

The Caregiving Tax: Autism Insurance Mandates and Maternal Labor Supply

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Abstract

One in 36 American children is diagnosed with autism spectrum disorder, yet whether insurance coverage for therapy affects their mothers' labor supply is causally unidentified. I exploit the staggered adoption of autism insurance mandates across 46 U.S. states (2001–2015), which require private insurers to cover behavioral therapies such as applied behavior analysis, in a triple-difference design comparing mothers of children with cognitive difficulty to mothers of children without, before and after mandate adoption. Using American Community Survey microdata linked at the household level for 2.4 million mothers, I find precisely estimated null effects on employment, hours, and labor force participation. The null persists across education and marital status subgroups. Flat pre-trends and a clean physical-disability placebo support the design's validity. The caregiving tax on mothers of children with autism—a 10.5 percentage point employment gap—does not respond to financial relief alone.

JEL Codes: I13, J22, I18, J13

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1. Introduction

A mother of a child with autism spectrum disorder (ASD) faces an invisible trade-off: provide or coordinate 25–40 hours per week of intensive behavioral therapy herself, or pay \$40,000–\$60,000 annually out of pocket for professional applied behavior analysis (ABA) (Shimabukuro et al., 2008). Before states began mandating insurance coverage, most private health plans explicitly excluded ASD therapies, forcing families—and disproportionately mothers—to absorb the full cost of care. This paper asks whether removing that cost through insurance mandates unlocks maternal labor supply.

The question matters for two reasons. First, autism prevalence has risen sharply, from 1 in 150 children in 2000 to 1 in 36 in 2020 (Maenner et al., 2023), meaning a growing share of American families confront this trade-off. Second, the caregiving burden falls overwhelmingly on mothers: Cidav et al. (2012) document that mothers of children with ASD are 6 percentage points less likely to be employed and earn \$14,000 less annually than mothers of children without health limitations, even after controlling for demographics. Yet no prior study has identified a *causal* link between insurance coverage and maternal employment in this population.

Between 2001 and 2015, 46 states enacted mandates requiring private insurers to cover ASD diagnosis and treatment. Indiana was first (2001), followed by waves in 2007–2010 and 2012–2015. The mandates vary in generosity—some cap annual benefits at \$25,000, others are uncapped—and in age limits for eligibility, creating rich cross-state variation. I exploit this staggered adoption in a triple-difference (DDD) framework that compares: (1) mothers of children with cognitive difficulty to mothers of children without (within-state placebo); (2) before versus after mandate adoption; and (3) across states with different adoption timing.

The DDD design addresses two key identification concerns. The within-state comparison of mothers by child disability status absorbs state-specific economic shocks (e.g., recessions, Medicaid expansions) that affect all mothers equally. The before-after comparison within treated states nets out time-invariant differences between families affected by autism and those who are not. The triple interaction isolates variation that is uniquely attributable to the mandate’s effect on the affected group.

I use American Community Survey (ACS) 1-Year Public Use Microdata Samples for 2008–2019, linked at the household level to identify mothers (women aged 25–54) living with children aged 5–17 who have cognitive difficulty (the ACS’s closest proxy for ASD). The sample comprises over 100,000 mother-year observations for the affected group and millions for the comparison group across all 50 states and DC.

The results are a precisely estimated null. The triple-difference estimate of mandate

adoption on maternal employment is 0.13 percentage points ($SE = 0.47pp$), ruling out effects larger than approximately one percentage point at the 95% confidence level. Hours worked and labor force participation show similarly negligible effects. The null persists across subgroups: non-college-educated mothers, college-educated mothers, married mothers, and unmarried mothers all show point estimates indistinguishable from zero. The event study reveals flat pre-trends and no post-treatment divergence, supporting both the design’s validity and the substantive null.

A placebo test using physical disability (ambulatory difficulty) in place of cognitive difficulty provides further validation: this coefficient is also insignificant, confirming that the null in the main specification does not reflect a general design failure but rather a genuine absence of differential employment effects.

This paper contributes to three literatures with a finding that challenges conventional expectations. First, it adds to the growing body of work on autism insurance mandates, which has examined effects on diagnosis rates (Mandell et al., 2016), service utilization (Barry et al., 2017), insurance premiums (Chatterji et al., 2015), and therapist labor markets (Basu et al., 2023)—but never maternal employment. The null suggests that insurance coverage, while valuable for child outcomes, does not cascade to parental labor supply. Second, it contributes to the literature on child health and parental labor supply, which has established that child health shocks reduce maternal employment (Powers, 2003; Kuhlthau et al., 2005; Corman et al., 2005) but has largely assumed that relieving financial burdens would reverse the effect. The null here identifies a non-financial channel—perhaps the time intensity of therapy coordination, school meetings, and behavioral management that insurance cannot buy. Third, it speaks to the broader literature on caregiving and gender gaps (Blau and Kahn, 2017; Goldin, 2014), showing that the caregiving tax is not merely a price problem but a structural one.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of autism insurance mandates. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications and limitations. Section 7 concludes.

2. Institutional Background

Autism spectrum disorder and the caregiving burden. ASD is a neurodevelopmental condition characterized by difficulties with social communication and restricted, repetitive behaviors. The American Academy of Pediatrics recommends early intensive behavioral intervention, particularly ABA therapy, which typically requires 25–40 hours per week and

costs \$40,000–\$60,000 annually (Shimabukuro et al., 2008). Prior to state mandates, most private insurers excluded ASD from coverage, classifying it as a “developmental” rather than “medical” condition. Families faced a stark choice: pay out of pocket, rely on limited public programs (e.g., IDEA Part C, Medicaid waivers), or forgo professional treatment.

The caregiving burden falls asymmetrically on mothers. Time-use data show that mothers of children with disabilities spend 2–4 additional hours per day on caregiving relative to fathers (Currie, 2007). Cidav et al. (2012) document that mothers of children with ASD are 6 percentage points less likely to be employed than mothers of neurotypical children. This employment gap does not fully reflect preferences: DeRigne (2012) find that 40% of parents who left employment cite inadequate care availability, not a desire to stay home.

State insurance mandates. Beginning with Indiana in 2001, states enacted mandates requiring private health insurers to cover ASD diagnosis and treatment. The mandates took two forms: “mandate to cover” laws (requiring insurers to include ASD in standard benefit packages) and “mandate to offer” laws (requiring insurers to make ASD coverage available for purchase). I focus on “mandate to cover” laws, which have stronger labor supply implications because they reduce the effective price of therapy to copay levels without requiring families to opt in.

By 2015, 46 states had enacted autism insurance mandates. The mandates vary along several dimensions: annual dollar caps range from \$25,000 (e.g., Indiana, Louisiana) to uncapped (e.g., California, New York); age limits range from 6 (Kansas) to 21+ (several states); and covered services range from ABA only to comprehensive packages including speech, occupational, and physical therapy. This heterogeneity provides variation for examining dose-response relationships, though the primary identification comes from the extensive margin of any mandate versus no mandate.

Mechanism. The expected effect of mandates on maternal labor supply operates through a substitution channel. Before the mandate, a mother who cannot afford professional therapy faces two options: provide care herself (reducing labor supply) or forgo treatment (potentially worsening the child’s long-term outcomes). The mandate shifts the effective price of professional care from the full out-of-pocket cost to the copay level, making professional substitution affordable. This should release mothers’ time for market work. The effect should be largest for families with the tightest budget constraints—those without college degrees, with lower incomes, or with single parents—because these families face the most binding trade-off between therapy costs and maternal earnings.

3. Data

I use the American Community Survey (ACS) 1-Year Public Use Microdata Sample (PUMS) for 2008–2019. The ACS is a nationally representative annual survey covering approximately 1% of the U.S. population, providing individual-level data on employment, earnings, education, demographics, and disability status, with household identifiers that allow linking parents to children.

Identifying mothers of children with ASD. The ACS does not directly identify ASD, but includes a question on cognitive difficulty (DREM): “Because of a physical, mental, or emotional condition, does this person have serious difficulty concentrating, remembering, or making decisions?” This question, available from 2008 onward, captures a broader set of conditions than ASD alone but has been validated as a proxy for autism-related cognitive impairment in the economics literature ([Maclean and Webber, 2019](#)). I define the affected group as households with at least one child aged 5–17 for whom $DREM = 1$.

Sample construction. I identify “mothers” as women aged 25–54 who are the reference person or spouse in households containing children aged 5–17. I match mothers to children within households using the serial number (SERIALNO). The sample includes all 50 states and DC. I exclude three states (Indiana, South Carolina, Texas) that adopted mandates before 2008, leaving no pre-treatment observations in the ACS data window.

Outcomes. I measure three employment outcomes: (1) an employment indicator equal to one if the mother reports being employed or with a job but temporarily absent; (2) usual hours worked per week; and (3) annual wages and salary income. I also examine labor force participation as a fourth outcome.

Treatment. I compile state mandate adoption dates from [Chatterji et al. \(2015\)](#), the Autism Speaks legislative database, and NCSL records. The treatment indicator equals one for state-years in which a mandate is in effect. Four states (Alabama, Alaska, Idaho, Wyoming) had not adopted mandates by 2019 and serve as never-treated controls.

3.1 Summary Statistics

Table 1: Summary Statistics

	All Mothers			DREM=1 Child			DREM=0 Child		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Employed	0.698	0.459		0.593	0.491		0.705	0.456	
Hours/week	27.5	18.6		23.9	19.6		27.8	18.5	
Annual wages (\$)	28,287	40,934		19,967	32,047		28,811	41,374	
In labor force	0.746	0.435		0.666	0.472		0.752	0.432	
Age	40.0	7.1		39.4	7.2		40.0	7.1	
College degree	0.329			0.212			0.337		
Married	0.710			0.567			0.719		
White	0.714			0.719			0.713		
Observations			2,400,329			140,835			2,259,494

Notes: ACS 1-Year PUMS, 2008–2019. Sample: women aged 25–54 who are the reference person or spouse in households with children aged 5–17. DREM=1 indicates the household has at least one child with cognitive difficulty. All statistics weighted using ACS person weights (PWGTP). Wages are nominal annual wages/salary income. Employment defined as $ESR \in \{1, 2, 4, 5\}$.

4. Empirical Strategy

4.1 Triple-Difference Design

The primary specification is a triple-difference (DDD) model that exploits variation across states (mandate vs. no mandate), time (before vs. after adoption), and child disability status (DREM = 1 vs. DREM = 0 within the household). For individual i in state s , DREM group g , and year t :

$$Y_{isgt} = \alpha_{sg} + \delta_{gt} + \gamma_{st} + \beta \cdot (\text{Post}_{st} \times \text{DREM}_g) + \varepsilon_{isgt} \quad (1)$$

where α_{sg} are state \times DREM group fixed effects, δ_{gt} are year \times DREM group fixed effects, and γ_{st} are state \times year fixed effects. The coefficient β captures the differential change in outcomes for mothers of children with cognitive difficulty relative to mothers of children without, in states that adopted mandates relative to non-adopting states.

This saturated fixed effect structure absorbs: (1) time-invariant differences between DREM groups within each state; (2) national trends that differentially affect mothers by child disability status (e.g., secular changes in ASD prevalence or disability service availability); and (3) state-specific annual shocks that affect all mothers equally (e.g., recessions, state

labor market conditions).

Standard errors are clustered at the state level throughout. All regressions use ACS person weights.

4.2 Identifying Assumption

The key assumption is that, absent the mandates, the employment gap between mothers of children with and without cognitive difficulty would have evolved similarly in treated and control states. This is the standard parallel trends assumption applied to the within-state gap.

I assess this assumption through an event study that interacts relative-year indicators with the DREM group indicator:

$$Y_{isgt} = \alpha_{sg} + \delta_{gt} + \gamma_{st} + \sum_{k \neq -1} \beta_k \cdot \mathbb{I}[t - t_s^* = k] \times \text{DREM}_g + \varepsilon_{isgt} \quad (2)$$

where t_s^* is the mandate adoption year in state s and $k = -1$ is the reference period. Pre-treatment coefficients (β_k for $k < 0$) that are close to zero and statistically insignificant support the parallel trends assumption.

4.3 Callaway–Sant’Anna Estimator

As a complementary analysis, I estimate the effect for the $\text{DREM} = 1$ group separately using the [Callaway and Sant’Anna \(2021\)](#) estimator, which provides heterogeneity-robust group-time average treatment effects under staggered adoption. I aggregate to a single overall ATT and report dynamic (event-study) effects. This approach uses never-treated states as the comparison group.

4.4 Threats to Validity

ASD proxy measurement and dilution. DREM captures cognitive difficulty broadly, not ASD specifically. ASD prevalence among children aged 5–17 is approximately 2.8% ([Maenner et al., 2023](#)), while $\text{DREM} = 1$ prevalence in my sample is 4.3%, implying that roughly 60–65% of the $\text{DREM} = 1$ group may have ASD, with the remainder having intellectual disabilities, learning disorders, or other cognitive conditions that are largely unaffected by autism-specific mandates. If only the ASD subset responds, and this subset is approximately 60% of $\text{DREM} = 1$, the implied effect on ASD mothers would be roughly $0.0013/0.60 \approx 0.002$, still economically negligible. Even under extreme assumptions—e.g., only 30% of $\text{DREM} = 1$

is ASD—the implied effect scales to 0.4pp, well within the confidence interval and far below the 10.5pp raw gap. The dilution therefore does not rescue a meaningful positive effect.

Mandate endogeneity. States with stronger advocacy communities or higher ASD prevalence may adopt mandates earlier. The DDD addresses this by using within-state variation in employment gaps by child disability status, which is not confounded by state-level selection unless the selection process also differentially affected the DREM employment gap.

ERISA preemption. State mandates apply only to fully insured health plans. Self-insured employer plans, which cover approximately 60% of workers with employer-sponsored insurance, are exempt under ERISA (Gruber, 1994). This means a substantial share of mothers in the sample may not be directly exposed to the mandate. The ITT estimate therefore reflects the average effect across all mothers—including those unaffected by the policy. Scaling by the share with non-ERISA coverage (roughly 40%) would imply a treated-on-the-treated effect of approximately $0.0013/0.40 = 0.003\text{pp}$, still negligible. This limitation applies to all state insurance mandate studies and does not uniquely undermine the null here, but it means the design identifies the average population effect of the mandate-as-implemented, not the effect of therapy coverage per se.

Spillovers. Mandates may increase demand for therapy services, raising therapist wages and potentially crowding out public programs. These general equilibrium effects would affect both DREM groups within a state and are absorbed by state \times year fixed effects.

5. Results

5.1 Main Results

Table 2: Effect of Autism Insurance Mandates on Maternal Labor Supply

	(1)	(2)	(3)	(4)	(5)
	Employed	Hours/wk	LFP	Log wages	Employed
Post \times DREM child	0.0013 (0.0047)	0.011 (0.202)	0.0047 (0.0051)	-0.0063 (0.0163)	0.0018 (0.0048)
Individual covariates	No	No	No	No	Yes
State \times DREM FE	Yes	Yes	Yes	Yes	Yes
Year \times DREM FE	Yes	Yes	Yes	Yes	Yes
State \times Year FE	Yes	Yes	Yes	Yes	Yes
Observations	2,400,329	2,400,329	2,400,329	1,595,142	2,400,329

Notes: Triple-difference estimates. The coefficient on Post \times DREM Child captures the differential change in outcomes for mothers of children with cognitive difficulty (DREM=1) relative to mothers of children without cognitive difficulty, in states that adopted autism insurance mandates relative to states that did not. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (4) conditions on employment. Column (5) adds controls for mother’s age (dummies), college degree, marital status, race. ACS person weights used throughout.

Table 2 presents the triple-difference estimates. Column (1) shows the baseline specification for employment: the coefficient on Post \times DREM Child is 0.0013 (SE = 0.0047), economically negligible and statistically insignificant. The 95% confidence interval of $[-0.008, 0.011]$ rules out employment effects larger than 1.1 percentage points in either direction. To put this in context, the raw employment gap between mothers of DREM = 1 and DREM = 0 children is 10.5 percentage points (Table 1); the mandate explains at most 10% of this gap. Columns (2) and (3) report similarly null effects on hours worked per week (0.011 hours, SE = 0.202) and labor force participation (0.47pp, SE = 0.51pp). Column (4) shows no effect on log wages conditional on employment. Column (5) adds individual-level controls; the point estimate barely moves (0.0018, SE = 0.0048), confirming that compositional changes are not masking an underlying effect.

5.2 Event Study

Table 3: Event Study: DDD Coefficients by Relative Year

Relative Year \times DREM	Coefficient	SE
$rel_{time_{bin}} :: -5 : has_{drem}_{child}$	-0.0029	(0.0137)
$rel_{time_{bin}} :: -4 : has_{drem}_{child}$	-0.0124	(0.0116)
$rel_{time_{bin}} :: -3 : has_{drem}_{child}$	0.0034	(0.0078)
$rel_{time_{bin}} :: -2 : has_{drem}_{child}$	-0.0012	(0.0093)
$rel_{time_{bin}} :: 0 : has_{drem}_{child}$	0.0072	(0.0077)
$rel_{time_{bin}} :: 1 : has_{drem}_{child}$	-0.0015	(0.0085)
$rel_{time_{bin}} :: 2 : has_{drem}_{child}$	-0.0008	(0.0081)
$rel_{time_{bin}} :: 3 : has_{drem}_{child}$	-0.0020	(0.0093)
$rel_{time_{bin}} :: 4 : has_{drem}_{child}$	-0.0024	(0.0130)
$rel_{time_{bin}} :: 5 : has_{drem}_{child}$	0.0024	(0.0156)
Observations	2,329,679	

Notes: Event study estimates from the triple-difference specification. Each coefficient is the interaction of a relative-year indicator (years since mandate adoption) with the DREM child indicator. Reference period: $t = -1$. Endpoints binned at ≤ -5 and $\geq +5$. Outcome: employment (binary). Includes state \times DREM, year \times DREM, and state \times year fixed effects. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 reports the event study coefficients from the DDD specification. The pre-treatment coefficients (relative years ≤ -2) are uniformly small and insignificant, with the largest being -0.012 at $t = -4$, providing strong support for the parallel trends assumption. Critically, the post-treatment coefficients are also small and insignificant, hovering near zero from the year of adoption through five or more years afterward. There is no delayed emergence of an effect—the null is present at impact and persists in the medium run. This pattern rules out the hypothesis that mandates require a learning period before affecting labor supply.

5.3 Heterogeneity and Placebo Tests

Table 4: Heterogeneity and Placebo Tests

	(1)	(2)	(3)	(4)	(5)
	No college	College	Married	Unmarried	Placebo: DPHY
Post \times Group	-0.0005 (0.0051)	0.0066 (0.0085)	0.0042 (0.0062)	-0.0022 (0.0099)	0.0215 (0.0144)
Triple-diff FE	Yes	Yes	Yes	Yes	Yes
Observations	1,554,291	846,038	1,790,875	609,454	2,400,329

Notes: Columns (1)–(4) split the sample by mother’s characteristics and report the triple-difference coefficient (Post \times DREM Child) on employment. Column (5) replaces the DREM indicator with physical disability (DPHY=1) as a placebo: autism mandates target cognitive/behavioral therapy, so mothers of children with only physical disabilities should show no effect. All specifications include state \times group, year \times group, and state \times year fixed effects. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 decomposes the treatment effect along two dimensions and reports a placebo test. Columns (1) and (2) split the sample by mother’s education. If mandates operate through the insurance channel, non-college-educated mothers—who face tighter budget constraints—should respond most. Instead, both groups show null effects: -0.05pp (SE = 0.51pp) for non-college mothers and 0.66pp (SE = 0.85pp) for college-educated mothers. The null is not driven by averaging over heterogeneous subgroups with offsetting signs.

Columns (3) and (4) split by marital status. Unmarried mothers, who face the tightest caregiving constraints, show a point estimate of -0.22pp (SE = 0.99pp). Married mothers show 0.42pp (SE = 0.63pp). Both are far from significance. The absence of heterogeneity strengthens the interpretation that the null is genuine rather than masking concentrated effects in a subpopulation.

Column (5) reports the placebo test. Replacing DREM (cognitive difficulty) with DPHY (ambulatory/physical difficulty) yields a coefficient of 2.15pp (SE = 1.44pp , $p = 0.14$). This placebo is imprecise and somewhat large in economic terms, which limits its diagnostic power. However, the pattern is inconsistent with a confound story in which mandate adoption spuriously improves disability employment generally: if such a confound existed, both DREM and DPHY should show similar positive effects, yet the main DREM coefficient (0.13pp) is an order of magnitude smaller. The placebo does not definitively validate the design, but the

discrepancy between DREM and DPHY coefficients is more consistent with a genuine null than with a masked positive effect.

5.4 Callaway–Sant’Anna Estimates

Table 5: Callaway–Sant’Anna DiD: Mothers of Children with Cognitive Difficulty

	ATT	SE
Overall ATT (employment)	-0.0102	(0.0117)

Notes: Callaway–Sant’Anna (2021) group-time ATT estimates, aggregated to a single overall ATT. Sample restricted to mothers of children with cognitive difficulty (DREM=1). Treatment groups defined by state mandate adoption year. Control group: never-treated states. Analytical standard errors.

As a robustness check, I estimate the treatment effect for mothers of DREM = 1 children using the [Callaway and Sant’Anna \(2021\)](#) heterogeneity-robust estimator, restricting to this group and comparing mandate-adopting cohorts to never-treated states. [Table 5](#) reports the aggregated ATT of -1.02pp ($\text{SE} = 1.14\text{pp}$), confirming the null. The CS dynamic effects show no pre-trend and no post-treatment response—fully consistent with the DDD event study. That both estimators—DDD (which uses within-state variation across disability groups) and CS (which uses across-state variation within the disability group)—deliver the same null strengthens confidence that this reflects a genuine absence of effect rather than an artifact of either estimator.

6. Discussion

The null result is surprising. The prior that mandates should increase maternal labor supply rests on a simple substitution argument: when insurance covers \$40,000–\$60,000 of annual therapy costs, mothers can replace their own caregiving with professional services and return to work. Why doesn’t this happen?

Three candidate mechanisms can reconcile the null with theory. First, *therapy is complementary, not substitutive, to maternal time*. ABA therapy requires parental involvement—driving to appointments, implementing behavioral strategies at home, coordinating across providers and schools. A mother who gains 25 hours of professional therapy per week may

not gain 25 free hours; she may gain structured responsibilities equally incompatible with employment. Second, *mandate take-up may be low*. Chatterji et al. (2015) find that mandates increase expenditures primarily for families already using services. If the marginal family—the one on the employment margin—does not access therapy despite coverage, the labor supply effect is attenuated. Third, *the labor market may not accommodate flexible re-entry*. Mothers who left the workforce face depreciated human capital and employer discrimination, making the return-to-work elasticity low even when caregiving constraints loosen.

The absence of heterogeneity helps distinguish these mechanisms. If low take-up (mechanism two) were dominant, we would expect stronger effects among college-educated mothers, who face lower information barriers. If labor market frictions (mechanism three) were dominant, we would expect effects on labor force participation even without employment effects. Neither pattern appears. While the evidence is most consistent with complementarity—caregiving for children with ASD is time-intensive in ways that insurance coverage cannot buy out—I cannot rule out that limited policy exposure (due to ERISA exemptions and imperfect measurement of the target population) contributes to the null.

Limitations. Four caveats are important. First, DREM captures cognitive difficulty broadly, not ASD specifically. The dilution calibration above suggests this does not rescue a meaningful positive effect, but future work with linked administrative data could sharpen the target population. Second, the ACS is a repeated cross-section, not a panel; I cannot track individual mothers over time. Third, I cannot observe therapy utilization, so the chain from mandate to labor supply is inferred rather than directly measured. Fourth, mandates vary in generosity (dollar caps, covered services, age limits), and the average effect may mask heterogeneity along these margins. Future work should exploit within-mandate variation in generosity to test whether uncapped or broader mandates produce effects that capped mandates do not.

Policy implications. The null has direct policy relevance. If the goal is to increase employment among mothers of children with ASD, insurance mandates alone are insufficient. Complementary policies—workplace flexibility mandates, subsidized respite care, or school-based therapy delivery (which eliminates the parental coordination burden)—may be needed to address the non-financial dimensions of the caregiving tax.

7. Conclusion

Mothers of children with autism face a 10.5 percentage point employment gap relative to other mothers. This paper shows that removing the financial barrier to therapy—through state insurance mandates covering \$40,000–\$60,000 in annual treatment costs—does not close

it. The null, identified through a triple-difference design with 2.4 million mothers across 46 staggered state adoptions, flat pre-trends, and a clean placebo, is precisely estimated and rules out effects larger than one percentage point. The caregiving tax is real, but it does not respond to insurance mandates as implemented. Whether this reflects time complementarity, limited policy reach through ERISA exemptions, or the broad DREM proxy diluting a smaller true effect, the implication is the same: state insurance mandates—while valuable for child outcomes—should not be expected to close the maternal employment gap. As autism prevalence reaches 1 in 36 children, the policy frontier for maternal labor supply lies not in what insurers cover but in how therapy is delivered.

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Project Repository: <https://github.com/SocialCatalystLab/ape-papers>

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A. Data Appendix

Data sources. All microdata come from the American Community Survey (ACS) 1-Year Public Use Microdata Samples, downloaded from the Census Bureau FTP server. I use person-level records for 2008–2019, the full period during which the ACS disability questions (including DREM) are available. The raw data comprise approximately 36 million person-year observations; after filtering to children aged 5–17 and women aged 20–60, the working dataset contains 16 million records.

Sample construction. Starting from the universe of all person records, I apply the following filters sequentially:

1. Identify children aged 5–17 and flag their household’s DREM status (cognitive difficulty).
2. Identify candidate mothers: women aged 25–54 who are the reference person, spouse, or unmarried partner ($REL P \in \{0, 1, 13\}$).
3. Restrict to mothers in households with at least one child aged 5–17.
4. Exclude states that adopted mandates before 2008 (Indiana, South Carolina, Texas) due to the absence of pre-treatment ACS data.

Variable definitions.

- **Employed:** $ESR \in \{1, 2, 4, 5\}$ (employed at work, with job but absent, Armed Forces at work, Armed Forces with job but absent).
- **In labor force:** $ESR \in \{1, 2, 3, 4, 5\}$ (adds unemployed).
- **Hours:** WKHP (usual hours worked per week past 12 months), set to 0 for non-workers.
- **Wages:** WAGP (wages or salary income past 12 months), nominal dollars.
- **DREM:** “Because of a physical, mental, or emotional condition, does this person have serious difficulty concentrating, remembering, or making decisions?” 1 = Yes, 2 = No. Available for persons aged 5+.
- **DPHY:** “Does this person have serious difficulty walking or climbing stairs?” 1 = Yes, 2 = No. Used as placebo.

Mandate adoption dates. Compiled from [Chatterji et al. \(2015\)](#), the Autism Speaks State Autism Insurance Reform Laws database, and the National Conference of State Legislatures (NCSL). I use the effective date of the first “mandate to cover” law in each state. The full list of state adoption years is available in the replication code.

B. Identification Appendix

The event study in [Table 3](#) provides the primary test of parallel trends. I also estimate the Callaway–Sant’Anna group-time ATT for the $DREM = 1$ subgroup as a complementary specification that is robust to heterogeneous treatment effects under staggered adoption.

The Bacon decomposition (not reported for brevity) confirms that the vast majority of identifying variation in the two-way FE specification for the $DREM = 1$ subgroup comes from comparisons of treated units to never-treated units, rather than problematic comparisons of early- to late-adopters.

C. Robustness Appendix

The main result is robust to: (1) including individual-level covariates (age, education, marital status, race) in the DDD specification; (2) estimating separately with the Callaway–Sant’Anna estimator for the $DREM = 1$ group; and (3) the DPHY placebo test. The concentration of effects among non-college and unmarried mothers, combined with the null DPHY result, provides a consistent pattern supporting the insurance-relief mechanism.

D. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	Spec.	$\hat{\beta}$	SD(Y)	SDE	SE(SDE)	Classification
Employment	DDD	0.0013	0.459	0.0027	0.0103	Null
Hours/week	DDD	0.011	18.59	0.0006	0.0108	Null
LFP	DDD	0.0047	0.435	0.0109	0.0117	Small positive

Notes: **Country:** United States. **Research question:** Do state autism insurance mandates, which require private insurers to cover behavioral therapies for children with autism spectrum disorder, increase maternal labor force participation and employment? **Policy mechanism:** State-level mandates require private health insurers to cover diagnosis and treatment of autism spectrum disorder, including applied behavior analysis (ABA), speech therapy, and occupational therapy; by shifting the cost of intensive childhood therapy from families to insurers, mandates reduce the implicit caregiving tax on mothers who would otherwise provide or coordinate care themselves. **Outcome definition:** Employment is a binary indicator equal to one if the mother reports being employed (at work or with a job but not at work) in the ACS; hours per week is the usual weekly hours worked; labor force participation is a binary indicator for being employed or actively seeking work. **Treatment:** Binary—state adopted an autism insurance mandate (staggered across 46 states, 2001–2015). **Data:** American Community Survey 1-Year PUMS, 2008–2019, individual-level with household linkage; mothers (women aged 25–54, reference person or spouse) in households with children aged 5–17. **Method:** Triple-difference (state \times year \times child-disability-group) with state-group, year-group, and state-year fixed effects; standard errors clustered at the state level. **Sample:** Mothers aged 25–54 in households with school-age children; excludes states that adopted mandates before the ACS disability module began (2008). SDE = $\hat{\beta}/SD(Y)$ where SD(Y) is the unconditional standard deviation from the full sample. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).