

Stanford University

From the Selected Works of John Donohue

Fall September, 2011

Assessing Post-ADA Employment: Some Econometric Evidence and Policy Considerations

John J. Donohue, *Stanford Law School*



Available at: https://works.bepress.com/john_donohue/88/

YALE LAW SCHOOL



John M. Olin Center for Studies in Law, Economics, and Public Policy
Research Paper No. 358

Assessing Post-ADA Employment: Some Econometric Evidence and Policy Considerations

by

John J. Donohue III

Yale Law School, National Bureau of Economic Research (NBER)

and

Michael Stein

William & Mary Law School, Harvard Law School

and

Sascha Becker

and

Christopher L. Griffin, Jr.

Yale Law School

This paper can be downloaded without charge from the
Social Science Research Network Paper Collection at:
<http://ssrn.com/abstract=1282307>

**ASSESSING POST-ADA EMPLOYMENT:
SOME ECONOMETRIC EVIDENCE AND POLICY CONSIDERATIONS**

John J. Donohue III[†]
Michael Ashley Stein
Sascha Becker
Christopher L. Griffin, Jr.

**PRELIMINARY DRAFT:
PLEASE DO NOT CITE WITHOUT AUTHORS' PERMISSION**

[†] Leighton Homer Surbeck Professor, Yale Law School, and corresponding author. Please send all communication to: John J. Donohue / Yale Law School / PO Box 208215 / New Haven, CT 06520; Phone: (203) 432-1994; Fax: (203) 432-1040; Email: j.donohue@yale.edu.

ABSTRACT

In this article, we offer innovative analysis and additional evidence on the relationship between the Americans with Disabilities Act (“ADA”) and the relative labor market outcomes for people with disabilities, the very class protected by its landmark provisions. Using individual-level longitudinal data from 1981 to 1996 derived from the previously unexploited Panel Study of Income Dynamics (“PSID”), we examine the possible effect of the ADA on (1) annual weeks worked; (2) annual earnings; and (3) hourly wages for a sample of 7120 unique male household heads between the ages of 21 and 65 as well as a subset of 1147 individuals appearing every year from 1981 to 1996. Our analysis of the larger sample suggests the ADA had a negative impact on the employment levels of disabled persons relative to non-disabled persons but no impact on relative earnings. However, our evaluation of the restricted sample raises questions about these findings. Using these data, we find little evidence of adverse effects on weeks worked but strong evidence of wage declines for the disabled, albeit declines beginning in 1986, well before the ADA’s passage. These results therefore cast doubt on the adverse ADA-related impacts found in previous studies, particularly Acemoglu and Angrist (2001). The conflicting narratives that emerge from our analysis shed new light on, but also counsel caution in reaching final conclusions about, the impact of the ADA on employment outcomes for people with disabilities.

1. Introduction

Research on the measurable effects of antidiscrimination initiatives indicates that numerous factors can impact the efficacy of civil rights statutes seeking to equalize opportunities for targeted groups.¹ Gerald Rosenberg, for example, has argued that judicial decisions alone were incapable of generating major social or economic improvements for African Americans (Rosenberg, 1991). Rather than being simply the by-product of the passage of the Civil Rights Act of 1964 (42 U.S.C. §§ 2000e-2000e-17), these economic gains were generated by a comprehensive federal government enforcement effort, concentrated in the South, of related antidiscrimination policies that included voting rights and school desegregation (Butler and Heckman, 1977; Donohue and Heckman, 1991; Heckman and Payner, 1989).

A key Congressional aspiration in enacting the Americans with Disabilities Act (“ADA”) was to increase labor market participation among disabled workers. In addition to prohibiting discriminatory action throughout the employment relationship, Title I compels employers to provide reasonable accommodations to “qualified” employees with disabilities.² This reasonable accommodation mandate has become both a central defining feature of the ADA as an antidiscrimination statute for its advocates (Stein, 2004) and a lightning rod for its critics (Epstein, 1992).

For most of its existence, however, Title I has not been administered in a fashion equipped to promote its articulated goals (Stein and Stein, 2007). The ADA set forth legislative findings documenting the widespread exclusion of people with disabilities from the workplace and expressing Congressional intent to remedy that situation.

Nonetheless, national policymakers have yet to systematically address obstacles to labor market participation that confronted those with disabilities.³ Trenchantly, ten years passed before federal benefits rules were amended to allow disabled persons to seek employment without being deprived of their health care benefits or monetary supplements.⁴ Moreover, Congress has yet to enact a single job program for workers with disabilities similar to those passed for non-disabled benefit recipients during the mid-1990s welfare reform.⁵ Finally, the few existing federal tax benefits designed to increase the employment of persons with disabilities remain profoundly underutilized.⁶

A growing debate has developed regarding the impact of the ADA on the employment of individuals with disabilities. Initial empirical studies and theoretical analyses of disabled employees' labor market participation comparing pre- and post-ADA employment data have painted a fairly dismal picture, suggesting that the law may actually have impaired the employment prospects of its intended beneficiaries. Some recent studies have raised questions about this dire assessment, although both sides concede that post-ADA disability-related employment has not significantly improved.

Achieving a greater understanding of post-ADA employment effects is of considerable importance both at home and abroad. Notably, the ADA's reasonable accommodation mandate has been adopted by the United Nations Convention on the Rights of Persons with Disabilities,⁷ as well as the European Union's Employment Framework Directive (an umbrella antidiscrimination policy). These enactments have triggered unprecedented interest in disability law and policy, especially for the three-quarters of the world's nations lacking any relevant domestic statute or legal provision (Stein and Lord,

2008). Consequently, the ripple effects from the ADA and its accommodation mandate have global implications.

This Article reexamines the impact of the ADA on the employment and wages of disabled workers. We contribute to the literature in two important ways. First, we expand the period of observation to understand longer time trends. Second, unlike many of the earlier studies, we employ individual-level panel data so that we can observe the same individuals before and after the adoption of the ADA. Expanding the timeframe and utilizing the unique features of our data should improve our ability to identify the true impact of the ADA by controlling for pre-existing trends and compositional changes in the disabled population. At the same time, there is a cost to our approach since the size of our Panel Study of Income Dynamics (“PSID”) data set is small relative to the Current Population Survey (“CPS”), and when we restrict our sample in order to follow the identical 1147 male, household-head workers over a sixteen year data period, we lose statistical power in order to avoid compositional biases.

To frame our discussion, Section 2 discusses the results of major studies on post-ADA employment effects. In Sections 3 and 4, we consider the difficulties with data used in these earlier studies, particularly regarding their cross-sectional design with respect to sampled individuals. Section 5 sets forth an initial analysis from the PSID, a longitudinal data set that, to our knowledge, has not been used to explore systematically Title I’s impact on the employment of the disabled. These data are unique in their ability to track the same individuals over time, from 1968 through 2003.⁸ By avoiding the shortcomings of data from the more frequently used CPS, PSID data allow us to chart the measured

employment and earnings of disabled workers over a broader span of time. Section 6 presents the results from our regression-based analysis without imposing any extraordinary constraints on the data. We find that employment levels for the disabled relative to the non-disabled when measured as the number of annual weeks worked, declined by about three weeks after 1994 (when Title I became fully enforceable) but that earnings (conditional on receipt) were not measurably affected. Section 7 adds the condition that individuals represented in the sample not change over time so that estimates across time are as comparable as possible. Using this “restricted” dataset, we see that our estimates can be highly sensitive to whether one controls for individual fixed effects. Without such controls (see Column 1 in Table 6), a picture of substantial relative declines in weeks worked by the disabled emerges in the 1990s; with such controls, no such employment decline appears.

These results are important for several reasons. First, they reinforce the growing evidence that econometric analyses of law and policy can yield quite different results across different data sets, years, and statistical models (Donohue and Wolfers, 2005). Second, we argue that the longitudinal data contained in the PSID reduces the confounding effect of the compositional change in who self-identifies as “disabled” as disability law changes. In this regard, our approach can give a cleaner estimate of the impact of the law on a given panel of workers over an extended period of time. This benefit does come at the cost of our diminished ability to observe the performance of younger disabled workers coming into the market at about the time of the adoption of the ADA, as well as from our relatively small sample size.⁹ Finally, this Article again

underscores the challenge confronting policymakers in trying to craft a disability policy that addresses the difficult conditions confronting disabled male household heads in the modern American economy. Section 8 concludes.

2. Previous Studies

Eighteen years after the ADA's passage, most scholars agree that the employment rates of disabled workers have declined, although there is still contention over the extent of and reasons for this decline. Some attribute the drop to the ADA itself and, in particular, to its reasonable accommodation mandate. (Jolls and Prescott, 2005; Acemoglu and Angrist, 2001; DeLeire, 2000a; DeLeire 2000b; Jolls 2000; Epstein, 1992). A second group attributes the decline to factors other than the ADA. (Hotchkiss, 2004; Houtenville and Burkhauser, 2004; Burkhauser and Stapleton, 2004; Hotchkiss, 2003; Wittenberg and Maag, 2003; Beegle and Stock, 2003; Autor and Duggan, 2003; Blanck et al., 2003; Tolin and Patwell, 2003; Kaye, 2003; Bound and Waidmann, 2002; Burkhauser et al., 2002; McNeil, 2000; Schwochau and Blanck, 2000)¹⁰

The ADA includes two key employment-related provisions: a prohibition of wage and employment discrimination against "qualified individuals with a disability," and a mandate that employers provide "reasonable accommodation" for those protected individuals in order to guarantee that they have equal employment opportunities. Although a number of studies have found that pre-1990 Civil Rights laws increased the employment of the primary targeted group (for instance, African-American workers), those laws involved only an antidiscrimination component and did not include a reasonable accommodation requirement. (Heckman and Payner, 1989; Donohue and

Heckman, 1991) Consequently, when initial evaluations of relative post-ADA employment rates concluded that the law *impaired* the employment of workers with disabilities, the reasonable accommodation mandate was quickly identified as the likely culprit.

Three major empirical studies have been central to the claim that the ADA's reasonable accommodation mandate has caused a relative decrease in the post-ADA employment rate of persons with disabilities.¹¹ Initially, DeLeire (2000a) examined the effect of the ADA on labor market opportunities for people with disabilities using data from the Survey of Income and Program Participation ("SIPP"). He hypothesized that compliance with the ADA's reasonable accommodation provision imposed significant costs on employers, both directly in crafting the accommodations and indirectly in generating legal costs associated with defending against Equal Employment Opportunity Commission ("EEOC") proceedings or antidiscrimination lawsuits. As a result, DeLeire anticipated that overall demand for disabled workers would fall.

DeLeire's sample included pooled panels of men aged eighteen to sixty-four, and the most fully specified regression models controlled for demographic characteristics, industry, and occupation. The SIPP panels varied in length up to two and a half years, and each individual was interviewed in four-month intervals to generate six to nine observations per individual in the sample.¹² DeLeire found that employment of disabled men declined by about 7.2 percentage points from 1990 to 1995 and that the largest declines occurred "for workers in manufacturing industries, workers in blue-collar or managerial occupations, workers with physical or mental disabilities, and workers who

became disabled for reasons besides a work-related injury.” However, he did not find any effect on the wages offered to disabled men. To buttress his claim that the ADA was responsible for the adverse employment experience of the disabled, DeLeire rejected other possible candidates for the observed decline (e.g., trends in disability insurance coverage, labor force participation among older men, or changes in disability insurance variables like denial rate, eligibility, or benefit rates). Ultimately, DeLeire concluded that his results “strongly suggest” that the accommodation provision of the ADA is “an ineffective way to increase labor market opportunities for people with disabilities.”

Acemoglu and Angrist (2001) also investigated whether the intended employment protection of the ADA improved economic conditions for the disabled. Their theoretical model captures the opposing influences of increased accommodation and firing costs, which most likely reduced disabled employment, versus sanctions against discrimination, which likely raised employment. Using CPS survey data for individuals aged twenty-one to fifty-eight during the period 1988-1997, Acemoglu and Angrist estimated the effect of the ADA on both weeks worked and log weekly earnings. Their specification included a disability indicator as well as controls for years, age, education, race, census region, and interactions.

Acemoglu and Angrist examined both employment effects on the number of weeks worked and wage effects, presenting distinct findings based on whether workers were over or under age 40. For both women and men under forty, they found an average annual decline in employment beginning in 1993 and 1992, respectively, ranging from 1.4 to 2.8 weeks. For those over forty, however, only men experienced a statistically

significant drop in employment of 2.1 weeks. However, the effects for older men were attributed to transfer payments and disappeared once Supplementary Security Income (“SSI”) and Social Security Disability Insurance (“SSDI”) were included in the analysis. The wage regressions were more ambiguous: no consistent effect emerged for women, while the decline for men after 1993 disappeared when a linear trend was included.

Although the papers by DeLeire and Acemoglu and Angrist are serious studies by highly respected researchers, their studies have the difficult challenge of trying to identify the effect of a uniformly imposed federal law such as the ADA using a simple before-after comparison in the employment experience of disabled individuals (Donohue and Heckman, 1991). By contrast, Jolls and Prescott (2005) attempted an alternative approach based on variations in disability law both across states and over time, thereby permitting a more nuanced assessment of treatment and control groups. Specifically, Jolls and Prescott utilized pre-ADA variation in state law prohibiting disability discrimination by private employers as a means of disentangling the effect of the accommodation provision from the generally higher firing costs imposed by the antidiscrimination provision. States that had already adopted accommodation and non-discriminatory hiring, firing and terms of employment provisions similar to the ADA were used as the control group. A second group (“Limited-Protection” states) included those jurisdictions that did not mandate reasonable accommodation, but otherwise had anti-discriminatory hiring and firing state laws in place. Finally, the third group (“No-Protection” states) did not have any special provisions for disabled persons before the ADA became law. Jolls and Prescott analyzed data from the CPS March Supplement for the years 1988 to 1998 and included

individuals aged 21-58. Using existing variation across different state regimes in the control and two treatment groups, Jolls and Prescott estimated the effects of different elements of the ADA, finding different patterns before and after 1993.

Jolls and Prescott found that in control group states that already enforced legal regimes similar to the ADA, employment of the disabled remained stable or increased until 1993. At the same time, however, (up to 1993) introducing a “reasonable accommodation” requirement seemed to *reduce* the employment of the disabled by around 10 percent. Moreover, the anti-discriminatory hiring and firing provisions were *not* deemed to have a strong stimulative effect on the employment of the disabled. Indeed, Jolls and Prescott concluded that the introduction of these provisions produced effects that are “small in magnitude, inconsistent in sign and never statistically significant.” Beginning in 1993, the differences across state groups broke down, and all three groups were found to experience declining disabled employment of roughly similar magnitude.

In sum, the first three main studies assessing relative post-ADA employment rates using SIPP and CPS agree that the statute has precipitated a decline in employment levels. Moreover, all three studies conclude that the ADA’s reasonable accommodation mandate is the most likely cause of the relative decline in employment.

The latest addition to the empirical literature assessing the ADA’s impact, by Burkhauser, Houtenville, and Rovba (2007), extends and refines the analysis of Acemoglu and Angrist in several important ways. First, Burkhauser and his colleagues follow Acemoglu and Angrist in using CPS data, but they expand the observation period

from 1988-1997 to 1982-2004. The analysis of this longer time period enables Burkhauser et al to conclude that the “decline in relative employment of working-age people with disabilities not only began well before the implementation of the ADA in 1992, but has continued long afterward.”

Second, Burkhauser et al. match two consecutive CPS surveys (the maximum possible) to capture some quasi-longitudinal variation in outcomes for the disabled population. They argue that this method is superior to non-matched analyses, since the longer-term disabled, i.e., those indicating a work limitation in two consecutive years, should be most affected by the ADA’s enactment. Burkhauser et al. find that Acemoglu and Angrist’s findings are not robust to the two-year disability definition. Rather, the decline in relative employment starts roughly at the same time SSDI and SSI eligibility rules were changed (in the mid-1980s)—not with the passage of the ADA. In addition, they find that the change in SSDI and SSI payments coincides with a shift in the sources of income for disabled persons. In particular, labor earnings declined according to all indicators, e.g., absolute real income, share of income total income, and proportion of earnings among the non-disabled, while SSDI and SSI benefits increased by 68% as a share of income from 1982 to 2004. As with their findings on employment, Burkhauser et al. locate these income trends in their extended CPS data well before enactment of the ADA and also discover that they continue long after. Ultimately, Burkhauser et al. conclude that the ADA neither caused the decline in relative employment and labor income nor prevented the continuation of this long term adverse trend:

A much more plausible explanation of the trends reported here is that major changes in the eligibility standards for SSDI and SSI that occurred in the mid-

1980s is responsible for the decline in the relative employment of working-age men and women with disabilities since then. While the relaxation of these standards offered additional protection to those with disabilities, it came at the price of substantial reductions in their future labor earnings.

3. Problems in Identifying Disabled Individuals

In trying to assess the ADA's impact, it is obviously crucial to have a reliable measure of disability. Yet, since the enactment of the ADA, on numerous occasions federal courts have redefined who is covered by the statute.¹³ Accordingly, it is unsurprising that no clear and consistent measure of disabled individuals who are covered under the ADA exists.¹⁴ The CPS studies discussed above all use the survey responses to the work-disability question¹⁵ as a proxy for disability. This in turn raises several concerns.

To begin with, the CPS question was not intended to correspond to the definition of disability under the ADA (McNeil, 2000). Thus, regardless of any self-reporting issues, affirmative answers to the work-disability question will capture some individuals who would not be protected under the ADA and at the same time will not capture others who would be protected. More specifically, people with severe disabilities who are unable to work even when provided with reasonable accommodations are not covered by the ADA, but nonetheless are very likely to answer affirmatively to the work-disability question. Similarly, a respondent might answer the work-disability question affirmatively even though she is not substantially limited in a major life activity and therefore outside ADA coverage. These problems of over- and under-inclusion result from the fact that the work-disability question was not designed to measure disability, but rather to serve as

“screener” to guide the flow of the interview. For example, only individuals that answered yes to the disability question are asked about sources of income related to disability (i.e. workers compensation, SSI, or SSDI) (Hale, 2001).

In addition, it is at least possible that the composition of respondents who answer affirmatively to the work-disability question changes substantially over time in ways that would undermine the reliability of estimates of the effect of the law (Kirchner, 1996; Kruse and Schur, 2003). In particular, the accommodation provision of the ADA could lead disabled people to reject the notion that their health problem or disability prevents them from working or limits the kind or amount of work they can do (Kirchner, 1996). By having the people that receive accommodation under the ADA provision drop out of the population that answers affirmatively to the work-disability question, one is potentially left with more severely disabled people that both have a harder time finding employment and self-report a work disability. This compositional change would bias the analysis towards findings of declining employment of the disabled population regardless, even if there were a positive effect on employment from the ADA accommodation provision (Kruse and Schur, 2003).

A further concern with using the work-disability question as proxy for disability status is that changing attitudes towards disability in society could impact the composition of people willing to self-identify as disabled. Historically, the stigma of being disabled could lead to an undercount in self-reported disabilities, but the magnitude of the undercount could change over time as being “openly” disabled becomes more socially accepted. For example, the enactment of the ADA could be considered a

milestone in the relationship between disabled individuals and society at large, which may have encouraged disabled people to be more forthcoming and self-report a disability. In addition, the ADA or other programs might induce some individuals to self-report a disability as they seek to benefit from the particular program. As before, this kind of compositional change in the self-reporting would likely—all else constant—be captured as declining employment for disabled individuals.

In light of the problems surrounding the work-disability question and similar survey questions, efforts have been made to discourage their use. In fact while various forms of disability data are collected in many government-sponsored surveys, there is very little effort to test whether the collected data are accurate and reliable (Hale, Kruse and Kim, 2005). The National Council on Disability (2002) counseled specifically against continued use of the existing measures “until a methodology for assessing employment rates among people with disabilities . . . can be developed.” The NCD admonition came in 2002, even though a 1998 Executive Order mandated that various government agencies “shall design and implement a statistically reliable and accurate method to measure the employment rate of adults with disabilities as soon as possible.” To this day, there is no measure for disability that would satisfy the requirements of the executive order. In fact, during the process of trying to develop a reliable measure, the extensive testing of the existing measures (much like the work-disability question) only increased the already existing concerns. The work-disability question in particular was deemed to generate too many false positives while also missing a large portion of the legally protected category of disabled individuals: during the testing for a reliable

measure, it missed 62% of the population of disabled persons.¹⁶ The question was furthermore not once represented in the top twenty-five sets of questions that were deemed most accurate and reliable to capture disability and accordingly dropped by the government from consideration as a measure of disability altogether.¹⁷

Faced with these obvious shortcomings, researchers relying on the CPS data and disability measures for their analyses will have to address the open empirical question whether one can design research strategies that utilize the imperfect measures to capture accurately disability employment trends.¹⁸ In particular if as argued above a compositional change in the disability measure relied upon implies the same effect that the authors ultimately find, it seems impossible to gauge whether the results are driven by the change in law, a compositional change in the measurement or both. In this context, serious questions exist whether one should just point out that one uses a measure of disability that other researchers have used before (Acemoglu and Angrist, 2001) and then merely spot check potential compositional changes as

Acemoglu and Angrist were alert to the problem of compositional change in the population self-identifying as disabled, and in fact tested for such changes with their matched sample analysis for the 1993-1994 periods. But even though they find their original estimates confirmed for three of the four sample groups in this limited timeframe, there is still a concern that a compositional change occurred over longer time periods (including the critical period just after ADA adoption).

Jolls and Prescott (2005) also grapple with this issue, and they note that their state panel data is less likely to be biased by any compositional change than the national time

series approaches used by DeLeire as well as by Acemoglu and Angrist. Thus, Jolls and Prescott acknowledge the possibility of compositional changes in the group that answers affirmatively to the work-disability question in the CPS, but point out that “[w]hile nationwide changes seem plausible, state-varying changes are less likely.” So while they “cannot entirely rule out” that changes in the composition that replies “yes” to the work-disability question have an effect on their results, their state panel data methodology enables them to probe some of the potential problems associated with the work-disability measure. For example, they test for (and reject) the potential compositional change of having “accommodated” disabled workers stop affirmatively answering the question by comparing the growth rates of the proportion of disabled persons across their three groups of states. Additionally, they address compositional changes driven by economic circumstances in the different state groups. The concerns that the state groups for which the ADA was a major innovation correspond to states in which wages grew slower or declined faster were originally suggested by others (Autor and Duggan, 2003), but Jolls and Prescott tried to account for these factors in their regressions with various controls for disability benefits and applications.

4. Other Data Issues: The CPS and SIPP

In order to resolve deficiencies with data collection methods in the Current Population Survey, the U.S. Census Bureau established the Survey of Income and Program Participation. The CPS does not explicitly track changes in household composition (which the PSID does). Rather than augment the CPS with additional questions about household characteristics or detailed financial statements, the Census

Bureau launched a new program, the SIPP, designed to capture economic conditions at multiple times within a two and one-half year window. This aspect of the design also relieved survey respondents from the task of remembering economic values or participation levels from up to 12 months prior to the survey.

When operating with full resources, the SIPP conducts eight waves of interviews within the 32-month panel period, each spaced four months apart. Due to funding shortages, however, some panels were interviewed as few as three (1989) or six times (1988). Members of a panel sample are randomly sorted into four subsets, and interviewed separately within the four-month reference period. This means that although one wave's respondents provide information from only the previous four months, responses within that wave are not contemporaneous and so may be confounded by monthly or seasonal factors.

The SIPP consists of a set of core questions, routinely posed at the beginning of each interview wave, as well as several topical modules asked sporadically throughout the panel window. Core data include basic demographic characteristics, employment status and earnings, other sources of income and participation in government subsidy programs. Topical modules include separate surveys on education, marital history, financial standing, and health and well-being. Much like the “gatekeeper” role played by the CPS and PSID disability questions, the core SIPP simply asks “Does . . . have a work disability?” in each wave. In the second wave of each panel, however, interviewers ask detailed questions about work disability histories when the respondent indicates a disability in the initial question. Follow-up queries include when the disability arose,

whether the individual was employed at the time, the exact nature of the disability and when the individual became unable to work because of the disability.

On the one hand, the SIPP questionnaire design would appear to be an improvement over both the PSID and CPS because of the helpful details provided by the work history disability topical module. On the other hand, the SIPP still resembles the cross-sectional nature of the CPS since panel samples change from year to year, while the PSID truly allows the researcher to follow individuals across time. Moreover, interviewers ask question from the disability module usually between June and September of the first year in the panel window. Consequently, any changes in disability status that occur in months from successive waves are not captured; the SIPP will only register an individual's disability if it existed before July-September of the first interview year (depending on the rotation group to which the individual was assigned).

DeLeire (2000a) relies exclusively on SIPP data to construct his disability proxy. Although the text does not state the source of his measure, the notes to his summary statistics tables indicate that he used the disability history module. Nevertheless, he fails to address any potential adverse implications from the sampling procedure. Importantly the advantages of the detailed module questions might be nullified by the formulation of the gatekeeper question. In other words, the disability module is only “triggered” if the interviewee responds affirmatively to the initial work disability question. But if, for example, an individual interprets the question to exclude disabilities that do not preclude employment then the SIPP will undercount the true existence of disabilities in the sample.

Thus it appears that clear tradeoffs exist when choosing among the three primary datasets for individual disability and employment status. The level of detail in the SIPP makes it more attractive than the vague questions in the CPS. The SIPP's major flaw in terms of disability reporting, however, is a failure to ask the question in the second and third years covered by each panel, which severely limits the reporting accuracy. Only the PSID data allow for reliable monitoring of individuals over time, specifically any real changes in disability status that may be attributed to passage of the ADA. Unfortunately, one must rely on a rather vague or at worst irrelevant question about physical or nervous limitations.

5. PSID Data and Preliminary Evidence

The inherent problems with the data previously utilized in ADA analysis apply to our own disability measure, which underscores the fragility of all such empirical studies. The advantage of the PSID, however, is that we are at least able to observe the identical individuals over time. The data appear in two formats: a year-by-year family file in which most of the survey questions pertain to the household head and a cross-year individual file with information on each household member. We create our dataset by matching the individual-level file to the family-level version for the years 1981 to 1997 and restricting observations to household heads (because survey questions regarding most variables of interest, including disability status, are asked only of the household head).¹⁹

Our initial matching algorithm yields longitudinal data for 10,934 individuals comprising 107,844 person-year observations. In order to generate data suitable for empirical testing, we restrict our sample in a number of ways. First, we collapse the race

reports and compare white versus non-white individuals.²⁰ Second, because we are concerned with market outcomes, we include individuals most likely to appear in the labor force, namely male household heads between the ages 21 and 65.²¹ After imposing these restrictions, our operational dataset consists of 7,120 individuals observed in at least one year during the period 1981 to 1996.

Tables 1 and 2 report summary statistics for two versions of the operational dataset: one for the overall “unrestricted” sample of 64,607 person-year observations and another describing the “restricted” sample, the 1437 individuals appearing each year from 1981 to 1996.²² The PSID question used to identify disability status among heads of households is: “Do you . . . have any physical or nervous condition that limits the type of work or the amount of work you can do?”²³

On average, individuals report being disabled 13% and 12% of the time in the unrestricted and restricted datasets, respectively. Note that this is different from the claim that 14% of the household heads in our data are disabled, since, as we show, disability reports vary over time for the same individual. In the unrestricted (restricted) data about 4900 (884) people, or 69% (62%) never report a disability, while only 21 individuals, or less than 1%, report having a disability each year they appear in the data.

With respect to average employment, PSID interviewees held jobs 85% of the time when the interview was conducted. This response variable reflects answers to a question about *current* employment status and, as such, potentially understates the extent of employment during the data collection year. The effects of relying on this variable for analyzing the impact of the ADA in a multiple regression framework are discussed in the

next section. We therefore use an alternative measure of annual employment available in the PSID survey: annual weeks worked, which is asked retrospectively of respondents.²⁴ Over the sixteen-year period of observation, individuals averaged approximately 41 weeks worked per year. Converting nominal earnings to constant 2000 dollars, we find that (unconditional) average annual wages and salaries amounted to just over \$31,000, while the average real hourly wage was approximately \$17.

Turning to demographic characteristics, we find that 60 percent of the men in our dataset were white and over the period 1981-1996 they had an average age of 39 years old. Most (37%) had obtained only a high school degree, while another 21% had some college, and another 22% had at least a college degree. Finally, we collected data on unemployment rates to proxy for economic conditions in each state in which PSID participants resided; the average across states and years was just under 7%.

TABLE 1: SUMMARY STATISTICS (UNRESTRICTED DATASET)

Variable	Number of Observations	Mean	Standard Deviation
Currently Disabled	64525	0.13	0.34
Currently Employed	64601	0.85	0.36
Annual Weeks Worked	56465	40.79	16.94
Real Annual Wages (2000)	63629	31055	36151
Real Hourly Wages (2000)	48833	17.29	33.30
Age	64607	39.16	11.20
White	64607	0.60	0.49
Less than high school education	64607	0.20	0.40
High School Graduate	64607	0.37	0.48
Some College Education	64607	0.21	0.40
College Graduate	64607	0.14	0.35
Some Postgraduate Study	64607	0.08	0.27
State Unemployment Rate	63190	6.78	1.98

Sources: Panel Study of Income Dynamics, 1981-1996 and the United States Statistical Abstract for unemployment.

Note: The incidence of disability is measured using the PSID's work limitation question.

TABLE 2: SUMMARY STATISTICS (RESTRICTED DATASET)

Variable	Number of Observations	Mean	Standard Deviation
Currently Disabled	22974	0.12	0.33
Currently Employed	22992	0.90	0.30
Annual Weeks Worked	20521	43.34	14.03
Real Annual Wages (2000)	22790	37508	41593
Real Hourly Wages (2000)	18759	19.23	22.18
Age	22992	40.77	9.04
White	22992	0.78	0.41
Less than high school education	22992	0.14	0.35
High School Graduate	22992	0.35	0.48
Some College Education	22992	0.22	0.41
College Graduate	22992	0.17	0.38
Some Postgraduate Study	22992	0.12	0.32
State Unemployment Rate	22869	6.81	2.08

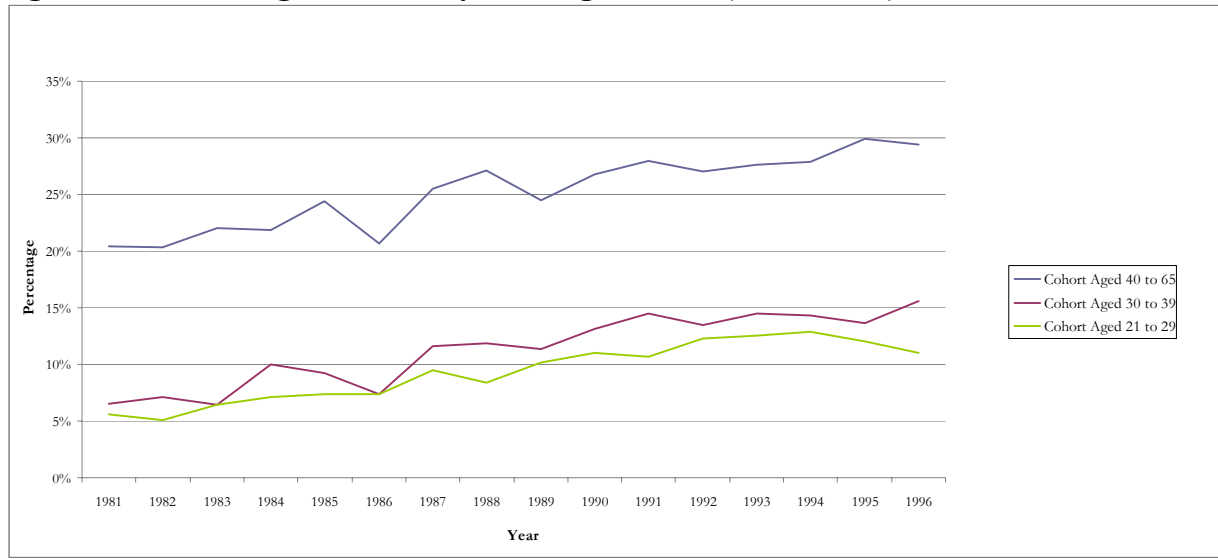
Sources: Panel Study of Income Dynamics, 1981-1996 and the United States Statistical Abstract for unemployment.

Note: The incidence of disability is measured using the PSID's work limitation question.

Figure 1A provides information on the percentage of individuals reporting a disability over the duration of our observation period. Since one might expect that the aging process would (naturally) drive this proportion upward, we separate the data into three cohorts defined by age reported in 1981. In keeping with our intuition, the disabled share in each cohort increases from 1981 to 1996, and there is a nearly monotonic relationship between initial cohort age and the level of each curve (with older initial cohorts reflecting greater disability shares). Note that the share of disabled persons in the cohort aged 40 to 65 starts out nearly four times the amount for those aged 21 to 39 and rises to a level of 30% by 1996. The cohorts aged 21 to 39, on the other hand, exhibit disabled population shares from 11% to 16% in the final year of observation. Despite the

steady increase in disability reports over time for each cohort, the share drops unexpectedly in 1986 (especially for the older two cohorts) without any attendant increase in the overall size of the cohort.²⁵

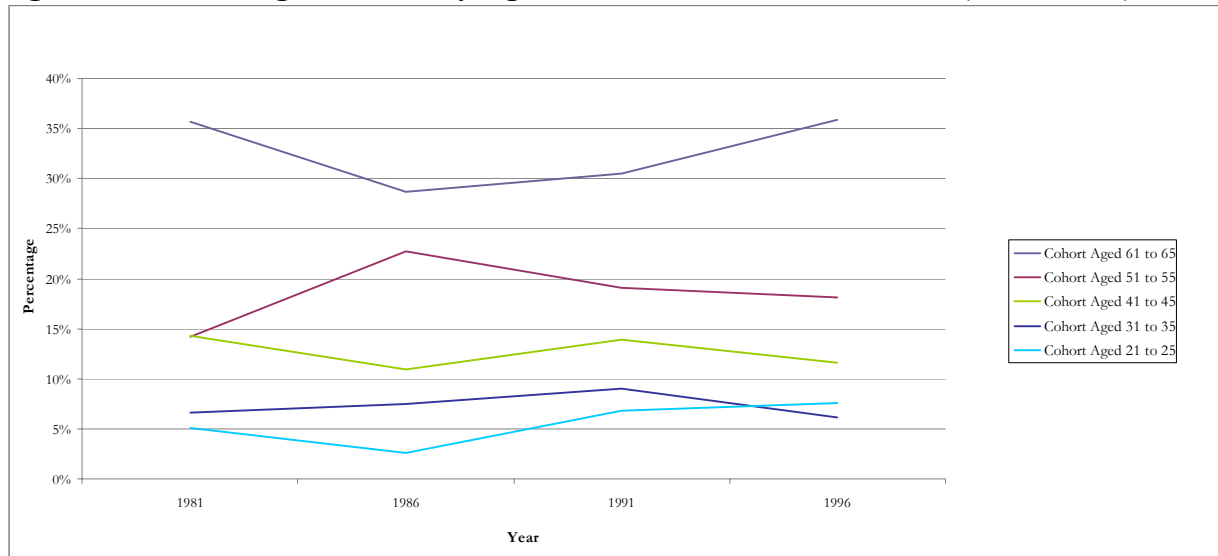
Figure 1A: Percentage Disabled by 1981 Age Cohort (1981 - 1996)



Source: Panel Study of Income Dynamics

Figure 1B portrays the evolution of cohort disability shares when we fix the age groups but allow their members to change over time. If the relationship between age and disability shifted significantly between 1981 and 1996, then our empirical model should control for this phenomenon accordingly. However, all five curves in Figure 1B suggest that disability shares were relatively constant in 1981 and 1996, albeit with some intervening swings over time within each cohort.

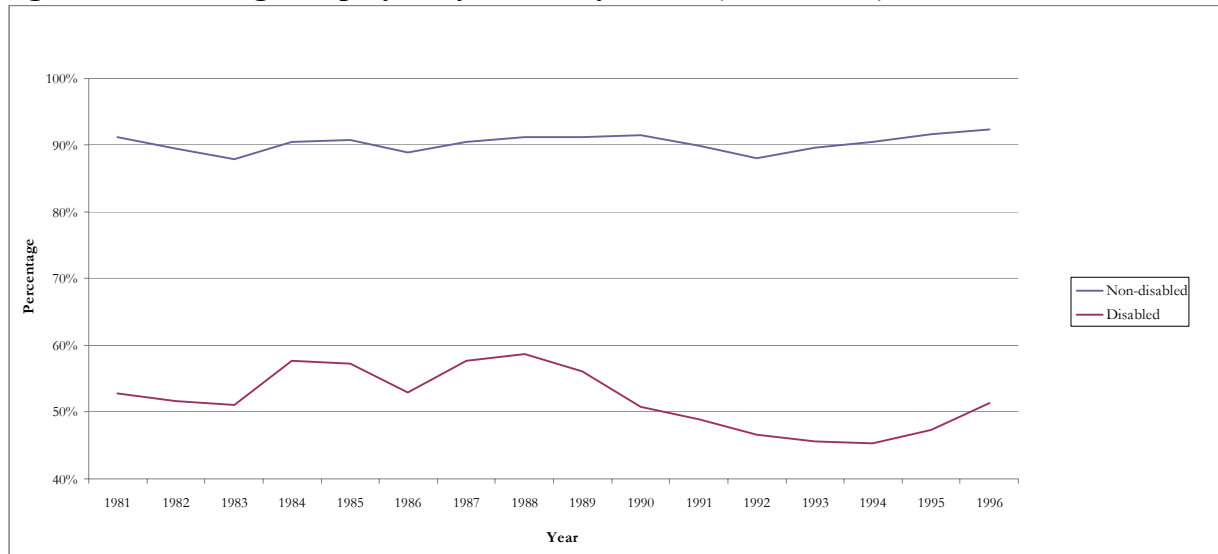
Figure 1B: Percentage Disabled by Age Cohort in Five-Year Intervals (1981 - 1996)



Source: Panel Study of Income Dynamics

Figures 2 through 5 document changes in employment and wage levels by disability status. As such, they represent the starting point for our inquiry into the labor market effects of the ADA on the disabled population relative to non-disabled people. Figure 2 shows how the percentage of non-disabled individuals with jobs remained relatively constant (averaging 90% between 1981 and 1996), while employment in the disabled community fell during the 1990s from a peak of 54% in 1988 to about 45% by 1995. Given that the ADA was signed into law on July 26, 1990, this deterioration of employment conditions for the disabled corresponds with the passage and initial enforcement of the ADA. At first glance, then, this crude measure of employment status intimates that disabled individuals suffered worse labor market outcomes under the ADA regime, while the non-disabled labor force was largely unaffected.

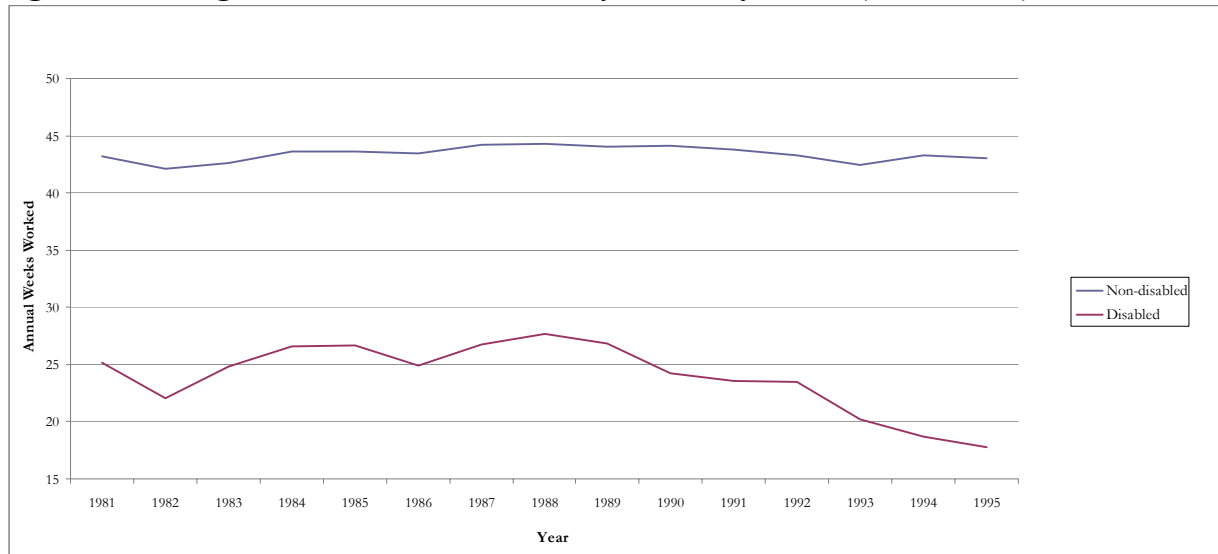
Figure 2: Percentage Employed by Disability Status (1981 - 1996)



Source: Panel Study of Income Dynamics

Figure 3 indicates a more dramatic decline in disabled employment rates using a finer measure of participation.²⁶ Observing the average number of annual weeks worked, the percentage of working non-disabled people remains fairly stable at about 43 weeks worked per year. Disabled individuals also experienced labor supply stability through the 1980s with an average of 24 weeks worked. This five-month difference in average annual employment between the two groups is significant in its own right. However, despite no material change for the non-disabled in the 1990s, the gap between the non-disabled and disabled grew substantially, with average weeks worked among the disabled declining from 23 to 18 between 1991 and 1995 (although again peak disabled employment appears to occur in 1988 at roughly 27 weeks).

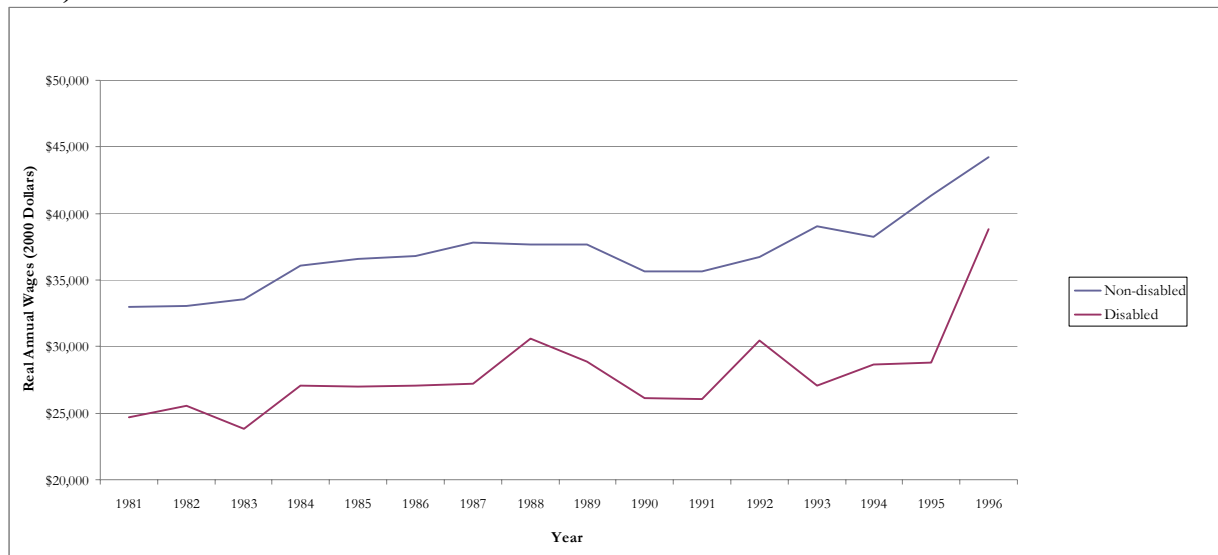
Figure 3: Average Annual Weeks Worked by Disability Status (1981 - 1995)



Source: Panel Study of Income Dynamics

Figures 4 and 5 illustrate changes in real wages and salaries earned (measured in constant 2000 dollars). The former shows total annual amounts, and the latter depicts earnings per annual hours worked. Examining relative changes in annual wages, we observe real income increases for both the disabled and non-disabled populations, which follow the same general pattern. Consequently, Figure 4 provides little to no evidence that the ADA had any appreciable effect on relative wages of the male household heads in our sample.

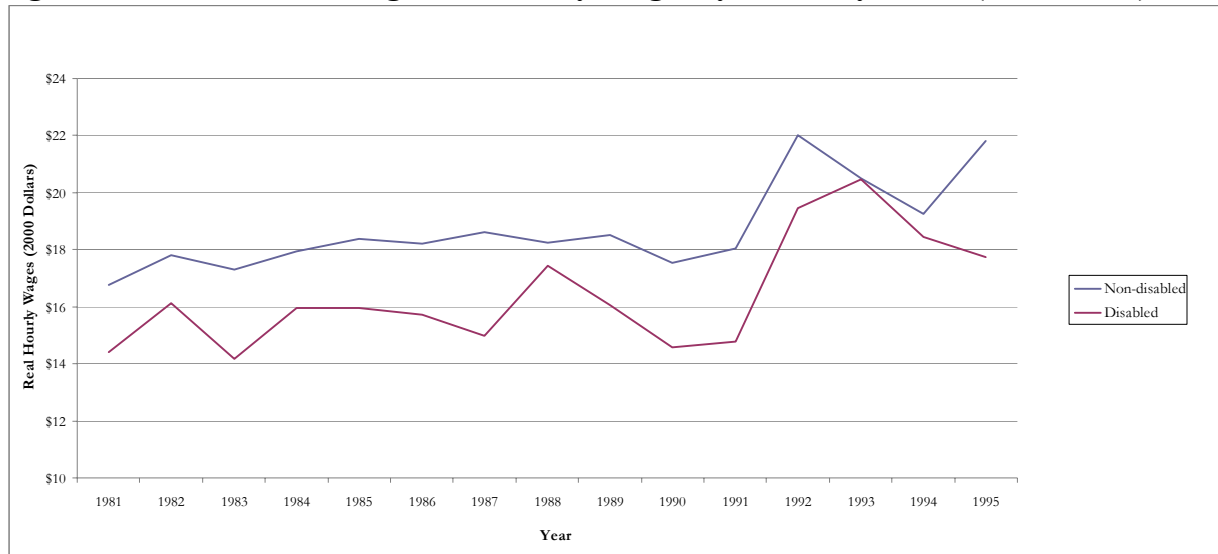
Figure 4: Conditional Average Real Annual Wages by Disability Status (1981 - 1996)



Source: Panel Study of Income Dynamics

Because Figure 4 ignores the extent to which hours worked affect the wages of non-salaried employees, we plot the average hourly real wage for both groups in Figure 5. An average distance of about \$2.30 separates the hourly wages of the non-disabled and disabled during the 1980s, and the only noticeable difference arises in 1983 when wages for the disabled become \$3 less than for the non-disabled. Unlike the time path for annual wages, hourly wages converge substantially by 1993 but *diverge* by 1995. Based on these data, one might be tempted to conclude that disabled individuals suffered a relative hourly wage decrease following final enforcement of the ADA in 1994.

Figure 5: Conditional Average Real Hourly Wages by Disability Status (1981 - 1995)



Source: Panel Study of Income Dynamics

However, deriving any firm conclusions from the evidence in Figures 2 through 5 risks the spurious identification of a causal role for the ADA. Such hasty reasoning may result either from failure to control for other individual characteristics that affect employment and wages and are correlated with disability (or at least the propensity to report a disability) or from the perilous strategy of measuring ADA effects after 1992 or 1994. The econometric analysis in subsequent sections isolates the standard “difference-in-differences” (“DD”) between the two categories so often employed in program evaluations. Figure 3 for example offers only a casual glimpse into the relative fortunes of disabled and non-disabled Americans. Our best statistical approximation to experimental conditions categorizes the two groups as treatment and control observations and determines the difference in annual differences for each set. Many DD analyses assume the existence of well-defined “before” and “after” periods relative to the enactment of a law or establishment of a program. As the following sections explain,

however, our approach avoids this somewhat arbitrary assignment and considers the difference-in-differences for each year relative to 1981.

6. Empirical Results

A. Employment Status

We begin our examination of the ADA's effect on labor market outcomes for the disabled population with an empirical model for employment status. Using a difference-in-differences framework, we determine whether the legislation impacted changes in the year-to-year probability that a non-disabled individual was employed relative to a disabled person. Thus, the general empirical specification is:

$$Employed_{it} = \beta_0 + \delta * dis_{it} + \sum_{j=82}^J \beta_{j-81} I_j * dis_{it} + \sum_{j=81}^J \lambda_{j-80} I_j + \varepsilon_{it} \quad (1)$$

where i and t index individuals and years, respectively; I_j is an indicator for each year; J is the maximum year of observation for the relevant dependent variable; and dis takes the value one when the individual is disabled and zero otherwise. Each of the coefficients β on the sixteen interaction terms represents the difference-in-differences estimator for a given year relative to 1981. Although this model generates raw DDs, its lack of controls for unobservable characteristics at the individual level implies that results based on (1) are naïve at best. The standard method of adjustment involves adding a vector of covariates that influence employment outcomes. In our fully specified model, we also add controls for individual fixed effects. This method essentially differences out factors specific to each individual that remain constant across time. The estimation process thus compares annual DDs with respect to the individual rather than across the pooled set of

records for each year. The data underlying all regressions in Part V are from the unrestricted set.

Equation (1) represents a linear specification for the difference-in-differences estimator whenever the dependent variable is continuous. The logistic equivalent to this model without fixed effects estimates the effects of the ADA using all 10,934 individuals, while the inclusion of fixed effects (conditional logit) reduces the sample size by about 58% to 6353 persons.²⁷

Note that the employment status question is asked at the time of the PSID interview, which can lead to imprecision in our estimates.²⁸ To see this, consider a respondent *A*, employed from January through May, at which time she lost her job, and respondent *B* who was unemployed the entire year. *A* and *B* will be observationally equivalent with respect to this employment status question as long as *A* is interviewed after the May job loss. Sample attrition and the crudeness of the employment status question prompt us to consider an alternative measure.

Table 3 displays estimates from equation (1) when *Employed_{it}* is measured as the number of weeks worked per year. PSID interviewers generate this variable through retrospective questions; in other words, respondents reveal how many weeks they worked in year *t* during an interview in year *t* + 1. Because these data are not available in the 1997 annual data, the observation period ends in 1995. The first column of Table 3 shows a strong and increasing disparity in annual weeks worked for the disabled beginning in 1993, the year after the ADA first became effective. Relative to 1981, disabled workers were employed between one and two months less than non-disabled laborers in the period

1993-1995. Adding controls in the second column increases the magnitude of the DD estimates from 1993 to 1995 (in absolute value) while also suggesting an earlier ADA effect dating to 1990, the year of enactment. However, once we control for individual fixed effects, the point estimates fall by about one half and are significant only in 1994 and 1995. Therefore, the regression analysis in Table 3 appears to validate the causal observation from Figure 3 that employment levels among the disabled declined following enactment of the ADA.

TABLE 3: ANNUAL WEEKS WORKED, UNRESTRICTED DATASET (1981 – 1995)

	(1)	(2)	(3)
DISABLED * 1982	-2.10 [*] (1.28)	-1.78 (1.18)	-0.26 (0.96)
DISABLED * 1983	0.18 (1.32)	1.12 (1.24)	2.48 ^{**} (1.08)
DISABLED * 1984	0.98 (1.38)	0.15 (1.27)	0.98 (1.09)
DISABLED * 1985	1.07 (1.47)	0.86 (1.37)	0.86 (1.16)
DISABLED * 1986	-0.58 (1.57)	-1.41 (1.48)	-0.11 (1.27)
DISABLED * 1987	0.58 (1.52)	-0.43 (1.39)	0.66 (1.23)
DISABLED * 1988	1.36 (1.55)	0.74 (1.43)	0.55 (1.22)
DISABLED * 1989	0.85 (1.55)	-0.53 (1.41)	-0.50 (1.24)
DISABLED * 1990	-1.89 (1.51)	-2.94 ^{**} (1.39)	-0.83 (1.21)
DISABLED * 1991	-2.18 (1.53)	-3.34 ^{**} (1.41)	-1.03 (1.21)
DISABLED * 1992	-1.73 (1.49)	-3.04 ^{**} (1.38)	-0.46 (1.20)
DISABLED * 1993	-4.18 ^{***} (1.54)	-4.98 ^{***} (1.44)	-0.93 (1.27)
DISABLED * 1994	-6.56 ^{***} (1.55)	-6.74 ^{***} (1.48)	-3.35 ^{**} (1.32)
DISABLED * 1995	-7.21 ^{***} (1.63)	-7.50 ^{***} (1.52)	-3.65 ^{***} (1.37)
COVARIATES INCLUDED?	No	Yes	Yes
FIXED EFFECTS?	No	No	Yes
<i>N</i>	56400	55341	55341

Source: Panel Study of Income Dynamics.

Notes: * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. Column 1 coefficients are difference-in-difference estimates from equation (1) when *Employed* is measured as weeks worked per year. Column 2 adds time-varying covariates to equation (1) and Column 3 adds time-varying covariates and individual fixed-effects to equation (1). Estimates are based on the unrestricted sample.

In contrast to the regressions analyzing wage differentials, we do not restrict the dependent variable in Table 3 to non-zero values. (About 7% of the observations used in the unrestricted dataset indicate no weeks worked.) In most empirical analyses in which the values of the dependent variable clump around zero, OLS yields biased coefficient estimates. Nevertheless, because the DD methodology merely estimates differences in averages (unadjusted in Column 1 and adjusted in Columns 2 and 3), the presence of zero observations does not contaminate the coefficients on the interaction terms.

B. Annual Earnings

Section 102 of the ADA prohibits not only hiring bias against the disabled, but also discrimination “against a qualified individual with a disability because of the disability of such individual in regard to . . . employee compensation . . . and other terms, conditions, and privileges of employment.” Thus, if the law served its intended protective function, we should not observe a significant difference between changes in relative wages. Unlike the approach taken with annual weeks worked, we regress annual earnings on our DD interaction terms *conditional on receiving positive wages*. Since detecting wage discrimination depends on comparisons of individuals receiving a non-zero wage, the empirical of Table 4 require exclusion of zero-wage observations.

In the next two sections, we estimate the model:

$$Earnings_{it} = \beta_0 + \delta * dis_{it} + \sum_{j=82}^J \beta_{j-81} I_j * dis_{it} + \sum_{j=81}^J \lambda_{j-80} I_j + \varepsilon_{it} \quad (2)$$

where $Earnings_{it}$ represents either annual or hourly wages. As in our analysis of employment levels, estimates for the baseline model captured by (2) are given in the first

column, while successive controls for individual time-varying characteristics and fixed effects are added in Columns 2 and 3, respectively.

TABLE 4: CONDITIONAL ANNUAL WAGES EARNED, UNRESTRICTED DATASET (1981 – 1996)

	(1)	(2)	(3)
DISABLED * 1982	746.34 (1621.09)	-249.65 (1526.83)	-2154.84 (1417.31)
DISABLED * 1983	-1439.19 (1559.51)	-1514.88 (1524.22)	-1345.93 (1313.98)
DISABLED * 1984	-677.34 (1593.68)	-2421.47 (1546.67)	-2767.64 ^{**} (1332.78)
DISABLED * 1985	-1327.12 (2081.52)	-3585.18 [*] (1931.97)	-1477.31 (2030.66)
DISABLED * 1986	-1466.81 (2165.08)	-3487.80 [*] (2006.62)	-3396.85 (2133.16)
DISABLED * 1987	-2292.60 (1999.33)	-5073.80 ^{***} (1846.38)	-2669.00 (2113.14)
DISABLED * 1988	1277.12 (2969.06)	-867.08 (2754.13)	-2713.09 (2201.51)
DISABLED * 1989	-472.67 (1851.57)	-3014.24 [*] (1747.88)	-3031.66 (1886.94)
DISABLED * 1990	-1230.19 (1638.32)	-2292.81 (1609.79)	-1958.07 (2301.52)
DISABLED * 1991	-1305.43 (1934.78)	-2492.40 (1767.55)	-2453.83 (2165.14)
DISABLED * 1992	2028.98 (2051.77)	-563.66 (1896.29)	-261.97 (2283.61)
DISABLED * 1993	-3601.38 [*] (2049.20)	-6315.26 ^{***} (1875.92)	-4472.93 ^{**} (2234.50)
DISABLED * 1994	-1341.10 (2094.20)	-3896.69 [*] (2065.38)	-2716.29 (2240.93)
DISABLED * 1995	-4185.18 [*] (2164.31)	-4315.02 ^{**} (1930.20)	-3075.65 (2169.05)
DISABLED * 1996	2903.04 (5285.71)	1537.88 (5297.89)	478.99 (4904.73)
COVARIATES INCLUDED?	No	Yes	Yes
FIXED EFFECTS?	No	No	Yes
<i>N</i>	54192	53050	53050

Source: Panel Study of Income Dynamics. *Notes:* * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. Column 1 coefficients are difference-in-difference estimates from equation (2) where *Earnings* is measured as dollars per year. Column 2 adds time-varying covariates to equation (2) and Column 3 adds time-varying covariates and individual fixed-effects to equation (2). Estimates are based on the unrestricted sample.

Although the signs of the point estimates in all three columns of Table 4 suggest a relative decline in wages among the disabled, no clear and sustained pattern of ADA influence emerges as with weeks worked. The emergence of (muted) statistical significance over time contrasts sharply with the results in Table 3. Such findings are most pronounced in Column 2, in which three DD estimates achieve marginal significance. Based on these estimates, the unrestricted data do not support the proposition that the ADA caused relative wages for disabled workers to deteriorate, although again 1993 appears to be an unusually bad year for disabled workers. The absence of a sustained pattern in wage differentials, combined with the positive coefficients in all three models for 1996, suggests (in keeping with Figure 4) that the wage gap might have begun to close in the mid 1990s. Whether this narrowing trend continued into the current century cannot be determined until additional data become available.

C. Hourly Earnings

Measurements of earnings power based solely on annual income may tell a different story than measurements based on hourly wage rates. Dividing annual earnings by the number of hours worked may refine our empirical understanding of earnings differences between disabled and non-disabled workers.

As Table 5 shows, there is no evidence from our difference-in-differences estimates that the disabled experienced an ADA-induced change in relative hourly wages. Consistent with a pattern observed earlier in Figure 5, the disabled experienced a notable (albeit short-lived) hourly wage increase relative to the non-disabled in 1993. Although

this was the largest change in absolute value over the entire observation period, the 1993 change was still not statistically significant in any of the three models.

The evidence in this Section would be consistent with the following conclusions: First, the ADA had a *negative* impact on the employment levels of the disabled relative to the non-disabled. The fact that our data extend back to the early 1980s helps rule out the possibility that the employment declines that we observe (and that were found in other recent studies) originated prior to the adoption of the ADA. Second, the ADA did *not* cause any appreciable decline in the wages of disabled workers (relative to the non-disabled). In other words, it would appear that the ADA did not induce a simple adverse shift in the demand for disabled workers, since this would result in both a decline in employment and a reduction in the wages of disabled workers. Rather, our initial evidence from our unrestricted sample would be consistent with a story in which, perhaps most notably when the ADA took effect in 1993, employers were particularly wary of disabled workers (perhaps out of concern that the law would impose onerous burdens on their employers), but that disabled workers who did secure employment retained their previous level of hourly wages. In the next Section, we consider whether these tentative conclusions will remain robust when we eliminate compositional changes in our sample of workers.

TABLE 5: CONDITIONAL HOURLY WAGES EARNED, UNRESTRICTED DATASET (1981 – 1995)

	(1)	(2)	(3)
DISABLED * 1982	0.65 (1.10)	0.42 (1.10)	-0.79 (1.02)
DISABLED * 1983	-0.76 (0.84)	-1.02 (0.83)	-1.45 ^{**} (0.72)
DISABLED * 1984	-0.37 (1.02)	-0.40 (1.03)	-0.74 (0.90)
DISABLED * 1985	-0.06 (1.05)	-1.31 (1.00)	-0.36 (1.22)
DISABLED * 1986	-0.14 (1.14)	-1.02 (1.06)	-1.08 (1.20)
DISABLED * 1987	-1.27 (1.07)	-1.98 ^{**} (1.01)	-1.35 (1.30)
DISABLED * 1988	1.55 (1.72)	0.75 (1.62)	-0.11 (1.33)
DISABLED * 1989	-0.11 (1.13)	-0.85 (1.08)	-1.30 (1.19)
DISABLED * 1990	-0.62 (0.82)	-0.76 (0.81)	-0.94 (1.20)
DISABLED * 1991	-0.93 (0.99)	-1.27 (0.95)	-1.69 (1.31)
DISABLED * 1992	-0.21 (1.94)	-1.09 (1.93)	-0.03 (1.55)
DISABLED * 1993	2.34 (3.09)	1.64 (3.08)	1.44 (3.56)
DISABLED * 1994	1.52 (1.45)	1.40 (1.50)	0.19 (1.69)
DISABLED * 1995	-1.73 (2.10)	-1.64 (2.06)	-1.19 (2.16)
COVARIATES INCLUDED?	No	Yes	Yes
FIXED EFFECTS?	No	No	Yes
<i>N</i>	45260	44470	44470

Source: Panel Study of Income Dynamics. *Notes:* * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. Column 1 coefficients are difference-in-difference estimates from equation (2) where *Earnings* is measured as dollars per hour. Column 2 adds time-varying covariates to equation (2) and Column 3 adds time-varying covariates and individual fixed-effects to equation (2). Estimates are based on the unrestricted sample.

7. Analyzing the Restricted Sample

In this Section, we restrict the data to include only those individuals who appear for the entire observation period. In Section 5, the baseline regressions without individual fixed effects counted persons whether or not they appeared in the reference year 1981. Thus, for example, the difference-in-differences estimate for 1994 may have been calculated using information on an interview respondent who first appeared in 1988. As such, the logic of the regression exercise would be confounded by this compositional change, especially if the individuals appearing after 1981 significantly altered the relative numbers of disabled persons in the data. The inclusion of individual fixed effects in the most fully specified model ameliorated the “anchoring” problem by requiring that all individuals appear in 1981. Otherwise the regression would drop the entire set of observations for that person. Still, the unrestricted data permitted potentially troublesome compositional changes as some workers dropped out of the sample over time. For example, even if two individuals, A and B, appeared continuously from 1981, but B contributed to estimates for three more years than A, estimates from those three years would not be fully comparable with the preceding results. The restricted dataset precludes compositional change by including only those persons that appear for the same (maximum) duration. As discussed in Section 4, the restricted sample contains the employment and wage histories of 1437 PSID respondents from 1981 to 1996, of whom 553 were identified as “disabled” for at least one year during the 16 years of data.

The first column of Table 6 initially suggests an even more robust decline in disabled employment levels than Table 3, as the magnitudes of the last three estimates in

Column 1 range from -5.7 to -8.6. But this story changes dramatically when one adds demographic covariates (Column 2) or controls for individual fixed effects (Column 3). Thus, if one accepts the proposition that the restricted dataset permits cleaner comparisons of individuals across time (i.e., it maximizes the value of the PSID's longitudinal structure, particularly when controlling for individual fixed effects), then Table 6 provides evidence against a depressive employment effect from the ADA. Moreover, the vast majority of point estimates (11 of 14) in Column 3 are positively signed, though not statistically significant at standard levels. This suggests that at least relative to 1981, male disabled household heads were not suffering employment losses (even if there may be some hint from the table that the improvements were weakening in the 1990s).

TABLE 6: ANNUAL WEEKS WORKED, RESTRICTED DATASET (1981 – 1995)

	(1)	(2)	(3)
DISABLED * 1982	-0.72 (3.04)	-1.20 (2.98)	0.98 (2.50)
DISABLED * 1983	-0.40 (3.06)	-0.10 (3.02)	2.17 (2.67)
DISABLED * 1984	1.31 (2.93)	1.87 (2.93)	2.71 (2.40)
DISABLED * 1985	-0.76 (3.33)	0.18 (3.47)	0.78 (2.99)
DISABLED * 1986	-3.82 (3.64)	-2.53 (3.81)	-0.78 (3.22)
DISABLED * 1987	-0.01 (3.20)	1.55 (3.34)	2.86 (2.95)
DISABLED * 1988	-0.57 (3.10)	1.99 (3.32)	3.07 (2.64)
DISABLED * 1989	0.08 (2.93)	3.24 (3.27)	3.70 (2.66)
DISABLED * 1990	-5.12 (3.27)	-1.26 (3.62)	0.21 (2.89)
DISABLED * 1991	-6.32* (3.36)	-2.06 (3.80)	-1.51 (2.98)
DISABLED * 1992	-1.51 (3.21)	2.85 (3.67)	2.48 (2.84)
DISABLED * 1993	-5.66* (3.30)	0.36 (3.95)	1.54 (3.16)
DISABLED * 1994	-8.35** (3.52)	-2.19 (4.28)	-1.11 (3.39)
DISABLED * 1995	-8.62** (3.62)	-0.16 (4.47)	1.36 (3.57)
COVARIATES INCLUDED?	No	Yes	Yes
FIXED EFFECTS?	No	No	Yes
<i>N</i>	8938	8912	8912

Source: Panel Study of Income Dynamics. *Notes:* * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. Column 1 coefficients are difference-in-difference estimates from equation (1) when *Employed* is measured as weeks worked per year. Column 2 adds time-varying covariates to equation (1) and Column 3 adds time-varying covariates and individual fixed-effects to equation (1). Estimates are based on the restricted sample.

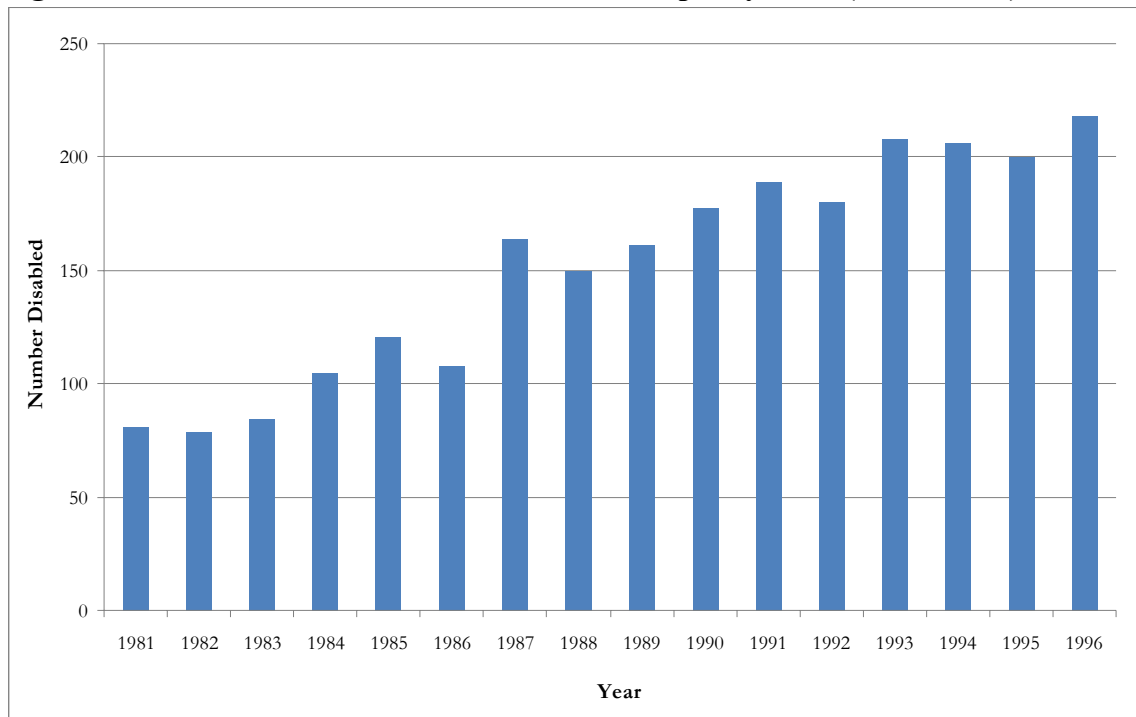
Table 7 examines the impact of the ADA on annual earnings for the restricted sample and finds, as was the case in the unrestricted sample, that there are no consistent patterns of change in the relative wages of disabled workers that can be confidently ascribed to the ADA. Once again 1993 stands out as an unusually bad year for the disabled with earnings shortfalls (relative to 1981) ranging from \$10,000 (Column 1) to \$5000 (Column 3). With the large year-to-year swings in the various estimated effects and the generally statistically insignificant coefficients, though, one is uncertain about what to conclude from this Table. On the one hand, the poor performance in 1993 suggests a story that the ADA damaged the earnings picture for the disabled by a substantial amount. A year later, however, the picture seemed to improve substantially. Might this suggest that employer fears about the possible costs of the ADA initially caused them to shun disabled workers, but this initial effect was quickly overturned? On the other hand, the poor performance in 1995 and 1996 may suggest that 1994 was more of the outlier. Still, 1996 does not look all that different from 1987 in terms of the relative earnings of the disabled, which may suggest that forces other than the ADA were the primary factors in driving the apparent deterioration in the earnings of the disabled.

TABLE 7: CONDITIONAL ANNUAL EARNINGS, RESTRICTED DATASET (1981 – 1996)

	(1)	(2)	(3)
DISABLED * 1982	39.15 (2961.84)	-1052.64 (2772.77)	-2782.06 (2247.19)
DISABLED * 1983	-4719.64* (2758.93)	-4221.60 (2739.45)	-1714.85 (1992.71)
DISABLED * 1984	-2745.25 (2583.90)	-2610.32 (2715.76)	-1746.00 (1997.35)
DISABLED * 1985	-7072.15** (3431.43)	-5604.07* (3038.50)	-1348.42 (4067.12)
DISABLED * 1986	-5275.17 (3801.62)	-2668.13 (3500.84)	-1348.96 (4007.97)
DISABLED * 1987	-8634.13** (3750.00)	-6209.00* (3318.33)	-3076.81 (4173.82)
DISABLED * 1988	-7146.01* (4111.81)	-5002.83 (3691.88)	-2901.21 (4185.68)
DISABLED * 1989	-4471.59 (3375.95)	-2798.21 (3315.48)	-4129.82 (3558.88)
DISABLED * 1990	-5970.43* (3230.28)	-2613.86 (3351.33)	-2326.68 (4608.96)
DISABLED * 1991	-8632.74** (3717.52)	-5330.25 (3413.56)	-3546.62 (3925.13)
DISABLED * 1992	-5749.60 (4376.94)	-5105.32 (4270.72)	-3621.80 (4379.63)
DISABLED * 1993	-10,357.58** (4126.61)	-7234.62* (3823.49)	-5256.63 (4137.56)
DISABLED * 1994	-4135.76 (4660.38)	-911.29 (4592.39)	-1517.86 (3980.10)
DISABLED * 1995	-9655.74** (4548.16)	-5885.46 (4355.23)	-4675.97 (3617.69)
DISABLED * 1996	-5379.76 (4367.12)	-3067.75 (4586.23)	-5261.67 (4406.75)
COVARIATES INCLUDED?	No	Yes	Yes
FIXED EFFECTS?	No	No	Yes
<i>N</i>	19762	19648	19648

Source: Panel Study of Income Dynamics. *Notes:* * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. Column 1 coefficients are difference-in-difference estimates from equation (2) where *Earnings* is measured as dollars per year. Column 2 adds time-varying covariates to equation (2) and Column 3 adds time-varying covariates and individual fixed-effects to equation (2). Estimates are based on the unrestricted sample.

Figure 6: Number Disabled in the Restricted Sample by Year (1981 - 1996)



Source: Panel Study of Income Dynamics

One explanation for why the Column 1 estimates in Table 7 suggest a significant downward wage effect, while Column 3 does not might be differential self-reports of disability status over time. Figure 6 plots the number of individuals reporting a disability among the 532 people in the restricted sample who reported a disability at least once but not for the entire observation period. Among these individuals, we do observe changes in self-identification that may have been influenced by the ADA's passage (or modifications to Social Security and other transfer payments). Since our constant restricted sample ages over time, the overall trend in the number reporting a disability increases as one might expect. However, with the exception of the 1986-1997 change (which should be discounted because of data reporting problems in 1986), the 1992-1993 increase represents the largest jump in our data. In fact, from 1993 through 1996 the number never

drops below 200, which is 38% of the 532 “sometimes-disabled” population. This post-1992 spike might be attributed to hopes that the ADA might benefit people who claim a disability them. Thus, when we control for individual fixed effects in Column 3, we capture the probable fact that those reporting a disability have better outcomes overall regardless of whether they claim to be disabled in a given year.

Table 8 estimates the hourly wage effects of the ADA, again using the restricted sample. While the unrestricted data had yielded no evidence of a sustained ADA impact on the hourly wages of the disabled, the same regressions estimated on the restricted set does suggest an adverse ADA impact on hourly wages (note again the adverse estimate in 1993). But when one looks at the timing of these adverse wage shifts the link to the ADA becomes less clear. For example, controlling for individual fixed effects in Column 3 reveals an adverse trend in hourly wages for the disabled, but one which begins in 1986, well before the ADA became law. While relative real hourly wages of the disabled fell by about \$6 beginning in 1992, the bulk of this decline first appears in 1986 as a \$4 shortfall relative to the non-disabled in 1981. Since the series of negative point estimates remains rather consistent from the pre-ADA period through 1994, we cannot readily attribute the decline in hourly wages to the legislation itself. Moreover, while one might conclude that the early days of the ADA at least did not help the hourly wages of the disabled, the estimates for 1995 suggest some positive news on the earnings front for the disabled had occurred in that final year of our data. Might this suggest that a decade of earnings erosion for the disabled, initially caused by other forces in the American labor market

than the ADA, was starting to reverse itself in 1995, or is this just another ephemeral swing?

TABLE 8: CONDITIONAL HOURLY WAGES EARNED, RESTRICTED DATASET
(1981 – 1995)

	(1)	(2)	(3)
DISABLED * 1982	0.01 (2.07)	-0.64 (2.06)	-1.25 (2.23)
DISABLED * 1983	-2.30 (1.71)	-2.59 (1.64)	-3.37 (2.07)
DISABLED * 1984	1.08 (3.33)	0.40 (3.25)	-0.72 (2.77)
DISABLED * 1985	0.19 (2.32)	-1.22 (2.28)	-0.79 (2.08)
DISABLED * 1986	-2.72 (2.66)	-2.63 (2.48)	-4.27** (1.83)
DISABLED * 1987	-2.15 (1.99)	-2.71 (1.87)	-5.26*** (1.82)
DISABLED * 1988	-0.90 (2.41)	-1.24 (2.33)	-2.88 (2.44)
DISABLED * 1989	-2.62 (1.94)	-3.12 (1.95)	-4.48** (2.14)
DISABLED * 1990	-2.89 (1.87)	-3.52* (1.93)	-5.03** (2.21)
DISABLED * 1991	0.62 (2.97)	1.30 (2.89)	-2.96 (2.48)
DISABLED * 1992	-3.58 (2.29)	-3.65 (2.29)	-6.34** (2.84)
DISABLED * 1993	-5.44** (2.46)	-6.30** (2.48)	-6.54** (2.77)
DISABLED * 1994	-3.91 (2.68)	-4.50 (2.74)	-6.22** (3.11)
DISABLED * 1995	1.59 (9.48)	0.82 (9.21)	0.38 (7.95)
COVARIATES INCLUDED?	No	Yes	Yes
FIXED EFFECTS?	No	No	Yes
<i>N</i>	7194	7169	7169

Source: Panel Study of Income Dynamics. *Notes:* * = significant at the 10% level, ** = significant at the 5% level, and *** = significant at the 1% level. Column 1 coefficients are difference-in-difference estimates from equation (2) where *Earnings* is measured as dollars per hour. Column 2 adds time-varying covariates to equation (2) and Column 3 adds time-varying covariates and individual fixed-effects to equation (2). Estimates are based on the restricted sample.

8. Conclusion

This paper has offered new evidence on the relationship between the Americans with Disabilities Act and the labor market fortunes of the class protected by its landmark provisions. Using individual-level longitudinal data from 1981 to 1996 from a source previously unexploited (the PSID), we examine the annual number of weeks worked, annual earnings, and hourly wages on a full sample of 7120 unique male household heads between the ages of 21 and 65 (a total of 64,607 person-year observations) and a “restricted” sample of 1147 individuals appearing each year from 1981 to 1986. The conflicting pictures that emerge from the analyses of these two different samples sheds new light on, but also counsels caution in reaching final conclusions about, the impact of the ADA.

While our analysis of the unrestricted sample suggested the ADA had a *negative* impact on the employment levels of the disabled relative to the non-disabled but no impact on relative earnings, our evaluation of the restricted sample raised questions about these findings. For the restricted sample—in which we look at the identical 1147 workers over our entire sample period—we see little evidence of adverse effects on weeks worked in our individual fixed effect model, but strong evidence of a pattern of wage decline for the disabled, albeit one that begins in 1986, well prior to the adoption of the ADA.

The restricted data set enables us to see how the identical set of 1147 workers fare over our 1981 – 1996 data period. Recall that 884 of these workers are never disabled over this time frame, 21 are always disabled, and the remaining 242 workers move in and out of what our PSID identifies as a “disabled” condition. While the restricted sample has

the considerable advantage of protecting against any biases caused by law-induced changes in self-identified disability status, the relatively small sample size does generate higher standard errors in our restricted sample estimates that make it hard to distinguish statistically insignificant effects from true non-effects.

Our analysis also underscores once again the difficult employment situation confronted by the average male household head with a disability, one which, at least given our admittedly imprecisely measured definition of disability, has not seen major and sustained improvements during the post-ADA years.

References

- Acemoglu, Daron, and Joshua D. Angrist. 2001. "Consequences of Employment Protection? The Case of the Americans with Disabilities Act," 109 *Journal of Political Economy* 915-957.
- Autor, David H., and Mark G. Duggan. 2003. "The Rise in the Disability Rolls and the Decline in Unemployment," 118 *Quarterly Journal of Economics* 157-205.
- Bagenstos, Samuel R. 2003. "The Americans with Disabilities Act As Welfare Reform," 44 *William and Mary Law Review* 921-1027.
- Beegle, Kathleen and Wendy A. Stock. 2003. "The Labor Market Effects of Disability Discrimination Laws," 38 *Journal of Human Resources* 806-859.
- Blanck, Peter, Lisa Schur, Douglas Kruse, Susan Schwochau, and Chen Song. 2003. "Calibrating the Impact of the ADA's Employment Provisions," 14 *Stanford Law & Policy Review* 267-290.
- Bound, John and Timothy Waidmann. 2002. "Accounting for Recent Declines in Employment Rates among the Working-Aged Men and Women with Disabilities," 37 *Journal of Human Resources* 231-250.
- Burkhauser, Richard V. 1997. "Post-ADA: Are People with Disabilities Expected to Work?," 549 *Annals of the American Academy of Political and Social Science* 71-83.
- Burkhauser, Richard V., Mary C. Daly, and Andrew Houtenville. 2001. "How Working-

Age People with Disabilities Fared over the 1990s Business Cycle,” in Peter P. Budetti, Richard V. Burkhauser, Janice M. Gregory, and H. Allan Hunt, eds., *Ensuring Health and Income Security for an Aging Workforce*. Kalamazoo, MI: W. E. Upjohn Institute for Employment Research.

Burkhauser, Richard V. and Petri Hirvonen. 1989. “United States Disability Policy in a Time of Economic Crisis: A Comparison with Sweden and the Federal Republic of Germany,” 67 *Milbank Quarterly* (Supplement 2, Part 1) 166-194.

Burkhauser, Richard V. and Mathis Schroeder. 2004. “A Cross-National Comparison of the Employment for Men with Disabilities: The United States and Germany in the 1980s and 1990s.” *Michigan Retirement Research Center Research Paper No. 2004-071*,

Burkhauser, Richard V. and David C. Stapleton. 2004. “The Decline in the Employment Rate for People with Disabilities: Bad Data, Bad Health, or Bad Policy?,” 20 *Journal of Vocational Rehabilitation* 185-201.

Burkhauser, Richard V., Andrew J. Houtenville, and Ludmila Rovba. 2007. “Accounting for the Declining Fortunes of Working-Age People with Disabilities,” Unpublished manuscript.

Butler, Richard and James J. Heckman. 1977. “The Government’s Impact on the Labor Market Status of Black Americans: A Critical Review,” in Farrell E. Bloch and

Leonard J. Hausman, eds., *Equal Rights and Industrial Relations*. Madison, WI:
Industrial Relations Research Association.

Collignon, Frederick C. 1997. "Is the ADA Successful? Indicators for Tracking Gains,"
549 *The Annals of the American Academy of Political and Social Science* 129-147.

Degener, Theresia and Gerard Quinn. 2002. "A Survey of International, Comparative and
Regional Disability Law Reform," in Mary Lou Breslin and Silvia Yee, eds.,
Disability Rights Law and Policy: International and National Perspectives. Ardsley,
N.Y.: Transnational Publishers.

DeLeire, Thomas. 2000a. "The Wage and Employment Effects of the Americans with
Disabilities Act," 35 *Journal of Human Resources* 693-715.

DeLeire, Thomas. 2000b. "The Unintended Consequences of the Americans with
Disabilities Act," 23 *Regulation* 21-24.

Dole, Bob. 1994. "Are We Keeping America's Promises to People with Disabilities?—
Commentary on Blanck," 79 *Iowa Law Review* 925-934.

Donohue III, John J. and James Heckman. 1991. "Continuous Versus Episodic Change:
The Impact of Civil Rights Policy on the Economic Status of Blacks," 29 *Journal of
Economic Literature* 1603-1643.

Donohue, John J. and Justin Wolfers. 2005. "Uses and Abuses of Empirical Evidence in
the Death Penalty Debate," 58 *Stanford Law Review* 791-846.

- Epstein, Richard A. 1992. *Forbidden Grounds: The Case Against Employment Discrimination Laws*. Boston: Harvard University Press.
- Hale, Thomas W. 2001. "The Lack of a Disability Measure in Today's Current Population Survey," 124 *Monthly Labor Review* 38-40.
- Heckman, James J. and Brook S. Payner. 1989. "Determining the Impact of Federal Antidiscrimination Policy on the Economic Status of Blacks: A Study of South Carolina," 79 *American Economic Review* 138-177.
- Hotchkiss, Julie L. 2003. *The Labor Market Experience of Workers with Disabilities: The ADA and Beyond*. Kalamazoo, MI: W. E. Upjohn Institute for Employment Research.
- Hotchkiss, Julie L. 2004. "A Closer Look at the Employment Impact of the Americans with Disabilities Act," 39 *Journal of Human Resources* 887-911.
- Houtenville, Andrew J. and Richard V. Burkhauser. 2004. "Did the Employment of People with Disabilities Decline in the 1990s, and Was the ADA Responsible? A Replication and Robustness Check of Acemoglu and Angrist (2001)," *available at* <http://digitalcommons.ilr.cornell.edu/edicollect/91>.
- Jolls, Christine. 2000. "Accommodation Mandates," 53 *Stanford Law Review* 223-306.
- Jolls, Christine and J. J. Prescott. 2005. "Disaggregating Employment Protection: The Case of Disability Discrimination," *available at*

<http://lsr.nellco.org/harvard/olin/papers/496>.

Kaye, H. Stephen. 2003. "Improved Employment Opportunities for People with Disabilities," *available at* http://dsc.ucsf.edu/view_pdf.php?pdf_id=27.

Kirchner, Corrine. 1996. "Looking Under the Street Lamp: Inappropriate Uses of Measures Just Because They Are There," 7 *Journal of Disability Policy Studies* 77-90.

Kruse, Douglas and Lisa Schur. 2003. "Employment of People with Disabilities Following the ADA," 42 *Industrial Relations* 11-30.

McMenamin, Terence M., Thomas W. Hale, Haejin Kim, and Douglas Kruse. 2005. "Designing Questions to Identify People with Disabilities in Labor Force Surveys: The Effort to Measure the Employment Level of Adults with Disabilities in the CPS," *available at* <http://www.bls.gov/osmr/pdf/st050300.pdf>.

McNeil, John M. 2000. "Employment, Earnings, and Disability," *available at* <http://www.census.gov/hhes/www/disability/emperndis.pdf>.

National Council on Disability. 2006. "National Disability Policy: A Progress Report," *available at* http://www.ncd.gov/newsroom/publications/2006/progress_report.htm.

Oyer, Paul and Scott Schaefer. 2002. "Sorting, Quotas, and the Civil Rights Act of 1991: Who Hires When It's Hard to Fire?," 45 *Journal of Law & Economics* 41-68.

Rosenberg, Gerald N. 1991. *The Hollow Hope: Can Courts Bring About Social Change?*

Chicago: University of Chicago Press.

Schwochau, Susan and Peter David Blanck. 2000. "The Economics of the Americans with Disabilities Act, Part III: Does the ADA Disable the Disabled?," 21 *Berkeley Journal of Employment and Labor Law* 271-313.

Stapleton, David C. and Richard V. Burkhauser, eds. 2003. *The Decline in Employment of People with Disabilities: A Policy Puzzle*. Kalamazoo, MI: W. E. Upjohn Institute for Employment Research.

Stein, Michael Ashley. 2000. "Empirical Implications of Title I," 85 *Iowa Law Review* 1671-1690. ‘

Stein, Michael Ashley. 2003. "The Law and Economics of Disability Accommodations," 53 *Duke Law Journal* 79-192.

Stein, Michael Ashley. 2004. "Same Struggle, Different Difference: ADA Accommodations as Antidiscrimination," 153 *University of Pennsylvania Law Review* 579-674.

Stein, Michael Ashley. 2004. "Under the Empirical Radar: An Initial Expressive Law Analysis of the ADA," 90 *Virginia Law Review* 1151-1192.

Stein, Michael Ashley. 2007. "Disability Human Rights," 95 *California Law Review* 75-122.

Stein, Michael Ashley and Janet E. Lord. 2008. "Future Prospects for the United Nations

Convention on the Rights of Persons with Disabilities,” in Gerard Quinn and Oddný Mjöll Arnardóttir, eds., *The UN Convention on the Rights of Persons with Disabilities: European and Scandinavian Perspectives*. Leiden: Martinus Nijhoff Publishers.

Stein, Michael Ashley and Penelope J. S. Stein. 2007. “Beyond Disability Civil Rights,” *58 Hastings Law Journal* 1203-1240.

Tolin, Tom and Martin Patwell. 2003. “A Critique of Economic Analysis of the ADA,” *23 Disability Studies Quarterly* 130-142.

Wittenberg, David and Elaine Maag. 2003. “Real Trends or Measurement Problems: Disability and Employment Trends from the Survey of Income and Program Participation,” *available at*
http://www.urban.org/UploadedPDF/410635_Real_or_Measurement.pdf.

¹ In this paper, efficacy is calibrated using econometrically measurable outcomes. An interesting issue is whether a narrow focus on the measured wages and employment data fully captures important but harder to quantify effects relating to the overarching purpose of civil rights laws in transforming social attitudes. For a discussion of this broader issue within the context of disability law, see Stein (2004).

² To be considered qualified, individuals must be capable of performing the essential job functions of the positions they seek, either with or without the provision of reasonable accommodations. For an overview of this fundamental mandate, see Stein (2003).

³ This disregard led Richard Burkhauser to criticize the ADA's lack of conjoined work initiatives by contrasting various European policies directed toward "transferring" people with disabilities from social welfare networks into the workforce (Burkhauser, 1997; Burkhauser and Hirvonen, 1989).

⁴ The Ticket to Work and Work Incentives Improvement Act of 1999 extended the length of time that people with disabilities receiving public assistance could continue to receive health care coverage after obtaining gainful employment. On July 26, 2000 (the ADA's tenth anniversary) the Clinton Administration announced a series of policy initiatives to allow disabled people receiving Social Security disability-related benefits to earn additional income without losing cash benefits.

⁵ For a description by one of its failed supporters, see Dole (1994).

⁶ A December 2002 study by the General Accounting Office, for instance, found that only a "very small proportion" of businesses utilized either of two available federal tax credits for hiring disabled workers. Combined, these incentives provide for annual credits of \$7,400 per eligible disabled worker (GAO, 2002).

⁷ For an overview of the treaty and its implications, see Stein (2007).

⁸ The PSID maintains annual family-level data files from the program's inception in 1968 through 1997 after which they appear biennially through 2003.

⁹ The PSID does add new individuals to the overall "roster," for example the children of original 1968 interviewed household heads. Our unrestricted dataset does generate some compositional change as older workers drop out of our sample (but to be included in any given year's coefficient estimate, one must have

a 1981 observation for comparison, so we are not adding new workers at the time of ADA adoption). Our restricted dataset, however, focuses on an unchanging group of individuals, i.e. those who appear each year from 1981 through 1996.

¹⁰ Several studies, which are referenced individually, are collected in Stapleton and Burkhauser (2003). Two important papers predating the post-ADA effect studies are Collignon (1997) and Kirchner (1996). Electronic versions of some of the above studies, as well as continuing research in this field, are posted on the Cornell University ILR School Employment and Disability Institute homepage, <http://www.ilr.cornell.edu/edi/m-pubs.cfm>.

¹¹ Two other studies that bear noting are Epstein (1992), which foretold a detrimental affect in advance of the ADA becoming operational, and Jolls (2000), which placed the Epstein assertions within a supply-demand model.

¹² Accordingly, DeLeire adjusts his standard errors to correct for correlation across repeated observations.

¹³ For a prominent example, see *Sutton v. United Airlines*, 527 U.S. 471 (1999).

¹⁴ See *Cleveland v. Policy Mgmt. Sys. Corp.*, 526 U.S. 795 (1999), in which the Supreme Court acknowledged the potential overlap between individuals deemed disabled under the ADA and those construed as such under the Social Security system.

¹⁵ The question is: Do you/Does anyone in this household) have a health problem or disability which prevents (you/them) from working or which limits the kind or amount of work (you/they) can do?, and the possible answers are “yes” or “no.”

¹⁶ This statistic was gleaned from personal correspondence with Terence McMenamin of the Division of Labor Force Statistics in March, 2006.

¹⁷ *Id.*

¹⁸ In the words of Hale, “the burden of proof is on those who use the data to infer the labor force status of people with disabilities. To proceed as though the data are valid measures of disabilities turns a data issue into a policy issue” (Hale, 2001).

¹⁹ When a string of household interview numbers in the individual-level set is arranged in chronological order and merged to the year-by-year family file, we are able to track household head responses consistently across time.

²⁰ Responses to the race survey question often generate inconsistency over time; we therefore apply the response that appears in more than half of the individual's total observations.

²¹ Male household heads comprise about 75% of all household heads. We note that Richard Burkhauser and his colleagues argue that PSID analysis should examine effects on men, "since their employment decline is much more pronounced than that of women" (Burkhauser and Schroeder, 2004). Also, "the PSID and the CPS, capture the same employment trends for men with disabilities over the 1980s and 1990s" (Burkhauser and Schroeder, 2004).

²² In this section, we refer to summary statistics from the unrestricted data in Table 1 unless otherwise noted.

²³ In another section of the PSID survey, however, the question posed is "[A]re you . . . working now, looking for work, retired, a student, (a housewife), or what?" The responses "temporarily disabled" or "permanently disabled" often are coded, but were not suggested to interviewees as possible answers. The fact that interview participants are not prompted to consider the answer "permanently disabled" implies a likely downward bias in the frequency of affirmative disability reports. As a result, we ignore the employment status question as a measure of disability.

²⁴ Requiring interviewees to recall information on employment status and earnings from the previous year undoubtedly introduces measurement error (due, for example, to reporting biases or imperfect recall). However, since these indicators are used as dependent variables in the analysis, such measurement error will not bias our estimates.

²⁵ We have attempted to reconcile this aberration with data analysts at the PSID. According to one helpful representative "[t]he only difference in 1986 compared to other waves is that [the work limitation questions] were asked after a series of questions on Activities of Daily Living ("ADLs"). This could be some kind of measurement issue -- asking a global questions about whether [one has] a physical or nervous

condition that limits the amount and type of work you do after...questions about whether you have ADLs somehow might reduce the likelihood of [one] reporting affirmatively to the first question.” Indeed, this is the only difference in the way the questions were asked from 1985 to 1987, and when the question regarding ADLs were removed in 1987, the percentage disabled returned to levels consistent with the year-to-year changes from 1981 to 1985.

²⁶ Figures 3 and 5 only include observations through 1995, because the necessary data are not available in 1996.

²⁷ Exactly 4581 persons drop out of our sample when fixed effects are included because these individuals experienced no variation in employment status across their observation span and thus do not contribute to the regression estimates.

²⁸ According to the PSID, “[t]he interview period (field season) is roughly between March and November, with 1993 and 1994 being exceptions and going into December.”