for the probability of being offered TAP within bases, the effect of TAP is properly identified.

To utilize the variation in TAP availability, I follow an IV approach: the constructed TAP offering variable is used to instrument for TAP participation. At individual level, the accessibility of TAP potentially led to variation in actually attending the program. Such variation is used to identify the effect of TAP. The main specification is as follows:

$$(2)\ TAP_{ist} = \beta_0 + \beta_1 * TAP\_offered_{ist} + \beta_2 * X_{ist} + \beta_3 * D_{ist} + \mu_s + \mu_t + e_{ist}$$

$$(3)\ Y_{ist} = \gamma_0 + \gamma_1 * TAP\_offered_{ist} + \gamma_2 * X_{ist} + \gamma_3 * D_{ist} + u_s + u_t + \varepsilon_{ist}$$

TAP_{ist} and $TAP\_offered_{ist}$ denote TAP participation and offering for individual *i*, who resides in state *s* and is surveyed in year *t*, respectively. X_{ist} includes a set of demographic characteristics and military characteristics. Survey year fixed effects are denoted as μ_t and u_t.

State of residence fixed effects are denoted as μ_s and u_s. Additionally, I include a set of dummies indicating the year of separation, denoted as D_{ist}. The year of separation dummies are included to purge year specific attributes that coincide with TAP rollout timing, such as macroeconomic condition, compositional change of active military personnel, and Gulf war. The causal effect of TAP on Y is identified as γ_1/β_1. The ratio measures the effect of TAP on the subgroup of veterans, whose participation decision is determined by having accessibility.[18]

The identification assumption is, conditional on observables, TAP offering and labor market outcomes are not correlated through channels other than the causal relationship of interest. Specifically, two things are crucial. First, veterans separated from large installations (early rollout) do not have systematically different labor market outcomes, compared to those separated from small installations (late rollout). Second, there are no other unobserved attributes that are jointly correlated with both TAP offering and labor market outcomes. Although the assumptions are not directly testable, supportive evidence is provided in section 6.

5. Results

5.1 Labor market outcomes

This section presents results for the effects of TAP on labor market outcomes. Following the literature, I mainly focus on labor force participation. I also look at two additional measures

[18] I use linear probability models for all analysis in this study. One potential concern is that the outcome variables are binary, suggesting non-linear models may be better proxy. I thus check the robustness of results by adopting Probit specification for the reduced form estimation. The results from Probit specifications show similar marginal effects and statistical power.

of labor market performance: whether one usually works full-time and whether annual family income exceeds $40,000.[19]

Figure 6 graphically illustrates the outcome trends for veterans of three groups: those who were not offered TAP, those who were offered TAP, and those who participated.[20] The trends suggest that veterans who were offered TAP seem to outperform veterans who were not offered TAP regarding all three outcomes. However, graphical analysis does not take into account observed differences across different groups. I thus turn to regression analysis.

Table 3 displays results for regression analysis. The dependent variable is labor force participation. As a benchmark, Panel A reports the coefficient of TAP participation using OLS specifications following equation (1). Panel B reports the preferred IV estimates, following equations (2) and (3). In each panel, a series of specifications are performed with various controls. The IV estimate in column 1, which includes no controls, shows an estimated treatment effect of 4.75 percentage points. Column 2 includes fixed effects for state of residence and year of survey. Additionally, it includes year of separation dummies to purge year specific attributes that are directly associated with labor force participation. The estimated treatment effect goes up to 6.47 percentage points.

Column 3 and 4 include controls for demographic characteristics. As shown in **Table 2**, veterans who are offered TAP are on average older than those who are not offered. For the analysis sample, whose average age at the time of survey is around 40, an older age is associated with lower labor force participation. Failure to take into account the observed age differences

[19] I check family structure as potential outcomes. Evidence suggests that TAP does not have significant effects on veterans' marital status or the presence of dependent children.
[20] The three groups are not mutually exclusive: those who were offered include participants and non-takers.

would lead to a downward bias. Column 3 of **Table 3** includes age and age squared as controls. As expected, the estimate increases to 7.22 percentage points. Column 4 adds other demographic characteristics (race, high school/GED, whether born native). The estimated treatment effect is robust.

Column 5 and 6 further add dummies for lengths of military service and for deployment history. The preferred specification in Column 6 suggests that TAP increased labor force participation of veterans by a significant 7.29 percentage points, approximately ten years after their separation. For all specifications, IV yield larger coefficients for TAP compared to OLS.[21]

I use similar specifications to look at other labor market outcomes. The estimated effect of TAP on full time work status is shown in **Table 4**. The magnitude and statistical power are similar to those of labor force participation. The preferred specification in column 6 reports that TAP led to a 7.47 percentage point increase in working full-time. The estimated effect of TAP on family income is shown in **Table 5**. The preferred specification in column 6 shows that TAP increased the probability of family annual income exceeding $40,000 by 12.01 percentage points.

Overall, the effects of TAP on labor market outcomes are large in magnitude. For example, the effect on labor force participation is 7.29 percentage points, out of a base of 0.92. A few factors may contribute to the large magnitude. First, TAP potentially increased educational attainments, which would lead to substantial improvement in labor market performance. Second, the sampled veterans are of varied ages at the time of survey. I show in later extensions that the

[21] See Appendix for further discussion.

labor force participation results are primarily driven by veterans near retirement age, who are more likely alter labor supply decisions.

5.2 VA benefits utilization

An important component of TAP is the information briefing session regarding benefits through the Department of Veterans Affairs. If TAP increases the awareness of the educational and medical benefits, participants are potentially more like to claim the benefits.[22] Their labor market outcomes would be indirectly affected. For example, if veterans increase the use of GI Bill, their educational attainment would increase, leading to improved labor market outcomes. On the other hand, veterans may also increase the claim of Disability Compensation, which offers a monthly payment to veterans with service-connected disability. Such monetary payment is potentially a disincentive to work. This section examines whether TAP facilitates VA benefit utilization, as possible mechanism by which labor market outcomes are affected.

I focus on three outcomes to measure VA benefit utilization. First, I focus on whether the veteran ever sought formal job training or job-related schooling after separation. [23] This indicator is used as a proxy for using the GI Bill. Second, I focus on whether veterans increase educational

[22] According to the National Survey of Veterans (2010), veterans being unaware of entitled benefits or finding application processes difficult are major barriers for benefit utilization. Notably, large share of veterans served on the active duty between 1975 and 1990 report that they have little understanding of VA benefits. The shares that indicated "not at all" to questions regarding how much they understood the benefits are: 40.7% for health care benefit; 42.6% for education and training benefits; 58.6% for life insurance benefits; and 32.1% for home loan guaranty benefits.

[23] The training/educational program asked in CPS were specified as "Any high school, college-or graduate-level course work taken to improve job prospects, knowledge, or skills DOES COUNT as formal job training."

attainment, measured by whether one ever obtained some college education.[24] Finally, I focus on whether the veteran self-report having service-connected disability.[25]

Figure 7 shows the outcome trends for veterans of the three groups: those who were not offered TAP, those who were offered TAP, and those who participated. The graphical trends suggest that veterans who were offered TAP seem to be more likely to obtain college education, to seek education/training, and to report having service-connected disability conditions. Again, I turn to regression analysis to adjust for differences in observed characteristics.

I follow the same specifications as in the previous section. Results regarding seeking education/training program after separation are shown in **Table 6.** The preferred specifications using IV are shown in Panel B. Column 1 reports that TAP increased seeking education or training by 11.18 percentage points when no controls are included. Column 2 includes state fixed effects, time of survey fixed effects and dummies for year of separation, and reports a slightly higher treatment effect. Columns 3 and 4 include age and other demographic characteristics. The coefficient for TAP increases when demographic characteristics are added. One possible explanation is that older veterans have less incentive to obtain education/training because they expect lower cumulative return to education. Such pattern is consistent with Heckman and Smith (2003), who find lower Job Training Partnership Act enrollment among older adults. Meanwhile, veterans' ages are positively correlated with TAP offering. Controlling for age would increase the estimated effect of TAP. Column 5 and 6 further add military characteristics. The preferred

[24] VA educational benefits are mostly used for college education, In FY2002, 91.3% of GI Bill beneficiaries use it for undergraduate education (83.3%), graduate education (4.1%) or a college with no degree (3.9%). Veterans Benefits Annual Benefits Report, Department of Veterans Affairs, 2003.
[25] The self-reported disabled population estimates match with administrative records from the Veterans Benefits Administration.

specification in column 6 reports that TAP increased seeking education or training program by 15.4 percentage points.

Results regarding having some college education are shown in **Table 7**. The preferred specifications using IV are shown in Panel B. Column 1 reports that TAP increases college education by 15.94 percentage points when no controls are included. Column 2 includes state fixed effects, time of survey fixed effects and dummies for year of separation. Column 3 and 4 include controls for age and other demographic characteristics. Column 5 and 6 further include controls for characteristics of military experience. The coefficient of TAP is robust. The preferred specification in Column 6 suggests TAP increased some college education by 14.96 percentage points.

Results regarding having a service-connected disability are shown in **Table 8**. The preferred specifications using IV are shown in Panel B. Column 1 shows that, with no controls, TAP significantly increases service-connected disability claims. However, anecdotal evidence suggests that service-connected disability have a higher prevalence as one serves longer, and the service lengths are also positively correlated with TAP offering. The rest of **Table 8** reveals that the magnitudes of the coefficients of TAP shrinks significantly as controls are added. The preferred specification in column 6 shows that TAP did not have significant effect on the disability onset for veterans. Longer service in the military and being deployed have significant positive correlations with service-connected disability.

Overall, the results suggest that TAP increased veterans' educational attainment, plausibly by encouraging them to use the GI Bill. However, TAP did not seem to increase claims for service-connected disability. The increased educational attainment potentially contributes to the improvement in labor market outcomes shown in previous section.

6. Discussion of identification assumptions

This study identifies the effect of TAP by exploiting variation in TAP availability generated by the program rollout at the installation level. To establish causal interpretation, two assumptions are essential: first, veterans separated from large installations (early rollout) and veterans separated from small installations (late rollout) do not differ systematically in labor market outcomes. Second, the estimated effects of TAP are not driven by other unobserved differences between those offered TAP and those not offered. This section provides supportive evidence for these assumptions.

6.1 Veterans from large vs. small installations

Large and small installations differ. Large installations are more complex in function, more likely to consist of multiple military branches, and more likely to be substantial component of the local economy. On the other hand, small installations usually have more specific function, and are less likely to affect or be affected by the local community (Zou 2014). Such differences yield two potential threats to identification.

First, military population density may be correlated with transition difficulty. Military installations often hire newly separated veterans as civilian employees or contractors to support daily operation. Such job opportunities are potentially more concentrated in areas with large military populations. If so, veterans separated from areas with large military installations, who also had earlier access to TAP, may have a better chance to find a job. The effects of TAP on labor market outcomes would be overstated.

To address the concern, I directly examine whether the labor market performance of newly separated veterans are correlated with the size of local military population. I draw

information from the American Community Survey for years from 2003 through 2007. Newly separated veterans are identified as those who report being on active duty during the past 12 months, but are not on active duty at the time of the survey. For each observation, the ACS records their states of residence one year ago, which are used as indicators for the states in which they separated. The state level military population is drawn from the DOD demographic report of 2004.

Figure 8 graphically illustrates the relationship between the outcomes of interest and state level military population. For each outcome of interest (labor force participation, full-time work, college education), I plot the sample mean outcomes and state level military population against the states of separation. No systematic differences in outcomes seem to exist between states with large military population and states with small military population.

To quantify any potential association between labor market outcomes and military population density, I further perform covariate-adjusted regression analysis as follows:

$$(4)\quad Y_{ist} = \alpha_0 + \alpha_1 * mil\_pop_s + \alpha_2 * X_{ist} + \eta_s + \eta_t + \xi_{ist}$$

Y_{ist} denote the outcomes of interest. State level military population is denoted as mil_pop_s. Additional controls include individual characteristics, X_{ist}, state fixed effects, η_s, and year of survey fixed effects, η_t. The coefficients are reported in **Table 9**. Panel A focuses on labor force participation. Column 1 reports the simple correlation with no controls. Column 2 includes a full set of controls. In both regressions, state level military population has little impact, which is not statistically different from zero, on labor force participation. Panel B and C show results using the same specification for full-time work and college education. None of the regressions reveals significant correlation between outcomes and military population. These results suggest that

military population density is not directly correlated with labor market outcomes or educational attainment among newly separated veterans.[26]

The second concern is that the ACS only reports veteran outcomes in the early 2000s. The studied period, the defense-downsizing era, may have further complications. For example, the military downsizing may have hurt the local economy more severely for communities with large installations. If so, veterans discharged from the large installations potentially faced a worse local economic condition and greater difficulty in finding a job. These veterans also tended to have earlier access to TAP. This would lead to a downward bias of estimated effect of TAP on labor market outcomes.

Though not directly testable, this hypothesis is not supported by previous literature. RAND (1996) suggests that military bases closure (the most severe case of downsizing procedure) did not translate into significant adverse shock to local labor market. The Congressional Research Service (1997) documents substantial federal efforts to assist local communities, local workers and businesses that were affected by base closure. As a result, relatively few communities that experienced base closure or realignment had unemployment rates above national average as of July 1995. Thus, the downsizing did not seem to create systematic difference in local economic conditions.

6.2 Unobserved military characteristics

One additional concern with the identification is that unobserved military characteristics may drive the results instead of TAP, if these unobserved characteristics are jointly correlated

[26] For all three outcomes, the point estimates from the preferred specification (one with controls) have negative signs. This suggests that, even if the point estimates are taken as the true correlation, a larger state level military population is associated with worse outcomes of veterans.

with TAP offering and outcomes of interest. Two unobserved characteristics that may be directly correlated with labor market outcomes are branch of service and whether the military service is the result of being called up from the reserve. Veterans from different military branches tend to differ in skills trained and thus potentially perform differently in the subsequent civilian labor market. As for reserve status, activated reservists often have the option to return to previous employment after service. They would potentially have higher labor force attachment than those who were not reservists. To address the issue, I preform robustness checks by restricting the analysis sample to a group of veterans for whom the above concerns are less severe.

The correlation between TAP rollout timing and the service branch of the installation are shown in panel A of **Figure 9**, which separately displays for each military branch the number of installations that have TAP by the end of each fiscal year. A disproportionally large number of the Air Forces installations established TAP in 1991. In other years, the rollout at installation level has similar pattern across branches. Thus, if the rollout timing differences across different branches were instead driving the results, we would expect the estimated effects to be most pronounced in 1991.

For reserve status, I examine whether the reservists contribute to a large share of the analysis sample. **Figure 9**, panel B shows the proportion of veterans whose most recent active duty experience was the result of being called up from the Reserve/National Guard by veterans' year of separation. This information come from the veterans supplement to the CPS of years 2007, 2009, and 2010. The share of this type of veterans was low (around 5%) except for in 1991, the year of the Operation Desert Storm. Such pattern suggests that, if the outcome differences of reservists and non-reservists were instead driving the results, we would expect the estimated effect to be most pronounced in 1991.

Thus, one natural way to check if the unobserved characteristics are driving the result is to exclude from analysis sample veterans who separated in 1991. I repeat regression analysis with the restricted sample, and find that all estimation results are robust (**Table 10**). Findings support that, although having unobserved characteristics remains a limitation, branch of service and reserve status are unlikely driving the results.

7. Extensions

7.1 Do the effects of TAP differ across subgroups?

Thus far, all analysis focuses on the pooled sample of veterans of different lengths of service. This section turns to subgroup analysis for robustness checks. I separately estimate treatment effects for veterans with different lengths of military service: veterans who served for less or equal to four years and veterans who served for more than four years.

Table 11 shows results regarding labor market outcomes. Each cell reports the coefficient of TAP using the IV specification, following equations (2) and (3). Panel A reports the effects of TAP on labor force participation. For the veterans who served for four years or less, the effect is modest. For the veterans who served for longer than four years, the effect is of a significant 9.05 percentage points. Panel B shows the effects of TAP on work full-time. For veterans who served for four years or less, TAP increased the full-time worker status by 14.35 percentage points. For veterans who served for longer than four years, the effect is small and insignificant.

Overall, TAP improves the labor market performance for groups with varied service experience. However, the margin of response differs across groups: for veterans with shorter military service, TAP significantly increases the likelihood of being a full time worker. For

veterans with longer military service, who are closer to retirement age, TAP significantly increases labor force participation.

7.2. Do the effects of TAP differ over time?

The main analysis focuses on the effects of TAP measured approximately ten years after separation. This section focus on the short-term and the long-term effects. A consensus view in the literature on active labor market program evaluation is that long-run effect differ from short-run effect.[27] Anecdotally, how the effects of TAP would evolve over time is unclear. The direct gain from job search assistance workshop is potentially most effective right after separation. On the other hand, the indirect gain from using educational benefits takes time to realize.

To investigate this issue, I extend the analysis period by drawing information from the veterans supplement to the CPS of years 1995 to 2010. CPS follows the same sampling method to ensure that a nationally representative sample is surveyed each year. Such sample design allows me to focus on the analysis population – veterans separated from 1990 to 1993 - and track their labor market outcomes in each survey year. For each survey year, I follow an OLS specification as follows:

$$(5)\quad Y_{is} = \rho_0 + \rho_1 * TAP_{is} + \rho_2 * X_{is} + \rho_3 * D_{is} + \nu_s + \xi_{is}$$

The coefficient of interest, ρ_1, and corresponding 95 percent confidence intervals are plotted against survey years, shown in **Figure 10.** The graphical trend suggests that the effect of TAP was largest right after separation, and attenuated with time.

[27] See Card et al. (2010) and Kluve (2010) for comparisons across studies.

There are two caveats to the analysis. First, the point estimates from the OLS specification do not represent the causal effect of TAP due to the individual selection issue discussed earlier. Survey redesigns in 2001 and 2007 prevent me from consistently constructing the IV variable and thus impede the preferred IV specification as in the main analysis.[28] Nevertheless, the relative magnitude of the estimates for different survey years provide suggestive evidence regarding how the effects of TAP change over time. Second, CPS underreported TAP participation during 1995 to 1999. If veterans who benefited less from TAP were more likely to misreport, the OLS estimates during these years would overstate the true difference between participants and non-participants.

7.3 Subjective evaluation of TAP

This section turns to individuals' subjective evaluation of TAP. While the main analysis focuses on the observed outcome such as labor force participation, hours worked, and earning, how participants perceive the effect of the program has equally important policy implications. Participant satisfaction affects the decision to complete or drop out (Heckman and Smith 1998; Philipson and Hedges 1998). Moreover, government bureaucracies sometimes use participant subjective evaluations as evidence for program effectiveness.[29] Thus, understanding how participants value TAP and whether their subjective evaluation reflects their true outcomes is essential.

[28] Prior to 2001, veterans were not asked if they were ever offered TAP. Starting in 2007, veterans who did not participate in TAP were asked to choose from a list of reasons for non-participation. "Not offered" is one of the options. However, other options include "commanding officer did not support" or "not offered at base," which also reflect inaccessibility.

[29] In the context of TAP, participants' satisfaction is documented as evidence for its effectiveness: see Sadacca *et al.* (1995), Logistics Management Institute (1997), and GAO (2002)

I examine how TAP participants' evaluations of TAP relate to their true outcomes. Such analysis is facilitated by CPS's additional question to TAP participants asking them which aspect of the program they found most helpful. I classify participants' responses into four categories: most helpful for career related service; most helpful for offering VA benefits information; most helpful for other services; and not helpful.[30] For each possible category of response, I generate a dummy variable that equals one if the veteran's response falls into the category, equals zero if the veteran responded otherwise or did not participate in TAP. These dummy variables are used as indicators of subjective evaluation of TAP.

I estimate a linear probability model as follows:

$$(6) \qquad Y_{ist} = \theta_0 + \theta_1 * TAP\_offered_{ist} + \theta_2 * Subjective_{ist} + \theta_3 * X_{ist} + \theta_4 * D_{ist} + \lambda_s + \lambda_t + \varsigma_{ist}$$

The outcome, Y_{ist}, is labor force participation. The explanatory variables include the TAP offering dummy, full set of controls, and the subjective evaluation dummies, denoted as $Subjective_{ist}$. The coefficients of interest, θ_2, measures the extent to which a specific subgroup of participants, identified by their subjective evaluation type, contribute to the main result. **Table 12** presents the regression results. As shown in column 2 and column 6, the effect of TAP on labor force participation is concentrated among participants who report that they find the career related service most helpful.

[30] Responses coded as "Helpful: career related" include "Advice on JOB-SEARCH behavior and strategies" "Advice on ways to improve JOB interviewing skills" and "Advice on RESUME WRITING". Responses coded as "Helpful: VA benefits" include "Information on VETERANS BENEFITS" and "Information on availability of UNEMPLOYMENT BENEFITS". Responses coded as "Helpful: other" include "Importance of copying SERVICE and MEDICAL RECORDS" "Importance of developing a civilian mind-set" and other unspecified responses. Response coded as "Not helpful" includes "did not find any advice or information to be useful".

Findings provide suggestive evidence that veterans' evaluation of TAP is consistent with their observed outcomes. It is worth noting that this correlation could also reflect an individual's self-justification: veterans who do well in the labor market are more likely to report that they benefited from TAP. The self-justification issue is discussed in Smith *et al.* (2013), who find little relationship between Job Training Partnership Act participants' observed outcomes and self-evaluation of program impact. They argue that individuals may have cognitive difficulty to rationally predict what their outcomes would be absent of the program. Thus, the coefficients obtained in this section shall not be interpreted as causal.

8. Conclusion

In this study, I examine the long-run effects of the Transition Assistance Program on the civilian labor market outcomes of veterans. Using an IV strategy, I exploit variation in TAP availability during its rollout stage. The findings suggest that the program has been successful in improving veterans' labor market outcomes. Measured approximately 10 years after initial intervention, TAP increased veterans' labor force participation by 7.29 percentage points. One potential mechanism is that TAP increased veterans' use of VA educational benefits, namely the GI Bill. I find that TAP increased in veterans obtaining some college education by 14.96 percentage points, and increased their seeking education/training program after separation by 15.4 percentage points. Overall, the effectiveness of TAP in promoting transition to civilian life and long-run professional development is substantial.

Findings in this study highlight the necessity of the DOD's career assistance programs to the veteran population. The upcoming defense downsizing projects an increase in service member separation. The Department of Veterans Affairs projects that over 1 million service members would separate from active duty from 2014 through 2020 (GAO 2014). Their transition

and long-term developments remains a challenge to policy makers. In addition, DOD's career assistance programs are potential policy tools for quality recruitment. Borgschulte and Martorell (2013) show that low unemployment rate in the civilian market provide disincentive for reenlistment. The end-of-service career assistance system and its long-term benefits would make the military a more attractive career option.

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Appendix. Individual selection: OLS vs. 2SLS

As is the case in many program evaluation literature, individual selection into participation impedes causal interpretation of simple OLS estimates. Characteristics unobserved to researchers, such as cognitive ability or latent preference, are potentially jointly correlated with outcome of interest and participation decision. In this section, I investigate the nature of unobserved characteristics, to show how IV estimates are favored over OLS estimates.

The analysis is facilitated by additional questions asked in CPS to veterans who were offered TAP but chose not to participate. These subgroup is directly asked the particular reason for non-takeup. I record the reasons into binary dummies. As shown in Appendix table 1, column 1, the most cited reasons include having jobs available prior to separation, having alternative educational plans, and finding the program useless.

Column 2 shows the OLS estimates, which suggest that participants have a 5.16 higher labor force participation rate than non-participants. Column 3 includes a dummy variable indicating that the veteran reports having a job lined up as the reason for non-takeup. This subgroup potentially has a higher labor market attachment, absent of TAP. Failure to take into account such selection mechanism would lead to a downward bias. Including the dummy drives OLS upward, as expected. Column 4-6 perform similar analysis for other potential selection mechanisms. Column 7 includes a full set of dummies, and reports a 6.13 percentage point treatment effect.

Overall, the results suggests that OLS underestimates TAP treatment effects. This is consistent with the reported selection mechanisms.

Table A1: Individual belief: reasons for non-takeup; dependent variable: labor force participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Percentage distribution	OLS	OLS	OLS	OLS	OLS	OLS	IV
TAP		5.16	5.75	5.56	5.07	5.13	6.13	7.29
		(2.23)**	(2.28)**	(2.26)**	(2.25)**	(2.23)**	(2.35)***	(3.34)**
Reasons for non-takeup								
Have a job waiting	32.9		Y				Y	
College/training program	15.2			Y			Y	
Think it's a waste of time	16.5				Y		Y	
Did not know or find out too late	4.1					Y	Y	
Other reasons	35.9						Y	
# of obs	170	1342	1342	1342	1342	1342	1342	1342

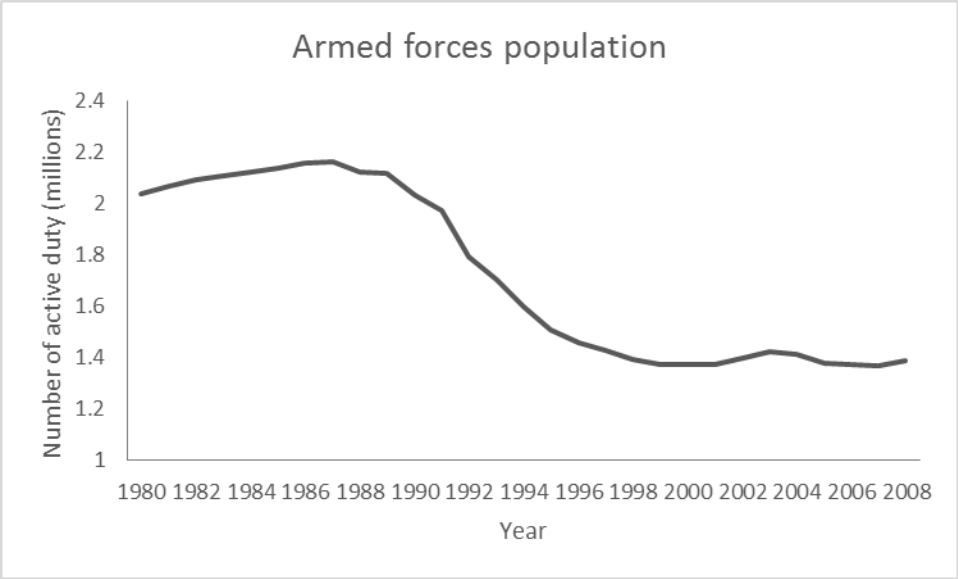
Notes: Sample analyzed include male veterans between age 26 and 64, who report year of separation between 1990 and 1993. Full set of controls are included. Other reasons include: conflict with mission, lack support from commanding officer, unavailable at current military base and unspecified reason. Robust standard errors are reported in parentheses. Estimates are factored by 100. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table A2: Military population by states

	Military population		Military population
California	167,098	North Dakota	7,840
Virginia	137,681	Nebraska	7,320
Texas	112,283	Connecticut	7,299
North Carolina	101,563	Ohio	7,086
Georgia	70,641	Utah	5,769
Florida	66,256	Arkansas	5,261
Washington	53,171	Idaho	4,665
Hawaii	44,068	Delaware	3,945
South Carolina	38,361	Montana	3,782
Kentucky	35,171	South Dakota	3,691
Illinois	26,702	Wyoming	3,447
Colorado	25,930	Maine	3,038
Maryland	25,842	Pennsylvania	2,947
Oklahoma	23,753	Rhode island	2,683
Arizona	22,215	Massachusetts	2,413
New York	18,779	Tennessee	2,335
Louisiana	17,724	Michigan	1,011
Alaska	17,601	Indiana	988
Kansas	16,811	New Hampshire	849
Mississippi	16,480	Minnesota	691
Missouri	15,138	Oregon	630
DC	13,776	Wisconsin	527
New Mexico	12,018	West Virginia	496
Alabama	10,295	Iowa	379
Nevada	9,364	Vermont	60
New Jersey	7,327		

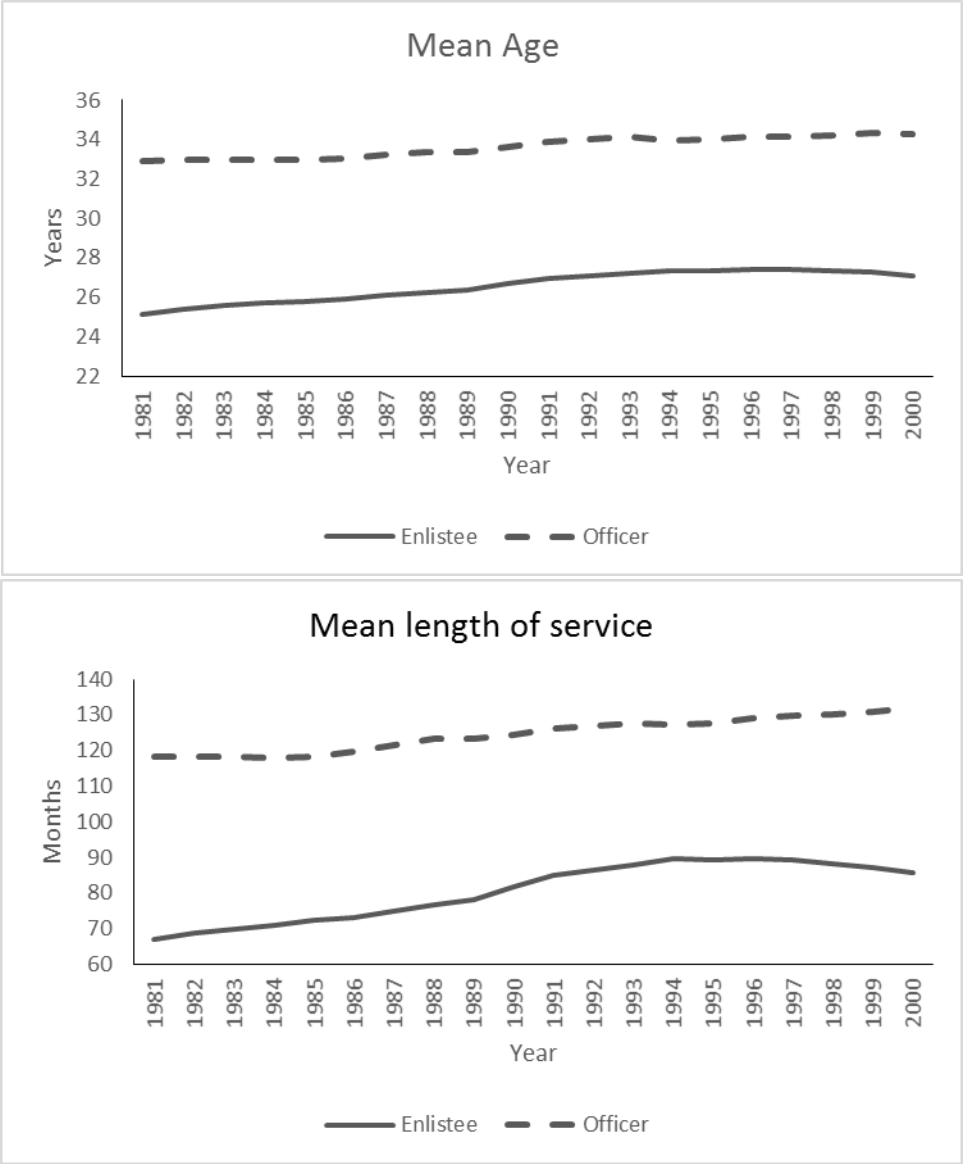
Source: DOD demographic reports, 2004

Figure 1-1: Active service population.



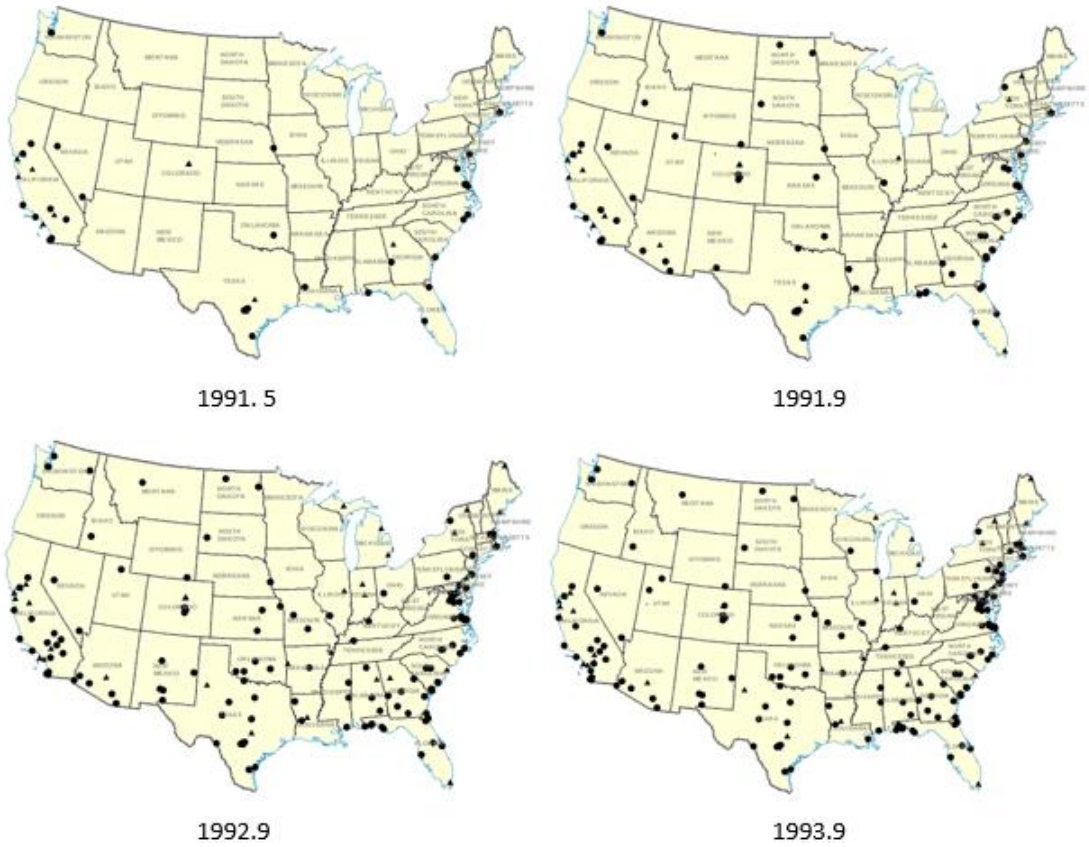
Source: Department of Defense, Defense Manpower Data Center

Figure 1-2: Characteristics of active service members.



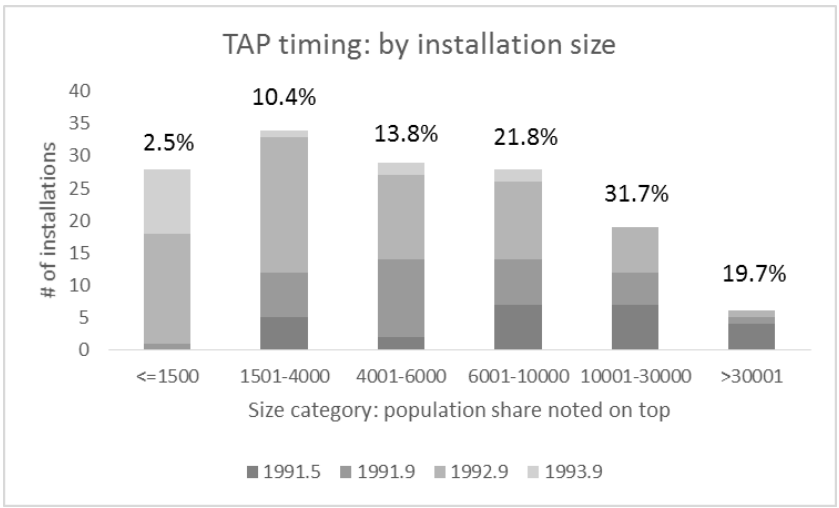
Source: Department of Defense, Demographic reports.

Figure 1-3: TAP rollout at installation level.



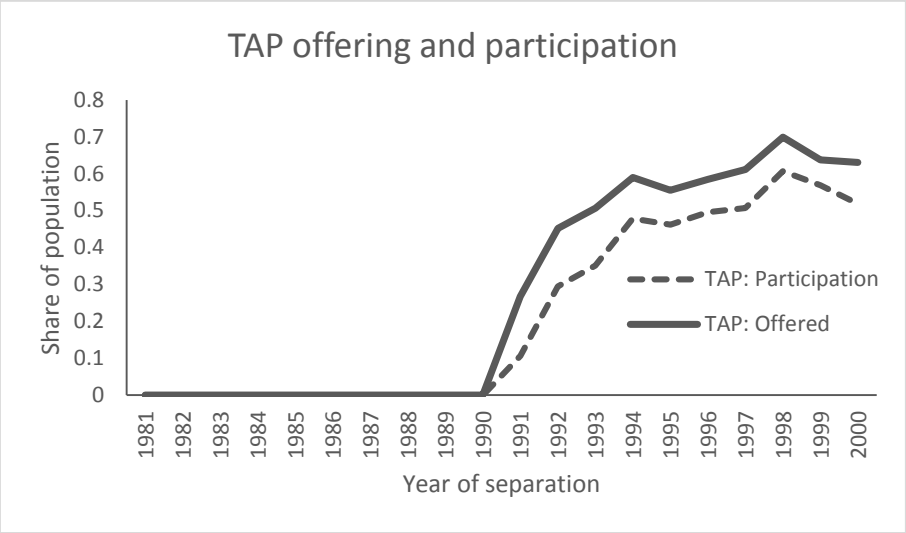
Source: Congressional Report, multiple years.

Figure 1-4: TAP rollout: by installation size.



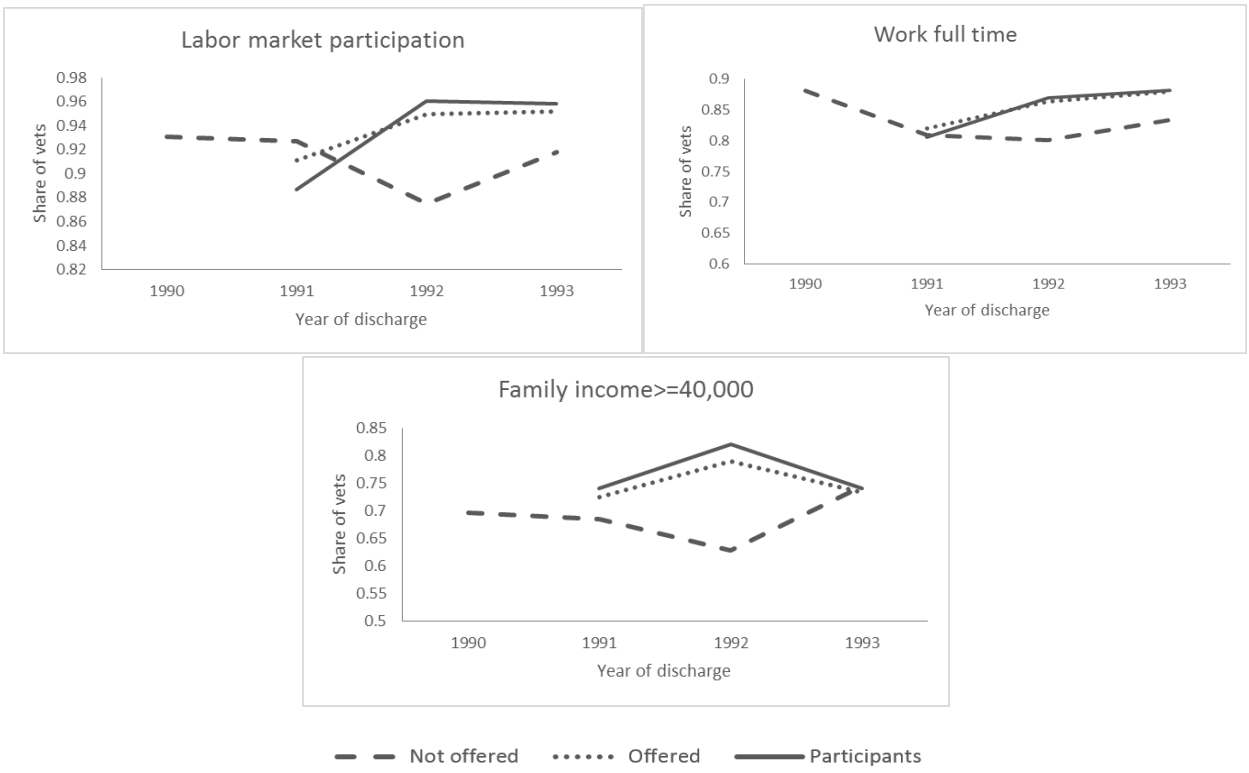
Source: Congressional Report, multiple years; DOD demographic report, 2005.

Figure 1-5: TAP availability and participation rate: by year of separation.



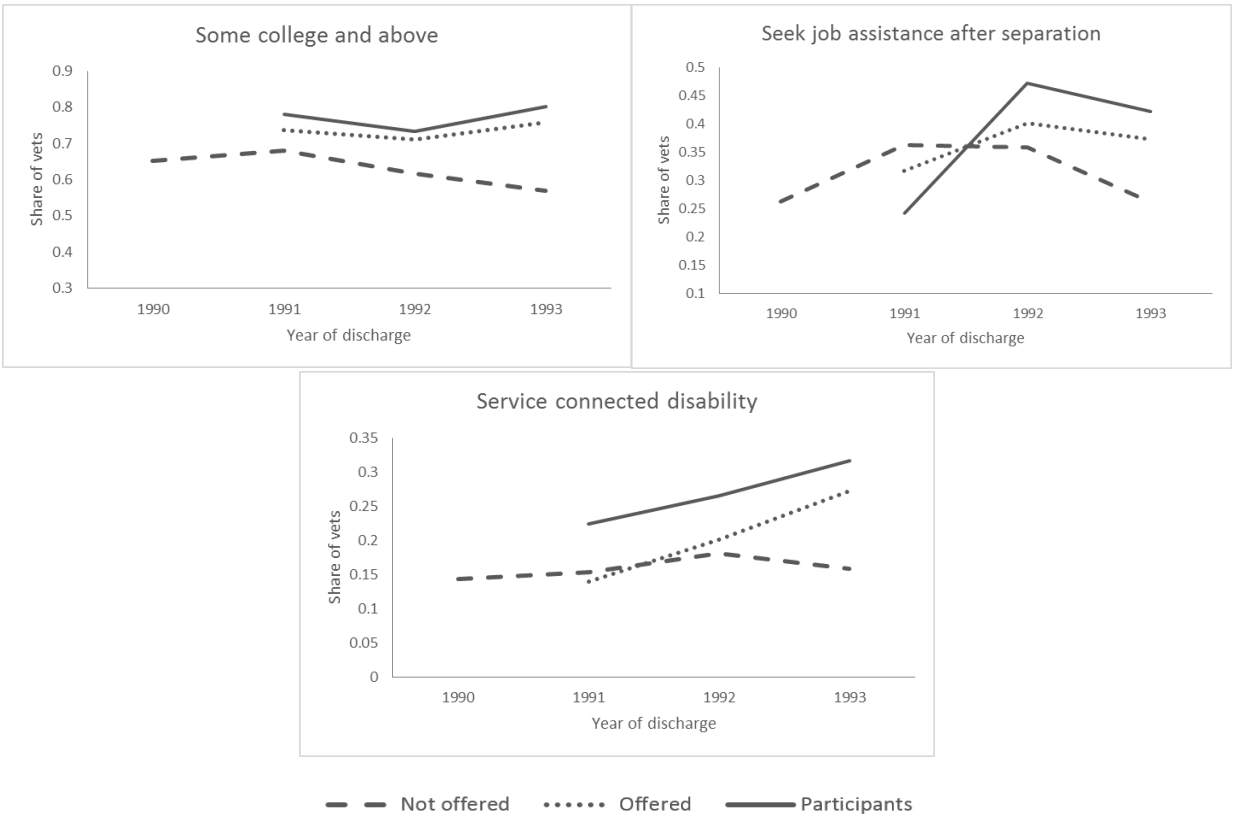
Source: CPS veteran supplements: 2001, 2003 and 2005. Sample includes all male veterans who reported year of separation, regardless of age.

Figure 1-6: Labor market outcomes, by TAP participation status.



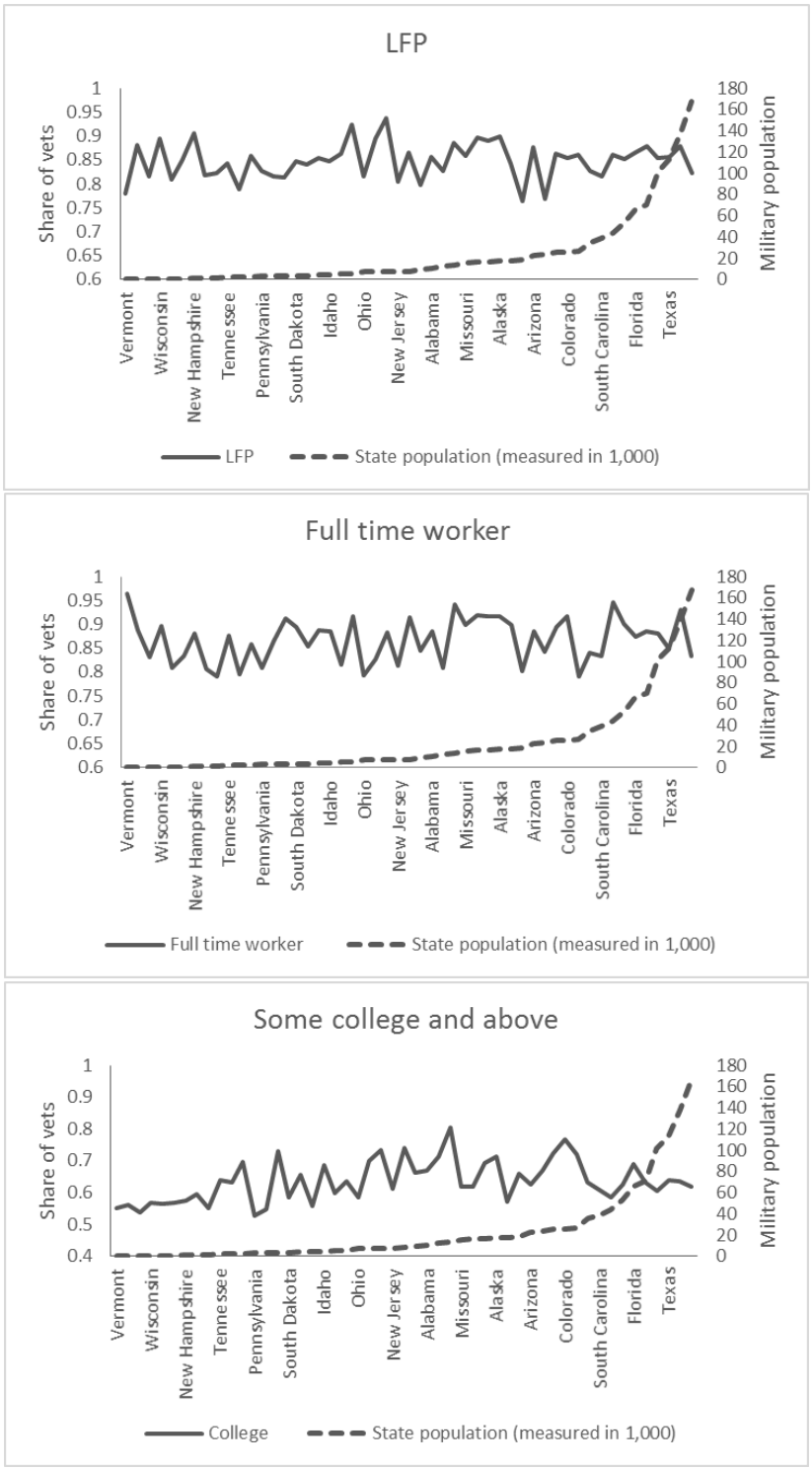
Source: Imputed use CPS veteran supplements, 2001, 2003 and 2005. Sample include male veterans between age 26 and 64, who report year of separation between 1981 and 2000. Supplement weights are used.

Figure 1-7: Other outcomes, by TAP participation status.



Source: Imputed use CPS veteran supplements, 2001, 2003 and 2005. Sample include male veterans between age 26 and 64, who report year of separation between 1981 and 2000. Supplement weights are used.

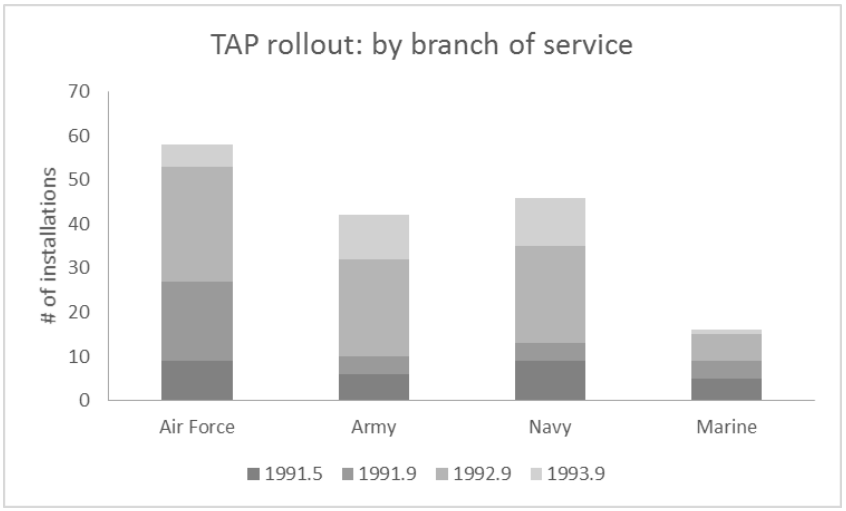
Figure 1-8: Outcomes by state of separation.



Notes: The solid lines show mean outcomes of veterans separated from the 50 states and DC. Data come from American Community Survey: 2003-2007. Sampled used for plotting these figures includes newly separated male veterans, whose ages are between 18 and 64 at the time of survey.
The dashed lines show state level military population. Data come from DOD demographic report, 2004.

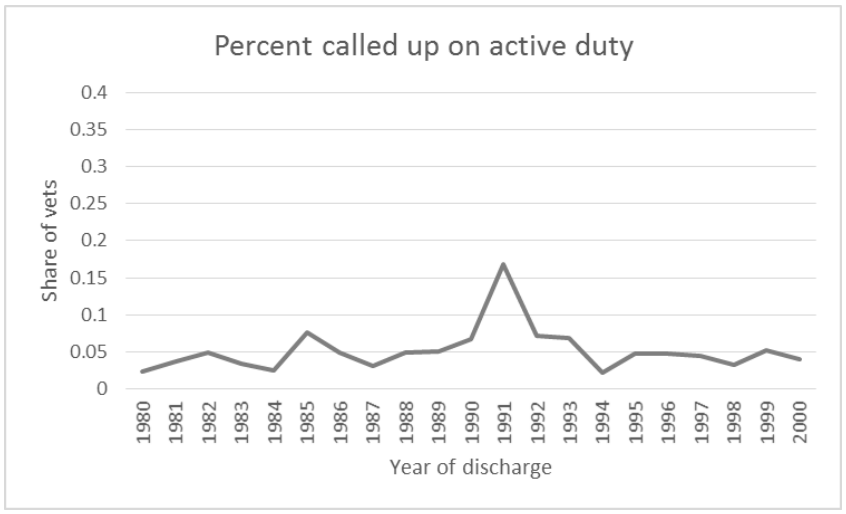
Figure 1-9: Trends of unobserved military characteristics.

A: TAP rollout pattern: by branch of service



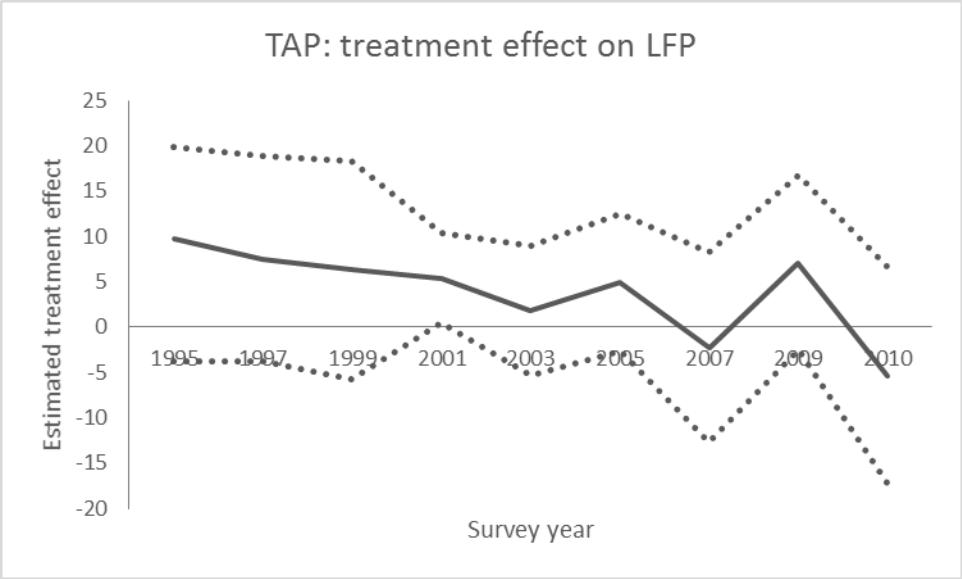
Source: Congressional Report, multiple years.

B: Share of reserves, by year of separation



Source: CPS veteran supplements, 2007, 2009 and 2010. Sample include male veterans between age 18 and 64, who report year of discharge between 1981 and 2000. Supplement weights are used.

Figure 1-10: Treatment effects by survey year.



Notes: The solid line plots the coefficients (factored by 100) of TAP participation of a linear probability model: labor force participation regressed on TAP participation and full set of controls (except for deployment history). Horizontal axis shows the survey years used (1995-2010). Dotted lines plot 95 percent confidence intervals. Supplement weights are used.

Table 1-1: Summary of characteristics of veterans, by year of separation

	1990	1991	1992	1993
Age at separation	27.63	28.16	28.69	28.9
	(8.07)	(8.22)	(8.25)	(8.64)
Other races	0.02	0.04	0.04	0.06
	(0.14)	(0.20)	(0.19)	(0.23)
Black	0.13	0.14	0.15	0.11
	(0.34)	(0.35)	(0.36)	(0.32)
HS or GED	0.97	0.98	0.99	0.99
	(0.17)	(0.14)	(0.11)	(0.10)
Born native	0.95	0.96	0.94	0.95
	(0.21)	(0.19)	(0.23)	(0.22)
Length of service<=4 years	0.59	0.61	0.52	0.51
	(0.49)	(0.49)	(0.50)	(0.5)
Length of service: 5-19 years	0.23	0.28	0.33	0.30
	(0.42)	(0.45)	(0.47)	(0.46)
Length of service>=20 years	0.18	0.11	0.16	0.19
	(0.38)	(0.31)	(0.36)	(0.40)
Deployment	0.33	0.43	0.49	0.50
	(0.47)	(0.50)	(0.50)	(0.50)
# obs.	267	363	392	325

Notes: pooled data from CPS vet supplement 2001, 2003, 2005. Observations restricted to male veterans between age 26 and 64. Standard deviations are reported in parentheses. Veteran supplement weights are used.

Table 1-2: Summary of characteristics by TAP status.

	Not offered	Offered			
		All	Participants	Non-takers	(2)-(1)
	(1)	(2)	(3)	(4)	(5)
Age	39.51	40.39	40.79	39.74	0.87
	(8.47)	(8.56)	(9.74)	(8.25)	(0.57)
Age at separation	27.78	29.63	30.21	28.72	1.84***
	(8.22)	(8.35)	(8.44)	(8.14)	(0.55)
Black	0.12	0.17	0.20	0.12	0.05*
	(0.32)	(0.38)	(0.40)	(0.33)	(0.026)
Other races	0.04	0.04	0.04	0.04	0.005
	(0.19)	(0.20)	(0.19)	(0.20)	(0.013)
High School/GED	0.98	0.99	1.00	0.99	0.016**
	(0.15)	(0.08)	(0.06)	(0.10)	(0.007)
Born native	0.95	0.95	0.95	0.93	-0.008
	(0.21)	(0.23)	(0.21)	(0.25)	(0.015)
Length of service<=4 years	0.62	0.43	0.38	0.51	-0.186***
	(0.49)	(0.50)	(0.49)	(0.5)	(0.034)
Length of service: 5-19 years	0.26	0.35	0.37	0.33	0.098***
	(0.44)	(0.48)	(0.48)	(0.47)	(0.032)
Length of service>=20 years	0.13	0.22	0.26	0.16	0.089***
	(0.33)	(0.44)	(0.44)	(0.36)	(0.025)
Deployment	0.41	0.52	0.55	0.47	0.113***
	(0.49)	(0.5)	(0.50)	(0.50)	(0.034)
# obs.	902	444	269	175	

Notes: Sample used is pooled CPS veterans supplement in 2001, 2003 and 2005. Observations restricted to male veterans between age 26 and 64, whose reported year of separation is between 1990 and 1993. Standard deviations are reported in parentheses. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table 1-3: Linear probability model. Dependent variable: labor force participation

	(1)	(2)	(3)	(4)	(5)	(6)
A: OLS						
TAP	3.24*	4.58**	5.27**	5.47**	5.19**	5.16**
	(1.92)	(2.22)	(2.14)	(2.15)	(2.24)	(2.23)
B: IV						
TAP	4.75*	6.47**	7.22**	7.31**	7.31**	7.29**
	(2.49)	(3.29)	(3.22)	(3.24)	(3.34)	(3.34)
Age			3.15**	3.00**	3.47**	3.49**
			(1.23)	(1.24)	(1.37)	(1.37)
Age squared			-0.04***	-0.04***	-0.05***	-0.05***
			(0.01)	(0.01)	(0.02)	(0.02)
Black				-4.53	-4.41	-4.37
				(3.37)	(3.40)	(3.41)
Other races				3.54	4.28	4.27
				(3.47)	(3.42)	(3.42)
HS/GED				18.85	17.55	17.56
				(11.97)	(12.11)	(12.1)
Born native				-2.10	-1.24	-1.28
				(3.77)	(3.75)	(3.75)
Deployed						0.63
						(1.68)
<u>Length of service</u>						
6 months to 2 years					-1.12	-1.16
					(4.30)	(4.33)
2 to 3 years					-3.68	-3.82
					(3.88)	(3.95)
3 to 4 years					-1.72	-1.92
					(3.79)	(3.90)
5 to 9 years					-4.17	-4.36
					(4.27)	(4.36)
10 to 14 years					-4.34	-4.53
					(5.82)	(5.86)
15 to 19 years					5.15	4.96
					(7.16)	(7.27)
20 years and over					3.18	2.97
					(5.81)	(5.94)
<u>Year of separation</u>						
1991		-1.05	-0.97	-1.14	-0.60	-0.66
		(2.59)	(2.48)	(2.49)	(2.47)	(2.50)
1992		-3.01	-3.44	-3.82	-3.70	-3.79
		(2.89)	(2.86)	(2.88)	(2.97)	(3.00)
1993		-0.62	-1.00	-1.66	-1.79	-1.88
		(2.79)	(2.78)	(2.79)	(2.81)	(2.82)
State & survey year FEs	N	Y	Y	Y	Y	Y
# of obs	1347	1347	1347	1347	1342	1342

Notes: Sample analyzed include male veterans between age 26 and 64, who report year of separation between 1990 and 1993. Robust standard errors are reported in parentheses. Estimates are factored by 100. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table 1-4: Linear probability model. Dependent variable: Usually work full-time

	(1)	(2)	(3)	(4)	(5)	(6)
A: OLS						
TAP	3.03	4.16	4.80	4.85	4.27	4.05
	(2.79)	(3.06)	(2.93)	(2.96)	(3.00)	(3.00)
B: IV						
TAP	4.56	7.35	7.89	7.79*	7.61*	7.47*
	(3.91)	(4.50)	(4.31)	(4.34)	(4.46)	(4.45)
Age			4.91	4.68***	5.59***	5.71***
			(1.56)	(1.59)	(1.69)	(1.68)
Age squared			-0.06***	-0.06	-0.08	-0.08***
			(0.02)	(0.02)***	(0.02)***	(0.02)
Black				-3.63	-3.56	-3.23
				(3.80)	(3.82)	(3.85)
Other races				-4.60	-4.01	-4.09
				(5.69)	(6..94)	(6.91)
HS/GED				18.74	16.64	16.76
				(11.90)	(12.18)	(11.99)
Born native				-6.23	-5.60	-5.86
				(5.69)	(5.74)	(5.76)
Deployed						4.45**
						(2.26)
<u>Length of service</u>						
6 months to 2 years					3.94	3.66
					(7.51)	(7.53)
2 to 3 years					2.46	1.51
					(6.64)	(6.69)
3 to 4 years					1.77	0.32
					(6.49)	(6.55)
5 to 9 years					-1.84	-3.19
					(6.76)	(6.83)
10 to 14 years					-0.89	-2.19
					(8.09)	(8.14)
15 to 19 years					13.31	11.96
					(9.01)	(9.10)
20 years and over					11.13	9.67
					(7.93)	(8.01)
<u>Year of separation</u>						
1991		-6.83**	-6.59**	-6.54**	-5.97*	-6.39*
		(3.40)	(3.25)	(3.25)	(3.26)	(3.28)
1992		-6.43*	-6.62*	-6.88**	-6.83*	-7.47**
		(3.52)	(3.44)	(3.46)	(3.48)	(3.51)
1993		-4.43	-4.34	-4.64	-4.85	-5.47
		(3.71)	(3.61)	(2.79)	(3.63)	(3.63)
State & survey year FEs	N	Y	Y	Y	Y	Y
# of obs	1347	1347	1347	1347	1342	1342

Notes: Sample analyzed include male veterans between age 26 and 64, who report year of separation between 1990 and 1993. Robust standard errors are reported in parentheses. Estimates are factored by 100. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table 1-5: Linear probability model. Dependent variable: Family annual income>=$40,000

	(1)	(2)	(3)	(4)	(5)	(6)
A: OLS						
TAP	8.50**	9.49**	9.52**	9.70***	8.69**	8.56**
	(3.48)	(3.76)	(3.74)	(3.69)	(3.68)	(3.68)
B: IV						
TAP	11.51**	13.96**	14.24**	14.19**	12.47**	12.01**
	(4.95)	(5.61)	(5.63)	(5.60)	(5.74)	(5.77)
Age			-2.01	-2.41	-1.60	-1.19
			(1.80)	(1.83)	(1.95)	(1.91)
Age squared			0.02	0.03	0.02	0.02
			(0.02)	(0.02)	(0.02)	(0.02)
Black				-7.42	-7.77	-7.61
				(4.73)	(4.66)*	(4.68)
Other races				-4.97	-5.92	-5.77
				(7.14)	(6.93)	(6.93)
HS/GED				34.87***	34.89***	35.48***
				(11.55)	(11.14)	(11.16)
Born native				-9.34	-9.87	-9.37
				(7.17)	(6.68)	(6.61)
Deployed						2.50
						(3.01)
<u>Length of service</u>						
6 months to 2 years					2.52	3.01
					(10.25)	(10.31)
2 to 3 years					18.54**	18.88**
					(9.38)	(9.45)
3 to 4 years					12.86	12.31
					(9.21)	(9.33)
5 to 9 years					8.59	7.74
					(9.42)	(9.56)
10 to 14 years					4.16	2.84
					(11.00)	(11.11)
15 to 19 years					14.39	13.65
					(12.43)	(12.45)
20 years and over					11.58	10.13
					(10.36)	(10.57)
<u>Year of separation</u>						
1991		-1.55	-1.75	-1.76	-2.09	-2.12
		(4.35)	(4.37)	(4.35)	(4.35)	(4.37)
1992		-3.48	-3.78	-4.31	-3.64	-3.4
		(4.67)	(4.70)	(4.69)	(4.66)	(4.68)
1993		-0.59	-1.15	-1.88	1.14	-0.48
		(4.66)	(4.77)	(4.77)	(4.79)	(4.82)
State & survey year FEs	N	Y	Y	Y	Y	Y
# of obs	1347	1347	1347	1347	1342	1342

Notes: Sample analyzed include male veterans between age 26 and 64, who report year of separation between 1990 and 1993. Robust standard errors are reported in parentheses. Estimates are factored by 100. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table 1-6: Linear probability model. Dependent variable: sought education/training after separation

	(1)	(2)	(3)	(4)	(5)	(6)
A: OLS						
TAP	11.48***	14.07***	14.88***	15.09***	15.11***	15.02***
	(3.85)	(4.09)	(4.05)	(4.05)	(4.06)	(4.06)
B: IV						
TAP	11.18**	13.83**	14.95***	14.93***	15.45***	15.40***
	(5.06)	(5.73)	(5.70)	(5.72)	(5.77)	(5.76)
Age			0.73	0.49	0.08	0.15
			(1.62)	(1.62)	(1.73)	(1.74)
Age squared			-0.01	-0.01	-0.01	-0.01
			(0.02)	(0.02)	(0.02)	(0.02)
Black				-3.77	-3.92	-3.77
				(4.54)	(4.45)	(4.45)
Other races				-3.09	-3.48	-3.5
				(6.41)	(6.56)	(6.55)
HS/GED				13.82*	15.32*	15.37**
				(8.33)	(8.51)	(8.41)
Born native				-12.20	-11.50	-11.61
				(7.51)	(7.47)	(7.48)
Deployed						1.94
						(2.94)
<u>Length of service</u>						
6 months to 2 years					0.87	0.73
					(10.68)	(10.70)
2 to 3 years					-1.71	-2.14
					(10.30)	(10.32)
3 to 4 years					2.17	1.52
					(9.90)	(9.92)
5 to 9 years					6.26	5.67
					(9.97)	(10.01)
10 to 14 years					-11.45	-12.02
					(10.37)	(10.35)
15 to 19 years					6.26	5.67
					(13.63)	(13.54)
20 years and over					-5.83	-6.43
					(10.81)	(10.80)
<u>Year of separation</u>						
1991		2.76	2.59	2.73	2.53	2.35
		(4.22)	(4.22)	(4.22)	(4.23)	(4.23)
1992		1.03	0.28	0.04	0.50	0.22
		(4.35)	(4.33)	(4.32)	(4.31)	(4.32)
1993		-4.48	-5.60	-5.93	-5.54	-5.82
		(4.68)	(4.69)	(4.70)	(4.71)	(4.74)
State & survey year FEs	N	Y	Y	Y	Y	Y
# of obs	1340	1340	1340	1340	1336	1336

Notes: Sample analyzed include male veterans between age 26 and 64, who report year of separation between 1990 and 1993. Robust standard errors are reported in parentheses. Estimates are factored by 100. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table 1-7: Linear probability model. Dependent variable: some college and above

	(1)	(2)	(3)	(4)	(5)	(6)
A: OLS						
TAP	12.58***	12.10***	10.90***	10.10***	10.22***	9.98***
	(3.52)	(3.70)	(3.72)	(3.69)	(3.73)	(3.73)
B: IV						
TAP	15.94***	16.84***	14.91***	14.83***	15.11***	14.96***
	(5.04)	(5.59)	(5.61)	(5.64)	(5.77)	(5.75)
Age			2.49	2.38	2.84	2.97
			(1.77)	(1.77)	(1.89)	(1.87)
Age squared			-0.02	-0.02	-0.03	-0.03
			(0.02)	(0.02)	(0.02)	(0.02)
Black				-0.22	-0.12	0.23
				(4.72)	(4.73)	(4.75)
Other races				-9.14	-9.00	-9.08
				(7.09)	(7.07)	(7.09)
Born native				-4.42	-4.54	-4.81
				(7.04)	(7.15)	(7.16)
Deployed						4.81
						(2.96)
<u>Length of service</u>						
6 months to 2 years					-3.21	-3.51
					(10.34)	(10.30)
2 to 3 years					-0.18	-1.21
					(9.75)	(9.73)
3 to 4 years					-3.90	-5.46
					(9.41)	(9.38)
5 to 9 years					-3.51	-4.97
					(9.60)	(9.59)
10 to 14 years					-0.63	-2.03
					(10.56)	(10.56)
15 to 19 years					-8.65	-10.10
					(13.05)	(13.01)
20 years and over					-3.50	-5.07
					(10.26)	(10.26)
<u>Year of separation</u>						
1991		2.59	3.16	3.40	3.57	3.11
		(4.33)	(4.30)	(4.31)	(4.34)	(4.35)
1992		-5.50	-3.94	-3.83	-3.53	-4.23
		(4.68)	(4.69)	(4.68)	(4.74)	(4.73)
1993		-4.86	-2.48	-2.15	-2.26	-2.92
		(4.96)	(5.05)	(5.05)	(5.07)	(5.09)
State & survey year FEs	N	Y	Y	Y	Y	Y
# of obs	1347	1347	1347	1347	1342	1342

Notes: Sample analyzed include male veterans between age 26 and 64, who report year of separation between 1990 and 1993. Robust standard errors are reported in parentheses. Estimates are factored by 100. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table 1-8: Linear probability model. Dependent variable: service-connected disability

	(1)	(2)	(3)	(4)	(5)	(6)
A: OLS						
TAP	13.07***	12.02***	10.22***	10.07***	8.43**	8.15**
	(3.53)	(3.82)	(3.72)	(3.74)	(3.76)	(3.79)
B: IV						
TAP	9.18**	6.93	4.23	4.01	1.04	0.83
	(4.38)	(4.85)	(4.79)	(4.82)	(4.93)	(4.92)
Age			0.22	0.27	0.11	0.27
			(1.51)	(1.50)	(1.53)	(1.52)
Age squared			0.01	0.01	0.01	0.01
			(0.02)	(0.02)	(0.02)	(0.02)
Black				3.18	2.66	3.11
				(3.74)	(3.80)	(3.78)
Other races				-3.63	-3.16	-3.28
				(5.46)	(5.39)	(5.43)
HS/GED				-6.35	-8.04	-7.87
				(8.36)	(8.48)	(8.23)
Born native				-1.87	-1.61	-1.96
				(5.22)	(5.17)	(5.19)
Deployed						6.05**
						(2.52)
<u>Length of service</u>						
6 months to 2 years					4.85	4.47
					(4.67)	(4.70)
2 to 3 years					6.39	5.1
					(4.46)	(4.43)
3 to 4 years					8.68**	6.71
					(4.18)	(4.21)
5 to 9 years					10.31**	8.47*
					(4.78)	(4.81)
10 to 14 years					10.27	8.5
					(6.57)	(6.42)
15 to 19 years					24.92**	23.1**
					(9.79)	(9.68)
20 years and over					25.43***	23.45***
					(6.75)	(6.72)
<u>Year of discharge</u>						
1991		-1.12	-0.64	-0.50	0.35	-0.23
		(3.34)	(3.20)	(3.20)	(3.20)	(3.18)
1992		0.32	2.35	2.54	2.44	1.54
		(3.57)	(3.50)	(3.50)	(3.54)	(3.55)
1993		3.10	6.02	6.43*	5.98	5.12
		(3.82)	(3.77)	(3.74)	(3.79)	(3.80)
State & survey year FEs	N	Y	Y	Y	Y	Y
# of obs	1340	1340	1340	1340	1335	1335

Notes: Sample analyzed include male veterans between age 26 and 64, who report year of separation between 1990 and 1993. Robust standard errors are reported in parentheses. Estimates are factored by 100. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table 1-9: Correlation between state military population and outcomes of interest

	Labor force participation		Usually work full-time		Some college and above	
	(1)	(2)	(3)	(4)	(5)	(6)
Population	0.003	-0.002	0.009	-0.001	-0.002	-0.004
	(0.01)	(0.009)	(0.02)	(0.02)	(0.01)	(0.01)
Age		3.43***		4.39***		6.49***
		(0.27)		(0.21)		(0.29)
Age squared		-0.05***		-0.06***		-0.08***
		(0.003)		(0.003)		(0.003)
White		6.72***		3.59***		-1.92
		(1.20)		(1.23)		(1.93)
Black		5.73***		-0.04		-6.57***
		(1.60)		(1.92)		(2.01)
Year dummy		Y		Y		Y
Lengths of service		Y		Y		Y
R squared	0.00	0.04	0.00	0.08	0.00	0.07
# obs	19096	19096	19096	19096	19096	19096

Note: Data source: American Community Survey 2003-2007. Sample include male veterans between age 18 and 64, who report been on active duty in the past year. Population variable denotes state level military population in 2004, measured in 1,000. Length of service is a dummy indicating if the individual served for longer than 2 years in active force. Standard errors are clustered by state of residence, and reported in parentheses. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table 1-10: Linear probability model: IV. Sample excludes veterans discharged in 1991.

Outcome	(1) LFP	(2) Full time	(3) College	(4) Sought training/education	(5) Service-connected disability
TAP	8.56**	8.02*	15.59**	19.74***	5.53
	(3.47)	(4.64)	(6.16)	(5.99)	(5.38)
Age	3.85**	5.09**	1.54	-0.19	-1.04
	(1.70)	(2.03)	(2.27)	(1.84)	(1.88)
Age squared	-0.05**	-0.07***	-0.01	-0.01	0.02
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Black	-2.76	-2.31	1.36	-6.00	2.09
	(3.92)	(4.43)	(5.73)	(5.37)	(4.52)
Other races	3.94	-7.12	-9.32	-8.91	-0.79
	(4.58)	(8.42)	(8.58)	(7.34)	(6.63)
HS/GED	7.73	2.60		5.88	-17.57*
	(12.42)	(11.88)		(10.76)	(10.62)
Born native	-2.58	-7.36	-9.04	-6.60	-0.80
	(4.45)	(6.60)	(7.99)	(7.75)	(5.78)
Deployed	0.85	2.65	6.32*	0.90	2.83
	(2.03)	(2.63)	(3.57)	(3.46)	(3.06)
<u>Length of service</u>					
6 months to 2 years	0.90	7.07	-8.38	-4.94	2.63
	(5.21)	(8.82)	(11.60)	(11.58)	(5.80)
2 to 3 years	-2.06	6.98	1.20	1.87	3.96
	(4.37)	(7.33)	(10.69)	(11.07)	(5.21)
3 to 4 years	0.17	7.87	-6.46	-0.86	5.47
	(4.40)	(7.18)	(10.25)	(10.42)	(4.86)
5 to 9 years	-4.55	-0.10	-1.95	1.11	8.86
	(4.96)	(7.62)	(10.53)	(10.61)	(5.65)
10 to 14 years	-4.29	-0.89	6.56	-12.72	8.56
	(7.18)	(9.52)	(11.49)	(11.21)	(7.63)
15 to 19 years	3.76	15.70	-5.78	17.37	27.85**
	(7.95)	(9.82)	(15.07)	(15.38)	(11.10)
20 years and over	2.73	12.35	-2.64	-6.11	22.80***
	(6.65)	(8.93)	(11.67)	(11.60)	(8.14)
State & survey year FEs	Y	Y	Y	Y	Y
Year of separation	Y	Y	Y	Y	Y
R squared	0.123	0.101	0.116	0.111	0.147
# of obs	980	980	980	976	973

Notes: Sample analyzed include male veterans between age 26 and 64, who report year of separation between 1990, 1992, and 1993. Robust standard errors are reported in parentheses. Estimates are factored by 100. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table 1-11: Labor market outcomes: by lengths of service

	<=4 years	> 4 years
	(1)	(2)
	2SLS	2SLS
A: Labor force participation		
TAP	3.64	9.05*
	(4.39)	(5.10)
B: Usually work full time		
TAP	14.35**	1.28
	(6.22)	(6.37)
# of obs	715	627

Notes: Sample analyzed include male veterans between age 26 and 64, who report year of separation between 1990 and 1996. Full set of controls are included. Robust standard errors are reported in parentheses, clustered by year of discharge. Estimates are factored by 100. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Table 1-12: Individual belief: find most helpful; Dependent variable: labor force participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share of participants	ITT	ITT	ITT	ITT	ITT	ITT
TAP: offered		4.25**	2.69	3.34	3.74*	4.61**	2.38
		(2.00)	(2.17)	(2.19)	(2.04)	(1.99)	(2.22)
Helpful: work related	45.6%		6.00***				5.23**
			(2.21)				(2.42)
Helpful: VA benefits	47.7%			3.22			1.91
				(2.96)			(3.20)
Helpful: Other	33.4%				2.67		0.85
					(2.93)		(3.28)
Not helpful	12.6%					-5.09	-2.69
						(6.15)	(6.29)
# of obs	270	1342	1342	1342	1342	1342	1342

Notes: Sample analyzed include male veterans between age 18 and 64, who report year of discharge between 1990 and 1993. Full set of controls are included. Standard errors are reported in parentheses, clustered by year of discharge. Helpful: other indicate that individual report TAP as being helpful in: prepare service and medical records; develop civilian mind-set; and unspecified reasons. Estimates are factored by 100. Veteran supplement weights are used. *, ** and *** indicate significant at ten, five and one percent level, respectively.

Chapter 2: Medicaid Eligibility, Insurance Coverage, and Birth Outcomes: Evidence from an Age Discontinuity in Eligibility Rule

1. Introduction

Since its inception in 1965, Medicaid serves as a part of the social safety net. The purpose of the program is to improve health conditions among low-income population by providing medical services at no cost. Literature find positive health effects for different target populations: newborn (Currie and Gruber 1996a), children (Currie and Gruber 1996b, Currie, Decker and Lin 2008), and individuals with chronic diseases (Cuellar and Markowitz 2007).

In addition to knowing the overall effectiveness, policymakers are equally interested in mechanisms through which Medicaid affect health outcomes. This question is not trivial because public provision of health insurance may lead to behavior responses on multiple dimensions. By lowering the cost of medical services, Medicaid may directly increase beneficiaries' use of care. At the same time, Medicaid may have an indirect effect on health by freeing resources previously used to pay for medical care. This would increase consumption for other goods. Additionally, public provision of health insurance may provide a disincentive to take up private health insurance, which on average provides medical care of higher quality (Currie and Gruber 2001, Decker 2007).

In this paper, I provide a more complete analysis of the behavior responses to Medicaid provision. The focus of this study is one of the most important target population of Medicaid: pregnant women. Particularly, I investigate the linkages between Medicaid availability, insurance coverage status, prenatal care utilization and health outcomes of newborns.

To establish causal relationships, I exploit variation in Medicaid eligibility across different demographic groups. Before early 1980s, pregnant women were entitled to Medicaid primarily through being beneficiaries of AFDC, which is a cash benefit program for families with dependent children. The eligibility scheme created discontinuity in Medicaid eligibility across different age groups: a first time pregnant woman may age out of Medicaid when she turns 18. Such discontinuity was eliminated by two federal expansions: the Deficit Reduction Act of 1984 (DRA84) and the Consolidated Budget Reconciliation Act of 1985 (COBRA85). The mandates untied Medicaid eligibility from AFDC, thus potentially increased Medicaid eligibility to first time pregnant women whose ages were above 18.

Empirically, I employ a difference-in-differences method. The treatment group includes pregnant women immediately affected by the expansions (18 or 19 year olds). The control group includes pregnant women not immediately affected by the expansions (16 or 17 year olds). The causal effects of the policy changes are identified as changes in outcome differences between the treatment group and the control group.

I use data come from two sources: the National Hospital Discharge Survey provides health insurance coverage status at delivery. The Birth Records from the National Vital Statistics System provide information regarding prenatal care use and the health outcomes of the baby. Both datasets contain demographic characteristics of the mother, which facilitate the implementation of the empirical strategy.

I obtain three main findings. First, the policy changes affected the insurance coverage status of women at delivery. The treated group significantly increased Medicaid coverage by 6.5 percentage points as the consequence of the policy changes. However, up to 60% of the new beneficiaries would have been covered by private insurance, absent of the Medicaid expansions.

Such crowd-out effect is more pronounced among the whites. For the blacks, the reduction in private insurance is small and insignificant.

Second, for the whole sample, the treatment group did not exhibit an increase in use of medical services, which is measured by early access to prenatal care. The policy changes also seem to have little effect on the birth outcome of babies, which is measured by the prevalence of low birth weight. Such insignificant effects are found for both whites and blacks.

Third, for the subsample of pregnant women who were giving birth for the first time – the subgroup most directly affected by the policy changes – the treatment effects on prenatal care use and prevalence of low birth weight varied across racial groups. For the whites, who most likely shifted from private insurance to Medicaid, the policy changes led to a small decrease in medical care use and worse health outcomes. For the blacks, who most likely shifted from no insurance to Medicaid, the policy changes led to a small increase in medical care use and better health outcomes.

This study is closely related to two papers. The pioneer work by Currie and Gruber (1996a) looks directly at the impact of Medicaid expansion on birth outcomes. Utilizing state level variation in eligibility expansion, they find that early Medicaid expansions, which targets at more disadvantaged individuals, led to a substantial decrease in the incidence of low birth weight. Dave et.al (2010) used a similar approach as Currie and Gruber (1996a), and focus on insurance coverage of pregnant female at the time of delivery. Their findings suggest that the Medicaid expansions increased Medicaid coverage, but at the same time reduced private coverage by at least 55%. They argue that the substantial crowd-out may suggest limited health improvement effects for birth outcome or maternal health. However, they did not provide further evidence to document changes in care utilization or health outcomes.

This study adds to the literature in two ways. First, the variation I use comes from federal level policy changes that affected individuals in all states at the same time, which avoids the potential concern of state level policy endogeneity. Second, I jointly examine the effect of Medicaid availability on insurance coverage status, use of medical care and health outcomes of the targeted group. Such distinctions are important to understand the channels through which Medicaid provision affects health outcomes, and to understand the cost associated with Medicaid provision.

2. Backgrounds

2.1 Prenatal care and birth

Receiving adequate and timely prenatal care is vital to the well-being of pregnant female and the health of the newborns. Typically, the recommended prenatal care lasts over the course of pregnancy, and consists of doctor visits on a regular basis. During these prenatal care visits, physicians do regular checkup to screen for potential birth defects, monitor fetus growth condition, and provide information and advice for dos and don'ts for daily lifestyle. Medical studies suggest that prenatal care is effective in improving the health condition of the newborns: babies of mothers who do not receive prenatal care are three times more likely to be of low birth weight.[31]

Prenatal care is associated with substantial costs. According to the Kaiser Family Foundation, the average total cost for typical prenatal care over the pregnancy period is around $2,000.[32] The cost is often covered by mother's health insurance.[33] However, if one was

[31] http://www.womenshealth.gov/publications/our-publications/fact-sheet/prenatal-care.html#a
[32] http://kaiserfamilyfoundation.files.wordpress.com/2013/01/7636.pdf
[33] One thing worth noting is that maternity related medical service are mandated by Pregnancy Discrimination Act of 1978 to be covered by group insurance for employees and spouses. However, such mandates do apply to

uninsured at the time of pregnancy, obtaining prenatal care can be a financial burden. Obtaining private individual health insurance plan after pregnancy would be potentially difficult, as pregnancy can be legally treated as preexisting condition. This means that insurance companies can refuse to reimburse cost associated with prenatal visits. For the disadvantaged population who are likely uninsured, obtaining timely prenatal care can be a financial burden to individuals from disadvantaged backgrounds.

2.2 Legislative history: Medicaid expansions

To address the concern that female of disadvantaged backgrounds lack necessary care, the federal and state governments expanded the scope of public assistance. In particular, the assistance is provided to low-income pregnant female through Medicaid. Medicaid provides beneficiaries with comprehensive medical services including prenatal care services, medical services delivered during delivery and the necessary care after delivery. Importantly, these services are provided to beneficiaries at no cost.

At its inception in 1965, Medicaid was designed as an add-on program to other social welfare programs. In the context of maternity relevant population, most individuals were qualified for Medicaid through being recipients of AFDC, which provides cash benefits to low-income families with dependent children. Two sets of eligibility rules apply for AFDC: the categorical eligibility and income/resource eligibility. To meet the categorical eligibility requirement, one must be in a family with one or more dependent children, of age under 18.[34] To

dependent coverage. If one is covered by her parents' insurance, she will potentially be denied coverage for prenatal care and delivery. Although official data not available, Mercer's performance audit group estimates that 70% of group insurance excludes such coverage.

[34] AFDC traditionally targets at single parent households. The AFDC-UP program also covers two parents' households (married or unmarried) with the head of the household unemployed. The income eligibility threshold varies for this type of eligibility. For the purpose of this study, I do not distinguish single headed households and two-parent households.

meet the income eligibility requirement, the household income must be below a certain threshold. Such thresholds are set at state level to adjust for varied costs of living.

The eligibility rules made it difficult for some low-income pregnant women to gain timely access to necessary medical care. In particular, a first time pregnant female may be deemed disqualified for AFDC, if there is no qualified child present in the household. Back in the early 1980s, 21 states did not provide AFDC, thus Medicaid, to first time pregnant women; 28 states started to provide Medicaid to first time pregnant women starting from the sixth months of pregnancy.[35]

Concerned about the low insurance coverage rate among pregnant women and high infant mortality rate, state and federal governments took initiative to expand the scope of coverage starting from early 1980s. Broadly speaking, the expansions toward pregnant women occurred in two phases. In the first phase, Medicaid eligibility for pregnant women was gradually untied from AFDC. This was done primarily through two federal mandates: the Deficit Reduction Act of 1984 (DRA84) and Consolidated Budget Reconciliation Act of 1985 (COBRA85). The DRA 84 mandated coverage for first-time pregnant women and pregnant women in AFDC-UP type families. The law was effective in October 1984, but states had until the regular session of the State legislature in 1985 (January for most of the states) to comply. The COBRA85 mandated coverage for all pregnant women who met the AFDC income/resource standard, regardless of family structure. The law was effective in July 1986. The two federal mandates significantly reduced the constraints caused of the categorical eligibility requirements: low-income pregnant

[35] Source: Hill (1984). Arizona did not provide Medicaid coverage at the time.

female were eligible for Medicaid coverage immediately after medical diagnose, regardless of family structure.

In the second phase of the expansions, both the federal and state legislations gradually increased the income eligibility threshold. These expansions extended coverage to individuals with higher income level. These policies included: the Omnibus Reconciliation Act of 1986, which increased the income eligibility threshold to 100 percent of poverty; the Omnibus Budget Reconciliation Act of 1987, which allowed states to increase the income eligibility threshold to as high as 185 percent of poverty; and the Omnibus Budget Reconciliation Act of 1989, which mandates the income eligibility threshold to be set equal to or above 133 percent of poverty.

3. Methodology

The purpose of this study is to measure the effects of Medicaid provision on low-income women's insurance coverage status, their access to prenatal care and the birth outcomes of newborns. Ideally, such effects could be measured by the association between individual level Medicaid availability and the outcomes of interest.

However, individual level Medicaid eligibility is affected by factors that are directly associated with the outcomes of interest, leading to difficulties establishing causal relationships. For example, Medicaid is provided to individuals with limited income and resources. The target group would have less access to medical services and worse health conditions compared to individuals who are not the target of Medicaid. Additionally, Medicaid availability is affected by macroeconomic conditions. A higher unemployment rate is potentially associated more people eligible for Medicaid. At the same time, a sluggish economy may also directly affect prenatal

care utilization and birth outcomes.[36] Finally, Medicaid availability varies across states. State characteristics may also vary on other dimensions such as number of hospital beds per capita and the generosity of other social insurance programs (e.g. AFDC). Such differences may directly affect the health conditions.

To circumvent these difficulties, I exploit exogenous variation in Medicaid availability generated by policy changes. In particular, I rely on the two federal mandates that focus on relaxing the categorical eligibility rules: DRA84 and COBRA85. As discussed in the background section, these federal mandates untied Medicaid eligibility from the family structure constraint embedded in AFDC eligibility, thus generated different effects on Medicaid eligibility of low-income women from different demographic groups.

To be more specific, suppose there are two pregnant women, one at the age of 17 and the other 19, of similar family background and income level. Prior to the expansions, the latter is less likely eligible for Medicaid, as she ages out of being a qualified dependent child for the AFDC program. After the expansions, both women are equally likely qualified for Medicaid. Thus, the expansions potentially led to an immediate increase in Medicaid eligibility for women above 18 years of age, while had no immediate impact on women with ages just below 18.

Empirically, I follow a difference-in-differences method. The treatment group includes women who were directly affected by the policy changes: those of age 18 or 19. The control group includes women who were slightly younger than the treatment group, and were not immediately affected by the policy changes: those of age 16 or 17. The causal effects of the

[36] For the association between health conditions and macro-economic conditions, see Ruhm (2000), Arkes (2007), Ruhm (2013), and Currie et al. (2013).

policy changes are identified as the differences in outcomes between the two groups, before and after the policy changes.

Based on the legislative history, I restrict the analysis to births occurred in years 1982 to 1986. This reflect a period when the Medicaid expansions focused primarily on the categorical eligibility rules. Specifically, the pre-policy period includes years 1982 to 1984. I use January 1985 as the first month of post-policy period, as this coincide with the state legislature convene timing of calendar year 1985 for most of the states. I end the post-policy period by December 1986 for two reasons. First, starting in April 1987, a subsequent policy change - the Omnibus Budget Reconciliation Act of 1986 (OBRA86) – was effective. The OBRA86 permitted states to increase income threshold for pregnant women to up to 100% of the poverty line. Variation occurred across states in adopting the expansion. On the other hand, the timing and generosity of state level expansions are potentially correlated with other state level unobserved attributes. To avoid state level legislative endogeneity concern, I exclude from the analysis sample births occurred after OBRA86 became effective. Second, I further exclude birth occurred in the first three month of 1987, to ensure that both pre-policy and post-policy sample are evenly distributed over the months of the year. Buckles and Hungerman (2013) documents substantial seasonality pattern of maternal characteristics.

The regression framework is as follows:

(1) $Y_{it} = \alpha_0 + \alpha_1 * Treatment_i + \alpha_2 * Post_t + \alpha_3 * Treatment_i * Post_t + \alpha_4 * X_{it} + \varepsilon_{it}$

Y_{it} denotes the outcomes of interest. $Treatment_i$ is a dummy variable indicating whether the observation is in the treatment group. $Post_t$ is a dummy variable indicating whether the time of birth is in the post-policy period. X_{it} denotes a vector of individual observed characteristics. The

error term is denoted as ε_{it}. The coefficient of interest is α_2, which measure the relative change in outcomes for the treatment group, as the consequence of the policy changes. The estimated causal effects are interpreted as intent-to-treat effects.

The difference-in-differences strategy addresses the identification issues noted earlier. First, the effects of Medicaid provision is identified by the exogenous shocks to the eligibility rules. These shocks led to an increase in Medicaid provision that was not directly associated with latent individual characteristics such as income level. Second, the inclusion of the control group purges time-specific attributes that may potentially affect the treatment and comparison group in similar manner, such as macroeconomic conditions. Finally, the policies that I focus on are federal level mandates that affect individuals from all states at the same time. The effects of state level unobserved attributes are differenced out by comparing outcomes before and after the policy changes.

The identification assumptions are: the outcomes of the treatment group and the comparison group would trend similarly had there been no policy changes. Additionally, there is no other factors that occurred at the same time as the Medicaid expansions, and would affect the treatment group and comparison group differently. Evidence to support these assumptions is provided in the following sections: the outcomes trended similarly for the treatment and comparison groups prior to the Medicaid expansion; the estimated treatment effects are largely robust to controlling for worrisome confounders.

4. Data and Sample

The main data come from two sources. For prenatal care use and birth outcomes, I draw information from Birth Records from the National Vital Statistics System. For health insurance coverage status, I draw information from National Hospital Discharge Survey.

4.1 Birth Records from the National Vital Statistics System

The National Vital Statistics System compiles official birth record information maintained by the Centers for Disease Control and Prevention. The information comes from individual level birth certificates, which is required to be completed for all birth by State laws. It covers every birth occurred within the US territory.

I draw two sets of information from the individual level birth records. First, I use information on the demographic characteristics of the mother and the baby. These demographic characteristics include age, race, maternity history and marital status of the mother. I also draw information regarding the gender of the baby and whether the delivery is a single birth. Second, I focus on indicators for prenatal care access and birth outcomes of the baby. Prenatal care access is measured by a dummy variable indicating whether the mother started regular prenatal care by the end of second trimester. This indicator is a reflection of whether prenatal care is initiated in a timely manner. For birth outcome, I follow previous literature and focus on the prevalence of low birth weight (birth weight less than 2,500g).

4.2 National Hospital Discharge Survey

The National Hospital Discharge Survey (NHDS) is an annual survey conducted by the Centers for Disease Control and Prevention. The survey provides information for a

representative sample of inpatients discharged from non-Federal short-stay hospitals.[37] The observation is at individual patient level. The survey covers information regarding basic demographic characteristics, expected methods of payment, date of discharge, length of stay, diagnosis and procedures performed during the stay. The information is collected by hospital personnel or Bureau of Census staff to ensure accuracy.

For the purpose of this study, I restrict the analysis sample to women whose hospital stay is maternity related. To do so, I reply on the primary diagnosis code (ICD-9-CM): an observation is identified as a pregnant woman if the primary diagnosis code is classified as "outcomes of delivery". One concern with the sample restriction is that it does not include female giving birth in a non-hospital setting, or those giving birth in federal hospitals. Such concern is likely minor, as the share of birth in non-hospital setting (<5% annually[38]) and share of federal hospitals (3.69%[39]) are both small.

Two sets of information facilitate the analysis. First, I use patient demographic characteristics including age, marital status and race. Second, I use insurance coverage status. I infer the information based on "Principal expected source of payment". The patient is coded as having Medicaid coverage, if Medicaid is reported as the expected source of payment. The patient is coded as having private coverage, if the expected source of payment is "Blue Cross, Blue Shield", or "Other private insurance". The patient is coded as having no coverage, if the expected payment source is "self-pay".

4.3 Sample

[37] Surveyed hospitals include those with an average length of stay of fewer than 30 days, exclusive of federal, military, the Department of Veterans Affairs hospitals and hospitals with fewer than six beds.
[38] Vital statistics, Birth Records.
[39] American Hospital Association. http://www.aha.org/research/rc/stat-studies/fast-facts.shtml

To implement the difference-in-differences method, I restrict the analysis sample to pregnant women between ages 16 and 19 at the time of delivery, and the delivery occurred between 1982 and 1986. Additionally, I restrict the sample to births given in a hospital setting because only this group is recorded in the National Hospital Discharge Survey. This restriction excludes approximately 5% of all births in the Birth Records.

One potential concern is that the sample is conditional on live births. If the Medicaid availability affects the decision to have children or affects the prevalence of fetal death, the estimated treatment effects would be biased. To address the concern, I draw information on aggregate birth rates and fetal death rates by mother's ages. The trends of both rates are similar for the treatment group and comparison group both before and after Medicaid expansions. This suggests that the expansions did not seem to significantly affected the decision to have children or the prevalence of fetal death.

Table 1 displays a summary of characteristics. Column 1 and 4 focus on the whole analysis sample. Some demographic characteristics are reported in both data sets, including age, marital status and race. Comparing the two datasets, NHDS and Birth Records, the whole sample displays similar characteristics.[40]

I separately show the summary statistics of the treatment and control groups in **Table 1**, columns 2-3 and 5-6. As documented in both datasets, the older cohort – women of ages 18 or 19 – are more likely to be married and be white. Panel B and C provide summary statistics of outcome variables. NHDS provides information regarding insurance coverage, which is shown in panel B, **Table 1**.The treatment and control groups have overall similar Medicaid coverage rates.

[40] One thing worth noting is that the Birth Records seem to report a higher share of whites. One possible reason for the mismatch is the two datasets code the race variable slightly differently.

The older cohort, those of 18 or 19 years old, are more likely covered by private insurance, and less likely paying out of pocket. The Birth Records provides information regarding prenatal care utilization and health outcomes of newborns. Summary statistics of the two key variables are shown in panel C, **Table 1.** On average, about 9.2% of all birth in the analysis sample have low birth weight. This condition have higher prevalence among 16 or 17 year olds. About 88.3% of all mothers had early access to prenatal care. The younger cohort, women of ages 16 or 17, had greater difficulty gaining early access to prenatal care.

5. Results

This section reports how the policy changes affected the three outcomes of interest: insurance coverage, use of prenatal care and birth outcomes. For each outcome, I first graphically show the outcome trends for the treatment group and comparison group, respectively. The graphical illustration examines whether two groups have similar outcome trends prior to the policy changes, and shows how the outcomes of the two groups are affected differently by the policy changes. I then perform regression analysis to quantify the potential effects of the policy changes.

5.1 Health insurance coverage

I first examine if the eligibility expansions led to an increase in Medicaid coverage. This is shown graphically in **Figure 1**. Panel A plots the share of women covered by Medicaid in the treatment group (18 or 19 year olds) and the comparison group (16 or 17 year olds) against the sample year. Prior to the expansions, the Medicaid coverage rates trended similarly for the two groups. The control group had a higher coverage rate than the treatment group. Additionally, the policy interventions significantly increased the coverage rate for the treatment group. In the post-

policy period, the treatment group appeared to have a higher Medicaid coverage rate than the comparison group. Panel B plots the Medicaid coverage rates of pre-policy period (1982-1984) and post-policy period (1985-1986) against the patients' ages. The trends confirm that the treatment group experienced a significant gain in Medicaid coverage after the policy changes. The comparison group did not present significantly different Medicaid coverage rates before and after the policy changes.

To quantify the relative gain in Medicaid coverage for the treatment group, I perform regression analysis. I use a series of specifications following equation (1), with varied sets of controls. The results are shown in **Table 2a**, Panel A. Column 1 reports results of the specification that includes no additional control variables. The coefficient of interest, that of the policy*post interaction term, suggests that the treated group experienced a relative increase of 5.5 percentage points in Medicaid coverage as the consequence of the expansions. To check the robustness of the estimated treatment effect, I further add demographic characteristics, month-of-discharge dummies and region fixed effects. The coefficients are reported in Column 2 and column 3. The magnitude and statistical power of the point estimates of the interaction term are robust. The preferred specification in column 3 suggests that the expansions led to a 6.5 percentage point relative increase in Medicaid coverage of the treatment groups.

The literature has long documented the "crowd-out" effect of public health insurance provision.[41] The crowd-out describes the phenomenon that public provision of health insurance may provide an incentive for eligible population to opt out from private insurance coverage. To examine the crowd-out effect of the Medicaid expansions, I turn to private insurance coverage as

[41] Studies examining crowd-out effect focus on various Medicaid expansions for different periods and target groups. See, for example, Cutler and Gruber (1996), Aizer and Grogger (2003), Ham and Shore-Sheppard (2005), Shore-Sheppard (2008), Hamersma and Kim (2012).

an outcome. I follow the same specifications. The results are reported in **Table 2a**, columns 4-6. As shown in the preferred specification in column 6, **Table 2a**, the eligibility expansions led to a 3.9 percentage point relative decrease in private insurance coverage of the treatment group. The point estimate suggests a crowd-out rate of about 0.6: 60% of the new Medicaid beneficiaries would have been covered by private insurance, had there been no expansions. However, the negative effect on the private insurance coverage is not statistically significant at five percent level.[42] The point estimate for the crowd-out rate may also lack statistical power.

I further investigate how the effects on insurance coverage differ across women of varied social economic status. Literature suggests that the effect of public programs may have different effects on people of different social economics status. In particular, the crowd-out effect would be less severe for women from more disadvantaged backgrounds, who would face greater difficulty gaining private insurance absent of Medicaid.[43] To examine if this is the case, I perform subgroup analysis using mother's race as an indicator for social economic backgrounds. **Table 2b** and **Table 2c** show the effects of the expansions on Medicaid coverage and private insurance coverage for the whites and blacks, respectively. Results suggest that the expansions led to both a significant increase in Medicaid coverage (6.9 percentage points) and a significant decrease in private insurance coverage (4.3 percentage points) for the whites. However, for the black women, who are less likely to have access to other forms of insurance coverage, the expansion led to an increase in Medicaid coverage (4.3 percentage points), with minimal decrease in private coverage (0.8 percentage points).

[42] The 95 percent confidence interval is [-0.092, 0.014].
[43] See, for example, Dubay and Kenney (1997) and Currie and Gruber (2001),

To sum up, findings suggest that the expansions increased Medicaid coverage at the expense of a significant reduction of private insurance. The shift of insurance coverage status differ across racial groups. For the white population, the crowd-out effect was large and significant. For the black population, the expansions did not lead to a significant reduction in private insurance coverage.

5.2 Access to prenatal care

This section examines whether the increase in Medicaid coverage led to an increase in the use to prenatal care. Particularly, I focus on whether one have early access to prenatal care, as measured by starting prenatal care as early as in the second trimester.

The outcome trends for the treatment and comparison groups are shown in **Figure 2**. On average, the treatment group had better access to prenatal care. This pattern did not seem to change after the policy intervention. I further split the sample by racial group. For both the blacks and the whites, the treatment groups had better access to prenatal care than the comparison groups. Such pattern also did not seem to diverge after the policy changes.

I turn to regression analysis to quantify any potential effects. I follow the same difference-in-differences specification as in the previous sections. The results for the whole sample are displayed in **Table 3a**. Column 1 reports the coefficients from the specification with no additional controls. The coefficient of interest, the one of the interaction term, suggests that the expansions led to a 0.49 percentage point decrease in access to early prenatal care for the treatment group. Such effect is robust to the inclusion of demographic controls, state fixed effects and months of birth fixed effects, as shown in columns 2 and 3.

Column 4 adds controls for legal drinking regulations, which correlates with age and may have direct effect on outcomes of interest. The National Minimum Drinking Age Act of 1984 imposed penalty to states that allowed persons below 21 years to purchase alcoholic beverages by reducing federal highway apportionment. In compliance to the federal regulation, states gradually raised their minimum drinking age (which was as low as 18 in some states) in throughout the 1980s. This notable regulatory change happened at about the same time as the Medicaid expansions. Legal drinking may also directly affect one's health condition and birth outcomes. I thus include controls for whether the observed individual can legally purchase alcohol, based on age and the state of residence. The result in column 4 suggests that the coefficient of interest is robust.

I further split the sample by race and report the results in **Table 3b** and **Table 3c**. For the whites, who experienced greater crowd-out effects, the treatment group had a relative decline of 0.55 percentage points in receiving prenatal care early, out of a baseline of 0.901. For the blacks, who experienced little crowd-out effects, the expansions did not have any effect on receiving early prenatal care.

5.3 Health outcome: low birth weight

This section focuses on the effect of Medicaid provision on birth outcome. Following the literature, I focus on the prevalence of low birth weight.[44] **Figure 3** graphically shows the trends of the prevalence of low birth weight for the treatment and comparison groups. Women in the treatment group are on average less likely to give birth to under-weight babies. For the whole

[44] In addition to low birth weight, I examine alternative measure for birth outcomes. I examine the effects of Medicaid eligibility on the prevalence of premature births. The estimated treatment effects are very similar to the effects on the prevalence of low birth weight. I also experiment with different cut-offs to denote low birth weight and find similar patterns.

sample (panel A) and the whites (panel B), the difference in low birth weight between treatment and comparison groups seem persistent both before and after the policy change. For the blacks (panel C), the trends are more noisy, plausibly due to the smaller sample size. The treatment group seem to have a relative drop in low birth weight prevalence after the policy change, relative to the treatment group.

I turn to regression analysis to quantify the effects. Again, I follow the difference-in-differences approach as in the previous sections. The results for the whole sample are shown in **Table 4a**. Column 1 reports the coefficients for the specification with no additional controls. The coefficient of interest, the one of the interaction term, suggests that the Medicaid eligibility expansions led to a 0.2 percentage point increase in the prevalence low birth weight, which is statistically significant at five percent level. Column 2 and 3 further include demographic characteristics of the mother (race, marital status), characteristics of the baby (gender, whether single birth), state fixed effects, and month of birth dummies. The coefficient of interest is small and insignificant. Column 4 further controls for whether the observation have legal access to alcohol, based on the age and the state of residence. The coefficient of interest remains small and insignificant.

I further split the sample by race. The results for the two racial groups, whites and blacks, are shown in **Table 4b** and **Table 4c**, respectively. For the whites, the preferred specification in column 4 shows that, the Medicaid expansions led to an 0.16 percentage points increase in the incidence of low birth weight. This point estimate reflects a 2% increase. However, given the enormous sample size, the point estimate is only significant at ten percent level. For the blacks, the point estimate suggest that Medicaid eligibility expansion led to a 0.2 percentage points

(1.5%) decrease in the incident of low birth weight. Again, the point estimate is statistically insignificant.

Overall, the results from the whole sample reveal no significant effects of the policy changes on use of medical care and birth outcomes. One concern with the whole sample analysis is that the marginal affected population – the low-income women shifting insurance coverage due to the policy changes – is potentially of a small share of the whole population. In the next section, I attempt to address the concern by restricting the analysis sample to individuals more likely affected by the policy changes.

6. Extensions

In this section, I perform subgroup analysis for two groups of women who are most likely directly affected by the policy changes, based on their states of residence and family structure.

6.1 States with more substantial expansions

As discussed in the background section, pregnancy condition alone did not lead to entitlement of Medicaid prior to the federal mandates. However, states took slightly different approaches in treating the fetus, which generated variation in Medicaid entitlement. Specifically, 21 states did not treat the fetus as a dependent child until birth; 28 states treated the fetus as a dependent child starting from the third trimester. For the former states, pregnant women without dependent child cannot have access to Medicaid throughout the course of pregnancy. This group thus tends to have benefited more from the eligibility expansions.

I repeat the analysis in the main analysis to the subsample of women who resided in the 21 states that had more substantial expansions. **Table 5** displays the results, following the preferred specification with the full set of control variables. For both outcome variables of

interest – medical care use and prevalence of low birth weight – the subsample analysis reveal similar pattern as in the main analysis: for the whites who experienced substantial crowd-out, expansions led to a small decrease in early access to prenatal care, but no significant change in prevalence of low birth weight babies. For the blacks who experienced minimum crowd-out, the expansions did not lead to significant change in both use of care and birth outcomes.

6.2 First-time pregnant women

In this section, I restrict the sample to women who were giving birth for the first time. Among the 2.15 million births given by women aged 16 to 19 during the five years, 1.48 million were first time birth.

I repeat the regression analysis for the first-time pregnant women. The results are shown in **Table 6**. For the whole sample, shown in columns 1 and 4, the policy expansions did not lead to significant effects on access to prenatal care or the prevalence of low birth weight. I further split the sample by race. The results are reported in columns 2, 3, 5 and 6. The policy changes had different effects on the whites and blacks. The white women experienced a significant decrease in early access to prenatal care by 0.43 percentage points, which reflects a 5.2% increase in the probability of not receiving timely prenatal care. The prevalence of low birth weight babies increased by 0.22 percentage points, which reflects a 3.3% increase. On the contrary, the black women had an increase in early access to prenatal care by 0.38 percentage points. This reflects a 3.58% increase. The prevalence of low birth weight among newborns reduced by 0.31 percentage points, reflecting a 2.60% reduction. The results for the blacks are only significant at ten percent level.

The divergent pattern of the treatment effects among the whites and the blacks is consistent with the findings regarding the crowd-out effect. For the whites, who most likely shifted from private insurance to Medicaid, the policy changes led to a decrease in medical care use and worse health outcomes. For the blacks, who most likely shifted from no insurance to Medicaid, the policy changes led to an increase in medical care use and better health outcomes.

6. Discussion and Future Work

This study investigates the effects of Medicaid provision to pregnant women on their insurance coverage status, use of medical care and birth outcomes. I exploit variation in Medicaid eligibility across different demographic groups and over time. Before early 1980s, the eligibility of Medicaid was tied to AFDC, which provided cash benefits to families with dependent children. This created discontinuity in Medicaid eligibility across different age groups: a first time pregnant woman may age out of Medicaid when she turns 18. Such discontinuity was eliminated by the two federal expansions in mid-1980s. As a result, female of age 18 or 19 would potentially have a significant increase in eligibility in Medicaid.

I employ a difference-in-differences method. Using data from the National Hospital Discharge Survey, I find that the policy changes increased Medicaid coverage for the treated group by a significant 6.5 percentage point. However, the expansions also led to a decrease in private insurance coverage, especially among the whites. With regard to the use of medical service and birth outcomes, I use data from the Birth Records from the National Vital Statistics System. Findings reveal no evidence that the treatment group increased use of prenatal care. There is also no significant evidence that the birth outcomes of newborns, measured by the incidence of low birth weight, are affected by Medicaid provision.

Findings in this study leave open important questions regarding channels through which the public provision of health insurance affects one's well-being. My findings suggest that the significant increase in Medicaid coverage did not translate into increase in use of medical service. However, increase in Medicaid coverage may have other implications. For example, new Medicaid beneficiaries may have a gain in disposable income, which was previously used to pay for private or group insurance premium or co-payment. The extra resources may be used for consumptions that may or may not be health improving. Thus, one natural avenue for future research is to examine the effect of Medicaid availability on consumption pattern. Additionally, Medicaid beneficiaries are often automatically enrolled in other social insurance programs. One important program for maternity health is the Special Supplemental Nutrition Program for Women, Infants and Children (WIC). The WIC is a federal assistance program that provides supplemental food, formula, nutrition education and access to healthcare and other social services. The potential interactions between WIC and Medicaid leave room for future research.

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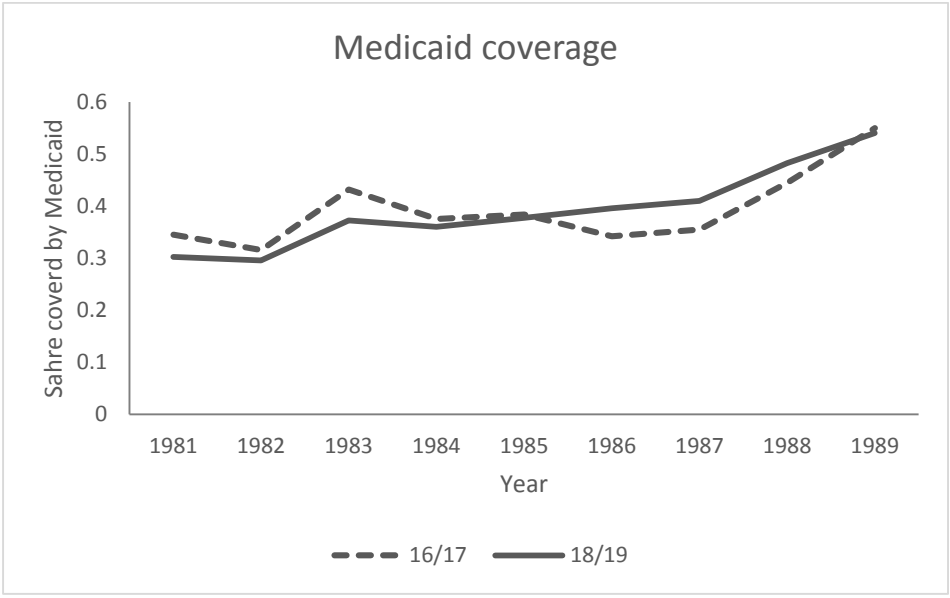
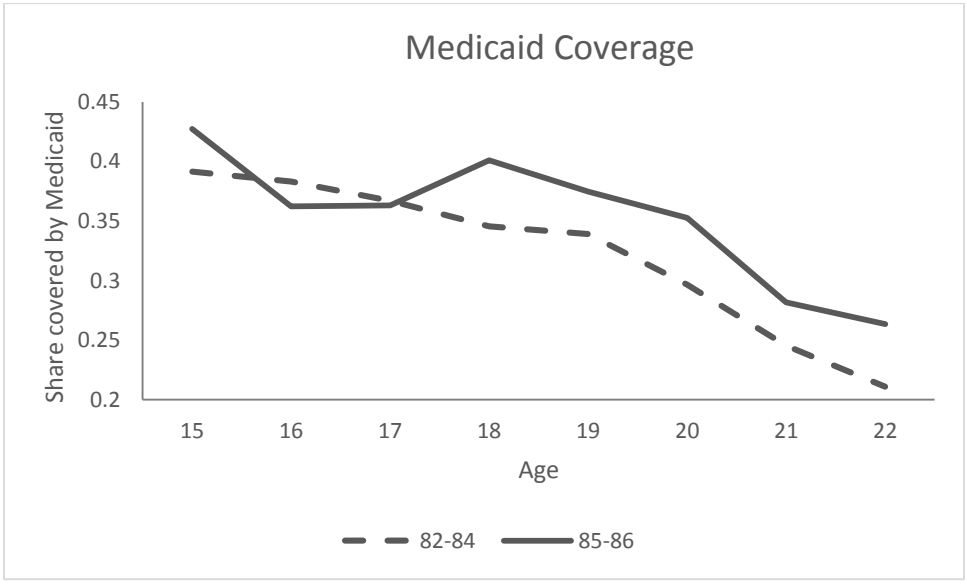
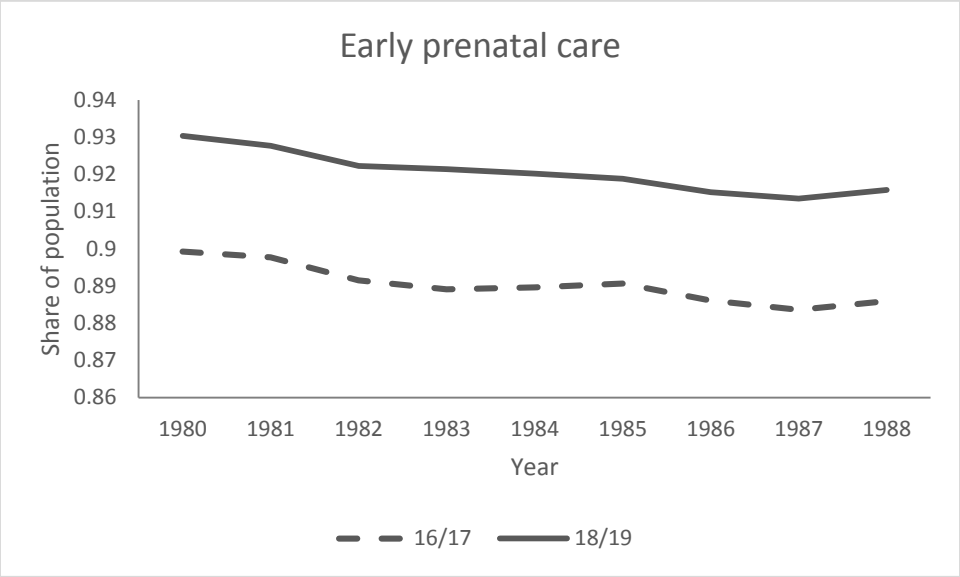
Figure 2-1: Trends in Medicaid Coverage



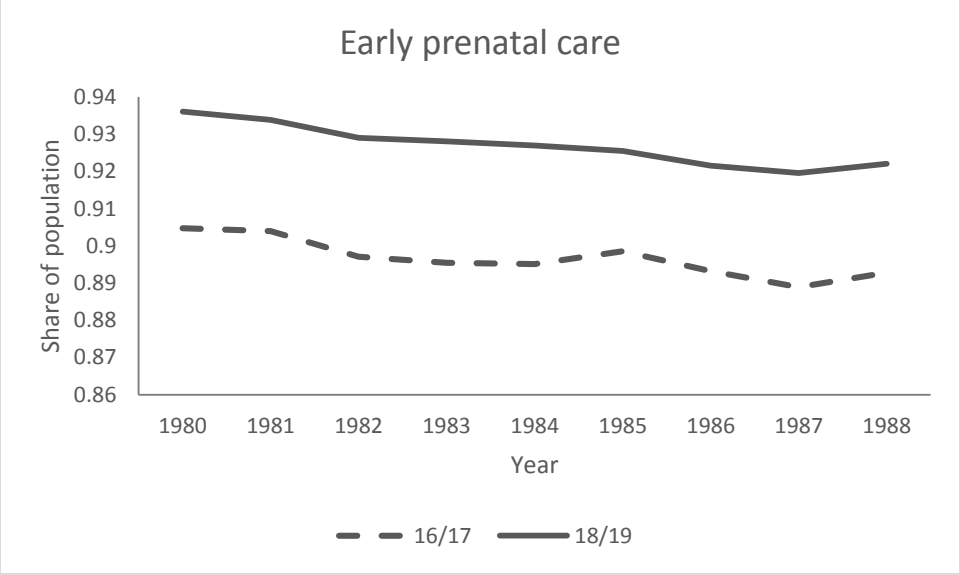


Notes: Data come from National Hospital Discharge Survey. Sample include patients whose the primary diagnosis code (ICD-9-CM) is classified as “outcomes of delivery”. Survey weights are used.

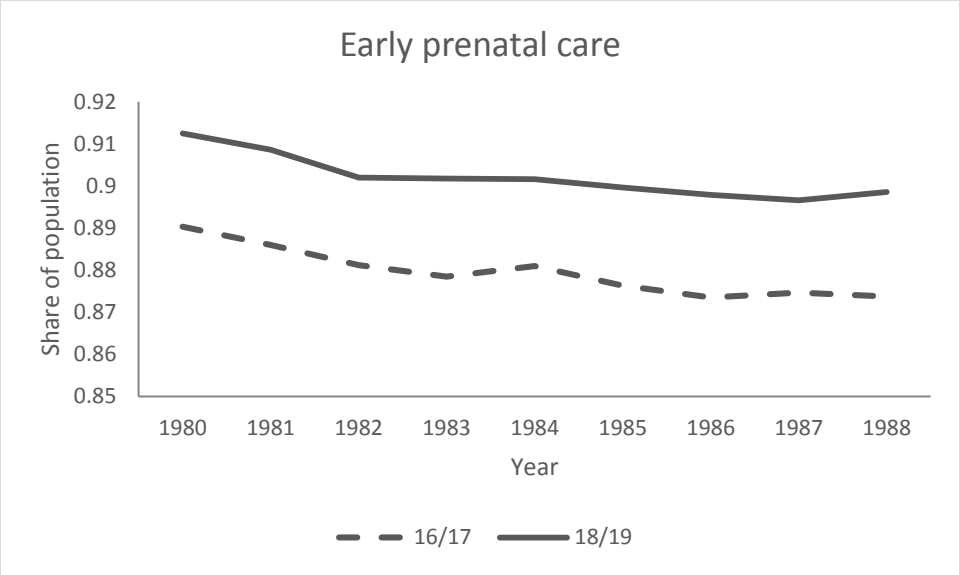
Figure 2-2: Outcome trends: started prenatal care as early as the second trimester



A: whole sample

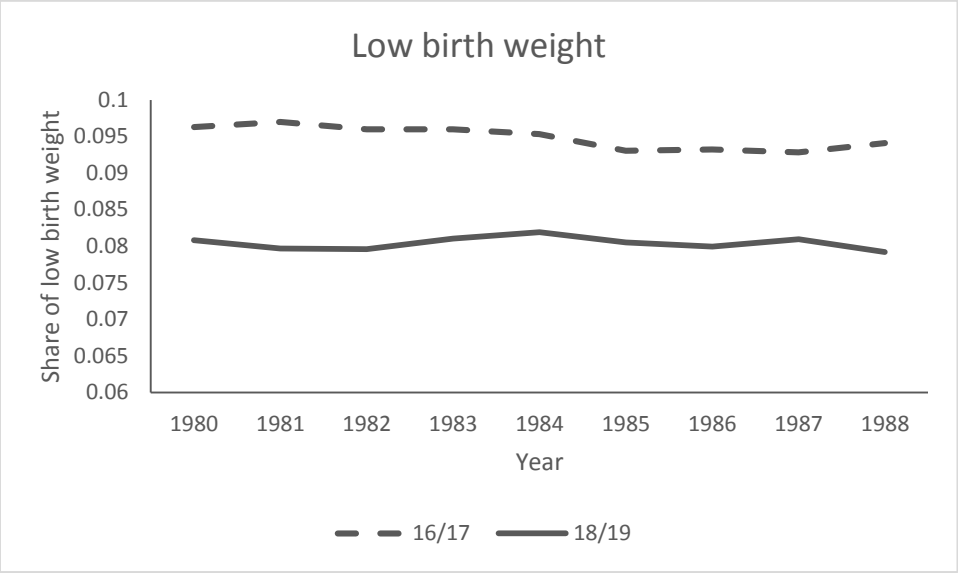


B: Whites

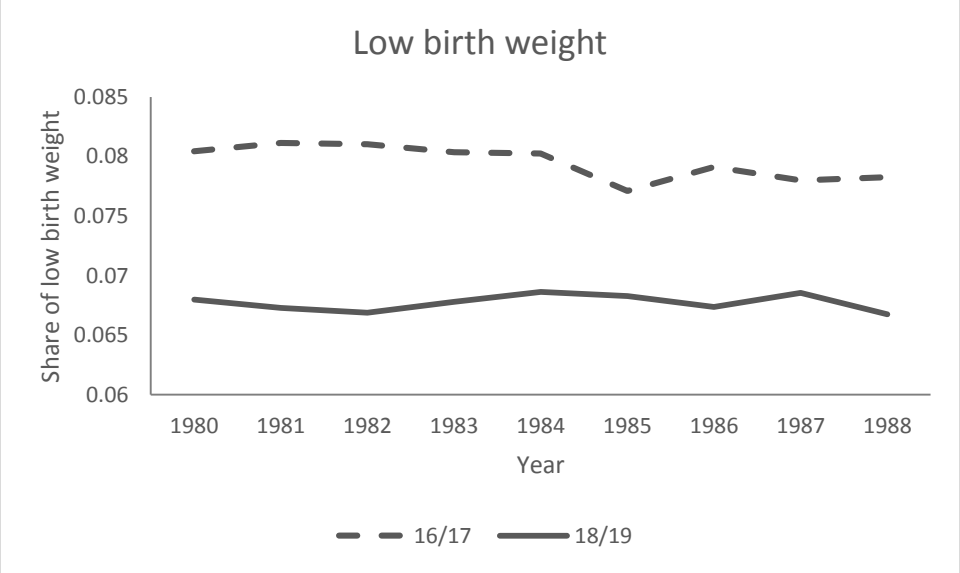

Early prenatal care
Share of population
0.92
0.91
0.9
0.89
0.88
0.87
0.86
0.85
1980
1981
1982
1983
1984
1985
1986
1987
1988
Year
16/17
18/19

C: Blacks

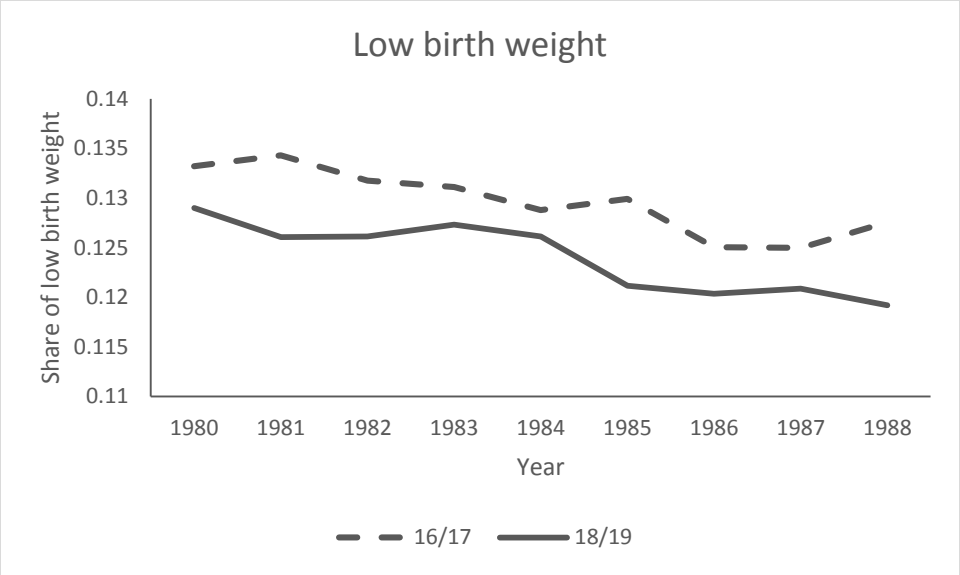
Figure 2-3: Outcome trends: low birth weight



A: whole sample



B: Whites


Low birth weight
Share of low birth weight
0.14
0.135
0.13
0.125
0.12
0.115
0.11
1980
1981
1982
1983
1984
1985
1986
1987
1988
Year
16/17
18/19

C: Blacks

Table 2-1: Summary of characteristics

	National Hospital Discharge Survey			Birth Records		
	(1)	(2)	(3)	(4)	(5)	(6)
	Whole sample	16 or 17	18 or 19	Whole sample	16 or 17	18 or 19
A: Demographics						
Age	17.93	16.64	18.56	17.94	16.62	18.57
	(1.02)	(0.48)	(0.50)	(1.03)	(0.48)	(0.50)
Married	0.43	0.31	0.48	0.45	0.33	0.52
	(0.49)	(0.46)	(0.50)	(0.50)	(0.47)	(0.50)
White	0.67	0.63	0.69	0.71	0.66	0.73
	(0.47)	(0.48)	(0.46)	(0.45)	(0.47)	(0.44)
Black	0.28	0.32	0.26	0.27	0.32	0.25
	(0.45)	(0.47)	(0.44)	(0.44)	(0.47)	(0.42)
B: Insurance Coverage						
Medicaid	0.36	0.37	0.36			
	(0.48)	(0.48)	(0.48)			
Private	0.3	0.28	0.32			
	(0.46)	(0.45)	(0.46)			
Self-pay	0.21	0.23	0.21			
	(0.41)	(0.42)	(0.40)			
C: Outcomes						
Low birth weight				0.092	0.102	0.088
				(0.29)	(0.30)	(0.28)
Early prenatal care				0.883	0.868	0.890
				(0.29)	(0.34)	(0.31)
# of obs.	10398	3412	6986	2107611	676608	1431003

Notes: Data come from the National Hospital Discharge Survey and the Birth Records from National Vital Statistics System. Sample includes births given by mothers of age 16-19, in years 1982 to 1986. For the Birth Record Data, only births given in a hospital setting are included in the sample. Survey weights are used for the National Hospital Discharge Survey. Standard deviations are reported in parenthesis.

Table 2-2a: Difference-in-differences: Insurance Coverage, whole sample

	(1)	(2)	(3)	(4)	(5)	(6)
	A: Medicaid coverage			B: Private insurance coverage		
Treatment	-0.031*	0.021	0.019	0.050	0.022	0.020
	(0.007)	(0.019)	(0.018)	(0.021)	(0.016)	(0.015)
Post	-0.010	-0.045**	-0.042**	-0.048*	-0.033	-0.027
	(0.006)	(0.007)	(0.006)	(0.011)	(0.017)	(0.016)
Interaction	0.055*	0.066*	0.065*	-0.036	-0.037	-0.039
	(0.010)	(0.016)	(0.014)	(0.018)	(0.018)	(0.017)
White		-0.055	-0.040		0.162**	0.162**
		(0.024)	(0.020)		(0.018)	(0.017)
Black		0.072*	0.103**		0.090*	0.068
		(0.022)	(0.015)		(0.027)	(0.028)
Married		-0.276**	-0.264**		0.157	0.150
		(0.041)	(0.043)		(0.092)	(0.092)
Discharge month			Y			Y
Region			Y			Y
Observations	11,758	9,329	9,329	11,758	9,329	9,329
R-squared	0.002	0.120	0.129	0.008	0.053	0.065

Notes: Data come from National Hospital Discharge Survey. Sample include patients whose the primary diagnosis code (ICD-9-CM) is classified as "outcomes of delivery". Survey weights are used. Standard errors are clustered by age, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Table 2-2b: Difference-in-differences: Insurance Coverage; whites only

	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid coverage			Private insurance coverage		
Treatment	-0.041**	0.002	-0.001	0.063	0.045	0.040
	(0.006)	(0.022)	(0.021)	(0.025)	(0.018)	(0.017)
Post	0.002	-0.033	-0.036	-0.058*	-0.040	-0.033
	(0.022)	(0.018)	(0.014)	(0.015)	(0.023)	(0.019)
Interaction	0.065	0.065	0.069*	-0.039	-0.042	-0.043
	(0.024)	(0.022)	(0.021)	(0.025)	(0.028)	(0.025)
Married		-0.253*	-0.238*		0.136	0.124
		(0.045)	(0.048)		(0.093)	(0.094)
Discharge month			Y			Y
Region			Y			Y
Observations	6,696	5,961	5,961	6,696	5,961	5,961
R-squared	0.004	0.078	0.089	0.010	0.029	0.048

Notes: Data come from National Hospital Discharge Survey. Sample include patients whose the primary diagnosis code (ICD-9-CM) is classified as "outcomes of delivery". Survey weights are used. Standard errors are clustered by age, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Table 2-2c: Difference-in-differences: Insurance Coverage; blacks only.

	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid coverage			Private insurance coverage		
Treatment	0.045	0.084	0.086	-0.010	-0.036	-0.035
	(0.023)	(0.030)	(0.033)	(0.003)	(0.021)	(0.024)
Post	-0.044	-0.049**	-0.042**	-0.039*	-0.025*	-0.021
	(0.021)	(0.001)	(0.006)	(0.010)	(0.007)	(0.008)
Interaction	0.049	0.050	0.043	-0.004	-0.003	-0.008
	(0.025)	(0.025)	(0.024)	(0.010)	(0.013)	(0.021)
Married		-0.407**	-0.401**		0.294*	0.295*
		(0.032)	(0.027)		(0.070)	(0.069)
Discharge month			Y			Y
Region			Y			Y
Observations	3,208	2,859	2,859	3,208	2,859	2,859
R-squared	0.005	0.063	0.075	0.003	0.048	0.056

Notes: Data come from National Hospital Discharge Survey. Sample include patients whose the primary diagnosis code (ICD-9-CM) is classified as “outcomes of delivery”. Survey weights are used. Standard errors are clustered by age, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Table 2-3a: Difference-in-differences: early prenatal care; whole sample

	(1)	(2)	(3)	(4)
Treatment	2.39**	1.24**	1.03**	0.752**
	(0.127)	(0.196)	(0.0834)	(0.114)
Post	-0.247	0.051	-0.0204	-0.0293
	(0.252)	(0.26)	(0.235)	(0.238)
Interaction	-0.491**	-0.408**	-0.425**	-0.305**
	(0.115)	(0.109)	(0.106)	(0.102)
Black		2.50	2.94**	2.94**
		(1.59)	(0.817)	(0.815)
White		3.04	3.40**	3.40**
		(1.51)	(0.528)	(0.527)
Married		5.76**	6.36**	6.34**
		(0.612)	(0.415)	(0.410)
Male child		0.135**	0.128**	0.128**
		(0.03)	(0.0442)	(0.0441)
Single birth		-1.56**	-1.53**	-1.53**
		(0.198)	(0.187)	(0.187)
State fixed effects			Y	Y
Month of birth			Y	Y
Legal drinking				Y
Observations	2,107,611	2,095,566	2,095,566	2,095,566
R-squared	0.001	0.010	0.029	0.029

Notes: Data come from the Birth Records from National Vital Statistics System. Sample include births given by mothers of age 16-19, in years 1982 to 1986. Reported coefficients are factored by 100. Standard errors are clustered by state, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Table 2-3b: Difference-in-differences: early prenatal care; whites

	(1)	(2)	(3)	(4)
Treatment	2.60**	1.49**	1.14**	0.878**
	(0.169)	(0.228)	(0.105)	(0.135)
Post	-0.107	0.351	0.359	0.348
	(0.215)	(0.243)	(0.241)	(0.245)
Interaction	-0.670**	-0.646**	-0.668**	-0.551**
	(0.115)	(0.110)	(0.106)	(0.101)
Married		6.12**	6.89**	6.88**
		(0.677)	(0.539)	(0.534)
Male child		0.0989	0.0921	0.0921
		(0.0495)	(0.0508)	(0.0507)
Single birth		-1.58**	-1.53**	-1.53**
		(0.232)	(0.211)	(0.211)
State fixed effects			Y	Y
Month of birth			Y	Y
Legal drinking				Y
Observations	1,486,782	1,486,256	1,486,256	1,486,256
R-squared	0.001	0.011	0.031	0.031

Notes: Data come from the Birth Records from National Vital Statistics System. Sample include births given by mothers of age 16-19, in years 1982 to 1986. Reported coefficients are factored by 100. Standard errors are clustered by state, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Table 2-3c: Difference-in-differences: early prenatal care; blacks

	(1)	(2)	(3)	(4)
Treatment	1.18**	0.854**	0.910**	0.569**
	(0.178)	(0.210)	(0.161)	(0.199)
Post	-0.539	-0.515	-0.743*	-0.748*
	(0.404)	(0.401)	(0.352)	(0.354)
Interaction	-0.155	-0.107	-0.133	0.0172
	(0.232)	(0.237)	(0.230)	(0.225)
Married		3.17**	3.43**	3.39**
		(0.519)	(0.306)	(0.297)
Male child		0.183**	0.181**	0.181**
		(0.0647)	(0.0658)	(0.0656)
Single birth		-1.64**	-1.66**	-1.66**
		(0.354)	(0.356)	(0.355)
State fixed effects			Y	Y
Month of birth			Y	Y
Legal drinking				Y
Observations	565,962	565,844	565,844	565,844
R-squared	0.000	0.001	0.022	0.022

Notes: Data come from the Birth Records from National Vital Statistics System. Sample include births given by mothers of age 16-19, in years 1982 to 1986. Reported coefficients are factored by 100. Standard errors are clustered by state, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Table 2-4a: Difference-in-differences: Low birth weight; whole sample

	(1)	(2)	(3)	(4)
Treatment	-1.48**	-0.963**	-0.936**	-0.768**
	(0.0876)	(0.0557)	(0.0537)	(0.0654)
Post	-0.336**	-0.426**	-0.375**	-0.276**
	(0.122)	(0.0914)	(0.0793)	(0.0656)
Interaction	0.207*	0.155	0.153	0.0806
	(0.0862)	(0.0836)	(0.0823)	(0.0890)
Black		5.62**	5.45**	5.45**
		(0.527)	(0.502)	(0.502)
White		0.665	0.696	0.696
		(0.541)	(0.511)	(0.509)
Married		-1.49**	-1.61**	-1.61**
		(0.135)	(0.121)	(0.120)
Male child		-1.09**	-1.09**	-1.09**
		(0.0586)	(0.0589)	(0.0590)
Single birth		-56.7**	-6.7**	-0.56.7**
		(0.364)	(0.365)	(0.365)
State fixed effects			Y	Y
Month of birth			Y	Y
Legal drinking				Y
Observations	2,162,933	2,150,047	2,150,047	2,150,047
R-squared	0.001	0.061	0.062	0.062

Notes: Data come from the Birth Records from National Vital Statistics System. Sample include births given by mothers of age 16-19, in years 1982 to 1986. Reported coefficients are factored by 100. Standard errors are clustered by state, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Table 2-4b: Difference-in-differences: Low birth weight; whites only

	(1)	(2)	(3)	(4)
Treatment	-1.28**	-1.16**	-1.12**	-0.928**
	(0.0845)	(0.0677)	(0.0631)	(0.0729)
Post	-0.255*	-0.397**	-0.331**	-0.269**
	(0.121)	(0.109)	(0.0768)	(0.0677)
Interaction	0.221*	0.249**	0.243**	0.160
	(0.103)	(0.0894)	(0.0870)	(0.0882)
Married		-1.40**	-1.55**	-1.54**
		(0.143)	(0.116)	(0.115)
Male child		-0.651**	-0.652**	-0.652**
		(0.0379)	(0.0382)	(0.0382)
Single birth		-55.7**	-55.7**	-55.7**
		(0.00464)	(0.00465)	(0.466)
State fixed effects			Y	Y
Month of birth			Y	Y
Legal drinking				Y
Observations	1,521,812	1,521,238	1,521,238	1,521,238
R-squared	0.000	0.055	0.056	0.056

Notes: Data come from the Birth Records from National Vital Statistics System. Sample include births given by mothers of age 16-19, in years 1982 to 1986. Reported coefficients are factored by 100. Standard errors are clustered by state, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Table 2-4c: Difference-in-differences: Low birth weight; blacks only

	(1)	(2)	(3)	(4)
Treatment	-0.500**	-0.528**	-0.527**	-0.386**
	(0.0975)	(0.0784)	(0.0767)	(0.0942)
Post	-0.430*	-0.480**	-0.464**	-0.249
	(0.178)	(0.174)	(0.173)	(0.168)
Interaction	-0.114	-0.136	-0.137	-0.200
	(0.168)	(0.159)	(0.159)	(0.171)
Married		-1.97**	-2.01**	-2.00**
		(0.145)	(0.146)	(0.144)
Male child		-2.29**	-2.29**	-2.29**
		(0.119)	(0.119)	(0.119)
Single birth		-59.0**	-58.9**	-58.9**
		(0.603)	(0.601)	(0.600)
State fixed effects			Y	Y
Month of birth			Y	Y
Legal drinking				Y
Observations	583,322	583,180	583,180	583,180
R-squared	0.000	0.050	0.051	0.051

Notes: Data come from the Birth Records from National Vital Statistics System. Sample include births given by mothers of age 16-19, in years 1982 to 1986. Reported coefficients are factored by 100. Standard errors are clustered by state, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Table 2-5: Difference-in-differences: sample restricted to births occurred in states that did not provide Medicaid to first time pregnant population.

	Early Prenatal Care			LBW		
	(1)	(2)	(3)	(4)	(5)	(6)
	Whole sample	White	Black	Whole sample	White	Black
Treatment	1.86**	2.09**	1.75**	-1.70**	-1.80**	-1.52**
	(0.247)	(0.273)	(0.392)	(0.130)	(0.152)	(0.233)
Post	-0.0405	0.419	-0.443	-0.289	-0.160	-0.468
	(0.336)	(0.458)	(0.476)	(0.147)	(0.181)	(0.308)
Interaction	-0.289	-0.509	-0.327	0.111	0.0041	0.131
	(0.270)	(0.325)	(0.481)	(0.210)	(0.220)	(0.350)
Black	1.81			6.07**		
	(1.13)			(0.493)		
White	2.71**			1.16*		
	(0.848)			(0.490)		
Married	6.43**	6.97**	4.64**	-2.28**	-2.16**	-2.80**
	(0.461)	(0.606)	(0.505)	(0.166)	(0.181)	(0.219)
Male child	0.0728	0.0469	0.103	-1.65**	-1.21**	-2.53**
	(0.0707)	(0.0884)	(0.133)	(0.0816)	(0.085)	(0.114)
Single birth	-4.48**	-4.48**	-4.65**	-55.8**	-54.8**	-57.9**
	(0.579)	(0.743)	(0.481)	(0.382)	(0.487)	(0.589)
State fixed effects	Y	Y	Y	Y	Y	Y
Month of birth	Y	Y	Y	Y	Y	Y
Legal drinking	Y	Y	Y	Y	Y	Y
Observations	643,401	411,795	217,169	663,954	423,743	224,666
R-squared	0.037	0.042	0.028	0.100	0.104	0.079

Notes: Data come from the Birth Records from National Vital Statistics System. Sample include births given by mothers of age 16-19, in years 1982 to 1986. Reported coefficients are factored by 100. Standard errors are clustered by state, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Table 2-6: Difference-in-differences: sample restricted to first birth.

	Early Prenatal Care			LBW		
	(1)	(2)	(3)	(4)	(5)	(6)
	Whole sample	White	Black	Whole sample	White	Black
Treatment	1.52**	1.50**	1.70**	-0.815**	-0.949**	-0.472**
	(0.0957)	(0.125)	(0.167)	(0.0710)	(0.0757)	(0.132)
Post	0.0596	0.407**	-0.781*	-0.311**	-0.314**	-0.237
	(0.158)	(0.145)	(0.355)	(0.0858)	(0.0843)	(0.205)
Interaction	-0.148	-0.431**	0.376	0.101	0.219**	-0.311
	(0.118)	(0.121)	(0.196)	(0.0765)	(0.0771)	(0.169)
Black	4.33**			4.92**		
	(0.757)			(0.587)		
White	3.19**			0.678		
	(0.444)			(0.599)		
Married	6.85**	7.33**	3.64**	-1.47**	-1.44**	-1.66**
	(0.488)	(0.581)	(0.296)	(0.114)	(0.105)	(0.230)
Male child	0.126*	0.0882	0.191	-0.836**	-0.433**	-2.12**
	(0.0528)	(0.0559)	(0.0997)	(0.0569)	(0.0387)	(0.140)
Single birth	-2.29**	-2.29**	-2.35**	-56.7**	-55.9**	-58.7**
	(0.298)	(0.379)	(0.442)	(0.528)	(0.596)	(1.06)
State fixed effects	Y	Y	Y	Y	Y	Y
Month of birth	Y	Y	Y	Y	Y	Y
Legal drinking	Y	Y	Y	Y	Y	Y
Observations	1,452,874	1,074,987	348,793	1,486,895	1,098,069	358,656
R-squared	0.029	0.032	0.024	0.036	0.032	0.027

Notes: Data come from the Birth Records from National Vital Statistics System. Sample include births given by mothers of age 16-19, in years 1982 to 1986. Reported coefficients are factored by 100. Standard errors are clustered by state, reported in parenthesis. * and ** denote 5% and 1% significant level, respectively.

Chapter 3: Medicaid Nursing Home Care and Elderly Asset Transfer: Evidence from the Deficit Reduction Act of 2005

1. Introduction

Social welfare programs, which target at promoting the well-being of the disadvantaged population, are potentially associated with behavior distortions. Almost all social welfare programs are provided to people with limited income and resources. Theoretically, the eligibility schemes generate a disincentive for individuals to accumulate income or saving. Empirically, literature finds evidence that social welfare programs lead to reduction in labor supply and saving for various target populations.[45] This study investigates how the provision of public insurance affects the asset holding behavior among the elderly.

For the elderly population, one of the biggest uninsured financial burdens is the huge cost of long-term care (Brown and Finkelstein 2008). The average cost of nursing home stay is as high as $6,000 to $7,000 per month as of 2010.[46] Such high cost would exhaust one's resources quickly.[47] When private sources of payment (out-of-pocket or private insurance) exhaust, the public insurance – namely Medicaid – takes over as the source of support for long-term care needs.

Despite its effectiveness as safety net for the elderly, Medicaid provision for long-term care needs raises concerns of policy makers. One particular worry is that the means-tested nature of Medicaid may discourage saving, which leads to less asset accumulation and lower quality of

[45] Krueger and Meyer (2002) provide comprehensive review of the literature on the relationship between social insurance and labor supply. For the effect of public insurance on saving, see Gruber and Yelowitz (1999) and Kuan and Chen (2003, 2013).
[46] Source: U.S. Department of Health and Human Services. http://longtermcare.gov/costs-how-to-pay/costs-of-care/
[47] As of 2010, only 14% of elderly are covered by private long-term care insurance (Health and Retirement Survey).

the care received.[48] At the same time, souring government spending imposes fiscal pressure at both the federal and the state levels. Understanding behavior distortion associated with Medicaid provision is crucial for policy design.

This study empirically examines the association between Medicaid policy and asset holding behavior of the elderly. Specially, I examine whether Medicaid nursing home care coverage induces the elderly to strategically reduce their asset level. One particular behavior margin – and the focus of this study – is to transfer their resources to their children.

To establish causal relationship, I exploit variation in Medicaid eligibility generated by an exogenous policy shock: the Deficit Reduction Act of 2005. The policy imposed stricter penalty to asset transfer behavior when one seeks Medicaid coverage. The change led to less leeway for the elderly to make inter-vivos transfer in anticipation of Medicaid needs. If the elderly were "gaming" with the Medicaid system by strategically transfer their assets, we would expect such transfer to drop significantly after the policy change.

The main data come from the Health and Retirement Survey (HRS), survey years 2000 to 2008. The HRS provide rich longitudinal information for the elderly population. Importantly, individuals are asked to report the history of making financial transfers to their children. Additionally, they are asked about their subjective nursing home entry expectation. The main analysis relies on the two pieces of information.

Empirically, I use a difference-in-differences method. The choice of the treatment and control groups is based on one's perceived nursing home entry risk. The treatment group includes individuals who self-report having positive nursing home entry anticipation, thus are

[48] For the relationship between Medicaid coverage and nursing home care quality, see Grabowski (2003) and Grabowski *et al.* (2004).

most likely affected by the policy change. The control group includes individuals who self-report having no nursing home entry anticipation, thus are unlikely directly affected by the policy change.

The findings suggest that the DRA2005 had significant effect on the asset transfer behavior of the elderly. The treatment group – individuals who perceived positive nursing home entry risk – reduced the likelihood of making a gift/transfer that exceeds $500 to children or grandchildren by 3.2 to 4 percentage points. The result is robust across demographic groups, and is more pronounced among groups with highest nursing home entry risk.

The findings provide suggestive evidence of moral hazard in the context of Medicaid nursing home care coverage: the elderly population is indeed aware of the public provision option and gaming with the system. Such behavior distortion increases burden of public expenditure on long-term care. The policy intervention to eliminate such "gaming" opportunities is shown to be effective.

The rest of the paper is organized as follows: section 2 provides background information on the institutional setting of Medicaid nursing home coverage, the policy change and informal evidence of the effect of policy. Section 3 summarizes previous literature on relevant issues. Section 4 describes the data source and the key variables used. Section 5 outlines the empirical method. Section 6 provides the results and robustness checks. Finally, section 7 concludes.

2. Backgrounds

2.1 Medicaid nursing home care provision

The cost for institutionalized long-term care is a big component of the elderly's consumption portfolio. Four out of ten elderly decedents have used nursing home for some time

over the life course (Kemper and Murtaugh 1991). The average annual cost of nursing home care can be as high as $78,000 for a semi-private room.[49] Such cost is a financial burden for the elderly, who has limited income after retirement.

Normally, people reply on out-of-pocket payments and private insurance as the first method of payment.[50] When the private resources exhaust, Medicaid takes over as the secondary payer. Due to the significant annual cost and the long-term nature of nursing home needs, Medicaid serves as the largest payer for nursing home care. According to the reports by the Centers for Medicare and Medicaid Service, Medicaid and other forms of public insurance cover more than half of the aggregate cost.[51]

To be eligible for Medicaid, one needs to meet the requirements for income and asset levels. For income requirements, the applicant for Medicaid cannot have any source of income above a certain threshold. For asset requirements, the applicant must demonstrate that the total asset he/she possesses is below a certain threshold at the time of application. Both income and asset thresholds are set at state level. For potential beneficiaries, who are institutionalized elderly, the income requirement is often met. However, the asset threshold is sometimes binding (Norton 1995). As of 2009, 30 states set the asset limit at $2,000 for individual applicants and $3,000 for couples (Kaiser Foundation 2010). When one's asset is deemed above the threshold, he/she must use the asset to pay for nursing home cost first, and seek Medicaid coverage when the remaining asset is exhausted.

[49] Source: Metlife Mature Market Institute, 2011.
[50] The market for private long-term care insurance is very small. As of 2010, only 8.9% of the national total cost of long-term care is covered by private insurance (Centers for Medicare & Medicaid Service).
[51] The other public programs covering nursing home cost include veteran specific programs and Medicare. Medicare covers nursing home cost when it is medically necessary and imposes a cap of 100 days to the length of full or partial coverage.

The two unique feature of Medicaid, being the secondary payer and means-tested, impose an implicit tax on the asset of the elderly who intent to seek Medicaid coverage. Coe (2007) summarizes Medicaid coverage as an insurance with deductible equal to savings and income above the eligibility limits. Anecdotally, there are two ways the elderly could respond to avoid the embedded tax: reduce saving overall, or make inter-vivos transfer to children. This study focuses on the latter.

2.2 The Deficit Reduction Act of 2005

The Deficit Reduction Act of 2005 was signed by the President in February 2006. The DRA2005 makes the biggest change to financial eligibility of Medicaid nursing home coverage since the Medicare Catastrophic Coverage Act of 1988. It affects the elderly who seek Medicaid nursing home coverage in two ways: imposed stricter penalty to asset transfer and set limits for housing equity and other form of resources (ex. annuity) which was previous exempted from the asset test.

The first and most direct change made by the DRA 2005 is the modification to the penalty associated with pre-application asset transfer. Medicaid rule designed to discourage strategic asset transfer behavior is a "look-back period", which specifies a certain time window prior to Medicaid application. Any asset transferred at below market value during the look-back period is deemed violating Medicaid regulation. If a transfer was made and detected, a penalty period would be triggered based on the amount of the transfer. During this penalty period, the elderly would not have access to Medicaid.

Effective as it seems, the penalty period prior to 2006 could be avoided by strategic timing of the transfer (Liu and Waidmann 2005). This is possible because the start of the penalty

period was on the date of the transfer, not on the date of Medicaid application. For example, if a transfer was made on Jan 1, 2005 and the individual applies for Medicaid on Jun 1, 2005. Medicaid asset test determines that a penalty period of 4 months is triggered.[52] However, since the penalty period starts on the date of the transfer (Jan 1, 2005), the individual is in fact not affected by the penalty when he seeks coverage 6 months later (Jun 1, 2005).

The loophole of the design of penalty was closed by the DRA 2005, as it moveed the start date of penalty period to the date of Medicaid application (Jun 1, 2005 in previous example). In the meantime, the length of the "look-back period" was extended from 36 months to 60 months, making it harder to plan.

The policy also set limits on the housing equity and other form of resources (ex. annuity) that was previously exempt from the asset test. Owner-occupied housing equity is exempt from the asset test when one first apply for Medicaid. However, the Medicaid Estate Recovery programs allow states to collect the housing once the beneficiaries pass away. These programs, which were adopted by most states prior to 2005, ensure that housing assets cannot be exempt from compensating nursing home costs in the end.[53] The DRA 2005 imposed caps to the value of housing equity. The total amount of equity allowed to keep was set a high level: $500,000, with states' option to raise it to $750,000. For most of the population this study focuses on, their housing equity is far below the cap.

[52] The length of the penalty period is determined based on the amount of the transfer and the average nursing home cost incurs

[53] Greenhalgh-Stanley (2012) provides discussion of the effects of the estate recovery programs on homeownership among the elderly. The findings suggest that states' adoption of the program significantly reduced the likelihood that resources be avoided by keeping resources in the form of housing.

Overall, the DRA2005 is expected to have direct effect on the asset transfer behavior of the elderly with higher nursing home entry risks. In the meantime, the policy is unlikely to have direct effect on the behavior of elderly who has low nursing home entry risk. The nature of the policy creates a quasi-experimental setting for empirical analysis.

Figure 1 provides evidence for the effects of the DRA 2005. I plot the share of households that made transfer during the two-year survey window against survey year. I split the sample based on risk factors of nursing home use. The high nursing home entry risk groups are defined based on the subjective risk factors: having a catastrophic health shock (stroke), having difficulty of daily activities (ADLs) and having mental health problems (depression).[54] The rest of the population, denoted as the comparison group, is plotted separately. The simple time series trends suggest that the groups with high nursing home entry risk reduced their asset transfer behavior after 2006. Formal analysis is performed in section 6 to quantify the effect.

3. Literature review

Hubbard, Skinner, and Zeldes (1995) is one of the early works that establishes the theoretical framework of how Medicaid nursing home care coverage affects elderly saving behavior. The study builds on a life cycle model of household wealth accumulation, which explicitly includes the asset-based, means-tested social insurance. The model demonstrates that asset-based social insurance has a negative effect on asset accumulation, especially for low-income households.

Empirical evidence for the effect of Medicaid on saving among the elderly population has remained understudied. One set of literature tries to identify the effect based on structural

[54] These factors are significant predictors for nursing home admission: Gaugler *et al.* (2007), Cai *et al.* (2009).

framework. Feinstein and Ho (2000), for example, further extends the idea of life cycle model by detailing asset management in terms of saving, spend-down and gift giving. Though their work does not explicitly focus on the effect of Medicaid, it provides indirect evidence that negative health shock, which potentially increases need for Medicaid and increases the likelihood of asset spending down.

A different set of literature builds their identification strategy on exploiting cross sectional variation in terms of perceived nursing home entry risk of elderly individuals. Basset (2007), for example, documents a positive relationship between self-assessed nursing home entry risk and the likelihood of making a gift/transfer to children. However, one concern with interpreting the correlation as causal is the omitted variable bias. An individual may report high nursing home entry expectation because her child is in financial hardship and unable to provide informal care. This individual may at the same time be more likely to give the child financial help. The correlation may reflect mechanisms other than Medicaid eligibility, thus a causal interpretation is problematic.

To capture causality rather than correlation, one needs to exploit variation in Medicaid rules. Coe (2007) studies the financial response of the elderly to Medicaid by jointly exploiting two sources of variation: individual level variation in nursing home entry expectation and state level variation in program generosity. By generating an IV method, she finds that elderly do shift their consumption and asset allocation in response to Medicaid.

This study takes a similar approach as Coe (2007). The identification strategy replies on exogenous policy shock in Medicaid eligibility by the Deficit Reduction Act 2005. The nature of the policy change ensures that the estimation result reflects a causal relationship between

Medicaid eligibility and asset allocation choice. By utilizing the panel aspect of the longitudinal data set, it also avoids most of the omitted variable bias that previous literatures have.

4. Data and sample

The main data come from the Health and Retirement Survey (HRS). The HRS is a longitudinal dataset conducted every other year. The survey provides comprehensive information on a nationally representative sample of elderly. The HRS is well suited for the study of elderly asset allocation decisions as it contains not only direct measures of income and wealth, but also sufficient information on the dynamics of health conditions, choice of insurance and care utilization.

The main outcome of interest is the asset transfer behavior of the elderly. Such behavior is directly asked in the HRS: for each financial respondent, he/she is asked whether he/she gave financial help or gifts of $500 or more to any child or grandchild during the past two years. If the answer is yes, the respondent is further asked to specify the amount of transfer made. In 2006, on average 38% of the sample report having transferred some financial asset or gift. The average amount of transfer is $4,500. For all transfers, 31% exceeded $5,000. This suggests that such form of asset transfer could play a significant role in manipulating asset levels in response to Medicaid asset test.

Another main variable is the individual expectation of nursing home entry risk. This is measured by one's response to the question asking expected probability of entering nursing home in the next five years. The respondent is asked to report a numerical expectation ranging from zero to 100, which I rescale to a probability between zero and one. This measure is reflects people's perception of potential health condition as well as attitude towards nursing home

utilization. This perception measure is more accurate reflection of risk than demographic controls as it directly links to behavior response.[55]

Other control variables for regression analysis include standard demographic information: age, gender, race, educational level, marital status, number of children, and the region of residence. I also use a set of variables to measure health condition. These variables include self-reported health status, whether having difficulties in Activities of Daily Living (ADLs) and nursing home utilization.

I restrict the sample to respondents born in or before 1936. By doing so, I ensure that almost all observations are at least at the age of 70, mostly retired and preparing for long term care arrangement around the time of the policy shock. The sample cohorts utilized in this study include the initial HRS cohort (born 1931 to 1941), the AHEAD cohort (born before 1924) and the Children of Depression (CODA) cohort (born 1924 to 1930). I also restrict the sample to financial respondents, which ensures that one observation is taken per household. This study uses data from 2000, 2002, 2004, 2006 and 2008, though only the data of 2006 and 2008 is used for quantitative analysis.

5. Empirical methodology

The purpose of this study is to examine the effect of Medicaid nursing home care coverage on the asset transfer behavior of the elderly. As discussed in previous sections, the methodological difficulty is that the decisions to make asset transfer and to enter nursing home are made jointly. For example, people may substitute nursing home care using informal care

[55] Lindrooth *et al.* (2000) and Finkelstein and McGarry (2005) discuss the justification of using subjective measurement for nursing home entry expectation.

provided by their children. This group would also more likely compensate their children with monetary transfer. This would generate a negative correlation between Medicaid nursing home care use and asset transfer, which do not represent causal relationship.

To establish causal relationship, I exploit variation in Medicaid eligibility generated by the policy change imposed by DRA2005. The policy change would directly affect one's asset allocation decision, if this individual foresees nursing home entry risk in the near future. On the other hand, the policy would not have direct impact on individuals who do not anticipate nursing home needs in the near future. I use the latter group as the counterfactual.

Empirically, I follow a difference-in-differences approach. The treatment group and the control group are defined based on individuals' subjective nursing home entry risks. To ensure that the reported nursing home entry expectation is not endogenous to the policy shock in 2006, I use the pre-policy nursing home entry expectation (of year 2004) to determine treatment status.

Table 1 provides descriptive statistics of demographics of the two groups. These demographic characteristics are measured in 2006. The treatment group reports positive expected nursing home entry probability in 2004 while the comparison group reports zero expected nursing home entry probability in 2004. A few facts are worth noting: first, the treatment and comparison groups are similar in average age and gender composition. Second, the treatment group on average have fewer children. Third, the treatment group are more likely white and have college degree or above, suggesting that they are more likely of higher social economic status. Finally, both groups show similar pattern in report being in poor health. The treatment group, however, are less likely to report being in excellent health.

These observed differences between the two groups reflect factors that are associated with nursing home needs: the group with nursing home entry expectation are on average of worse health, have potentially worse access to informal care provided by children, and potentially have more wealth to compensate nursing home needs. One potential concern for identification is that the observed differences may lead to systematically different outcomes for the two groups. I address the concern by gradually including controls for the observed differences to examine the robustness of the results.

To quantify the policy effect, I perform the following Probit specification:

$$(1) \quad \Pr\{\text{transfer\_children}_{it}\} = \alpha + \beta_1 \text{NH\_expectation}_i + \beta_2 \text{Post\_policy}_t + \beta_3 \text{NH\_expectation} * \text{Post\_policy}_{it} + \beta_4 X_{it} + \epsilon_{it}$$

The dependent variable is a binary indicator of whether a transfer of $500 or more was made during the past two years. The right hand variables include a treatment dummy (NH_expectation) that equals one if the observation is in the treatment group (positive nursing home expectation in 2004), zero otherwise; a policy dummy (Post_policy) equals one if the observation is made in 2008, zero if the observation is in 2006; a time policy interaction term (NH_expectation $*$ Post_policy) and a set of control variables. The coefficient of interest, β_3, captures the effect of the policy.

The main identification assumption of the difference-in-differences method is that the treatment group would have the same trend in asset transfer behavior as the comparison group, absent of the policy shock. To examine the validity of this assumption, I graphically plot the time

trend of asset transfer behavior of the two groups using data from 2000 to 2008.[56] The time trends are shown in **Figure 2**. On average, the treatment group has a higher probability of making a transfer to their children or grandchildren. Prior to the policy change in 2006, the two groups have similar time trends in asset transfer behavior. This provides support to the validity of the identification assumption: there did not seem to be systematic difference in the time trends of asset transfer behavior over time between the treatment group and the comparison group. After the policy shock, the treatment group on average reduced their transfer, while the comparison group shows no significant drop. Such pattern provide graphically illustration of the treatment effect.

Additionally, the validity of the DID method requires that the relative change in asset transfer behavior between the two groups cannot be explained by factors other than the policy change. One potential concern is that the treatment group may develop worse health conditions over time, relative to the comparison group. If so, the drop in asset transfer behavior of the treatment group could be the result of the increasing medical cost. To test this possibility, I examine the trends in health condition and actual nursing home utilization of the two groups. **Figure 3** shows the time trends for three different measurements of interest. First, self-reported health condition is shown in panel A, ranking from one to five, with one denote being in perfect health and five denote being in poor health. Both groups overall show a worsened health condition over the period of interest. There is no significant difference of the two groups, especially for the time around 2006. Panel B plots the time trend of difficulties in Activities of Daily Living (ADLs). Again, both groups have similar trends over time. Panel C plots the actual

[56] The HRS provides information on asset transfer as early as 1998, but a large part of the observations were not retired nor near retirement. Including this wave would cause complications irrelevant to the purpose of this study.

nursing home utilization rate of the two groups. Both groups show increasing nursing home use with similar time trends. The treatment group does have a higher nursing home utilization. However, the difference of utilization appears as early as 2004, is time persistent and comparatively small in magnitude, thus unlikely to be the cause of the sudden change in asset transfer behavior observed for the treatment group after 2006. In the regression analysis in section 6, I include controls for health conditions and nursing home utilization for robustness check.

Another potential confounding factor is that the macroeconomic condition may affect the two groups in different ways. One story consistent with the observed pattern is that the treatment group appears to be of higher social economic conditions. The wealthier group would be more likely hurt in the financial crisis around 2006, as a higher portion of their asset tends to be invested in high-risk assets. To test if the financial crisis was instead driving the results, I plot the time trend of asset transfer behavior for different educational attainment groups in **Figure 4**. For different educational groups (high school dropouts, high school and some college, college and above), the time trends of average asset transfer are similar both before and after the policy change. This provides inconclusive but suggestive evidence that macroeconomic condition is unlikely an explanation of the drop in asset transfer of the treatment group.

6. Results and robustness checks

6.1 Baseline results

The results of the baseline specification following equation (1) are shown in **Table 2**. The coefficients reported are marginal effects.[57] Standard errors are clustered at individual level. In

[57] The marginal effects of the interaction term is calculated based on Norton *et al.* (2004).

column 1, I show the results of a specification with no additional controls. The results represent a quantitative illustration of the pattern in **Figure 2**. The coefficient of interest, the one of the interaction term, suggests that the treatment group reduced the probability of making a financial transfer to children or grandchildren by 3.25 percentage points after the policy change. The magnitude represents a 9% decrease of the baseline likelihood of making a transfer.

To take into account the observed differences in demographics between the treatment and the control groups, I further include a set of demographic controls. These control variables include age, age square, educational attainment, gender, race, marital status and number of children. As shown in column 2, the inclusion of demographic controls does not change the magnitude and the statistical significance of the estimate.

Column 3 further includes census region controls. These are included to control for geographical variation in Medicaid eligibility and generosity. As discussed in the background section, states vary in the income and asset limits when determining Medicaid eligibility. The income and asset limits may directly affect perceived nursing home entry risk as well as asset holding behavior. State identifier is not available in the public use version of the HRS. I instead use census region indicators as proxy. The estimate is robust.

The preferred specification in Column 4 further includes controls for health conditions. Three sets of controls are included: self-reported health condition, level of difficulties with ADLs and the use of nursing home in the past two years. Again, the estimate is robust. The results suggest that, the treatment group reduced asset transfer behavior by 3.2 percentage points relative to the comparison group as the result of the policy change.

6.2 Alternative treatment and comparison groups

The baseline specification divides the whole sample into the treatment and comparison based on having zero or positive expected nursing home entry risk. One concern with the choices of treatment and comparison groups is the potential measurement error in self-reports. People tend to give round number as answers to the survey question. Additionally, individuals who report a small value of probability, such as 0.01, may in fact indicate no nursing home entry expectation.

To address these issues, I examine the robustness of the estimates using alternative probability cutoffs to determine treatment and comparison groups. Following Coe (2007) and Finkelstein and McGarry (2005), I use 0.1 and 0.5. The rationale of such division is that even though a numerical value for reported nursing home entry risk may not accurately measure an individual's belief, it provides sufficient information of the individual's perception in a categorical manner. In other words, when an individual report a probability of 0.6, it may not indicate that this individual is facing a higher probability than someone who reports 0.4, but reflects that this individual sees herself as having high nursing home entry risk in the near future.

I perform the difference-in-differences specifications using the alternative treatment and comparison groups. The results are shown in **Table 3** and **Table 4**. For all specifications with various sets of control variables, the results suggest that policy change led to a drop in asset transfer behavior among the treatment group. The magnitudes of the treatment effect range from 3.6 percentage points to 4.1 percentage points. Moreover, the policy effect is more pronounced for the group with the highest self-reported nursing home entry risk (**Table 4**). Such pattern is consistent with the intuition that the most affected group is those who are entering nursing home in the nearest future.

6.3 Subgroup analysis

This section examines how the effects of the policy change differ across various demographic groups. To test the robustness of the result across different demographic groups, I further split sample based on age, educational level and gender of the respondent. The results using different subgroup are reported in **Table 5**. Each column reports the marginal effects using the preferred specification with the full set of control variables.

Columns 1 and 2 show how the effects differ across age groups. I split the sample based on their age in 2006. The results suggest that the estimated treatment effect in the baseline specification is primarily driven by the elderly of ages 70 to 80. For the oldest olds, who are of age greater than 80 in 2006, the policy change did not have significant effect. This suggest that the oldest olds were not as sensitive as younger cohorts were to Medicaid eligibility, which may reflect their lack of cognition ability.

Column 3 and 4 show how the effects differ for individuals with varied educational attainments. The results suggest that high school dropouts are most responsive to the policy change. Such pattern is consistent with findings in previous literature (Basset 2007, Goda et al 2011) that the less educated group has lower income and asset level, and is more likely to rely on Medicaid. Such group is more likely the marginal individuals affected by the policy changes of Medicaid eligibility.

Column 5 and 6 display the effects of the policy change on male and female, respectively. The results suggest that the treatment effects are consistent for the male and the female subgroups.

7. Conclusion and Discussion

This study examines the effect of Medicaid coverage for nursing home care cost on the asset allocation of the elderly. In particular, I examine whether the means-tested nature of Medicaid induces the elderly to transfer their resources away to their children. To establish causal relationship, I rely on a policy change in DRA2005, which imposed strict penalty on asset transfer behavior prior to Medicaid application. I perform a difference-in-differences approach using data from the Health and Retirement Survey. I find that the policy change significantly reduced the likelihood of inter-vivos transfer among the elderly who anticipated nursing home entry in the near future.

The results provide supportive evidence that Medicaid provision leads to behavior distortions among the elderly. In particular, potential beneficiaries of Medicaid would strategically reduce their asset level in order to qualify for the coverage. One possible avenue to reduce the asset level is to transfer the asset to their children. Findings in this study also suggest that regulatory change targeted at eliminating such behavior – enforcing strict penalty when asset transfer is detected – seems to be effective.

A major limitation of the study is the potential reporting errors in the self-reported asset transfer. One particular worry is that the reporting error may be directly affected by the respondent's' perception of the policy change. Knowing that making transfer to children may trigger stricter penalty, individuals who anticipate nursing home needs may underreport any transfer history even though they did not change their true behavior. Alternative data source that documents asset transfer behavior, for example tax return files, may provide a better description of true behavior than the self-reports.

There are two important yet unaddressed questions for future analysis. First, the analysis extended to two years after the policy change. It remains unclear whether the drop in asset

transfer in the observed period reflects an overall reduction of inter-vivos transfer, or instead reflects a shift in timing of the transfer. The difficulty associated with conducting long-term analysis is the nature of the population: the targeted group of Medicaid nursing home care coverage are the elderly who are very old and would decease relatively quickly. Thus, extending the study period is at the cost of reduction in size and change in composition of the sample.

Second, it remains unclear whether the drop in gift giving reflects a true reduction in asset avoidance. One possible alternative to gift giving is to preserve assets through trusts. According to Medicaid regulations, resources put in irrevocable trusts are exempt from asset test, but are subject to the look back period and the associated penalty. On the one hand, having strict penalties would potentially reduce asset avoidance through trust. On the other hand, the elderly may potentially respond to the policy change by planning ahead and establish trust before the five years look back period. Greenhalgh-Stanley (2012) provide evidence that the elderly would seek alternative source of asset transfer, when the housing equity is no longer exempt from reimbursing Medicaid. Additional research is needed to understand the effect of the policy on alternative behavior margins.

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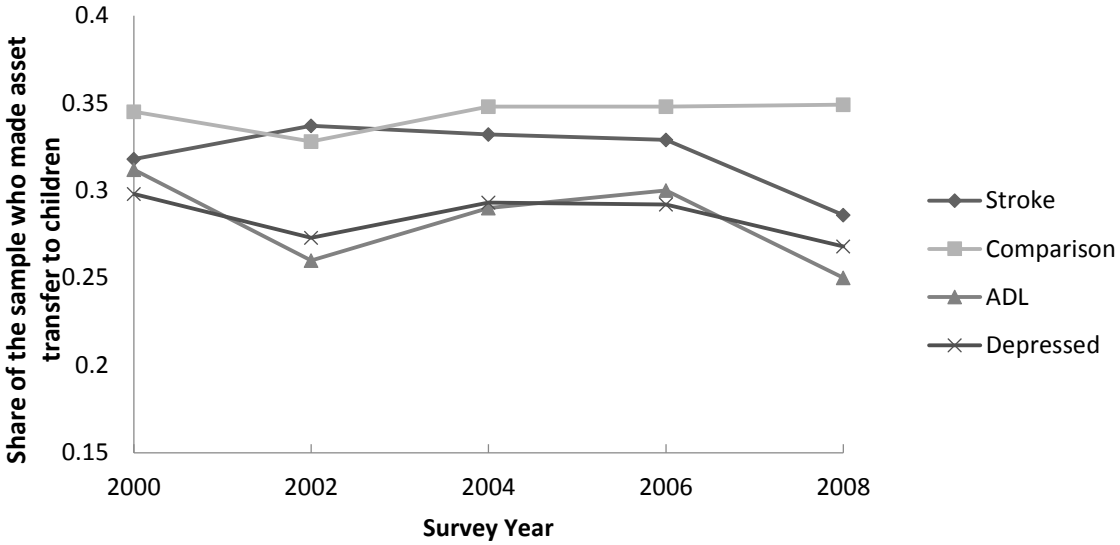
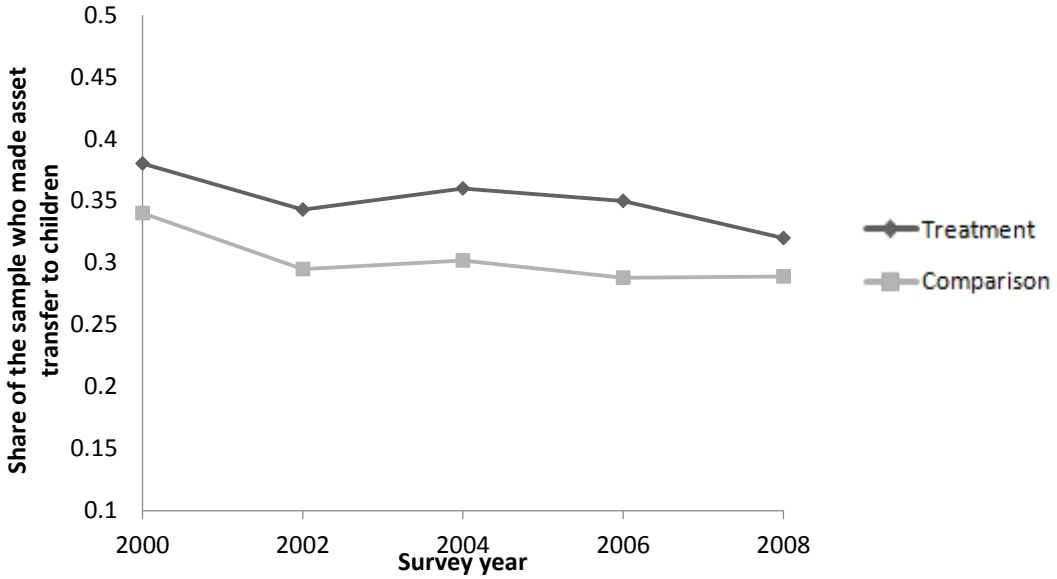
Figure 3-1: Time trends: share of respondents made a transfer of $500 or more during the past two years, by health conditions.



Notes: Data come from Health and Retirement Survey, survey years 2000, 2002, 2004, 2006 and 2008. Sample include all financial respondents who responded to the question regarding asset transfer. The full sample is split into four groups: “Stroke” includes individuals who were diagnosed stroke since last interview; “ADL” includes individuals who reported having difficulties in Activities of Daily Living for the first time since last interview; “Depressed” includes individuals who reported feeling depressed since last interview; “Comparison” includes all other individuals.

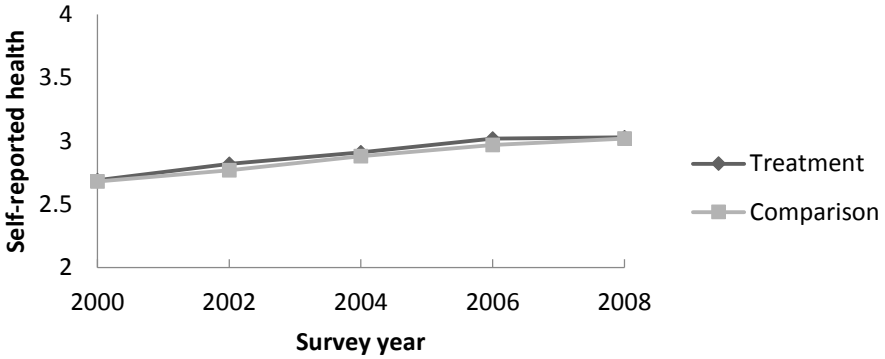
Figure 3-2: Time trends: share of respondents made a transfer of $500 or more during the past two years; by treatment status.



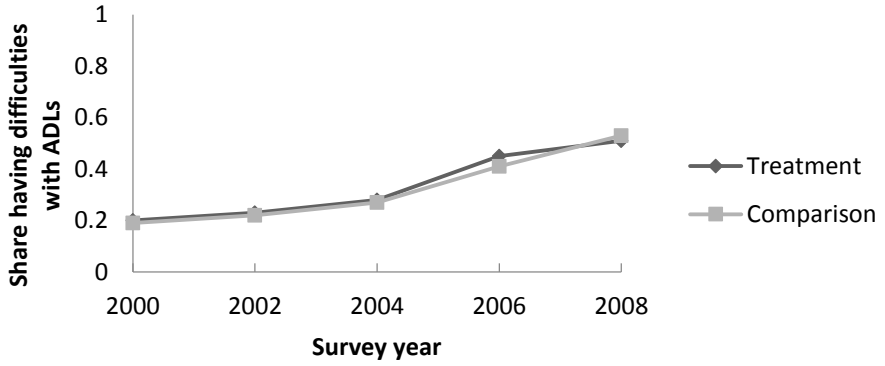
Notes: Data come from Health and Retirement Survey, survey years 2000, 2002, 2004, 2006 and 2008. Sample include all financial respondents who responded to the question regarding asset transfer. The treatment group consists of respondents who reported positive nursing home entry probability in 2004. The comparison group consists of respondents who reported zero nursing home entry probability in 2004.

Figure 3-3: Time trends of health condition and nursing home care utilization: 2000-2008

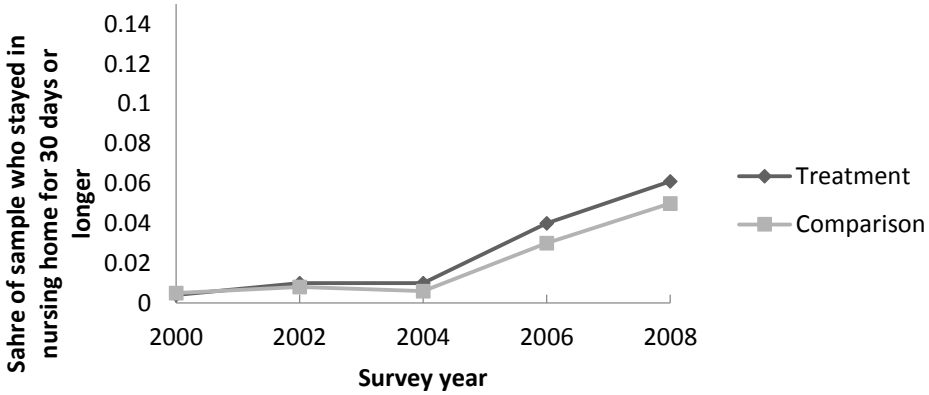
A: Self-reported health.



B: Difficulty of Activities of Daily Living (ADLs)

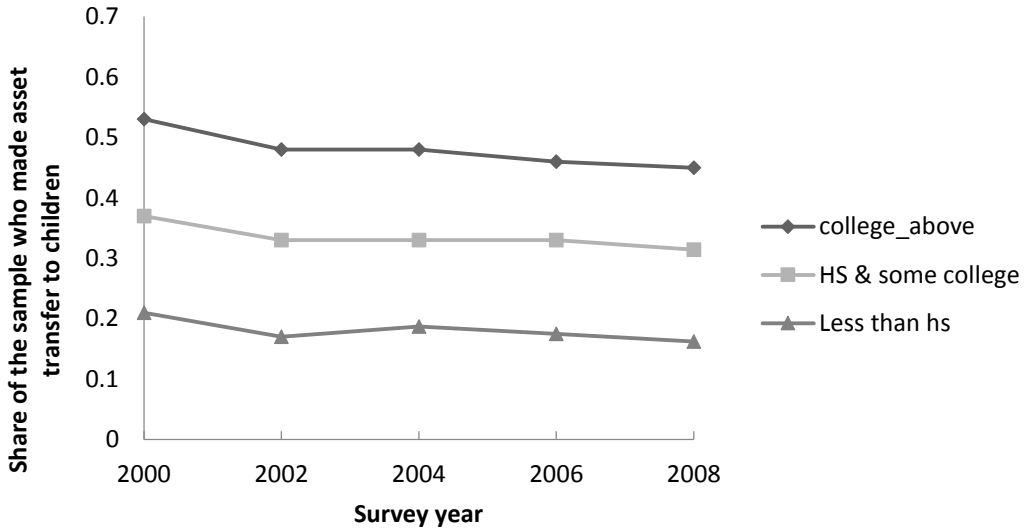


C: Long-term nursing home stay (30 days or longer)



Notes: Data come from Health and Retirement Survey, survey years 2000, 2002, 2004, 2006 and 2008. Sample include all financial respondents who responded to the question regarding asset transfer. The treatment group consists of respondents who reported positive nursing home entry probability in 2004. The comparison group consists of respondents who reported zero nursing home entry probability in 2004.

Figure 3-4: Time trends for asset transfer behavior by educational attainment: 2000-2008.



Notes: Data come from Health and Retirement Survey, survey years 2000, 2002, 2004, 2006 and 2008. Sample include all financial respondents who responded to the question regarding asset transfer.

Table 3-1: Summary of characteristics (in 2006), by treatment status

	Control group	Treatment group
Age	77.32	78.37
	(6.388)	(6.610)
Number of children	3.546	3.182
	(2.449)	(2.157)
HS/GED	0.344	0.330
	(0.475)	(0.47)
Some college	0.186	0.195
	(0.389)	(0.396)
College degree and above	0.141	0.242
	(0.348)	(0.428)
White	0.820	0.883
	(0.385)	(0.321)
Married	0.398	0.420
	(0.49)	(0.494)
Male	0.410	0.426
	(0.492)	(0.495)
Excellent health	0.093	0.069
	(0.29)	(0.254)
Poor health	0.097	0.099
	(0.296)	(0.299)
# of obs.	2571	2350

Notes: Sample include financial respondents born in or before 1936, and have responded to the question regarding nursing home anticipation in 2004. The outcome variables are measured in 2006. Standard deviations are reported in parentheses.

Table 3-2: Baseline results.

	(1)	(2)	(3)	(4)
NH expectation	0.065***	0.0364***	0.0359***	0.0378***
	(0.0137)	(0.0136)	(0.0136)	(0.0136)
Post_policy	0.001	0.0033	0.034	0.0056
	(0.0110)	(0.0111)	(0.0111)	(0.0112)
NH expectation*Post_policy	-0.0325**	-0.0317**	-0.0317**	-0.0320**
	(0.0156)	(0.0156)	(0.0156)	(0.0156)
Demographics	N	Y	Y	Y
Region fixed effects	N	N	Y	Y
Health condition	N	N	N	Y
Observations	8462	8462	8460	8439

Notes: Sample include financial respondents born in or before 1936, and have responded to the question regarding nursing home anticipation in 2004. NH expectation equals one if self-reported probability of NH entry is positive, zero if self-reported probability of NH entry is zero. Demographics controls include age, age square, educational attainment, gender, race, marital status and number of children. Health condition controls include self-reported health condition, difficulties in ADL and nursing home utilization dummy. Marginal effects are reported. Standard errors are clustered at individual level, reported in parenthesis. ***, **, * denotes statistical significance at the 1 percent, 5 percent, and 10 percent level respectively.

Table 3-3: Alternative probability cutoff: 0.1

	(1)	(2)	(3)	(4)
NH expectation	0.0648***	0.0425***	0.0424***	0.0446***
	(0.0138)	(0.0137)	(0.0137)	(0.0137)
Post_policy	0.0011	0.0039	0.0039	0.0052
	(0.0104)	(0.0106)	(0.0106)	(0.0106)
NH expectation*Post_policy	-0.0368**	-0.0367**	-0.0367**	-0.0376**
	(0.0158)	(0.0158)	(0.0158)	(0.0158)
Demographics	N	Y	Y	Y
Region fixed effects	N	N	Y	Y
Health condition	N	N	N	Y
Observations	8462	8462	8460	8439

Notes: Sample include financial respondents born in or before 1936, and have responded to the question regarding nursing home anticipation in 2004. NH expectation equals one if self-reported probability of NH entry is greater than 0.1, zero if self-reported probability of NH entry is smaller than or equal to 0.1. Demographics controls include age, age square, educational attainment, gender, race, marital status and number of children. Health condition controls include self-reported health condition, difficulties in ADL and nursing home utilization dummy. Marginal effects are reported. Standard errors are clustered at individual level, reported in parenthesis. ***, **, * denotes statistical significance at the 1 percent, 5 percent, and 10 percent level respectively.

Table 3-4: Alternative probability cut-off: 0.5

	(1)	(2)	(3)	(4)
NH expectation	0.0343*	0.0455**	0.0460**	0.0578***
	(0.0181)	(0.0179)	(0.0179)	(0.0180)
Post_policy	-0.0075	-0.0051	-0.0050	-0.0040
	(0.0085)	(0.0088)	(0.0088)	(0.0088)
NH expectation*Post_policy	-0.0409**	-0.0385**	-0.0384**	-0.0393**
	(0.0213)	(0.0214)	(0.0214)	(0.0215)
Demographics	N	Y	Y	Y
Region fixed effects	N	N	Y	Y
Health condition	N	N	N	Y
Observations	8462	8462	8460	8439

Notes: Sample include financial respondents born in or before 1936, and have responded to the question regarding nursing home anticipation in 2004. NH expectation equals one if self-reported probability of NH entry is greater than 0.5, zero if self-reported probability of NH entry is smaller than or equal to 0.5. Demographics controls include age, age square, educational attainment, gender, race, marital status and number of children. Health condition controls include self-reported health condition, difficulties in ADL and nursing home utilization dummy. Marginal effects are reported. Standard errors are clustered at individual level, reported in parenthesis. ***, **, * denotes statistical significance at the 1 percent, 5 percent, and 10 percent level respectively.

Table 3-5: Subgroup analysis.

	(1)	(2)	(3)	(4)	(5)	(6)
	Age <=80	Age>80	HS dropouts	HS/GED and above	Male	Female
NH expectation	0.0405**	0.0243	0.0799***	0.0230	0.0310	0.0441**
	(0.0169)	(0.0219)	(0.0224)	(0.0166)	(0.0219)	(0.0174)
Post_policy	0.023	-0.0181	0.0124	0.0023	0.0022	0.00673
	(0.0139)	(0.0197)	(0.0185)	(0.0138)	(0.0184)	(0.0141)
NH expectation*Post_policy	-0.0445**	-0.0125	-0.0481*	-0.0279	-0.0366	-0.0297
	(0.0190)	(0.0259)	(0.0277)	(0.0189)	(0.0255)	(0.0197)
Demographics	Y	Y	Y	Y	Y	Y
Region fixed effects	Y	Y	Y	Y	Y	Y
Health condition	Y	Y	Y	Y	Y	Y
Observations	5689	3101	2193	6242	3529	4910

Notes: Sample include financial respondents born in or before 1936, and have responded to the question regarding nursing home anticipation in 2004. NH expectation equals one if self-reported probability of NH entry is positive, zero if self-reported probability of NH entry is zero. Demographics controls include age, age square, educational attainment, gender, race, marital status and number of children. Health condition controls include self-reported health condition, difficulties in ADL and nursing home utilization dummy. Marginal effects are reported. Standard errors are clustered at individual level, reported in parenthesis. ***, **, * denotes statistical significance at the 1 percent, 5 percent, and 10 percent level respectively.

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