

ABSTRACT

Title of dissertation: **THREE ESSAYS IN PUBLIC FINANCE**

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Much of public economics research examines the public sector's use of policy to influence the behavior of individuals to achieve societal goals. Because there may be many different policies to achieve the same societal goal, public economists are generally concerned with policy efficiency: Which policy among many will yield the desired result with the least amount of distortion or welfare loss? The aim of these three essays is to contribute to this discussion by examining the intended and unintended consequences of contemporary social policy.

The first essay estimates the elasticity of taxable earnings to taxation. Taxation may improve social welfare by redistributing income and to support public infrastructure. However, taxation may also generate disincentives to work; so as tax rates rise, the amount of income subject to taxation plausibly declines. Because the net effect of proposed tax reform on government revenue depends on how elastic taxable earnings are to taxation, the response of earnings to taxation is fundamental in assessing the efficiency of the US tax code.

The second essay examines the impact of increasing the Social Security normal retirement age from 65 to 67 on the Social Security Disability Insurance (DI) rolls. Although increasing the full retirement age was intended to decrease program expenditures by providing incentives to delay the transition to retirement, the policy simultaneously increased the incentive to receive DI benefits. DI benefits are generally more generous and received over a longer period of time relative to retirement benefits; so the effect of increasing the normal retirement age on program expenditures may be overstated if the rise in the DI rolls is not taken into account.

The third and final essay examines a recent policy change to Title 38 which granted disability benefits to Vietnam veterans for diabetes considered *presumptively* related to herbicide exposure during military service. In this essay, we explore the impact of this policy on the rolls and expenditures of the VA disability compensation program.

THREE ESSAYS IN PUBLIC FINANCE

by

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DEDICATION

I thank my family for their sacrifices. Without your love and encouragement, this dissertation would not have been possible.

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The Effect of Taxes on Taxable Earnings

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ACING TO RETIRE?

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FEDERAL POLICY AND THE RISE IN DISABILITY ENROLLMENT

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Chapter 1

The Effect of Taxes on Taxable Earnings: Evidence from the 2002-2004 US Federal Tax Acts

Abstract: I estimate the elasticity of taxable earnings to taxation among married couples at the joint and individual levels, identified by changes in marginal tax rates due to the implementation of marriage penalty relief provisions in 2003. The main data source for the empirical analysis is unique to the taxation literature: the 2002 and 2003 Current Population Surveys linked to individual level W-2 forms submitted to the Internal Revenue Service. Similar to previous studies, I instrument the observed change in the marginal tax price between 2002 and 2004 with the predicted change in the marginal tax price based on real earnings in 2002. However, there are two potential concerns for this strategy. First, as stressed in the literature, earnings in 2002 may be correlated with systemic movements in income that are unrelated to the reform. And second, a single year of prereform earnings may be a noisy measure of permanent income. To address these particular concerns, I consider an alternative instrument constructed from the average of multiple years of prereform earnings. Average taxable income, when taken as an estimate of the fixed

effect on taxable income, is arguably better at identifying the effect of taxation on earnings. I find that elasticity estimates increase considerably using these alternative instrumental variables compared to using a single year of prereform earnings. For example, the estimated elasticity of taxable earnings to taxation among married men increases from .164 (.112) to .664 (.310) when one versus four years of prereform earnings, respectively, are used to construct predicted changes in marginal tax prices. Qualitatively, the results suggest that earnings of married, educated men are more sensitive to taxation relative to their less educated counterparts and married women when these alternative instruments are used.

1.1 Introduction

The elasticity of taxable income or earnings to taxation is fundamental in evaluating the efficiency of the income tax code, having direct implications for efficient revenue maximization and welfare loss. Unfortunately, economic theory provides very few unambiguous predictions regarding the effects of taxation. Indeed, the standard labor model with proportional taxation predicts that an increase in the after-tax wage generates counteracting income and substitution effects, yielding an ambiguous effect on labor hours and therefore gross income. Due to this theoretical limitation, studies that examine the response of earnings to taxation are empirical. However, empirically identifying the behavioral response is complicated by the fact that theoretically important tax parameters are determined simultaneously with income. This arises due to the progressive nature of the US income tax code where

marginal tax rates increase with income. To identify the response of earnings to taxation, recent tax studies have examined the relationship between *changes* in income to *changes* in marginal tax rates induced by reforms to the tax code. Among the class of studies that use this quasi-experimental approach, elasticity estimates of income to taxation range from zero to three (Gruber and Saez, 2002).

The objective of this paper is to estimate the effect of taxation on taxable earnings by exploiting differential changes in marginal tax rates generated by the Economic Growth and Tax Relief Reconciliation Act (EGTRRA) of 2001. The EGTRRA, estimated to cost more than \$1.35 trillion over the first ten years of its enactment, was the largest tax reform in over 20 years. To my knowledge, there has been no systematic analysis of the EGTRRA regarding the behavioral response of earnings to taxation. Unlike variation in marginal tax rates considered in many quasi-experimental tax studies, the EGTRRA differentially affected marginal tax rates at very dense segments of the income distribution. Thus, elasticity estimates are derived for a segment of the income distribution which are generally difficult to identify using quasi-experimental methods.

The cleanest experiment, and the focus of this paper, occurs at taxable incomes (TIs) around \$46,700 (in 2002 dollars), the TI threshold that defines the 15% income tax bracket from the 27% bracket for joint filers.¹ According to the EGTRRA, this threshold for joint filers was scheduled to increase relative to the threshold for single filers, reaching twice the threshold faced by single filers by 2008 (prior

¹Taxable income is the definition of income applied to the income tax rate schedule, defined as adjusted gross income (a comprehensive measure of income subject to taxation) minus personal exemptions and deductions.

to EGTRRA, the threshold for married couples was just 67% larger than those faced by single filers). The Jobs Growth and Tax Relief Reconciliation Act of 2003, however, fully implemented this change in tax year 2003. As a result, couples with TIs roughly between \$46,700 and \$54,194 (the new threshold in 2004 expressed in 2002 dollars) would have experienced a decline in the marginal tax rate from 27% to 15% from tax year 2002 to 2004 if real taxable income had remained constant. According to the 2001 version of the Statistics of Income (SOI) Public Use Tax File, a data set compiled from tax returns filed with the Internal Revenue Service, this provision affected approximately 3.2 million (8.2%) married couples. If the decline in marginal tax rates had an effect on income through, for example, labor supply or 401(k) contributions, then the change in income among these affected couples should reflect to some degree the behavioral response of income to taxation. There was little to no predicted change in marginal tax rates for couples with joint TIs just below \$46,700 and above \$54,194, so these couples serve as an obvious comparison group.

The data source for this study - the 2002 and 2003 March Supplements of the Current Population Survey (CPS) linked to the Social Security Administration's Detailed Earnings Record (DER) file - is unique to the taxation literature. The DER is compiled from individual level W-2 forms submitted to the Internal Revenue Service. For each person matched to the CPS, the DER provides longitudinal information on total wage income, taxable wage income, Social Security and Medicare taxable wages, and Social Security and Medicare taxable self-employment income. These income measures are available annually from 1978 to 2004.

The CPS-DER matched data has several advantages compared to other data previously used in quasi-experimental tax studies. First, unlike earnings data compiled from multiple cross-sections, earnings of treated and comparison couples may be examined before and after a tax law change. Second, the link of the DER and the CPS provides rich demographic information which is typically unavailable in administrative data alone. This allows, for example, the ability to assess whether treatment and comparison groups in quasi-experimental tax studies are indeed comparable and to examine heterogeneous responses to taxation across demographic dimensions. Third, the CPS-DER matched data allows for the examination of income among married couples at the individual rather than couple level. Studies that use data compiled from federal tax returns have been limited in this regard since most married couples file, and therefore report income, jointly.

There are two main limitations of the CPS-DER for computing TI required to assign couples into treated and comparison groups: the CPS-DER does not contain (1) other sources of income, in addition to taxable wage income and positive self-employment income, subject to taxation and (2) deduction amounts, whether standard or itemized. These issues are examined and addressed using the SOI.

The empirical strategy consists of regressing the change in log taxable earnings between 2002 and 2004 on the actual, observed change in the log marginal tax price (one minus the marginal tax rate) over the same period. However, as Gruber and Saez (2002) and others have mentioned, taxable earnings and the marginal tax price are determined simultaneously, so shocks to earnings that are unrelated to the reform may affect the observed change in the tax price. As a result, the estimated

elasticity of income to taxation may be biased when using an ordinary least squares estimator.

To isolate variation in tax prices due to the reform, studies generally instrument the observed change in the marginal tax price with the predicted change in the marginal tax price based on a single year of prereform earnings, i.e. the marginal tax price after the reform had real prereform earnings remained constant. This method too has been called into question since a single year of prereform earnings may be differentially correlated with systemic movements in earnings before and after the reform which are not related to the reform itself. Furthermore, if earnings are considered to reflect transitory shocks or measurement error, then another concern with this instrument is that assignment of couples into treatment and comparison groups may be imperfect.²

In this study, I address these particular concerns by considering alternative instrumental variables constructed from the average of multiple years of prereform earnings. Intuitively, average prereform earnings presumably isolates the effect of time-invariant factors on earnings. The instrument is the predicted change in the marginal tax price based on average prereform earnings. If this alternative instrument, constructed from estimated fixed effects, is less correlated with transitory movements in income before and after the reform, then any insinuating bias that may arise from constructing an instrument based on a single year of earnings would be attenuated when these alternative instruments are used. Additionally, averaging

²Empirically, contaminated treatment and comparison groups implies non-classical measurement error predicted change in marginal tax prices. This point is discussed in more detail in the estimation strategy section.

several years of prereform earnings plausibly nets out measurement error in earnings, so assignment of couples into treatment and control groups based on average prereform earnings reduces the degree of cross-contaminated treatment and comparison groups.

In the Results section, I first present elasticity estimates using the estimation strategy considered in much of the previous tax literature. In particular, I instrument the observed change in the marginal tax price between 2002 and 2004 using the predicted change in marginal tax price based on 2002 earnings singly. The estimated, joint earnings elasticity is .063 (.063), which is comparable to the joint broad income elasticity estimate of .071 (.130) in Gruber and Saez (2002).³ Additionally, elasticity estimates appear similar when estimated among husbands and wives separately: .164 (.112) and .135 (.179) for husbands and wives, respectively. This is consistent with comparable, individual estimates in Looney and Singhal (2004), but the estimates differ in magnitude: .411 (.464) and .360 (.665) for husbands and wives, respectively.

I then estimate earnings elasticities using the proposed instrument, calculated as the predicted change in the marginal tax price from 2002 to 2004 based on the average of multiple years of prereform earnings. I consider up to three lags of annual earnings; from 2002 back to and including 1999. Elasticity estimates for married men increase considerably when using these alternative instruments. For example, constructing the instrument using a single lag of earnings increases the

³Gruber and Saez define broad income as total income minus capital gains and social security benefits.

elasticity estimate from .164 (.112) to .421 (.175). Averaging prereform earnings back to 1999 further increases the estimate to .664 (.310). Additionally, when these alternative instruments are used, the response of earnings to marginal tax prices appears to be greater among educated men compared to less educated men. Among husbands with some college and beyond, the estimated elasticity increases from .138 (.114) to .657 (.388), compared to an increase from .175 (.183) to .468 (.695) among husbands with no college experience. In contrast to husbands, there is no patterned increase in elasticity estimates among wives. Taken together, the results suggest that earnings among educated, married men are more responsive to taxation than their less educated counterparts and married women.

1.2 Background

1.2.1 Source of Identification

The EGTRRA of 2001, estimated to cost \$1.35 trillion over the first ten years of its enactment, was the largest tax reform in 20 years (Steurle, 2004).⁴ In anticipation of continued budget surplus, the EGTRRA reduced marginal tax rates for approximately 62 percent of taxpayers and reduced average tax rates for all taxpayers (Kiefer et al., 2002). Many provisions of EGTRRA were designed to phase in slowly over time and would become inapplicable after 2010, the Act's sunset year.

However, the Jobs Growth and Tax Relief Reconciliation Act (JGTRRA) of 2003

⁴Relative to the size of the economy, the 1981 tax cut was about twice as large as the EGTRRA (Steurle, 2004).

and the Working Family Tax Relief Act (WFTRA) of 2004 expedited the phase in of many EGTRRA provisions (effective dates of select EGTRRA provisions and estimated costs are provided in appendix tables A.1 and A.2, respectively).

The implementation of the EGTRRA and related Acts generated considerable shifts in federal marginal tax rates which can be used to identify the effect of taxation on income. The cleanest tax experiment, and the focus of this paper, results from a provision designed to ameliorate marriage penalties in the tax code. A marriage penalty occurs when, holding individual incomes constant, a couple's joint tax liability increases upon marriage.⁵ To see how marriage penalties arise, consider a couple who is contemplating marriage in 2000. If each individual files singly, both may claim the standard deduction of \$4,400 (the standard deduction amount in 2001 for an individual who files singly). Upon marriage, if the couple files jointly, they may claim the standard deduction of \$7,350, resulting in a loss of \$1,450 in deductions. Since deductions serve to reduce the amount of income subject to taxation, the resulting increase in taxes owed depends on the couple's marginal tax rate. If the marginal tax rate for this couple is 28%, the couple faces a marriage penalty of \$406.⁶

A second marriage penalty results from the tax rate schedule because the TI thresholds that define income tax brackets for married, joint filers are less than twice the thresholds for single filers. For example, the TI thresholds that define the 15%

⁵Conversely, a marriage subsidy occurs when a couple's tax liability declines upon marriage. See Alm, Dickert-Conlin, and Wittington (1999) for an analysis of marriage penalties in the US tax code.

⁶In 2000, the marginal tax rate was 28 percent for joint taxable income between \$43,850 and \$105,950 for married couples and individual taxable income between \$26,250 and \$63,500 for single filers.

tax bracket from the 28% tax bracket were \$26,250 and \$43,850 for single and joint filers, respectively, in 2000. To see how this feature of the tax schedule generates a marriage penalty, assume each individual in the example above each have \$30,000 in TI. In 2000, prior to marriage, each individual owes \$4,987.50 (15% rate up to \$26,250 and 28% for the remainder); but upon marriage, the couple is liable for an additional \$1,124.50 in taxes (15% rate up to \$43,850 and 28% for the remainder, totaling \$11,099.5 in tax liability).⁷

The EGTRRA was designed to eliminate these marriage penalties by gradually increasing both the standard deduction and the upper bound of the 15% tax bracket for joint filers to twice those faced by single filers. Although the standard deduction and upper bound of the 15% tax bracket for married, joint filers were not originally scheduled to reach twice the bound and bracket faced by single filers until 2009 and 2008, respectively, the JGTRRA fully implemented both of these increases in tax year 2003. The exact tax code parameters are provided in Table 1.1: Panel A reflects tax parameters according to EGTRRA and Panel B reflects additional changes due to the JGTRRA and the WFTRA. The values of the standard deduction amount and the taxable income threshold in 2003 with and without the passing of JGTRRA are underlined in Panels A and B.

As a result, absent any change in real TI, couples whose 2002 joint TI was roughly between \$46,700 and \$54,194 (the new threshold in 2004 expressed in 2002 dollars) would have experienced a decline in their marginal tax rates from 27% to

⁷The size of marriage penalty due to this feature of the tax rate schedule depends on the composition of joint earnings. This is not a cause for concern in this study since the sample is conditioned on those already married, and the provisions used for identification affected all married couples similarly.

15% by tax year 2004.⁸ If taxation does affect earnings through, for example, hours worked and 401(k) contributions, then the change in earnings between 2002 and 2004 among these couples should reflect the effect of taxation on earnings. Couples whose joint TI was just below \$46,700 serve as a comparison group as marginal tax rates for these couples would have remained constant at 15% from 2002 to 2004 (henceforth referred to as comparison group one). Additionally, couples with TIs just above \$54,194 would have experienced a decline in marginal tax rates of just 2 percentage points (from 27% to 25%), so these households serve as an appropriate comparison group as well (henceforth referred to as comparison group two).

To illustrate this quasi-experiment, I plot federal marginal tax rates in 2002 and 2004 by adjusted gross income (AGI) (in 2002 dollars) and family size (Figure 1.1).⁹ Since AGI in 2004 is expressed in 2002 dollars, these marginal tax rate changes reflect real changes that result from the tax reform rather than changes that result from nominal wage growth. I plot these rates against AGI because AGI is a more comprehensive measure of income than TI. To calculate TI, which is the definition of income that determines the marginal tax rate, personal exemptions (based on family size) and the standard deduction amount for married, joint filers were subtracted from AGI.

In the top-left panel of Figure 1.1, comparison group one, the treated group, and comparison group two are respectively labeled C1, T, and C2. Although the TI

⁸To be more precise, the range is more accurately between \$47,900 and \$55,394 if the couple uses the standard deduction since the standard deduction for married, joint filers increased in real terms from \$7,850 to \$9,050 (2002 dollars) from 2002 to 2004. In the empirical analysis, the treated and comparison groups are identified by the predicted change in federal marginal tax rate.

⁹Adjusted gross income is defined as the sum of income subject to taxes minus certain statutory adjustments (e.g. IRA contributions).

thresholds that define the treated from comparison groups is the same for all married couples, Figure 1.1 shows that the corresponding thresholds in terms of AGI depend on family size. This is because a couple is entitled to an additional personal exemption for each additional dependent.¹⁰ The assignment of couples into treatment and control groups also depends on the amount of deductions claimed since deductions are subtracted from AGI to determine TI. In Figure 1.1, the standard deduction was assumed for all; but in reality, couples may either claim the standard deduction or choose to itemize their deductions. The lower (upper) bound of AGI used to define comparison group one (two) in the empirical analysis is illustrated by the vertical line at \$55,000 (\$95,000). To be shown, when itemized deductions are taken into account, the \$55k-\$95k range adequately spans the treatment and comparison groups.

Unlike variation in marginal tax rates considered in many tax studies, the EGTRRA differentially affected marginal tax rates at very dense segments of the income distribution. This is important because, as Gruber and Saez (2002) show, the negative behavioral response among taxpayers whose marginal tax rate increased is proportional to the share of taxpayers in that bracket. To illustrate the number couples presumably affected by these provisions, I use the 2001 version of the SOI (Statistics of Income) to plot the distribution of AGI among married, joint filers along with the percent of couples whose predicted federal marginal tax rate was expected to decline from 27% to 15% from 2002 to 2004.¹¹ The 2001 version of the

¹⁰For couples with two children, the TI thresholds that define the treated group (\$46,700 and \$54,194 in 2002 dollars) correspond to AGI levels of \$66,550 and \$74,044; compared to \$60,550 and \$68,044 for couples with no children.

¹¹TI is reported separately from AGI in the SOI, so it is not assumed that all households choose

SOI, constructed from tax returns filed with the IRS, represents the 130.6 million federal tax returns filed in 2001. Sample exclusion restrictions in addition to being married were chosen to mimic the CPS-DER sample examined in the empirical section (see appendix for more details on the SOI and sample selection).¹²

The distribution of AGI among married, joint filers (up to \$200,000 in 2002 dollars, which excludes 5.4% of the weighted sample) and the percent of treated couples within each AGI class are illustrated in Figure 1.2. There are two notable features of this graph. First, the effect of increasing the upper bound of the 15% tax bracket affected a significant number of married, joint filers. In fact, according to calculations from the SOI, approximately 3.2 million (8.2%) of these married couples would have experienced the 44.4% decline in federal marginal tax rates if income had remained constant between 2001 (the tax year of the survey) and 2003 (the year when the provision was fully implemented). And second, the graph for the treatment group is skewed to the right when plotted against AGI. This occurs because many couples with relatively high AGIs also have considerable amounts of personal exemptions and deductions, reducing TI enough to be affected by these marriage penalty relief provisions.

1.2.2 Previous Literature

Previous empirical work suggests that taxation has small effects on hours worked among primary earners but may have relatively larger effects on other margins of the standard deduction.

¹²Sample is selected to include married, joint filers (1) who file a form 1040, (2) whose tax form pertains to the 2001 tax year, (3) who have zero farm income, (4) who either standardize or itemize their deductions, and (4) whose only dependents are own children (residing at home or not).

behavior. In a recent review of the labor supply and taxation literature, Blundell and MaCurdy (1999) conclude that labor hours of married men are rather inelastic to changes in the net-of-tax wage. However, as Feldstein (1995) and others have noted, the focus on labor hours ignores several other margins of behavior that may respond to income taxation. For example, expenditures on certain types of goods may be diverted from income taxation, so taxation may not only affect labor supply but also the allocation of gross income to tax favored and non-favored goods. Other margins of behavioral response, including charitable contributions and forms of compensation, appear to be relatively more responsive to taxation (Slemrod, 1990), so ignoring these margins may lead to misleading calculations of welfare loss (Feldstein, 1999).

In a seminal paper, Feldstein (1995) examined the effect of taxation on taxable income by exploiting variation in tax prices generated by the Tax Reform Act of 1986. He argued that the response of taxable income to taxation reflects other margins of behavioral response in addition to labor hours alone; labor intensity, types of compensation, willingness to report income, and shifts in expenditures between tax favored and non-favored goods. Perhaps the most novel aspect of his analysis was the use of a Treasury Department panel of tax returns filed in 1985 and 1988 which allowed him to follow treatment and comparison groups before and after the tax law change. This identification strategy contrasts with earlier studies which compare static income distributions over time (Lindsey, 1987; Feenberg and Poterba, 1993). Using difference-in-differences estimators, Feldstein's estimates of the elasticity of taxable income to taxation range from 1 to over 3.

Feldstein's study generated considerable interest in the response of income to taxation and gave rise to the "New Tax Responsiveness" literature (Goolsbee, 2000a) - a class of studies that estimate the elasticity of income to taxation using quasi-experimental methods. Similar to Feldstein (1995), most of these studies rely on reforms to the tax code for identification; the Economic Recovery Tax Act of 1981 (Lindsey, 1987; Navratil, 1995), the Tax Relief Act of 1986 (Auten and Carroll, 1999; Moffitt and Wilhelm, 2000), the Omnibus Budget Reconciliation Act of 1993 (Sammartino and Wiener, 1997; Carroll, 1998; Goolsbee, 2000b), and various state and federal tax law changes during the 1980's (Gruber and Saez, 2002). Elasticity estimates from these other studies range from zero to one (see Gruber and Saez, 2002 for a review).

The general identification problem in the new tax responsiveness literature is that even though a reform to the tax code generates variation in marginal tax prices; the actual, observed change in the marginal tax price also reflects changes in income that are unrelated to the reform. Since unrelated changes in income also affect the observed change in the marginal tax price, the endogeneity of the tax price may bias elasticity estimates of income to taxation. Therefore, to identify the structural effect of taxation on income, an instrument is required to isolate the variation in observed changes in marginal tax prices which is plausibly attributable to the reform.

The general strategy is to instrument the observed change in the marginal tax price with the predicted change in the marginal tax price. In most studies, the predicted change comes from computing the marginal tax price after the reform based on a single year of prereform earnings (Auten and Carroll, 1999; Gruber

and Saez, 2002; Saez, 2003; and Looney and Singhal, 2004). The most notable concern with this method, however, is that the instrument, based on a single year of prereform earnings, may still be correlated with systemic movements in income that are unrelated to the reform. In the next section, I discuss this concern in more detail and consider an alternative instrumental variable which plausibly addresses it.

1.3 Empirical Specifications

1.3.1 Estimation Strategy

The empirical objective is to estimate the effect of taxation on earnings using variation in marginal tax prices resulting from the implementation of the EGTRRA's marriage penalty relief provisions. Consistent with a large class of empirical studies¹³, I consider a linear equation which relates changes in earnings to changes in marginal tax prices. In log form, this equation is given by:

$$\log(y_{t+1}/y_t) = \alpha_0 + \alpha_1 \log[d_{t+1}(z_{t+1})/d_t(z_t)] + (\epsilon_{t+1} - \epsilon_t). \quad (1.1)$$

The terms y_τ and $d_t(z_\tau)$ are earnings and the marginal tax price, respectively, in period τ . The marginal tax price is defined by both the tax law in period τ , $d_\tau(\cdot)$, and taxable income, z_τ . While y_τ may be earnings at either the couple or individual level, the marginal tax price is determined by combined income among married, joint

¹³For examples, see Feldstein, 1995; Sammartino and Weiner, 1997; Moffit and Wilhelm, 2000; Saez, 2003; Gruber and Saez, 2002; and Looney and Singhal, 2004.

filers. The parameter of interest, α_1 , is the uncompensated elasticity of earnings to the marginal tax price. Typically, studies consider two to three year windows when calculating changes in earnings and marginal tax prices. In this case, the marriage penalty relief provisions were implemented in 2003, so periods t and $t+1$ correspond to 2002 and 2004, respectively.¹⁴

Quasi-experimental variation in the tax policy variable is derived by pooling treatment and comparison groups into a single sample. In this manner, some of the variation in the observed change in the marginal tax price reflects the differential effects of the reform on the function $d(\cdot)$. However, as mentioned above, this variation in the marginal tax price may also reflect movements in earnings that are unrelated to the reform; so the actual change in marginal tax prices $\log[d_{t+1}(z_{t+1})/d_t(z_t)]$ (henceforth denoted $\Delta_A d$) is likely correlated with unrelated movements in taxable earnings ($\epsilon_{t+1} - \epsilon_t$) (henceforth denoted $\Delta\epsilon$). Therefore, the ordinary least squares estimate of α_1 from equation (1.1) would be biased.¹⁵

To consistently estimate α_1 , an instrumental variable is needed to plausibly isolate the variation in the observed change in marginal tax price that can be attributable to the reform. A valid instrument must be correlated with $\Delta_A d$, uncorrelated with $\Delta\epsilon$, and excludable from equation (1.1). In this study, I use an instrument that addresses particular concerns with the instrument considered in much of the

¹⁴Typically, studies consider two to three year windows when calculating changes in earnings and marginal tax prices. Feldstein's (1995) estimates were based on a three year window: 1985 to 1988. Gruber and Saez (2002) vary the difference window from one to three years and find that their initial estimates are robust to differing window lengths. The administrative data is not available after 2004, so considering 2005 earnings or after is not possible.

¹⁵With a progressive tax schedule, positive shocks to income ($\Delta\epsilon$) would be associated with a decrease of the marginal tax price ($\Delta_A d$), resulting in a downward biased OLS estimate of α_1 .

tax literature: the predicted change in the marginal tax price based on the average of multiple years of prereform taxable incomes.

Intuitively, the average of prereform taxable income isolates the effect of time-invariant factors (at least in the short run) on taxable income. To see this, and to explicitly outline the assumptions required for this identification strategy, consider the following model for taxable income,

$$z_\tau = \beta_{0,\tau} + \beta_1 d_\tau(x_\tau) + \beta_2 x_\tau + \phi_\tau, \quad (1.2)$$

which is the linear form of equation (1.1) with the outcome variable replaced with taxable income.¹⁶ In this model, taxable income is determined by the set of couple characteristics, x_τ , which have a direct effect on taxable income (β_2) as well as an indirect effect through determining the marginal tax price (β_1). Assuming x_τ , $d_\tau(\cdot)$, their effects on taxable income, and the per period fixed effect $\beta_{0,\tau}$ remain constant during the years considered, equation (1.2) can be expressed as,

$$z_\tau = \gamma + \phi_\tau, \quad (1.3)$$

where γ is the effect of time-invariant factors on joint taxable income. Using multiple years of prereform earnings, which strengthens the assumption that $d_\tau(\cdot)$ has not changed, the estimate of γ is taken as $\bar{z}_{t,m} = \text{mean}\{z_t, \tilde{z}_{t-1}, \dots, \tilde{z}_{t-m}\}$. The subscript m indicates the number of lags of prereform taxable income used to estimate the fixed effect, and the tilde above z_{t-m} implies that taxable income in year $t - m$ is

¹⁶This is precisely the model considered in Moffitt and Wilhelm (2000).

indexed to t dollars.

The instrument, which is the predicted change in marginal tax price based on the average of multiple years of prereform taxable incomes, is expressed as $\Delta_P^m d \equiv \log[d_{t+1}(\bar{z}_{t,m})/d_t(\bar{z}_{t,m})]$. Since $\bar{z}_{t,m}$ is interpreted as the fixed component of income prior to the reform, the instrument measures the predicted change in the marginal tax price sans a behavioral response to the reform.

As mentioned, other quasi-experimental tax studies instrument the observed change in the marginal tax price with the predicted change in marginal tax price based on a single year of prereform earnings (i.e. $m = 0$). In this case, the instrument would be $\Delta_P^0 d \equiv \log[d_{t+1}(z_t)/d_t(z_t)]$, which is the predicted change in the marginal tax price if real taxable income in period t had remained constant through period $t + 1$.

The most notable concern with this strategy is that an instrument based on a single year of prereform earnings ($\Delta_P^0 d$) may be correlated with movements in income that are unrelated to the reform ($\Delta\epsilon$). In the case of quasi-experimental tax studies, where treated and comparison groups are defined by distinct segments of the income distribution, the concern is that one income segment may exhibit differential movements in income relative to another even in the absence of a tax reform (e.g. systemic mean reversion or changing income inequality). To illustrate how unrelated movements in income may affect the IV estimate of α_1 in equation (1.1), consider the case where $\Delta\epsilon = \gamma T(z_t) + v$, where $\gamma \neq 0$ and $\text{Cov}(T(z_t), v) = 0$ ($T(\cdot)$ is an indicator function which equals one if treated). Then if $\Delta_P^0 d = \kappa T(z_t)$ (where $\kappa \neq 0$)

along a specified range of prereform income, then $plim(\hat{\alpha}_{1,IV}) = \alpha_1 + \frac{cov(\Delta\epsilon, \Delta_P^0 d)}{cov(\Delta_A d, \Delta_P^0 d)} = \alpha_1 + \frac{\gamma\kappa\bar{T}(1-\bar{T})}{cov(\Delta_A d, \Delta_P^0 d)}$, where $\bar{T} \equiv plim\left(\frac{\sum T(z_t)}{N}\right)$. Thus, the IV estimator is inconsistent - the direction and magnitude of the bias depend on the sign and size of $\gamma\kappa$ and the magnitude increases as the correlation between the instrument $\Delta_P^0 d$ and the endogenous variable $\Delta_A d$ declines.¹⁷

Another concern with this strategy is that observed income in period t is assumed to be equal to income in period $t+1$ in the absence of the reform. Implicitly then, in addition to the assumptions made for the alternative identification strategy described above, this strategy further assumes that $\phi_\tau = 0$ in equation (1.2). But if observed income in any given year is recognized to reflect temporaneous shocks or measurement error, then assignment of couples into treatment and comparison groups may be imprecise.¹⁸ In practice, contaminated treatment and comparison groups implies mismeasurement of $\Delta_P^0 d$; and since the value of $\Delta_P^0 d$ is bounded (i.e. $\Delta_P^0 d$ equals either 0 or κ), the measurement error is non-classical.¹⁹ Non-classical measurement error in $\Delta_P^0 d$ conceivably decreases $cov(\Delta_A d, \Delta_P^0 d)$, magnifying the omitted variables bias in $plim(\hat{\alpha}_{1,IV}) = \alpha_1 + \frac{\gamma\kappa\bar{T}(1-\bar{T})}{cov(\Delta_A d, \Delta_P^0 d)}$.

There are three methods considered in the quasi-experimental tax literature to address the omitted variable bias. First, some studies control for mean rever-

¹⁷It may be of interest to note that \bar{T} is depends on the prespecified range of income that defines the sample of interest. So in this case, the expression for $plim \hat{\alpha}_{1,IV}$ suggests that IV estimates may be particularly sensitive to sample specification.

¹⁸If individuals respond to tax changes associated with their fixed component of taxable income γ , then contemporaneous shocks and measurement error are synonymous.

¹⁹To see this, consider the following model: $\Delta_P^0 d = \Delta_T d + u_P^0$, where $\Delta_T d$ is the true indicator of treatment and u_P^0 is measurement error. Since, u_P^0 equals 0 or κ when $\Delta_T d = 0$ and equals 0 or $-\kappa$ when $\Delta_T d = \kappa$, u_P^0 and $\Delta_P^0 d$ are likely negatively correlated. See Aigner (1973); Freeman (1984); and Black, Berger and Scott (2000) for a discussion on non-classical measurement error.

sion by including base year taxable income, z_t , in equation (1.1) (see Gruber and Saez, 2002; and Looney and Singhal, 2004). However, in quasi-experiments with only one reform, changes in income due to mean reversion may not be separately identified from the response of earnings to the tax reform since both mean reversion and the predicted change in the marginal tax price are modeled as functions of base year taxable income (Moffitt and Wilhelm, 2000).²⁰ A second strategy considered by Moffitt and Wilhelm (2000) is to construct instruments based on specific, time-invariant factors such as education.²¹ If these factors are considered exogenous and time-invariant, at least in the short run, then they are less arguably correlated with the mean reverting trends in earnings and thus serve as the strongest instruments (Moffitt and Wilhelm, 2000). And finally, two studies assign observations into treatment and comparison groups based on average prereform and postreform earnings; Goolsbee (2000b) and Liebman and Saez (2006). But, if there is a behavioral response to taxation, postreform earnings would reflect the effect of the reform. As argued, earnings should be averaged over prereform years only to more convincingly estimate the fixed effect on taxable income.

The motivation for the identification strategy considered here is similar in spirit to the latter two strategies outlined above; estimate a couple's fixed effect on taxable income by averaging several years of prereform earnings. But given the assumptions above, the estimated fixed effect reflects the effect of all short-run, time-invariant factors on taxable income and, in contrast to the strategy considered by Moffitt and

²⁰Moffitt and Wilhelm (2000) also note that the inclusion of base year taxable income changes the interpretation of α_1 .

²¹The consideration of education as exogenous to tax reforms has also been considered in Bludell, Duncan, and Meghir (1998) and Bernheim, Lemke, and Scholz (2004).

Wilhelm (2000), is not limited to a particular factor like education. Additionally, if the model implied by equation (1.3) is correct, then an instrument constructed from average prereform earnings contains less measurement error compared to an instrument based on a single year of earnings.

However, there is an inherent trade-off to increasing the number of lags in prereform taxable income used to estimate the fixed effect $\bar{z}_{t,m}$. On one hand, increasing the number of lags may yield a more precise estimate of the fixed effect on taxable income. On the other hand, factors such as marital status and the number of dependents, and their effects on taxable earnings, may change over longer periods of time. Additionally, this strategy is most credible if there were no major tax law changes which may cause structural shifts in taxable earnings during the years considered in averaging. Notable changes due to the EGTRRA include the increase of the child tax credit amount from \$500 to \$600 (which increased after-tax income by \$100 for each dependent child under age 17) and the introduction of the new 10% tax bracket (which increased after-tax income by \$600 for married, joint filers), both of which were implemented in 2001 (see Table 1.1). In the Results section, I first present elasticity estimates using an instrument constructed from 2002 taxable income only. I then consider up to three annual lags of taxable income (the average of taxable incomes in 2002 through 1999) to construct the instrumental variable. In all specifications, the outcome variable is the change in log taxable earnings between 2002 and 2004.

1.3.2 Data

For the empirical analysis, I employ a data set unique to the taxation literature: the 2002 and 2003 March Supplement of the Current Population Survey (CPS) linked to the Detailed Earnings Record (DER). The DER is an administrative data file compiled from individual W-2s submitted to the Internal Revenue Service and maintained by the Social Security Administration. The DER contains several measures of income annually from 1978 and 2004, including total wage income, taxable wage income, Social Security and Medicare taxable wages, and Social Security and Medicare taxable self-employment income. The disparity between total wage income and taxable wage income occurs when a portion of total wage income is deferred from income taxation into 401(k) accounts.²²

1.3.3 Definition of Variables

In contrast to previous empirical tax studies, I define the outcome variable as taxable earnings (TE) reported in the DER rather than TI. TE is defined as the sum of taxable wage income plus positive amounts of self-employment income which, discussed in more detail below, is a fairly accurate approximation of AGI. Thus, changes in taxable earnings potentially reflect the amount and intensity of work, the type compensation received, the willingness to report income, and changes in income deferred into 401(k) accounts. Other margins of response measured by TI

²²Since 401(k) contributions are subject to payroll taxation, total wage income generally equals Social Security and Medicare taxable wages. These definitions may be different if (1) total wage income exceeds the Social Security tax base or (2) the employee's job is not covered by Social Security.

(i.e. other sources of income, personal exemptions, and deductions) are not reflected in TE.

Although the outcome measure is TE, a measure of TI is still required to determine marginal tax rates. There are two limitations of the CPS-DER matched data regarding the imputation of TI. First, taxable wage income and self-employment income (Schedule C income) are just two of several sources of income subject to taxation. Other sources of income that would otherwise increase TI are not contained in the CPS-DER. Additionally, the DER only reports positive values of self-employment income.²³ This is potentially problematic since self-employment income losses can be claimed to reduce TI.

The second limitation is that the CPS-DER does not indicate whether the couple claims the standard deduction or itemizes their deductions and, in the latter case, the precise deduction amount. One option is to assign the standard deduction to all households. But a significant number of joint filers itemize their deductions, achieving considerably larger deductions compared to the standard deduction amount. Therefore, arbitrary assignment of the standard deduction to all households may severely bias the assignment of couples into treatment and comparison groups.

To examine further the prevalence and amounts of itemized deductions, as well as the performance of the other variable specifications, I examine the 2001 version of the Statistics of Income (SOI) Public Use Tax File. The SOI, compiled from tax returns filed with the Internal Revenue Service, contains 143,221 records

²³The omission of negative self employment income occurs because only self-employment income above \$400 is subject to the self-employment tax (the self-employment tax is the self-employment income equivalent to the Social Security tax levied on wage and salary income).

representing the 130.6 million tax returns filed for tax year 2001. Using the SOI, I can compare actual income variables to income variables computed using only the information that would be available had it been the CPS-DER (e.g. actual AGI versus taxable earnings). Restricting the sample to incomes that adequately span the treated and comparison groups, analysis of the Statistics of Income sample reveals two potential concerns (details are provided in appendix).²⁴ First, among the range of treated and comparison couples, taxable earnings captures 95.1% of actual AGI (taxable earnings is referred to as AGI-DER in appendix). The disparity arises most notably from pension income, IRA disbursements and taxable social security benefits, which cumulatively represent 3.6% of AGI. Thus, the CPS-DER sample used in the empirical analysis are restricted to ages that are less likely to receive these alternative sources of income.²⁵

The second, more acute issue is that approximately 81.0% of couples within this income range itemized their deductions, receiving \$15,262 (in 2002 dollars) in deductions compared to the standard deduction amount of \$7,850 in 2002.²⁶ Therefore, I assign the average standard deduction by state calculated from the Statistics of Income to couples in the CPS-DER by state of residence.²⁷ Generally, when a precise measure of taxable income is not available, many studies neglect itemized

²⁴The SOI sample is restricted to couples whose AGI-DER fall between \$55k and \$95k. I further restrict the sample to joint filers (1) who file a 1040 for tax year 2001, (2) who do not have farm income, (3) whose dependents are exclusively own children (whether living at home or not), (4) who either standardize or itemize their deductions, and (5) whose state of residence is reported.

²⁵The SOI does not contain age, so checking the performance of a sample selection criterion based on age is not possible.

²⁶According to the SOI 2001, approximately two-thirds of all married, joint filers itemize their deductions.

²⁷Sincere mortgage interest payments are tax deductible, home ownership reported in the CPS may be used to improve the imputation of deductions. However, in the income range considered, nearly all couples in the CPS own or are paying a mortgage on their residence.

deductions (Looney and Singhal, 2004; Dokko, (2005). Other studies assign average deductions by adjusted gross income class (for example, Moffitt and Wilhelm, 2000). However, I impute deductions by adjusted gross income class and state of residence given the strong correlation between state of residence and average deductions, which is associated with differing state income tax laws and prevalence of home ownership.²⁸

1.3.4 CPS-DER Sample Selection

Because the marriage penalty relief provisions differentially affected joint filers, the specified population of interest is married couples. I first restrict the sample to single family households where only the married couple and (if applicable) own child dependents reside in the home.²⁹ I also delete couples who report positive farm income in the CPS since farm income may receive special tax treatment. Among the 156,575 interviewed households, 75,333 households remain after these selection criteria are imposed (a complete list of criteria and sample counts are provided in Table 1.2).

Because the DER is the primary source of income data examined in the analysis, the sample is further restricted to couples which both husband and wife are matched to the DER.³⁰ Of the 75,333 remaining households, 6.2% of married cou-

²⁸State income taxes paid and mortgage interest payments are related to itemization since these expenditures may be deducted from taxable income. In the appendix, I present auxiliary analysis on the relationship between the prevalence of deduction itemization by state (estimated from the SOI) with both average state income tax liability and home ownership.

²⁹I assume all children in the household under age 17 are claimed for the child tax credit and all children in the household under age 19 or under age 24 and attending school are claimed as dependents.

³⁰A match is determined by whether the couple reported social security numbers at the time

ples were excluded because just one spouse was linked to administrative data, and another 22.0% were excluded because neither spouse was linked. Thus, 71.8% of couples remain after this selection criterion is imposed.

Sample summary statistics by whether both spouses are linked to administrative data are presented in Table 1.3. In general, matched couples seem to be more educated, younger, employed, and have children. To plausibly preserve the representation of the CPS-DER matched sample to the original CPS sample of married couples, I divide the sample into cells with respect to the husband's education, wife's education, and number of children. The sample weights within each cell among CPS-DER matched couples were then rescaled to sum to the sum of weights within each corresponding cell among the full sample of CPS couples. Assuming that matched and non-matched couples are similar conditional on educational attainment and number of children, this method preserves the representation of the CPS-DER sample relative to the full CPS sample of married couples. The reported results reflect the use of these rescaled weights and are not substantively different than the results using the original sampling weights.

The sample is further restricted to couples whose joint taxable earnings is between \$55k and \$95k in 2002. This range of AGI-DER adequately spans the treated and comparison groups because TI is much less than AGI on average (see

of the CPS interview. The DER file only contains observations of individuals who have at least one W-2 issued between 1978 and 2004. Therefore, it may be the case that a survey respondent provided the necessary information for an administrative match but is not observed in the DER. To identify those who provide information for a match regardless of previous earnings, I merge the CPS to the Summary Earnings Record, a separate data source that contains information such as dates of birth and death. Since a Summary Earnings Record exists for all those with social security numbers regardless of one's earnings history, I define an administrative match according to a match to the Summary Earnings Record.

Treated (%) in appendix Table B.2). To plausibly increase the accuracy of the CPS-DER measure of TI, I restrict the sample to couples where at least one of the members are between the ages of 24 and 50. This restriction not only reduces the likelihood that the couple receives other sources of income but also reduces the potential for retirement behavior to confound the empirical results.

Based on this sample, I calculate marginal tax rates which reflect federal, state, and payroll taxes using NBER's TAXSIM model (see appendix for details on the computation of tax parameters). Because the DER data is confidential, I can not remotely submit the data to the TAXSIM model. Thus, I first calculate marginal tax rates for hypothetical data of married couples that contains all combinations of (1) \$100 increments of joint taxable earnings from \$100 to \$200,000, (2) zero to seven children satisfying the dependent exemption criteria, (3) zero to seven children satisfying the child tax credit criteria, and (4) state of residence. I then merge these tax parameters to the CPS-DER based on these four dimensions. Eight observations were dropped because the number of dependent children exceeded seven.

The next sample selection criterion arises because the outcome variable in equation (1.1) is not defined for individuals with zero TE. I restrict the sample to couples where the husband has non-zero taxable earnings in 2002 and 2004, which is the case for 96.3% (96.7% weighted) of all couples. In this manner, the number of household and husband observations are the same, but the sample is reduced when examining changes in taxable earnings among wives.³¹ From the remaining couples,

³¹There may be responses at the extensive margin due to EGTRRA. However, since labor force participation is theoretically predicated on average tax rates, measuring these extensive-margin effects requires informed imputations of counterfactual average tax rates which is beyond the scope of the study here.

fifty four observations were deleted because their 2004 taxable earnings rounded to \$100 was \$0 or greater than \$200,000.

1.4 Results

1.4.1 Source of Variation and Preliminary Results

Summary statistics for the remaining sample are provided in Table 1.4. I first divide the remaining 12,721 couples into three groups - comparison group one (49.8%), treated (19.6%), and comparison group two (30.6%) - identified by the predicted change in federal marginal tax prices based on 2002 earnings only.

Presented in panel A, the average predicted change in the log marginal tax price is .190 for treated couples which implies that, on average, treated couples would have experienced a 19.0% increase in marginal, after-tax wages had income remained constant from 2002 to 2004. This compares to an average change of -.001 and .039 for couples in comparison groups one and two, respectively. These predicted changes are strongly correlated with realized changes in marginal tax prices, which are also presented in panel A of Table 1.4. Regressing the observed change in the marginal tax price between 2002 and 2004 on the instrument constructed based on 2002 earnings singly yields a first-stage estimate of .893 (.009) (F-statistic: 8501). When average earnings from 2002 through 1999 are used to construct the instrument, the first-stage estimate declines to .262 (.014) (F-statistic: 375), which is expected since the instrument is defined as the predicted change in the marginal tax price between

2002 and 2004 based on average, prereform earnings in both years.³²

In panel B, I report average nominal changes in log taxable earnings between 2002 and 2004. At both the joint and individual levels, the average change in earnings are greater among the treated group relative to both comparison groups. The average increase in log taxable income among husbands in the treated group is .025, compared to .004 and .010 among husbands in comparison groups one and two, respectively. The average increase in log taxable income among wives in comparison group one, the treated group, and comparison group two is .051, .080, and .033, respectively.

One important aspect of this quasi-experiment that requires mention is the differential effects of the reform on couples in comparison group one relative to comparison group two. Although there were no substantial differences in the predicted change in the marginal tax price, indicated in panel A, there is a difference in the predicted change in average tax rates between these two groups. In particular, comparison group two couples would have approximately \$7,500 (\$54,194-\$46,700) more in pre-tax income shifted from the the 27% tax bracket to the 15% bracket, generating a possible income effect among comparison group two couples relative to comparison group one couples. However, the change in marginal tax rates generates a substitution effect among comparison group two couples as well, and whether the income or substitution dominates is theoretically ambiguous.

There are two measures of labor supply to consider when evaluating this in-

³²The sample is restricted from 12,721 observations to 9,914 observations when the instrument is constructed from four years of prereform earnings. Details on this restriction is given in the Results section.

come effect; labor force participation and taxable earnings. A priori, one would predict a relative decline in labor force participation and taxable earnings among comparison group two relative to comparison group one. These predictions are most evident among wives. First, there was a relative decline in wife labor force participation of -1.6% points (t-stat: -2.58) among comparison group two couples relative to comparison group one. And second, there is a difference of -.018 (t-stat: -1.24) in the average change in log taxable earnings among wives in comparison group two couples relative to comparison group one. While these figures suggest that labor force participation and earnings are sensitive to changes in after-tax income among wives, these results may simply reflect reversion to the mean. In the empirical section, I first estimate equation (1.1) ignoring differential changes in after-tax income between the two comparison groups. I then control for systemic differences between the two comparison groups by including an indicator variable signifying comparison group two.³³

In panel F, I provide basic demographic information to assess whether treatment and comparison groups are indeed comparable. First, the average age of both husbands and wives does not vary substantially across these groups. But surprisingly, given the narrow range of incomes considered, educational attainment of both husband and wife and the probability of having a child do differ considerably across these groups. In the specification check section, I estimate alternatives to the base-

³³In theory, one should include a measure of virtual income in the baseline specification to control for differential changes in after-tax income. However, the calculation of virtual income requires longitudinal information on gross wages which, without imposing fairly restrictive assumptions on the data, are not available. Furthermore, changes to virtual income is not a smooth function of changes in after-tax income, so including after-tax income explicitly is not considered.

line specification which controls for these factors. I then consider heterogeneous effects of taxation along these dimensions.

1.4.2 Baseline Results

I first present estimation results using the estimation strategy used in much of the quasi-experimental tax literature: instrument the actual, observed change in the log marginal tax price using an instrument constructed from 2002 income singly. The baseline elasticity estimates, at both the joint and individual levels, are presented in Table 1.5.

The elasticity estimate of joint earnings is most directly comparable to other estimates in the tax literature. This is because, as mentioned, tax studies that use tax return data are limited to estimating the response of income or earnings at the joint level. As indicated in Table 1.5, the magnitude of the estimated elasticity of joint taxable earnings is .063 (.063) and statistically insignificant. This estimate falls among the low end of estimates in this literature and is comparable to the estimated elasticity of joint broad income of .071 (.130) reported in Gruber and Saez (2002).³⁴

Using the same identification strategy, I consider heterogeneous effects of taxation among husbands and wives separately. To estimate the response among husbands, I replace the outcome variable with the earnings of the husband only. For wives, I first condition the sample on wives who have positive earnings in both 2002 and 2004, decreasing the sample from 12,721 to 10,353. I then estimate equation

³⁴The empirical strategy in Gruber and Saez (2002) is similar to the one employed here. The identification comes from various state and federal tax studies from 1979 and 1990 using a panel of tax returns filed over the same period. The estimate cited corresponds to married filers with incomes above \$10k in 1992 dollars where observations are weighted by income.

(1.1) with changes in wife's earnings as the outcome variable. Because of this sample respecification for wives, and since all outcome variables are specified in logs, the joint elasticity estimate is not linear in the individual level elasticity estimates.

As indicated, the resulting elasticity estimates for husbands and wives are fairly comparable: .164 (.112) and .135 (.179) among husbands and wives, respectively. Neither estimate is statistically significant. Comparable, individual elasticity estimates between husbands and wives is consistent with individual elasticity estimates reported in Looney and Singhal (2004): .411 (.464) and .360 (.665) among husbands and wives, respectively. However, the point estimates of Looney and Singhal are larger in magnitude.³⁵

1.4.3 Elasticity Estimates Using Alternative Instrumental Variables

I next consider the same estimation equation (1.1) but instead calculate the predicted change in the marginal tax price based on average earnings from 2002 back to 1999. As argued, instruments based on average prereform earnings serve to attenuate the plausible bias that may arise when the instrument is based on 2002 earnings singly.

A series of joint earnings elasticity estimates are presented in Table 1.6. The baseline estimate is located in the first row and first specification column. Each other

³⁵A notable difference between their quasi-experiment and the one considered here is that the marginal tax rate changes they consider are anticipated. Thus, the theoretical response of taxable earnings may be larger in their case since the response is not confounded by lifetime wealth effects. MaCurdy (1981) discusses this point in detail.

cell represents an estimate from a single regression and corresponds to changes to the empirical specification along two dimensions. The first dimension, which varies by row, is the number of lag years used to construct the instrumental variable. The number of years of prereform earnings used is indicated in the first column: the expression $\Delta_P^m d$ implies that the instrument is calculated using m lags of annual prereform earnings. Thus, the entire first row corresponds to a series of estimates when the instrumental variable is constructed from 2002 earnings only.

The second dimension, which varies by column, is different sample specifications. When using the alternative instruments, which are based on average earnings, I drop observations whose average earnings also fall outside the earnings range of interest; \$55k through \$95k. Arguably, if the average earnings falls outside of this range, the couple is less likely to be treated or, if previously in a comparison group, less comparable to treated couples. Sample specification (2) consists of all couples in specification (1) less couples whose average earnings in 2002 and 2001 is less than \$55k or greater than \$95k. In specifications (3) and (4), the sample is further restricted to couples whose average earnings from 2002 to 2000 (and average earnings from 2002 to 1999 for specification (4)) also lies within the income range.

When using the alternative instruments, I first consider the cases where the instrumental variable and the sample specifications align. Estimates in these cases are located along the the diagonal in Table 1.6. When average earnings in 2002 and 2001 are used to estimate the predicted effect of the reform on marginal tax prices, the joint taxable earnings elasticity increases from .063 (.063) to .140 (.087). And when the instrument is constructed from average earnings in 2002 through 2000 and

2002 through 1999, the estimate increases to .164 (.139) and .206 (.177), respectively. Thus, the point estimate appears to increase monotonically as the number of lags used in the computing the instrumental variable increases and, in the final specification, is over three times the estimate when a single year of prereform earnings are used. However, all estimates remain rather imprecise and not statistically significant.

To suggest that the new elasticity estimates in row (2), specification (2); row (3), specification (3); and row (4), specification (4) are not driven by sample selection bias, I reestimate the elasticity using the baseline instruments among the new sample specifications (2) through (4). These estimates are presented in the off diagonal of Table 1.6. Generally, the elasticity estimates do not vary considerably across the different sample specifications when the instrumental variable specification is held fixed.

I next estimate the elasticity of taxable earnings for husbands and wives separately. Again, these estimates are generally not estimable using administrative tax return data since most married couples file, and therefore report income, jointly. Elasticity estimates for husbands and wives are presented in panels A and B in Table 1.7, respectively. For husbands, constructing the instrument using average earnings in 2002 and 2001 rather than 2002 earnings singly increases the point estimate from .164 (.112) to .421 (.175) and becomes significant at the 5% level of confidence (row (1), specification (1) compared to row (2), specification(2)). The estimate using 2002 through 2000 earnings further increases the estimate to .618 (.248), but averaging back to 1999 increases the estimate slightly to .664 (.310). As

with the joint earnings elasticities, these elasticity estimates appear robust to the different sample specifications when the instrumental variable specification remains fixed.

In contrast to husbands, there is no discernable pattern in elasticity estimates among married wives when the instrument is constructed from multiple years of prereform earnings relative to using a single year of earnings. In Table B of Table (1.7), the estimated elasticities using one, two, three, and four year averages are .135 (.179), -.205 (.213), .208 (.286), and .211 (.443), respectively. Although the subsequent point estimates are considerable compared to the baseline estimate of .135, all estimates are highly imprecise.

1.4.4 Specification Checks

I next consider a set of specification checks relevant to the empirical analysis thus far. These checks and the corresponding elasticity estimates are presented in Table 1.8. Because the previous results indicate that much of the behavioral response of earnings among married couples is driven by husbands, the subsequent analysis considers earnings of married men only. Unless otherwise noted, sample specification (4) from the previous analysis is the population of interest.³⁶

I first consider whether the baseline elasticity estimates are robust to the inclusion of additional controls. The set of observable characteristics include the educational attainment of both the husband and wife, labor force participation of

³⁶Similar to the baseline results, estimates from the specification checks are robust to the different sample specifications; so the estimates from these other sample specifications are suppressed for brevity.

the wife, number of dependent children, and state fixed effects. As indicated in Table 1.8, estimates after the inclusion of these additional variables are very similar to the baseline estimates.

I next consider including an indicator variable signifying control group two. As mentioned, the predicted change in after-tax income for comparison group two is somewhat larger relative to comparison group one and the treatment group. Thus, the inclusion of a comparison group fixed effect serves to control for this systemic difference among comparison group two couples. As indicated in Table 1.8, the inclusion of this control variable has little effect on the baseline estimates.

Another deviation from the baseline specifications is to use the alternative instruments on the full sample without dropping observations based on average earnings. The estimates among the unadulterated sample are presented in row four of Table 1.8. Although the elasticity estimates among the constant sample are generally smaller than the baseline estimates, the elasticity estimates do increase monotonically and become statistically significant as additional years of lagged earnings are used to construct the instrument variable.

The final specification check arises because the change in the marginal tax price may reflect a change in the dependent status of a child. Since this source of change to tax prices is largely anticipated, so that the response is not confounded by a lifetime wealth effect, the baseline elasticity estimates may be biased upward.³⁷

The distinction between anticipated and unanticipated changes in marginal tax rates

³⁷See MaCurdy (1981) for a discussion on the labor supply response to either anticipated or unanticipated changes to wages.

has been the focus of recent tax studies (Looney and Singhal, 2004; Dokko, 2005).

To suggest that the elasticity estimates largely reflect the effect of the reform and not a result of anticipated changes in dependency status of children, I restrict the sample to the 9,381 couples whose number of eligible dependents remains constant from 2002 and 2004. These estimates are presented in the last row of Table 1.8. As suggest by theory, the elasticity estimates are somewhat smaller than when the sample is restricted to couples whose predicted change in marginal tax rates do not reflect a change in dependency status. But the estimates are not considerably different from the baseline elasticity estimates among the full sample of married men.

1.4.5 Heterogeneous Effects

Using the detailed demographic data contained in CPS, the sample of married men can be split across certain demographic dimensions to estimates heterogeneous responses of earnings to taxation. Estimated elasticities among subgroups of married men, along with the baseline estimates among all married men, are presented in Table 1.9.

According to Table 1.4, educational attainment of husbands differs considerably across the treatment and comparison groups; however, there is also considerable variation in educational attainment within groups. Thus, I first consider whether estimated elasticities for married men with some college experience or beyond are substantively different than men with no college experience. According to Table 1.9,

the estimate elasticity among uneducated versus educated married men are similar when the instrument is constructed from 2002 earnings only: .175 (.196) and .216 (.111) among the uneducated and educated, respectively. However, constructing the instrument based on average earnings increases the estimated elasticities considerably among married men with some college relative to married men with no college. In particular, by using average earnings from 2002 through 1999, the estimated elasticity increases to .657 (.338) compared to the baseline estimate of .138. The elasticity estimates among married men with no college experience, on the other hand, increases to just .468 (.695).

Also in Table 1.4, the percent married couples with a child in the household also varies across the treatment and comparison groups. However, nearly three fourths of the total sample (approximately 85% of the unweighted sample) has a child; so splitting the sample based on the presence of a child would yield a small sample of households without children. Nevertheless, I consider estimating the response of husbands' earnings separately for those who have a child residing in the household and those who do not. These estimates are presented in the last two rows of Table 1.9. Using the alternative instruments (i.e. column (4)), the results suggest that the elasticity among households without a child is nearly twice as large as that of households with at least one child: 1.22 (1.11) and .581 (.304) among the former and latter, respectively.

1.5 Discussion and Conclusion

The empirical objective of this study is to estimate the elasticity of taxable earnings to taxation. There are a few substantive contributions of this paper to the existing “New Tax Responsiveness” literature. First, to my knowledge, no other tax study has considered identifying the effect of taxation from the implementation of the Economic Growth and Tax Relief Reconciliation Act of 2001. Additionally, the data used in the analysis - the March CPS linked to the Detailed Earnings Record - is unique to the taxation literature.

Second, the identification strategy considered in this study addresses a key concern for the identification strategy considered in much of the taxation literature. In particular, I construct instrumental variables that are the predicted change in marginal tax prices based on the average of multiple years of pre-reform earnings. Authors of previous tax studies construct instruments using only one year of pre-reform earnings to construct instruments. The concern with this instrument is that systemic mean reversion to or measurement error in earnings may generate biased elasticity estimates of earnings to taxation. These concerns are arguably addressed by constructing instruments based on the average of multiple years of pre-reform earnings, which essentially serves as an estimate of a couple’s fixed effect.

The use of these alternative instruments appear to considerably increase the estimated elasticities of taxable earnings at the joint and husband earnings levels compared to elasticity estimate derived from instruments based on a single year of pre-reform earnings. The estimated elasticities among married women using these

alternative instruments, however, appear mixed.

And finally, the response of earnings to taxation may be examined at the individual level among married couples and across different demographic dimensions. Generally, tax studies conducted using administrative tax return data are limited in both regards. The results suggest that the elasticity of earnings to taxation are greater among married men than women; changes in earnings at the joint level may mask important behavioral variation in taxable earnings at the individual level. Furthermore, the response of taxable earnings appear larger among educated, married men. This suggests that married, educated men may simply be more sensitive to changes in marginal tax prices or that they have more discretion in the determining taxable earnings through 401(k) contributions or types and amounts of compensation. Examining these particular margins of behavioral response is a promising direction for future work.

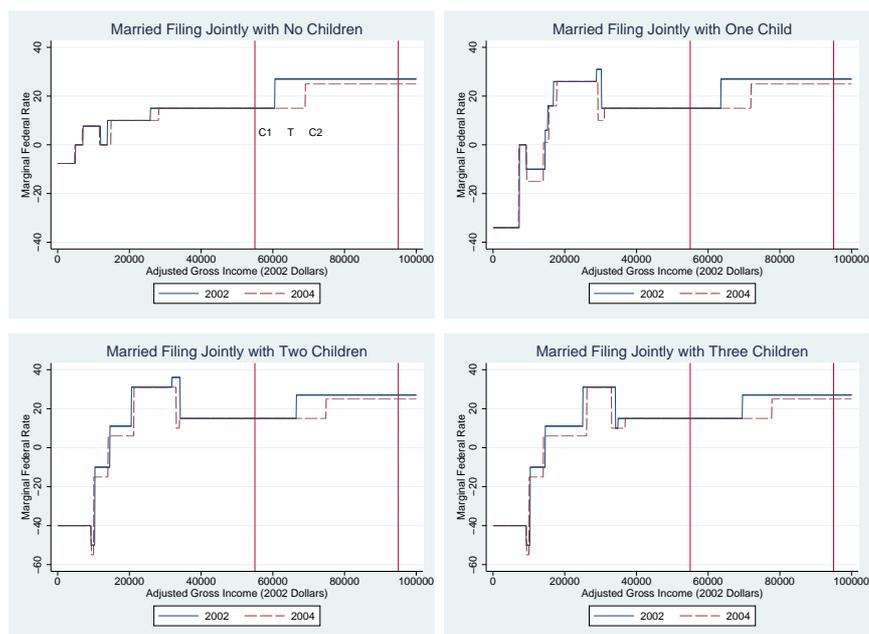


Figure 1.1: Federal marginal tax rate by adjusted gross income (2002 dollars) for married couples in tax years 2002 and 2004. The standard deduction was assigned for all households and children were assumed to satisfy the eligibility criteria for the personal exemption and the child tax credit. These rates were simulated using NBER's TAXSIM model.

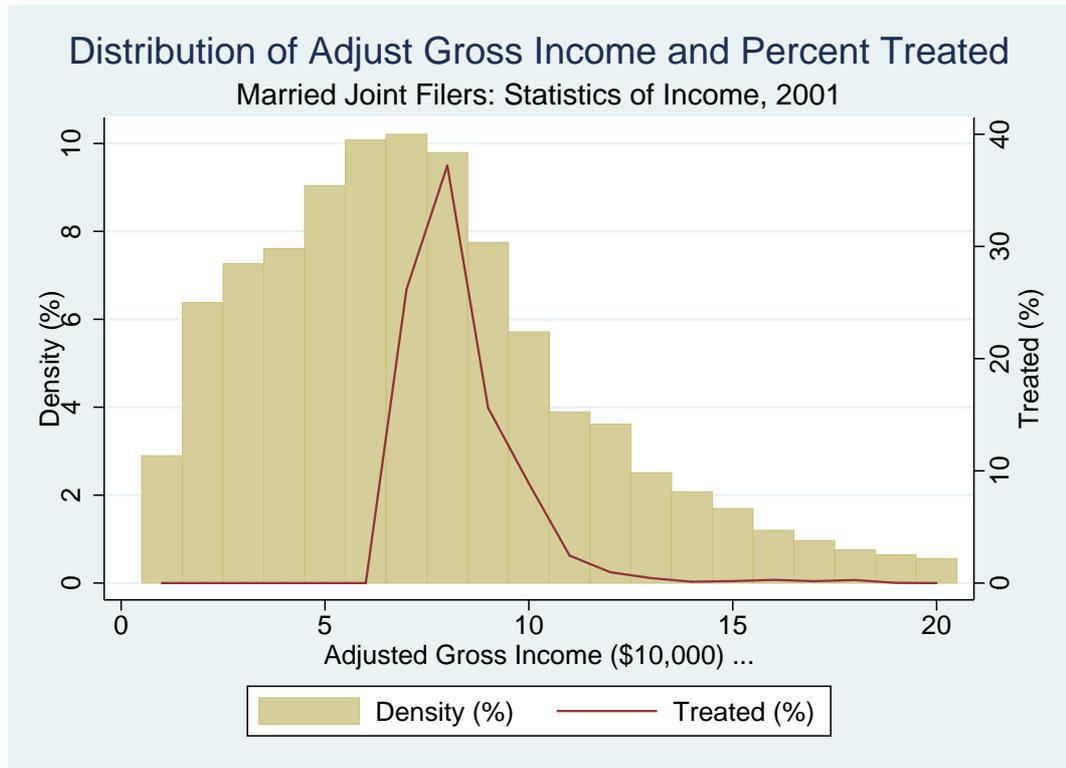


Figure 1.2: Distribution of Adjusted Gross Income for married couples and percent of couples whose taxable income was between \$47,900 and \$55,394 (in 2002 dollars). This taxable income range defines the couples whose predicted federal marginal tax rate would decline from 27% to 15% from 2002 and 2004 due to the marriage penalty relief provisions (increase in the standard deduction and 15% tax bracket for married, joint filers). Couples with adjusted gross incomes above \$200,000 are not shown which represent 5.4% of the weighted sample. Data for this figure come from the 2001 version of the Statistics of Income. Refer to appendix for a description of the SOI sample used to construct this figure.

Table 1.1: Tax Parameters for Married Joint Filers: Tax Years 2000 to 2005

Tax Year	2000	2001	2002	2003	2004	2005
A. EGTRRA (2001)						
Deduction	\$7,350	\$7,600	\$7,850	\$7,849	\$8,100	\$8,350
Personal Exemption	\$2,800	\$2,900	\$3,000	\$3,050	\$3,100	\$3,200
Marginal Tax Rates						
10%	-	-	\$0-\$12,000	\$0-\$12,000	\$0-\$12,000	\$0-\$12,000
15%	\$0-\$43,850	\$0-\$45,200	46,700	47,400	48,500	49,600
28%	105,950	109,250 (27.5%)	112,850 (27%)	114,650 (27%)	117,250 (26%)	119,950 (26%)
31%	161,450	166,500 (30.5%)	171,950 (30%)	174,700 (30%)	178,600 (29%)	182,800 (29%)
36%	288,350	297,350 (35.5%)	307,050 (34%)	311,950 (34%)	319,000 (33%)	326,450 (33%)
39.6%	inf.	inf. (39.1%)	inf. (38.6%)	inf. (38.6%)	inf. (37.6%)	inf. (37.6%)
Child Tax Credit						
Credit Amount (NI)	\$500	\$600	\$600	\$600	\$600	\$700
Refundable	No	Yes	Yes	Yes	Yes	Yes
Refund Rate	-	10%	10%	10%	10%	15%
Threshold	-	\$10,000	\$10,350	\$10,500	\$10,750	\$11,000
B. EGTRRA (2001), JGTRRA (2003) and WFTRA (2004)						
Deduction	\$7,350	\$7,600	\$7,850	\$9,500	\$9,700	\$10,000
Personal Exemption	\$2,800	\$2,900	\$3,000	\$3,050	\$3,100	\$3,200
Marginal Tax Rates						
10%	-	-	\$0-\$12,000	\$0-\$14,000	\$0-\$14,300	\$0-\$14,000
15%	\$0-\$43,850	\$0-\$45,200	46,700	56,800	58,100	59,000
28%	105,950	109,250 (27.5%)	112,850 (27%)	114,650 (25%)	117,250 (25%)	119,950 (25%)
31%	161,450	166,500 (30.5%)	171,950 (30%)	174,700 (28%)	178,600 (28%)	182,800 (28%)
36%	288,350	297,350 (35.5%)	307,050 (35%)	311,950 (33%)	319,000 (33%)	326,450 (33%)
39.6%	inf.	inf. (39.1%)	inf. (38.6%)	inf. (35%)	inf. (35%)	inf. (35%)
Child Tax Credit						
Credit Amount (NI)	\$500	\$600	\$600	\$1,000	\$1,000	\$1,000
Refundable	No	Yes	Yes	Yes	Yes	Yes
Refund Rate	-	10%	10%	10%	15%	15%
Threshold	-	\$10,000	\$10,350	\$10,500	\$10,750	\$11,000

Author's compilation of US federal tax codes during years 2000 to 2005. Panel A reflects tax code changes based on EGTRRA only, and Panel B reflects tax codes changes based on EGTRRA, JGTRRA, and WFTRA. The new 10% tax bracket was implemented in tax year 2001 but is not reflected in the tax code until 2002.

Table 1.2: Sample Selection: Married Couples

CPS-DER Match Data: 2003			
Survey Year	2002	2003	Total
Primary Family Husband/Wife*	40,839	40,603	81,442
Farm Income, Delete	37,737	37,596	75,333
Couple DER Match	27,663	26,389	54,052
Joint Taxable Earnings: \$55k-\$95k	8,089	7,623	15,712
Age: 25-49	6,810	6,440	13,250
Dependent Children > 7	6,807	6,435	13,242
Husband LFP: 2002 and 2004	6,574	6,201	12,775
$\$0 = \text{Round}(\text{Actual 2004 Income}) > \200k	6,548	6,173	12,721
Final Sample	6,548	6,173	12,721

There were 156,575 households interviewed in the 2002 and 2003 March Supplement of the Current Population Survey. * includes single family households headed by husband and wife, excluding armed service members.

Table 1.3: Summary Statistics on CPS-DER Match among Married Couples: 2003

	Husband			Wife		
	No Match	Match	t-stat	No Match	Match	t-stat
Education (%)						
Less than HS	15.2	12.2	7.7	13.3	9.5	10.9
HS	30.8	28.4	4.8	34.8	31.9	5.5
Some College/Associate	22.8	25.2	-5.0	24.8	27.9	-6.4
College and Beyond	31.2	34.1	-5.6	27.1	30.6	-6.8
Age Distribution (%)						
<25	1.6	1.7	-0.6	3.2	3.3	-0.5
25-34	15.6	15.7	-0.2	18.0	19.1	-2.5
35-44	22.2	25.1	-6.2	23.4	26.1	-5.5
45-54	21.6	24.5	-6.3	21.8	23.9	-4.6
55-64	18.4	16.1	5.7	16.9	14.5	6.1
>65	20.7	16.9	8.7	16.7	13.1	9.2
White (%)	87.9	87.8	0.3	87.4	87.4	0.0
Labor Force Status (%)						
Working	71.1	75.0	-7.8	55.5	61.9	-11.6
Unemployed	3.1	3.3	-1.2	1.8	2.4	-3.4
Not in Labor Force	25.8	21.7	8.7	42.6	35.7	12.8
Earnings by Labor Status (\$)						
Working	55,248	58,543	-3.6	31,248	30,255	1.7
Unemployed	30,219	31,392	-0.3	21,869	12,641	1.4
Not in Labor Force	3,817	3,826	0.0	1,427	1,508	-0.5
Children (%)						
0	52.9	42.4	18.9			
1	18.4	20.6	-4.8			
2	17.8	24.0	-13.2			
>3	10.8	13.0	-5.9			

Figures were estimated from the remaining CPS-DER sample up to and including the sample restriction regarding positive farm income (see Table 1.2). Thus, statistics are calculated based on 75,333 couples; 54,052 of which provided the necessary information required to link both husband and wife to administrative data.

Table 1.4: Summary Statistics

Group	Comparison 1	Treated	Comparison 2	Total
Observations (unweighted)	6,468	2,501	3,752	12,721
Distribution (%)	49.8	19.6	30.6	100.0
A. Avg. change in marginal tax prices (2002 to 2004):				
Predicted	-0.001 (0.004)	0.190 (0.017)	0.039 (0.043)	0.049 (0.076)
Actual	0.000 (0.065)	0.179 (0.073)	0.058 (0.114)	0.053 (0.108)
B. Avg. taxable earnings (2002) (\$):				
Household	63,460 (5505)	74,915 (4486)	85,989 (5316)	72,604 (11189)
Husband	43,798 (14978)	50,162 (17040)	57,197 (18683)	49,148 (17598)
Wife*	23,155 (12663)	28,283 (13900)	32,416 (15828)	27,079 (14556)
C. Avg. change in log taxable earnings (2002 to 2004):				
Household	0.030 (0.345)	0.036 (0.292)	0.022 (0.302)	0.029 (0.322)
Husband	0.004 (0.571)	0.025 (0.499)	0.010 (0.476)	0.010 (0.530)
Wife*	0.051 (0.785)	0.080 (0.679)	0.033 (0.656)	0.051 (0.726)
D. Labor force participation of wife (%):				
2002	84.9 (35.8)	87.5 (33.1)	88.8 (31.5)	86.6 (34.0)
2004	83.5 (37.1)	85.6 (35.1)	85.8 (34.9)	84.6 (36.1)
2002 and 2004	79.2 (40.6)	82.3 (38.1)	83.0 (37.5)	81.0 (39.2)
E. 401(k) contributions:				
Contributors (%)	62.9 (48.3)	69.1 (46.2)	75.9 (42.8)	68.1 (46.6)
Amount Contributing* (\$)	3,040 (2540)	4,044 (3142)	4,834 (3579)	3,853 (3153)
F. Demographic characteristics:				
Some college, husband (%)	62.8 (48.3)	67.2 (47.0)	73.6 (44.1)	66.9 (47.0)
Some college, wife (%)	63.9 (48.0)	71.0 (45.4)	75.4 (43.1)	68.8 (46.3)
Age, husband	39.7 (7.5)	40.5 (7.7)	40.9 (7.7)	40.2 (7.7)
Age, wife	37.6 (7.2)	38.3 (7.1)	38.9 (7.4)	38.1 (7.2)
Children (%)	81.1 (39.1)	70.8 (45.5)	66.9 (47.1)	74.8 (43.4)

Statistics calculated among the 12,721 couples retained for the empirical analysis. Unless otherwise noted, all figures are weighted. Couples are assigned into groups by their predicted change in federal marginal tax prices between 2002 and 2004 based on 2002 earnings only. * Average earnings and contributions are conditional on working and contributing, respectively.

Table 1.5: Taxable Earnings Elasticities by Earnings Level

IV Estimates: Instrument based on 2002 earnings only

Earnings Level	Joint	Husband	Wife
$\Delta_A d$	0.063 [0.063]	0.164 [0.112]	0.135 [0.179]
Constant	0.026 [0.008]**	0.001 [0.009]	0.044 [0.013]**
Observations	12721	12721	10353

Estimation equation: $\Delta y = \alpha_0 + \alpha_1 \Delta_A d + \Delta \epsilon$. Δy is the log change in taxable earnings and $\Delta_A d$ is the change in log marginal tax price, both between 2002 and 2004. Robust standard errors are clustered by state. * and ** imply significance at the 5% and 1% levels, respectively.

Table 1.6: Taxable Earnings Elasticities - Joint Earnings

IV Estimates: Alternative Instrumental Variables

Sample Specification	(1)	(2)	(3)	(4)
Instrument for $\Delta_A d$:				
$\Delta_P^0 d$	0.063 [0.063]	0.063 [0.069]	0.051 [0.070]	0.064 [0.077]
$\Delta_P^1 d$		0.14 [0.087]	0.106 [0.092]	0.12 [0.101]
$\Delta_P^2 d$			0.164 [0.139]	0.163 [0.148]
$\Delta_P^3 d$				0.206 [0.177]
Observations	12721	11386	10534	9914

Estimation equation: $\Delta y = \alpha_0 + \alpha_1 \Delta_A d + \Delta \epsilon$. Δy is the log change in taxable earnings and $\Delta_A d$ is the change in log marginal tax price, both between 2002 and 2004. The latter is instrumented with $\Delta_P^m d$ - the predicted change based averaged real earnings over m lags. Observations decline across columns because real average earnings falls outside the earnings range of interest; \$55k to \$95k. Robust standard errors are clustered by state. * and ** imply significance at the 5% and 1% levels, respectively.

Table 1.7: Taxable Earnings Elasticities - Individual Earnings

IV Estimates: Alternative Instrumental Variables				
Sample Specification	(1)	(2)	(3)	(4)
Instrument for $\Delta_A d$:				
A. Husbands				
$\Delta_P^0 d$	0.164 [0.112]	0.206 [0.123]	0.177 [0.112]	0.204 [0.114]
$\Delta_P^1 d$		0.421 [0.175]*	0.361 [0.187]	0.427 [0.219]
$\Delta_P^2 d$			0.618 [0.248]*	0.646 [0.274]*
$\Delta_P^3 d$				0.664 [0.310]*
Observations	12721	11386	10534	9914
B. Wives				
$\Delta_P^0 d$	0.135 [0.179]	0.094 [0.167]	0.165 [0.157]	0.081 [0.141]
$\Delta_P^1 d$		-0.205 [0.213]	-0.165 [0.202]	-0.226 [0.193]
$\Delta_P^2 d$			0.208 [0.286]	0.147 [0.289]
$\Delta_P^3 d$				0.211 [0.443]
Observations	10353	9309	8645	8163

Estimation equation: $\Delta y = \alpha_0 + \alpha_1 \Delta_A d + \Delta \epsilon$. Δy is the log change in taxable earnings and $\Delta_A d$ is the change in log marginal tax price, both between 2002 and 2004. The latter is instrumented with $\Delta_P^m d$ - the predicted change based averaged real earnings over m lags. Observations decline across columns because real average earnings falls outside the earnings range of interest; \$55k to \$95k. Robust standard errors are clustered by state. * and ** imply significance at the 5% and 1% levels, respectively.

Table 1.8: Taxable Earnings Elasticities -Husband Earnings

Specification Checks				
Instrumental Variable:	$\Delta_P^0 d$	$\Delta_P^1 d$	$\Delta_P^2 d$	$\Delta_P^3 d$
Specification:				
Baseline Estimates (N=9914)	0.204 [0.114]	0.427 [0.219]	0.646 [0.274]*	0.664 [0.310]*
Inclusion of Covariates (N=9914)	0.192 [0.114]	0.378 [0.216]	0.615 [0.291]*	0.632 [0.311]*
Group 2 Control (N=9914)	0.202 [0.114]	0.424 [0.219]	0.65 [0.274]*	0.685 [0.309]*
Non-Restricted Sample (N=12721)	0.164 [0.112]	0.364 [0.148]*	0.446 [0.222]*	0.444 [0.322]
Change in Dependency Status (N=9381)	0.185 [0.117]	0.411 [0.221]	0.550 [0.270]*	0.602 [0.296]*

Estimation equation: $\Delta y = \alpha_0 + \alpha_1 \Delta_A d + \Delta \epsilon$. Δy is the log change in taxable earnings and $\Delta_A d$ is the change in log marginal tax price, both between 2002 and 2004. The latter is instrumented with $\Delta_P^m d$ - the predicted change based averaged real earnings over m lags. Observations decline across columns because real average earnings falls outside the earnings range of interest; \$55k to \$95k. Robust standard errors are clustered by state. * and ** imply significance at the 5% and 1% levels, respectively.

Table 1.9: Taxable Earnings Elasticities -Husband Earnings

Heterogeneous Effects				
Instrumental Variable:	$\Delta_P^0 d$	$\Delta_P^1 d$	$\Delta_P^2 d$	$\Delta_P^3 d$
Specification:				
Baseline Estimates (N=9914)	0.204 [0.114]	0.427 [0.219]	0.646 [0.274]*	0.664 [0.310]*
No College Experience (N=3167)	0.175 [0.196]	0.183 [0.420]	0.475 [0.499]	0.493 [0.557]
College Experience (N=6747)	0.216 [0.111]	0.554 [0.217]*	0.723 [0.263]**	0.697 [0.373]
No Child in Household (N=1556)	0.804 [0.482]	0.814 [0.563]	1.327 [0.905]	1.221 [1.107]
Child in Household (N=8358)	0.041 [0.111]	0.351 [0.184]	0.533 [0.270]	0.581 [0.304]

Estimation equation: $\Delta y = \alpha_0 + \alpha_1 \Delta_A d + \Delta \epsilon$. Δy is the log change in taxable earnings and $\Delta_A d$ is the change in log marginal tax price, both between 2002 and 2004. The latter is instrumented with $\Delta_P^m d$ - the predicted change based averaged real earnings over m lags. Observations decline across columns because real average earnings falls outside the earnings range of interest; \$55k to \$95k. Robust standard errors are clustered by state. * and ** imply significance at the 5% and 1% levels, respectively.

Chapter 2

Aching to Retire? The Rise in the Full Retirement Age and its Impact on the Disability Rolls (with Mark Duggan and Jae Song)

Abstract: The Social Security Amendments of 1983 reduced the generosity of Social Security retired worker benefits in the U.S. by increasing the program's full retirement age from 65 to 67 and increasing the penalty for claiming benefits at the early retirement age of 62. These changes were phased in gradually, so that individuals born in or before 1937 were unaffected and those born in 1960 or later were fully affected. No corresponding changes were made to the program's disabled worker benefits, and thus the relative generosity of Social Security Disability Insurance (SSDI) benefits increased. In this paper, we investigate the effect of the Amendments on SSDI enrollment by exploiting variation across birth cohorts in the

policy-induced reduction in the present value of retired worker benefits. Our findings indicate that the Amendments significantly increased SSDI enrollment since 1983, with an additional 0.6 percent of men and 0.9 percent of women between the ages of 45 and 64 receiving SSDI benefits in 2005 as a result of the changes. Our results further indicate that these effects will continue to increase during the next two decades, as those fully exposed to the reduction in retirement benefit generosity reach their fifties and early sixties.

2.1 Introduction

During the last two decades, the fraction of adults in the U.S. receiving benefits from the Social Security Disability Insurance (SSDI) program has steadily increased. For example, among individuals between the ages of 45 and 64, the rate of SSDI enrollment rose from 4.5 percent in 1983 to 6.7 percent by 2005. A similarly striking increase occurred for younger workers, with the fraction of 25 to 44-year old workers on SSDI rising from 0.7 to 1.6 percent during this same period. A number of explanations have been advanced for the growth in SSDI enrollment, including a 1984 policy change that liberalized the program's medical eligibility criteria, the aging of the "baby boom" population, and an increase in female labor force attachment that resulted in more women being insured for SSDI (Autor and Duggan, 2006).

In this paper, we investigate whether and to what extent a policy-induced reduction in the generosity of Social Security retired worker benefits also contributed to the increase in SSDI enrollment. The Social Security Amendments of 1983, which

were signed into law on April 20th of that year, increased the age at which individuals were eligible for their full retirement benefits from 65 to 67 while simultaneously increasing the penalty for claiming benefits at the early retirement age of 62. Combined with an increase in the payroll tax rate and several other provisions,¹ the main goal of this legislation was to improve the short and long-term fiscal health of Social Security, also known as the Old Age, Survivors, and Disability Insurance (OASDI) program.

As Figure 2.1 demonstrates, the increase in the full retirement age and in the early retirement penalty were phased in gradually and occurred in two main stages. Individuals born in 1937 or earlier were unaffected by the change, their counterparts born between 1938 and 1959 were partially affected, and those born in 1960 or later were fully affected. The first half of the policy change was phased in from the 1938 to 1943 birth cohorts while the second was phased in from the 1955 to 1960 birth cohorts. In each year of the phase-in, the full retirement age was increased in two-month increments and the fraction of full benefits that individuals could receive at the early retirement age of 62 fell by 0.833 percentage points. This latter change implied that the penalty for claiming at 62 increased from 20 to 30 percent of full benefits.

While the generosity of retired worker benefits declined as a result of the Amendments, no corresponding changes were made to SSDI benefits. Theoretically, it is plausible that some individuals who would have otherwise claimed retired worker

¹For a full description of the 1983 Social Security Amendments, see <http://www.ssa.gov/history/1983amend.html>.

benefits responded to this change by applying for and perhaps ultimately receiving SSDI benefits. All else equal, one would expect the incentive to do this to be significantly greater for those born in more recent years and for those closer to the age of claiming retired worker benefits. This is because year-of-birth and age are the key determinants of the decline in the value of retired worker benefits induced by the policy.² As Figure 2.2 demonstrates, the decline at each age was three times larger for those born in 1943 than for their counterparts born in 1939. Similarly, the decline was almost twice as large in dollar terms for 62-year olds as it was for 45-year olds (\$8900 versus \$4700 for a man born in 1943).

To estimate the effect of the policy on SSDI enrollment, we exploit variation by cohort³ and by age in the change in the present value of retired worker benefits. More specifically, we estimate first difference models that control for age-specific trends in SSDI enrollment and for common changes in each year in SSDI enrollment. Our key explanatory variable is the change in the present value of retired worker benefits from one year to the next at a certain age. We focus primarily on estimating the effect of the first half of the policy described above, which was phased in from the 1938 to the 1943 birth cohorts, as those affected by the second half were relatively young and thus less likely to be affected in our most recent year of data.

We utilize aggregate data on SSDI enrollment rates by age, gender, and year-of-birth in each year from 1983 to 2005 and restrict attention to individuals between

²It is worth noting that the present value of retired worker benefits for the average individual has been steadily increasing over time, both because of the increase in life expectancy and because of the indexation of social security benefits to average wages. But we only consider the change in the present value that is caused by the policy, essentially assuming that these other changes are relatively smooth over time.

³Here and elsewhere in the paper we use the term cohort to refer to a specific year-of-birth.

the ages of 45 and 64. We essentially test whether SSDI enrollment increases more rapidly at each age when the present value of retired worker benefits at that age is declining. If the Amendments did affect SSDI enrollment through the mechanism described above, then one would expect SSDI enrollment rates at a certain age to increase more rapidly when cohorts born between 1938 and 1943 reach that age.

Using data for the 1934 through 1946 cohorts, our estimates strongly suggest that the decline in the generosity of Social Security retired worker benefits led to a significant increase in SSDI enrollment among both women and men. For each \$5,000 decline in the average present value of OA benefits, we estimate that SSDI enrollment increased by 0.6 percentage points, with the estimated effects approximately twice as large for women as for men (0.8 versus 0.4 percentage points). We obtain similar estimates if we restrict attention to those between the ages of 45 and 54 or between the ages of 55 and 64, though because the change in the present value increases with age, the effect on SSDI enrollment does as well. We also obtain similar estimates when we expand our analysis sample to consider individuals born between 1920 and 1960, with these estimates incorporating the effect of the second phase of the policy change described above.

In the final section of our paper, we calculate how much of the increase in SSDI enrollment from 1983 to 2005 can be explained by the reduction in the generosity of retired worker benefits. Our point estimates suggest that SSDI enrollment was 0.58 percentage points higher among men between the ages of 45 and 64 and 0.89 percentage points higher among women in this same age range in December of 2005 than it otherwise would have been. Given that the actual increases during this same

period were 1.64 and 3.41 percentage points for men and women, respectively, our findings suggest that this policy change was an important contributor to the rise in SSDI enrollment. Our estimates further suggest that the effect on SSDI enrollment twenty years from now, when those who received the maximum reduction in retired worker benefits will have aged into their fifties and early sixties, will be almost twice as large than at present.

2.2 Background on the OASDI Program and the 1983 Amendments

2.2.1 Retired Worker and Disabled Worker Benefits Prior to the Amendments

The Old Age, Survivors, and Disability Insurance (OASDI) program currently provides retirement, disability, and survivor benefits to more than 48 million U.S. residents. Payments to retired workers and their dependents accounted for more than 70 percent of program spending during the 2004 calendar year. To be eligible for retired worker benefits, an individual must be at least 62 years old and must have accumulated at least forty quarters of coverage during his or her working years. The amount of earnings necessary for each quarter of coverage is generally increasing over time, with \$3680 or more sufficient to earn four quarters during the 2005 calendar year. Thus a person with a significant amount of earnings in ten or more years would generally be eligible for retired worker benefits upon reaching the age

of 62.

The eligibility criteria are somewhat different for Social Security Disability Insurance benefits. First, a person younger than 18 or older than the full retirement age at the time she became disabled would not be eligible for disabled worker benefits. Second, the individual must have accumulated at least twenty quarters of coverage during the ten years leading up to the onset of the disability.⁴ Third, the person must not be engaging in substantial gainful activity, which is currently defined to be earnings in excess of \$860 per month. And finally, the individual must apply for SSDI benefits at a local Social Security Administration (SSA) field office. If the SSA determines that the individual is unable to work, then an SSDI award is made.

To determine an individual's retired worker or disabled worker benefits, the SSA first calculates her average indexed monthly earnings (AIME). For a retired worker, this is equal to the average of her 35 highest years of indexed earnings.⁵ The number of years used in the AIME calculation for a disabled worker is lower given that she would have fewer possible work years. For example, the SSA would use the highest 21 years of indexed earnings for an SSDI recipient disabled at the age of 47 versus 34 years for someone disabled at the age of 60.⁶ The AIME is then

⁴The number of quarters of coverage needed to be SSDI-insured is lower for younger workers.

⁵Taxable earnings are indexed in each year using an indexing factor, which is equal to the ratio of average wages in the year that the person reaches the age of 60 to average wages in the year considered. Earnings at ages 61 and up are not indexed. For a list of indexing factors used for each year of earnings by year-of-birth, see Table 2.A.8 in the Social Security Administration's Annual Statistical Supplement (2006).

⁶For SSDI recipients disabled at the age of 47 or later, the number of years used is equal to "the number of full calendar years elapsing between the age 21 and the year of first eligibility, usually excluding the lowest 5 years. Workers disabled before the age of 47 have 0 to 4 years excluded." (SSA, 2006).

used to compute the Primary Insurance Amount (PIA), which is the monthly Social Security benefit payable to a retired worker if they first claim benefits at the full retirement age or to a disabled worker when they begin receiving benefits at any age. The PIA formula is progressive, so that the fraction of earnings replaced by social security benefits declines as one's AIME increases.⁷

Upon reaching the early retirement age of 62, insured persons have the option to claim retired worker benefits, though at a reduced rate. For individuals born in 1937 or earlier, the penalty for claiming in the month of attaining the age of 62 is 20 percent, so that the person's monthly benefit would be just 80 percent of the PIA. For each month that the person chooses to delay claiming, the penalty declines by 5/9 of a percentage point, with this penalty reaching zero at the full retirement age of 65. This adjustment to benefits for claiming early was designed to be approximately actuarially fair for a person with average mortality.

The monthly Social Security benefit for an SSDI recipient is equal to 100 percent of the PIA when they are first awarded benefits and, like retired worker benefits, this benefit is indexed to inflation in subsequent years. SSDI recipients are converted to Social Security's retirement program (with the same monthly benefit) upon reaching the full retirement age.

⁷More specifically, in 2005 the first \$627 of the AIME is replaced at 90 percent, the next \$3152 is replaced at 32 percent, and any remaining AIME is replaced at 15 percent. Only earnings that were subject to OASDI payroll taxes are considered when calculating the AIME.

2.2.2 The Social Security Amendments of 1983

On April 20, 1983, the Social Security Amendments of 1983 were signed into law. The main motivation for this legislation was to improve both the short and the long-term fiscal health of the OASDI program. Included in these Amendments were a number of significant changes to social security, including an increase in the payroll tax rate, an expansion in the number of individuals covered by the program, and an increase in the actuarial adjustment factors beyond the full retirement age. Perhaps the most significant change of all, however, was a two-year increase in the full retirement age and a corresponding increase from 20 to 30 percent in the penalty for claiming retired worker benefits at the early retirement age of 62.

These reductions in the generosity of Social Security retired worker benefits were phased in gradually and occurred in two main stages. Individuals born in 1937 or earlier were unaffected by the change. The full retirement age then increased in two-month increments by subsequent birth cohort until reaching 66 for those born in 1943, where it remained until again increasing in two-month increments from the 1955 to 1960 cohorts. Along with this change, the fraction of full benefits that individuals could receive at the early retirement age of 62 fell from 80 percent for those born in 1937, to 75 percent for those born between 1943 and 1954, and to 70 percent for those born in 1960 or later.⁸ The changes in the full retirement age and

⁸This policy also changed the actuarial adjustment factors beyond the age of 62 from 5/9 of a percentage point per month to 5/12 of a percentage point per month. This converted back to 5/9 of a percentage point 36 months before the full retirement age. Thus a person born in 1943 could receive 75 percent of his or her PIA at the age of 62, 80 percent at the age of 63, 86.67 percent at the age of 64, 93.33 percent at the age of 65, and 100 percent at age 66. For more details, see Table 2.A.17.1 in the 2005 Annual Statistical Supplement (SSA, 2006).

in the fraction of the PIA available at the early retirement age of 62 are summarized in Figure 1.

As a result of this legislation, the generosity of OASDI retired worker benefits for individuals born in 1938 or later declined relative to what they otherwise would have been. To estimate the average impact of this policy change on retired worker benefits, we consider the case of an individual age 62 or younger who is planning to claim retirement benefits at the early retirement age of 62, the most common age of claiming retired worker benefits in every year of our study period. The average change in the present value of retirement benefits for an individual at age A born in year B is as follows:

$$\Delta PV_{A,B} = R_B S_{A,62} (1+r)^{A-62} \sum_{t=62}^{119} S_{62,t} (1+r)^{62-t} PIA \quad (2.1)$$

with $S_{X,Y}$ equal to the probability of surviving from age X to age Y (note that $S_{62,62} = 1$ and the assumption that $S_{62,120} = 0$), R_B equal to the percentage point reduction in early retirement benefits for individuals born in year B, r equal to the interest rate used to discount future benefits, and PIA is equal to the average primary insurance amount upon reaching the age of 62.⁹ For this same person at age A beyond the age of 62, the change in the present value of benefits can be written as:

⁹This formula does not account for variation over time in an individual's PIA. More specifically, the PIA depends partially on the age at which it is computed because, for example, different years of earnings may be used in the calculation. But this was true before and after the policy change and thus we do not attempt to model the change in the individual's PIA here.

$$\Delta PV_{A,B} = R_B \sum_{t=A}^{119} S_{A,t} (1+r)^{A-t} PIA \quad (2.2)$$

The key source of variation for our purposes in equations 2.1 and 2.2 arises through R_B , which represents the change in the generosity of retired worker benefits caused by the 1983 amendments. This ranges from a low of 0 for those born in 1937 or earlier to a high of 10 percentage points for those born in 1960 or later. As described above and in Figure 2.1, these increases in the penalty for claiming at the early retirement age were made in increments of 0.833 percentage points from 1938 to 1943, then remained at 5 percentage points from 1943 to 1954, and again increased in 0.833 percentage point increments from 1955 to 1960.

Of course, the average present value of retired worker benefits will vary across cohorts not only because of the 1983 Amendments. For example, mortality rates have generally been declining over time and this will tend to increase the value of benefits from one cohort to the next. Similarly the program's benefit formula is indexed to average wage growth, and thus the real value of the average PIA tends to increase over time. But to the extent that these changes produce a smooth trend in the value of retired worker benefits, the policy induced a decline in this trend relative to what it would have otherwise been.¹⁰

In Figure 2.2, we plot the average policy-induced decline in the present value of retired worker benefits as a function of age for men between the ages of 45 and 64

¹⁰For example, suppose that the present value was increasing by an average of 1 percent per year up through and including the 1937 cohort. The policy will reduce this to essentially zero, given that the present value is declining more than one percent per year as a result of the policy (0.833 percentage points divided by 80 percentage points).

who were born in 1939, 1941, and 1943. For these calculations, we use age-specific mortality rates for men from the SSA's Office of the Actuary, assume an annual discount rate of 3 percent, and set PIAB equal to the average primary insurance amount for men claiming retired worker benefits in 1999, the last year in which no retired workers would have been affected by the Amendments. As the figure demonstrates, the impact of the policy was significantly lower for cohorts born in 1939 than those born in 1941 or 1943. Additionally, the impact of the policy increased with age, both because of time discounting and because of the non-trivial probability that an individual would die before reaching his or her early retirement age. The effect of the policy then declines following the 62nd birthday, reflecting the fact that one or two years of reduced benefits would already have been received.

The variation in the policy's effect summarized in this Figure and in Table 2.1 increases by a factor of almost two from a 45-year old male to a 62-year old male.¹¹ More specifically, among men born in 1943, the decline in the present value of retired worker benefits for 45-year olds is \$4716 versus \$8878 for 62-year olds. These declines are one-third as large for the 1939 cohort and two-thirds as large for the 1941 cohort. And though their changes are not summarized in this Figure, the effect of the policy for individuals born in 1960 or later would be twice as large as for those born between 1943 and 1954. To calculate these same changes for women, we use female-specific mortality rates and the average PIA for women claiming retired worker benefits in 1999. At each age these declines are approximately one-third lower for women than

¹¹Of course the effect of the 1983 Amendments likely varied not only across cohorts but also within cohorts. One might expect, for example, high income individuals to be less responsive than their low income counterparts to the change given that social security accounts for a much smaller share of their income (Mitchell and Phillips, 2000).

for men. Thus despite the fact that mortality rates for women are much lower than those for men, the fact that their average PIA in 1999 is 40 percent lower more than offsets this.

2.2.3 Previous Research

Previous work for other government programs has suggested that reductions in the generosity of one program can lead to increases in enrollment in other programs.¹² In this case, Social Security retired worker benefits were reduced while no corresponding changes were made to SSDI benefit generosity. Thus, to the extent that individuals can potentially substitute disability for retirement benefits, a policy designed to reduce spending on retired worker benefits may have inadvertently increased SSDI applications and enrollment. Since social security benefits are the major source of income for 65 percent of elderly recipients and account for more than 90 percent of income for one-third of those individuals (SSA, 2006), it seems plausible that the significant decline in the generosity of retirement benefits due to the 1983 Amendments influenced individual behavior. Policymakers did recognize that the Amendments could lead to an increase in disability enrollment, with a U.S. General Accounting Office report stating that the magnitude of this increase would depend on the responsiveness of individuals to the increased incentive to apply for SSDI (GAO, 1998).¹³

Recent studies, however, find little evidence to suggest that SSDI enrollment

¹²See Garrett and Glied (2001), Kubik (2003), Schmidt and Sevak (2004), and Duggan and Kearney (2005).

¹³See Benitez-Silva et al (1999) and Hausman and Halpern (1986) for an examination of the determinants of the SSDI application decision.

should increase significantly as a result of the 1983 Social Security Amendments. For example, Mitchell and Phillips (2000) use data from the Health and Retirement Study to estimate a choice model with three potential retirement pathways: claim at the early retirement age, claim at the full retirement age, or apply for SSDI benefits. Their parameter estimates suggest that a \$25,000 cut in the present value of Social Security retired worker benefits¹⁴ would increase SSDI enrollment by 0.6 percentage points. They argue that this effect is small given that three times as many individuals would delay claiming Social Security retirement benefits as a result of the benefit cut. Additionally, Bound, Stinebrickner, and Waidman (2004) estimate a structural model of retirement to consider the impact of changes in the OASDI program on labor market behavior. Their simulations suggest an even smaller effect of the increase in the normal retirement on SSDI applications and enrollment.

While both of these studies make important contributions to knowledge, they do have some limitations. For example, both studies apply parameter estimates from their models to simulate the effects of reducing retirement benefits rather than using the actual changes in SSDI enrollment by birth cohort to estimate these effects. To the extent that the models do not fully capture key aspects of individual behavior, the results from these simulations could be misleading.¹⁵ Additionally both studies use a relatively small sample of individuals¹⁶ and thus even if the models were properly specified, it would be difficult to obtain precise estimates. And finally, the

¹⁴Using the average monthly retired worker benefit in 1999, this is approximately 40 percent larger than the average cut for a 62-year old even once the Amendment changes are fully phased in, as shown in Table 1.

¹⁵Mitchell and Phillips that it is hard to know what to attribute differences to.”

¹⁶The sample sizes used to estimate the models are 196 for Bound et al and 1544 for Mitchell and Phillips.

studies use data up through just 1998 (M,P) or 2000 (B,W,S). They are therefore unable to observe SSDI enrollment beyond the age of 60 or 62 for anyone affected by the Amendments.¹⁷

In the sections that follow, we build on this previous work by utilizing administrative data on SSDI enrollment through 2005 for a ten percent sample of the U.S. population along with an alternative identification strategy to investigate the effect of the policy-induced reduction in the generosity of retired worker benefits on SSDI enrollment.

2.3 The Rise in Disability Enrollment

The fraction of non-elderly adults receiving Social Security Disability Insurance benefits has risen substantially since the 1983 Social Security Amendments. When this legislation was enacted, 4.5 percent of those between the ages of 45 and 64 were receiving benefits and enrollment had actually been declining. But since the passage of the Amendments, the rate of SSDI enrollment in this age range has increased by 50 percent, with 6.7 percent of adults aged 45 to 64 on SSDI in December of 2005. The enrollment growth has been similarly striking for younger workers, with the fraction of individuals between the ages of 25 and 44 receiving SSDI benefits increasing from 0.7 to 1.6 percent. As a result of these increases, the fraction of Social Security spending accounted for by SSDI rose from 10 percent in 1983 to

¹⁷In related work, Gustman and Steinmeier (2005) estimate a structural model that considers the effect of an increase in the early retirement age on behavior, including application for SSDI benefits.

more than 17 percent by 2005.

2.3.1 Why Has SSDI Enrollment Increased?

Several factors have contributed to the increase in SSDI receipt.¹⁸ First, because the rate of SSDI receipt rises with age, the aging of the baby boom cohorts has contributed to the growth. But as Figures 2.3 and 2.4 demonstrate, the fraction of both men and women receiving SSDI has increased substantially at every age between 45 and 64. For example, in 1983 the fraction of 55-year old men receiving SSDI stood at 5.8 percent, but this increased to 7.5 percent by 2005. The corresponding rise for 55-year old women was even more striking, with enrollment for them increasing from 2.6 to 6.2 percent. The larger growth in SSDI enrollment among women is partly attributable to a second important contributing factor, the rise in female labor force attachment, which has increased the fraction of women who are insured for SSDI benefits.

A third factor that has contributed to the growth in SSDI enrollment was a liberalization of the program's medical eligibility criteria implemented in 1984, which made it easier for individuals with more subjective conditions such as mental disorders, back pain, and arthritis to qualify for the program. Perhaps partly as a result, the number of awards per SSDI insured person with these diagnoses more than tripled from 1983 to 2003 while there was no corresponding change for conditions that are easier to verify such as cancer, heart attacks, and stroke. SSDI

¹⁸Most previous work on SSDI has examined the program's effect on labor supply rather than the determinants of its growth. See for example Parsons (1980, 1991), Bound (1989, 1991), and Bound and Burkhauser (1999).

enrollment also increased during this period because of an increase in the replacement rate (the ratio of potential benefits to earnings) for low-skilled workers. This resulted from the interaction between a rise in income inequality and the progressive benefit formula described above. And finally, the recessions of both 1991 and 2001 led to a significant increase in the number applying for and ultimately receiving SSDI benefits.¹⁹

2.3.2 Changes in SSDI Enrollment by Birth Cohort

The 1983 Social Security Amendments increased the incentive for individuals to apply for the SSDI program by reducing the generosity of retired worker benefits. Individuals born in 1937 or earlier were unaffected by these Amendments, with the effect on an individual born later depending on her year-of-birth. The increase in the full retirement age and in the penalty for claiming benefits at the early retirement age of 62 for those born between 1943 and 1954 was twice as large as for their counterparts born in 1940 and six times as large as for a person born in 1938.

If the declining generosity of retired worker benefits did influence SSDI enrollment, one would expect - all else equal - individuals born in a year such as 1943 to be significantly more likely to receive SSDI benefits than individuals born in earlier years. To shed light on this issue, Figure 2.5 displays rates of SSDI enrollment for men between the ages of 60 and 64 by birth cohort.²⁰ As the figure shows, the rates

¹⁹See Rupp and Stapleton (1985), Gruber and Kubik (1997), Kreider (1997), Gruber (2000), Black, Daniel, and Sanders (2002), Autor and Duggan (2003), and Duggan and Imberman (2006) for an examination of the effect of screening stringency, the replacement rate, economic conditions, or demographic factors on disability enrollment.

²⁰Because 2005 is our most recent year of data, the rate at the ages of 63 and 64 are missing for the 1943 cohort.

in this age range are generally increasing with year-of-birth. For example, the rate of SSDI enrollment increases from 12.7 percent among those born in 1937 to 13.6 percent for men born in 1943. The differences displayed in Figure 2.6 for 62-year old women are even larger, with their enrollment rate increasing from 7.9 to 10.2 percent from the 1937 to the 1943 birth cohort.

Of course part of this increase could simply represent the continuation of a pre-existing trend in SSDI enrollment. To explore this possibility, the top two graphs of Figure 2.7 display rates of SSDI enrollment by birth cohort for 62-year old men and women, respectively. The first of these figures shows that enrollment among men was actually declining with year-of-birth prior to 1937. More specifically, the fraction of 62-year old men on SSDI fell from 13.0 to 12.7 percent from 1934 to 1937. And while for women there was a positive pre-existing trend prior to the 1938 birth cohort, the annual increase of 0.38 percentage points from 1937 to 1943 was more than twice as large as the corresponding increase of 0.17 percentage points from 1934 to 1937.

Thus the enrollment trends for both men and women at the age of 62 strongly suggests that individuals born in 1938 and later were significantly more likely to receive SSDI benefits than their counterparts born in earlier years and that this effect steadily increased in each year through the 1943 cohort. But even these trends do not rule out the possibility that some factor other than the 1983 Amendments is the main explanation for the break in trend. For example, perhaps macroeconomic conditions deteriorated sufficiently after 1999 to induce a rise in SSDI enrollment. If this were true, then this could be at least partially responsible for the SSDI

enrollment increase among 62-year olds that began in 2000.

But interestingly, as the bottom two graphs of Figure 2.7 show, the same pattern with year-of-birth emerges for these cohorts a decade earlier. For example, SSDI enrollment among 52-year old men declined from 4.5 to 4.3 percent from the 1934 to the 1937 cohort and then steadily increased to 5.5 percent for the 1943 cohort. The rate of SSDI enrollment for the 1946 cohort, whose retirement benefit reduction was identical to the 1943 cohort's, was also 5.5 percent. This figure and the corresponding one for women are consistent with the hypothesis that the 1983 Social Security Amendments increased SSDI enrollment. In the next section we outline our strategy for probing more systematically on the magnitude of this effect.

2.4 Identification Strategy

The 1983 Amendments reduced the present value of retired worker benefits for individuals born in or after 1938. As Figure 2.2 demonstrates, the average dollar impact of this change varied substantially with both age and year-of-birth. For individuals choosing between alternative pathways to retirement, the Amendments reduced the attractiveness of claiming retired worker benefits. Given that there were no corresponding changes to SSDI benefits, some individuals may have responded to the change by applying for SSDI, thereby pursuing an alternative path to retirement. This change in behavior could occur long before the age of 62 if individuals were forward-looking and recognized the increase in the relative attractiveness of SSDI benefits. If instead individuals were myopic, they might only react to the policy

once they were very close to or had already reached the early retirement age, when they might be more likely to carefully compare the difference between retired and disabled worker benefits.

If the Amendments did lead a substantial number of individuals to apply for and ultimately enroll in the SSDI program, one would expect to observe an increase in age-specific rates of enrollment as the 1938 and later birth cohorts reached each age. To accurately measure this effect, it is important to control for trends in SSDI enrollment, which could be changing for reasons unrelated to the Amendments. These pre-existing trends were especially apparent for women in the two right graphs in Figure 2.7, with this trend perhaps partly driven by the steady increase in female labor force attachment that made more women potentially SSDI eligible. Additionally, it is important to control for macroeconomic and other common factors that could exert an effect on SSDI enrollment at all ages.

Given these issues, we estimate models of the following type when testing for an effect of the 1983 Amendments on the fraction of individuals receiving SSDI benefits:

$$\Delta SSDI_{A,t} = \alpha_t + \beta \Delta PVR_{A,t} + \mu_t + \epsilon_{A,t} \quad (2.3)$$

In this equation, $SSDI_{A,t}$ is equal to the change in the fraction of individuals at age A in year t receiving SSDI benefits. SSDI enrollment is reported by SSA as of December in each year, and thus the year-of-birth for individuals of age A in year t would be t-A. We use age and gender-specific population data from the U.S. Census

Bureau and from the National Center for Health Statistics as the denominator when calculating these rates.²¹ We include year effects in this model α_t to control for common factors such as changes in macroeconomic conditions that might influence SSDI enrollment in each year and age effects μ_A to control for age-specific trends in SSDI enrollment.²²

Our key explanatory variable is $\Delta PVR_{A,t}$, which represents the average change in the present value of Social Security retired worker benefits at age A in year t induced by the Amendments. This is set equal to zero if birth cohorts $t-A$ and $t-A-1$ have the same generosity of retired worker benefits. For example, in 1987 this first difference would be equal to zero for 50-year olds because it would represent the difference in generosity between the 1936 and 1937 cohorts, both of which had a full retirement age of 65 and received 80 percent of full benefits when claiming at the age of 62. However in 1988 it is negative, as the generosity of retired worker benefits is significantly lower for the 1938 cohort than for their predecessors born in 1937. More specifically, using equation 2.1 from above, the present value of retired worker benefits is \$786 lower for the 1938 cohort of men than for their 1937 counterparts and \$592 lower for women born in 1938 than those born in 1937.²³ As Table 2.1 demonstrates, these amounts increase in magnitude with age until the age of 62,

²¹See the Data Appendix for a description of our data sources.

²²Our approach is similar in spirit to Attanasio and Brugiavini (2003), who estimate the effect of a change in Italy's Social Security program on household savings. Like the 1983 Social Security Amendments in the U.S., the magnitude of the change in Italy's social security benefits depended partially on individuals' year-of-birth.

²³Thus for 50-year olds the value of $\Delta PVR_{A,t}$ would be 0 in 1985, 1986, and 1987, would equal -.932 for men and -.695 for women in each year between 1988 and 1993, and would then equal 0 in 1994, 1995, and 1996. The variable is defined similarly at other ages, though the size of $PVR_{A,t}$ in the treatment years varies as shown in Table 1.

at which point they decline somewhat in each year given that one or more year of benefits would already have been received (assuming early claiming at the age of 62 as in equation 2.2 above).

If properly estimated, the parameter from equation 2.3 represents the average effect of Social Security retired worker benefit generosity on SSDI enrollment. As described above, the 1983 Amendments reduced the generosity of retired worker benefits in two main stages. Because those affected by the latter set of changes were relatively young in 2005, our most recent year of data, we begin by focusing on the first set of changes, which were phased in from the 1938 to the 1943 birth cohorts. To do this we restrict consideration to the 1934 to 1946 birth cohorts, which gives us up to twelve first differences at every age. The first three of these are for the 1934 to 1937 cohorts and thus prior to the change in retired worker benefits and the last three are for the 1943 to 1946 cohorts and thus after these changes have been phased in.

The main advantage of considering a relatively small number of cohorts is that in each year the ages that we consider are fairly similar. For example, as shown in Table 2.2, in 1995 the treatment group consists of individuals between the ages of 52 and 57, while the control group includes people between the ages of 49 and 51 and between 58 and 60. Our model essentially tests whether in this year and in the 21 other years after the 1983 Amendments, SSDI enrollment increases more rapidly for those in the treatment group in each year than for their counterparts in the control group. By considering a more homogeneous group in every year, it is more likely that any unobserved factors that might influence SSDI enrollment would have a similar

effect on our treatment and control groups, which is an important assumption of our model. We later expand the size of our analysis sample by considering individuals born as early as 1920 and as late as 1960.

We further restrict attention to individuals between the ages of 45 and 64. Our primary reason for doing this is that, as Table 2.2 demonstrates, this restriction ensures that we have at least four “treatment” changes at each age.²⁴ It also seems reasonable given both that the change in the present value of benefits was lower for younger workers and that their baseline rates of SSDI enrollment were several times lower as well.²⁵

2.5 Empirical Results

Our first main set of results is summarized in Table 2.3, with columns 1 through 4 providing the results from specifications similar to 2.3 for men and the next four columns displaying the analogous results for women. The unit of observation in every case is the change in the fraction of either men or women receiving SSDI at a certain age from one year to the next. All specifications include age effects to control for different trends across age groups in SSDI enrollment. We also include year effects to control for factors such as macroeconomic conditions that might influence SSDI

²⁴If we included 40-year olds, for example, then there would be no treatment changes at this age given our definition because the 1983 to 1984 change reflects the difference between the 1943 and 1944 birth cohorts, both of which had a full retirement age of 66 and an early retirement penalty of 25 percent. Despite this, there may still have been a policy-induced increase at his age from 1983 to 1984 because of the lag in the SSDI application process.

²⁵To the extent that the 1983 Social Security Amendments also influenced SSDI enrollment for individuals between the ages of 18 and 44, we are likely to understate the overall effect of the policy below.

enrollment from one year to the next.

In this table, we report coefficient estimates for our key explanatory variable $\Delta PV R_{A,t}$, which is equal to the average change (in thousands of dollars) in the present value of retired worker benefits at each age. This variable has a mean of -0.600 and a standard deviation of 0.579 for men in our analysis sample, with the corresponding statistics for women at -0.440 and 0.422. The ages that serve as both treatment and control groups in these regressions are listed in Table 2.2. As this table shows, there are 215 first differences that we consider over a 22 year period, with 116 of these in the treatment group and 99 in the control group. It is important to emphasize that the intensity of treatment varies with age, with the change in the present value being much larger for the average 62-year old than for the average 45-year old.

In column 1 of Table 2.3, we report results for the effect of changes in Social Security retired worker benefits on SSDI enrollment among men between the ages of 45 and 64. The statistically significant estimate of -.0778 suggests that each \$5000 reduction in the present value of Social Security retired worker benefits induces a 0.39 percentage point increase in SSDI enrollment.²⁶ Given that the present value effects of the policy are increasing with age, this implies a smaller increase in enrollment at younger ages. For example, \$5000 is approximately equal to the change in the present value from the 1937 to the 1943 cohort for a 47-year old man. Thus if

²⁶When estimating these models, we account for the fact that the policy's effect varies with year-of-birth by using the StataTM cluster command and clustering by year-of-birth. Recent econometric evidence suggests that clustering may be problematic when the number of clusters is smaller than approximately 50. In our final set of specifications we consider 41 birth cohorts and obtain similar results.

correct, our estimates imply a 0.39 percentage point increase from this first part of the policy change. However, the implied effect in SSDI enrollment for 62-year olds is almost twice as large.

In the specifications summarized in columns 2 through 4, we test whether the effect of this policy varies by age. The estimated effect of $-.1066$ summarized in column 2 for 45 to 54-year olds is slightly greater than the corresponding estimate of $-.0736$ for 55 to 64 year old men in column 3. Both of these estimates are statistically significant. In the fourth column we include all ages but interact PVR_A , with an indicator for whether this cell is between the ages of 55 and 64. While the estimate for this interaction is positive, it is not statistically significant. Thus one cannot conclude from these estimates that younger individuals are more or less responsive to the policy change.

In the next four columns we present an analogous set of estimates for women. The statistically significant coefficient estimate of $-.1639$ in column 5 is more than twice as large as the corresponding estimate for men from column 1. But because the effect of the Amendments on the average present value of retired worker benefits was considerably lower for women than for men, the implied effect on overall SSDI enrollment is not twice as large, as we show in the next section. One possible explanation for the greater responsiveness among women is that their baseline rates of labor force participation are significantly lower throughout our study period. This would imply that more women than men could respond to the policy by applying for SSDI without having to first leave their job. Interestingly, the results in columns 6 through 8 suggest that older women are somewhat more responsive to the policy

change, though the difference is not statistically significant.²⁷

We next pool rates of SSDI enrollment for men and women and present an analogous set of specifications for all individuals in columns 1 through 3 of Table 2.4. Our explanatory variable here is simply the average of the change in the present value for women and men. Not surprisingly, our estimates in each case lie between the estimates for men and women. For example, the estimated effect of -.1151 is approximately the average of the corresponding estimates of -.0778 and -.1639 for men and women, respectively. In this case the estimates for younger and older individuals are almost identical, as shown in columns 2 and 3.

One possible concern with this first set of estimates is that they rely exclusively on the first part of the Amendments, which were phased in across the 1938 to 1943 cohorts. In the next specification we present the results from an analogous specification in which we consider only the second change, which was phased in from 1954 to 1960.²⁸ To preserve consistency with the first set of estimates, we consider 13 birth cohorts, though in this case we use only "pre-treatment" birth cohorts (born from 1948 to 1954) given that no "post-treatment" cohorts had yet reached the age of 45 by 2005. Our estimated effect for this latter policy is similar to those for the initial one, though the standard errors are considerably larger as well.

This is presumably because we have fewer observations and because the variation

²⁷Our results are very similar if we instead use an annual discount rate of 2 or 4 percent when calculating the present value of retired worker benefits. Of course they would also be very similar if we used the average PIA from a different year (instead of 1999), as this would simply multiply our current values of PVRT,A by a common factor.

²⁸For this change, we observe 6 years of the treatment for 45 year olds (first differences from 2000 to 2005 for cohorts 1955-60), 5 years for 46 year olds (2001 to 2005), four years for 47 year olds (2002 to 2005), three years for 48 year olds (2003 to 2005), two years for 49 year olds (2004 to 2005), and one year for 50 year olds (2005).

in $\Delta PVR_{A,t}$ is smaller given that the treated individuals are relatively young. In any case, this negative point estimate provides suggestive support for an effect of retired worker benefit generosity on SSDI enrollment.

In the next three columns we merge these two groups (along with the 1946-47 and 1947-48 first differences) and thus simultaneously consider 27 birth cohorts. Our estimates for the effect of SSDI benefits are slightly smaller than are the ones for the first part of the policy though they remain statistically significant. When we differentiate between those between the ages of 45 and 54 and their counterparts aged 55 to 64, our estimates suggest an effect that is approximately 50 percent greater for older workers. Estimates for both age groups remain statistically significant.

In the final specification, we consider individuals between the ages of 45 and 64 in every year between 1984 and 2005. By doing this, we consider 41 birth cohorts, those born between 1920 and 1960. Our point estimate from column 5 is virtually unchanged, though the standard error rises slightly and thus our estimates there are significant at just the ten percent level.

It is worth emphasizing that our finding that individuals responded to the reduction in retirement benefits long before reaching the early retirement age strongly suggests both that they are forward-looking and - if they left their jobs as a result of the policy change to apply for SSDI - that they are not liquidity constrained.²⁹ If individuals were forced to live on current income because they were liquidity constrained, they would not have the flexibility to respond to the reduction in fu-

²⁹It is of course possible that many of the individuals who applied for and ultimately received SSDI as a result of the policy change were already unemployed or out of the labor force. In other words, the finding that the policy increased SSDI enrollment does not necessarily imply that those who responded would otherwise have been working.

ture retirement benefits by leaving their job in order to apply for SSDI benefits. An important avenue for future work would be to calibrate a life cycle model to determine which individuals would be most likely to respond to the 1983 Social Security Amendments by applying for SSDI and at what point in their life cycle these responses would be most likely to occur.³⁰

Taken together, the results presented in this section combined with the graphical evidence from the preceding section strongly suggest that the reduction in the generosity of Social Security retired worker benefits caused by the 1983 Social Security Amendments led to a significant increase in SSDI enrollment. While the confidence intervals surrounding our estimates are compatible with a wide range of effects, this policy seems to have made an important contribution to the steady growth in SSDI enrollment during the past two decades. In the next section, we explore how much of the growth in enrollment can be explained by the Amendments and what the long-run effect will be on the program given that its effects will not be fully felt until those born in 1960 reach their full retirement age in 2027. At that point, all adults between the ages of 18 and 66 would have a full retirement age of 67 and a maximum early retirement penalty of 30 percent.

³⁰See Card, Chetty, and Weber (2006) for a recent example for the case of unemployment insurance in Austria. In this paper the authors calibrate and test several different intertemporal models.

2.6 The Contribution of the 1983 Amendments to the Rise in SSDI Enrollment

By December of 2005, the changes made by the 1983 Social Security Amendments had reduced the present value of retired worker benefits for all non-elderly adults. The magnitude of this change varied with age, with much of this variation attributable to the policy not yet being fully phased in for older workers. For example, individuals who were 62 years old in 2005 were born in 1943 and thus were exposed to just the first half of the reduction in benefit generosity, with their early retirement penalty and full retirement age at 25 percent and 66 years old, respectively. However, their counterparts born in 1960 were fully affected by the Amendments, with an early retirement penalty of 30 percent and a full retirement age of 67. Thus as Table 2 demonstrates, the average reduction in retired worker benefits was larger for 45 year olds (\$9432 for men and \$7104 for women) than for 62 year olds (\$8878 for men and \$6348 for women). The smallest changes in benefits were present for 64-year olds, who were exposed to just one-third of the eventual reduction in benefit generosity because they were born in 1941.³¹

To estimate the contribution of the 1983 Social Security Amendments to the rise in SSDI enrollment during our study period, we multiply these changes in retired worker benefits at each age by our gender-specific point estimates for the effect of these changes from Table 2.3.³² Specifically we use the estimate of -.0778 from

³¹The benefit reduction is also smaller for them than for 62-year olds because they would already have received one or two years of benefits, which would thus not be included in the calculation.

³²Here we neglect the mechanical effect of the Amendments on SSDI enrollment, which occurs because there will now be some 65 and 66 year old individuals on the program. Absent the

specification 1 for men and -.1639 from specification 5 for women. We report both our estimates for the policy's effect on age-specific SSDI enrollment and the actual change in SSDI enrollment during our study period in the columns with the headings FRA Impact and Actual Δ , respectively.

The results presented in Table 2.5 suggest that the 1983 Social Security Amendments had a substantial effect on SSDI enrollment. For example, our estimates suggest that the policy increased enrollment by 0.52 percentage points among 55-year old men versus an actual change of 1.77 percentage points. The corresponding estimate for 55-year old women is 0.81 percent versus an actual change of 3.54 percentage points. In general, while the implied effects for women are larger than those for men at every age, they explain a smaller share of the increase for women. For 55-year old adults, the effect of the reduction in retired worker benefits can explain 29 percent of the growth in SSDI enrollment for men versus 23 percent for women.

In the final row of this table, we estimate the impact of the policy change on overall SSDI enrollment among 45 to 64-year old men and women. In doing this, we use the age distribution for both genders in December of 2005. Our estimates there suggest that the retired worker benefit changes caused by the 1983 Social Security Amendments increased SSDI enrollment among men by 0.58 percentage points and among women by 0.89 percentage points. When compared with the actual increases, our estimate suggests that the policy change can explain 35 percent of the increase in SSDI enrollment among men between the ages of 45 and 64 and 26 percent for women. Additionally, our findings suggest that the number of non-

Amendments, an SSDI recipient would shift to retired worker benefits on her 65th birthday.

elderly individuals receiving SSDI benefits was 568,000 greater in late 2005 than it would have been in the absence of the Amendments.

In the final column for both men and women, we estimate the long-run effect of the reduction in retired worker benefits on SSDI enrollment. This represents our estimate of the effect in 2024 and all subsequent years, when all individuals between the ages of 45 and 64 would have a full retirement age of 67 and an early retirement penalty of 30 percent. To do this we first calculate the change in the present value of benefits assuming that the policy has been fully phased in. This was already true for 45-year olds by 2005, but for all other ages the policy had been just partially phased in. For example, those between the ages of 51 and 62 had been "treated" with just half of the policy by 2005 and 64-year olds had been "treated" with just one-third of the policy change. Thus our estimate of the long-run effect for 64-year olds is three times greater than the estimated effect in 2005.

Our estimate for the long-run effect of the policy on SSDI enrollment for men and women between the ages of 45 and 64 is reported in the final row of the Long-Run columns. The estimates of 1.00 percentage points for men and 1.56 percentage points for women are approximately 75 percent greater than the analogous estimated effects as of 2005. It therefore appears that the reduction in the generosity of retired worker benefits caused by the 1983 Social Security Amendments will contribute almost as much to SSDI enrollment during the next two decades as it has during the past two decades.

2.7 Conclusion

The primary goal of the 1983 Social Security Amendments was to increase the short and long-term fiscal health of the OASDI program. Perhaps the most significant change resulting from this legislation was a reduction in the generosity of Social Security retired worker benefits. No corresponding changes were made to disabled worker benefits. Our findings in this paper suggest that one important effect of this legislation was an increase in the number receiving SSDI benefits. More specifically, we find that the Amendments can explain more than one-third of the increase in SSDI enrollment among men since 1983 and more than one-fourth of the increase among women during this same period. Because the reductions in benefit generosity have not yet been fully phased in, the long-run effect on SSDI enrollment will be almost twice as large.

These results suggest that any changes to Social Security retired worker benefits may have important spillover effects to the SSDI program. In the case considered here, part of the reduction in spending for Social Security retired worker benefits was offset by an increase in spending on disabled worker benefits. Recent proposals to reform Social Security, such as those presented by the President's Commission in 2001, have largely ignored the SSDI program. The findings presented in this paper suggest that policymakers may want to incorporate SSDI when considering how optimally to reform the U.S. Social Security program.

In this paper we have explored just one response to the 1983 Social Security Amendments, though this legislation may have had other important effects as well.

For example, reductions in the actuarial adjustment factors beyond the age of 62 may have changed individuals' optimal timing for claiming social security retired worker benefits. Similarly, by reducing the present value of Social Security retirement wealth, the legislation may have affected individuals' optimal labor supply and savings decisions. More work on the effect of this legislation, which represented one of the most important set of changes to Social Security since its inception more than seventy years ago, is clearly warranted.

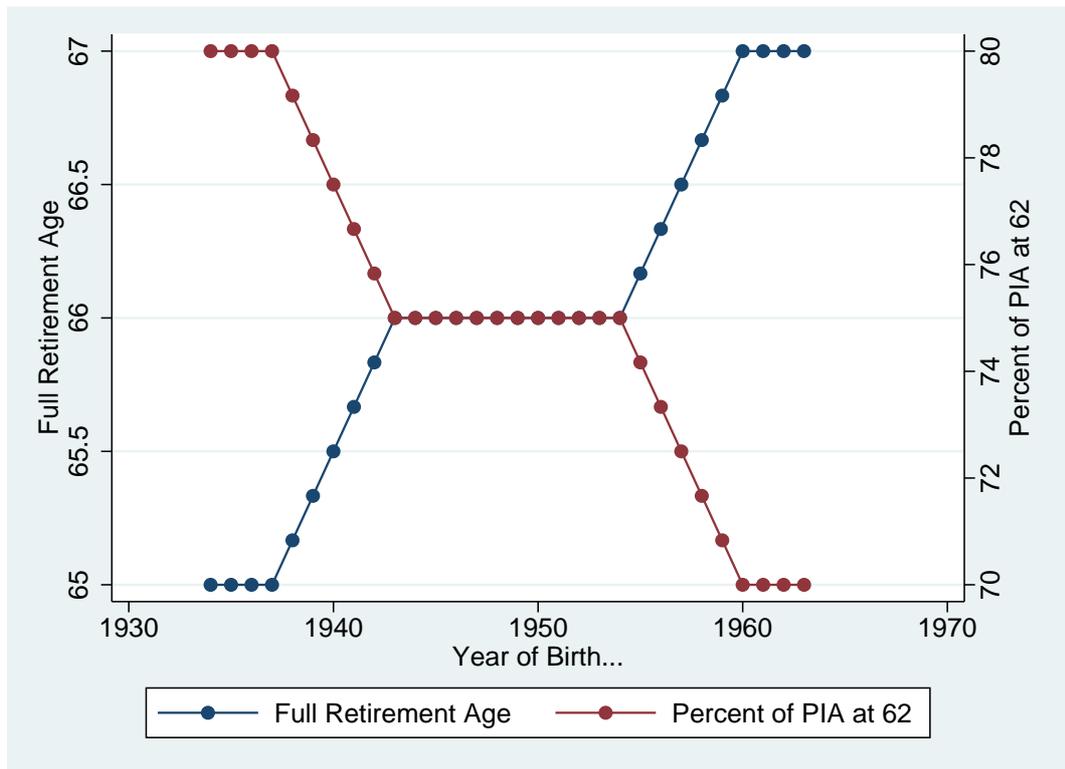


Figure 2.1: Social Security Retired Worker Benefit Generosity by Year-of-Birth

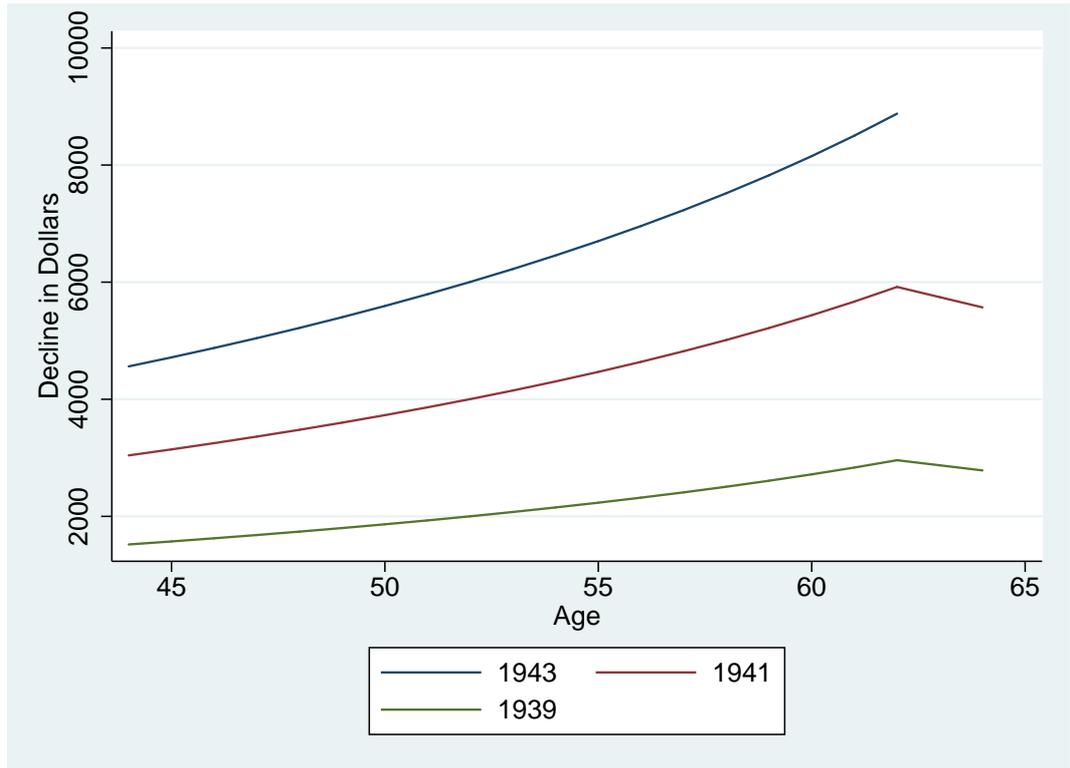


Figure 2.2: Average Decline for Men in PV of Retired Worker Benefits by Age and Year-of-Birth

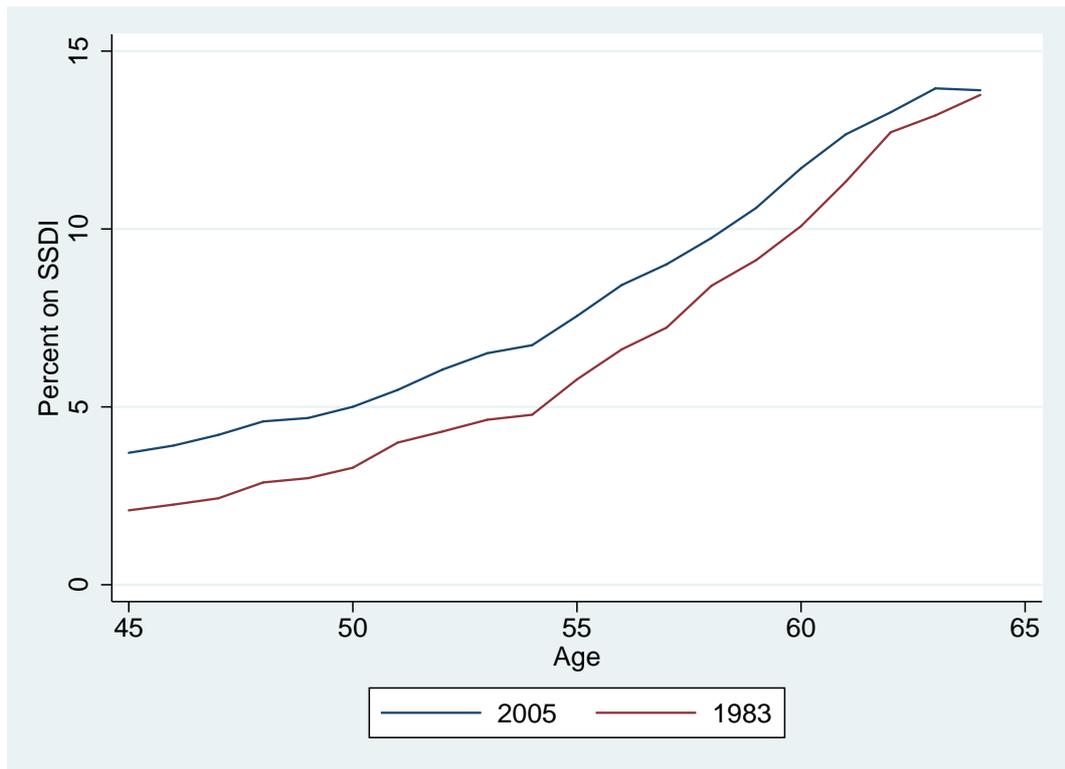


Figure 2.3: Percent of Men Receiving SSDI by Age in 1983 and 2005

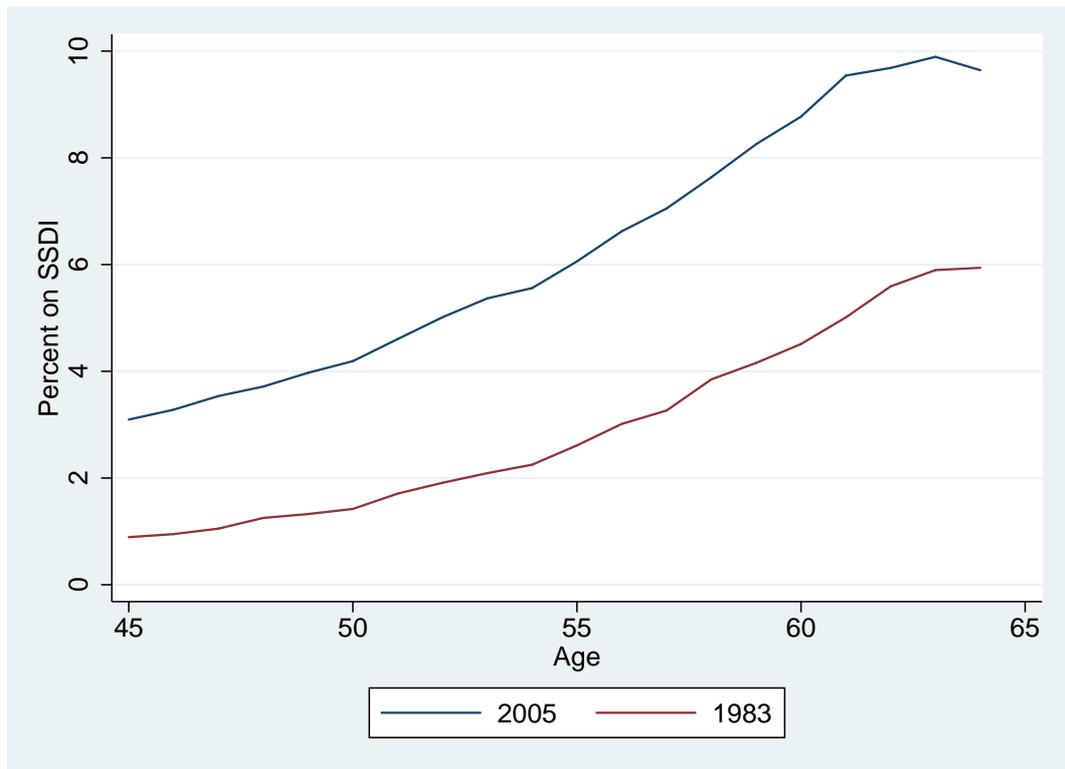


Figure 2.4: Percent of Women Receiving SSDI by Age in 1983 and 2005

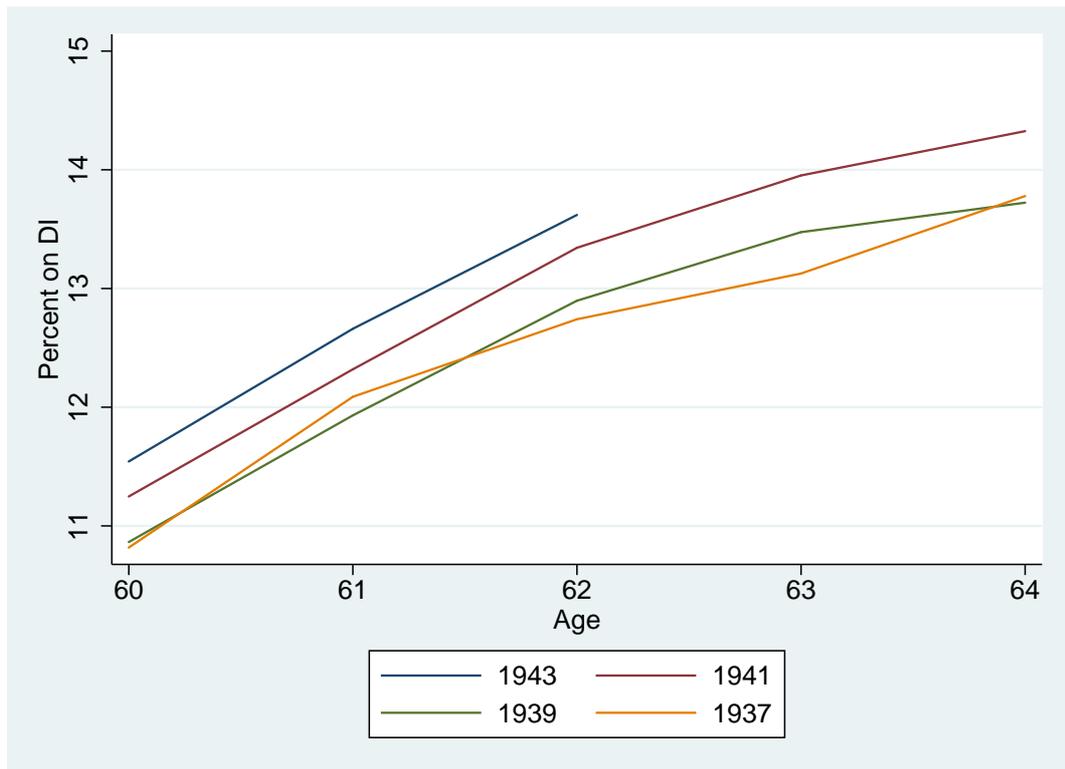


Figure 2.5: Percent of Men Receiving SSDI by Age and Year of Birth

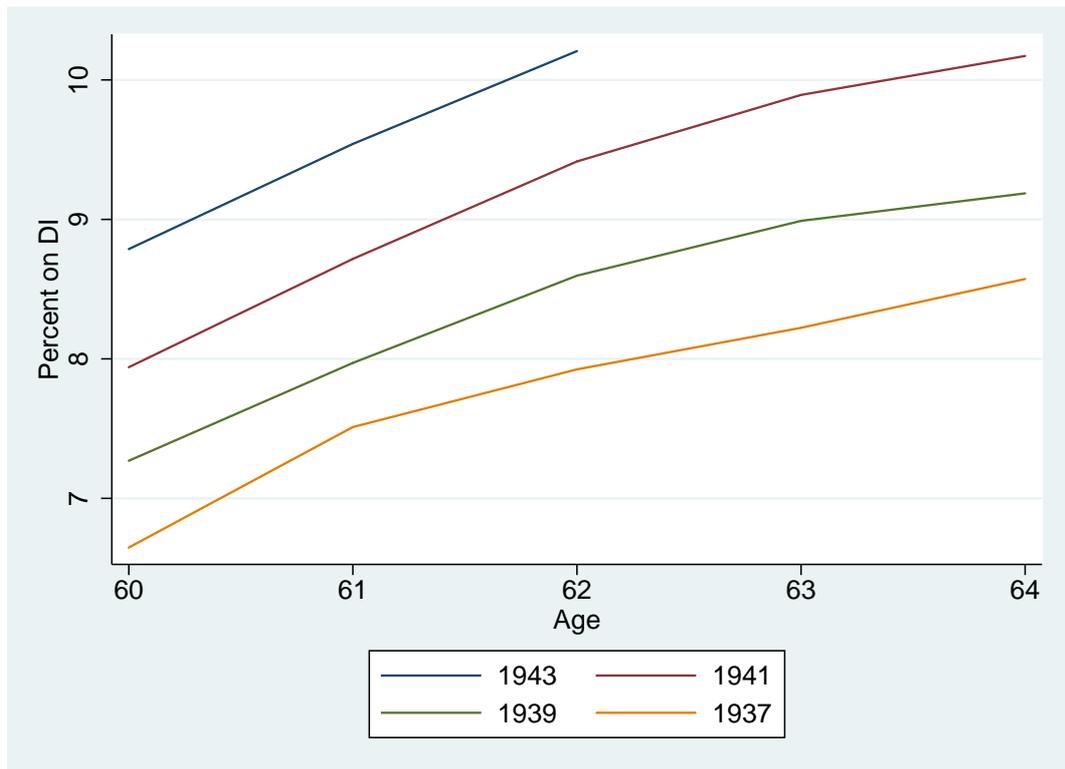


Figure 2.6: Percent of Women Receiving SSDI by Age and Year of Birth

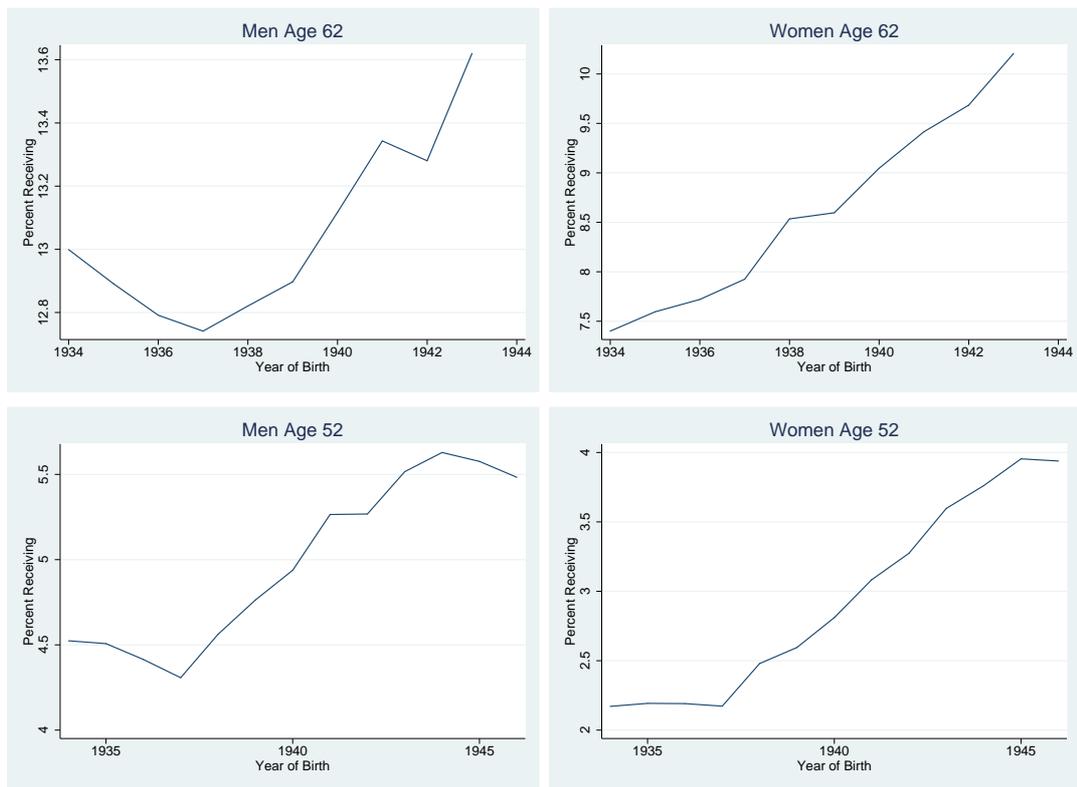


Figure 2.7: Percent Receiving SSDI by YOB, Sex and Age

Table 2.1: Average Present Value of Social Security Retired Worker Benefits by Gender and Birth Cohort

age	<i>Men by Year-of-Birth</i>				<i>Women by Year-of-Birth</i>			
	<=1937	1943-1954	>=1960	ANNUAL Δ	<=1937	1943-1954	>=1960	ANNUAL Δ
45	\$75,452	\$70,736	\$66,020	-\$786	\$56,829	\$53,277	\$49,726	-\$592
46	\$78,019	\$73,143	\$68,267	-\$813	\$58,667	\$55,000	\$51,334	-\$611
47	\$80,700	\$75,656	\$70,612	-\$841	\$60,575	\$56,789	\$53,003	-\$631
48	\$83,501	\$78,282	\$73,063	-\$870	\$62,557	\$58,647	\$54,737	-\$652
49	\$86,429	\$81,027	\$75,626	-\$900	\$64,616	\$60,577	\$56,539	-\$673
50	\$89,492	\$83,899	\$78,305	-\$932	\$66,755	\$62,583	\$58,411	-\$695
51	\$92,700	\$86,906	\$81,112	-\$966	\$68,980	\$64,669	\$60,358	-\$719
52	\$96,065	\$90,061	\$84,057	-\$1,001	\$71,297	\$66,841	\$62,385	-\$743
53	\$99,598	\$93,373	\$87,148	-\$1,037	\$73,714	\$69,107	\$64,500	-\$768
54	\$103,311	\$96,854	\$90,397	-\$1,076	\$76,237	\$71,472	\$66,707	-\$794
55	\$107,218	\$100,517	\$93,816	-\$1,117	\$78,876	\$73,947	\$69,017	-\$822
56	\$111,337	\$104,378	\$97,420	-\$1,160	\$81,641	\$76,539	\$71,436	-\$850
57	\$115,690	\$108,459	\$101,229	-\$1,205	\$84,543	\$79,259	\$73,975	-\$881
58	\$120,304	\$112,785	\$105,266	-\$1,253	\$87,592	\$82,118	\$76,643	-\$912
59	\$125,209	\$117,384	\$109,558	-\$1,304	\$90,803	\$85,128	\$79,453	-\$946
60	\$130,442	\$122,290	\$114,137	-\$1,359	\$94,191	\$88,304	\$82,417	-\$981
61	\$136,043	\$127,540	\$119,038	-\$1,417	\$97,772	\$91,662	\$85,551	-\$1,018
62	\$142,055	\$133,176	\$124,298	-\$1,480	\$101,569	\$95,221	\$88,873	-\$1,058
63	\$137,871	\$129,254	\$120,637	-\$1,436	\$98,872	\$92,693	\$86,513	-\$1,030
64	\$133,675	\$125,320	\$116,966	-\$1,392	\$96,154	\$90,144	\$84,135	-\$1,002

Values displayed in this table represent the average present value of retired worker benefits for three different groups - those unaffected by the 1983 Amendments (born in or before 1937), those affected by half of the long-run reduction in benefit generosity (born between 1943 and 1954), and those fully affected by the reduction in benefit generosity (born in or after 1960). For these present value calculations, we use equations 2.1 and 2.2 from the text. We use the average PIA for retired worker benefits in 1999 for men (\$1061) and for women (\$674), a 3 percent annual discount rate, and the age and gender specific mortality rates from the SSA's Office of the Actuary. The Annual D column represents the annual average change in this present value during the phase-in periods from cohort 1937 to 1943 and from cohort 1954 to 1960.

Table 2.2: Cohort and Age Group Considered in Each Year: 1984-2005

Year	Cohorts	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	Total
1984	1934-1944	T	T	C	C	C																5
1985	1934-1945	T	T	T	C	C																6
1986	1934-1946	T	T	T	C	C																7
1987	1934-1946	T	T	T	T	C	C		C													8
1988	1934-1946	T	T	T	T	T	C	C	C													9
1989	1934-1946	C	T	T	T	T	T	C	C	C												10
1990	1934-1946	C	C	T	T	T	T	T	C	C	C											11
1991	1934-1946	C	C	C	T	T	T	T	T	C	C	C										12
1992	1934-1946		C	C	C	T	T	T	T	T	C	C	C									12
1993	1934-1946			C	C	C	T	T	T	T	T	C	C	C								12
1994	1934-1946				C	C	C	T	T	T	T	T	T	C	C							12
1995	1934-1946					C	C	C	T	T	T	T	T	T	C	C						12
1996	1934-1946						C	C	C	T	T	T	T	T	T	C	C					12
1997	1934-1946							C	C	C	T	T	T	T	T	T	C	C				12
1998	1934-1946								C	C	C	T	T	T	T	T	T	C	C			12
1999	1934-1946									C	C	T	T	T	T	T	T	T	C	C		12
2000	1935-1946										C	C	T	T	T	T	T	T	T	C	C	11
2001	1936-1946											C	C	T	T	T	T	T	T	T	C	10
2002	1937-1946												C	C	C	T	T	T	T	T	T	9
2003	1938-1946													C	C	C	T	T	T	T	T	8
2004	1939-1946														C	C	C	T	T	T	T	7
2005	1940-1946															C	C	C	T	T	T	6
Total	215	8	9	10	11	12	12	12	12	12	12	12	12	12	12	12	11	10	9	8	7	215

Each row of the table lists the treatment (T) and control (C) groups in a particular year and at each age for the specifications summarized in Table 3 and in the first three specifications of Table 4, which utilize SSDI enrollment data for the 1934 - 1946 birth cohorts. The variable DPVR is equal to 0 if an age-year cell is denoted C and is otherwise equal to the value listed in Table 1 for each specific age. Thus DPVR is equal to 0 for 52-year old men in the three first differences from 1987-1989 (which represent the changes from the 1934-35, 1935-36, and 1936-37 birth cohorts), is equal to -1.001 in the next six years (first differences from 1937-43), and is equal to 0 in the three first differences from 1996-1998 (first differences from 1943-46).

Table 2.3: The Impact of Changes in Retired Worker Benefits on Male and Female SSDI Enrollment

	Men				Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ PVR	-0.0778** (.0334)	-0.1066** (.0489)	-0.0736** (.0307)	-0.0918* (.0499)	-0.1639** (.0642)	-0.1302** (.0464)	-0.1915** (.0840)	-0.1242** (.0463)
Δ PVR * (Age 55-64)				0.0217 (.0378)				-0.0630 (.0651)
Ages Included	45-64	45-54	55-64	45-64	45-64	45-54	55-64	45-64
Years Included	1984-2005	1984-2000	1990-2005	1984-2005	1984-2005	1984-2000	1990-2005	1984-2005
# Observations	215	110	105	215	215	110	105	215
Mean of Δ SSDI	0.0858	0.0923	0.0789	0.0858	0.1910	0.1266	0.2584	0.1910
Mean of Δ OA PV	-0.600	-0.496	-0.710	-0.600	-0.440	-0.370	-0.514	-0.440
R-squared	0.505	0.555	0.502	0.506	0.536	0.627	0.375	0.573

The dependent variable is equal to the fraction of men (specifications 1-4) or women (specifications 5-8) on SSDI in an age-year cell. The unit of observation is age*year. The ages included in each year are listed in Table 2. The variable Δ PVR is equal to the change in the present value of retired worker benefits in an age cell from one year to the next. All first difference regressions include 22 year effects and 20 age effects. Standard errors are included in parentheses and are clustered by year-of-birth. Δ SSDI is measured in percentage points and Δ PVR is measured in thousands of 1999 dollars.

Table 2.4: The Impact of Changes in Retired Worker Benefits on Overall SSDI Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ PVR	-0.1151** (.0404)	-0.1168** (.0455)	-0.1240** (.0440)	-0.0834 (.0668)	-0.0866** (.0351)	-0.0731** (.0365)	-0.1129** (.0423)	-0.0803* (.0414)
Ages Included	45-64	45-54	55-64	45-56	45-64	45-54	55-64	45-64
Years Included	1984-2005	1984-2000	1990-2005	1994-2005	1984-2005	1984-2005	1990-2005	1984-2005
Cohorts Included	1934-46	1934-46	1934-46	1948-60	1934-60	1934-60	1934-50	1920-60
# Observations	215	110	105	78	320	220	115	440
Mean of Δ SSDI	0.1415	0.1105	0.1740	0.0961	0.1323	0.1085	0.1747	0.1198
Mean of Δ OA PV	-0.520	-0.433	-0.612	-0.196	-0.397	-0.307	-0.559	-0.289
R-squared	0.533	0.607	0.454	0.258	0.431	0.371	0.412	0.358

The dependent variable is equal to the fraction of individuals on SSDI in an age-year cell. The unit of observation is age*year. The ages included in each year are listed in Table 2. The variable Δ PVR is equal to the change in the present value of retired worker benefits in an age cell from one year to the next. All first difference regressions include 22 year effects and 20 age effects. Standard errors are included in parentheses and are clustered by year-of-birth. Δ SSDI is measured in percentage points and Δ PVR is measured in thousands of 1999 dollars.

Table 2.5: Implied Effect of Change in Present Value of Retired Worker Benefits on SSDI Enrollment

Dec-05		Men						Women					
Age	YOB	PV Δ	FRA Impact	Actual Δ	Long-Run	PV Δ	FRA Impact	Actual Δ	Long-Run	PV Δ	FRA Impact	Actual Δ	Long-Run
45	1960	\$9,432	0.73%	1.55%	0.73%	\$7,104	1.16%	2.21%	1.16%	\$7,104	1.16%	2.21%	1.16%
46	1959	\$8,940	0.70%	1.69%	0.76%	\$6,722	1.10%	2.42%	1.20%	\$6,722	1.10%	2.42%	1.20%
47	1958	\$8,406	0.65%	1.82%	0.79%	\$6,310	1.03%	2.53%	1.24%	\$6,310	1.03%	2.53%	1.24%
48	1957	\$7,828	0.61%	1.68%	0.81%	\$5,865	0.96%	2.60%	1.28%	\$5,865	0.96%	2.60%	1.28%
49	1956	\$7,202	0.56%	1.87%	0.84%	\$5,385	0.88%	2.81%	1.32%	\$5,385	0.88%	2.81%	1.32%
50	1955	\$6,525	0.51%	1.84%	0.87%	\$4,868	0.80%	2.93%	1.37%	\$4,868	0.80%	2.93%	1.37%
51	1954	\$5,794	0.45%	1.58%	0.90%	\$4,311	0.71%	3.04%	1.41%	\$4,311	0.71%	3.04%	1.41%
52	1953	\$6,004	0.47%	1.73%	0.93%	\$4,456	0.73%	3.20%	1.46%	\$4,456	0.73%	3.20%	1.46%
53	1952	\$6,225	0.48%	1.87%	0.97%	\$4,607	0.76%	3.45%	1.51%	\$4,607	0.76%	3.45%	1.51%
54	1951	\$6,457	0.50%	2.25%	1.01%	\$4,765	0.78%	3.63%	1.56%	\$4,765	0.78%	3.63%	1.56%
55	1950	\$6,701	0.52%	1.77%	1.04%	\$4,930	0.81%	3.54%	1.62%	\$4,930	0.81%	3.54%	1.62%
56	1949	\$6,959	0.54%	1.87%	1.08%	\$5,103	0.84%	3.78%	1.67%	\$5,103	0.84%	3.78%	1.67%
57	1948	\$7,231	0.56%	2.01%	1.13%	\$5,284	0.87%	4.14%	1.73%	\$5,284	0.87%	4.14%	1.73%
58	1947	\$7,519	0.59%	1.54%	1.17%	\$5,475	0.90%	3.99%	1.79%	\$5,475	0.90%	3.99%	1.79%
59	1946	\$7,826	0.61%	1.54%	1.22%	\$5,675	0.93%	4.26%	1.86%	\$5,675	0.93%	4.26%	1.86%
60	1945	\$8,153	0.63%	1.47%	1.27%	\$5,887	0.96%	4.44%	1.93%	\$5,887	0.96%	4.44%	1.93%
61	1944	\$8,503	0.66%	1.29%	1.32%	\$6,111	1.00%	4.60%	2.00%	\$6,111	1.00%	4.60%	2.00%
62	1943	\$8,878	0.69%	0.90%	1.38%	\$6,348	1.04%	4.61%	2.08%	\$6,348	1.04%	4.61%	2.08%
63	1942	\$7,181	0.56%	0.84%	1.34%	\$5,150	0.84%	4.35%	2.03%	\$5,150	0.84%	4.35%	2.03%
64	1941	\$5,570	0.43%	0.56%	1.30%	\$4,006	0.66%	4.23%	1.97%	\$4,006	0.66%	4.23%	1.97%
All 45-64	1941-60	\$7,391	0.58%	1.64%	1.00%	\$5,448	0.89%	3.41%	1.56%	\$5,448	0.89%	3.41%	1.56%

Table summarizes the estimated effect of changes in Social Security retired worker benefits on the fraction of individuals receiving SSDI benefits by December of 2005. PV D columns lists the average change in the present value of retired worker benefits by age for men and women in 2005. The FRA Impact columns represent the estimated effect of this change on the fraction receiving SSDI, using the parameter estimates for men and women from specifications (1) and (5), respectively, in Table 3. Actual Δ columns list the actual change in age-specific SSDI enrollment, and Long-Run columns list out estimates of the effect once the policy change is fully phased in (i.e. individuals born in 1960 or later at each age).

Chapter 3

Federal Policy and the Rise in Disability Enrollment: Evidence for the VA's Disability Compensation Program (with Mark Duggan and Robert Rosenheck)

Abstract: The US Department of Veterans' Affairs (VA) currently provides disability benefits to 2.72 million veterans of US military service through the Disability Compensation (DC) program. Until recently, the medical eligibility criteria for this program were the same across service eras, with the key condition being that the disability was caused or aggravated by military service. But in July of 2001, the VA relaxed the eligibility criteria for Vietnam veterans by including diabetes in the list of conditions covered by DC. This change was motivated by an Institute of Medicine report which linked exposure to Agent Orange and other herbicides to onset of diabetes. In this paper, we investigate the impact of this policy change on

DC enrollment and expenditures. Our findings demonstrate that the Agent Orange decision increased DC enrollment by 7.6 percentage points among Vietnam veterans and that an additional 2.5 percent enjoyed an increase in their DC benefits. Our estimates further suggest that the policy change increased program expenditures by \$2.69 billion during the 2006 fiscal year and by \$45 billion in present value terms. Taken together, our results suggest that even a relatively narrow change in the medical eligibility criteria for federal disability programs can have a powerful effect on program enrollment and expenditures

3.1 Introduction

In August of 2006, the U.S. Department of Veterans' Affairs (VA) provided cash benefits to more than 11 percent of the nation's 24 million military veterans through the Disability Compensation (DC) program. Total program enrollment in that month was 2.72 million and expenditures for the 2006 fiscal year were approximately \$25 billion. To qualify for DC benefits, a veteran must have one or more disabilities that were caused or aggravated by his military service. The DC recipient then receives a monthly benefit along with essentially free medical care for the treatment of their disabilities through the Veterans Health Administration.

Until recently, the medical eligibility criteria for DC benefits have been essentially the same for veterans from all service eras. The key requirement was that a disability must have been caused or aggravated by military service. Thus individuals rarely qualified for DC because of conditions such as cancer and diabetes

that first affected people long after their period of military service and for which service-connectedness would be difficult to prove. However in October of 2000 the National Institute of Medicine issued a report that linked exposure to Agent Orange, an herbicide used by the U.S. military in Vietnam, to the onset of diabetes. In July of 2001, the VA responded to this report by adding diabetes to the list of conditions for which a veteran who served in Vietnam during the war could qualify for DC benefits. There was no corresponding change for veterans from other eras.¹

In this paper we aim to estimate the impact of this policy change on DC enrollment and expenditures. Many previous authors have investigated these same types of issues Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI), the federal government's two other major disability programs (Autor and Duggan, 2003; Black, Daniel, and Sanders, 2002). However, virtually no previous work has investigated the causes or consequences of DC enrollment.²

As Figure 3.1 demonstrates, this policy change coincided with a sharp break in trend in DC enrollment. From 1996 to 2001, the number of DC beneficiaries grew by less than 0.6 percent per year. But during the next five years, the annual growth rate was five times greater at 3.3 percent. Of course, other factors may have been at least partly responsible for this break in trend. We therefore use veterans from peacetime eras, almost all of whom served shortly before or after the Vietnam era,

¹Korean War veterans would also become eligible for DC benefits for diabetes, but their diabetes would not be "presumptively" related to their military service.

²In Bound and Burkhauser's 1999 Handbook of Labor Economics chapter on disability programs, 44 papers examine SSDI, 17 consider SSI, and just 1 studies DC. The one that considers DC is a descriptive paper that compares the economic well-being of individuals who receive SSDI, SSI, DC, or Workers' Compensation benefits (Burkhauser and Daly, 1999) in the U.S. with those who receive disability benefits in Germany.

as our comparison group to estimate the effect of the policy. While this group of veterans is clearly not a perfect control group, they had mortality rates and trends in DC enrollment that were quite similar to those for Vietnam-era veterans prior to the policy change as shown in Figure 3.2.

Using aggregate data by service era in each year, our differences-in-differences estimates suggest that the expansion of the DC program's eligibility criteria increased the number of Vietnam veterans on the program in September of 2006 by 175,000 over what it would otherwise have been. This increase represents 2.3 percent of all Vietnam-era veterans and 7.6 percent of those who actually served in Vietnam during the conflict there, as the policy change applied only to this latter group.

An additional possible effect of the Agent Orange decision was that Vietnam veterans already on the program could increase their monthly benefits if they were found to have diabetes. The DC program pays benefits that are an increasing function of the recipient's combined disability rating (CDR). The CDR depends on the ratings for all of a recipient's rated disabilities, and thus a recipient who could obtain a rating for another condition would typically experience an increase in monthly benefits. Our results suggest that approximately 58,000 Vietnam veterans qualified for an increase in their benefits because of the 2001 policy change. Combined with the effect on enrollment, this suggests that 10.1 percent of the veterans who served in Vietnam and were still alive in 2006 experienced an increase in their DC benefits or became eligible for the program because of the less stringent medical eligibility criteria.

We next investigate the effect of the change in the DC program's medical eligibility criteria on short and long-term expenditures for the program. To do this we estimate the impact of the policy change on the number of Vietnam-era DC recipients with each of the eleven possible CDRs, and then multiply this by average monthly benefits within each CDR. This algorithm captures the effect due to the increase in the number of recipients as well as the increase in benefits for some existing DC recipients. Our estimates suggest that DC expenditures during the 2006 fiscal year were \$2.69 billion higher than they would have been in the absence of the Agent Orange decision. Aggregating the effect across all years, our estimates suggest that the present value of Disability Compensation spending increased by more than \$45 billion as a result of the policy change.

Finally, we examine the effect of the Agent Orange decision on outcomes associated with an increase in DC benefit receipt; labor force status, income from various sources, health status, and health insurance status. The data come from the Current Population Survey March Supplements for years 1998 through 2006. Using a difference-in-differences type model, we first estimate the impact of the Agent Orange decision DC receipt. Then, using a similar model, we estimate the reduced-form effects of the policy change.

Using veterans who did not serve during the Vietnam Era as a comparison group, we estimate that the Agent Orange decision increased DC receipt by 2.3% points among Vietnam Era veterans - the precise estimate derived using aggregate administrative data. Additionally, we find no effect of the Agent Orange decision on VA pension receipt among Vietnam Era veterans, suggesting that the differential

rise in DC receipt among Vietnam Era veterans is indeed a result of the policy change.

We then estimate the effect of the Agent Orange decision on outcomes associated with DC receipt. First, we find little to no differential change in labor force participation among Vietnam Era veterans. However, we do estimate a differential 3.0% increase in the probability of reporting zero work hours in the previous week and 2.1% point decline in the probability of having positive income from earnings among Vietnam Era veterans after the policy change. We also find a decline in the prevalence of health insurance through one's employer and a comparable increase in the prevalence of health insurance through the VA among Vietnam Era veterans. Thus, it appears that a rise in DC receipt, and subsequent eligibility for VA health care, may crowd out the private insurance market.

Taken together, our findings for the VA's Disability Compensation program suggest that changes in the medical eligibility criteria for disability programs can have an important impact on program enrollment and expenditures. These findings are consistent with the results from recent research on other federal disability programs such as SSDI (Autor and Duggan, 2003). But the main contribution of our study relative to work for other disability programs is that, because the change to DC applied only to Vietnam veterans, we can use other veterans as a comparison group to obtain a more reliable estimate of the policy impact. Changes to the SSDI and SSI programs have applied equally to essentially all potential applicants of those programs, and it has therefore been difficult to disentangle the effect of changes to these programs from the effect of other factors such as macroeconomic conditions.

3.2 The Department of Veterans Affairs and the Disability Compensation Program

The U.S. Department of Veterans Affairs (VA) provides benefits to veterans of military service and their families. At the end of the 2006 fiscal year, the VA estimated that there were 24 million veterans residing in the U.S. and that an additional 45 million were potentially eligible for VA benefits as family members or survivors of veterans. According to VA estimates, the number of living veterans fell by almost 10 percent from September of 2000 to September of 2006.³

As Table 3.1 demonstrates, this change in the veteran population has been associated with a substantial change in its composition, both because of mortality among veterans from earlier eras and because of entry by those serving during the Gulf War era. Most strikingly, the number of veterans from the World War II era declined by 44 percent (from 5.59 to 3.15 million) during this six-year period while the number of veterans from the Gulf War era increased by 51 percent from 2.84 to 4.30 million. Veterans from the Vietnam era were the largest group in both years, with their ranks declining 8.01 to 7.63 million but their share of all veterans increasing from 30 to 32 percent.⁴

³The data on dependents and population were obtained from the VA's website at http://www.va.gov/about_va/ and <http://www.va.gov/vetdata/demographics/Vetpop2004/VP2004B.htm>, respectively.

⁴According to the VBA's 2004 Annual Benefits Report, the approximate service dates by era were: World War II (September 1940 - July 1947), Korea (June 1950 - January 1955), Vietnam (August 1964 - May 1975), and Gulf War (August 1990 - present). Peacetime includes veterans who served during all other periods. These population estimates assign veterans who served in two or more eras to their earliest era of service. However a veteran who served during a conflict and during peacetime would always be assigned to the conflict.

It is worth noting that, while veterans are categorized based on their period of service, the number serving in specific conflicts tend to be much lower than the era-specific population data would suggest. For example, a recent estimate by the VA suggests that just 2.3 million of the 7.7 million Vietnam-era veterans alive in 2005 actually served in Vietnam.

3.2.1 VA Programs and Expenditures

Despite the significant decline in the veteran population since 2000, total VA expenditures have increased by an average of 6 percent per year during the same period. Table 3.2 lists total VA spending by category for the 1998 through 2005 fiscal years. As Table 3.2 shows, Compensation and Pension (CP) was the largest category of spending throughout this period, with the \$32.1 billion in CP program benefits representing 46 percent of total spending by the VA during the 2005 fiscal year. The Veterans Health Administration (VHA) provided medical care to 4.96 million patients during this same year at a cost of \$30.7 billion. The remaining \$7.5 billion in VA spending was spread across several categories, including operating expenses, construction, insurance, housing assistance, vocational rehabilitation, training programs, and burial and memorial benefits.

CP benefits are paid through four main programs. The largest in terms of both enrollment and expenditures is the Disability Compensation program, which according to the data displayed in Tables 3.3 and 3.4, accounted for 75 percent of CP enrollment and 74 percent of expenditures, respectively, during the 2005 fiscal

year. The DC program pays benefits to disabled veterans of military service whose conditions were caused or aggravated by their military service. The program is not means-tested and an individual's DC benefits are not directly affected by his earnings. This is in contrast to the SSDI and SSI programs, which substantially reduce recipients' incentives to work.

The Disability Pension (DP) program is targeted at low-income wartime veterans who either are age 65 or older or are permanently and totally disabled (regardless of service-connectedness). This program accounted for just 10 percent of CP enrollment and 9 percent of CP spending during the 2005 fiscal year. Beneficiaries of both the DC and DP programs are eligible for health care through the VHA and their expenditures accounted for much of the \$30.7 billion in VHA spending during this same year.⁵ The VA also paid \$4.5 billion in cash benefits to the survivors of 0.54 million deceased veterans through the Death Compensation and Death Pension programs.

3.2.2 The Disability Compensation Program

To apply for Disability Compensation benefits, a veteran must submit an application at one of 63 regional offices of the Veterans Benefit Administration (VBA). At this stage, the 'authorization unit' collects necessary information regarding the claimant's application. These would include military service records and medical records from both VA medical facilities and private providers. The application is

⁵The VHA provides care to other veterans as well, with 4.96 million served by the VHA during the 2004 fiscal year.

then forwarded to a Rating Board, which determines whether each disability for which an application is submitted is service connected and assigns an appropriate degree of disability according to the Schedule for Rating Disabilities. These ratings range from 0 to 100 percent (in 10 percent increments) depending on the type and severity of the disability, with more severe conditions receiving a higher rating.⁶ The recipient's combined disability rating (CDR) is a function of the individual ratings. If the award is made for just one condition then the CDR is equal to the rating for that condition. If the award is made for multiple conditions then the CDR is generally greater than any of the individual ratings, though the CDR is not simply a sum of the remaining ratings.⁷

If a DC award is made, the CDR is used to determine the monthly cash benefit amount, which is an increasing function of this rating.⁸ The monthly benefit can increase beyond this base for DC recipients with a CDR of 30 percent or more and who have dependent spouses, children, or parents. The benefit can also increase for those with ratings of 60 percent or more and who are deemed unemployable. The second column of Table 3.5 lists the baseline monthly benefit amounts for the 2006 fiscal year by disability rating. As the table shows, benefit amounts increase with the CDR and the dollar increment from one category to the next also increases with

⁶The possible ratings depend on the disability. For example type II diabetes can have ratings of 10, 20, 40, 60, or 100 percent, whereas arthritis can only be assigned a rating of 10 or 20 percent. For a list of conditions and possible ratings see <http://www.warms.vba.va.gov/bookc.html>.

⁷If a claimant has multiple disabilities, only the claimant's residual ability is considered when determining the impact of the next disability considered. For example, if a veteran has two disabilities rated at 50%, then only 50% of his ability is considered when determining the impact of the second disability. Therefore, his CDR would be 80%; the sum of 50% for the first and 25% for the second (.5*(1-.5)) rounded to the nearest increment of 10%.

⁸The VBA considers the average reduction in earnings capacity to determine the benefit amount associated with each value of the CDR.

the CDR. For example the monthly payment rates for ratings of 10, 40, 70, and 100 percent are \$112, \$485, \$1099, and \$2393, respectively. The next three columns in the table show the adjustments to these baseline amounts if the veteran has one or more dependents.

The first three columns of Table 3.5 list the total number of recipients, the total benefits paid, and the average monthly benefit in each CDR category in June of 2006 (the most recent month available). The average payment to the 2.70 million DC recipients in that month was \$780 for a total of \$2.11 billion in cash benefits. Those with ratings between 0 and 20 percent accounted for 45 percent of recipients but just 9 percent of dollars paid. The corresponding shares for those with ratings at or above 70 percent were 21 and 62 percent, respectively.⁹

As the next several columns of this table demonstrate, there was considerable variation across service eras in the distribution of the combined disability rating.¹⁰ For example, among Vietnam era DC recipients, 32 percent had CDRs of 70 percent or more. The corresponding share for DC recipients who incurred their disabilities during the Gulf War era was just 13 percent. Because of this, average monthly benefits also varied widely by service era, from a low of \$578 for Gulf War veterans to a high of \$1029 for veterans serving in Vietnam.

⁹The average amounts for those rated 60 percent and higher are much greater than the baseline amounts because many of these recipients are eligible for the 100 percent payment amount because they are deemed unemployable.

¹⁰DC recipients are assigned to eras based on where their most significant disability occurred. This will introduce some measurement error in our estimates for era-specific DC enrollment rates because the population data are constructed differently, though the importance of this should not change significantly over short periods of time.

3.2.3 The DC Program's Medical Eligibility Criteria

In a typical year more than 70 percent of DC applicants apply for benefits for more than one condition. One of three outcomes is possible - outright rejection, an award for some but not all conditions, or an award for all conditions. During the 2000 fiscal year, 14 percent of applications considered were awarded for all conditions, 48 percent for some conditions, and 38 percent were outright rejected (VBA, 2001).¹¹ During the course of the year a total of 83,159 DC awards were made, with the average number of rated conditions among new recipients equal to 3.2.

Until July of 2001, the medical eligibility criteria for DC benefits were essentially the same across service eras. Any veteran who was honorably discharged and whose disability did not result from his willful misconduct could qualify for DC benefits if his disability "was a result of disease or injury incurred or aggravated during active military service." Many conditions would clearly have resulted from military service. For example if a soldier lost one or more limbs during a battle then there would be no uncertainty about whether the injury was service-connected. The same would also be true for scars, the most commonly compensated condition among DC recipients. The existence of such a causal link for the typical tinnitus (persistent ringing in the ears) or post-traumatic stress disorder application might be somewhat less clear cut, though still certainly plausible.¹²

¹¹These decisions are frequently appealed. Existing DC recipients can also apply for an increase in their benefit amount, either because of an increase in the severity of a rated condition or because a new health problem arises.

¹²A listing of the top twenty impairments at the end of each fiscal year can be found in the VBA's annual report. In September of 2000 more than 12 percent of DC recipients had "scars, other" as one of their qualifying conditions. Next most common were skeletal conditions (10.6 percent), knee impairment (9.8 percent), and arthritis due to trauma (8.0 percent). Tinnitus (6.2 percent) and PTSD (5.8 percent) were the 6th and 9th most common conditions.

Proving that such a link existed for a condition such as diabetes would undoubtedly be much more difficult. Indeed a 2000 report by the National Academy of Sciences argued that the most important determinants of diabetes were physical inactivity, family history, and obesity. The report further argued that any increased risk posed by wartime exposure to herbicides appeared to be small or nonexistent (NAS, 2000). Despite this, approximately 1.6 percent of DC recipients had diabetes as one of their covered conditions in September of 2000. While this share is not trivial, diabetes was not one of the twenty most common conditions among DC recipients at that time, nor was it one of the ten most common conditions for DC recipients from any of the five major service eras.

3.3 The Institute of Medicine Report on Agent Orange Exposure

While the DC program's medical eligibility criteria were essentially the same for all military veterans up until July of 2001, the types of disabilities incurred undoubtedly varied across service eras. For example, one might expect a significantly higher rate of service-connected post traumatic stress disorder (PTSD) among DC recipients who served in the Vietnam War than among their counterparts from other eras given the intensity of the conflict there. And this was indeed the case, with 13.3 percent of DC recipients from the Vietnam era receiving compensation for PTSD in September of 2000 versus just 2.2 percent of all other DC recipients (VBA, 2001).

Another reason that the disabilities incurred might vary across service areas is that different weapons and techniques were used by the U.S. military and their opponents over time. One notable example of this is the use of herbicides in the Vietnam War. Agent Orange was one of fifteen herbicides used by the U.S. military to defoliate trees that might otherwise provide cover to opposing forces. Although the use of Agent Orange did not begin until 1965, it represented more than 80 percent of the 19 million gallons of herbicides sprayed in Vietnam (VA, 2003).

Soon after the war ended, many Vietnam veterans voiced concerns about the possible long term effects of exposure to Agent Orange and other herbicides used in Vietnam. In response to these concerns, the VA established the Agent Orange Registry in 1978, which provided voluntary medical examinations to veterans who served in Vietnam between 1962 and 1975. Thirteen years later, the Agent Orange Act of 1991 was enacted, which charged the National Academy of Sciences' Institute of Medicine with conducting a review of the existing scientific literature regarding the possibility of a link between Agent Orange exposure and the prevalence of certain medical conditions.

In a series of five reports released between 1994 and 2003, the Institute of Medicine (IOM) grouped forty different medical conditions into one of four categories - (1) sufficient evidence of an association between Agent Orange and the condition (2) limited or suggestive evidence of an association (3) inadequate or insufficient evidence and (4) limited or suggestive evidence of no association. Interestingly, none of the four categories required causal evidence. In the first three reports, diabetes was placed in the third category, with the IOM concluding that there was

insufficient evidence to establish an association between dioxin exposure and the onset of diabetes.

But soon after the third IOM report was released in 1999, two new studies were released that provided supporting evidence of an association between dioxin exposure and diabetes (Calvert et. al., 1999; AFHS, 2000). In October of 2000, the IOM evaluated the new studies in the context of previous research and concluded that there was suggestive evidence of an association between Agent Orange exposure and the onset of diabetes (IOM, 2000). This moved diabetes from category three to category two. In response to this report, the Secretary of the VA announced in November of 2000 that type II diabetes would be compensable under the DC program and, more importantly, that diabetes would be "presumptively" service-connected among those veterans who served in Vietnam.¹³ Diabetes would not become compensable, however, until July of 2001.

Shortly after this policy change, the growth in total DC enrollment increased substantially, as shown in Figure 3.1. From September of 1996 to 2001, the number of DC recipients increased by just 0.6 percent per year. But during the next five years the annual growth rate was 3.3 percent, suggesting that the expansion in the eligibility criteria for Vietnam veterans was the main cause.

However this was not the only possible explanation for the increase in the rate of DC enrollment growth. For example, the Veterans Claim Assistance Act, which was enacted in 2000, required the VA to provide more assistance to DC applicants

¹³No such presumption was made for other Vietnam-era veterans, with the exception of those who served in Korea in either 1968 or 1969 because the U.S. military used herbicides there during this period as well (VA, 2005).

from all eras and to add more resources to the processing of DC applications (GAO, 2002). Similarly the economic downturn and the corresponding increase in the unemployment rate may have caused some veterans to seek out alternative sources of income. And finally, the wars in Afghanistan and Iraq may have led to a significant increase in the prevalence of disabilities among veterans serving in these conflicts. In the next two sections we estimate the impact of the Agent Orange decision on DC enrollment and expenditures while controlling for these and other potentially confounding factors.

3.4 The Effect of the Agent Orange Decision on Enrollment in the DC Program

Theoretically, one would expect the Agent Orange decision to have increased the propensity of veterans who served in Vietnam to apply for DC benefits. As Parsons (1980), Bound (1989), and others have noted, a key determinant of an individual's decision to apply for disability benefits is the probability that an award is made. It seems likely that this award probability increased following the July 2001 policy change, especially for Vietnam veterans who knew they had diabetes. But it may also have increased the incentive for other Vietnam veterans. For example, a veteran who thought there was some chance that he had diabetes might go for a medical checkup.¹⁴ This medical checkup could identify other health problems, and thus he

¹⁴According to a CDC report (2003), approximately one third of diabetics in US are undiagnosed. See Singleton (2006) for an analysis of self-reported rates of diagnosed diabetes among veterans and non-veterans in response to the Agent Orange decision.

could subsequently qualify for the DC program even if he did not have diabetes.

The policy change would also have increased the incentive for existing DC recipients who served in Vietnam to apply for an increase in their monthly benefit. As described above, a veteran's DC benefit is a function of the combined disability rating (CDR), which generally increases when an additional condition is rated at 10 percent or more. Thus the Agent Orange decision could have increased both DC enrollment and the amount of benefits paid to existing DC recipients.

3.4.1 Differences-in-Differences Estimates of the Impact on DC Enrollment

In its publication titled Annual Benefits Report, the Veterans Benefit Administration provides detailed information each year on the characteristics of individuals receiving DC benefits at the end of the previous fiscal year. This information includes the number of DC recipients with certain diagnoses, the number with each of the eleven possible combined disability ratings (0 to 100 percent), the average monthly benefit received, and many other variables of interest. This data is further broken down by service era, which can be used to estimate the impact of the policy change described above on enrollment in the DC program.

Because the Agent Orange decision differentially affected veterans who served in the Vietnam War, one can essentially use veterans from other eras to control for other changes occurring at the same time that might also have affected DC enrollment. For example, the Veterans Claim Assistance Act that was passed in

the year 2000 influenced the DC application and award process for veterans from all eras (GAO, 2002). To the extent that this policy, the economic downturn, and other factors did not have a different effect on Vietnam veterans than on veterans from other eras, their effects could be captured by the time effects t in the following differences-in-differences model:

$$DC_{jt} = \beta_0 + \beta_1 X_{jt} + \beta_2 Vietnam_j + \beta_3 (Vietnam_j)(Post_t) + \sum_{t=\tau_1}^{\tau_2} \theta_t + \epsilon_{jt} \quad (3.1)$$

In this model, the outcome variable DC_{jt} , is equal to one if individual j received DC benefits in year t and zero otherwise. The variable $Vietnam_j$ is equal to one if individual j is a Vietnam era veteran and zero otherwise. $POST_t$ is set equal to one after the policy takes effect, though to the extent that the impact is not immediate it may be more appropriate to allow the policy's impact to vary over time.¹⁵ The parameter of particular interest in this model is 3, which is the coefficient on the interaction between the $Vietnam_j$ and $POST_t$ variables and represents the impact of the policy change on the probability of DC enrollment among Vietnam era veterans. The key assumption for reliable estimation of 3 is that there are not unobserved factors that influence DC enrollment differentially for Vietnam era veterans following the policy change.

Ideally when estimating a differences-in-differences model such as this one, the treatment and comparison groups would be identical on background characteristics

¹⁵Note that the inclusion of year indicators in this model makes it unnecessary to add a $POST$ variable separately, as this would then be a linear function of certain year indicators.

such as age, education, and other possible determinants of DC enrollment. Of course, veterans who served in Vietnam will differ from other veterans in many respects. For example, they are older on average than Gulf War veterans and younger than veterans who served in Korea. But to the extent that the enrollment effect of these differences does not change at the time of the policy change, it would be captured by era-specific fixed effects and era-specific time trends.

3.4.2 Choosing the Comparison Group

The data summarized in Table 3.7 lists the number of veterans receiving DC benefits by service era in September of each year from 1998 to 2006. This table also lists the percentage change in this number from the previous year, the number of veterans in each service era, and the fraction of veterans receiving DC benefits. Before considering the effect of the 2001 policy change, three points are worth noting from this table. First, the number of DC recipients who served in World War II is declining steadily throughout this time period because of the high mortality rate among this group. Second, the number of DC recipients who served in the Gulf War era increased rapidly throughout this period. While this has largely been driven by an increase in the number of Gulf War era veterans (those serving since August 1990), the increase in the fraction receiving benefits from 9.6 to 16.2 percent has been nearly as important. And finally, the trends from 1997 to 2000 in DC enrollment are fairly similar for the other three service eras. During this period, DC enrollment increased by an average of 0.8 and 1.7 percent per year, respectively, among Vietnam

and peacetime era veterans, while declining by an average of 2.2 percent per year among those serving in the Korea conflict.

Given these trends, it seems clear that veterans from either the World War II or Gulf War eras would not be an appropriate comparison group for estimating the effect of the 2001 policy change on DC enrollment. Which of the other two eras is more appropriate is not as obvious. On the one hand, just prior to the Agent Orange decision, peacetime and Vietnam era veterans had similar rates of DC enrollment at 8.6 and 9.3 percent, respectively. The corresponding rate among Korean War era veterans was much lower at 4.9 percent. And in terms of average age, those classified as peacetime were much more similar to Vietnam era veterans because most served either shortly before or after the Vietnam War.¹⁶ But on the other hand, many veterans from the Korean and Vietnam War eras incurred their disabilities in a military conflict and thus their service-connected disabilities may be more similar. But this seems unlikely to be as important as the age and DC enrollment similarities, and we therefore use individuals who served in peacetime as our comparison group.

¹⁶According to data from the VA, the average ages of Korea War, Vietnam War, and peacetime era veterans in September of 2002 were 72, 57, and 53, respectively. Table 3.1 demonstrates that veterans who served between the Korean and Vietnam War eras accounted for 53 percent of the peacetime era veteran population in September of 2000, with those serving after Vietnam but before the Gulf War era accounting for an additional 44 percent.

3.4.3 The Impact of the Agent Orange Decision on DC Enrollment Rates

Figure 3.2 displays the fraction of Vietnam and peacetime era veterans receiving DC benefits in September of each year from 1998 through 2006.¹⁷ As is clear from the figure, the trends for the two groups were fairly similar from 1998 to 2001, with the rate of enrollment increasing from 9.0 to 9.4 percent among Vietnam veterans and from 8.1 to 8.7 percent among peacetime veterans.¹⁸ The trend for the peacetime group was quite similar during the next five years, with 9.7 percent of peacetime veterans receiving DC benefits by the end of the 2006 fiscal year. But the 3.0 percentage point increase in DC enrollment among Vietnam era veterans was exactly three times as large during this same five-year period, with their enrollment rising from 9.4 to 12.4 percent. Our differences-in-differences estimate of the effect of the Agent Orange decision on the change in DC enrollment from September of 2001 to September of 2006 is therefore 2.0 percent.¹⁹

Our baseline estimate of 2.0 percentage points does not account for the fact

¹⁷As described above, the assignment of DC recipients to eras (the numerator) differs somewhat from the assignment of veterans to eras for population estimates (the denominator). For example, to be counted as peacetime in the population data a veteran must have served only in peacetime. To be assigned to peacetime as a DC recipient the veteran must have incurred his most severe disability during peacetime. This will introduce measurement error in our estimated enrollment rates. But as long as the impact of this has a smooth trend over time, it should not bias this comparison or the results that follow.

¹⁸The trends are similar through 2001 as well, though as our diagnosis data below demonstrates, the policy change had already started to have an effect by the end of 2001 and thus we consider it as post policy here.

¹⁹This estimate and the one in the next paragraph would be almost identical if we instead used 2000 as the baseline. One possible source of bias in this estimate is that DC recipients who served both in peacetime and in Vietnam could, after qualifying from diabetes, switch from being classified as peacetime era to being classified as Vietnam era. While there is no way to rule out this possibility, the fact that the trend in DC enrollment for peacetime era veterans did not change significantly after 2001 suggests that it is not an important source of bias.

that the pre-existing trends in DC enrollment were slightly different for veterans from the Vietnam and peacetime eras. We next account for this by assuming that the average annual increase for each group from 1998 to 2001 continued for the next five years. Given this assumption, the predicted rates of DC enrollment in September of 2006 for peacetime and Vietnam era veterans were 9.6 and 10.1 percent, respectively. The actual rate of 9.7 percent for our control group was almost identical to their predicted rate. But the same was not true for veterans from the Vietnam era, whose actual DC enrollment of 12.4 percent was 2.3 percentage points higher in 2006 than predicted.

Given that there were approximately 7.6 million Vietnam-era veterans alive in September of 2006, this latter estimate suggests that the Agent Orange decision increased DC enrollment by 175,000 above what it otherwise would have been by September of 2006. But this decision applied only to the 2.3 million veterans who served in Vietnam. Thus the expanded eligibility criteria induced a 7.6 percentage point increase in DC enrollment among those veterans who actually served in Vietnam. Furthermore, this increase can explain more than 53 percent of the acceleration in overall Disability Compensation enrollment since September of 2001 that is apparent in Figure 3.2.²⁰

²⁰The number of DC recipients increased at a 0.64 percent annual rate from 1998 to 2001. Had this growth continued during the next five years, the number of DC recipients would have been 329,044 lower in September of 2006. Thus the induced increase of 175,000 accounts for more than 53 percent of this. Essentially all of the remaining acceleration is attributable to the growing importance of entry by Gulf War veterans and the declining importance of exits by World War II DC recipients.

3.5 The Effect on Existing DC Recipients and on Program Expenditures

The results in the previous section estimated the effect of the Agent Orange decision on the number of veterans receiving DC benefits but this did not include any resulting increase in benefits for existing DC recipients. In this section, we estimate this latter effect by using aggregate data from the VBA's Annual Benefits Report on the diagnoses of new and existing DC recipients in each year. We then investigate the effect of the policy change on total DC expenditures, which incorporates both the benefits paid to new recipients and the increase in benefits for existing recipients.

3.5.1 The Number of Vietnam Veterans Experiencing an Increase in DC Benefits

The top two rows of Table 7 list the number and percentage, respectively, of DC recipients receiving compensation for diabetes in each year. At the end of the 2000 fiscal year, just 1.6 percent of DC recipients were paid for this condition, with this fraction unchanged from the previous year. But in the years following the 2001 policy change, this percentage increased consistently, reaching a peak of 8.4 percent by the end of 2005 (the most recent year of this VBA data). This increase was driven almost entirely by Vietnam era DC recipients, with 20.8 percent of them receiving compensation for diabetes in September of 2005 versus just 1.8 percent of all other

DC recipients.²¹ Just three years after the policy change, diabetes had become the most frequently compensated condition among Vietnam era DC recipients after not being in the top ten in September of 2001.

The number of Vietnam-era DC recipients compensated for diabetes increased from 18,993 in September of 2000 to 190,199 by September of 2005, a difference of 171,206 cases. This increase reflects the coverage of diabetes among both new and existing DC recipients, though the VBA does not report how many of the new diabetes cases were already receiving DC at the time of the policy change. To estimate this, we first calculate how many new DC recipients would have been covered for diabetes by September of 2005 if the number of new diabetes awards in each year had remained at its 2000 level.²² Using the data listed in Appendix Table 2, we estimate that the total number of new awards for diabetes after the Agent Orange decision would have been lower by 122,796 during this period. If one makes the conservative assumption that none of these awardees would have exited the program by the end of 2005, then an additional 48,410 individuals who were receiving DC benefits at the time of the 2001 Agent Orange decision enjoyed an increase in their benefits by September of 2005 because their diabetes was covered.²³

However, this estimate excludes the number enjoying an increase in benefits during

²¹Values in this table with an asterisk were imputed. See the notes to Appendix Tables E.1 and E.2 for a description of our imputation procedure.

²²There were 125,756 new diabetes awards from 2001 to 2005 versus the 2,960 (= 592 * 5) that we estimate would otherwise have been made. This is lower than the increase in the number of Vietnam-era DC recipients by September of 2005, perhaps because many of the applications had diabetes rejected but other conditions accepted. The most common outcome of a DC application is to have one or more conditions accepted and others rejected.

²³To calculate this, we subtract the increase in the number of Vietnam era veterans with diabetes from 2000 to 2005 (171,206) from the increase in the number of new DC recipients with diabetes as a covered condition (122,814).

the 2006 fiscal year, and we therefore factor this estimate by 1.2 to arrive at our estimate of 58,092 for the number of existing DC recipients who enjoyed an increase in their monthly benefits because of the Agent Orange decision.²⁴

When combined with the results from the previous section, our estimates suggest that approximately 233,000 Vietnam veterans enrolled in DC or enjoyed an increase in their DC benefits as a result of the Agent Orange decision by 2006. This represents approximately 10.1 percent of the 2.3 million veterans who served in Vietnam and were still living in September of 2006.

3.5.2 The Impact on Short and Long-Term Disability Compensation Expenditures

The effect of the Agent Orange decision on Disability Compensation expenditures depends on the characteristics of both those newly awarded DC benefits and of their counterparts already on the program who enjoyed an increase in their DC benefits. The main determinant of the short-term increase in spending is the CDR of new recipients and the increase in the CDR for existing recipients. If the 175,000 Vietnam veterans awarded benefits all had a CDR of just 10 percent, for example, then the effect on spending would be relatively modest. The same would be true if the 58,000 enjoying an increase in their benefits rose from a CDR of 10 to just 20 percent.

²⁴Some of those applying for an increase in benefits may have applied for multiple conditions or for an increase in ratings for existing conditions. Even if their diabetes applications were turned down, some recipients may have enjoyed an increase in benefits because of the application. This is one reason that our estimate may be too low.

To estimate the effect of the Agent Orange decision on benefits paid, one would ideally use individual-level longitudinal data on DC enrollment and benefit amounts for all veterans. This would allow us to estimate which new recipients enrolled in the program because of the policy change and which existing recipients enjoyed an increase in their benefits. Aggregating up the monthly benefits for these individuals, we could then calculate the effect on DC spending. Unfortunately we do not have this type of data. An (admittedly imperfect) alternative is to utilize aggregate data on the distribution of CDRs by service era in the years leading up to and following the policy change. As in the previous section, here we control for pre-existing trends in DC spending among Vietnam-era veterans to estimate the change that would have occurred in the absence of the Agent Orange decision if the pre-2000 trends had continued through June of 2006 (the most recent month available). Specifically we estimate the annual change from 1998 to 2000 in the number of Vietnam era veterans with each CDR and use this to predict the number with this CDR in 2006 as follows:²⁵

$$\hat{V}_{DC,j,06} = V_{DC,j,00} + 5.75 \left(\frac{V_{DC,j,00} - V_{DC,j,98}}{2} \right) \quad (3.2)$$

with $V_{DC,j,t}$ equal to the number of Vietnam era DC recipients in CDR j in year t . We attribute any difference between the actual and predicted number of recipients within each CDR to the Agent Orange decision. To estimate the effect on spending we simply multiply these CDR-specific effects by the average monthly benefit

²⁵We multiply by 5.75 because we are considering the change from September of 2000 to June of 2006.

amount for that CDR. If our assumptions are accurate, this estimate captures the spending effect that is attributable both to new recipients and to existing recipients.

Of course, the trend in the number of Vietnam era DC recipients for each CDR may have changed after 2000 even in the absence of the change in the program's medical eligibility criteria. For example, the Veterans Claim Assistance Act, the economic downturn, and related factors could have induced a break in trend. We therefore follow our approach from above and use veterans from the peacetime era as a control group. If our algorithm does a reasonable job of predicting the actual change in the number in each CDR bin for this group, it suggests that our estimates for the effect of the Agent Orange decision are not biased significantly by potentially confounding factors.

The results from this analysis are summarized in Table 3.8. The first and second panels include data for Vietnam and peacetime era veterans, respectively. The first three columns of the top panel list the number of Vietnam era veterans on the DC program with each of the eleven possible CDRs in 1998, 2000, and 2006, respectively. An examination of this data suggests that, despite the fact that the number of DC recipients was not changing much from 1998 to 2000, the distribution of the CDRs was. For example the number of DC recipients with ratings between 10 and 40 percent ratings declined by 3 percent, while the corresponding number with ratings between 50 and 100 percent increased by 12 percent. A similar pattern existed for peacetime era DC recipients from 1998 to 2000, with an increase of just 1 percent for ratings between 10 and 40 percent versus an increase of 13 percent for ratings between 50 and 100 percent. These pre-existing trends suggest that, even

in the absence of the policy change, the distribution of CDRs among Vietnam and peacetime era DC recipients would have changed after the Agent Orange decision.

The fourth column lists the change in the number with each CDR that would have occurred from 2000 to 2006 if these pre-existing trends had continued. To calculate these predicted changes, we use equation 3.2 above. According to our estimates, the number of Vietnam era veterans with a rating of 10 percent would have fallen from 227,800 to 201,658 while the number with a rating of 100 percent would have increased from 85,994 to 109,172 during this five year period. The first of these two estimates is relatively accurate, as the number with a ten percent rating in June of 2006 was 206,429. But the latter estimate is much too low, with the actual number rated at 100 percent standing at 137,020 at the end of our period.

The discrepancy between our estimates and the actual change for all eleven possible CDRs is listed in column seven. In every case our estimates are too low, which is not so surprising given the substantial increase in DC enrollment among Vietnam era veterans from 2000 to 2006. But in general the discrepancies are greatest for the highest CDRs. For example, we predicted an increase of 38,752 in the number with ratings of 80 percent or more, but the actual increase was substantially higher at 100,937.²⁶ Multiplying these CDR-specific discrepancies by the average monthly benefit in June of 2006 for each CDR, we estimate that DC expenditures were \$2.69 billion higher during the 2006 fiscal year than they would have been if the pre-2000 trend had continued. This represents more than 23 percent

²⁶This would be surprising if DC recipients with diabetes had low benefits on average. But in December of 2004 the average benefit was 19 percent greater among Vietnam-era DC recipients with diabetes than among their counterparts without diabetes. This is because recipients with diabetes tend to also be covered for other conditions.

of benefits paid for Vietnam-era DC recipients in the 2006 fiscal year.

The bottom panel repeats this exercise for veterans from the peacetime era. In this case the discrepancies between our predictions and the actual number in each CDR are much smaller. As was true for Vietnam era veterans, we tend to underestimate more for the higher CDRs, though the total estimated dollar value of our discrepancies is just \$0.02 billion for the 2006 fiscal year, which is 99.4 percent lower than the corresponding estimate of \$2.69 billion for Vietnam era DC spending. Additionally, this represents just 0.4 percent of DC spending on peacetime era DC recipients. The similarity between actual and predicted DC expenditures for peacetime era veterans suggests that our estimate for the effect of the Agent Orange decision on Vietnam era DC spending is not driven by other factors such as macroeconomic conditions or the Veterans Claims Assistance Act.

Of course, the Agent Orange decision did not only affect expenditures during the 2006 fiscal year, but in several previous years and in many future years as well. To estimate the impact of the Agent Orange decision on the present value of DC spending, we take the following simple approach. First, for the 2002 to 2005 fiscal years, we simply linearly interpolate the 2006 estimate. This would, for example, assume that 40 percent of the \$2.69 billion expenditure effect had occurred by 2003. For future years, we deflate the 2006 estimate by the VA's estimated decline in the Vietnam era veteran population. For example, the VA estimates that their ranks will decline by 16.2 percent from 2006 to 2016, and we therefore assume an expenditure effect of \$2.26 billion in that latter year.²⁷

²⁷The VA indexes DC benefits to the Consumer Price Index and thus we do not scale for the

Using this algorithm along with an annual real discount rate of 3 percent, we estimate that the present value of DC spending increased by \$45.1 billion dollars (in 2006 dollars) as a result of the policy change. For two reasons, this estimate is likely to understate the actual effect on the present value of VA spending. First, it assumes that there is no effect beyond the 2006 fiscal year on the number of DC recipients or on the benefits paid for existing DC recipients.²⁸ Second, it does not incorporate the effect on health care spending for DC recipients through the Veterans Health Administration, which we cannot reliably estimate with the available data. On the other hand, the estimate may be biased upward given that mortality rates of Vietnam-era DC recipients affected by the Agent Orange decision are likely to be higher than for the average Vietnam era veteran. But even when we adjust our present value calculations to account for the higher baseline mortality rates of Vietnam era DC recipients,²⁹ our estimated effect falls by just 19 percent to \$36.7 billion.

3.6 Reduced-Form Effects of the Agent Orange Decision

In the previous sections, we estimate the impact of the Agent Orange decision on DC rolls and expenditures using aggregate enrollment and expenditure data. With

effect of inflation.

²⁸It also neglects any effect beyond 2033, as the VA does not make population projections beyond that year.

²⁹The mortality rate in 2000 of Vietnam era DC recipients was 1.55% versus 0.71% for all Vietnam era veterans. We therefore scale the estimated mortality rates for all Vietnam veterans by 2.18 when calculating the present value.

peacetime veterans as a comparison group, the Agent Orange decision increased DC enrollment among Vietnam era veterans by an estimated 2.3 percentage points and DC expenditures by \$2.69 billion in 2006 fiscal year alone. We next consider other possible effects associated with an increase in DC receipt and benefit amounts. For example, the increased generosity of DC benefits may have discouraged Vietnam veterans to work. If this were the case, we would expect a decline in labor force participation among Vietnam veterans relative to comparable veterans or non-veterans who were not affected by the policy. Since the effect of the Agent Orange decision may indeed extend beyond just DC receipt and benefit generosity, estimating these reduced-form effects are important for comprehensive policy evaluation. To estimate these reduced-form effects of the Agent Orange decision, we employ micro-level data from the March Supplement of the Current Population Survey (CPS). By pooling CPS surveys for years 1998 to 2006, we first replicate the first-stage results of the effect of the Agent Orange decision on DC receipt. To do so, we derive and estimate a difference-in-differences type model of DC receipt which reflects the aggregate trends in DC receipt exhibited in Figure 3.2. Then, using a similar estimation equation, we estimate the effect of the Agent Orange decision on factors such as labor supply and health insurance status.

3.6.1 Sample Specification and Summary Statistics

The sample is constructed by pooling the March Supplement of the CPS for years 1998 through 2006 (reference years 1997 through 2005).³⁰ The policy was implemented in July 2001, so the sample spans four pre-reform years (1997 to 2000), four post-reform years (2002 to 2005), and the year during which the policy was implemented (2001).

In contrast to the aggregate data, these micro-level data provide the additional advantages of constructing more reliable comparison groups (for example, by conditioning the sample on year-of-birth) and controlling for demographic factors such as age, race, and educational attainment. One such comparison group, which was not possible using aggregate data, is comparably aged non-veterans. However, since veterans are predominately male, it would be circumspect to compare Vietnam veterans to all comparably aged non-veterans. Thus, in the empirical analysis, we restrict the sample to males.

Veteran status estimates by year-of-birth among males in the 2000 CPS are provided in Table 3.10. As indicated, era of service is well defined by birth cohort. For example, the prevalence of Vietnam veterans is highest among birth cohorts 1944 and 1948, peaking at 43.3% of males born in year 1947. Korean veterans, on the other hand, are generally older than Vietnam veterans - approximately 50% of individuals born between 1930 and 1933 are Korean War veterans. The prevalence of peacetime veterans is highest among birth cohorts between those that define

³⁰Prior to pooling, all person weights within a sample year were rescaled to sum to the number of person observations within the same sample year.

Korean War and Vietnam Era veterans and birth cohorts born shortly after those that define Vietnam Era veterans.

Similar to the aggregate data analyses, we first consider using veterans who did not serve during the Vietnam Era in comparison to Vietnam Era veterans; however, in addition to peacetime veterans used in the aggregate analyses, we also include Korean veterans in the comparison group. Conditioning the sample on males born between 1930 and 1959, summary statistics among Vietnam Era veterans and all other veterans (excluding the few World War II veterans born during these years) are presented in Table 3.11. Naturally, these other veterans are slightly older than Vietnam Era veterans (58.9 versus 52.5). This age disparity manifests itself in other statistics associated with age; labor force non-participation (43.4% versus 18.0%), any income from earnings (62.1% versus 85.0%), receipt of Social Security benefits (42.8% versus 7.6%), and any disability (15.1% versus 12.5%). In regards to VA benefits, Vietnam Era veterans relative to Other veterans are more likely to receive DC benefits (7.1% versus 3.6%) and any VA benefits (9.3% versus 5.6%), but the amount of VA benefits among those receiving benefits are comparable among Vietnam Era veterans relative to Korean War and peacetime veterans (11508*versus*12070).

To be consistent with the previous analyses, we first consider Korean War and peacetime veterans as a comparison group to Vietnam veterans; but given the age disparity among these two groups, we also consider comparably aged non-veterans as a comparison group. Because there are a substantial number of non-veterans in all birth years, we can constrict the birth cohorts under consideration

from 1930 to 1959 to birth cohorts 1939 to 1954. Summary statistics among Vietnam Era veterans and non-veterans born between 1940 and 1950 are also presented in Table 3.11. As indicated, the average age among these two groups are similar: 52.3 and 51.0 among Vietnam Era veterans and non-veterans, respectively. Additionally, labor force non-participation among Vietnam Era veterans is similar to non-veterans (16.1% versus 14.6%), as well as any income from earnings (86.8% versus 85.9%), receipt of Social Security benefits (4.8% versus 5.5%), and any disability (12.5% versus 11.3%). However, demographics among these two groups differ in important ways: Vietnam Era veterans are more likely to have completed high school and attended college relative to non-veterans but are less likely to have completed college.

3.6.2 Equation Specification and First-Stage Results

Failure to replicate the previous first-stage estimates would cast doubt on subsequent reduced-form results. Thus, we first consider estimating impact of the Agent Orange decision on DC receipt among Vietnam Era veterans using other veterans as a comparison group. To estimate the impact of the policy on DC receipt, we model receipt according to the aggregate trends in Figure 3.2. The model considered is given by

$$\begin{aligned}
 DC_{jt} = & \alpha_0 + \alpha_1 Viet_j Posttrend_t + \alpha_2 Trend_t + \alpha_3 Korean_j + \alpha_4 Peace_j \\
 & + \alpha_5 Korean_j Trend_t + \alpha_6 Peace_j Trend_t + \alpha_7 \mathbf{X}_j + \epsilon_{jt}
 \end{aligned}$$

In this estimation equation, we allow for era specific intercepts and trends in DC receipt: $Korean_j$ and $Peace_j$ are indicator variables for era status and $Trend_t$ equals zero in reference year 1997 and increases by one for each year thereafter. When indicated, fixed factors such as race (black, white, and other), educational attainment (less than high school, high school, some college, and college and beyond), and individual age effects are included, denoted by the vector \mathbf{X}_j in the specification equation above.

The coefficient of interest is α_1 which corresponds to the interaction of an indicator for Vietnam Era status $Viet_j$ and a post-trend variable $Posttrend_t$. In contrast to $Trend_t$, $Posttrend_t$ is equal to zero during years 1997 to 2001 and increases by one each year thereafter. In accord with aggregate trends in DC receipt in Figure 3.2, this interaction term allows for a differential trend in DC receipt among Vietnam Era veterans that begins after the Agent Orange decision was implemented. Assuming that DC receipt among veterans from various eras would have followed their preexisting linear trends in the absence of the policy, the coefficient α_1 is the percentage point increase in DC receipt among Vietnam Era veterans due to the policy for each year after it was implemented.

The baseline estimate of α_1 ; sans race, educational attainment, and age controls and using veterans who did not serve during the Vietnam Era as a comparison group; is .46 and statistically significant at the 5% level of confidence (Table 3.12). The estimated effect of the policy on DC receipt among Vietnam Era veterans by 2006, calculated as .46 factored by 5, is 2.3 percentage points. This estimate corresponds precisely with the estimated 2.3 percentage points obtained from aggregate

administrative data.

As indicated in Table 3.11, Vietnam Era veterans more educated than other veterans, a factor which may cast doubt on the identification assumptions outlined above. Therefore, in the next specification, we include race and educational fixed effects to the regression equation. When these effects are included, the estimate of α_1 increases to .49, suggesting a 2.45 percentage point increase in DC receipt among Vietnam Era veterans by 2006. Also indicated in Table 3.11, Vietnam Era veterans are younger than other veterans, so in the third specification we include race, educational attainment, and individual aged fixed effects. As shown, the estimate of α_1 is .48. Thus, the baseline estimate of 2.3 percentage points is robust to the inclusion of race, educational attainment, and age fixed effects.

If unknown factors other than the Agent Orange decision generated a differential increase in DC receipt among Vietnam Era veterans, then it would be careless to attribute the 2.3 percentage point increase in DC receipt among Vietnam Era veterans to the policy. For example, according to Table 3.11, Vietnam Era veterans are younger and substantially more likely to be in the labor force. So if Vietnam Era veterans exhibit a greater incidence of retirement, and thus a greater propensity to seek alternative sources of income, relative to other veterans then the differential increase in DC receipt among Vietnam Era veterans cannot be entirely attributed to the reform.

To explore this alternative explanation, we employ the same linear probability specification to model VA pension receipt. Intuitively, if factors other than the Agent Orange decision generated the differential rise in DC receipt among Vietnam

Era veterans and if these other factors similarly increased the prevalence of VA pension receipt, then we expect to reject the null hypothesis that α_1 is zero.

Estimation results when the outcome of interest is VA pension receipt are presented in Table 3.12. As indicated, the estimate of α_1 is negligible and statistically insignificant. Since VA pension receipt did not rise in tandem with DC receipt, and since the Agent Orange only affected DC eligibility, these results cast doubt that factors unrelated to the Agent Orange decision generated the differential rise in DC receipt among Vietnam Era veterans.

3.6.3 Reduced-Form Results

We next consider the effects of the Agent Orange decision on outcomes associated with a rise in DC generosity. These outcomes include labor supply, sources of income, poverty status, health status, and health insurance status. The preferred linear probability model is given by,

$$Y_{jt} = \beta_0 + \beta_1 Viet_j Posttrend_t + \beta_2 Viet_j Post_t + \beta_3 Trend_t + \beta_4 Korean_j + \beta_5 Peace_j + \beta_6 Korean_j Trend_t + \beta_7 Peace_j Trend_t + \beta_8 X_j + v_{jt}$$

where Y_{jt} is the outcome of interest. The model is identical to the one considered above with one modification: the inclusion of $Viet_j Post_t$, where $Post_t$ is equal to one in reference years 2001 to 2005 and zero otherwise. This interaction term allows for a discreet change in the probability of Y_{jt} among Vietnam Era vet-

erans after the Agent Orange decision is implemented. (This term was suppressed in the DC receipt model estimated above because, when included, it did not affect the estimate of α_1 and its associated coefficient was negligible and insignificant.) This permits, for example, a discreet and permanent shift in the probability of work among Vietnam Era veterans in anticipation of receiving DC benefits.

The first set of outcomes considered is labor supply. The CPS asks whether a veteran is not participating in the labor force and, if not participation, asks for one of three reasons why; retirement, disability, or other. Estimation results of labor force non-participation are in Table 3.14. As indicated, there does not appear to be a statistically significant shift or trend effect of the Agent Orange decision on labor force non-participation among Vietnam Era veterans. However, when the outcome variable is defined as not working in the past week, the policy appears to have decreased the probability of working by 3.0 percentage points.

The second set of outcomes considered is income from various sources. Although the results above do not suggest a decline in labor force non-participation, there does appear to be a decrease in the probability of reporting positive income from earnings during the reference year. According to the estimates from our model, Vietnam Era veterans were 2.1% points less likely to report income from earnings after the policy was implemented. Also, in accord with our DC receipt estimates, the receipt of any VA benefits differentially trended upwards among Vietnam Era veterans relative to other veterans, reaching a differential increase of 2.8% points by 2006. And finally, the estimates suggest a 1.5% point increase in the receipt of "other retirement benefits" which, according to the way the question was phrased,

excludes retirement benefits from the SSA and the VA.

We next consider the effect of the Agent Orange decision on health status. In particular, we estimate two probability models of whether a respondent reports a disability and whether he reports being in poor health. As shown in previous studies, the advent or receipt of disability benefits may influence the response to such questions in a survey. However, our results, also presented in Table, suggest that policy had no effect on prevalence of self-reported disability or poor health among Vietnam Era veterans.

And finally, we estimate the effect of the policy on health insurance status. As previously stated, Veterans may become eligible for VA health care upon receiving DC benefits for a service-related disability. Thus, if public provision of health insurance crowds out the private health insurance market, it may be possible that an increase in DC receipt may correspond to a decrease in insurance through one's employer. As shown in Table 3.14, the estimates suggest a 1.1 percentage point increase in the probability of insurance through the VA among Vietnam Era veterans per year after the policy was implemented. Additionally, there is a 1.4 percentage point drop decline in the probability of insurance through one's employer per year after the policy was implemented. These estimates imply that by 2006 5.5 percent of Vietnam Era veterans became insured through the VA and 7.0 percent discontinued insurance through his employer. Thus, the results are consistent with the crowd-out hypothesis: Vietnam Era veterans dropped health insurance through their employers after being newly eligible for DC benefits and corresponding health insurance benefits through the VA.

3.6.4 Results Using Non-Veterans as the Comparison Group

The results thus far reflect the use of veterans who did not serve during the Vietnam Era as the comparison group; however, for reasons outlined above, these veterans may not be ideal. Thus, we next consider using comparably aged non-veterans as a comparison group to Vietnam Era veterans. Because there are a substantial number of non-veterans during all birth cohorts, we restrict the analysis to Vietnam Era veterans and non-veterans born between 1939 and 1954. Naturally, the main and interacted effects of $Korean_j$ and $Peace_j$ are omitted from the equation above.

We first return to the first-stage regressions: the probability of DC receipt. As indicated in Table 3.13, the estimated effects using non-veterans as a comparison group are larger than those obtained using veterans who did not serve during the Vietnam Era. The larger estimates are not surprising given that few non-veterans receive DC benefits in any given year, so the variable $Trend_j$ imposed on both Vietnam Era veterans and non-veterans alike is unfounded. Nonetheless, it is reassuring to confirm the qualitative result that DC receipt increases differentially among Vietnam Era veterans after the policy was implemented.

Similar to the first-stage results, many of the reduced-form results are robust to the use of non-veterans as a comparison group. First, Vietnam Era veterans were 2.4 percentage points less likely to be working in the past week after the policy was implemented (Table 3.14). In terms of income, Vietnam Era veterans were less likely to report positive income from earnings (1.6 percentage point decline after the policy was implemented) and were more likely to receive any income from

the VA (.56 percentage point increase per year after the policy was implemented). Interestingly, the health insurance status results are also robust, further suggesting that the increased eligibility of VA health insurance due to the Agent Orange decision lead to a decline in insurance through one's employer.

3.7 Discussion

The findings in this paper suggest that a change in the medical eligibility criteria for the VA's Disability Compensation program that applied only to Vietnam veterans induced a 7.6 percentage point increase in disability enrollment among this group and increased the monthly benefit amount for an additional 2.5 percent. The effects of this change on VA expenditures were substantial, with our estimates suggesting that DC spending during the 2006 fiscal year was \$2.69 billion higher than it otherwise would have been and that the present value increase in VA expenditures was approximately \$45 billion. These estimates for enrollment and expenditures are likely to be conservative, as we have not considered any effect on veterans not already affected by June of 2006 nor have we incorporated the resulting increase in health care spending through the Veterans Health Administration.

What do these findings imply for other disability programs such as SSDI and SSI? Because only the 2.3 million veterans who served in Vietnam were directly affected by this policy change, it is clear that one cannot assume that a similar change for those programs would have the same response. Additionally, because the DC program is quite different from SSDI and SSI, which pay benefits on an all-or-

nothing basis and do not allow recipients to have significant labor market earnings, the effect of such a change for these other programs might be quite different. However the findings do demonstrate that a relatively narrow change in the medical eligibility criteria for the DC program led to an increase in disability benefits for 10.1 percent of the individuals potentially affected by the policy. This makes it more plausible that the 1984 reforms to SSDI and SSI, which expanded the medical eligibility criteria for these programs, could have been largely responsible for the significant increase in enrollment for these two programs during the past two decades.

A potentially important direction for future research would be to estimate the effect of the induced increase in DC enrollment on the health, labor supply, and material well-being of veterans who served in Vietnam. As a result of this policy change, more Vietnam veterans received essentially free health care through the Veterans Health Administration. This, along with the increase in benefits, could plausibly have improved the health of Vietnam veterans. Similarly while the DC program does not introduce a high marginal tax rate on earnings, it is plausible that a DC award or an increase in DC benefits could reduce labor supply through an income effect.

More generally, the VA's Disability Compensation program is a large and rapidly growing program that has essentially been ignored in prior economic research. At present there are 2.72 million veterans receiving DC benefits with \$25 billion paid in benefits during the 2005 fiscal year. More work on this program, which is an increasingly important source of income and insurance for the nation's 24 million veterans and 45 million of their family members, seems warranted.

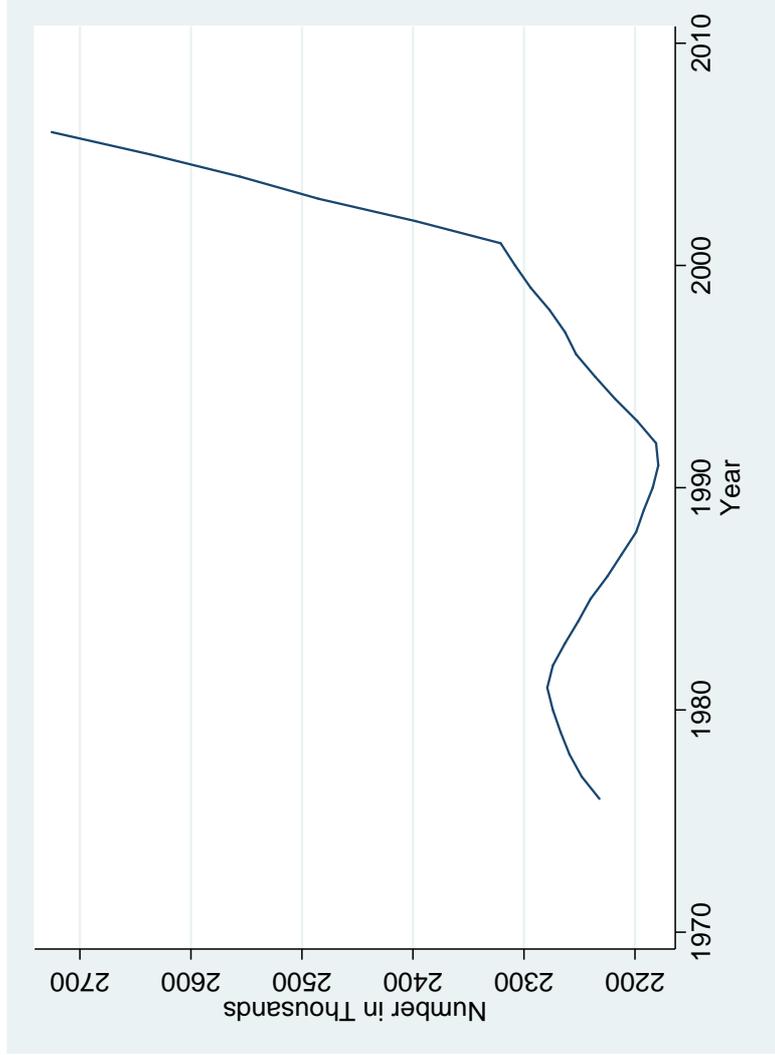


Figure 3.1: Number of Disability Compensation Recipients: 1976-2006

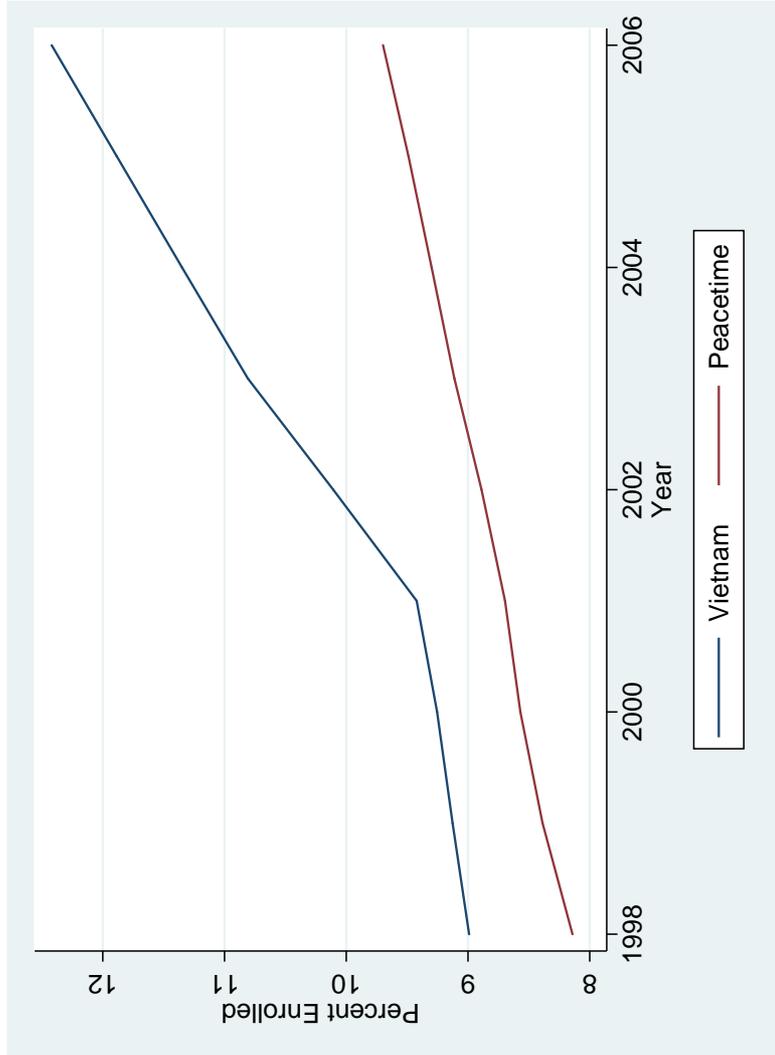


Figure 3.2: DC Enrollment for Vietnam and Peacetime Era Veterans: 1998-2006

Table 3.1: Veteran Population Estimates by Era of Service in September of 2000 and 2006

	Number in Millions			Share	
	2000	2006	% Change	2000	2006
World War II Era	5.59	3.15	-43.6%	21.1%	13.1%
Between WWII and Korea	0.24	0.16	-33.3%	0.9%	0.7%
Korean War Era	3.50	2.76	-21.1%	13.2%	11.5%
Between Korea and Vietnam	2.88	2.54	-11.8%	10.8%	10.6%
Vietnam War Era	8.01	7.63	-4.7%	30.2%	31.8%
Between Vietnam and Gulf War	3.49	3.45	-1.1%	13.1%	14.4%
Gulf War Era	2.84	4.30	51.4%	10.7%	17.9%
Total # of Veterans	26.55	23.98	-9.7%	100.0%	100.0%

Data includes the number of veterans alive in September of 2000 and September of 2005 from each of seven different service eras.

Individuals who served during a conflict and during peacetime are assigned to the conflict. Individuals serving during two or more of the four conflicts are assigned to the earliest conflict. Data were obtained from the VA's VP04 Ver 1.0 population estimates, which are available online at <http://www.va.gov/vetdata/demographics/> and represent the VA's most recent estimates as of May, 2006.

Table 3.2: US Department of Veterans' Affairs Expenditures: 1998-2005

	1998	1999	2000	2001	2002	2003	2004	2005
Compensation and Pension	\$23,532	\$24,231	\$24,138	\$24,927	\$27,479	\$29,698	\$30,807	\$32,068
Medical Expenditures	\$21,087	\$20,786	\$23,267	\$23,600	\$25,073	\$27,014	\$29,086	\$30,675
Education and Voc Rehab	\$1,716	\$1,814	\$1,848	\$1,715	\$2,153	\$2,398	\$3,081	\$3,155
Insurance and Indemnities	\$2,435	\$2,307	\$2,223	\$2,148	\$2,063	\$1,995	\$1,928	\$1,809
Operating Expenses	\$1,539	\$1,517	\$1,698	\$1,722	\$1,933	\$1,999	\$2,003	\$2,238
Construction	\$655	\$552	\$396	\$444	\$481	\$445	\$438	\$303
Total Spending	\$50,964	\$51,206	\$53,569	\$54,556	\$59,182	\$63,549	\$67,343	\$70,248

Data represents VA expenditures by category (in millions of 2005 dollars). Data for the 1999-2004 fiscal years is available online at <http://www.va.gov/vetdata/GeographicInformation/index.htm>. Data

for 1998 and 2005 were obtained from the Department of Veterans Affairs following an email request.

Table 3.3: Compensation and Pension Recipients by Program: 1998-2006

	1998	1999	2000	2001	2002	2003	2004	2005	2006
Disability Compensation	2,277,049	2,294,453	2,308,186	2,321,103	2,398,287	2,485,229	2,555,696	2,636,979	-239,725
Disability Pension	390,978	378,712	364,220	348,052	346,579	346,555	342,903	335,787	-30,526
Death Compensation	310,299	311,631	313,408	313,540	315,731	319,998	324,019	329,184	-29,926
Death Pension	291,483	274,106	257,106	241,467	230,267	223,553	215,253	206,594	-18,781
Total C and P Recipients	3,269,809	3,258,902	3,242,920	3,224,162	3,290,864	3,375,335	3,437,871	3,508,544	-318,959

Data represents the number of recipients of each program in September of each year. Data for the 1999-2004 fiscal years is available online from the VBA's Annual Benefits Report publication at <http://www.vba.va.gov/reports.htm>. Data for the 1998 and 2005 fiscal years were obtained from the Department of Veterans Affairs following an email request.

Table 3.4: Compensation and Pension Expenditures by Program: 1999-2004

	1999	2000	2001	2002	2003	2004
Disability Compensation	\$16,298	\$16,755	\$17,430	\$19,095	\$20,736	\$21,290
Disability Pension	\$2,520	\$2,444	\$2,396	\$2,470	\$2,530	\$2,530
Death Compensation	\$3,897	\$3,892	\$3,926	\$3,873	\$4,027	\$4,011
Death Pension	\$807	\$750	\$715	\$705	\$701	\$692
Total Estimated	\$23,520	\$23,842	\$24,467	\$26,274	\$27,993	\$28,523

Data represents the total estimated expenditure by program in each fiscal year (in millions of 2005 dollars). Data for the 1999-2004

fiscal years is available online from the VBA's Annual Benefits Report publication at <http://www.vba.va.gov/reports.htm>. Totals

differ slightly from those listed in Table 2 because this data is estimated while the data in Table 2 represents actual spending.

Table 3.5: Monthly Disability Compensation Benefit Amounts During the 2006 Fiscal Year

CDR	Baseline	1 child	1 spouse	1 child and 1 spouse	Each Add Child < 18	Each Add In School 18+
10%	\$112	\$112	\$112	\$112	\$0	\$0
20%	\$218	\$218	\$218	\$218	\$0	\$0
30%	\$337	\$364	\$377	\$406	\$20	\$64
40%	\$485	\$521	\$539	\$578	\$27	\$86
50%	\$690	\$735	\$757	\$806	\$34	\$107
60%	\$873	\$927	\$954	\$1,012	\$40	\$129
70%	\$1,099	\$1,162	\$1,193	\$1,262	\$47	\$150
80%	\$1,277	\$1,349	\$1,385	\$1,463	\$54	\$172
90%	\$1,436	\$1,517	\$1,557	\$1,645	\$61	\$193
100%	\$2,393	\$2,484	\$2,528	\$2,626	\$68	\$215

Data represents the monthly benefit amount by combined disability rating (CDR) and presence of dependents for Disability

Compensation recipients during the 2006 fiscal year. This data and the corresponding data for earlier years can be obtained at

<http://www.vba.va.gov/bln/21/Rates/>.

Table 3.6: Number of DC Recipients and Average Monthly DC Benefits by CDR in June 2006

CDR	Recipients	Dollars	Avg Benefit	Gulf	Vietnam	Korea	WW II	Peacetime
0%	14,394	\$ 1,075	\$75	0.1%	0.3%	2.5%	1.4%	0.4%
10%	774,887	\$ 87,336	\$113	27.6%	22.0%	30.6%	33.2%	37.4%
20%	415,510	\$ 91,437	\$220	17.9%	13.4%	13.0%	12.5%	17.8%
30%	332,768	\$ 124,874	\$375	15.3%	10.7%	12.5%	13.2%	11.0%
40%	256,487	\$ 139,326	\$543	12.5%	9.0%	8.2%	8.1%	8.0%
50%	159,003	\$ 121,343	\$763	7.1%	5.9%	5.4%	5.8%	4.6%
60%	181,254	\$ 230,850	\$1,274	6.9%	7.0%	7.6%	7.1%	5.6%
70%	162,202	\$ 302,969	\$1,868	4.7%	8.5%	5.5%	5.3%	4.1%
80%	110,699	\$ 229,674	\$2,075	3.3%	5.6%	4.1%	4.0%	2.7%
90%	58,865	\$ 133,267	\$2,264	1.6%	3.1%	2.2%	2.3%	1.4%
100%	235,971	\$ 645,755	\$2,737	3.0%	14.6%	8.5%	7.2%	6.9%
Total	2,702,040	\$ 2,107,906	\$780	100.0%	100.0%	100.0%	100.0%	100.0%
		Total Era-Specific Enrollment	674,021	939,200	160,007	334,222	594,590	
		Total Era-Specific Expenditures	\$389,318	\$966,165	\$126,226	\$244,833	\$381,364	
		Average Era-Specific Benefit	\$578	\$1,029	\$789	\$733	\$641	

Data in the first three columns provides the number of DC recipients, the total amount paid (in thousands of dollars), and the average monthly DC benefit by combined disability rating (CDR) in June of 2006. Data in the last five columns provides the share of DC recipients from each service era with each value of the CDR. Data were obtained from the Department of Veterans Affairs following an email request.

Table 3.7: Veterans Receiving Disability Compensation Benefits by Service Era and Year: 1998-2006

		1998	1999	2000	2001	2002	2003	2004	2005	2006
WWI + WWII	# Receiving	578,459	541,312	505,133	470,217	440,477	413,702	385,504	356,194	327,570
	% Change	-6.1%	-6.4%	-6.7%	-6.9%	-6.3%	-6.1%	-6.8%	-7.6%	-8.0%
	# Vets / 1000	6,544	6,044	5,582	5,155	4,732	4,319	3,916	3,526	3,151
	% Receiving	8.8%	9.0%	9.0%	9.1%	9.3%	9.6%	9.8%	10.1%	10.4%
Korean War	# Receiving	178,682	174,807	170,616	166,362	164,728	164,482	163,635	161,512	159,749
	% Change	-2.1%	-2.2%	-2.4%	-2.5%	-1.0%	-0.1%	-0.5%	-1.3%	-1.1%
	# Vets / 1000	3,730	3,614	3,502	3,392	3,276	3,154	3,027	2,894	2,757
	% Receiving	4.8%	4.8%	4.9%	4.9%	5.0%	5.2%	5.4%	5.6%	5.8%
Vietnam War	# Receiving	729,402	735,627	740,976	749,554	798,549	848,156	883,092	916,220	947,601
	% Change	0.8%	0.9%	0.7%	1.2%	6.5%	6.2%	4.1%	3.8%	3.4%
	# Vets / 1000	8,113	8,060	8,007	7,955	7,901	7,848	7,781	7,709	7,629
	% Receiving	9.0%	9.1%	9.3%	9.4%	10.1%	10.8%	11.3%	11.9%	12.4%
Peacetime	# Receiving	549,862	560,567	566,833	569,190	575,413	582,863	587,331	591,324	595,634
	% Change	2.1%	1.9%	1.1%	0.4%	1.1%	1.3%	0.8%	0.7%	0.7%
	# Vets / 1000	6,753	6,683	6,614	6,546	6,473	6,397	6,316	6,231	6,142
	% Receiving	8.1%	8.4%	8.6%	8.7%	8.9%	9.1%	9.3%	9.5%	9.7%
Gulf War	# Receiving	240,644	282,140	324,628	365,780	419,120	476,026	536,134	611,729	694,918
	% Change	19.2%	17.2%	15.1%	12.7%	14.6%	13.6%	12.6%	14.1%	13.6%
	# Vets / 1000	2,510	2,669	2,838	3,017	3,199	3,474	3,753	4,027	4,297
	% Receiving	9.6%	10.6%	11.4%	12.1%	13.1%	13.7%	14.3%	15.2%	16.2%
Total	# Receiving	2,277,049	2,294,453	2,308,186	2,321,103	2,398,287	2,485,229	2,555,696	2,636,979	2,725,472
	% Change	0.6%	0.8%	0.6%	0.6%	3.3%	3.6%	2.8%	3.2%	3.4%
	# Vets / 1000	27,522	27,028	26,542	26,066	25,582	25,191	24,793	24,387	23,977
	% Receiving	8.3%	8.5%	8.7%	8.9%	9.4%	9.9%	10.3%	10.8%	11.4%

Entries in this table represent the number of DC recipients, the number of veterans, and the fraction of veterans on DC by service era and year. Population data were obtained from the VA's most recent population estimates (summarized in Table 1) and DC enrollment data were obtained from the VBA's Annual Benefits Report publication, which is available online at <http://www.vba.va.gov/reports.htm> for the 1999-2004 fiscal years. Data for 2005 and 2006 were obtained from the Department of Veterans Affairs in response to an email request.

Table 3.8: The Fraction of DC Recipients and Awardees with Diabetes as a Covered Condition

	1999	2000	2001	2002	2003	2004	2005
# with Diabetes	37808	37985	46395	111932	161551	191649	220532
% with Diabetes	1.65%	1.65%	2.00%	4.67%	6.50%	7.50%	8.36%
# Vietnam with Diabetes	18904*	18993*	27403*	88259	135011	163485	190199
% Vietnam with Diabetes	2.57%	2.56%	3.03%	11.05%	15.92%	18.51%	20.76%
# All Other with Diabetes	18904*	18992*	18992*	23673	26540	28164	30333
% All Other with Diabetes	1.21%	1.21%	1.21%	1.48%	1.62%	1.68%	1.76%

The first two rows summarize the number and percentage of DC recipients with diabetes at the end of each fiscal year. The next two rows provide this same information for Vietnam era veterans and the last two rows provide this data for veterans from all other eras. Entries with an asterisk are imputed. See Appendix Tables 1 and 2 for an explanation of the imputation procedure.

Table 3.9: Number of DC Recipients by Combined Disability Rating for Vietnam and Peacetime Era

CDR	Panel A: Vietnam Era DC Recipients									
	<i>Number of Recipients</i>				<i>Change 2000-2006</i>				Avg Monthly DC Benefit	Est. Impact on 2006 DC \$
	1998	2000	2006		Predicted	Actual	Difference			
0%	1,413	1,644	2,567	664	923	259	\$75	\$232		
10%	236,893	227,800	206,429	-26,142	-21,371	4,771	\$113	\$6,453		
20%	106,162	102,836	125,994	-9,562	23,158	32,720	\$220	\$86,405		
30%	103,715	100,946	100,286	-7,961	-660	7,301	\$375	\$32,877		
40%	66,412	66,094	84,843	-914	18,749	19,663	\$543	\$128,175		
50%	42,490	43,772	55,642	3,686	11,870	8,184	\$763	\$74,950		
60%	39,097	41,805	65,423	7,786	23,618	15,833	\$1,274	\$241,977		
70%	29,063	38,443	79,443	26,968	41,000	14,033	\$1,868	\$314,527		
80%	17,712	21,450	52,705	10,747	31,255	20,508	\$2,075	\$510,596		
90%	8,513	10,192	28,848	4,827	18,656	13,829	\$2,264	\$375,693		
100%	77,932	85,994	137,020	23,178	51,026	27,848	\$2,737	\$914,494		
Total	729,402	740,976	939,200	33,275	198,224	164,949	-	\$2,686,380		

Panel B: Peacetime Era DC Recipients (cont'd)

CDR	<i>Number of Recipients</i>			<i>Change 2000-2006</i>		Avg Monthly DC Benefit	Est. Impact on 2005 DC \$
	1998	2000	2006	Predicted	Actual		
0%	2,791	2,704	2,406	-250	-298	\$75	-\$43
10%	242,816	239,621	222,500	-9,186	-17,121	\$113	-\$10,733
20%	105,401	107,411	106,064	5,779	-1,347	\$220	-\$18,817
30%	62,041	64,169	65,530	6,118	1,361	\$375	-\$21,421
40%	38,409	41,738	47,361	9,571	5,623	\$543	-\$25,734
50%	20,200	22,196	27,544	5,739	5,348	\$763	-\$3,576
60%	22,104	25,477	33,496	9,697	8,019	\$1,274	-\$25,652
70%	11,878	14,732	24,527	8,205	9,795	\$1,868	\$35,633
80%	7,230	9,048	15,864	5,227	6,816	\$2,075	\$39,568
90%	3,281	4,152	8,035	2,504	3,883	\$2,264	\$37,460
100%	33,711	35,585	41,263	5,388	5,678	\$2,737	\$9,532
Total	549,862	566,833	594,590	48,792	27,757	-	\$16,216

Data summarized in columns 2-4 represent the actual number of DC recipients by CDR in September of 1998 and 2000 and in June of 2006 for the Vietnam and peacetime service eras. The next column

lists the predicted change from 2000 to 2006 if the 1998-2000 trend had continued through June of 2006. Columns 6 and 7 list the actual change and the discrepancy between the actual and predicted

values. Column 8 lists the average monthly benefit by CDR in September of 2005 and the final column multiplies this amount by 12 (to annualize) and by the discrepancy listed in column 7.

Table 3.10: Veteran and Armed Service Status of Men by Year of Birth

YOB	Vietnam Vietnam	Korean Korean	WWII WWII	Other/ Peacetime	Armed Services	Non veteran/ armed services
1910-1919	0.0	0.4	20.2	0.8	0.0	78.6
1920-1929	0.4	5.9	23.4	0.9	0.0	69.4
1930	2.6	49.9	1.8	5.8	0.0	39.9
1931	2.0	55.6	0.0	4.8	0.0	37.6
1932	3.0	52.1	0.0	8.5	0.0	36.4
1933	3.2	45.2	0.0	8.8	0.0	42.8
1934	4.3	27.1	0.0	18.7	0.0	50.0
1935	6.8	21.7	0.0	23.2	0.0	48.3
1936	6.7	15.8	0.0	23.8	0.0	53.8
1937	8.8	8.7	0.0	28.1	0.0	54.4
1938	9.2	5.0	0.0	27.5	0.0	58.4
1939	11.4	0.0	0.0	23.1	0.0	65.5
1940	17.2	0.0	0.0	18.5	0.0	64.2
1941	22.5	0.0	0.0	15.3	0.1	62.2
1942	29.8	0.0	0.0	10.1	0.0	60.2
1943	29.9	0.0	0.0	9.4	0.4	60.4
1944	38.6	0.0	0.0	4.2	0.0	57.2
1945	41.1	0.0	0.0	3.4	0.0	55.5
1946	41.0	0.0	0.0	3.1	0.3	55.6
1947	43.4	0.0	0.0	2.3	0.2	54.1
1948	35.0	0.0	0.0	3.0	0.5	61.5
1949	27.6	0.0	0.0	2.9	0.0	69.5
1950	23.4	0.0	0.0	3.6	0.1	72.9
1951	18.1	0.0	0.0	2.8	0.2	78.9
1952	15.5	0.0	0.0	3.2	0.5	80.8
1953	11.5	0.0	0.0	4.5	0.3	83.7
1954	10.5	0.0	0.0	5.8	0.1	83.6
1955	7.8	0.0	0.0	5.4	0.8	86.0
1956	5.0	0.0	0.0	10.1	0.7	84.1
1957	2.8	0.0	0.0	10.7	1.4	85.0
1958	0.9	0.0	0.0	11.2	0.8	87.1
1959	0.0	0.0	0.0	10.5	0.6	88.9
1960-1969	0.0	0.0	0.0	6.1	0.7	93.2
1970-1979	0.0	0.0	0.0	2.8	0.7	96.4

Statistics were derived from males in the March Supplement of the 2000 Current Population Survey. Service era among veterans is determined by the period during which the veteran served. Veterans who served during a conflict and other/peacetime are assigned to the conflict era. Veterans who served in more than one conflict are assigned to the most recent conflict. Year of birth is determined as 2000 - age - 1, where age is self-reported in the CPS. Prevalence rates of veteran status were estimated using the March CPS person weights.

Table 3.11: Summary Statistics of Males by Veteran Status and Year of Birth

Years of Birth Veteran Status	1930-1959		1939-1954	
	Vietnam	Other Veterans	Vietnam	Non Veterans
Observations	3,555	3,149	3,169	8,337
Age	52.5 (4.9)	58.9 (9.4)	52.3 (3.7)	51.0 (4.6)
Education (%)				
Less than HS	4.3 (20.2)	10.5 (30.7)	4.1 (19.9)	14.8 (35.5)
HS	33.0 (47.0)	38.4 (48.6)	32.7 (46.9)	29.4 (45.6)
Some College	35.1 (47.8)	27.9 (44.9)	35.3 (47.8)	22.1 (41.5)
College and Beyond	27.6 (44.7)	23.2 (42.2)	27.9 (44.8)	33.7 (47.3)
Race (%)				
White	88.6 (31.7)	87.7 (32.8)	88.9 (31.4)	84.1 (36.6)
Black	9.1 (28.7)	10.2 (30.3)	8.9 (28.5)	10.4 (30.6)
Other	2.3 (15.0)	2.1 (14.2)	2.2 (14.5)	5.5 (22.8)
Health Status				
Any Disability	12.5 (33.1)	15.1 (35.8)	12.5 (33.0)	11.3 (31.6)
Bad Health	5.8 (23.4)	6.8 (25.2)	5.9 (23.5)	5.4 (22.7)
Insurance Status				
Employer	66.1 (47.3)	55.2 (49.7)	67.3 (46.9)	61.6 (48.6)
Private	7.6 (26.5)	14.7 (35.5)	6.9 (25.4)	7.7 (26.7)
VHA	5.2 (22.2)	4.0 (19.6)	5.0 (21.8)	0.1 (3.3)

Summary Statistics of Males by Veteran Status and Year of Birth (cont'd)

Labor Force Status (%)				
Not in Labor Force	18.0	43.4	16.1	14.6
	(38.5)	(49.6)	(36.8)	(35.3)
NILF - Retired	8.2	35.4	6.0	4.3
	(27.4)	(47.8)	(23.7)	(20.2)
NILF - Disabled	7.4	5.7	7.7	7.5
	(26.2)	(23.2)	(26.6)	(26.4)
NILF - Other	2.5	2.3	2.5	2.8
VA Receipt (%)				
Disability Compensation	7.1	3.6	6.9	0.4
	(25.7)	(18.6)	(25.4)	(6.1)
Veteran Pension	2.2	2.2	1.7	0.0
	(14.8)	(14.7)	(12.8)	(2.2)
	(15.5)	(14.9)	(15.5)	(16.5)
Sources of Income Receipt (%)				
Earnings	85.0	62.1	86.8	85.9
	(35.7)	(48.5)	(33.9)	(34.8)
VA Payments	9.3	5.6	8.7	0.4
	(29.1)	(23.1)	(28.2)	(6.6)
Social Security Benefits	7.6	42.8	4.8	5.5
	(26.6)	(49.5)	(21.5)	(22.8)
Other Retirement	11.8	30.5	9.5	4.4
	(32.2)	(46.1)	(29.4)	(20.5)
Other Disability	2.2	1.3	2.2	1.5
	(14.6)	(11.5)	(14.6)	(12.3)
Investment Income	65.9	65.7	65.7	59.8
	(47.4)	(47.5)	(47.5)	(49.0)
Public Assistance	0.1	0.3	0.0	0.4
	(2.5)	(5.0)	(1.8)	(6.1)
Sources of Income Amount (2005\$)*				
Earnings	58613	45909	59156	61296
	(49671)	(45700)	(48595)	(55636)
VA Payments	11508	12070	11197	10103
	(11641)	(12363)	(11363)	(9621)
Social Security Benefits	11303	12218	11490	11492
	(5444)	(5208)	(6031)	(5203)
Other Retirement	21396	17122	20966	21004
	(15229)	(14858)	(14536)	(16172)
Other Disability	16396	14119	17293	11939
	(17670)	(18797)	(18328)	(17481)
Investment Income	3897	5188	3738	3945
	(10832)	(12307)	(10526)	(9861)
Public Assistance	1244	2037	304	4603
	(1059)	(1820)	(8)	(3596)

Statistics were constructed using the 2000 March Supplement of the Current Population Survey (reference year 1999). The sample was initially restricted to all civilian males. Statistics reflect the use of person sample weights. Sample standard deviations are in parentheses. * Implies that zeroes amounts were excluded.

Table 3.12: Linear Probability of DC and DP Receipt - March CPS Supplements 1998 through 2006

Dependent Variable	Disability Compensation			Disability Pension		
Vietnam*Posttrend	0.46** (0.22)	0.49** (0.22)	0.48** (0.22)	0.08 (0.14)	0.09 (0.14)	0.01 (0.14)
Korean	-2.15*** (0.52)	-2.15*** (0.52)	-1.40*** (0.63)	-0.08 (0.38)	-0.03 (0.38)	-1.87 (0.49)
Peacetime	-3.69*** (0.42)	-3.72*** (0.42)	-3.21*** (0.46)	-0.80 (0.28)	-0.81 (0.28)	-1.00 (0.32)
Trend	-0.04 (0.12)	-0.05 (0.12)	0.01 (0.12)	0.06 (0.08)	0.06 (0.08)	0.05 (0.08)
Korean*Trend	0.00 (0.15)	0.02 (0.15)	-0.19 (0.17)	0.01 (0.10)	0.02 (0.10)	-0.12 (0.13)
Peacetime*Trend	0.19 (0.13)	0.21 (0.13)	0.11 (0.14)	0.09 (0.08)	0.10 (0.08)	-0.06 (0.10)
Education Fixed Effects	No	Yes	Yes	No	Yes	Yes
Race Fixed Effects	No	Yes	Yes	No	Yes	Yes
Age Fixed Effects	No	No	Yes	No	No	Yes
Year Fixed Effects	No	No	No	No	No	No
Year/Era Fixed Effects	No	No	No	No	No	No
Observations	70451	70451	70451	70451	70451	70451
R-Square	0.01	0.01	0.01	0.00	0.00	0.00

The data are constructed by first pooling the March supplements of the CPS from years 1998 to 2006 (reference years 1997 through 2005). Prior to pooling, family (individual) weights were rescaled to sum up to the total number of family (individual) observations. The sample is then restricted to male veterans born between 1930 and 1959, excluding the few World War II veterans who were born during these years. Although the CPS questionnaire explicitly asks whether one receives disability compensation benefits, the CPS questionnaire asks whether one receives a veterans' pension, which may not necessarily be interpreted as disability pension benefits. The variable Trend increases by 1 for each reference year, starting at 0 for reference year 1997 and increasing to 8 in 2005. The variable Posttrend is equal to 0 for reference years 1997 to 2001 and increases by 1 for each year thereafter, reaching 4 by reference year 2005. Education fixed effects (no high school, high school, some college, and college and beyond), race fixed effects (white, black, and other), and individual age fixed effects may be included as indicated. Robust standard errors are in parentheses.

Table 3.13: Linear Probability of DC and DP Receipt

Dependent Variable	Disability Compensation		Disability Pension	
Veterans Only: YOB 1930-1959 (70451 obs)				
Vietnam*Posttrend	0.46** (0.22)	0.49** (0.22)	0.48** (0.22)	0.09 (0.14)
			0.08 (0.14)	0.01 (0.14)
Vietnam and Nonveterans: YOB 1939-1954 (123394 obs)				
Vietnam*Posttrend	0.63*** (0.23)	0.64*** (0.23)	0.67*** (0.23)	0.02 (0.13)
			0.02 (0.13)	-0.02 (0.13)
Education Fixed Effects	No	Yes	Yes	Yes
Race Fixed Effects	No	Yes	Yes	Yes
Age Fixed Effects	No	No	Yes	No

The data are constructed by first pooling the March supplements of the CPS from years 1998 to 2006 (reference years 1997 through 2005). Prior to pooling, family (individual) weights were rescaled to sum up to the total number of family (individual) observations. The sample is then restricted to male veterans born between 1930 and 1959, excluding the few World War II veterans who were born during these years. Although the CPS questionnaire explicitly asks whether one receives disability compensation benefits, the CPS questionnaire asks whether one receives a veterans' pension, which may not necessarily be interpreted as disability pension benefits. The variable Trend increases by 1 for each reference year, starting at 0 for reference year 1997 and increasing to 8 in 2005. The variable Posttrend is equal to 0 for reference years 1997 to 2001 and increases by 1 for each year thereafter, reaching 4 by reference year 2005. Education fixed effects (no high school, high school, some college, and college and beyond), race fixed effects (white, black, and other), and individual age fixed effects may be included as indicated. Robust standard errors are in parentheses.

Table 3.14: Linear Probability Models of Labor Supply, Income Sources, and Health Insurance Status

Outcome Variable	Labor Supply				Zero Wrk Hours	Any Earnings	Any VA	Any Soc Sec	Sources of Income			Inv Income	Pub Asst
	NILF Retired	NILF Other	NILF Disabled	Any Dis					Any Oth Ret	Any Oth Dis	Any Oth Ret		
Veterans Only: YOB 1930-1959 (70451 obs)													
Vietnam*Post	0.85 (1.00)	0.99 (0.72)	-0.30 (0.39)	0.17 (0.68)	2.99** (1.11)	-2.05* (0.95)	0.19 (0.74)	0.51 (0.65)	-0.45 (0.37)	1.45* (0.84)	-2.39 (1.25)	0.10 (0.13)	
Vietnam*Posttrend	-0.20 (0.38)	-0.25 (0.27)	-0.17 (0.14)	0.22 (0.26)	-0.37 (0.42)	-0.43 (0.36)	0.56* (0.28)	0.36 (0.26)	0.06 (0.14)	-0.23 (0.32)	0.02 (0.47)	-0.08 (0.05)	
Vietnam and Nonveterans: YOB 1939-1954 (123394 obs)													
Vietnam*Post	-0.03 (1.04)	0.37 (0.72)	-0.15 (0.40)	-0.25 (0.73)	2.39** (1.16)	-1.64* (0.99)	0.08 (0.76)	0.89 (0.65)	-0.58 (0.40)	1.79** (0.86)	-1.80 (1.32)	0.07 (0.12)	
Vietnam*Posttrend	-0.32 (0.39)	-0.49* (0.27)	-0.05 (0.15)	0.22 (0.28)	-0.41 (0.44)	-0.39 (0.38)	0.68** (0.29)	0.68*** (0.26)	0.00 (0.15)	0.14 (0.33)	0.00 (0.50)	-0.03 (0.04)	
Education Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Race Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	

These estimates examine the use of comparably aged non-veterans as a comparison group for Vietnam era veterans. The comparison groups are listed in the first column: the first set of results use all other veterans as a comparison group, the second set of results use non veterans born between 1940 and 1950 as a comparison group, and the third set of results use non veterans born between 1939 and 1954 as a comparison group. Each cell contains an estimate associated with the post. policy trend interacted with Vietnam era veteran status. Education, race, and age fixed effects are included as indicated. Robust standard errors are in parentheses.

Linear Probability Models of Labor Supply, Income Sources, and Health Insurance Status (cont'd)

	Poverty Status		Health Status			Health Insurance		
	<.5 of Poverty Line	<1.5 of Poverty Line	Disability	Poor Health	Employer	Private	VA Health	
Veterans Only: YOB 1930-1959 (70451 obs)								
0.28 (0.40)	0.97* (0.59)	1.85** (0.76)	0.36 (0.89)	-0.50 (0.61)	-2.36 (1.27)	-0.08 (0.70)	0.30 (0.58)	
0.11 (0.15)	0.06 (0.23)	0.02 (0.29)	0.43 (0.34)	0.16 (0.23)	-1.43** (0.48)	-0.19 (0.27)	1.13** (0.23)	
Vietnam and Nonveterans: YOB 1939-1954 (123394 obs)								
0.04 (0.43)	0.61 (0.62)	1.78** (0.80)	-0.04 (0.94)	-0.98 (0.65)	-1.58 (1.34)	0.65 (0.72)	0.47 (0.61)	
0.19 (0.17)	0.06 (0.24)	-0.05 (0.30)	0.31 (0.36)	0.18 (0.24)	-1.71*** (0.51)	0.02 (0.27)	1.19*** (0.24)	
Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	

Appendix A

Provisions and Estimated Costs of EGTRRA and Related Acts

The increase and refunding of the child tax credit were the most notable EGTRRA provisions relating to children. When first introduced in 1998, the child tax credit equaled \$400 per child. Unlike personal exemptions and deductions, which reduce TI, the child tax credit reduced taxes owed. Thus, the value of the child tax credit did not depend on a filer's marginal tax rate, and the relative value of the child tax credit declined as TI increased. Because of this, and since the child tax credit phased out at very high levels of income, the child tax credit was originally considered progressive. However, any child credit in excess of taxes owed was not refunded, so individuals with no tax liability prior to EGTRRA did not benefit from the child tax credit.

The EGTRRA provided a phase-in increase of the child tax credit amount and, more importantly, made it partially refundable for some families with little or no tax liability. According to the EGTRRA, the child tax credit was scheduled to increase from \$500 in 2000 to \$600 in 2001 through 2004, \$700 in 2005 through 2008, \$800 in 2009, and \$1,000 in 2010. However, the JGTRRA and WFTRA expedited the increase of the child tax credit amount to \$1,000 for tax years 2003 through 2009. Also, in 2001, the EGTRRA made the child tax credit partially refundable for couples with income TI above \$10,000.¹ The refund amount would equal 10 percent

¹The correct definition for calculating the child tax refund is taxable earned income.

of taxable income above \$10,000 in excess of taxes owed.² Thus, for example, a couple in 2001 with \$17,000 in TI, no tax liability and two children would receive \$700 ($.1 * (\$17,000 - \$10,000)$) of its full \$1,200 child tax credit amount (2 children times \$600). The \$10,000 threshold was indexed for inflation in subsequent years; and the refund rate, originally scheduled to increase from 10 to 15 percent in 2005, increased to 15 percent in 2004 with the implementation of the WFTRA.

Another set of provisions discussed here changed the tax rate schedule. First, the EGTRRA legislation replaced the lower portion of the preexisting 15 percent tax bracket with a new 10 percent tax bracket at TI between \$0 and \$12,000 for married, joint filers.³ The upper bound of this new 10 percent bracket was not indexed for inflation, but it was scheduled to increase from \$12,000 to \$14,000 for married, joint filers in tax years 2008 through 2010. The JGTRRA and WTFRA, however, implemented this increase beginning in tax year 2003. And second, the EGTRRA provided a gradual reduction of marginal tax rates for the remaining tax brackets: the tax brackets in 2000 with marginal tax rates of 28%, 31%, 36%, and 39.6% were scheduled to decline to 25%, 28%, 33%, and 35%, respectively, by 2006. The JGTRRA fully implemented both the increase in the upper bound of new 10 percent tax bracket and the reduction in marginal tax rates in 2003.

Appendix Table A2 provides net total cost estimates for each Act and select provisions from the year of their enactment until 2011. The projected net cost of EGTRRA from 2001 to 2011 was \$1.35 trillion. The estimated cost of the three sets of provisions discussed above - tax benefits relating to children, changes to the income tax rate structure, and marriage penalty relief - account for 82.9% of net total cost of the EGTRRA, with changes to the income tax rate structure alone accounting for over two-thirds (64.9%). Tax provisions related to educational incentives; estate, gift, and generation-skipping expenses; pension provisions; alternative minimum tax relief; and a few other miscellaneous provisions account for the remaining 17.1% of total costs.

The projected costs of the subsequent Acts from the year of their enactment to 2011 - \$349 billion and \$146 billion for JGTRRA and WFTRA, respectively - were modest relative to the ten year cost estimate of the EGTRRA. One reason is that neither of the subsequent Acts repealed the EGTRRA's sunset provision. Also, many of the provisions of JGTRRA were temporary and became inapplicable after tax year 2005. And finally, while the WFTRA accelerated the phase-in of many of the original provisions, many of these provisions were already scheduled to be partially phased in by 2005. Thus, the estimated cost of the WFTRA only reflects the costs in addition to the those already accounted for by the EGTRRA.

²An alternative calculation of the child tax credit refund may be possible for families with three or more children, are ineligible for EITC, and are low income.

³In 2001, the 10% tax rate was implemented through a rate reduction credit - the new bracket does not appear in the income tax schedule until tax year 2002. Also, the upper bound of the 10 percent tax bracket was scheduled to increase with inflation beginning in tax year 2009.

Table A.1: Select Provisions of EGTRRA 2001, JGTRRA 2003, and WFTRA 2004

	EGTRRA - 2001		JGTRRA - 2003		WFTRA - 2004	
	Eff. Date	End Date	Phase-In	End Phase-In	Eff. Date	End Date
Tax Benefits Relating to Children						
Increase in the child tax credit	1-Jan-01	1-Jan-11	Yes	1-Jan-10	1-Jan-05	1-Jan-11
Partial refund of the child tax credit (1)	1-Jan-01	1-Jan-11	Yes	1-Jan-05	1-Jan-04	1-Jan-11
Extension and expansion of adoption tax benefits (2)	1-Jan-02	1-Jan-11	No	-		
Expansion of dependent care tax credit	1-Jan-02	1-Jan-11	No	-		
Tax credit for employer-provided child care facilities	1-Jan-02	1-Jan-11	No	-		
Income Tax Rate Structure						
New 10 percent rate bracket	1-Jan-01	1-Jan-11	Yes	1-Jan-08	1-Jan-08	
Reduction of individual income tax rates	1-Jul-01	1-Jan-11	Yes	1-Jan-06	1-Jan-03	N/A
Phase out of itemize deduction limitations	1-Jan-06	1-Jan-11	Yes	1-Jan-10		
Phase out of personal exemption restrictions	1-Jan-06	1-Jan-11	Yes	1-Jan-10		
Marriage Penalty Relief Provisions						
Increase the standard deduction marriage	1-Jan-05	1-Jan-11	Yes	1-Jan-09	1-Jan-05	1-Jan-09
Expansion of the 15 percent rate bracket	1-Jan-05	1-Jan-11	Yes	1-Jan-08	1-Jan-05	1-Jan-08
Earned income credit expansion for married couples	1-Jan-02	1-Jan-11	Yes	1-Jan-07		

Author's compilation from the Joint Committee on Taxation (2001) JCX-50-01 and (2003) JCX-42-03 and (2005) The General Explanation of Tax Legislation Enacted in the 108th Congress (1) The

EGTRRA made the child tax credit partially refundable starting in 2001 at a 10 percent rate. The WFTRA increased the rate to 15 percent in tax year 2004, one year earlier than legislated by the

EGTRRA. (2) Provisions that extend the adoption credit regardless of whether the taxpayer has qualified adoption expenses become effective in January 1, 2003.

Table A.2: Estimated Costs (in millions) of EGTRRA 2001, JGTRRA 2003, and WFTRA 2004

Years	EGTRRA - 2001 2001-2011	JGTRRA - 2003 2003-2011	WFTRA - 2004 2005-2011
Tax Benefits Relating to Children			
Increase and refund of the child tax credit	171,782	32,488	63,567
Extension and expansion of adoption tax benefits	3,135		
Expansion of dependent care tax credit	2,991		
Tax credit for employer-provided child care facilities	1,405		
Total	179,313	32,488	63,567
Income Tax Rate Structure			
New 10 percent rate bracket	421,321	11,906	29,361
Reduction of individual income tax rates	420,606	74,185	
Phase out of itemize deduction limitations	24,872		
Phase out of personal exemption restrictions	8,140		
Total	874,939	86,091	29,361
Marriage Penalty Relief Provisions			
Inc. the std. marriage deduction and expand 15 percent bracket	47,652	35,074	15,693
Earned income credit expansion for married couples	15,643		
Total	63,295	35,074	15,693
Net Total, Entire Act	1,348,537	349,667	145,942

Source: Author's compilation from the Joint Committee on Taxation (2001) JCX-52-01, (2003) JCX-55-03, and (2004) JCX-64-04. EGTRRA, JGTRRA, and WFTRA figures are in respective year dollars.

Appendix B

2001 Statistics of Income

The Statistics of Income (SOI) is a public-use data file constructed from federal tax returns filed with the Internal Revenue Service. The 2001 version contains 143,218 records representing the 130.6 million returns filed for that year.

The SOI sample is restricted to married, joint filers (1) who filed a Form 1040 for year 2001, (2) who did not receive farm income, (3) whose dependents are own children only, (4) who claimed the itemized or standard deduction, and (5) whose state of residence was reported. The last requirement, a state identifier, was not imposed to construct Figure 1.2.

The table below contains details of this selection process. The first column describes the sample selection criteria. The second column contains the percent of total observations that satisfy the selection criteria with and without using sample weights. The third column is a running tally of observations remaining after each additional criteria is imposed. The remaining 6,384 observations were used to estimate average deductions by state.

I construct summary statistics from the SOI for married, joint filers whose AGI-DER is between \$55k and \$95k in 2002 dollars by increments of \$10k (Appendix Table B1). Again, AGI-DER is defined as taxable wage income plus non-negative values of self-employment income and is the approximation of actual AGI in the CPS-DER. This range of AGI-DER was chosen to adequately span the treatment and comparison groups of interest, evident by the reported estimate of the percent “Treated” (equaling one if actual 2002 TI is between \$47,900 and \$55,394) which across AGI-DER categories increases from 11.9% to 33.5% and 24.3% and then declines to 10.9%. All dollar figures were adjusted from 2001 to 2002 dollars using an average wage index.

I first compare AGI-DER to actual AGI reported in the SOI. According to the SOI, taxable wage income and self-employment income (referred to as business income in Appendix Table B1) represent 95.6% of total income, so AGI-DER appears to be a relatively accurate measure of AGI. However, the omission of certain sources of income results in AGI-DER underestimating AGI across all categories (\$72,699 compared to \$76,466), with the largest disparity occurring among lower levels of AGI-DER.

I then examine average exemptions reported in the SOI. Again, personal exemptions are based on family size and are subtracted from AGI to determine TI.

Reported in Panels C and D, the average exemption was \$9,506, which reflects children living both at and away from home. I then calculate average exemptions based only on children residing in the home, labeled *Exemptions -DER*, which is \$9,426.¹ Comparison of *Exemptions* and *Exemptions - DER* suggests that the inability to observe dependent children living away from home is not a major concern for computing TI.

And finally, I examine the prevalence and amount of itemized deductions. According to the SOI, approximately 81.0% of these filers had itemized their deductions and claimed an average deduction of \$15,262. Thus, as mentioned above, assigning the standard deduction amount of \$7,850 (in 2003) would severely bias the assignment of couples into treatment and comparison groups. To address this concern, I estimate average deduction amounts by state in the SOI and assign them to households by state of residence in the CPS-DER (see Appendix C for details).

¹The personal exemption amount inflated to 2002 dollars is \$2,929 compared to the actual exemption amount of \$3,000 in 2002. For consistency, I calculate *Exemptions -DER* using \$2,929. Reported under *Miscellaneous Statistics* in Panel D, less than 1% of these couples claimed children living away as dependents.

Table B.1: Sample Selection Criteria, Statistics of Income 2001

Sample Criteria	% of Total Sample (Unwgt/Wgt)	Remaining Observations
Total	100/100	143,218
Married, Filing Jointly	65.8/39.2	94,216
File 1040	89.0/62.0	91,216
Tax Year 201	95.4/97.2	87,348
No Farm Income	95.3/98.5	81,736
Dependent Children Only	99.0/97.8	81,387
Itemized or Standard Deduction	94.0/98.9	77,398
State Identifier	48.7/97.5	27,378
\$55k<AGI-DER<\$95k	9.35/13.67	6,384

Appendix C

Computation of Tax Parameters

Marginal and average tax rates were calculated using the National Bureau of Economic Research's TAXSIM model Version 7. Based on certain income and demographic parameters, the TAXSIM model calculates federal, state, and FICA tax liability and marginal tax rates.

Data can be directly submitted to NBER's TAXSIM model to compute tax parameters. However, for confidentiality purposes, the CPS-DER could not be directly submitted to TAXSIM. Instead, artificial data were created that contained all combinations of (1) integers of joint income of \$100 from \$100 to \$200,000, (2) number of eligible dependents for the personal exemption from zero to seven, (3) number of eligible dependents for the child tax credit from zero to seven, and (4) state of residence. Differentiating by state of residence is necessary because state income tax systems vary across states and since deduction amounts were estimated by state. The artificial data were then submitted to NBER's TAXSIM model for the computation of tax liability and marginal tax rates. The returned values were then merged to the CPS-DER sample based on the four dimensions listed above.

According to the SOI, a significant number of married couples itemize their deductions. To address this concern, I estimate average deduction amounts by

Table B.2: Summary Statistics, Statistics of Income 2001

AGI - DER Category	55k-65k	65k-75k	75k-85k	85k-95k	Total
Obs.	1,991	1,761	1,497	1,135	6,384
% , Weighted	30.25	28.58	23.93	17.24	100
Treated (%)	11.9	33.5	24.3	10.9	20.9
A. Income Subject to Taxation					
Taxable Wage Income	57,698	67,454	76,732	86,608	70,024
Interest	676	711	775	783	728
Dividends	319	389	378	533	390
Business Income	1,998	2,198	2,520	2,754	2,310
Capital Gains	464	392	373	420	414
Other Gains	-3	16	-20	-53	-10
Pensions	2,090	1,724	1,567	1,713	1,795
Schedule E	587	369	590	71	436
Other Income*	1,718	1,594	1,388	1,347	1,540
B. Survey of Income: Actual Values					
AGI	64,546	73,726	83,017	92,831	76,466
Exemptions	9,408	9,459	9,551	9,693	9,506
Deductions	13,670	14,670	16,161	17,787	15,262
Taxable Income	42,152	50,363	58,177	66,367	52,507
C. Survey of Income: DER Comparison					
AGI - DER	60,029	69,976	79,677	89,766	72,699
Exemptions - DER	9,325	9,380	9,476	9,612	9,426
Deductions - DER	14,291	14,829	15,317	16,155	15,012
Taxable Income - DER	36,187	45,540	54,655	63,767	48,033
D. Miscellaneous Statistics					
Business Income +/- (%)	23.0	20.0	21.5	20.8	21.4
Business Income - (%)	7.0	5.7	6.5	6.1	6.4
Itemizers (%)	76.5	80.4	83.0	87.0	81.0
AMT (%)	0.3	0.3	1.0	1.1	0.6
Mortgage Interest Paid (%)	72.4	76.7	78.3	80.8	76.5
Child Away from Home (%)	0.8	0.7	1.3	0.7	0.8

state in the SOI and assign them to households by state of residence in the CPS-*DER*. Because deduction amounts generally increase with AGI-*DER*, I estimate the average deduction amount only among the couples whose joint AGI-*DER* falls between \$55k and \$95k. The accuracy of this imputation is improved by exploiting state of residence given the strong correlation between state of residence and average deduction amount. For example, among joint filers with AGI-*DER* between \$55 and \$95k, the average deduction for residents of Texas was \$12,102 (57.9% of them itemizing) compared to \$17,336 (87.8% of them itemizing) in New York.

There are two reasons why the prevalence of deduction itemization varies by state. First, differing state income tax codes generate variation in state taxes owed which may be claimed as a deduction. To estimate the effect of state tax codes on the prevalence of itemization, I regress the proportion of itemizers by state on average state taxes owed among the restricted sample of 6,384 joint filers described in Appendix Table B1. To plausibly surmount the inherent relationship between income and itemization, I calculate the average state tax liability as if all 6,384 couples reside in that state. The estimate suggests that an additional \$1,000 increase in state taxes owed increases the probability of itemization by 5.6% points. Second, the proportion of itemizers may be correlated with the prevalence of home ownership across states since mortgage interest payments are also deductible. A regression of the proportion of itemizers by state on home ownership in the same state indicates that home ownership is associated with a 18.5% point increase in the probability of itemizing.

If the number of observations in a given state was less than 25, the standard deduction was assigned to couples in the CPS-*DER* residing in that state. The states with less than 25 observations include the Alaska, Delaware, the District of Columbia, Montana, North Dakota, Rhode Island, South Dakota, Vermont, and Wyoming. The standard deduction amount was assumed constant in real terms from 2002 and 2004. Since the TAXSIM model uses its own calculated state income tax liability to determine deductions, state income taxes paid were first subtracted from itemized deductions prior to computing marginal tax rates. Comparison of these imputed deductions, *Deductions - DER*, to actual deductions (Panels B and C in Appendix Table B2) suggests that this imputation is fairly accurate on average.

All children residing in the home were considered eligible for the personal exemption or the child tax credit if age appropriate. First, I calculated the children's ages in 2002 and 2004 by adding or subtracting the appropriate number of years to or from the age reported in the CPS. If the child is less than 19 years of age or less than 24 years of age and a student, the child is considered eligible for the personal exemption. If the child is less than 17, then the child is considered eligible for the child tax credit. Since children's ages are adjusted by two years from 2002 to 2004, a child may become newly eligible or ineligible for the personal exemption or the child tax credit from 2002 to 2004. These changes in eligibility are reflected in the

actual and predicted changes in marginal and average tax rates.

Appendix D

Aching to Retire Data Appendix

SSDI Enrollment data by single year of age and gender for the 1983-2004 calendar years were obtained from the Social Security Administration's Annual Statistical Supplement. In producing this data, the SSA uses a ten percent sample of the Master Beneficiary Record (MBR). This same data was tabulated for December of 2005 using the full MBR. For the December 2004 data see

<http://www.ssa.gov/policy/docs/statcomps/supplement/2005/5a.pdf>

Population data by single year of age and gender for the 1990-2005 calendar years was obtained from the National Center for Health Statistics' "U.S. Census Populations with Bridged Race Categories," which can be found at

<http://www.cdc.gov/nchs/about/major/dvs/popbridge/popbridge.htm>

Population data by single year of age and gender for the 1983-1989 calendar years was obtained from the U.S. Census Bureau's quarterly population estimates, which can be found at

<http://www.census.gov/popest/archives/1980s/>

Population data were reported as of July 1 in each year. We therefore took the average of the values in year t and year $t+1$ to estimate the population in December of year t . To estimate the population data in December of 2005, we multiplied the July 1, 2005 estimate for age A by the ratio of the population at age $A-1$ on July 1, 2005 to the population at age $A-1$ on July 1, 2004.

Mortality rate estimates by gender and age were obtained from the Social Security Administration's Office of the Actuary. This data can be found at

<http://www.ssa.gov/OACT/STATS/table4c6.html>

The mortality data used in this paper were updated by SSA on June 27, 2006.

Appendix E

VA Appendix Tables

Table E.1: Service-Connected Disabilities by Body System for DC Recipients at End of Fiscal Years

Body System	1998	1999	2000	2001	2002	2003	2004	2005
Musculoskeletal System	2204797	2280843	2346864	2412412	2524243	2652380	2786986	3002239
Skin	697081	711700	722474	731378	750407	770083	778521	799131
Impairment of Auditory Acuity	463306	483532	505298	530931	587524	665419	742211	822413
Neurological Conditions	302864	313252	322904	331653	369377	422448	581442	521970
Mental Disorders	395329	403175	409071	414679	433618	463223	488333	520497
Digestive System	424188	429546	432920	434606	440931	448128	452307	457934
Cardiovascular System	326947	339195	348645	357259	385924	419039	442640	471455
Respiratory System	286199	293179	298789	303890	314021	325106	334866	347190
<i>Endocrine System</i>	<i>56416</i>	<i>57576</i>	<i>58719</i>	<i>68040</i>	<i>134905</i>	<i>185908</i>	<i>217126</i>	<i>247423</i>
<i>Diabetes Only</i>	-	<i>37808</i>	<i>37985</i>	<i>46395</i>	<i>111932</i>	<i>161551</i>	<i>191649</i>	<i>220532</i>
<i>Diabetes and Vietnam Only</i>	-	<i>18904*</i>	<i>18993*</i>	<i>27409*</i>	<i>88259</i>	<i>135011</i>	<i>163485</i>	<i>190199</i>
<i>Diabetes and All Other</i>	-	<i>18904*</i>	<i>18992*</i>	<i>18992*</i>	<i>23673</i>	<i>26540</i>	<i>28164</i>	<i>30333</i>
<i>Not Diabetes</i>	-	<i>19768</i>	<i>20734</i>	<i>21645</i>	<i>22973</i>	<i>24357</i>	<i>25477</i>	<i>26891</i>
Genitourinary System	132164	136852	141583	145938	161387	180785	196268	214036
Eye	103007	103704	104050	104472	108407	113553	117256	121443
Infectious Diseases	49754	49042	47980	46714	46586	46576	46045	45076
Gynecological System	28939	32004	34547	36667	39325	41905	44156	46880
Dental and Oral Conditions	24715	25798	26798	27572	28924	30171	31114	32211
Hemic and Lymphatic System	20354	20792	21153	21471	22216	23122	24996	25988
Total	5516060	5699958	5842529	5989327	6370768	6812203	7309744	7675787

Data were obtained from the 2000 and 2004 versions of the VBA's Annual Benefits Report. Entries with an asterisk were estimated because they were not publicly available. We assume that the number of Vietnam DC recipients with diabetes is equal to the corresponding number of non-Vietnam DC recipients with diabetes in 1999 and 2000. This is likely to be approximately correct given that 46 percent of DC recipients with diabetes in October of 1998 were from the Vietnam era. We further assume that the number of non-Vietnam veterans with diabetes in 2001 is unchanged from its 2000 level. The values for 2002, 2003, and 2004 are not imputed.

Table E.2: Service-Connected Disabilities by Body System for New DC Recipients in Fiscal Year

Body System	1998	1999	2000	2001	2002	2003	2004	2005
Musculoskeletal System	107400	119485	111663	110520	156339	164970	164297	194331
Skin	32013	34236	30334	28047	41453	42766	36955	41161
Impairment of Auditory Acuity	23370	27321	28654	31995	59241	75316	76836	88366
Neurological Conditions	13124	13567	13261	12927	28794	33575	28922	33602
Mental Disorders	17043	17680	16613	16065	25402	31022	23564	33308
Digestive System	17873	18823	16807	15109	21501	22017	19078	21281
Cardiovascular System	13638	15588	14594	14253	26643	28069	28315	26577
Respiratory System	14855	15842	14423	14190	19304	20678	19239	21903
<i>Endocrine System</i>	<i>2350</i>	<i>2501</i>	<i>2485</i>	<i>5918</i>	<i>39852</i>	<i>36897</i>	<i>26206</i>	<i>26274</i>
<i>Diabetes Only</i>	-	<i>1217</i>	<i>1183</i>	<i>4741</i>	<i>38652*</i>	<i>35697*</i>	<i>25006*</i>	<i>24615</i>
<i>Diabetes and Vietnam Only</i>	-	<i>609**</i>	<i>592**</i>	<i>4150*</i>	<i>38061*</i>	<i>35106*</i>	<i>24415*</i>	<i>24024*</i>
<i>Diabetes and All Other</i>	-	<i>608*</i>	<i>591*</i>	<i>591*</i>	<i>591*</i>	<i>591*</i>	<i>591*</i>	<i>591*</i>
<i>Not Diabetes</i>	-	<i>1284</i>	<i>1302</i>	<i>1177</i>	<i>1200*</i>	<i>1200*</i>	<i>1200*</i>	<i>1659</i>
Genitourinary System	6411	6716	6502	6270	13392	14993	12884	14670
Eye	3129	3314	3043	2998	5320	5708	4774	5529
Infectious Diseases	2486	2524	2280	2081	3300	3233	2702	2705
Gynecological System	2958	3154	2678	2285	2795	2780	2487	2670
Dental and Oral Conditions	1365	1533	1518	1310	2087	1915	1616	1868
Hemic and Lymphatic System	985	1032	1025	923	1262	1484	1822	1814
Total	259000	283316	265880	264891	446685	485423	449697	516059

Data were obtained from the 2000 and 2004 versions of the VBA's Annual Benefits report. Entries with an asterisk were estimated because they were not publicly available. We assume that the number of diabetes awards to Vietnam veterans was the same as the number of diabetes awards to all other veterans in the 1999 and 2000 fiscal years. This is likely to be approximately correct given that 46 percent of DC recipients with a diabetes diagnosis in October of 1998 were from the Vietnam era. We further assume that the number of diabetes awards to non Vietnam era veterans does not change after the 2001 reform, which seems reasonable given the much larger increase in diabetes cases among Vietnam veterans. And finally, we assume that the number of endocrine system awards that are not diabetes remains unchanged following the 2001 reforms. This seems reasonable given that the number of diabetes cases increased by 159,000 from 2000 to 2004 whereas the corresponding increase for other endocrine conditions was approximately 5,000.

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