The Effects of State Mental Health Parity Legislation on Perceived Quality of Insurance Coverage, Perceived Access to Care, and Use of Mental Health Specialty Care

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Objective. To assess the impacts of recent state mental health parity legislation on perceived quality of health insurance coverage, perceived access to needed health care, and use of mental health specialty services by individuals with likely need for mental health care.

Data Sources. The study sample came from two waves of a national household survey first fielded in 1997–1998 and then in 2000–2001. The analysis used a subset of the sample.

Study Design. The study took the Difference-in-Difference-in-Difference approach to investigate changes in self-perceived quality of health insurance coverage and access to needed health care, and use of mental health specialty care by the group with mental disorders (relative to those without) in states with parity legislation of different comprehensiveness (relative to the nonparity states) in the years after the law (relative to before the law).

Principal Findings. Overall, there were no significant or consistent effects of the parity legislation. Descriptive statistics showed significant changes in some (but not all) outcome variables, but these results disappeared in detailed statistical analyses by controlling for important covariates.

Conclusions. The null findings of the effects of state mental health parity mandates suggest that under ERISA (Employee Retirement Income Security Act), the scope of state parity legislation may have been restricted because of large proportion of self-insured employers. Furthermore, comprehensiveness of state legislation appears to be related to the traditional level of use of mental health specialty care, which becomes another confounder for the potential policy effects.

Key Words. Mental health parity legislation, health care access, insurance coverage, mental health specialty care

States have in recent years taken a more prominent role in social policy, including health care and welfare programs that were previously administered at the federal level (the so called “new federalism”). In the arena of health care

policy, new federalism often takes the form of health insurance mandates. While some applaud the increased activism of states in health policy, others criticize state legislation as creating an impenetrable jungle of regulations that increases health care costs, possibly causing employers (usually of small firms) to drop insurance benefits and therefore increasing the uninsurance rate. Therefore the overall question of policy interest is whether state legislation can make a substantial difference at the population level and serve as a substitute for federal legislation. This study focuses on one area that has been prominent in the past decade: mental health benefits.

Traditionally, insurance benefits for mental health care have been more restrictive than benefits for medical and surgical services. The 1996 federal Mental Health Parity Act (MHPA; see, e.g., <http://www.cms.hhs.gov/hipaa/hipaa1>, for a summary of the legislation and the statutory text) was an attempt to address this discrepancy and prohibited differential dollar limits for mental health and medical care in employer-sponsored insurance plans, but allowed differential limits in terms of hospital days and outpatient visits as well as differential cost-sharing features such as copayments, coinsurance, or deductibles. Thus, this legislation resulted in virtually no substantial changes in consumers' health benefits, their access to mental health care, or health care costs related to the parity bill (General Accounting Office 2000). However, the federal legislation may have had an important symbolic value and encouraged many states to follow up with stronger mandates. By 2001, 31 states passed some form of parity legislation (National Advisory Mental Health Council 2001; Gitterman et al. 2001). In this article, we study the question to what extent recent state parity legislation changed perceived health insurance benefits, perceived access to care, and use of mental health specialty care, using survey data from 1998 and 2001.

Preliminary evaluations of early state parity legislation suggested that at least the short-term effects were minimal or nonexistent (Sturm 2000; Pacula and Sturm 2000). The findings were criticized for their focus on individuals of all types of insurance status, not just those in plans that are subject to changes as a consequence of the legislation, the small number of (possibly atypical)

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states that quickly adopted mental health legislation, and the related argument that legislation may have been a consequence, rather than a cause, of differences in the use of mental health care across states (Zuvekas 2000; Sturm and Pacula 1999). Overall, these limitations cast doubt on the validity of any cross-sectional evaluation and suggest the need for investigating the pre-post changes across states. Since 1998, 15 more states passed new parity laws and other states either expanded initial legislation (for example, Connecticut and Missouri), or enacted statewide parity after a pilot program among state employees (Indiana, Massachusetts, North Carolina, South Carolina, and Texas) (Gitterman et al. 2001; National Advisory Mental Health Council 2001, Appendix E). It is now possible to distinguish between stronger and weaker legislation in terms of comprehensiveness of the mandate. Furthermore, having a larger number of states now subject to parity legislation increases the statistical power and enhances the generalizability of the results (i.e., it reduces the probability that factors particular to an individual state affect the conclusions about the effect of the policy).

DATA

The data analyzed here come from two waves of the Health Care for Communities (HCC) household survey, a component of the Robert Wood Johnson Foundation's Tracking Initiative, which follows up on participants in the Community Tracking Study (CTS). Wave 1 of HCC was fielded in 1997/1998 and reinterviewed 9,585 CTS participants (64 percent response rate). Wave 2 was fielded in 2000/2001 and included two separate components: a longitudinal component that reinterviewed 6,659 respondents from HCC Wave 1 (70 percent response rate), and a second component that reinterviewed 5,499 participants from a new cross-sectional sample of CTS (59 percent response rate). Combining the two waves of data gives a sample size of 21,743 interviews, with interviewed individuals residing in 48 states and the District of Columbia (there were no respondents from Vermont or Hawaii). The study designs of HCC have been described in prior publications (Sturm et al. 1999; Kemper et al. 1996) and detailed documentation and data are available through the Inter-University Consortium for Political and Social Research at the University of Michigan (<http://www.icpsr.umich.edu>).

Since state mental health parity legislation only applies to private insurance plans (group insurance or individual plans, or both), we restrict all our analyses to the adult population that are covered by either employer-provided

insurance or self-bought insurance, but these individuals may have other types of coverage in addition to the two types of private insurance. By only considering privately insured individuals, we address one of the criticisms of the earlier evaluations, namely that the inclusion of individuals with public insurance and the uninsured inappropriately dilutes the effects of parity legislation (Zuvekas 2000). Yet it is not clear that the restriction adopted in the current study provides a “better” estimate of the effects of state legislation because parity legislation may price some small employers out of the market (Jensen and Morrissey 1999; Jensen and Gabel 1992). However, we do not expect these indirect effects to be very important and in preliminary tests did not find any association between change in insurance status and mental health parity legislation. Nevertheless, to the extent that parity mandates cause a shift from private insurance to no-insurance or public insurance coverage that are not affected by parity legislation, our analysis would overstate increases in perceived insurance benefits, perceived access, or utilization associated with parity legislation. A second potential criticism regarding the study sample is that state legislation may not apply to self-insured employer plans and one should therefore only study plans that are subject to state legislation (Zuvekas 2000). Unfortunately, one risks making an evaluation tautological by studying only positive responders: Employer decisions about legal arrangements of the health benefits they offer are directly affected by state mandates and therefore an outcome of legislation. So there is a reason to study all employer-sponsored plans.

The effective date of mental health parity legislation in some states preceded the HCC study. In some other states, mandates were adopted while HCC Wave 1 was in the field. In order to identify the policy effects of state mental health parity legislation, we choose to focus on parity mandates that took effect in year 1999 and 2000, dropping individuals who were either residing in states that adopted parity prior to 1999 or interviewed in HCC Wave 2 in the year of 2000. We also drop cases from Massachusetts, the only state that adopted parity in 2001. Thus we have a longitudinal analysis sample in which the “before” cases were interviews conducted in HCC Wave 1 (from September 1997 to December 1998), and the “after” cases were HCC Wave 2 interviews conducted between January 2001 and January 2002. The policy effects we identify through this analysis sample are thus the effects of more recent state mental parity mandates. The size of the analysis sample is 8,057, with 4,989 coming from HCC Wave 1, and 3,068 from HCC Wave 2. Of the 3,068 interviews from HCC Wave 2, 2,891 were follow-ups from HCC Wave 1, and the remaining 2,177 were sampled based on a new cross section of CTS

Wave 2. The two components of HCC Wave 2—the follow-up interviews from HCC Wave 1 and the new cross-section sample—are quite similar in terms of the distribution of individuals across different states. Analyses in the study will take into account the sample design of the two waves of HCC.

Dependent Variables

We investigate changes in two sets of variables as a function of state parity. The first set indicates respondents' perception of changes in insurance coverage quality and access to care, based on responses to the questions:

- “Compared to two years ago, is your health insurance coverage now better, worse, or about the same?”
- “Compared to two years ago, is it easier, harder, or about the same, to get good health care when you need it?”

The second set studies utilization of mental health specialty care in the 12 months before the interview: any mental health specialty visits and the number of visits by users. Mental health specialty visits were defined as visits to a mental health provider, such as a psychiatrist, psychologist, social worker, psychiatric nurse, or counselor for emotional or mental health problems. We choose not to study inpatient mental health care because the sample size for such rare events is too small.

Main Explanatory Variable

The primary explanatory variable is whether the respondent lives in a state in which some form of mental health parity legislation took effect in the year of 1999 or 2000. Parity statute is further categorized into “strong” versus “medium” parity according to the comprehensiveness of the legislation. Specifically, strong state parities are those that require equality in all cost-sharing dimensions and allow no exemptions, while medium parity laws, though comprehensive in coverage, allow exemptions for small employers, exemptions for employers that experience cost increase due to the mandate, or contain “if offered” provisions. In this study, we group “weak” parity states with the “no parity” states. “Weak” parity states passed parity laws of mandated offering rather than a benefit mandate, which only affects insurance plans, not employers. The “no parity” states either have no parity laws or passed legislation matching the federal MHPA. One earlier study (Gitterman et al. 2001) did not find statistically significant difference between the “weak parity” states and the “no parity” states in the percent of employees with same

Table 1: States Included in the Study by the Comprehensiveness of State Mental Health Parity Legislation

<i>Strong Parity States</i>	California, Connecticut, Delaware, Montana
<i>Medium Parity States</i>	Indiana, Kentucky, Louisiana, Missouri, Nebraska, Nevada, New Mexico, Oklahoma, Pennsylvania, Tennessee, Texas, Virginia
<i>No/weak Parity States</i>	Alabama, Alaska, Arizona, District of Columbia, Florida, Georgia, Idaho, Illinois, Iowa, Kansas, Michigan, Mississippi, New York, North Carolina, Ohio, Oregon, South Carolina, Utah, Washington, West Virginia, Wisconsin

Note: States are included in the study if their parity legislation took effect in year 1999 or 2000, or if they had no parity mandates or only "weak parity," as defined in the Data section, as of the end of 2001.

deductible, or equal copay/coinsurance for mental health coverage as for medical or surgical coverage. Nor did that study find the probability of having any inpatient or outpatient visit limit to be significantly different between the two types of states. Table 1 is a list of states with residents interviewed in HCC and included in the current analysis sample by parity status. (For details on state mental health parity provisions and date enacted/became effective, see Table 1 in Gitterman et al. 2001 or Appendix E in National Advisory Mental Health Council 2001).

Other Explanatory Variables

An important advantage of the HCC survey is that it has independent measures of mental health status, which allows us to compare effects of the legislation on individuals with clinical need versus effects on the population without identified need of mental health care. The first measure of mental health is an indicator of a likely mental disorder. The screening version of the Composite International Diagnostic Interview (CIDI-SF) was used for major depressive, dysthymic, and generalized anxiety disorder (Kessler et al. 1998). For panic disorder, the CIDI stem items were complemented by requiring a limitation in role functioning on the SF-12 to reduce the number of false positives. The CIDI stem item for lifetime manic symptoms was used for bipolar disorder. Psychotic disorder was determined if the respondent ever had an overnight stay for psychotic symptoms, or ever received a diagnosis of schizophrenia from a doctor. An individual is determined to be at risk of "any mental health disorder" if he or she met any of these screening criteria. A second measure is the 5-item Mental Health Inventory or MHI-5, a psychological distress scale based on the five items that best predict a summary score

from the longer 38-item Mental Health Inventory. The MHI-5 assesses general mood or affect, including depression, anxiety, and positive well-being in the last month (Wells et al. 1996). The index runs from 0 to 100, with lower score indicating greater psychological distress (worse mental health).

Other variables we control for in the analyses include sociodemographic information of the individual (age, gender, racial and ethnic group, and education), physical health of the individual (number of chronic medical conditions), and whether the individual was covered by Medicare or Medicaid in addition to her private insurance coverage. A dummy variable for self-bought insurance is also included to control for the fixed effect of nongroup private insurance relative to group plans.

METHODS

In order to identify the effects of the parity mandates on changes in perceived quality of health insurance coverage, perceived health care access, and mental health care utilization by the targeted population of the policy, we adopt a difference-in-difference-in-difference (DDD) approach. (For an example of using the DDD approach to estimate the effects of a particular public policy, see Gruber 1994.) Specifically, we examine changes in the dependent variables by the group with mental disorders (relative to those without) in states with parity legislation (relative to the nonparity states) in the years after (relative to before) the laws took effect. The assumption we rely on is that, in the absence of the parity legislation, trend in a particular outcome for the group with mental disorders relative to that for the group without disorders would be the same across states of different parity status. If we denote the outcome as y , conceptually, the DDD statistics is:

$$\begin{aligned} & \left\{ (y^{w/Mh\ Disorder} - y^{w/o\ Mh\ Disorder})_{After\ Parity} \right. \\ & \quad \left. - (y^{w/Mh\ Disorder} - y^{w/o\ Mh\ Disorder})_{Before\ Parity} \right\}^{Parity\ States} \\ & - \left\{ (y^{w/Mh\ Disorder} - y^{w/o\ Mh\ Disorder})_{After\ Parity} \right. \\ & \quad \left. - (y^{w/Mh\ Disorder} - y^{w/o\ Mh\ Disorder})_{Before\ Parity} \right\}^{Nonparity\ States} \end{aligned}$$

We first calculate descriptive DDD statistics on perceived changes in insurance generosity/access to health care and use of mental health specialty care, without any further adjustments for covariates. For perceived health insurance

benefits and perceived health care access, we generate dummy variables corresponding to each ordered outcome. For each outcome of interest, we first calculate DDD statistics for parity versus nonparity, and then calculate DDD statistics for strong parity/medium parity versus no parity, respectively. Statistics are weighted to be nationally representative, with weights based on the inverse of the probability of selection, nonresponse, and nontelephone households.

We further conduct regression analyses within the DDD framework, pooling the two waves of data. We fit an ordered probit model to the first two outcomes of interest: whether insurance coverage was perceived to be worse, same, or better compared to two years earlier, and, whether access to health care was perceived to be harder, same, or easier compared to two years earlier. If we number the ordered outcome (from the worst to the best) to be 0, 1, and 2, the ordered-probit model is as follows:

$$\begin{aligned}P(y_i = 0) &= \Phi(-\tau'x_i), \\P(y_i = 1) &= \Phi(\mu - \tau'x_i) - \Phi(-\tau'x_i), \\P(y_i = 2) &= 1 - \Phi(\mu - \tau'x_i),\end{aligned}$$

where μ is the unknown parameter pertaining to the data-generating process of the ordered outcome and is to be estimated with the other parameters, and Φ is the cumulative density function of the standard normal distribution. $\tau'x_i$ stands for

$$\begin{aligned}&\lambda_0 + \lambda_1(Par_i * yr01_i * MhDx_i) + \lambda_2(Par_i * yr01_i) + \lambda_3(yr01_i * MhDx_i) \\&+ \lambda_4(Par_i * MhDx_i) + \lambda_5Par_i + \lambda_6yr01_i + \lambda_7MhDx_i + X_i'\phi\end{aligned}$$

The variable *Par* in the equations above is the indicator of states that adopted the parity policy in year 1999 or 2000. We may call it the indicator of the “eventual parity status” of the states. By using the eventual parity status, rather than state dummies, to control for time-invariant differences across states (or state fixed effects), we have made the assumption that states with the same eventual parity status share the same state fixed effects. *MhDx*, a dummy variable for “any mental health disorder” (the “intervention group”), controls for the intrinsic difference of the intervention group versus the non-intervention group. Of the three second-order interactions, *Par*yr01* controls for the specific time trend for the parity states (relative to the nonparity states), *yr01*MhDx* controls for the specific time trend for the intervention group (relative to the nonintervention group), and *Par*MhDx* controls for differential

outcome of the intervention group relative to the nonintervention group in the parity states relative to the nonparity states.

The product of eventual parity status, mental health disorder, and the year dummy for 2001 is the term of interest. In other words, λ_1 is the DDD estimate for the policy effect of parity, because it stands for the differential relative trend between the intervention group and the nonintervention group in parity states versus the nonparity states, which we attribute to the policy of state mental parity legislation. We will use these estimates to calculate the counterfactual: what would outcomes for people with mental disorders be if their states adopted parity legislation versus if their states did not adopt the legislation.

We use the zero-inflated negative binomial model (ZINB) for the utilization of mental health specialty care (Lambert 1992; Greene 1994). When modeling health care incidences that contain a significant portion of zeros, ZINB has the advantage (relative to the traditional two-part model [Duan et al. 1983]) that it explicitly takes into account the two processes that generate the zero outcomes: (1) individuals may never use the health service; (2) individuals have the potential to use the service, yet didn't use any during the recall period of the study. Given that mental health specialty care is much more rare than general medical care, accounting for zero-probability of service use becomes even more important. Empirically, the ZINB has been shown to outperform the two-part model in the context of mental health specialty care in terms of out-of-sample prediction (Bao 2002). We base our estimates on a ZINB model, but also estimate a traditional two-part model as a specification check.

The ZINB model looks as follows:

$$\begin{aligned}\Pr(\text{Visit}_i = 0) &= \frac{\exp(Z'_i \gamma_Z)}{1 + \exp(Z'_i \gamma_Z)} \\ &\quad + \frac{1}{1 + \exp(Z'_i \gamma_Z)} f(\text{Visit}_i = 0 | x_i, \beta_Z), \\ \Pr(\text{Visit}_i = j) &= \frac{1}{1 + \exp(Z'_i \gamma_Z)} \\ &\quad f(\text{Visit}_i = j | x_i, \beta_Z) \quad \forall j > 0,\end{aligned}$$

where f is the probability density function of the negative binomial distribution. While the probability of never having any visit follows the logistic process, the count of mental health specialty visits by potential users is assumed to follow a negative binomial distribution, parameterized by $x'_i \beta_Z$. Independent

variables, as denoted by x_i , are the same as in the specifications for the first two outcomes. The parameterization of the logistic process (Z_i), on the other hand, only contains individual characteristics, because factors other than personal need for mental health care (for example, public policy or secular trend) should not play a role in determining whether the individual would never use the service.

For each regression, we also estimate a similar model that distinguishes between different parity comprehensiveness. In particular, there are two third-order interactions (one for “strong,” one for “medium” parity), two more second-order interactions, and one more first-order control in the model. For all regression analyses, standard errors of coefficients are adjusted by taking into account the fact that some of the interviews were from the same individuals ($n = 891$).

Estimated coefficients of the models are not directly interpretable. We derive predicted outcomes (probability of having worse or better perceived insurance coverage than two years earlier, probability of having harder or easier perceived access to health care than two years earlier, and number of mental health specialty visits in the past 12 months) for individuals with probable mental health disorders in states with eventual parity status. In particular, we conduct the prediction on the post-parity subsample, and calculate mean predicted outcomes conditional on the presence and absence of the parity mandates, respectively. (We derive the predictions by first setting the cross product of eventual parity status, mental health disorder, and the year 2001 dummy to 0, and then to 1.) By doing so, we compare the outcomes of interest of parity legislation to what they would have been had there not been any parity legislation.

RESULTS

Table 2 shows the weighted means of the outcomes by the presence of any mental disorder and by states' parity status, both in HCC Wave 1 (pre-parity) and in HCC Wave 2 (post-parity). In general, the mentally ill population were slightly more likely to perceive a positive change, but also much more likely to perceive a negative change, in the quality of their insurance coverage and access to care, and have much higher utilization rate of mental health outpatient specialty care when compared to individuals without probable mental disorders. Comparison between the parity states and the nonparity states indicate no clear pattern for the two perceived outcomes. However, the parity

Table 2: Weighted Mean Outcomes by Parity Status and Presence of Probable Mental Disorders, HCC Wave 1 and 2

<i>Outcomes</i>	<i>Before Parity (HCC Wave 1)</i>		<i>After Parity (HCC Wave 2)</i>	
	<i>No Mental Disorder</i>	<i>Any Mental Disorder</i>	<i>No Mental Disorder</i>	<i>Any Mental Disorder</i>
<i>Perceiving insurance to be better (%)</i>				
Parity states	19.2	21.1	14.5	14.7
Nonparity states	18.7	21.0	16.1	17.5
<i>Perceiving insurance to be worse (%)</i>				
Parity states	8.3	16.0	11.4	14.4
Nonparity states	7.8	12.6	11.7	25.3
<i>Perceiving access to be easier (%)</i>				
Parity states	13.0	13.9	12.1	18.6
Nonparity states	12.7	15.0	11.5	11.2
<i>Perceiving access to be harder (%)</i>				
Parity states	14.3	24.2	10.1	21.6
Nonparity states	12.6	20.9	9.1	27.2
<i>Any mental health specialty visits (%)</i>				
Parity states	2.7	25.6	2.1	17.7
Nonparity states	2.2	22.0	1.3	15.3
<i>Number of mental health specialty visits, if any</i>				
Parity states	13.9	15.6	10.9	17.6
Nonparity states	9.6	12.6	10.1	12.6
<i>Unweighted sample size</i>				
Parity states	1,529	346	1,013	190
Nonparity states	2,496	613	1,536	277

Note: The total sample size is slightly lower than what is reported in the Data section because of missing values for probable mental disorder for some individuals.

states are shown to have a higher rate of specialty care use than the nonparity states both before and after parity.

Table 3 shows descriptive DDD statistics for the parity versus no-parity analysis and the analysis of strong/medium parity versus no-parity. All the statistics indicate positive effects of state parity legislation on perceived mental health coverage, access to care, and utilization of mental health specialty care, but not all of them are statistically significant. When we compare parity to no-parity (i.e., without distinguishing different comprehensiveness of parity), the only significant result is the reduction in percentage of individuals who perceived their insurance coverage to be worse than two years earlier. The DDD estimate is -13.5 percent ($p < 0.05$). When we distinguish between strong and medium parity, and contrast each of the two groups to no-parity, we saw a few

Table 3: Difference-in-Difference-in-Difference Statistics

	<i>Parity vs. No/Weak Parity</i>	<i>Strong Parity vs. No/Weak Parity</i>	<i>Medium Parity vs. No/Weak Parity</i>
Perceiving insurance to be better (%)	− 0.9 (6.3)	2.1 (5.9)	− 3.4 (5.9)
Perceiving insurance to be worse (%)	− 13.5** (6.3)	− 22.7*** (5.3)	− 4.5 (5.3)
Perceiving access to be easier (%)	8.1 (6.1)	12.6* (7.2)	5.4 (7.2)
Perceiving access to be harder (%)	− 8.2 (7.8)	− 15.4*** (6.0)	− 1.4 (6.0)
Any mental health specialty visit (%)	− 1.5 (5.1)	− 4.5 (4.6)	− 2.7 (4.6)
Number of positive mental health specialty visits	5.4 (6.0)	8.9* (4.9)	5.3 (4.9)

Notes: Statistics are weighted to be nationally representative. Standard errors are in parentheses. Statistically significance with $p < 0.01$, $p < 0.05$, and $p < 0.10$ is indicated by ***, **, and *, respectively.

more significant results, all with the strong versus no parity comparison. The DDD estimate for percentage of individuals reporting “worse (perceived) coverage” is -22.7 percent ($p < 0.01$) when we compare the effect of strong parity to that of no-parity. Also, strong parity is associated with less report of “harder access to health care” (a 15.4-percentage-point reduction; $p < 0.01$). Finally, when we look at number of positive mental specialty visits during one year, the DDD estimate for strong- versus no-parity comparison is 4.9 ($p < 0.1$), which is more than one-third of the baseline rate of the group with any mental disorder.

The results of the regression analyses are presented in the form of conditional predictions of the outcomes for those with probable mental disorders residing in a parity state after the legislation took effect (Table 4).

Controlling for individual level covariates, the regression analysis shows that parity laws have had little, if any, impact on perceived quality change in insurance coverage, perceived change in access to care, or mental health specialty care of individuals with mental disorders relative to those without disorders. Although estimated coefficients in the analysis are in the direction that parity improves perceived coverage, access, or mental health specialty care, none is statistically significant at the 5 percent level. The predicted mean visits based on a two-part model are 5.0 and 6.1 under no/weak parity and under parity, respectively, which suggests even smaller effect of parity both in

Table 4: Predicted Effects of State Mental Health Parity Legislation for Persons with Probable Mental Disorders

<i>Parity vs. No/Weak Parity</i>	<i>No/Weak Parity</i>		<i>Parity</i>	
Perceiving insurance to be better (%)	14.6		15.0	
	(3.3)		(2.4)	
Perceiving insurance to be worse (%)	13.8		13.5	
	(3.1)		(2.4)	
Perceiving access to be easier (%)	10.6		13.2	
	(2.5)		(2.5)	
Perceiving access to be harder (%)	17.0		13.8	
	(3.3)		(2.5)	
Number of mental health specialty visits	3.6		5.7	
	(1.6)		(1.2)	
<i>Strong/Medium Parity vs. No/Weak Parity</i>				
	<i>Strong vs. No/Weak Parity</i>		<i>Medium vs. No/Weak Parity</i>	
	<i>No/Weak Parity</i>	<i>Strong Parity</i>	<i>No/Weak Parity</i>	<i>Medium Parity</i>
Perceiving insurance to be better (%)	10.7	13.2	18.8	16.5
	(3.6)	(2.8)	(4.1)	(3.9)
Perceiving insurance to be worse (%)	18.1	15.0	10.6	12.2
	(4.9)	(3.0)	(2.9)	(3.4)
Perceiving access to be easier (%)	8.3	11.4	12.9	14.7
	(2.7)	(2.8)	(3.3)	(3.4)
Perceiving access to be harder (%)	20.3	15.5	14.3	12.5
	(4.9)	(3.5)	(3.5)	(3.4)
Number of mental health specialty visits	4.7	7.1	1.6	4.4
	(2.2)	(2.0)	(1.4)	(1.3)

Notes: Numbers are weighted means of predictions based on the estimated models for individuals with probable mental disorder in states with strong or medium parity legislation in the post-parity year. Bootstrapped standard errors are in parentheses.

absolute and relative terms. The analysis did show that, for individuals with probable mental health disorders who resided in states with medium parity legislation, parity legislation increased use of mental health specialty care by 2–3 annual visits on average ($p = 0.067$). Also, as shown in the second panel in Table 4, strong parity legislation seemed to have a larger effect (both in absolute and relative terms) on perceived insurance coverage quality and access to needed health care than medium parity.

It is worth noticing that the controls for eventual parity status (or state fixed effects by eventual parity status) in some of the utilization models are

large in magnitude and statistically significant. For example, the eventual parity dummy in the ZINB model for mental specialty visits is positive and has a p -value of .023, suggesting traditionally higher utilization among the states that passed parity laws in year 1999 or 2000. Further, results that distinguish between strong and medium parity indicate that while strong parity states traditionally had much higher utilization of mental specialty care than states with no or only weak parity (the magnitude of the coefficient of eventual strong parity status is even greater than that of mental disorder), the same is not true for states that enacted medium parity recently.

DISCUSSION

Mandating mental health benefits is an ongoing policy process. The profile of state parity legislation has changed dramatically since the first states mandated equal coverage. In fact, in the few years since 1998, more than twice as many states enacted legislation and the mandates are typically more comprehensive than the earlier ones. While President Bush called for improved mental health benefits, Congress has not yet followed up with stronger federal legislation. Thus, one of the salient questions is whether state legislation can serve as a substitute for federal legislation. While a conclusive answer to this question is beyond the scope of this study, we provide an updated assessment of the policy effects of state mental health parity legislation on perceived quality of health insurance coverage, perceived access to needed health care, and use of mental health specialty care by individuals with probable mental health disorders—presumably the primary beneficiaries of the legislation. Using longitudinal data and differentiating between comprehensiveness of parity legislation does not alter the basic conclusions reached by earlier studies using cross-sectional data: overall, there is no significant or consistent effect of the parity legislation. Unadjusted statistics (i.e., not controlling for differences in sociodemographics, mental and physical health conditions, and additional insurance status) show significant changes in some (but not all) dependent variables, but these results disappeared in a detailed statistical analysis by controlling for important covariates.

One likely explanation for the lack of significant policy effects is that state legislation simply did not reach enough individuals to make a noticeable difference at the population level. The primary constraint is likely to be the fact that state mandates are not binding for self-insured employers under ERISA (Employee Retirement Income Security Act). In the year of 1998, it was

estimated that 50 percent of insured workers were enrolled in self-insured plans (Gabel et al. 1999). We are not able to tell self-insured plans from the other private insurance in our data and therefore cannot analyze this issue further.

Another possible explanation for the lack of effects found is that many consumers in parity states might not be aware of their improved coverage. This would imply that rather than giving up on parity laws, states should do more to publicize such laws once passed.

A third explanation for the lack of significant impact is that parity legislation may have accelerated the development of managed care in the mental health care arena, especially the proliferation of behavioral carve-outs, which separates nominal benefits from actual benefits (Hennessy and Goldman 2001; National Advisory Mental Health Council 1998). This wedge between nominal and actual benefits may be responsible for the perception of constant access to care or the absence of changes in utilization even if financial benefits increase. However, this interpretation is somewhat inconsistent with the empirical evidence that introducing carve-outs in private plans generally resulted in increased rate of any mental health services, but lower intensity of services (Sturm 1999). Our null findings about perceived changes in generosity also suggest that the more likely explanation is the absence of meaningful changes in benefit design, rather than an increased role of managed care.

Finally, the political economy of legislation provides another potential explanation: the passage of mental health parity legislation reflects the balance of power among interest groups within a state. If employers do not expect parity to substantially affect health care costs in their state, they would be less likely to oppose legislation, but this type of selection would suggest smaller observed effects of legislation passed. Earlier studies found that states that appeared to have lower rates of service use were more likely to pass *some* parity legislation by 1999 (Sturm and Pacula 1999), but we now find the opposite for recent adopters of strong parity. If policies are determined by the relative strength of interest groups within a state, it could be an important confounder of the potential effects of state legislation (even if the results correctly reflect the actual effects of legislation).

In summary, our study does not provide evidence that the recent state mental health parity legislation has had any significant effects on perceived quality of insurance coverage, perceived access to care, or specialty care utilization for individuals with likely need for mental health care. The findings suggest that state legislation is unlikely to be an effective substitute for strong federal legislation, but limitations of the study preclude a conclusive answer.

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