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

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

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[*] C. Wendell and Edith M. Carlsmith Professor of Law, Stanford Law School. The authors wish to thank Christine Jolls, participants at the 2009 American Law and Economics Association Annual Meeting, and two anonymous referees for extremely helpful feedback.
[**] Executive Director, Harvard Law School Project on Disability; and Cabell Research Professor, William & Mary School of Law.
[***] Visiting Assistant Professor, Duke Law School, and corresponding author. Please send all communication to: Christopher L. Griffin, Jr. / Duke Law School / 210 Science Drive / Box 90360 / Durham, NC 27708; Phone: (919) 613-7213; Email:cgriffin@law.duke.edu.
[****] UC Berkeley School of Law, Ph.D. candidate in Jurisprudence and Social Policy.

ABSTRACT

This article explores the relationship between the Americans with Disabilities Act ("ADA") and the relative labor market outcomes for people with disabilities. 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 1437 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.

I. 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 (1991), for example, has argued that judicial decisions alone were incapable of generating the major economic improvements for African-Americans that emerged around 1965. These economic gains were not simply the by-product of judicial decisions following the passage of the Civil Rights Act of 1964 but were instead 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 increasing 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, 2004a) and a lightning rod for its critics (Epstein, 1992).

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. But both the congressional and judicial support for the effort to incorporate the disabled more fully into the workforce has not been consistent and

comprehensive throughout the existence of the ADA (Stein and Stein, 2007).[3] Indeed, for the first ten years after the ADA's passage, the effectiveness of the statue was undercut by federal benefits rules that penalized disabled workers who secured employment by depriving them of their federal health care benefits or monetary supplements.[4] Moreover, Congress has yet to enact a comprehensive jobs program for workers with disabilities similar to those passed for non-disabled benefit recipients during the mid-1990s welfare reform.[5] Finally, while Congress did enact federal tax benefits designed to increase the employment of persons with disabilities, these tax credits have been profoundly underutilized (GAO, 2002).

A growing debate has developed regarding the impact of the ADA on the employment of individuals with disabilities. Initial empirical studies comparing pre- and post-ADA employment data have painted a fairly dismal picture of disabled employees' labor market participation, 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 improved significantly.

Achieving a greater understanding of post-ADA employment effects is of considerable importance both within and outside the United States. Notably, the ADA's reasonable accommodation mandate has been adopted by the United Nations Convention on the Rights of Persons with Disabilities,[6] as well as the European Union's Employment Framework Directive (an umbrella antidiscrimination policy) (European Union Council, 2000). These enactments have considerably broadened interest in disability law and

policy, although it should still be noted that three-quarters of the world's nations lack any relevant domestic statute or legal provision protecting the disabled from employment discrimination (Stein and Lord, 2008).

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 long-term trends in the employment status of the disabled. Second, unlike many of the earlier studies, we employ individual-level panel data to observe the same individuals before and after the adoption of the ADA. Expanding the timeframe and utilizing the longitudinal structure of our data 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 to the same 1437 male, household-head workers over a sixteen-year data period, we sacrifice some statistical power to reduce compositional bias.

To frame our discussion, Section II discusses the results of major studies on post-ADA employment effects as well as data-related pitfalls left unaddressed in the literature. Section III sets forth our initial analysis from the PSID, a longitudinal data set that, to our knowledge, has not been previously used to explore 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 2007.[7] Unlike the more frequently used CPS, PSID data allow us to chart the measured employment and earnings of the same disabled workers over a

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broader span of time. Section IV 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 V adds the condition that individuals represented in the sample remain the same over time so that longitudinal estimates are perfectly comparable. Using this "restricted" dataset, we observe that the initial estimates are 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 compositional change, i.e., who self-identifies as "disabled" after disability laws change. In this regard, our approach can provide 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 entering the market at about the time of the adoption of the ADA, as well as from our relatively small sample size.[8] Finally, this article underscores the challenge confronting policymakers in crafting a disability policy that addresses the

difficult conditions confronting disabled male household heads in the modern American economy. Section VI concludes.

II. Previous Studies

A. Prior Literature

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 outcome. 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., 2001; McNeil, 2000; Schwochau and Blanck, 2000).[9]

The ADA includes two key employment-related provisions: a prohibition on 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 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.[10] 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"). Analyzing pooled panels of men aged eighteen to sixty-four, 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.

Acemoglu and Angrist (2001) also investigated whether the intended employment protection of the ADA improved economic conditions for the disabled. 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. For 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, these effects for older men were attributed to

changes in 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 articles by DeLeire and Acemoglu and Angrist are serious and careful studies by highly respected researchers, the authors faced the difficult challenge of identifying 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. Although they found a steady decline in employment levels among disabled workers during the 1990s relative to the period before the ADA's passage, Jolls and Prescott's work revealed no connection between declines from 1993-1994 and onward and whether the federal law was an innovation over preexisting state law protections. Any adverse outcomes for the disabled population were confined to the immediate post-passage period, before the ADA's effective date, before employers' attitudes about workers with disabilities could improve, and before the courts had defined more precisely the ADA's requirements. Hotchkiss (2004) also exploited state-level variation in disability protection law to conclude that the ADA was not responsible for declining labor force participation rates among the disabled.

These leading studies assessing relative post-ADA employment rates using SIPP and CPS all reached the same conclusion that the statute has precipitated a decline in employment levels, although Jolls and Prescott found this effect to be limited to the short term. Moreover, all three studies (again, with Jolls and Prescott's caveat regarding short- and long-term effects) concluded that the ADA's reasonable accommodation mandate is the most likely cause of the relative decline in employment.

More recent work, such as Burkhauser, Houtenville, and Rovba (2007), has raised questions about whether the observed decline in the employment of the disabled can be attributed to the ADA. Burkhauser and his coauthors extend and refine the analysis of Acemoglu and Angrist in several important ways. After expanding the observation period from 1988-1997 to 1982-2004, Burkhauser, Houtenville, and Rovba 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."

B. Identifying Disabled Individuals

When assessing the ADA's impact, it is crucial to have a reliable measure of disability. Yet, since the enactment of the ADA, federal courts on numerous occasions have redefined who is covered by the statute.[11] Accordingly, it is unsurprising that no clear and consistent measure exists of the disabled individuals protected by the ADA.[12] The CPS studies discussed above all use the survey's responses to the work-disability question as a proxy for disability.[13] This fact, in turn, raises several concerns, since the problems surrounding the work-disability question and similar survey questions have long been recognized (Hale, 2001).

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To begin with, the CPS work-disability question was not intended to correspond to the definition of disability under the ADA (McNeil, 2000). In addition, it is at least possible that the composition of respondents who answer affirmatively to the work-disability question changes over time in ways that would undermine the reliability of estimates of the effect of the law (Kirchner, 1996; Kruse and Schur, 2003).

One can imagine two ways in which the presence of the law could influence self reports of disability. On the one hand, some non-working individuals who did not previously identify themselves as disabled might now choose to do so in the wake of the advent of the law. This could result from either greater awareness of the concept of disability in the workplace, a renewed sense that the disabled were discriminated against, or even hopes that re-characterizing oneself as disabled might lead to improved employment prospects once the law takes effect. In this event, the "disabled" population would grow in a way that might be disproportionately augmented by those with poor prospects in the labor market. The effect of this changing composition of the disabled would likely make the law look worse than it actually was.

On the other hand, the passage of the ADA could also lead to a reduction in the number of those who self-reported as disabled. This effect would operate if the accommodation provision of the ADA led 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 accommodations under the ADA drop out of the population that answers the work-disability question affirmatively, 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 also bias the analysis towards findings of declining employment of the disabled population, even if there were a positive effect on employment from the ADA accommodation provision (Kruse and Schur, 2003).

C. The Possible Sources of Data -- CPS, SIPP, and PSID

The studies discussed above have relied on either the CPS or the Survey of Income and Program Participation (SIPP). Inevitably, different data sources have advantages and disadvantages. In order to resolve deficiencies in the CPS, the U.S. Census Bureau originally established the SIPP to capture economic conditions at multiple times within a two and one-half year window. This aspect of the data instrument also relieved survey respondents from the task, required in the CPS, of remembering economic values or participation levels from up to twelve months prior to the survey. But these advantages of the SIPP are potentially offset in a study of disability in that SIPP interviewers only ask questions from the disability module 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.[14]

The SIPP only modestly improves on the pure cross-sectional structure of the CPS in terms of allowing the researcher to follow the same individuals across time. The PSID, which provides longitudinal data over a significant time span, represents a clear improvement in this dimension over the CPS and SIPP. Thus, it appears that clear tradeoffs exist when choosing among the three primary datasets capturing individual disability and employment status. The level of detail in the SIPP makes it more attractive

than the vague questions in the CPS and PSID. 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 compromises reporting accuracy. Only the PSID data allow for reliable monitoring of individuals over time and thus for observing changes in disability status that may be attributed to passage of the ADA (albeit with a vague question about disability). With these advantages and disadvantages in mind, we turn to our study, which is the first evaluation of the impact of the ADA to use the PSID.

III. Our Analysis Using PSID Data

The PSID 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).[15]

Our initial matching algorithm yields longitudinal data for 18,342 individuals comprising 107,844 person-year observations. We then restrict this sample to male household heads between the ages 21 and 65 to explore the labor market behavior of those most persistently attached to the labor force.[16] After imposing these restrictions, our operational dataset consists of 7120 individuals observed in at least one year during the period 1981 to 1996 (a total of 64,607 person-year observations). Our analysis will compare white versus non-white individuals.[17]

Tables 1 and 2 report summary statistics for two versions of the operational dataset: one for the overall "unrestricted" sample of 7120 individuals and another describing the "restricted" sample, the 1437 individuals appearing each year from 1981 to 1996. 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?"[18] A χ^2 test for the equality of means in each sample indicates that the two groups are statistically indistinguishable with the sole exception of state unemployment rates, the difference between which is marginally different from zero at the 5% level.

On average, the male household heads in our sample report being disabled 13% and 12% of the time in the unrestricted and restricted datasets, respectively.[19] Note that this is different from the claim that 12-13% of the household heads in our data are disabled, since, as we show, disability reports can vary over time for the same individual. In the unrestricted (restricted) data about 4889 (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 restricted dataset.

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 a result, 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.[20] 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 70% of the men in our dataset were white and that average age over the period 1981-1996 was 39 years. Thirty-seven percent of our sample had obtained only a high school degree, while another 21% had some college, and another 14% 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%.

Figure 1A provides information on the percentage of individuals reporting a disability over the duration of our observation period. Since natural aging and continuing exposure to potentially disabling disease or injury will tend to drive this proportion upward, we separate the data into three cohorts defined by age reported in 1981. As expected, the disabled share in each cohort increases from 1981 to 1996, and the older initial cohorts have higher rates of disability. Note that the share of disabled persons in the cohort aged 40 to 65 rises from roughly 20% to 30% by 1996. The cohorts aged 21 to 39, on the other hand, begin with a much lower rate of disability closer to 5% in 1981, rising to 16% in the final year of observation.[21]

Table 1: Summary Statistics (Unrestricted Dataset)

Variable	Number of Observations	Mean	Standard Deviation
Currently disabled	64,525	0.13	0.34
Currently employed	64,601	0.85	0.36
Annual weeks worked	56,465	40.79	16.94
Real annual wages (2000)	63,629	31,055	36,151
Real hourly wages (2000)	48,832	17.29	33.30
Age	64,607	39.16	11.20
White	64,607	0.70	0.46
Less than high school education	64,607	0.20	0.40
High school graduate	64,607	0.37	0.48
Some college education	64,607	0.21	0.40
College graduate	64,607	0.14	0.35
Some postgraduate study	64,607	0.08	0.27
State unemployment rate	63,190	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	22,974	0.12	0.33
Currently employed	22,992	0.90	0.30
Annual weeks worked	20,521	43.34	14.03
Real annual wages (2000)	22,790	37,508	41,593
Real hourly wages (2000)	18,759	19.23	22.18
Age	22,992	40.77	9.04
White	22,992	0.78	0.41
Less than high school education	22,992	0.14	0.35
High school graduate	22,992	0.35	0.48
Some college education	22,992	0.22	0.41
College graduate	22,992	0.17	0.38
Some postgraduate study	22,992	0.12	0.32
State unemployment rate	22,869	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: 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 at five-year intervals. 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 comparing 1981 and 1996, albeit with some intervening swings over time within each cohort. The biggest shifts across the endpoints of the entire 1981 through 1996 period were the *increase* in disability experienced by the 21-25 and 51-55 age cohorts, and the *decrease* in disability for the 41-45 age cohort.

Figure 1B: Percentage disabled by age cohort in five-year intervals (1981 - 1996)
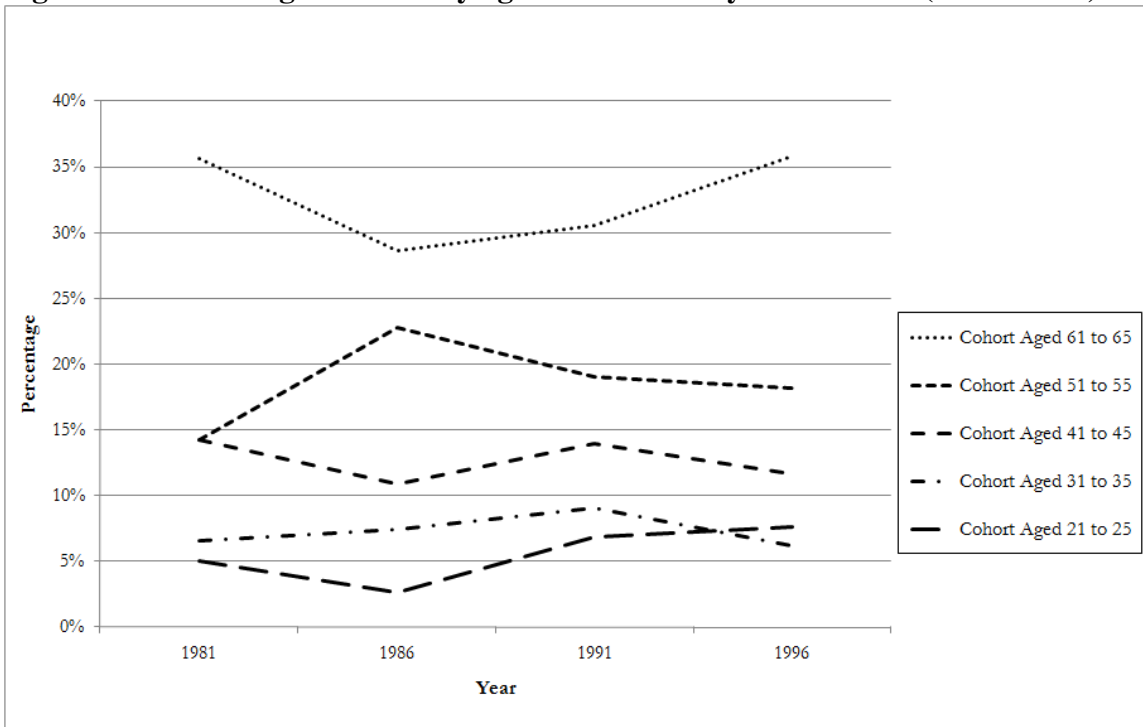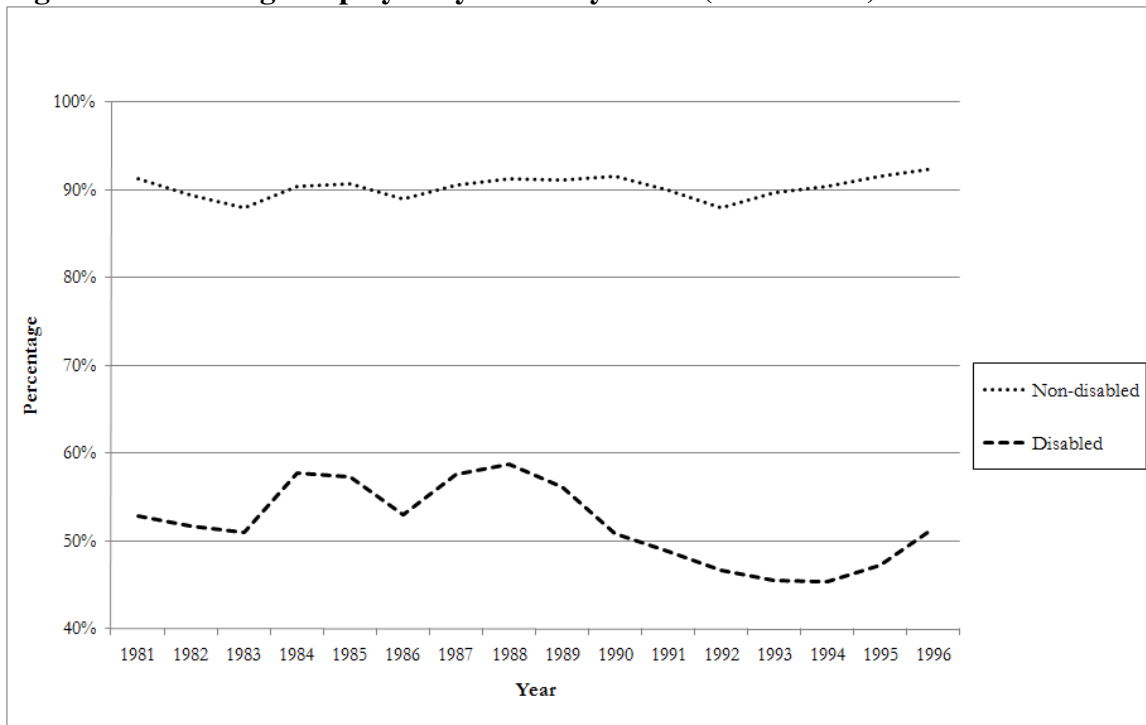

Source: Panel Study of Income Dynamics

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 that between 1981 and 1996 the percentage of non-disabled individuals with jobs remained high and relatively constant at about 90%, while employment in the disabled community fell during the 1990s from a peak of nearly 60% in 1988 to about 52% by 1996. Given that the ADA was signed into law on July 26, 1990, this

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deterioration in the employment levels of the disabled appears to begin just prior to enactment and continues through the early years of ADA enforcement before showing signs of reversal after 1994. At first glance, then, this simple graphical depiction intimates that disabled individuals may have faced increasing employment obstacles 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 provides a somewhat finer measure of participation by showing the average number of annual weeks worked over time for disabled and non-disabled individuals.[22] Average weeks worked for the non-disabled remain fairly stable at about 43 weeks per year. Disabled individuals also experienced labor supply stability through the 1980s with an average of 25 weeks worked and a peak of about 27 weeks in 1988. 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.

Figure 3: Average annual weeks worked by disability status (1981 - 1995)
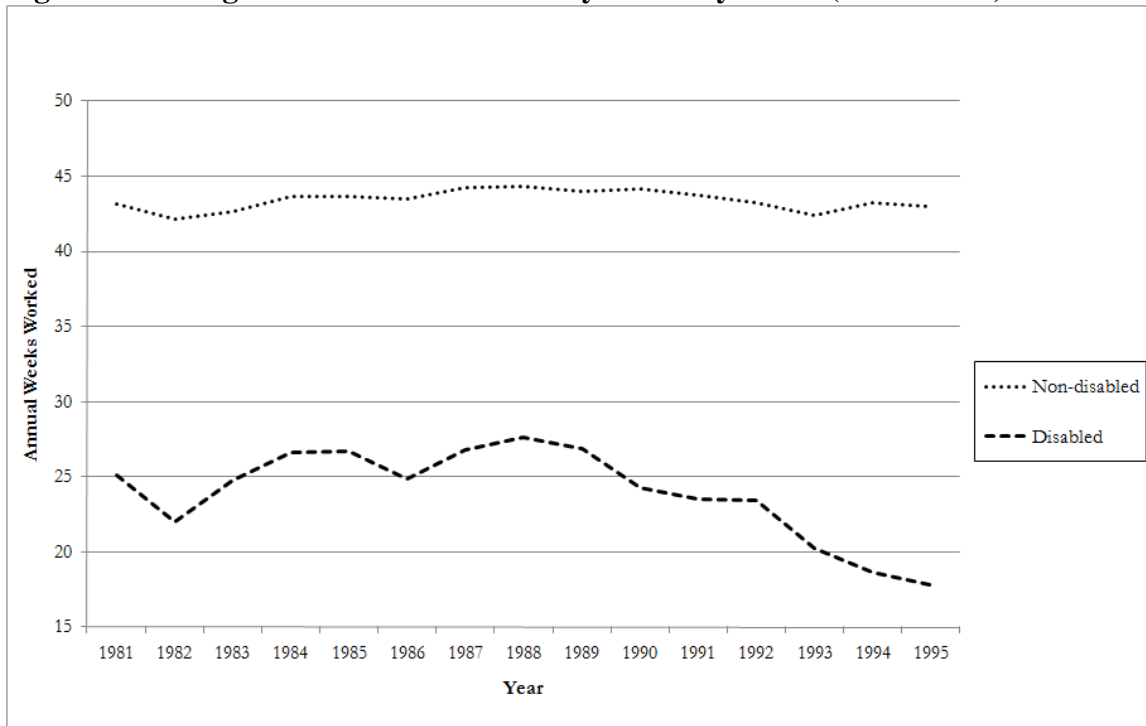
Source: Panel Study of Income Dynamics

Figures 4 and 5 illustrate the time series for real wages and salaries (measured in constant 2000 dollars), showing annual earnings and earnings per annual hours worked. Figure 4 depicts real income increases for both the disabled and non-disabled populations, which follow the same general pattern, albeit with an unusually sharp increase for the earnings of the disabled in 1996. Unless the 1996 jump represents some delayed ADA-induced improvement, Figure 4 provides little 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

Figure 5, which plots the average hourly real wage for both groups, reveals that the non-disabled earned about $2.30 more during the 1980s. Unlike the time path for annual earnings, hourly wages converge substantially by 1993 but *diverge* in 1995.

Figure 5: Conditional average real hourly wages by disability status (1981 - 1995)

Source: Panel Study of Income Dynamics

Figures 2 through 5 offer an overall sense of the relative labor market performance of the disabled over time, but to assess better the causal impact of the ADA, one must control for individual characteristics that affect employment and wages and that are correlated with disability (or at least the propensity to report a disability) as well as for other factors that may have differential impact on the disabled over time. The following section will use our individual-level panel data to illuminate how the ADA influenced the employment and earnings of the disabled.

IV. Empirical Results

A. *Employment Status*

We begin our regression analysis 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:

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

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_{it} 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 ("DD") estimator for a given year relative to 1981. Although Equation (1) generates raw DDs, we can improve on this stark model by in two ways. First, we add a vector of covariates that influence employment outcomes. Second, in our fully specified model, we add controls for individual fixed effects.[23] This procedure 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 this section 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 18,342 individuals,

while the inclusion of fixed effects (conditional logit) reduces the sample size by about 25% to 13,760 persons.[24]

Note that the PSID employment status question indentifies this status only at the time of the PSID interview, which can lead to imprecision in our estimates.[25] To understand this point, 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 the 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 file, 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.[26] 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 casual

observation from Figure 3 that employment levels among the disabled declined following enactment of the ADA.

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.) For most empirical analyses in which the values of the dependent variable cluster 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.

Table 3: Annual Weeks Worked, Unrestricted Dataset (1981 – 1995)

	(1)	(2)	(3)
Disabled * 1982	-2.10* (1.28)	-1.78 (1.18)	-0.26 (1.02)
Disabled * 1983	0.18 (1.32)	1.12 (1.24)	2.48** (1.16)
Disabled * 1984	0.98 (1.38)	0.15 (1.27)	0.99 (1.17)
Disabled * 1985	1.07 (1.47)	0.86 (1.37)	0.86 (1.2)
Disabled * 1986	-0.58 (1.57)	-1.41 (1.48)	-0.11 (1.35)
Disabled * 1987	0.58 (1.52)	-0.43 (1.39)	0.67 (1.32)
Disabled * 1988	1.36 (1.55)	0.74 (1.43)	0.55 (1.30)
Disabled * 1989	0.85 (1.55)	-0.53 (1.41)	-0.50 (1.32)
Disabled * 1990	-1.89 (1.51)	-2.94** (1.39)	-0.81 (1.29)
Disabled * 1991	-2.18 (1.53)	-3.34** (1.41)	-1.01 (1.30)
Disabled * 1992	-1.73 (1.49)	-3.11** (1.37)	-0.51 (1.28)
Disabled * 1993	-4.18*** (1.54)	-4.98*** (1.44)	-0.93 (1.36)
Disabled * 1994	-6.56*** (1.55)	-6.75*** (1.48)	-3.35** (1.41)
Disabled * 1995	-7.21*** (1.63)	-7.51*** (1.52)	-3.64** (1.46)
Covariates included?	No	Yes	Yes
Fixed effects?	No	No	Yes
N	56,400	55,341	55,341

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$_{it}$* 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, and all standard errors are clustered at the individual level.

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 strategy of Table 4 requires exclusion of zero-wage observations.

In the next two sections, we estimate the model:

$$Earnings_{it} = \beta_0 + \delta * dis_{it} + \sum\nolimits_{j=82}^{J} \beta_{j-81} I_j * dis_{it} + \sum\nolimits_{j=81}^{J} \lambda_{j-80} I_j + \varepsilon_{it} \tag{2}$$

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)	-250.37 (1526.88)	-2154.84 (1514.88)
Disabled * 1983	-1439.19 (1559.51)	-1515.24 (1524.29)	-1345.93 (1404.43)
Disabled * 1984	-677.34 (1593.68)	-2422.66 (1546.76)	-2767.64* (1424.52)
Disabled * 1985	-1327.12 (2081.52)	-3586.90* (1931.97)	-1477.31 (2170.44)
Disabled * 1986	-1466.81 (2165.08)	-3489.78* (2006.62)	-3396.85 (2280.00)
Disabled * 1987	-2292.60 (1999.33)	-5075.32*** (1846.41)	-2669.00 (2258.61)
Disabled * 1988	1277.12 (2969.06)	-868.92 (2754.10)	-2713.09 (2353.06)
Disabled * 1989	-472.67 (1851.57)	-3015.72* (1747.91)	-3031.66 (2016.83)
Disabled * 1990	-1230.19 (1638.32)	-2293.60 (1609.82)	-1958.07 (2459.95)
Disabled * 1991	-1305.43 (1934.78)	-2493.43 (1767.57)	-2453.83 (2314.18)
Disabled * 1992	2092.27 (2052.83)	-547.28 (1898.40)	-261.97 (2440.80)
Disabled * 1993	-3601.38* (2049.20)	-6316.76*** (1875.92)	-4472.93* (2388.32)
Disabled * 1994	-1341.10 (2094.54)	-3886.13* (2065.29)	-2716.29 (2395.19)
Disabled * 1995	-4185.18* (2164.31)	-4293.32** (1930.05)	-3075.65 (2318.36)
Disabled * 1996	2903.04 (5285.71)	1553.52 (5297.91)	478.99 (5242.35)
Covariates included?	No	Yes	Yes
Fixed effects?	No	No	Yes
N	54,191	53,049	53,049

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$_{it}$* 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, and all standard errors are clustered at the individual level.

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 mild statistical significance over time contrasts sharply with the results in Table 3. Based on these estimates, the unrestricted data do not support the proposition that the ADA caused relative wages for disabled workers to deteriorate, although 1993 appears again 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 started closing 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 disabled persons 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 is 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 nondisabled). 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, the 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 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 remain robust when we eliminate compositional changes in the 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 (1.10)
Disabled * 1983	-0.76 (0.84)	-1.02 (0.83)	-1.45* (0.78)
Disabled * 1984	0.37 (1.02)	-0.40 (1.03)	-0.74 (0.97)
Disabled * 1985	-0.06 (1.05)	-1.31 (1.00)	-0.36 (1.32)
Disabled * 1986	-0.14 (1.14)	-1.02 (1.06)	-1.08 (1.29)
Disabled * 1987	-1.27 (1.07)	-1.98** (1.01)	-1.35 (1.40)
Disabled * 1988	1.55 (1.72)	0.75 (1.62)	-0.11 (1.44)
Disabled * 1989	-0.11 (1.13)	-0.85 (1.09)	-1.30 (1.29)
Disabled * 1990	-0.62 (0.82)	-0.76 (0.81)	-0.94 (1.29)
Disabled * 1991	-0.93 (0.99)	-1.27 (0.95)	-1.69 (1.41)
Disabled * 1992	-0.18 (1.94)	-1.07 (1.93)	-0.03 (1.67)
Disabled * 1993	2.34 (3.09)	1.64 (3.08)	1.44 (3.84)
Disabled * 1994	1.52 (1.45)	1.40 (1.50)	0.19 (1.83)
Disabled * 1995	-1.73 (2.10)	-1.64 (2.06)	-1.19 (2.33)
Covariates included?	No	Yes	Yes
Fixed effects?	No	No	Yes
N	45,259	44,469	44,469

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$_{it}$* 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, and all standard errors are clustered at the individual level.

V. Analyzing the Restricted Sample

In this section, we restrict the data to include only those individuals who appear each year for the entire observation period. In Section IV, 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. Consequently, 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*, initially appeared continuously from 1981, but *B* contributed to estimates for three more years than *A* because *B* was interviewed three more times 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 III, 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 persuasive 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 result suggests that, at least relative to 1981, male disabled household heads were not suffering employment losses (even if there may be some indication from Table 6 that any 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 (2.59)
Disabled * 1983	-0.40 (3.06)	-0.10 (3.02)	2.17 (2.77)
Disabled * 1984	1.31 (2.93)	1.87 (2.93)	2.71 (2.49)
Disabled * 1985	-0.76 (3.33)	0.18 (3.47)	0.78 (3.11)
Disabled * 1986	-3.82 (3.64)	-2.53 (3.81)	-0.78 (3.34)
Disabled * 1987	-0.01 (3.20)	1.55 (3.34)	2.86 (3.06)
Disabled * 1988	-0.57 (3.10)	1.99 (3.32)	3.07 (2.74)
Disabled * 1989	0.08 (2.93)	3.24 (3.27)	3.70 (2.76)
Disabled * 1990	-5.12 (3.27)	-1.26 (3.62)	0.21 (3.00)
Disabled * 1991	-6.32* (3.36)	-2.06 (3.80)	-1.51 (3.10)
Disabled * 1992	-1.51 (3.21)	2.85 (3.67)	2.48 (2.95)
Disabled * 1993	-5.66* (3.30)	0.36 (3.95)	1.54 (3.28)
Disabled * 1994	-8.35** (3.52)	-2.19 (4.28)	-1.11 (3.52)
Disabled * 1995	-8.62** (3.62)	-0.16 (4.47)	1.36 (3.70)
Covariates included?	No	Yes	Yes
Fixed effects?	No	No	Yes
N	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$_{it}$* 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, and all standard errors are clustered at the individual level.

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, the conclusions one can draw from this Table 7 are uncertain. On the one hand, the poor outcomes in 1993 suggest a story in which the ADA damaged the earnings profile for the disabled by a substantial amount. A year later, however, circumstances 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 that this initial effect was quickly overturned? On the other hand, the return to substantial (though insignificant) disparities in 1995 and 1996 may suggest that 1994 was simply an 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 primarily drove the apparent deterioration in the earnings of the disabled.

Table 7: Conditional Annual Wages Earned, Restricted Dataset (1981 – 1996)

	(1)	(2)	(3)
Disabled * 1982	39.15 (2961.84)	-1052.64 (2772.77)	-2782.06 (2332.88)
Disabled * 1983	-4719.64* (2758.93)	-4221.60 (2739.45)	-1714.85 (2068.69)
Disabled * 1984	-2745.25 (2583.90)	-2610.32 (2715.76)	-1746.00 (2073.51)
Disabled * 1985	-7072.15** (3431.43)	-5604.07* (3038.50)	-1348.42 (4222.21)
Disabled * 1986	-5275.17 (3801.62)	-2668.13 (3500.84)	-1348.96 (4160.80)
Disabled * 1987	-8634.13** (3750.00)	-6209.00* (3318.33)	-3076.81 (4332.98)
Disabled * 1988	-7146.01* (4111.81)	-5002.83 (3691.88)	-2901.21 (4345.29)
Disabled * 1989	-4471.59 (3375.95)	-2798.21 (3315.48)	-4129.82 (3694.59)
Disabled * 1990	-5970.43* (3230.28)	-2613.86 (3351.33)	-2326.68 (4784.707)
Disabled * 1991	-8632.74** (3717.52)	-5330.25 (3413.56)	-3546.62 (4074.81)
Disabled * 1992	-5749.60 (4376.94)	-5105.32 (4270.72)	-3621.80 (4546.63)
Disabled * 1993	-10,357.58** (4126.61)	-7234.62* (3823.49)	-5256.63 (4295.34)
Disabled * 1994	-4135.76 (4660.38)	-911.29 (4592.39)	-1517.86 (4131.87)
Disabled * 1995	-9655.74** (4548.16)	-5885.46 (4355.23)	-4675.97 (3755.64)
Disabled * 1996	-5379.76 (4367.12)	-3067.75 (4586.23)	-5261.67 (4574.79)
Covariates included?	No	Yes	Yes
Fixed effects?	No	No	Yes
N	19,762	19,648	19,648

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*$_{it}$ 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, and all standard errors are clustered at the individual level.

Figure 6: Number disabled in the restricted sample by year (1981 - 1996)
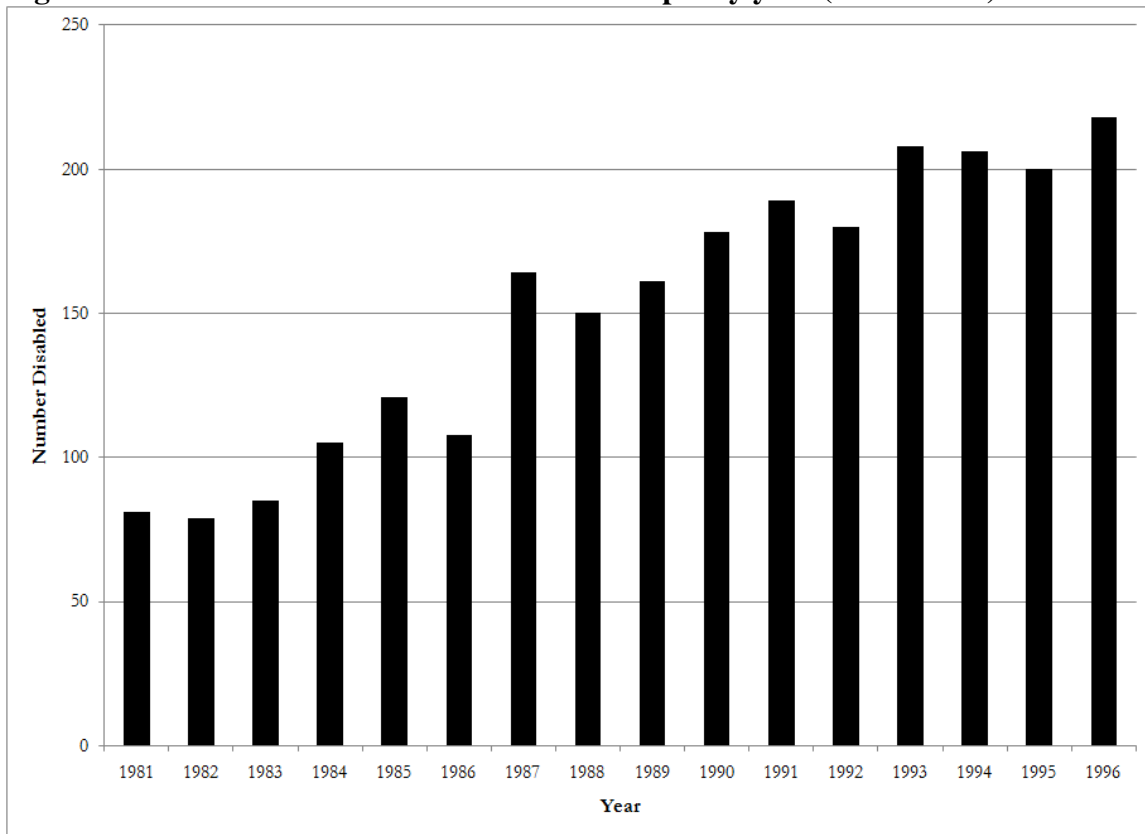
Source: Panel Study of Income Dynamics.

One explanation for why the Column 1 estimates in Table 7 display 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 naturally ages over time, the overall trend in the number reporting a disability increases as one might expect. However, with the exception of the 1986-1987 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 would benefit people who claim a disability then. 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 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 that begins in 1986, well before the ADA became law. While relative real hourly wages of the disabled fell by about $6 beginning in 1992, such disparity 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, though one might conclude that the early days of the ADA did not boost the hourly wages of the disabled, the estimates for 1995 suggest some positive news regarding earnings. Might this suggest that a decade of earnings erosion for the disabled, initially caused by forces in the American labor market

other than the ADA, was beginning to reverse 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 (2.33)
Disabled * 1983	-2.30 (1.71)	-2.59 (1.64)	-3.37 (2.16)
Disabled * 1984	1.08 (3.33)	0.40 (3.25)	-0.72 (2.90)
Disabled * 1985	0.19 (2.32)	-1.22 (2.28)	-0.79 (2.18)
Disabled * 1986	-2.72 (2.66)	-2.63 (2.48)	-4.27** (1.92)
Disabled * 1987	-2.15 (1.99)	-2.71 (1.87)	-5.26*** (1.90)
Disabled * 1988	-0.90 (2.41)	-1.24 (2.33)	-2.88 (2.55)
Disabled * 1989	-2.62 (1.94)	-3.12 (1.95)	-4.48** (2.24)
Disabled * 1990	-2.89 (1.87)	-3.52* (1.93)	-5.03** (2.31)
Disabled * 1991	0.62 (2.97)	1.30 (2.89)	-2.96 (2.60)
Disabled * 1992	-3.58 (2.29)	-3.65 (2.29)	-6.34** (2.98)
Disabled * 1993	-5.44** (2.46)	-6.30** (2.48)	-6.54** (2.90)
Disabled * 1994	-3.91 (2.68)	-4.50 (2.74)	-6.22* (3.26)
Disabled * 1995	1.59 (9.48)	0.82 (9.21)	0.38 (8.32)
Covariates included?	No	Yes	Yes
Fixed effects?	No	No	Yes
N	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*$_{it}$ 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, and all standard errors are clustered at the individual level.

V1. Conclusion

This article 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 the PSID, a previously unexamined source, 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 1437 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 economic 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 1437 workers over our entire sample period—we see little evidence of adverse effects on weeks worked in our individual fixed effects model, but strong evidence of wage declines for the disabled, albeit one that began 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 were never disabled over this timeframe, 21 were always disabled, and the remaining 242 workers move in and out of what the PSID identifies as a "disabled" condition. The restricted sample has

the considerable advantage of protecting against biases caused by law-induced changes in self-identified disability status, but this benefit comes with two costs. First, the relatively small sample size necessarily generates higher standard errors in our restricted sample estimates that make it hard to distinguish statistically insignificant effects from true non-effects. Second, by focusing on more mature workers at the time of ADA enactment, we miss what is happening to new entrants to the labor market. Thus, workers who already have jobs (or at least more job experience) when the ADA comes into effect may be in a different position than those coming into the labor market for the first time.

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. On the other hand, this article provides evidence that the more mature workers who were found in every year of our sample did not suffer the same disemployment effects that previous empirical analyses have attributed to the ADA.

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[1] In this article, 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 (2004b).

[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 the reasonable accommodation 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 the failed effort to enact a federal jobs program for the disabled by one it its congressional supporters, see Dole (1994).

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

[7] The PSID maintains annual family-level data files from the program's inception in 1968 through 1997 after which they appear biennially through 2007.

[8] The PSID does add new individuals to the overall "roster"; for example the children of original 1968 interviewed household heads. Our unrestricted dataset generates 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 a static group of individuals, i.e., those who appear each year from 1981 through 1996.

[9] Several studies, which are referenced individually, are collected in Stapleton and Burkhauser (2003). Two important articles 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.

[10] Two other studies that bear noting are Epstein (1992), which foretold a detrimental effect in advance of the ADA becoming operational, and Jolls (2000), which analyzed Epstein's assertions using a price-theoretic framework.

[11] For a prominent example, see Sutton v. United Airlines, 527 U.S. 471 (1999).

[12] 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.

[13] 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."

[14] The SIPP will only register an individual's disability if it existed before June-September of the first interview year (depending on the rotation group to which the individual was assigned).

[15] We are able to track household head responses consistently across time by arranging a string of household interview numbers in the individual-level set in chronological order and then merging this data with it.

[16] 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).

[17] 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.

[18] 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.

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

[20] 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.

[21] The older two cohorts experienced a dip in disability share in 1986 that we pursued 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 questions regarding ADLs were removed in 1987, the percentage disabled returned to levels consistent with the year-to-year changes from 1981 to 1985.

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

[23] A variance inflation factor test run after each regression confirms that none of the difference-in-differences estimates is affected by multicollinearity. We thank an anonymous referee for raising this point.

[24] 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.

[25] 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."

[26] We focus on 1993 as an important year for observing ADA-related changes in our dependent variables because, although the ADA's first effective date was 1992 (for firms with at least twenty-five employees). We allow for a one-year lag to capture any potential employment and wage effects. For similar reasons, 1995 represents another crucial observation date since the ADA's coverage extended to firms with at least fifteen employees in 1994.