

Heterogeneity and the effect of mental health parity mandates on the labor market

By: [Martin Andersen](#)

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Abstract:

Health insurance benefit mandates are believed to have adverse effects on the labor market, but efforts to document such effects for mental health parity mandates have had limited success. I show that one reason for this failure is that the association between parity mandates and labor market outcomes vary with mental distress. Accounting for this heterogeneity, I find adverse labor market effects for non-distressed individuals, but favorable effects for moderately distressed individuals and individuals with a moderately distressed family member. On net, I conclude that the mandates are welfare increasing for moderately distressed workers and their families, but may be welfare decreasing for non-distressed individuals.

Keywords: mental health | benefit mandates | insurance | labor supply

Article:

1. Introduction

Mental illness is associated with a significant reduction in the labor market performance of individuals with mental illness (Bartel and Taubman, 1986, Bartel and Taubman, 1979, Ettner et al., 1997, Chatterji et al., 2007, Chatterji et al., 2011, Frank and Gertler, 1991). Although treatment is effective at improving productivity on the job (Berndt et al., 1998), it is unclear if mentally ill workers receive these productivity gains as higher wages. Even though treatment for mental illness is associated with improvements in productivity, many employers have been reluctant to provide coverage for mental illness because of stigma, concerns about adverse selection, and the large demand elasticity for mental health care (Gruber, 1994b, McGuire and Montgomery, 1982, Newhouse, 1993). In order to increase coverage of mental health benefits, many states have laws mandating coverage of mental illness in most or all state-regulated health insurance plans, with the number of states mandating that health insurance plans cover mental illness rising from 5 states in 1996 to 26 states in 2008. The federal government also mandates that all health insurance plans provide mental health coverage through the Affordable Care Act and the Mental Health Parity and Addiction Equity Act.

Despite the significant increase in the number of states with parity mandates and the universal reach of the federal mental health parity mandate, there is little research on the labor market effects of parity mandates and all of that research is focused on employees of firms with

fewer than 100 employees. Cseh (2008) provides the most recent and comprehensive study of the labor market effects of mental health parity mandates using data from the 1999 through 2004 March CPS on workers at firms with fewer than 100 employees. His work finds no evidence that parity mandates affect insurance coverage, hours worked, or wages. Earlier work by Kaestner and Simon (2002) reached broadly similar conclusions studying parity mandates and a number of other state laws affecting health insurance benefits using data from the 1989 through 1998 March CPS, again on workers at firms with fewer than 100 employees.

In this paper, I use data from the National Health Interview Survey and the Medical Expenditure Panel Survey to study the effect of parity mandates on employment, insurance coverage, earnings, and hours worked. Because these mandates are intended to help individuals in poor mental distress, I extend the previous literature by allowing for differential effects by mental distress.¹ When I do not account for heterogeneity by mental distress, I find, as in Cseh (2008) and Kaestner and Simon (2002), that parity mandates have no effect on any of these outcomes. However, the average masks significant and important heterogeneity in the effect of the mandate—I consistently find that parity mandates are associated with a higher probability of insurance coverage, higher wages, and longer working hours for individuals with moderate mental distress. These results would imply either an increase in the productivity of mentally distressed workers or an outward shift in the labor demand curve, with ambiguous effects on labor supply. When I study the effect of parity mandates on family members of individuals for whom I observe mental distress, I find that parity mandates increase hours worked, but not average wages, for individuals with a moderately distressed family member, indicating that there is an increase in labor supply associated with parity mandates. I interpret these results as evidence that parity mandates increase social welfare for moderately distressed workers and their families and argue that the wage increase I find for moderately distressed individuals in my main analysis arises from a productivity effect (Berndt et al., 1998).

2. Background

2.1. Effects of mental health and potential benefits of parity mandates

Poor mental health is strongly associated with worse labor market outcomes, including lower earnings, reduced labor supply, and a lower likelihood of being employed. Evidence for this adverse impact comes from a variety of studies employing different datasets and methods. Bartel and Taubman, 1986, Bartel and Taubman, 1979 study a sample of twins and find that worse mental health, based on physician diagnoses, was associated with lower wages, working fewer hours, and a lower likelihood of employment, which aggregate up to a 50% decline in earnings due to poor mental health. Frank and Gertler (1991) consider how measurement of mental health affects the association with earnings; two different indicators of mental distress, including a population-based survey measure that is similar to the measure I use, indicate that poor mental health was associated with a 22% reduction in log earnings (in separate regressions). Subsequent research, using instrumental variable models of psychiatric distress in the National Comorbidity Survey (Ettner et al., 1997), has found smaller adverse associations between poor mental health and labor market outcomes, implying that poor mental health is associated with 10–30% lower earnings than peers in good mental health. Chatterji et al., 2007, Chatterji et al., 2011 use data from the National Latino and Asian American Study and the National Comorbidity Survey–Replication to estimate the adverse consequences of a diagnosed mental illness and mental distress. Their estimates indicate

that a diagnosed psychiatric disorder reduces the probability of employment by 5–20 percentage points, but they find no evidence that mental illness or distress affects wages or hours worked.

Despite the previous literature, it is not clear that mental health causes worse labor market outcomes. This concern is not allayed by instrumental variable research designs since many of the instruments use either family history or the early onset of mental health problems as instruments, which are likely to be correlated with human capital (Fletcher and Wolfe, 2008, Fletcher, 2014, Currie and Stabile, 2006).² Cseh (2008), using the NLSY79, finds that poor mental health is correlated with worse labor market outcomes in models without fixed effects, but this result goes away when he includes fixed effects in his models. He argues that these results imply that any correlation between mental health and labor market outcomes actually reflects differences in personality, rather than mental health.

Just as poor mental health is associated with worse labor market outcomes, there is suggestive evidence that treatment for mental illnesses may improve productivity on the job. Berndt et al. (1998) uses data from a clinical trial of a depression treatment and obtains suggestive evidence that individuals report improved work performance following treatment and that this improvement is correlated with reductions in the severity of their symptoms. However, it is unclear if these changes in work performance translate into higher earnings for treated individuals.

These adverse correlates of mental illness, and the improvement in productivity from treatment, imply that there may be significant welfare gains for workers in poor mental health (and, potentially, their employers) from mental health treatment, yet employers have historically been reluctant to cover mental illness. There are a variety of reasons given for the differential treatment of mental and physical health conditions including stigma, beliefs that mental illnesses are somehow less “real” than physical illnesses, employer fears of adverse selection (McGuire and Montgomery, 1982), and more elastic demand for mental health treatment (Newhouse, 1993).

Beginning in the 1970s, state policymakers responded with requirements that insurance companies at least offer mental health benefits and in most cases these mandates neither applied to employers nor regulated cost-sharing and other aspects of the mental health benefit. Since the early 1990s, states have taken a more aggressive stand on mental health benefits by passing mandates that affect both employers and insurers and regulate the financial terms of the mental health benefit, often by requiring equal cost-sharing for mental and physical health benefits (referred to as “mental health parity”).

In order for these benefit mandates to improve the wellbeing of individuals in poor mental health, there must be an effect of parity on mental health treatment. Several single firm case studies indicate that mental health parity has a modest, if any, effect on mental health treatment. For example, when the Federal government implemented mental health parity in its employee benefit plan, there was no evidence of an increase in mental health utilization after the implementation of parity, relative to before, when compared to a control group of commercial insurance plans (Goldman et al., 2006). Zuvekas et al. (2002) presents a second case-study from implementing mental health parity in an employer setting, finding that parity combined with managed behavioral health care was associated with a large reduction in spending on mental health and substance abuse services, compared to firms that did not implement parity. It is not entirely clear that results from either of these case studies generalizes to the population because in both cases there was the potential for contamination from the implementation of managed behavioral health care that occurred contemporaneously with parity.³

An alternative to single-firm case studies is to study the state level mandates from the 1970s through the present day. The early parity and other benefit mandates (e.g. those which took effect

before 1996/1997) did not appear to be particularly effective at improving mental health, for example, there was no reduction in suicides associated with these mandates (Klick and Markowitz, 2006), even though one would expect an effective mental health benefit mandate to reduce the suicide rate. Likewise Pacula and Sturm (2000) and Bao and Sturm (2004) found some evidence that parity mandates worsened access to mental health care for most individuals, although there was an increase in the number of visits with specialist mental health providers for individuals in greater mental distress. The more recent wave of benefit mandates, however, appear to have been effective, with these mandates associated with an increase in mental health care utilization for employees of smaller firms and for individuals in worse mental distress (Busch and Barry, 2008), a reduction in the likelihood of high out-of-pocket spending (Barry and Busch, 2007), increased access to mental health care for more distressed individuals (Harris et al., 2006), and a reduction in suicide rates (Lang, 2013).

Given the lack of evidence that early mental health benefit mandates affected treatment, it is not surprising that studies have consistently found no effect of such mandates on the labor market or insurance coverage. For example, Jensen and Gabel (1992) and Gruber (1994b) studied benefit mandates in the middle of 1980s and found no evidence that firms were less likely to offer health insurance coverage or that employees of small firms were less likely to have health insurance coverage.⁴ Subsequent studies by Sturm (2000), Kaestner and Simon (2002), and Cseh (2008) have also found no evidence that parity mandates affect wages, hours worked, or insurance coverage.

However, the previous studies of labor market effects did not take into account potential heterogeneous effects by mental distress and parity mandates only appear to affect treatment for individuals in worse mental health. The idea that not all individuals are affected by a benefit mandate was introduced by Gruber (1994a), who noted that mandating a maternity benefit is unlikely to affect labor market outcomes for unmarried men, but should affect outcomes for married women of child-bearing age. Under this assumption, he shows that mandated maternity benefits reduced wages of married women, but did not reduce hours worked. Subsequent research on infertility mandates (Lahey, 2012) has also incorporated interactions with a group that is unlikely to be affected by the mandate. I follow this literature and allow for the effect of parity mandates to vary by pre-existing levels of mental distress, hypothesizing that parity mandates are unlikely to benefit workers with no mental distress, but should benefit workers with some mental distress.

2.2. State and federal regulation of mental health benefits

An important complicating factor in studying mental health (and other health insurance) benefit mandates is the role of state versus federal regulation of insurance benefits. In the period that I study—1998 through 2003—states are the main actors establishing mental health benefit mandates: 32 states enact a mental health benefit mandate of any kind with 19 of these laws establishing mental health parity. In 1996 Congress established a federal mental health parity mandate which prohibited those employers that chose to offer mental health coverage from discriminating with respect to annual or lifetime benefit limits, but otherwise the law left employers free to design mental health benefits as they saw fit (Gitterman et al., 2001).

In most cases, state mental health parity mandates require insurance plans offered by employers to provide coverage of mental illness on financial terms that are comparable to coverage for physical illness—that means uniform copayments or coinsurance rates, mental health visit

limits that are as great or greater than visit limits for physical health needs, and identical out-of-pocket, annual, and lifetime maximums. Yet these laws do not affect all firms in a state. Many laws exempt firms that are below a certain size threshold—often between 10 and 50 employees—or for which health insurance premiums are one or more percent higher because of the mandated mental health benefits. Finally, state mandates may not apply to all health insurance plans offered by a firm since firms may self-insure an insurance plan, in which case the plan is exempt from state regulation under the 1974 Employee Retirement Income Security Act (ERISA) (Acs et al., 1996).⁵ Collectively the exemptions built into state law and the preemption of state parity mandates by ERISA implies that less than the entire population of a state will be affected by a state mandate.⁶

As a result of ERISA and the various size and cost exemptions built into mental health parity mandates, many firms in a state with a mandate will not actually be subject to the mandate. The effect of ERISA and any cost exemptions on the estimated effect of parity mandates corresponds to measurement error, in which case estimates of the effect of parity mandates will be attenuated towards zero and hypothesis tests of the effect of a benefit mandate will be underpowered (Wooldridge, 2002, Zuvekas, 2000). Two solutions have been employed in the literature, which can be described as the “small firms” and “all firms” strategies. The “small firms” strategy restricts to individuals who are employed at smaller firms, the idea being that the probability that a firm offers a self-insured health insurance plan, hence exempt from state regulation, is lower (see footnote 5). Examples of the “small firms” strategy include Jensen and Gabel (1992), Gruber (1994b), and Kaestner and Simon (2002), while Cseh (2008) goes further and accounts for exemptions to parity mandates based on firm size that are written into state laws.⁷ However, such a solution implies that one cannot evaluate the effect of mandates on employment, since the sample must be employed in order for firm size to be known, hence the “small firms” approach (may) reduce measurement error at the cost of introducing selection bias.⁸ The alternative, “all firms” strategy, accepts the presence of measurement error, the bias from which, at least in the classical case, can be signed, in order to avoid the uncertain effects of selection bias (examples of the “all firm” strategy include Gruber, 1994a, Bitler and Schmidt, 2012, Lahey, 2012). For this paper, I adopt the “all firms” strategy.

3. Data and empirical approach

3.1. Data

I use linked data from the 1998 through 2003 Medical Expenditure Panel Survey (MEPS) and the 1997 through 2001 National Health Interview Surveys (NHIS). The MEPS is a nationally representative longitudinal survey conducted by the Agency for Healthcare Research and Quality (AHRQ) that follows individuals for two years. Each individual in the survey was a respondent to the NHIS in the year before he or she was recruited into the MEPS or can be linked to such an individual (e.g. by birth or marriage). The NHIS is a nationally representative cross-sectional survey of non-institutionalized adults conducted by the Centers for Disease Control (CDC).⁹ The time restrictions reflect changes in privacy safeguards and question content in the NHIS. Beginning in 1997, the NHIS includes a series of questions on mental distress (discussed below), while 2001 was the last year in which the NHIS identified residents of “large MSAs” which, along with an indicator for the size of the MSA and region of residence, can be used to assign almost every individual living in one of the identified MSAs to a state.¹⁰ Once individuals are assigned to an MSA using the NHIS, I adopt an “intent-to-treat” approach and assume that individuals

continue to live in that state (less than 2.5% of the weighted sample (2.0% unweighted) resides in a different region in the MEPS data than in their NHIS interview).

Between 1997 and 2001, there were 342,102 adult participants in the NHIS, of whom 46,435 can be linked to individuals in the MEPS. However, of these 46,435 individuals, less than half (20,840) can be uniquely assigned to a state and less than 10,000 individuals were selected as “sample adults” and completed the mental distress questionnaire (discussed in more detail below).¹¹ My sample is reduced even further because I restrict my sample to individuals of working age—25 to 64—which yields a total of 6,993 individuals for whom I have data from at one least year of the MEPS. The final sample size for my main analyses is 13,108, reflecting the fact that 756 individuals are only in one year of the MEPS. In some supplementary analyses, I use the mental distress of a family member, rather than one's own mental distress, in which case I have 8843 observations on 4753 individuals (I only have data from one wave of the MEPS for 541 individuals).

The MEPS collects information on insurance coverage, employment, weekly hours worked, and weekly and annual earnings. An individual has health insurance from her own employer if the individual reports having a private health insurance policy from her own employer or union and the employer or union contributes towards the cost of coverage. In order to exclude a small handful of outliers, I trim the wage distribution at the 1st and 99th percentiles of the wage distribution.¹² I convert both earnings and the nominal wage into real 2010 dollars using the CPI-U. I include both hourly and salaried workers in my main sample. The longitudinal structure of the combined NHIS–MEPS sample allows me to focus on measures of mental distress that predate both the parity mandates that I am studying and the various outcomes of interest.

To the NHIS–MEPS data, I append data on state mental health regulations, based on Lang (2013), to indicate the presence of a mental health parity mandate.¹³ In order to capture the effect of parity mandates on mental health, I coded dummy variables to indicate the presence of a parity mandate in the state of residence at the time of the NHIS interview and for each of the subsequent two years for use with the MEPS data. For example, a NHIS respondent living in Kansas in 2000 would have no mandate indicated at the time of the NHIS or for the first year in the MEPS, but would have a mandate in the second year in the MEPS.

Table 1 presents summary statistics on my sample for states that do not have a mandate between 1997 and 2003 and those states that will have a mandate in that time period, before and after the mandate is implemented. In all three columns, the distribution of mental distress is broadly consistent, whether measured by the sum of all six items, or by grouping into none through severe levels of distress. The comparison between the two “No Mandate” columns indicates that states that went on to adopt a parity mandate did not differ in the burden of mental illness relative to those states that did not adopt a mandate by 2003, which suggests that the mandates were unlikely to have been a response to a high burden of mental distress. However, while insurance outcomes are similar between all three sets of states, with little evidence of an association between parity mandates and insurance coverage, states that had not adopted a mandate by 2003 had lower employment, earnings, and hourly wages than states that adopted a mandate by 2003. Lastly, demographic characteristics are, for the most part, comparable for individuals in states that will eventually adopt a parity mandate, with the exception of the racial/ethnic breakdown and share of the population with a GED or college degree, where there are more Black individuals and individuals with a GED, but fewer college graduates, in states that do not adopt a parity mandate by 2003 than in states that will adopt a parity mandate. The final row of the table counts the number of unique states in each group, which indicates that in my main analysis the effect of parity

Table 1. Summary statistics.

	Never mandate			Ever mandate			Mandate	
	No mandate		P	No mandate		P		
Mental distress ^a								
Six items	2.59	(3.96)	0.875	2.55	(3.94)	0.729	2.46	(3.60)
None	0.47	(0.50)	0.944	0.46	(0.50)	0.503	0.44	(0.50)
Low	0.33	(0.47)	0.888	0.34	(0.47)	0.297	0.37	(0.48)
Moderate	0.16	(0.37)	0.901	0.16	(0.37)	0.638	0.17	(0.38)
Severe	0.04	(0.19)	0.622	0.03	(0.18)	0.217	0.02	(0.15)
Employed	0.81	(0.40)	0.010	0.84	(0.36)	0.303	0.83	(0.37)
Offered health insurance	0.56	(0.50)	0.636	0.57	(0.49)	0.671	0.56	(0.50)
Insured by own employer	0.59	(0.49)	0.632	0.60	(0.49)	0.660	0.61	(0.49)
Insured by any employer	0.73	(0.44)	0.899	0.74	(0.44)	0.997	0.74	(0.44)
Earnings	49,108	(35,571)	0.016	54,948	(38,178)	0.098	50,428	(34,669)
Hourly wage	22.55	(13.26)	0.037	24.97	(14.41)	0.267	23.48	(13.18)
Hours worked	41.1	(10.8)	0.550	41.5	(10.9)	0.627	41.1	(10.2)
Age	42.4	(10.7)	0.969	42.4	(10.7)	0.384	41.9	(10.7)
Female	0.54	(0.50)	0.408	0.55	(0.50)	0.842	0.55	(0.50)
<i>Race/ethnicity</i>								
White	0.69	(0.46)	0.443	0.63	(0.48)	0.444	0.60	(0.49)
Black	0.18	(0.38)	0.097	0.12	(0.32)	0.706	0.13	(0.34)
Hispanic	0.10	(0.29)	0.216	0.17	(0.38)	0.418	0.21	(0.40)
Other	0.03	(0.18)	0.149	0.07	(0.26)	0.538	0.06	(0.24)
<i>Education</i>								
High school dropout	0.12	(0.32)	0.833	0.11	(0.32)	0.438	0.12	(0.33)
GED	0.04	(0.20)	0.003	0.03	(0.16)	0.623	0.03	(0.18)
High school	0.45	(0.50)	0.304	0.42	(0.49)	0.675	0.41	(0.49)
College	0.20	(0.40)	0.018	0.25	(0.43)	0.390	0.23	(0.42)
More than college	0.11	(0.31)	0.632	0.12	(0.32)	0.878	0.12	0.11
Other degree	0.08	(0.27)	0.504	0.08	(0.26)	0.956	0.08	(0.27)
<i>Marital status</i>								
Married	0.49	(0.50)	0.884	0.50	(0.50)	0.922	0.50	(0.50)
Widowed	0.03	(0.18)	0.114	0.02	(0.14)	0.686	0.02	(0.15)
Divorced	0.19	(0.39)	0.204	0.17	(0.37)	0.626	0.17	(0.38)
Separated	0.03	(0.16)	0.118	0.04	(0.19)	0.019	0.03	(0.16)
Never married	0.26	(0.44)	0.555	0.27	(0.45)	0.988	0.27	(0.45)
No. full sample	5776			2218			5114	
No. employed	3591			1428			3215	
Unique states	17			7			13	

a Mental distress ranges from 0 to 24, subgroups are defined as 0 = none, 1–5 = low, 6–12 = moderate, 13–24 = severe distress.

Note: Standard deviation in parentheses. P-values for comparison of means of columns to the left and right of the P-value columns clustered on state.

Source: Author's analysis of the 1997–2001 National Health Interview Survey and the 1998–2003 Medical Expenditure Panel Survey.

mandates arises from seven states that switched from not having a mandate to having a mandate during this period.¹⁴

3.2. Measuring mental distress

The NHIS includes the six item Kessler scale as a measure of mental distress. This scale was designed for use in the NHIS and has been adapted for use in other surveys. As part of the development process, a validation study was undertaken that compared the predictive accuracy of the six-item score with clinical diagnoses using the Structured Clinical Interview for DSM-IV (Kessler et al., 2002), which is the gold standard for diagnosis in psychiatry. The validation study indicated that the six-item score is predictive of mental distress, with an area under the receiver operating characteristic curve of 0.879, which is the probability that a randomly chosen individual with a clinically diagnosed mental illness scores higher than a randomly chosen individual without such a diagnosis (Obuchowski, 2003). The six items ask individuals how often, during the past thirty days, an individual felt: nervous; hopeless; restless or fidgety; so depressed that nothing could cheer you up (simplified as “sad” in the NHIS); that everything was an effort; and worthless. The individual can choose among: all of the time; most of the time; some of the time; little of the time; or none of the time. The original items were scored on the basis of item-response theory, which provides weights for the likelihood of a clinical diagnosis for each response pattern, however, these weights are roughly linear, so it has become common to simply score each item from 0 to 4, with 0 corresponding to none of the time, while 4 corresponds to all of the time and construct a six-item score as the sum of the individual item scores.

3.3. Empirical approach

In my main analysis, I use a triple-difference specification for individual i in state s and year t . The differences arise from comparing individuals in states that do versus do not adopt a mandate, before versus after adopting the mandate, across different levels of mental distress. However, unlike the conventional triple-difference specification, I do not assume that within a “treated” state there is a group of individuals who are not affected by the mandate.¹⁵ For outcome O_{ist} , the specification is:

$$O_{ist} = \beta_1 \text{Mandate}_{st} + \beta_2 \text{NHISMandate}_{st} + \beta_3 \text{Distress}_i + \beta_4 \text{Mandate}_{st} \times \text{Distress}_i + \beta_5 \text{NHISMandate}_{st} \times \text{Distress}_i + X_{it}\Gamma + \sigma_s + T_t + \epsilon_{ist}$$

where O_{ist} is an outcome for individual i in state s during year t . Mandate_{st} is a dummy that equals 1 if the state enacted a new parity mandate after the initial National Health Interview Survey year, while NHISMandate_{st} is a dummy that equals 1 if the state had a parity mandate in effect in the year of the National Health Interview Survey year. Distress_i is a vector of indicators for an individual's mental distress,¹⁶ measured in the NHIS. X_{it} is a vector of individual covariates—age, marital status, race/ethnicity, educational achievement, and self-rated health status—and σ_s and T_t are state and year fixed effects.

The interaction between NHISMandate_{st} and Distress_i controls for any potential endogeneity of the mental distress measure due to the presence of a parity mandate in the year of assessment, while the interaction with Mandate_{st} is the object of interest as it measures the

differential association of parity mandates with the outcome variable by the level of mental distress. The result is that β_1 measures the association between the new parity mandate and the outcome variable, while β_4 measures the difference in the association at various levels of mental distress. Identification rests on the assumption that prospective changes to parity laws do not affect mental distress.

A final issue in estimating (1) is that employment related outcomes—offered insurance from an employer, having health insurance from one's own employer, earnings, wages, and weekly hours worked—are censored for individuals who are not employed. As a result, the coefficients of (1) cannot be consistently estimated using standard linear or Probit regression. The essential problem is that employment may be informative of these other outcomes in a way that is not observable. With some abuse of notation (and introducing Empl_{ist}^* as a latent index for employment), the underlying structural model is (see Heckman, 1979, for a complete derivation):

$$\begin{aligned}\text{Empl}_{ist}^* &= \beta X_{ist} + u_{ist} \\ O_{ist}^* &= \gamma X_{ist} + v_{ist} \\ O_{ist} &= O_{ist}^* \text{ if } \text{Empl}_{ist}^* \geq 0\end{aligned}$$

As a result, the expectation of O_{ist} conditional on X_{ist} will yield:

$$E[O_{ist} | X_{ist}, \text{Empl}_{ist} \geq 0] = \gamma X_{ist} + E[v_{ist} | \beta X_{ist} \geq -u_{ist}]$$

Assuming that u_{ist} and v_{ist} are jointly normal then one can consistently estimate γ using Heckman's (1979) maximum likelihood procedure (or a corresponding version for binary outcomes with a Probit second stage). In estimating these models, I follow previous work (Mulligan and Rubinstein, 2008) and include additional covariates in the first stage regression as instruments for employment status so that identification of the remaining coefficients of γ is not solely due to functional form. The instruments that I employ are the number of children between ages 0 and 6, ages 7 and 18, and the interaction of these counts with gender. These instruments can be excluded from the second stage of the model if the presence of children does not affect mental health. I test this restriction by regressing the continuous mental distress score on the number of children in each age group, gender, their interaction with gender, and all controls. The resulting coefficients on the instruments are all non-significant and the joint F-statistic for significance of the number of children and the interaction with female is 1.21, which is significantly smaller than the 5%, or even 10% significance level for an F-statistic with 4 and 21 degrees of freedom (2.84 and 2.23, respectively).¹⁷ When I replace the dependent variable with an indicator for employment, the joint F statistic is 44.6, which rejects the null of no association with employment at less than the 0.0001 level. I conclude from these results that the number of children under 7 and between 7 and 18 and their interaction with gender are associated with employment and can be excluded from the second-stage regressions when I estimate Heckman-corrected regression models.

In presenting my results for binary outcomes, for which I use Probit (or “Heckit”) models, I estimate average marginal effects using the approach outlined in Ai and Norton (2003) and standard errors using the delta method. The specific approach that I take assumes that $\text{NHISMandatest} = 0$ and estimates the marginal effect of the mandate at each level of mental

distress and the marginal effect of mental distress when there is no mandate in effect. As a result, the marginal effects are interpretable in a manner that is consistent with a linear specification.

4. Results

4.1. Employment and insurance coverage

Table 2 presents results of estimating (1) for employment, offered health insurance by one's employer, and insured by one's employer. Because the policy that I study varies at the state-level, I construct covariance matrices assuming that the error term in (1) is correlated within states (Bertrand et al., 2006). All point estimates are average marginal effects and the standard errors were calculated using the Delta method. The first two columns, which look at the probability of employment, indicate that individuals in severe mental distress in the NHIS year are less likely to be employed one to two years later, when they respond to the MEPS, which is consistent with previous work (e.g. Bartel and Taubman, 1986, Chatterji et al., 2007, Chatterji et al., 2011, Ettner et al., 1997). Once I account for heterogeneity by mental distress, I find a marginally significant increase in the probability of employment associated with the parity mandate, but the interactions with mental distress are very imprecisely estimated. In general the size of the employment effect of the mandate is plausible, except for severely distressed individuals, for which I find a sixteen percentage point increase in the probability of employment. However, there are only 135 observations on individuals with severe distress in states that adopt a mandate, hence one should not put too much weight on the size of the effect, but rather focus on the direction.

Columns (3)–(6) demonstrate that distressed individuals are less likely to be offered health insurance by their employer, particularly severely distressed employees. However, after a state implements a parity mandate, there is no longer any evidence of a disparity in insurance offer rates for distressed individuals. Similar results hold in the last four columns of the table, which indicates that distressed workers are less likely to have insurance from their employer (when one does not account for selection into employment) and parity mandates increase insurance coverage among moderately distressed individuals, relative to non-distressed individuals. Collectively, these results demonstrate that parity mandates have only a limited effect on employment, but significantly increase offers and take up of insurance coverage for moderately distressed individuals.

Table 3 presents results from estimating (1) for log annual earnings, log weekly wage, or log weekly hours worked. The first three rows demonstrate that mental distress is associated with worse labor market outcomes, as in previous work (Bartel and Taubman, 1986, Bartel and Taubman, 1979, Ettner et al., 1997, Chatterji et al., 2007). As was the case with Table 2, models without the distress-by-mandate interaction replicate previous results and demonstrate that parity mandates are not associated with earnings, wages, or hours worked (Cseh, 2008). However, once I allow for an interaction between distress and the mandate, I find evidence of marginally significant reductions in earnings and hours worked and a non-significant reduction in wages for non-distressed individuals (in selection-corrected models). The presence of such a large wage reduction, even though it is non-significant, and the marginally significant reduction in hours worked, implies that even non-distressed workers are affected by the mandate because either: (i) employers take account of within-family spillover effects and I am picking up reductions in labor demand for non-distressed workers who have distressed family members on the same insurance plan or (ii) the incidence of the benefit mandate is neither individual nor subgroup-specific, so employers cannot target certain employees or groups of employees for wage and hours reductions.

Table 2. Effect of mental health parity mandates on employment and insurance coverage.

	Employed		Offered employer insurance				Insurance from own employer			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Probit	Probit	Probit	Heckit	Probit	Heckit	Probit	Heckit	Probit	Heckit
Low distress	0.014 (0.012)	0.024 (0.015)	-0.003 (0.015)	-0.005 (0.015)	-0.023 (0.020)	-0.026 (0.021)	-0.014 (0.021)	-0.016 (0.021)	-0.030 (0.025)	-0.034 (0.027)
Moderate distress	-0.018 (0.012)	-0.026 (0.016)	-0.040* (0.018)	-0.031 (0.018)	-0.063* (0.022)	-0.050* (0.023)	-0.051+ (0.028)	-0.041 (0.028)	-0.081* (0.033)	-0.065+ (0.034)
Severe distress	-0.154** (0.031)	-0.155** (0.042)	-0.140** (0.035)	-0.067+ (0.034)	-0.114* (0.043)	-0.042 (0.038)	-0.144** (0.049)	-0.065 (0.044)	-0.101 (0.060)	-0.023 (0.051)
Mandate	0.015+ (0.008)	0.028+ (0.015)	0.022 (0.017)	0.017 (0.015)	0.002 (0.020)	-0.005 (0.017)	0.017 (0.018)	0.012 (0.017)	-0.011 (0.019)	-0.018 (0.017)
× Low distress		-0.043 (0.029)			0.048 (0.040)				0.059 (0.039)	0.067 (0.039)
× Moderate distress		-0.017 (0.023)			0.046* (0.021)				0.083** (0.026)	0.080** (0.028)
× Severe distress		0.159** (0.048)			-0.116 (0.151)				-0.256* (0.120)	-0.321* (0.120)
Mean	0.82	0.82	0.75	0.75	0.75	0.75	0.69	0.69	0.69	0.69
N	13,108	13,108	10,213	13,108	10,213	13,108	10,213	13,108	10,213	13,108
# Censored				2895		2895		2895		2895

Note: Dependent variable indicated by column header. Models also include presence of a mandate during the NHIS interview (interacted with distress when the mandate is interacted with distress), race/ethnicity, education, age, marital status, gender, self-rated health status, and state and year fixed effects. Heckit models include number of children under 6 and between 7 and 18 and the interaction of number of children with gender as excluded instruments for employment. All point estimates are average marginal effects; standard error of average marginal effect calculated via the Delta method and clustered on state in parentheses. Regressions weighted to be representative of the 25 to 64 year old population of the largest MSAs in the United States.

+ P < 0.1. *P < 0.05. **P < 0.01.

Source: Author's analysis of the 1997–2001 National Health Interview Survey linked to the 1998–2003 Medical Expenditure Panel Survey.

Table 3. Effect of mental health parity mandates on earnings, wages, and labor supply.

	Log annual earnings				Log weekly wage				Log weekly hours			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	OLS	Heckman	OLS	Heckman	OLS	Heckman	OLS	Heckman	OLS	Heckman	OLS	Heckman
Low distress	0.012 (0.023)	-0.002 (0.022)	0.008 (0.023)	-0.022 (0.027)	0.030* (0.012)	0.025* (0.012)	0.020 (0.016)	0.010 (0.016)	0.011 (0.008)	0.005 (0.008)	0.017 (0.012)	0.004 (0.013)
Moderate distress	-0.086* (0.035)	-0.061 (0.040)	-0.119* (0.048)	-0.080 (0.052)	-0.040* (0.017)	-0.031 (0.021)	-0.068** (0.021)	-0.054* (0.026)	-0.017 (0.015)	-0.005 (0.017)	-0.046* (0.022)	-0.028 (0.022)
Severe distress	-0.102 (0.071)	0.156+ (0.082)	-0.130+ (0.075)	0.126 (0.097)	-0.066 (0.044)	0.037 (0.059)	-0.050 (0.053)	0.053 (0.073)	0.046 (0.030)	0.164** (0.034)	0.058* (0.024)	0.176** (0.032)
Mandate	-0.043 (0.031)	-0.059 (0.037)	-0.067 (0.047)	-0.111+ (0.061)	0.004 (0.022)	-0.004 (0.025)	-0.032 (0.038)	-0.049 (0.040)	-0.011 (0.018)	-0.018 (0.017)	-0.027 (0.028)	-0.047+ (0.024)
× Low distress			0.020 (0.069)	0.089 (0.091)			0.044 (0.060)	0.067 (0.062)			0.007 (0.027)	0.035 (0.027)
× Moderate distress			0.088 (0.084)	0.125 (0.087)			0.126+ (0.062)	0.141* (0.064)			0.089* (0.034)	0.110** (0.030)
× Severe distress			0.178 (0.107)	-0.055 (0.165)			0.055 (0.135)	-0.047 (0.131)			0.011 (0.037)	-0.109 (0.066)
Mean	10.6	10.6	10.6	10.6	2.99	2.99	2.99	2.99	3.67	3.67	3.67	3.67
<i>N</i>	8234	11,129	8234	11,129	8234	11,129	8234	11,129	8234	11,129	8234	11,129
# Censored		2895		2895		2895		2895		2895		2895

Note: Dependent variable indicated by column header. Models also include presence of a mandate during the NHIS interview (interacted with distress when the mandate is interacted with distress), race/ethnicity, education, age, marital status, gender, self-rated health status, and state and year fixed effects. Heckman models include number of children under 6 and between 7 and 18 and the interaction of number of children with gender as excluded instruments for employment. Standard errors clustered on state in parentheses. Regressions weighted to be representative of the 25–64 year old population of the largest MSAs in the United States.

+*P* < 0.1. **P* < 0.05. ***P* < 0.01.

Source: Author's analysis of the 1997–2001 National Health Interview Survey linked to the 1998–2003 Medical Expenditure Panel Survey.

The latter implication is starkly different from previous studies (e.g. Gruber, 1994a, Bhattacharya and Bundorf, 2009) that find evidence of wage offsets that apply to affected individuals—either women who may bear children (Gruber, 1994a) or obese workers (Bhattacharya and Bundorf, 2009).

When I incorporate heterogeneity into the effect of the mandate, I find that there is also a statistically significant increase in wages and hours for moderately distressed workers, which has not been found in any previous studies. The wage and hours effects only appear for moderately distressed workers, which is the set of workers who are more likely to be offered and to take-up employer health insurance coverage (see Table 2); this concentrated effect supports the validity of my results since the only way parity mandates should affect labor market outcomes is by altering insurance coverage.

Results on wages and earnings provide an initial view of the welfare effects of parity mandates. The reduction in hours worked for non-distressed individuals suggests that there was an inward shift of the labor demand curve, since wages did not go up for this population. On the other hand, the increase in hours worked and wages for moderately distressed workers is consistent with an increase in labor demand, with ambiguous effects on labor supply. The increase in demand for moderately distressed workers is consistent with reductions in mental distress and improvements in productivity, which is suggested by Berndt et al. (1998). I return to this point below.

In order to understand the sources of the changes in wages and hours, Table 4 presents estimates that interact the mandate, distress, and the mandate-by-distress interaction with an indicator for belonging to a group. In columns (1)–(4), group is an indicator for having health insurance from one's employer. In principle a mandate that affects insurance benefits should not affect employees who are not offered (or do not have) health insurance coverage by (from) an employer. In columns (5)–(8) group is an indicator for working at a firm with <200 employees, since mandates are more likely to be binding on smaller, rather than larger, firms because large firms are more likely to be self-insured (see footnote 5). Because of missing data on firm size, the sample for the smaller firm analysis is slightly smaller than for having health insurance from one's employer. All eight models are based on a Heckman corrected specification, but I exclude the group interaction in the selection step of the Heckman model since the groups are not defined for unemployed individuals. Results using OLS are similar and available from the author on request. In the interests of space, Table 4 only reports coefficients that are interacted with the mandate dummy.

For individuals without health insurance from their employer, I find no evidence that parity mandates affects their wages or hours worked, and this lack of an effect persists after I include the interaction with mental distress (even numbered columns). I also find no evidence that workers with health insurance receive, on average, lower wages or work longer hours after a parity mandate is passed. But, when I include the interaction with mental distress, I find that distressed workers with health insurance from their employer receive higher wages after a parity mandate than workers without health insurance coverage. In addition, when I account for the entire effect of the mandate (by summing rows a and e, b and f, etc.), I find that mentally distressed workers with health insurance coverage receive statistically significantly higher wages and work longer hours when there is a mandate than in the absence of a parity mandate. These results are consistent with the idea that parity mandates affect labor market outcomes for individuals with health insurance coverage, but not those without insurance coverage.

A similar analysis using firm size indicates that all of the effect of parity mandates on wages and hours worked arise from smaller, rather than larger, firms, which are also the firms that are

Table 4. Subgroup effects of mental health parity mandates on earnings.

	Health insurance from employer				Smaller firm			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log wage	Log wage	Log hours	Log hours	Log wage	Log wage	Log hours	Log hours
(a) Mandate	-0.022 (0.055)	0.006 (0.056)	-0.010 (0.029)	-0.027 (0.041)	0.002 (0.022)	-0.041 (0.041)	-0.004 (0.019)	-0.011 (0.026)
(b) × Low distress		-0.115 (0.074)		0.053 (0.070)		0.095 (0.067)		0.015 (0.032)
(c) × Moderate distress		0.003 (0.086)		0.024 (0.052)		0.045 (0.079)		0.025 (0.041)
(d) × Severe distress		0.027 (0.160)		-0.139 (0.103)		0.035 (0.128)		-0.124 (0.076)
(e) × Group	0.028 (0.066)	-0.045 (0.057)	-0.011 (0.032)	-0.014 (0.037)	-0.017 (0.029)	-0.015 (0.038)	-0.007 (0.014)	-0.048* (0.021)
(f) × Low distress		0.180** (0.069)		-0.044 (0.068)		-0.060 (0.063)		0.038 (0.043)
g) × Moderate distress		0.135± (0.082)		0.084± (0.044)		0.152 (0.108)		0.146± (0.076)
(h) × Severe distress		-0.140 (0.193)		0.038 (0.080)		-0.230 (0.167)		0.023 (0.067)
N (group = 0)	2201	2201	2201	2201	3142	3142	3142	3142
N (group = 1)	6033	6033	6033	6033	4711	4711	4711	4711
Mean (group = 0)	2.54	2.54	3.50	3.50	3.06	3.06	3.69	3.69
Mean (group = 1)	3.04	3.04	3.72	3.72	2.81	2.81	3.65	3.65
<i>P</i> -values								
(a) + (e) = 0	0.832	0.318	0.249	0.071	0.625	0.163	0.496	0.024
(b) = (c) = (d) = 0		0.013		0.183		0.560		0.134
(f) = (g) = (h) = 0		0.000		0.005		0.160		0.004
(b) + (f) = (c) + (g) = (d) + (h) = 0		0.067		0.000		0.029		0.000

Note: Dependent variable indicated by column title; group indicated by header. Models also include presence of a mandate during the NHIS interview, interacted with group, (interacted with distress when the mandate is interacted with distress), race/ethnicity, education, age, marital status, gender, self-rated health status, and state and year fixed effects. All models estimated using Heckman's selection corrected model, using the number of children under 6 and between 7 and 18 and the interaction of number of children with gender as excluded instruments for employment; because group is only observed for individuals who are employed, the selection equation does not include interactions with group membership. All point estimates are average marginal effects; standard error of average marginal effect calculated via the Delta method and clustered on state in parentheses. Regressions weighted to be representative of the 25–64 year old population of the largest MSAs in the United States.

+P < 0.1. *P < 0.05. **P < 0.01.

more likely to be affected by a parity mandate since they are less likely to self-insure, if they offer health insurance coverage (Kaiser Family Foundation, 2011). These results are also suggestive that parity mandates improve the welfare of moderately distressed workers, for whom there is a significant increase in hours worked if they work at a small firm, relative to a large firm, and there is no statistically significant difference in wages between large and small firm employees, although the sums of rows (b) and (f), (c) and (g), and (d) and (h) are jointly significantly different from 0, implying that there is a statistically significant effect of parity mandates on wages among workers at smaller firms with mental distress.

4.2. Effects on family members

In order to study the effect of parity mandates on labor supply, I turn to individuals for whom I do not expect an increase in wages due to a productivity effect, but for whom there may be an increase in hours worked—family members of the NHIS “sample adult”. Essentially, the question is how do parity mandates affect employment, insurance coverage, and labor supply of individuals who live with someone with mental distress. Table 5 presents results from estimating my main specification on family members; in the interest of space I only report average marginal effects from Heckit and Heckman models (except for column (1), which is from a Probit model).

I find a marginally significant effect of the parity mandate on employment and little evidence that distress among other family members affects employment or insurance coverage, suggesting that family members do not enter the workforce or seek jobs with insurance coverage as a result of the parity mandate, no matter the level of mental distress of their family member. However, I do find a large and statistically significant increase in hours worked, but not wages, for individuals with a moderately distressed family member after a parity mandate takes effect. The increase in hours worked, but not wages, suggests that family members of moderately distressed individuals increase their labor supply in response to parity mandates.

These results help to interpret the results from Table 3 where the effect of parity mandates on labor supply for distressed workers was ambiguous, while there was a clear outward shift of the labor demand curve. Given that family members of mentally distressed individuals increase their labor supply following a parity mandate, it is likely that mentally distressed individuals would do so as well. Therefore, I interpret the results for distressed workers in Table 3 as evidence of an outward shift in both labor supply and labor demand. Such an outward shift is consistent with a productivity increase due to an improvement in mental health.

One objection to the presence of a productivity increase is why didn't employers do this voluntarily without a mandate? The answer lies in the fact that many employers did provide mental health benefits in the absence of any type of mandate (e.g. Gruber, 1994b), so some employers were paying to induce this kind of productivity improvement. The second explanation is that mental health is a form of general human capital, which, in the simplest model, implies that firms should not pay for mental health coverage since the productivity benefit may accrue to subsequent employers instead.

4.3. Specification checks and falsification tests

I conducted a number of falsification checks using a “pseudo-mandate” that takes effect three years before the actual mandate and exclude observations from states and years with a mandate in effect. In most cases, the pseudo-mandate is either non-significant (and typically small), which supports

the identifying assumption that there are no differential trends in states that enact a mandate, or has the opposite sign of the effect of the actual mandate, in which case the observed mandate effect may arise from mean reversion (results available upon request). There are only two cases—being offered health insurance by one's employer and taking up that offer for which this may be the case, however, in neither case is the interaction of the pseudo-mandate with either low or moderate distress significant and the relatively small number of people with severe distress means that while I can reject that the differential effect is 0, I cannot reject that the total effect of the mandate on insurance coverage for this sample is zero.

In addition, I conducted a number of specification checks, including augmenting (1) with census region or year-by-distress group fixed effects¹⁸, distress score interacted with state and/or year, omitting observations from states with a parity mandate in effect during the year of the NHIS interview, using a quadratic specification in the distress score, rather than the four distress groups, and dropping each state that adopted a mandate from the sample. Results were generally consistent across all of the alternative specifications for each dependent variable and are available from the author upon request.

5. Discussion

Previous research on the effects of mental health parity mandates has focused on narrow parts of the population, typically employees of firms with fewer than 100 workers. Those studies, by and large, do not find the expected effects of parity mandates—a reduction in labor demand and a (potential) increase in labor supply—leading to a consensus that parity mandates do not affect the labor market. By contrast, the results in this paper demonstrate that parity mandates, if anything, increases employment for all individuals, and makes it more likely that moderately distressed individuals work for employers who offer health insurance coverage, receive insurance coverage from their own employer, have higher weekly wages, and work more hours per week. Why is there such a disconnect between these results and the previous literature? The essential reason that I find different results from the previous literature (e.g. Cseh, 2008, Kaestner and Simon, 2002) is because I parametrically account for heterogeneous treatment effects by mental distress. When I do not do so, I replicate previous findings of no effect of parity mandates on the labor market. Essentially, the positive effects of mental parity mandates on moderately distressed workers do not “show up” in the average effect because moderately distressed individuals make up less than 20% of the labor force.

A related question is why I find that parity mandates increase wages for moderately distressed workers? In the classical Summers (1989) framework this is a very puzzling result since it implies an outward shift in labor demand following a (presumably) costly mandate. However, an alternative interpretation comes from Berndt et al. (1998), which demonstrates an increase in on-the-job productivity associated with treatment for clinical depression—parity mandates make mentally distressed workers more productive. This interpretation requires that the mandates are effective at reducing depression, which is suggested by Lang (2013), who documents a reduction in suicides associated with many of the same parity mandates that I study.

Other mechanisms may also generate increases in wages for mentally distressed workers. For example, the labor market may be segmented into higher and lower paying segments, with a greater toll on mental health in the higher paying segments. In this scenario, parity mandates may induce workers to shift into the higher paying segment because they can offset the toll on mental health with better mental health coverage. Alternatively, enhanced health insurance benefits may

reduce worker turnover, encouraging workers and employers to make productivity-enhancing investments in human capital.

The heterogeneous benefits of parity mandates also indicates that the welfare benefits of parity mandates are not distributed uniformly in society. Non-distressed workers at smaller employers suffer from a reduction in labor demand, that is not offset by an increase in labor supply, while moderately distressed workers at those same types of firms appear to increase labor supply in response to the mandate, indicating a welfare improvement.

Because the mechanism appears to be related to the availability of valued benefits, there are likely to be important implications associated with the Affordable Care Act and the implementation of the Mental Health Parity and Addiction Equity Act (MHPAE). The MHPAE will, for the first time, impose meaningful requirements on larger, as well as smaller, firms to provide mental health benefits at parity with physical health benefits (if they provide any mental health coverage), while the Essential Health Benefits provision of the ACA will require that plans offer mental health coverage. As a result, eventually all plans will be required to provide mental health benefits at financial parity.¹⁹ My results indicate that both mentally distressed workers and their family members value these benefits and, in the case of the workers, appear to experience a significant productivity increase associated with the benefits. The result is that the ACA will increase labor supply among moderately distressed workers and their families, allow for a resorting of these workers between smaller and larger firms, and increase the productivity of mentally distressed workers.

6. Conclusions

This paper evaluates the effect of mandating a health insurance benefit, mental health parity, the requirement that state-regulated insurance plans have equivalent cost-sharing requirements for mental and physical health care, on the labor market. In a standard difference-in-difference specification parity mandates had no effect on employment, insurance coverage, or earnings. However, when I allow for differential effects by mental health status, I find statistically significant increases in insurance coverage, weekly wages, and hours worked for moderately distressed individuals. I also argue that the increase in wages for moderately distressed individuals does not imply that the labor demand curve shifted out, but rather that there was an increase in the productivity of moderately distressed workers since I find similar effects on hours worked, but not wages, for individuals who live with a moderately distressed worker, which I interpret as a within household spillover of insurance coverage from other family members onto the distressed family member.

Results comparing individuals with insurance from their employer, versus without, and who work at smaller, versus larger, firms are consistent with an effect of the mandate only for workers with insurance coverage and an effect only for workers at firms that are likely to be subject to the mandate. Additional falsification tests fail in the desired manner and my results are robust to a number of specification checks.

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