

# Distinguishing Income from Substitution Effects in Disability Insurance

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A set of studies conducted over the last 15 years has produced a near consensus that the Social Security Disability Insurance system (SSDI) has substantial disincentive effects on the labor supply of near elderly males, diminishing labor force participation, increasing the sensitivity of labor force exit decisions to adverse economic shocks, and encouraging those nearing retirement to claim disability benefits and subsequently transfer into the Social Security retirement program.<sup>1</sup> Yet, efforts by the Social Security Administration (SSA) to encourage labor supply among the disabled by removing the work disincentives built into SSDI have been almost entirely unsuccessful. Most notably, in 1999, Congress authorized the Ticket to Work program, which provides an array of inducements for current SSDI beneficiaries to take up employment, including permitting a trial work period of up to nine months, providing 7.75 years of ongoing Medicare eligibility following return to work, and providing three years of automatic benefit reinstatement when claimants' workplace earnings fall below a threshold level. Each of these steps reduces the implicit tax placed on labor supply by the SSDI program. Despite these lures, fewer than 1,400 (0.01 percent) of the 12.2 million tickets issued to date have led to successful workforce integration (Autor and Duggan 2006).

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<sup>1</sup>On the first point, see John Bound and Timothy Waidmann (1992) and David C. Stapleton and Richard V. Burkhauser (2003). On the second point, see Daniel Black, Kermit Daniel, and Seth Sanders (2002) and Autor and Duggan (2003). On the third point, see Duggan, Perry, Singleton, and Jae Song (forthcoming).

This paper calls attention to, and presents preliminary evidence on, a neglected explanation of why efforts to encourage the disabled to return to work by reducing the implicit tax on labor supply have met with little success. Our core observation is that SSDI, and indeed all nonwork-contingent retirement programs, discourage work through two channels. The first is the canonical substitution effect: because a return to work ultimately means sacrificing benefits (what some beneficiaries call “the cash cliff”), SSDI recipients face a financial incentive to remain nonemployed. The second is the income effect—given the transfer payments and in-kind services (particularly medical care) provided by SSDI, many beneficiaries may prefer leisure to labor or, more precisely, an early retirement, even if work is not implicitly taxed by the SSDI program. Concretely, a hypothetical SSDI beneficiary granted \$12,000 per year in income support plus Medicare benefits covering an average of \$7,700 annually in health care expenses may prefer an early retirement over continued participation in the labor force. This scenario seems particularly plausible when one considers that the modal SSDI recipient is a near-elderly male with a high-school education (thus, below-median potential earnings on average) and possibly a significant degree of physical discomfort in performing workplace tasks.<sup>2</sup>

The distinction between these two channels—income and substitution effects—through which SSDI reduces labor supply and expedites early retirement is central to policy. To our knowledge, all prior efforts by Congress and the SSA to increase labor force participation among SSDI recipients—including the Ticket to Work program—have targeted the substitution effect. That is, they have reduced the implicit tax on

<sup>2</sup> Reinforcing this point, Mark Aguiar and Erik Hurst (2005) demonstrate that the fall in living standards that workers experience at the time of retirement is much less precipitous than measured declines in expenditure would suggest.

work. Such policies rest on the assumption that, were it not for the implicit tax that SSDI levies on labor supply, many beneficiaries would prefer to work (i.e., while keeping their benefits). If, however, the primary means by which SSDI reduces labor force participation and hastens retirement is through an income effect, such efforts may be close to ineffectual.

The conceptual distinction between income and substitution effects is also central to welfare analysis. If SSDI reduces labor supply through the substitution effect, this implies a deadweight loss. In effect, SSDI pays beneficiaries *not* to work. By contrast, reductions in labor supply that are due to the income effect do not imply a deadweight loss since there is no distortion of incentives (though, of course, the funding of transfer programs may incur deadweight losses).

### **I. Estimating Income Effects of Disability Insurance Receipt on Labor Supply**

We know of no research that attempts to distinguish income from substitution effects in the relationship between SSDI receipt and labor supply (aside from our companion work on this topic, Autor and Duggan 2007).<sup>3</sup> A likely reason is that, since its inception, the SSDI program has provided benefits exclusively on a work-contingent basis, so income and substitution effects cannot readily be separated. SSDI is not, however, the sole transfer program that provides income support to the nonelderly disabled. Though almost ignored by researchers, the US Department of Veterans' Affairs Disability Compensation program (VDC) provides substantial cash benefits and health insurance through the Veterans Health Administration to more than 2.7 million disabled veterans of military service. Unlike SSDI benefits, VDC benefits are not work-contingent (nor are they means tested). Hence, any reduction in labor supply—generally in the form of early retirement or a shift to part-time work—caused by the award of VDC benefits is plausibly attributable to the pure “income effect” of receiving an unconditional, lifetime grant of monthly income and health insurance.

The key requirement for VDC eligibility is that a veteran's disability must be caused or aggravated by military service. Due to this stipulation, veterans rarely qualify for VDC benefits for medical conditions that develop late in life, such as cancer or diabetes, since these conditions are not normally directly attributable to military service. In 2001, a unique policy change within the VDC program unexpectedly extended cash disability benefits and enhanced medical care to near-elderly veterans of the Vietnam era. In response to a National Institute of Medicine study linking exposure to Agent Orange (a herbicide used extensively in Vietnam) to diabetes, the Department of Veterans' Affairs added diabetes to the list of conditions for which a veteran who served in the Vietnam War could qualify for (or increase) VDC benefits.

As Figure 1 demonstrates, the unanticipated extension of benefits in 2001 coincided with a sharp break in trend in VDC enrollment. While in the four years prior to the policy change (1997 through 2001), the number of VDC beneficiaries grew at only 0.6 percent annually, the annual growth rate jumped to 3.2 percent between 2001 and 2006. Estimates by Duggan, Robert Rosenheck, and Perry Singleton (2006) suggest that the 2001 policy change increased the number of Vietnam veterans in the VDC program in September of 2006 by approximately 175,000 over what it would otherwise have been (2.3 percent of all Vietnam-era veterans still alive at that time). An additional 75,000 Vietnam veterans (1.0 percent) who were already receiving VDC received an increase in their benefits as a result of the Agent Orange decision.<sup>4</sup> This policy change provides an opportunity to study the income effect of receipt of disability benefits on the labor supply and retirement decisions of a relevant population of near-elderly individuals, the majority of whom were work-capable at the time of benefit receipt though not necessarily in good health.

<sup>3</sup> Dora L. Costa (1995) estimates sizable income effects of pension benefits on the labor supply of Union Army soldiers following the US Civil War. Raj Chetty (2005)

distinguishes income from substitution effects in the effect of unemployment insurance benefits on job spell durations.

<sup>4</sup> In total, approximately 11 percent of individuals who had “boots on the ground” in Vietnam during the conflict were affected directly.

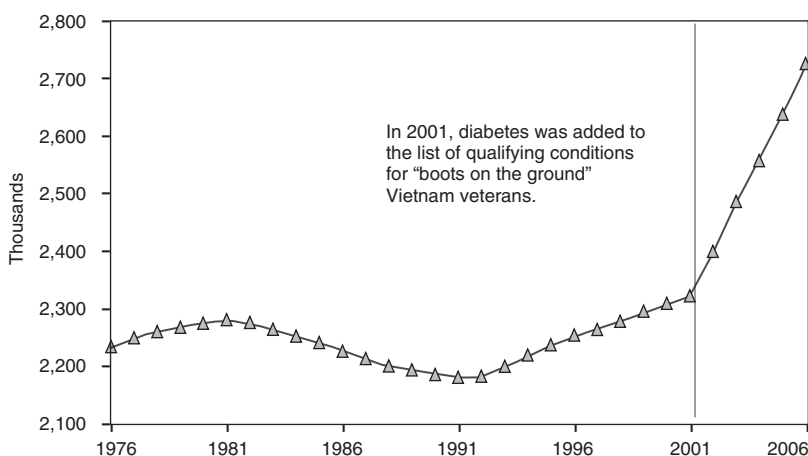


FIGURE 1. NUMBER OF VETERANS DISABILITY COMPENSATION RECIPIENTS, 1976–2006

Source: US Department of Veterans Affairs, published and unpublished data.

## II. Preliminary Analysis Using the Current Population Survey

Our informal discussion above, and a formal model given in Autor and Duggan (2007), suggests that unanticipated increases in unearned income caused by the change in the VDC program should have reduced labor supply among a subset of individuals affected by the policy *despite* the absence of nonwork incentives in the VDC program. To provide an initial test of these labor supply effects, we utilize data from the annual March Supplement to the Current Population Survey (CPS) for calendar years 2000 through 2006 (earnings years 1999 through 2005). The March CPS has a number of virtues for our analysis. It offers detailed individual-level information on labor supply measures and it collects information on each person's veteran status and lists their service era if relevant (e.g., Vietnam, Korea, etc.) It also provides detailed self-reported information on numerous components of unearned income, including benefits from the Veterans Administration (VA), Social Security, and all other retirement and disability income.

Because 97 percent of Vietnam-era veterans are males, according to the March 2001 CPS, we focus exclusively on males in our empirical analyses. Additionally, nearly 80 percent of Vietnam-era veterans were born between 1941

and 1952, and 30 percent of all currently living men born between 1941 and 1952 and alive in 2001 were Vietnam-era veterans (with a maximum of 44 percent for the 1947 birth cohort). We therefore further restrict attention to Vietnam-era veteran males born between 1941 and 1952.

We compare labor supply measures for Vietnam-era veterans before and after the Agent Orange policy change with the corresponding trends for observably similar individuals who were not directly affected. The use of an appropriate control group should serve to capture the effect of other factors, such as aging and macroeconomic conditions, that might also have influenced the labor supply decisions of near-elderly men during our study period. Perhaps the ideal control group for our analysis would be Vietnam-era veterans who did *not* serve in Vietnam, as these individuals were in the military during the same period as “boots on the ground” veterans, but their VDC coverage was not expanded similarly by the Agent Orange policy. Unfortunately, the March CPS does not provide information on where veterans actually served (only when), so we must select an alternative control group.<sup>5</sup>

<sup>5</sup> Our in-progress work in Autor and Duggan (2007) exploits detailed data from the US military to individually identify Vietnam-era soldiers who did and did not have “boots on the ground.”

In the absence of the preferred comparison group, there are two other logical candidates: other veteran males born between 1941 and 1952 and nonveteran males born during this same period. Unfortunately, there are relatively few veterans from other service eras in these 12 birth cohorts. In the 1946 to 1949 birth cohorts, the ratio of Vietnam-era veterans to all other veterans is 14.4 to 1. In contrast, the number of nonveteran males born during this period is substantial and exceeds the number of Vietnam-era veteran males in every one of our 12 birth cohorts of interest. We therefore use nonveteran males as a control group, but we emphasize that the comparison is not ideal. For example, according to the March 2001 CPS, just 4 percent of Vietnam-era veterans born between 1941 and 1952 are high-school dropouts versus 15 percent of nonveteran males born during these same years. Interestingly, nonveteran males are much more likely to have a college degree than veterans (35 percent versus 28 percent). Recognizing that our treatment and control groups differed in potentially important ways prior to the policy change, we view this CPS-based analysis as exploratory, with definitive evidence awaiting better data.

We estimate specifications of the following type:

$$\begin{aligned}
 (1) \quad Y_{it} = & \beta_0 VEV_{it} + \beta_1 VEV_{it} \times POST_t \\
 & + \mathbf{A}_{it} + \mathbf{S}_i + \mathbf{R}_i + \lambda_t + (\mathbf{A}_{it} \times \lambda_t) \\
 & + (\mathbf{S}_i + \lambda_{it}) + (\mathbf{R}_i \times \lambda_t) + \varepsilon_{it}.
 \end{aligned}$$

In this equation,  $i$  and  $t$  denote individuals and years, respectively. The variable  $VEV_{it}$  takes on a value of one if individual  $i$  is a Vietnam-era veteran and zero otherwise. We define the variable  $POST$  to equal zero in both 1999 and 2000, prior to the policy change, and to equal one from 2002 through 2005. Since the policy change occurred in July of 2001, we set the  $POST$  variable to 0.5 in this year. All specifications control for a vector of background characteristics including 12 single year-of-age indicators ( $\mathbf{A}$ ), four education indicators ( $\mathbf{S}$ ), and three race indicators ( $\mathbf{R}$ ), each interacted with a vector of seven year indicators ( $\lambda$ ).

The coefficient of interest in this equation is  $\beta_1$ , which measures the differential change in the outcome variable of interest,  $Y$ , for Vietnam-

era veterans following the policy change. The identifying assumption of this model is that, absent the policy change, the change in  $Y$  would have been comparable for Vietnam-era veteran males and nonveteran males after controlling for the interactions of race, education, and single year of age with year. Under these assumptions,  $\beta_1$  measures the average causal effect of the change in the VDC program on Vietnam-era veterans.

The first column presents estimates for the probability of being out of the labor force during 1999 through 2005. The highly significant point estimate of  $-3.21$  for  $\beta_1$  indicates that between 2001 and 2005, labor force participation of Vietnam-era veteran males fell by more than 3 percentage points relative to that of similarly aged nonveteran males. Notably, labor force participation of Vietnam-era veterans was slightly higher than that of nonveterans in the pre-policy period (0.36 percentage points), but this contrast was not significant.

Columns 2 and 3 reveal that the differential decline in labor force participation among Vietnam-era veterans is accounted for, in roughly equal proportions, by a rise in the probability of being retired and a rise in the probability of being out of the labor force due to disability. Notably, veterans did not become significantly more likely than nonveterans to be out of the labor force for reasons other than retirement or disability (see column 4).

Complementing these findings, column 5 shows that, after 2001, there is a significant increase in the probability that Vietnam-era veterans report a work-limiting disability or health condition relative to nonveteran males of the same age. This finding may indicate that there was a differential decline in the health of veterans after 2001. Or it may reflect the fact that, all else equal, those who receive disability payments are more likely to report themselves as disabled (John Bound and Timothy Waidmann 1992; Michael Baker, Mark Stabile, and Catherine Deri 2004). In untabulated results, our March data also show a significant differential rise after 2001 in the probability that Vietnam-era vets received unearned income from the Veterans Administration or the Social Security Administration. These increases likely reflect a combination of disability and pension payments.

TABLE 1—MEASURES OF LABOR FORCE ATTACHMENT BEFORE AND AFTER THE 2001 POLICY CHANGE:  
CONTRASTING VIETNAM ERA VETERAN VERSUS NON-VETERAN MALES

	(1) NILF	(2) NILF–Retired	(3) NILF–Disabled	(4) NILF–Other	(5) Any disability
Vietnam-era veteran	0.36 (0.64)	0.92* (0.41)	0.09 (0.47)	−0.66* (0.27)	1.47* (0.59)
VEV * Post	3.21* (0.85)	1.77* (0.59)	1.31* (0.61)	0.12 (0.34)	1.30* (0.75)
Mean	20.1	8.6	8.6	2.8	13.5
R-squared	0.091	0.086	0.068	0.008	0.062

*Notes:* Table entries represent coefficient estimates from linear probability models. The dependent variable for each specification is listed at the top of each column. “NILF” denotes Not in Labor Force. All specifications include race (three categories) by year interactions, education (four categories) by year interactions, and age (12 single-year categories) by year interactions. The number of observations is 75,952 in all specifications. Sample includes all Vietnam-era veteran males and all nonveteran males born between 1941 and 1952 inclusive (with year-of-birth approximated as survey year–1–age). Seven years of the March CPS (2000–2006) are used. Specifications are weighted by person weights (scaled by the inverse of the sum of person weights for the sample) and robust standard errors are included in parentheses. Coefficient estimates and standard errors are multiplied by 100 and thus should be interpreted as percentage points.

\* Significantly different from zero at the 5 percent level.

One item of concern for our empirical strategy is also visible in Table 1. Column 5 reveals that, even prior to the 2001 policy change, Vietnam-era veterans were significantly more likely (1.47 percentage points, or 11 percent) than nonveteran males to report a work-limiting disability. This baseline difference is by no means puzzling. Many Vietnam-era veterans saw combat, which is known to have lasting, negative, long-term effects on health (Kelly Bedard and Olivier Deschênes 2006). Nevertheless, this contrast underscores that nonveteran males provide an imperfect comparison group for veteran males of comparable age.

### III. Conclusions

Our results provide initial evidence that the increase in unearned income resulting from the expansion of the VDC program’s medical eligibility criteria in 2001 substantially reduced labor-force participation among Vietnam-era veterans. Such large behavioral responses to the VDC program are noteworthy given that the program does not affect the incentive to work as does the SSDI program and its means-tested counterpart the Supplemental Security Income program. These findings, therefore, highlight the possibility that income effects on labor supply may be sizable for near-elderly adults in moderate to poor health.

We stress that these results must be viewed as preliminary. Perhaps the most important limitation of our analysis is that nonveteran males differ in many observable and presumably unobservable ways from Vietnam-era veteran males. Thus, the differential declines in labor supply observed here may have occurred even in the absence of this policy change. A definitive test of the labor-supply response to the extension of VDC benefits awaits better data (which we are currently compiling).

Accurately assessing the magnitude of income and substitution effects of receipt of transfer income on labor supply is critical to US disability policy. While economists have typically regarded the substantial reductions in labor force participation associated with receipt of disability benefits as an incentive problem (i.e., a substitution effect), it appears plausible to us that a significant share of this response is explained by the (nonincentive) income effect. When granted permanent, inflation-indexed income and government-provided health insurance, many near-elderly adults in moderate to poor health may prefer an early retirement to continued labor force participation. If so, there may be limited scope for public policy to increase a return to work among nonelderly disability recipients by reducing the implicit tax on labor income as is done, for example, by the Social Security Administration’s Ticket-to-Work program. For this reason and the others outlined above,



further study of the effect of the labor supply effects of the VDC program—which currently provides cash benefits and health insurance to more than 11 percent of military veterans—is warranted.

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