# How does Access to Short Term Disability Insurance Impact SSDI Claiming?*

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Abstract

More than 40 percent of full-time workers in the U.S. receive short-term disability (STD) insurance through their employers. Distinct from private long-term disability or Social Security Disability Insurance (SSDI) benefits, worker can access STD benefits with virtually no waiting period, which may enable workers to overcome temporary health-related work limitations and subsequently return to work. Alternatively, STD benefits may provide a pathway out of the labor force that ultimately encourages SSDI claims. We empirically assess whether the provision of STD benefits increases or reduces inflows onto the SSDI system by exploiting cross-state, cross-sector variation in STD coverage stemming from coverage mandates in five U.S. states. Initial results find that policy-induced increases in STD coverage decrease inflows into the SSDI program. Subsequent analysis, however, indicates that this relationship is not reliable: the estimated effect on SSDI enrollment is implausibly large; and the reduction in SSDI enrollment appears to occur among groups that do not see an increase in STD utilization. We caution against viewing the current results as reliable, but conclude that the question of how STD coverage affects the SSDI program remains important, and we touch on potential alternative research strategies.

Keywords: Disability, Short-Term Disability, Insurance, Program Interactions

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The Social Security Disability Insurance system (SSDI) is one of the largest social insurance programs in the United States. In 2012, SSDI cash transfer payments totaled $137 billion, while the cost of Medicare for SSDI beneficiaries added another $80 billion. Since 1990, SSDI outlays grew at 5.6 percent per year in real terms, compared to just 2.2 percent for all other Social Security spending. As a result, SSDI's share of total Social Security outlays has risen from one in ten dollars in 1988 to almost one in five dollars today. Moreover, SSDI expenditures now exceed the payroll tax revenue dedicated to funding the program by more than 30 percent, with the program's trust fund projected to be exhausted in 2016. As a result, it is critical to assess options that can reduce or even reverse the rapid growth of expenditures on this program.

One such option is to address disabilities earlier. Under the current SSDI system, workers must be unable to work for at least five months to apply for benefits, at which point they may face many more months of an uncertain application process. But more than 40 percent of full-time workers in the U.S. have short-term disability (STD) insurance through their employers. The typical STD policy has a maximum duration of 26 weeks, a replacement rate of 60 percent, and a maximum weekly benefit of $550. In contrast to private long-term disability or Social Security Disability Insurance (SSDI) benefits, there is essentially no waiting period for STD benefits.

Making STD available without a waiting period for access to benefits may enable workers to overcome temporary health-related work limitations and subsequently return to work rather than losing employment, thereby discouraging inflows onto SSDI. This possibility is of particular interest in light of recent policy proposals that focus on providing individuals suffering from work-limitations with rapid access to income replacement, workplace accommodations and

vocational rehabilitation so as to enable ongoing employment and thus discourage SSDI applications.

Conversely, providing short-term disability insurance could also increase use of the SSDI system. If receipt of STD leads workers' skills to atrophy or reduces workers' commitment to employment, this would likely increase SSDI claims. STD might also increase SSDI claims by improving the financial circumstances of potential claimants during the lengthy application process. The long wait for SSDI benefits may discourage applications from those with limited financial ability to endure the extended period of non-employment while awaiting an award. If STD in part relaxes this liquidity constraint by providing partial income replacement for several months during the SSDI application process, this might encourage additional applications.

Thus, an important question is whether STD programs are a substitute for, or a pathway onto, longer term SSDI receipt. Accurately estimating the effect of STD coverage on participation in the SSDI program is difficult, however, because workers employed by firms that offer STD coverage are likely to differ in many respects from their counterparts at firms that do not offer this coverage. As a result, a simple comparison of workers with and without STD coverage is unlikely to yield reliable estimates of the causal impacts of STD coverage on SSDI accessions.

To address this issue, this study exploits policy-induced variation in STD coverage. Because this source of policy-induced variation should be unrelated to workers' underlying health or demand for disability benefits, it may potentially inform the question of how providing access to STD affects workers' propensity to obtain SSDI benefits. There are currently five states in the U.S. that require employers to provide and/or finance STDI coverage for their workers. Approximately 25 million workers in the states of California, Hawaii, New Jersey, New York,

and Rhode Island have this coverage, and total expenditures by these programs exceeded $6 billion in 2009 (SSA, 2012). An examination of data from the Bureau of Labor Statistics suggests that these state policies induce significant variation across states with respect to STD coverage. For example, in the Middle Atlantic region (which includes New York and New Jersey as well as Pennsylvania) the fraction of workers with this coverage is 68 percent versus just 33 percent in the South Atlantic.[1] The corresponding variation is minimal for private long-term disability coverage, with 30 percent covered in the Middle Atlantic and actually somewhat higher 35 percent coverage in the South Atlantic. Given the very similar rates of private LTD coverage between these two regions, it seems unlikely that the large differences between them in STD coverage are simply driven by differences in employer or worker characteristics.

By itself, this cross state variation in STD coverage is not suitable for assessing the impact of STD on SSDI enrollment since the states that mandate STD coverage may themselves differ from other states along a number of dimensions that impact SSDI receipt. We therefore pursue a "differences-in-differences" (DD) strategy that exploits cross-sectoral variation in the voluntary rate of STD benefits in conjunction with the policy-induced, cross-state variation in STD coverage. In particular, we use private data tabulations from the Bureau of Labor Statistics to show that there is enormous variation across sectors in voluntary STD provision in states that do not mandate STD coverage. For example, excluding the mid-Atlantic and western regions, which contain virtually all STD-mandated states, STD coverage rates vary from a low of 4% in repair and maintenance to a high of 94% in rail transportation. In the STDI states, by contrast, coverage is virtually 100% in all sectors. If these sectors are otherwise similar across the two

[1] BLS estimates that STD coverage is actually quite low in the Pacific region at just 28 percent. This is presumably because virtually all of the STD coverage in California is through a state-operated fund, and thus is not a benefit provided by employers. Despite this, it is still a source of short-term disability insurance for individuals who are unable to work because of an injury, illness, or disability.

groups of states, then differences between them in SSDI receipt should plausibly reflect the influence of STDI and not the effect of other factors.

We implement this test using data from years 2001 through 2011 of the Current Population Survey (CPS) Annual Social and Economic Supplement, which collects information on income receipt for workers and non-workers alike. We match information on sectoral STD coverage rates to the CPS data to estimate difference-in-differences models of receipt of both state disability income and SSDI income. We find strong evidence that having legislatively induced STD coverage increases the rate of STD receipt. We additionally find some evidence that this STD coverage is also associated with lower SSDI receipt. But this evidence is unfortunately not very convincing: the patterns by gender between STD receipt and SSDI receipt do not match; there is a robust but wrong-signed impact of STD coverage on reported disability rates; and we fail a critical falsification test in terms of observable worker characteristics. Despite the face validity of the research design, we conclude that our empirical strategy is insufficient to convincingly estimate the causal impact of STD availability on SSDI receipt.

## Part I: Empirical Strategy

The objective of our study is to assess how the availability of employer-provided STD coverage affects the likelihood that workers subsequently obtain SSDI benefits. To confront this question using longitudinal data on workers' STD coverage and SSDI receipt would require overcoming two key empirical limitations. First, it would demand longitudinal data on worker SSDI receipt over a large sample and a fairly long time period, which is rarely available. Second, it would likely suffer from omitted worker and firm characteristics that are correlated with both STD availability and SSDI receipt. For example, workers at firms that offer STD benefits may be

of higher skill and therefore less likely to go on SSDI independent of any STD effects. Or workers who are at higher risk of disability may self-select into firms that offer STD coverage, leading them to be more likely to end up receiving SSDI.

To address the former concern, we move from a firm-level perspective to a sector-level perspective. That is, we assess whether workers are more or less likely to receive SSDI benefits as a function of sectoral variation in STD coverage effects. This is simply a sector-level aggregation of the firm-level approach suggested above, and thus does not require longitudinal worker or firm level data. But it does not solve the second problem: workers are still likely on average quite different in sectors with and without different rates of STD coverage, meaning that there may be a unobserved correlation between workers' sectoral affiliation and their probability of requiring SSDI.

To address this concern, we make use of the state-level STD coverage laws in California, Hawaii, New Jersey, New York and Rhode Island. In those states, STD is made available to virtually all workers by state mandate. In California, the insurance is provided by a state fund, paid for by mandated employer contributions; in the other states, there is mandatory purchase (or employer self-provision) of STD coverage. The relatively minor exclusions from these laws include some domestic workers, workers with very low earnings, and some farm workers, student workers, and other small categories.

While it might be tempting to compare SSDI receipt in the five states with mandatory STD to states that do not have this mandate, this comparison is unlikely to be informative because of underlying differences in worker characteristics across states. In particular, the mandatory STD states are on average high wage, high education states, and are therefore likely to have relatively low SSDI receipt rates independent of any differences in STD coverage.

To overcome this hurdle, we apply a difference-in-differences strategy that combines both the sectoral and state variation in STD coverage. In particular, we contrast the same sector across states that do and do not have mandated STDI programs, controlling for both national differences across sectors, and overall differences across states. For sectors with low voluntary STD coverage, the STDI laws should cause a large rise in STD coverage rates. For sectors with high voluntary STD coverage, the STDI laws should lead to a smaller increase in STD coverage rates. Therefore, we can model SSDI receipt as a function of the mandated increase in STD coverage by state and sector.

It is instructive to consider our empirical strategy with a two-sector, two-state example. Suppose that in the construction sector, just 10 percent of workers have STD coverage in those states without an STD mandate, while the corresponding coverage rate in the finance sector is 80 percent. With these coverage rates, the difference in STD coverage between states with and without a mandate would be much higher in the construction sector. To the extent that STD increases (lowers) the likelihood of SSDI receipt, one would expect to see higher (lower) rates of SSDI enrollment among construction workers in states with mandatory TDI. The same would also be true for the finance sector, though the difference would be smaller.

To implement this strategy in practice, we estimate regressions of the following form:

$$(1)\quad Y_i = \alpha + \beta \times \mathrm{COVGAIN}_{j(i),s(i)} + X_i\lambda + \delta_{s(i)} + \gamma_{j(i)} + \varepsilon_{i,j(i),s(i)}$$

where $i$ indexes individuals, $j(i)$ denote $i's$ sector of employment, $s(i)$ denotes $i's$ state of residence, $X$ is a set of individual covariates, and $\delta$ and $\lambda$ are vectors of state sector dummies. The outcome measure $Y$ is one of several dependent variables described below. COVGAIN is a measure of the increase in coverage predicted for a sector from being in an STDI state. It is defined as:

$$\text{COVGAIN}_{j,s} = \begin{Bmatrix} 0 \text{ if } s \text{ is not an STD mandate state} \\ (100 - \text{National coverage rate in } j) \text{ if } s \text{ is an STD mandate state} \end{Bmatrix}$$

If states have no legislated STDI program, then COVGAIN is zero. But if states have a STDI program, then the gain in coverage is defined as 100 (the percentage coverage rate in STDI states) minus the sectoral average coverage rate in states where there is no STDI mandate.

We estimate this equation using a linear probability model. The regression also controls for a full set of worker characteristics: sex, age, race/ethnicity, education, and marital status (detailed in a later section of this report). Inclusion of a full set of state and sector fixed effects allows us to control for variation in COVGAIN that arises solely from sectoral or state differences and to account for differences across states in the fraction of workers in each sector. The identifying variation in COVGAIN stems from the contrast between STD coverage rates in each sector between mandatory STD and non-mandatory STD states. Sectors that have low coverage in non-mandatory states will have substantially higher STD coverage in mandatory STD states; conversely, sectors that have high coverage in non-mandatory states will comparable coverage in mandatory STD states. To the extent that mandatory STD coverage increases an outcome variable such as SSDI enrollment, one would a positive estimate for $\beta$ in equation (1) above.

This identification strategy embeds an important assumption: there is no systematic correlation between COVGAIN and the underlying propensity of workers (absent STD coverage) to receive SSDI. To see how this assumption could be violated, suppose that in STDI states the sectors with low voluntary STD coverage elsewhere (e.g. performing arts, spectator sports, and related industries) attract healthier workers. These sectors would see relatively low SSDI accession rates, causing a spurious negative correlation of COVGAIN and SSDI receipt. There is no perfect way to address this identification concern, but we carry out two tests below to check for likely violations of this assumption. Unexpectedly, these tests suggest that the key identifying

assumption of our identification strategy may in fact be violated, which unfortunately limits our confidence in the primary findings.

## Part II: Data

*Current Population Survey Data*

Data are drawn from the Current Population Survey (CPS) between the years 2002 and 2012. The CPS is a large, national survey that is administered to roughly 60,000 households each month by the United States Census Bureau on behalf of the Bureau of Labor Statistics (BLS). The BLS utilizes the basic monthly CPS to construct measures of activity in the U.S. economy (e.g., unemployment rate, number of long-term unemployed workers). In addition to the basic monthly CPS, the BLS administers supplements to measure important social and economic indicators. We take advantage of the Annual Social and Economic ("March") Supplement (henceforth the "supplement"), which collects detailed information on income levels and sources, health insurance coverage, and program participation in addition to basic demographics and employment information. Because the supplement includes income information for the previous year, our data capture income received between 2001 and 2011 (thus the year fixed effects in our regression model outlined in an earlier section of this report are in fact lagged one year). Data were extracted from the National Bureau of Economic Research NBER CPS Supplements data archive (http://www.nber.org/data/current-population-survey-data.html; accessed 4/20/2013).

The pooled 2001 to 2011 supplement data set includes 2,303,241 respondents. We make several restrictions on the pooled sample to construct our analytic sample. To focus on those individuals who have completed their education and have not yet transitioned into retirement, we

exclude respondents younger than 23 years and older than 64. A critical component of our analysis is the ability to merge sector-level STD coverage rates from the BLS (detailed in a later section of this report) into the supplement. We therefore exclude respondents who do not provide a valid industry of longest duration held in the past year (this survey question is asked to respondents regardless of employment status at the time of the survey). We additionally exclude respondents with missing personal characteristics (detailed in a later section of this report), and those employed in the small subset of sectors for which the BLS does not provide STD coverage rates (for example, military and farm sectors). The CPS does not regularly collect information on the duration of time spent outside the labor market beyond one calendar year prior to the survey date. However, in the Outgoing Rotation Group (ORG)[2] of the CPS, respondents who report being out of the labor force—that is, they are neither employed nor unemployed at the time of the survey—and who have not held a job in the past five years, are asked for information on the industry of their longest job. Thus, to exclude respondents with minimal attachment to the labor market we first restrict our sample to the ORG and second exclude respondents who have not held a job in the past five years. Our final analysis sample size includes 240,972 individuals over an eleven-year period.[3]

*Income variables*

Our outcome variables include indicators for any STD and Social Security (SS) income receipt in the previous calendar year. A limitation of the CPS, like many other social science

[2] CPS respondents are interviewed monthly for four months, not interviewed for four months, and then interviewed for four months. Thus the ORG sample is a combination of respondents entering the four month period in which they are not interviewed and respondents who are exiting the CPS survey entirely.

[3] We exclude 1,063,454 respondents younger than 23 years and older than 64 years; 237,596 respondents with an invalid industry value; 40,194 respondents that we cannot match their industry to our private tables from the BLS; 1,352 respondents with missing personal information (e.g., education); 713,457 respondents who are not in the OGR group; and 6,216 respondents who have not held a job in the past 5 years.

surveys, is that it imperfectly measures SSDI coverage, SS coverage and program participation more broadly. Therefore, although we selected the most appropriate variables available in the CPS we likely measure both STD and SS with some error. Additionally, because our income measures pertain to the previous calendar year they may be vulnerable to recall bias.

To proxy STD claiming, we utilize the question "Other than social security or veterans' assistance benefits did you receive any income in 20XX [where 20XX is the year prior to the survey] as a result of health problems?" We code respondents one if they report this source of income, and zero otherwise. We measure SSDI claiming with the following survey question "Did you receive Social Security Income in 20XX?"[4] We construct an indicator variable coded one if the respondent reports this source of income in the previous calendar year, and zero otherwise.

*Industry coverage*

We obtained private tabulations on sector STD coverage rates based on the 2012 National Compensation Survey from the BLS. These data provide the share of workers in each of 77 sectors with access to STD coverage through employers. Sectors are measured at the three-digit 2007 NAICS codes. Our coverage rate calculations exclude the Mid-Atlantic and Western regions, since the former is dominated by New York and New Jersey, and the latter by California, all of which have STD mandates. Sectoral coverage rates range from 4% to 94% of

[4] This will lead us to incorrectly code some individuals who are receiving Social Security for another reason, such as early retirement from the age of 62 to 64 and survivors benefits. An examination of data from the Social Security Administration reveals that 62 percent of non-elderly adults receiving social security are on SSDI. The March supplement to the CPS asks respondents through which program they are receiving social security benefits but the monthly surveys do not.

employees. See Table 1 for sectors and their related STD coverage rates. We merge these data into the CPS on an individual's industry of longest duration.[5]

*Personal Characteristics*

We construct measures of personal characteristics that are conceptually related to our income measures. Specifically, we include age in years, indicators for race/ethnicity (African American, other race, and Hispanic, with White race as the omitted category), indicators for educational attainment (high school, some college, and a college degree, with less than high school as the omitted category), and marital status indicators (divorced/separated/widowed and never married, with married as the omitted category). All personal characteristics pertain to the interview date (thus they are measured three months following the period in which income was received).

Table 2 provides summary statistics for our full analysis sample, and separately for men and women. STD claiming is relatively rare in our sample: roughly 0.25% of the sample reports this source of income and the prevalence rate is comparable across the sexes. 1.3% of the full sample reports any SS income in the past year, and the prevalence ranges from 1.0% among men to 1.6% among women.[6]. Roughly 33% of our sample has access to STD coverage, and 22% resides in a state with an STD mandate in place. The average age is 42 years and the breakdown by sex is 53% male and 47% female.[7] The racial and ethnic breakdown is 82% White, 11%

[5] The CPS collects industry information using Census industry codes. The BLS coverage rate tabulations are available at the three-digit NAICS code level. We utilize a crosswalk provided by the U.S. Census to map our CPS Census codes to the three-digit NAICS codes. Moreover, between the 2002 and 2003 supplements the CPS moved from using the 1990 Census industry codes to the 2000 Census industry codes.. We utilize an additional crosswalk to map from the 1990 to the 2000 Census codes. We achieved a match rate of over 95%. More details on our matching procedure are available on request.

[6] Social Security income is substantially underreported in the Current Population Survey.

[7] The slightly skewed sex breakdown is likely attributable to our sample selection restrictions, which exclude respondents who have minimal attachment to the labor market.

African American, 7% other race, and 13% Hispanic. 9% of the sample reports less than a high school diploma while 29%, 29%, and 34% report a high school diploma, some college, and a college degree or higher. Lastly, 60% of the sample is married while 18% is divorced, separated, or widowed and 23% has never married. These patterns are broadly comparable for the male and female samples.

## Part III: Results

Table 3 reports estimates of equation (1), where the dependent variable is an indicator equal to one if a person is receiving (non-SSDI and non-VA) personal disability income, and zero otherwise. We present results for all workers, as well as separately for men and for women, as the sexes differ substantially in both labor supply and disability receipt.

We find strong evidence that mandatory STD coverage increases the receipt of personal disability income. Overall, the statistically significant coefficient of 0.0033 in column 1 indicates that each one percentage point increase in the probability of having STD coverage raises the probability of receiving personal disability income by 0.003%. Given the mean rate of personal disability income receipt in this sample is 0.25%, this effect is substantial. For example, moving from an industry with a value of 0.2 for COVGAIN to a sector with a value of 0.5 would be lead to an increase of 0.09%, which is more than one-third of the sample mean.

The next two columns show that that access to STD coverage has a substantially larger effect on the odds of receiving STD income among women than men. In particular, the point estimate for females is substantially larger than for males, but the prevalence of STD income claiming is comparable across the sexes—implying a larger proportionate effect.

Turning to other control variables, age is positively associated with the probability of past year STD income: aging one year is associated with a 0.0001 percentage point (4%) increase in the probability of receipt of STD income in the past year, and this association is comparable for men and women. Other race and Hispanic ethnicity are negatively associated with the probability of STD income receipt, although the relationship may be stronger for women than men. We observe an inverse relationship between educational attainment and probability of STD income in the past year: those with higher education have lower probability of STD income receipt. Interestingly, the relationship appears to plateau at some college education and is stronger for women than for men. Those who are not married (divorced, separated, widowed, or never married) have higher risk of past year STD income receipt that those who are currently married.

These results clearly establish that having STD coverage raises the odds of receiving private (non-SSDI and non-VA) disability income receipt. But does it raise or lower the odds of SSDI receipt? We investigate this question in Table 4. Here, we use the same regression framework as in Table 3, but we replace the dependent variable with an indicator for SSDI receipt. The estimated coefficient is a negative 0.0011. Taken at face value, this suggests that for each three persons receiving short-term disability income as a result of STDI laws, one person no longer receives SSDI. Unfortunately, however, this estimate is imprecise, and is much smaller than its standard error.

Moreover, when we split the sample by gender, we find the opposite pattern to that indicated above. Whereas we found that STDI-induced coverage gains led to more receipt of disability income among women, here we find that the reduction in SSDI receipt is concentrated among men. Indeed, the coefficient on COVGAIN in Table 4 is actually larger than that for men in Table 3, which would imply, implausibly, that for every one person receiving disability

income due to a STDI law, three persons no longer receive SSDI income. For women, the coefficient in table 4 is positive, indicating that more short-term disability income leads to more SSDI receipt, but the coefficient is insignificant once again.

Unfortunately, these results preclude us from drawing any strong conclusion about the impact of STDI programs on SSDI receipt. Partly this uncertainty is due to a lack of precision in our estimates. But it may also reflect a flaw in our underlying identification assumptions: even controlling for state and sector fixed effects, there may be critical sector/state differences in worker selection into sectors that bias our estimates. To illustrate this potential problem, we re-estimated our main model, using as a dependent variable an indicator equal to one for individuals who had completed a college degree or higher.[8] This is a "placebo test:" if these sectors are comparable across states, controlling for general state differences, then COVGAIN should not be predictive of the education level of workers employed in each sector. But in fact, as table 5 shows, this is not the case. The coefficients on the COVGAIN variable are negative and highly statistically significant ($p < 0.01$) in the full sample and in the sex-specific samples. This result implies that increases in sector-level STD coverage induced by STD mandates lead to increased entry of non-college workers into these sectors.

Thus, this placebo test suggests that our main identifying assumption is violated—specifically, that the STD mandate directly affects the selection of workers of different underlying disability risk into STD coverage across sectors. This means that the resulting relationship between mandatory STD coverage and SSDI accessions is potentially biased by sample selection and does not exclusively reflect the causal effect (if any) of STD coverage on subsequent SSDI accessions.

[8] In this specification we remove the education controls from the regression model.

## Part IV: Conclusions

Understanding the impact of STDI programs on SSDI claiming is an important issue for public policy. In this paper we proposed one strategy for doing so, using cross-sectional variation in private sector coverage rates for STDI in states that do and do not mandate STDI coverage. Unfortunately, our empirical strategy appears confounded by other state-by-sectoral differences that are correlated with STDI coverage. This empirical approach therefore does not provide conclusions with any confidence as to the degree of interaction between STDI and SSDI claiming – the problem is not one of precision, but rather of identification.

This unfortunate result does not diminish the importance of this topic, and future research could usefully pursue alternative approaches. For example, one alternative strategy for estimating the effect of state STDI policies on SSDI enrollment and other outcome variables of interest would be to use detailed longitudinal data from a survey such as the Survey of Income Program and Participation (SIPP). This would allow one to explore how the trajectories of income receipt and labor market outcomes evolve in the months following a well-defined health shock, and whether this differs by state and sector as outlined above. This would also permit a comparison of whether and to what extent the COVGAIN variable is correlated with individual characteristics and the magnitude of the health shock.

Another alternative would be to improve data quality by moving from population-based surveys to administrative data. Data from private insurers of short- and long-term disability could provide information on claiming rates for these policies over time that would allow longitudinal exploration in states with and without STDI policies. Moreover, such data might allow for the incorporation of employer-specific policies that impact both short- and long-term

disability claiming, such as provisions for worker rehabilitation and different definitions of disability. This and related strategies remain fruitful avenues for future research.

**Table 1. Short term disability coverage rates by sector (three-digit 2007 NAICS codes)**

| Sector Code | Sector name | STD Coverage (%) |
|---|---|---|
| 211 | Oil and Gas Extraction | 64 |
| 212 | Mining (except Oil and Gas) | 73 |
| 213 | Support Activities for Mining | 32 |
| 221 | Utilities | 47 |
| 236 | Construction of Buildings | 16 |
| 237 | Heavy and Civil Engineering Construction | 25 |
| 238 | Specialty Trade Contractors | 22 |
| 311 | Food Manufacturing | 67 |
| 312 | Beverage and Tobacco Product Manufacturing | 51 |
| 313 | Textile Mills | 59 |
| 321 | Wood Product Manufacturing | 38 |
| 322 | Paper Manufacturing | 82 |
| 323 | Printing and Related Support Activities | 43 |
| 325 | Chemical Manufacturing | 66 |
| 326 | Plastics and Rubber Products Manufacturing | 63 |
| 327 | Nonmetallic Mineral Product Manufacturing | 37 |
| 331 | Primary Metal Manufacturing | 79 |
| 332 | Fabricated Metal Product Manufacturing | 62 |
| 333 | Fabricated Metal Product Manufacturing | 73 |
| 334 | Computer and Electronic Product Manufacturing | 67 |
| 335 | Electrical Equipment, Appliance, and Component Manufacturing | 77 |
| 336 | Transportation Equipment Manufacturing | 76 |
| 337 | Furniture and Related Product Manufacturing | 39 |
| 339 | Miscellaneous Manufacturing | 66 |
| 423 | Merchant Wholesalers, Durable Goods | 47 |
| 424 | Merchant Wholesalers, Nondurable Goods | 48 |
| 425 | Wholesale Electronic Markets and Agents and Brokers | 25 |
| 441 | Motor Vehicle and Parts Dealers | 25 |
| 442 | Furniture and Home Furnishings Stores | 12 |
| 443 | Electronics and Appliance Stores | 31 |
| 444 | Building Material and Garden Equipment and Supplies Dealers | 23 |
| 445 | Food and Beverage Stores | 31 |
| 446 | Health and Personal Care Stores | 26 |
| 447 | Gasoline Stations | 14 |
| 448 | Clothing and Clothing Accessories Stores | 15 |
| 451 | Sporting Goods, Hobby, Book, and Music Stores | 12 |
| 452 | General Merchandise Stores | 18 |
| 453 | Miscellaneous Store Retailers | 11 |
| 454 | Nonstore Retailers | 54 |
| 481 | Air Transportation | 32 |
| 482 | Rail Transportation | 94 |
| 484 | Truck Transportation | 30 |
| 485 | Transit and Ground Passenger Transportation | 17 |
| 488 | Support Activities for Transportation | 52 |
| 492 | Couriers and Messengers | 60 |
| 493 | Warehousing and Storage | 58 |
| 511 | Publishing Industries (except Internet) | 62 |
| 515 | Broadcasting (except Internet) | 47 |
| 517 | Telecommunications | 79 |
| 518 | Data Processing, Hosting, and Related Services | 71 |
| 519 | Other Information Services | 19 |
| 522 | Credit Intermediation and Related Activities | 69 |
| 523 | Securities, Commodity Contracts, and Other Financial Investments and Related | 74 |

| | | |
|---|---|---|
| | Activities | |
| 524 | Insurance Carriers and Related Activities | 71 |
| 525 | Funds, Trusts, and Other Financial Vehicles | 43 |
| 531 | Real Estate | 31 |
| 532 | Rental and Leasing Services | 35 |
| 541 | Professional, Scientific, and Technical Services | 51 |
| 551 | Management of Companies and Enterprises | 69 |
| 561 | Administrative and Support Services | 19 |
| 611 | Educational Services | 18 |
| 621 | Ambulatory Health Care Services | 15 |
| 622 | Hospitals | 39 |
| 623 | Nursing and Residential Care Facilities | 13 |
| 624 | Social Assistance | 12 |
| 711 | Performing Arts, Spectator Sports, and Related Industries | 9 |
| 721 | Accommodation | 20 |
| 722 | Food Services and Drinking Places | 11 |
| 811 | Repair and Maintenance | 4 |
| 812 | Personal and Laundry Services | 18 |
| 813 | Religious, Grantmaking, Civic, Professional, and Similar Organizations | 20 |
| 921 | Executive, Legislative, and Other General Government Support | 20 |
| 922 | Justice, Public Order, and Safety Activities | 16 |
| 923 | Administration of Human Resource Programs | 18 |
| 924 | Administration of Environmental Quality Programs | 7 |
| 925 | Administration of Housing Programs, Urban Planning, and Community Development | 15 |
| 926 | Administration of Economic Programs | 30 |

*Notes*: NAICS Industry 313 includes NAICS Industries 313, 314, 315, and 316. NAICS Industry 323 includes NAICS Industries 323 and 324. NAICS Industry 515 includes NAICS Industries 512 and 515. NAICS Industry 482 includes NAICS Industries 482 and 483. NAICS Industry 485 includes NAICS Industries 485, 486, and 487. NAICS Industry 532 includes NAICS Industries 532 and 533. NAICS Industry 561 includes NAICS Industries 561 and 562. NAICS Industry 711 includes NAICS Industries 711, 712, and 713.
*Source*: Bureau of Labor Statistics National Compensation Survey (March, 2012). Unpublished Data.

**Table 2. Weighted summary statistics: Current Population Survey Annual Social and Economic Supplement Outgoing Rotation Group**

| | Full sample | Men | Women |
|---|---|---|---|
| *Income variables* | | | |
| STD income | 0.0025 | 0.0025 | 0.0025 |
| SS income | 0.0129 | 0.0098 | 0.0164 |
| *STD variables* | | | |
| Sector STD coverage rate | 0.3333 | 0.3562 | 0.3075 |
| STD law in state of residence | 0.2221 | 0.2276 | 0.2159 |
| *Personal characteristics* | | | |
| Male | 0.5296 | 1.0000 | 0.0000 |
| Female | 0.4704 | 0.0000 | 1.0000 |
| Age | 41.9135 | 41.7146 | 42.1374 |
| White | 0.8171 | 0.8330 | 0.7993 |
| African American | 0.1131 | 0.0970 | 0.1313 |
| Other race | 0.0697 | 0.0701 | 0.0694 |
| Hispanic | 0.1334 | 0.1509 | 0.1138 |
| Less than high school | 0.0886 | 0.1081 | 0.0666 |
| High school | 0.2890 | 0.3029 | 0.2734 |
| Some college | 0.2858 | 0.2651 | 0.3090 |
| College graduate | 0.3367 | 0.3239 | 0.3510 |
| Married | 0.5976 | 0.6162 | 0.5767 |
| Divorced/separated/widowed | 0.1751 | 0.1399 | 0.2148 |
| Never married | 0.2273 | 0.2439 | 0.2085 |
| Unweighted N | 240,972 | 124,807 | 116,165 |

*Notes*: CPS weights for the sample that underwent the Annual Social and Economic Supplement applied. Observations with missing information or that do not report an industry included in the BLS industry STDI coverage data are excluded from the sample. Respondents younger than 23 years and older than 64 years excluded. States with an STD law include California, Hawaii, New York, New Jersey, and Rhode Island. Sectors are three digit 2007 NAICS sectors.

**Table 3. Effect of STDI coverage on probability of past year personal STD income among adults ages 23 to 64 years 2001 to 2011 CPS Annual Social and Economic Supplement Outgoing Rotation Group**

| | Full sample | Men | Women |
|---|---|---|---|
| *Proportion* | 0.0025 | 0.0025 | 0.0025 |
| Coverage*STD state | 0.0033** | 0.0020 | 0.0044*** |
| | (0.0014) | (0.0017) | (0.0014) |
| Male | 0.0003 | -- | -- |
| | (0.0003) | | |
| Age | 0.0001*** | 0.0001*** | 0.0001*** |
| | (0.0000) | (0.0000) | (0.0000) |
| African American | -0.0002 | 0.0000 | -0.0005 |
| | (0.0005) | (0.0005) | (0.0007) |
| Other race | -0.0014** | -0.0014** | -0.0015* |
| | (0.0006) | (0.0005) | (0.0008) |
| Hispanic | -0.0010* | -0.0016** | -0.0001 |
| | (0.0006) | (0.0007) | (0.0005) |
| High school | -0.0016** | -0.0024*** | -0.0005 |
| | (0.0006) | (0.0008) | (0.0008) |
| Some college | -0.0013** | -0.0023*** | -0.0000 |
| | (0.0006) | (0.0007) | (0.0007) |
| College degree | -0.0022*** | -0.0037*** | -0.0006 |
| | (0.0006) | (0.0008) | (0.0007) |
| Divorced/separated/widowed | 0.0015*** | 0.0012** | 0.0017*** |
| | (0.0004) | (0.0006) | (0.0006) |
| Never married | 0.0012*** | 0.0010*** | 0.0013** |
| | (0.0003) | (0.0004) | (0.0005) |
| Unweighted N | 240,972 | 124,807 | 116,165 |

*Notes*: All equations estimated with a linear probability model. All models apply CPS weights for the sample that underwent the Annual Social and Economic Supplement, and adjust for sector, state, and year fixed effects. Omitted categories are female (full sample only), White race and married. Standard errors are clustered by the state and reported in parentheses. Observations with missing information or that do not report an industry included in the BLS industry STDI coverage data are excluded from the sample.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

**Table 4. Effect of STDI coverage on probability of past year personal Social Security income among adults ages 23 to 64 years 2001 to 2011 CPS Annual Social and Economic Supplement Outgoing Rotation Group**

| | Full sample | Men | Women |
|---|---|---|---|
| *Proportion* | 0.0129 | 0.0098 | 0.0164 |
| Coverage*STD state | -0.0011 | -0.0049* | 0.0045 |
| | (0.0032) | (0.0025) | (0.0057) |
| Male | -0.0028*** | -- | -- |
| | (0.0008) | | |
| Age | 0.0012*** | 0.0011*** | 0.0012*** |
| | (0.0001) | (0.0001) | (0.0001) |
| African American | -0.0034*** | -0.0042*** | -0.0029** |
| | (0.0010) | (0.0013) | (0.0013) |
| Other race | -0.0026*** | -0.0022** | -0.0029** |
| | (0.0008) | (0.0010) | (0.0013) |
| Hispanic | -0.0056*** | -0.0030* | -0.0091*** |
| | (0.0014) | (0.0016) | (0.0018) |
| High school | -0.0027* | -0.0016 | -0.0042 |
| | (0.0015) | (0.0013) | (0.0030) |
| Some college | -0.0060*** | -0.0052*** | -0.0074** |
| | (0.0017) | (0.0015) | (0.0031) |
| College degree | -0.0119*** | -0.0106*** | -0.0139*** |
| | (0.0018) | (0.0021) | (0.0025) |
| Divorced/separated/widowed | 0.0133*** | 0.0037*** | 0.0210*** |
| | (0.0010) | (0.0013) | (0.0014) |
| Never married | 0.0111*** | 0.0099*** | 0.0120*** |
| | (0.0011) | (0.0012) | (0.0014) |
| Unweighted N | 240,972 | 124,807 | 116,165 |

*Notes*: All equations estimated with a linear probability model. All models apply CPS weights for the sample that underwent the Annual Social and Economic Supplement, and adjust for sector, state, and year fixed effects. Omitted categories are female (full sample only), White race and married. Standard errors are clustered by the state and reported in parentheses. Observations with missing information or that do not report an industry included in the BLS industry STDI coverage data are excluded from the sample.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

**Table 5. Association between STDI coverage and the probability of holding a college degree or higher among adults ages 23 to 64 years 2001 to 2011 CPS Annual Social and Economic Supplement Outgoing Rotation Group**

| | Full sample | Men | Women |
|---|---|---|---|
| *Proportion* | 0.3367 | 0.3239 | 0.3510 |
| Coverage*STD state | -0.0353*** | -0.0555*** | -0.0353*** |
| | (0.0102) | (0.0133) | (0.0131) |
| Male | 0.0657*** | -- | -- |
| | (0.0039) | | |
| Age | -0.0007*** | -0.0373*** | -0.0028*** |
| | (0.0002) | (0.0138) | (0.0003) |
| African American | -0.1192*** | 0.0011*** | -0.1175*** |
| | (0.0059) | (0.0002) | (0.0059) |
| Other race | 0.0809*** | -0.1246*** | 0.0886*** |
| | (0.0127) | (0.0079) | (0.0158) |
| Hispanic | -0.1780*** | 0.0712*** | -0.1809*** |
| | (0.0198) | (0.0123) | (0.0257) |
| Divorced/separated/widowed | -0.0759*** | -0.1639*** | -0.0625*** |
| | (0.0048) | (0.0156) | (0.0055) |
| Never married | -0.0048 | -0.0782*** | 0.0273*** |
| | (0.0042) | (0.0075) | (0.0063) |
| Unweighted N | 240,972 | 124,807 | 116,165 |

*Notes*: All equations estimated with a linear probability model. All models apply CPS weights for the sample that underwent the Annual Social and Economic Supplement, and adjust for sector, state, and year fixed effects. Omitted categories are female (full sample only), White race and married. Standard errors are clustered by the state and reported in parentheses. Observations with missing information or that do not report an industry included in the BLS industry STDI coverage data are excluded from the sample.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.