HSR Health Services Research

DOI: 10.1111/1475-6773.12220
RESEARCH ARTICLE

Young Adult Dependent Coverage: Were the State Reforms Effective?

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Objective. To examine the robustness of findings regarding state-level adult dependent coverage expansions using detailed outcomes that specify coverage source.

Data Sources. This study uses the 2001–2009 files of the Current Population Survey's Annual Social and Economic Supplement, covering calendar years 2000–2008, and considers young adults ages 19 through 29.

Study Design. Difference-in-differences methods were used to estimate the effect of state-level dependent coverage expansions on finely detailed categories of coverage, and falsification tests were used to evaluate the models themselves.

Principal Findings. Certain published results on state-level parental coverage expansions are flawed, with reported increases driven by changes in spousal coverage. Other published results appear to be in fact driven by parental coverage, but they are not robust to alternative model adjustments.

Conclusions. This study shows evidence that one study's results on "dependent" coverage are in fact driven by changes in rates of spousal coverage. Results from a second study, though not robust to use of a more conventional DD model, would seem to apply most strongly to individuals at ages at which one would typically have lost parental coverage before reform, consistent with a "passive" effect rather than an "active" effect that enrolls previously uninsured youths.

Key Words. Health care reform, health insurance regulation, health policy, health economics, dependent coverage

Uninsurance in the United States is most highly concentrated in young adults. In 2009, the year prior to the passage of the Affordable Care Act, 29.3 percent of adults aged 18–24, and 28.1 percent of adults ages 25–34, had no health insurance at any point during the year, compared to 9.7 percent of children, 21.0 percent of adults 35–44, 15.6 percent of adults 45–64, and only 1.7 percent of adults 65 and older (DeNavas-Walt, Proctor, and Smith 2011). In response to this problem, dozens of states and the federal government have passed "adult dependent" coverage expansions that allow adult children to remain on their parents' health insurance plans past the default limiting age of

19. Although the opening of access to new forms of potentially more affordable insurance may positively impact young adults' probabilities of being insured, this impact could be limited if insured individuals simply switch to less expensive or more generous forms of coverage, while most uninsured individuals remain uninsured. Previous research published in the literature has suggested that state reforms have in fact increased access to private or dependent coverage.

At least three studies, which are summarized in Table 1, focus on state increases in the limiting age on health insurance coverage among young adults (Levine, McKnight, and Heep 2011; Monheit et al. 2011; Depew 2013). Despite slight differences in data, methods, samples, the coding of reform states, and the assumed timing of law adoption, all are in general agreement that the reforms increased access to certain forms of coverage, and they may have also increased the overall insurance rate among certain subpopulations of eligibles.[1] All of these studies employ difference-in-differences (DD) or difference-in-difference-in-differences (DDD) models (with some disagreement over counting the numbers of differences), with the first two also using the Current Population Survey's (CPS) Annual Social and Economic Supplement (ASEC). Monheit et al. (2011) (hereafter, MCDB) report that state reform eligibility is associated with a modest increase in employer-sponsored health insurance received as a dependent, a decline in own-name employer-sponsored coverage, and no change in the overall uninsured rate. Levine, McKnight, and Heep (2011) (hereafter, LMH) find that private insurance rates increased by 2.2–4.4 percentage points in their "full sample" of adults ages 19 through 24. However, neither of these two studies takes advantage of the detailed information on coverage type tracked by ASEC, which offers an important and overlooked check on the validity of a causal interpretation of this prior work.[2] The focus on outcomes such as "private coverage" or even "employer-sponsored dependent coverage" may overlook important shifts between subcategories—or worse, it could inappropriately attribute changes in one type of coverage to another. For example, an increase in employment-related coverage received as a dependent may be driven by changes in spousal coverage rather than policies targeting parental coverage.

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Table 1: Summary of Articles on State-Level Dependent Coverage Expansions

Article	*MCDB (2011)*		*LMH (2011)*			*Depew (2013)*
Model	*1*	*2*	*1*	*2*	*3*	*1*
Dataset	CPS ASEC	CPS ASEC	CPS ASEC	CPS ASEC	CPS ASEC	SIPP
Survey years	2001–2009	2001–2009	2001–2009	2001–2009	2001–2009	2001, 2004, 2008 panels
Sample	Ages 19–29	Ages 19–25	Ages 19–24	Ages 19–24 and "eligible"	Ages 19–24	Females ages 19–25
Insurance outcome	Employer-sponsored dependent	Employer-sponsored dependent	Private insurance coverage	Private insurance coverage	Private insurance coverage	Dependent coverage (parental)
Model	"difference-in-differences"	"difference-in-differences"	"difference-in-difference"	"difference-in-difference"	"triple-difference" (difference-in-difference-in-differences)	"difference-in-difference-in-difference"
Treatment group	Target population in pre- or postreform states	Target population in pre- or postreform state	Individuals in postreform states	Eligible individuals in postreform states	Eligible individuals in reform states	Eligible individuals in reform or prereform states
Control group	Nontarget individuals in prereform or reform states, and all individuals in nonreform states	Nontarget individuals in prereform or reform states, and all individuals in nonreform states	Individuals in prereform or nonreform states	Eligible individuals in prereform states and unmarried individuals in nonreform states	Eligible individuals in prereform states, unmarried individuals in nonreform states, and ineligible individuals in reform states	Noneligible individuals in pre- or postreform states, eligible and ineligible individuals in nonreform states

continued

Table 1. *Continued*

Article	*MCDB (2011)*		*LMH (2011)*			*Depew (2013)*
Model	*1*	*2*	*1*	*2*	*3*	*1*
Predictor variable	TARGET × POLICY	TARGET × POLICY	LAW	LAW	LAW × ELIGIBLE	LAW × ELIGIBLE
Effect (coefficient, SE)	1.52 (0.69)** [percentage points]	2.77 (0.81)*** [percentage points]	0.010 (0.007)	0.022 (0.007)**	0.044 (0.010)**	Females: 0.0571 (0.0093)***; Males: 0.0299 (0.0092)***

***$p < .01$; **$p < .05$.

In this study, I reevaluated the impact of state reforms on the composition of insurance held by young adults to better understand and assess the causal effects of the policies using the main dataset used to estimate insurance coverage rates in the United States generally. I employed DD (or DDD) models previously applied in the literature by MCDB and by LMH, while decomposing the insurance outcomes into nine alternative types: employment-related parental coverage, employment-related spousal coverage, employment-related coverage from an unknown source, privately purchased parental coverage, privately purchased spousal coverage, privately purchased coverage from an unknown source, employment-related insurance held in one's own name, privately purchased insurance held in one's own name, and insurance received through someone that lives outside of one's own household. If the state reforms truly worked, estimated effects on parental dependent coverage, or on coverage received from someone outside of one's own household, should be larger than the estimated effects on spousal dependent coverage, which was not specifically targeted by state dependent coverage expansions.

Data and Methods

Data

Following LMH and MCDB, this study uses the ASEC of the CPS, which is the main source of yearly health insurance coverage estimates in the United States. Also known as the "March Supplement," it is conducted in March of each year for the primary purpose of gathering information about work experience, income, and health insurance coverage during the previous calendar year. Though this retrospective quality may be a limitation of the dataset, and there is debate over the significance of this feature (Ringel and Klerman 2005; Klerman et al. 2009), the data remain the primary source of health insurance coverage estimates in the United States.[3] This study will check the robustness of prior findings regarding the effectiveness of state-level dependent coverage reforms within the context of the ASEC.

For this study, I used the 2001 through 2009 files of the ASEC, covering the time period from the beginning of 2000 through the end of 2008, just prior to the implementation of the ACA's federal limiting age increase. This allows me to repeat the analyses of LMH and of MCDB, both of which consider the same time period. For the MCDB portion of the analysis, I received generous assistance from Joel Cantor, who helped me replicate the MCDB model as closely as possible, as well as their paper's assumptions about which states

count as "reform" states, when they started as reform states, and to whom the reforms applied. For the LMH portion of the analysis, I was able to exactly replicate their findings by using the dataset and coding made available by the team online. I used both papers' original sample restrictions, limiting the age range from 19 through 24 (inclusive) for the LMH portion, and 19–29 (inclusive) for the MCDB portion. I excluded the states of Utah, Hawaii, and Massachusetts for the MCDB portion; for the LMH portion, I only excluded Massachusetts.

Research Hypotheses

Following Cantor et al. (2012) and Antwi, Moriya, and Simon (2012), this study divides commonly used insurance outcomes, such as dependent or private coverage, into subcategories that could in theory respond quite differently to limiting age increases. Setting aside insurance received from outside of one's own household, the ASEC asks about two specific types of coverage received as a dependent: privately purchased and employment related. A follow-up question ascertains an identification number associated with the insurance holder, allowing researchers to create mutually exclusive spousal and parental forms of these two variables (in my samples, fewer than 4 percent of individuals with employment-related dependent coverage had "holder" line numbers that failed to match that of a spouse or parent).

In this paper, "dependent coverage" is understood to include three types of coverage that could have been impacted by a parental coverage expansion: employment-related dependent coverage received through a parent, privately purchased insurance received through a parent, and insurance received from someone outside of one's own household. While one would expect to find an association between regulatory increases in limiting ages for adult children and employment-related parental insurance coverage, one should not expect to find a positive effect on the two forms of spousal coverage, which were not targeted. In fact, spousal coverage may become relatively less attractive for a young adult with newly gained access to coverage through a parent. While this basic analysis holds true for parental and spousal forms of privately purchased dependent coverage, the predicted effect of dependent coverage expansions on privately purchased parental coverage is somewhat ambiguous because this category would be competing with newly available—and likely less expensive—employment-related parental coverage. Insurance received from outside of one's own household is a somewhat vague category, and data limitations do not allow for division into spousal and parental forms; however, as it

likely includes many parental policies, we might expect to see an association between reform eligibility and this type of coverage. Coverage held in one's own name, whether privately purchased or employment related, may be "crowded out" when state reforms open access to parental coverage, and could therefore decline in prevalence.

Empirical Specification

To keep results as relevant to the original source papers as possible, this study uses the exact empirical specifications used by LMH and MCDB. But rather than consider relatively broad outcomes such as "private coverage" or "dependent coverage" as other studies have done, this study will use additional information embedded in ASEC to determine the type (employer-related or privately purchased) and source (parental, spousal, or unknown) of insurance. In most cases, source can be determined using a variable that indicates the holder's line number, or by using additional information on living arrangements. For example, out of the 52,615 individuals of age 19–29 in the 2001–2009 ASEC with employment-related dependent coverage, the holder's line number matched that of a spouse in 20,489 cases, and matched that of a parent in 20,919 cases (there is only one line number for a parent, although many individuals have more than one parent), leaving 11,207 remaining. In a second step, I assumed 9,097 of these 11,207 to be parental because the respondent was living with his or her parents (and of these, 8,846 had a holder line number equal to 1 or 2, likely a parent or guardian), leaving 2,110 from an unknown source. Similar subvariables were created for privately purchased dependent coverage.

For the MCDB portion of the analysis, I estimated the following linear probability DD model:

$$D_{ist} = \boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 S_s + \boldsymbol{\beta}_3 Y_t + \boldsymbol{\beta}_4 (S_s * R_t) + \boldsymbol{\beta}_5 T_{is} + \boldsymbol{\beta}_6 P_{ist} + \boldsymbol{\beta}_7 (T_{is} * P_{ist}) + \boldsymbol{\beta}_8 X_{ist} + \boldsymbol{\beta}_9 A_{ist} + e_{ist} \quad (1)$$

where D_{ist} is an indicator for having a certain type of insurance, and S_s and Y_t are fixed effects for state and year, respectively. S_s is also interacted with a linear time trend, R_t.[4] T_{is} flags individuals in reform states who are members of their states target population—as defined by variables such as age and marital status—regardless of whether their state had yet implemented the reform. The control group, with values of T_{is} set equal to zero, includes ineligible individu-

als in nonreform or prereform states, as well as all individuals in nonreform states. P_{ist} is an indicator for having a reform policy in an individual's state for all or part of the reference year, regardless of one's own eligibility. The interaction of T_{is} and P_{ist} flags individuals in states that have implemented reforms and who are members of their state's target populations, and the coefficient of this interaction β_7, is the difference-in-differences estimator. The vector X_{ist} includes "age, gender, race/ethnicity, marital status, educational attainment, status as a full-time or part-time student, whether young adult is in fair/poor health, family income as percent of federal poverty line, [and] percent of state population that are college graduates" (Monheit et al. 2011). To account for possible policy endogeneity, this model also controls for A_{ist}, a host of predictors of reform adoption, including "whether the state has democratic governor and legislature; a budget surplus; the number of state insurance benefit mandates; unemployment rate; share of young adults in state population." The variable e_{ist} represents the individual error term. As in MCDB's analysis, CPS sampling weights are used, strata are defined at the metropolitan core-based statistical area, and errors are clustered at the household level.

For the LMH portion of the analysis, I estimated a somewhat different linear probability model:

$$D_{iast} = \boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 L_{st} + \boldsymbol{\beta}_3 G_a + \boldsymbol{\beta}_4 Y_t + \boldsymbol{\beta}_5 S_s + \boldsymbol{\beta}_6 U_{st} + \boldsymbol{\beta}_7 X_{ist} + e_{aist} \qquad (2)$$

where D_{iast} is the "insurance status of an individual i in age group a, state s, and year t." P_{st} is an indicator for having a adult dependent coverage policy in effect in an individual's state and year, G_a is an indicator for age, Y_t is an indicator for year, S_s is an indicator for state, U_{st} is an indicator for the unemployment rate specific to that state and year, and X_{ist} is a vector of "individual-level covariates, including gender, marital status, student status, an indicator for residence with a parent, household income as a share of the poverty line, and the square of household income as a share of the poverty line" (Levine, McKnight, and Heep 2011). The coefficient of interest in this model is β_2, which is the estimated effect of having an adult dependent coverage policy on an individual's probability of having a certain type of insurance. Identification in this model comes from L_{st}, the reform or "law" indicator, which may be thought of as an interaction of state, year, and policy reform, and will only be equal to one for individuals living in reform states, in the first year of reform and each year thereafter. Note that this model diverges from the MCDB model and other DD models in that it does not explicitly control for being a member of the target or eligible population (using a variable, I would call E_{st}, analogous to MCDB's T_{is}), nor does it focus on the interaction of indicators for eligibility

and reform. All individuals in the sample, even in nonreform states, are assumed to be "eligible," more or less, even if not technically so, with identification resting on the differences in outcomes between individuals in postreform states and those in prereform or nonreform states. The variable e_{aist} represents the individual error term. Following LMH, and departing from MCDB, standard errors are clustered on state and sampling weights are not used. Also following LMH, I experimented with limiting the sample to young adults who actually are more strongly assumed to be "eligible," either because they meet most of the eligibility criteria of their reform state or because they are pseudo-eligibles who are unmarried in a nonreform state. A third specification interacts an eligibility indicator with L_{st}, as well as with all other predictors, to generate what LMH refer to as a "triple-difference" estimate of the effect of the policy on the set of eligible individuals. (Although the eligibility indicator is interacted with all other predictors on the model, the indicator itself is dropped from this model.) This model's control group is comprised of individuals in postreform or prereform states that do not meet the eligibility requirements of their state, as well as married individuals in nonreform states.

Both MCDB and LMH also analyze outcomes for population subsets such as students and individuals living with their parents. For the sake of brevity, and because these subcategories group people according to endogenous outcomes which could be a result of the reforms themselves, I did not repeat these analyses.

RESULTS

Monheit et al. (2011)

The first panel of Table 2 replicates MCDB's main results (Table 3 in the original paper). MCDB report a 1.52 percentage point increase in employer-sponsored dependent health insurance among adults age 19–29; I estimated a similarly sized increase of 1.62 percentage points. Disaggregation of this outcome into its parental and spousal components makes it clear that the estimated effect is being driven by the spousal type, which seems to have increased by 1.37 percentage points, and which was not targeted by the reforms. Similar results are seen when conducting estimates on the subsample of individuals under age 26. Interestingly, this model also strongly predicts not being married and not living with one's parents, which suggests that the model may be capturing influences other than the state dependent coverage expansions (for obvious reasons, these two runs excluded marital status

Table 2: MCDB Results Driven by Spousal Coverage (Percentage Point DD Estimates)

		MCDB (2011)	*This Study*											
				Parental Source		*Unknown Source*		*Spousal Source*		*Own Name*				
Sample	*Model*	*Employer-Sponsored Dependent*	*Employer-Sponsored Dependent*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*From Outside of One's Own Household*	*Married*	*Lives with Parents*
Panel 1—Original model														
Ages 19–29	Original	1.52 (0.69)**	1.62 (0.65)**	0.31 (0.57)	0.24 (0.18)	−0.07 (0.19)	0.14 (0.14)	1.37 (0.26)***	0.04 (0.07)	−0.53 (0.81)	0.04 (0.36)	−0.65 (0.46)	−2.05 (0.63)***	−2.48 (0.88)***
Ages 19–25	Original	2.77 (0.81)***	2.45 (0.77)***	1.20 (0.69)*	−0.16 (0.24)	0.08 (0.21)	0.09 (0.16)	1.17 (0.30)***	0.06 (0.08)	−1.45 (0.88)	0.29 (0.40)	−0.69 (0.58)	−2.98 (0.69)***	−3.34 (0.96)***
Panel 2—Falsification tests														
Ages 19–29	Reforms 1 year early	n/a	1.28 (0.62)**	0.25 (0.54)	−0.17 (0.19)	−0.06 (0.18)	0.11 (0.12)	1.09 (0.25)***	−0.13 (0.07)*	−1.06 (0.77)	0.11 (0.34)	−0.71 (0.44)	−1.72 (0.69)***	−2.82 (0.82)***
	Reforms 1 year late	n/a	1.84 (0.76)**	−0.16 (0.65)	−0.30 (0.20)	0.24 (0.25)	0.13 (0.18)	1.76 (0.30)***	0.00 (0.08)	−1.13 (0.94)	−0.46 (0.39)	−0.45 (0.52)	2.69 (0.71)***	−2.35 (1.03)**
	Shift state codes up	n/a	1.57 (0.72)**	1.09 (0.66)*	−0.05 (0.20)	0.38 (0.20)*	−0.13 (0.09)	0.10 (0.24)	0.12 (0.07)	−0.88 (0.81)	0.07 (0.35)	0.57 (0.50)	5.56 (0.58)***	5.70 (0.90)***
	Shift state codes down	n/a	−0.47 (0.55)	−1.90 (0.50)***	−0.21 (0.18)	0.12 (0.15)	0.07 (0.09)	1.30 (0.20)***	0.05 (0.06)	−1.09 (0.66)*	−0.17 (0.31)	−0.89 (0.42)*	5.75 (0.48)***	−2.95 (0.73)***

continued

Table 2. *Continued*

		MCDB (2011)	*This Study*										
				Parental Source		*Unknown Source*		*Spousal Source*		*Own Name*			
Sample	*Model*	*Employer-Sponsored Dependent*	*Employer-Sponsored Dependent*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*From Outside of One's Own Household*	*Married*	*Lives with Parents*
Ages 19–25	Reforms 1 year early	n/a	1.92 (0.72)***	0.96 (0.65)	−0.05 (0.24)	−0.03 (0.20)	0.08 (0.14)	0.99 (0.29)***	−0.14 (0.08)*	−1.22 (0.83)	0.35 (0.38)	0.83 (0.55)	−2.70 (0.66)***	−3.87 (0.89)***
	Reforms 1 year late	n/a	2.49 (0.89)***	0.58 (0.79)	−0.24 (0.27)	0.40 (0.27)	0.12 (0.20)	1.50 (0.34)***	−0.06 (0.10)	−2.21 (1.01)**	−0.26 (0.44)	−0.31 (0.67)	−3.28 (0.79)***	−3.39 (1.12)***
	Shift state codes up	n/a	1.19 (0.81)	0.87 (0.75)	−0.06 (0.25)	0.28 (0.22)	−0.13 (0.12)	0.04 (0.25)	0.00 (0.07)	−1.04 (0.86)	−0.20 (0.40)	1.23 (0.60)**	5.12 (0.61)***	5.38 (0.97)***
	Shift state codes down	n/a	1.03 (0.64)	0.22 (0.58)	−0.06 (0.22)	0.17 (0.17)	0.10 (0.11)	0.64 (0.24)	0.04 (0.07)	−1.44 (0.71)	−0.34 (0.34)	−0.89 (0.51)	−6.41 (0.54)	−1.59 (0.79)

Note. Difference-in-differences results show estimated percentage point changes in coverage type. MCDB's estimates are reported in their Table 3.
***$p < .01$; **$p < .05$; *$p < .10$.

Table 3: Private Coverage Effect Estimated by LMH Seemingly Driven by Parental Employment-Related Dependent Coverage

		LMH (2011)	*This Study*											
				Parental Source		*Unknown Source*		*Spousal Source*		*Own Name*		*From Outside of One's Own Household*		
Sample	*Model*	*Private Insurance*	*Private Insurance*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*		*Married*	*Living with Parents*
Panel 1—Original model														
Full sample	Original	0.010 (0.007)	0.010 (0.007)	0.014 (0.007)*	0.000 (0.002)	−0.001 (0.002)	0.001 (0.001)	−0.002 (0.002)	0.001 (0.001)	0.004 (0.007)	0.003 (0.002)	−0.002 (0.003)	0.004 (0.007)	0.011 (0.007)
Eligible sample	Original	0.022 (0.007)***	0.022 (0.007)***	0.020 (0.007)***	0.001 (0.002)	0.001 (0.003)	0.001 (0.001)	−0.001 (0.001)*	0.000 (0.000)	0.003 (0.008)	0.003 (0.002)	−0.001 (0.005)	0.001 (0.003)	0.021 (0.007)***
Triple-diff sample	Original	0.044 (0.010)***	0.044 (0.010)***	0.032 (0.007)***	0.003 (0.002)	0.001 (0.003)	0.001 (0.001)	−0.005 (0.010)	0.003 (0.002)	0.007 (0.017)	0.000 (0.005)	0.005 (0.009)	0.008 (0.012)	0.038 (0.010)***
Panel 2—Falsification tests														
Full sample	Reforms 1 year early	n/a	0.005 (0.007)	0.009 (0.005)*	−0.001 (0.001)	0.002 (0.002)	−0.000 (0.001)	0.000 (0.001)	0.001 (0.000)**	−0.008 (0.007)	0.003 (0.002)	−0.004 (0.004)	0.010 (0.006)	0.014 (0.007)*
	Reforms 1 year late	n/a	0.009 (0.006)	0.009 (0.006)	0.001 (0.001)	0.000 (0.002)	0.001 (0.001)	−0.002 (0.002)	0.001 (0.001)	−0.006 (0.009)	0.002 (0.002)	−0.006 (0.004)	−0.001 (0.008)	0.005 (0.007)
	Shift state codes down	n/a	0.012 (0.007)	0.000 (0.001)	0.000 (0.000)	0.001 (0.001)*	0.000 (0.000)	0.009 (0.005)*	−0.001 (0.001)	−0.004 (0.008)	0.001 (0.003)	0.001 (0.001)	0.005 (0.006)	0.000 (0.004)
	Shift state codes up	n/a	0.002 (0.006)	0.006 (0.006)	0.002 (0.001)	−0.002 (0.002)	−0.002 (0.001)**	−0.002 (0.002)	−0.000 (0.001)	0.004 (0.006)	−0.002 (0.002)	−0.000 (0.004)	0.002 (0.007)	0.006 (0.007)

continued

Table 3. *Continued*

		LMH (2011)	*This Study*											
				Parental Source		*Unknown Source*		*Spousal Source*		*Own Name*		*From Outside of One's Own Household*		
Sample	*Model*	*Private Insurance*	*Private Insurance*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*		*Married*	*Living with Parents*
Eligible Sample	Reforms 1 year early	n/a	0.013 (0.006)**	0.012 (0.006)*	−0.001 (0.002)	0.002 (0.002)	−0.000 (0.001)	−0.001 (0.000)*	−0.000 (0.000)	−0.006 (0.009)	0.003 (0.003)	0.003 (0.003)	−0.000 (0.003)	0.019 (0.008)**
	Reforms 1 year late	n/a	0.007 (0.007)	0.013 (0.007)*	0.002 (0.002)	0.001 (0.002)	0.002 (0.001)	−0.001 (0.001)	−0.000 (0.000)	−0.007 (0.011)	0.003 (0.003)	−0.007 (0.007)	0.002 (0.002)	0.011 (0.007)
	Shift state codes down	n/a	0.023 (0.011)**	−0.001 (0.002)	0.000 (0.001)	0.001 (0.002)	0.000 (0.001)	0.001 (0.006)	0.000 (0.001)	0.018 (0.012)	0.001 (0.006)	0.002 (0.002)	−0.004 (0.002)**	0.018 (0.008)**
	Shift state codes up	n/a	0.003 (0.006)	0.008 (0.008)	0.002 (0.002)	−0.002 (0.002)	−0.003 (0.001)**	−0.001 (0.001)	−0.000 (0.000)	0.004 (0.006)	−0.002 (0.002)	0.002 (0.005)	−0.003 (0.003)	0.003 (0.007)

continued

Table 3. *Continued*

		LMH (2011)	*This Study*											
				Parental Source		*Unknown Source*		*Spousal Source*		*Own Name*		*From Outside of One's Own Household*		
Sample	*Model*	*Private Insurance*	*Private Insurance*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*		*Married*	*Living with Parents*
Triple-difference full sample	Reforms 1 year early	n/a	0.005 (0.007)	0.009 (0.005)	−0.001 (0.001)	0.002 (0.002)	−0.000 (0.001)	0.000 (0.002)	0.001 (0.000)**	−0.008 (0.007)	0.003 (0.002)	−0.004 (0.004)	0.010 (0.006)	0.014 (0.007)*
	Reforms 1 year late	n/a	−0.001 (0.011)	0.018 (0.010)*	0.004 (0.002)*	0.004 (0.003)	0.003 (0.002)*	−0.002 (0.007)	−0.002 (0.002)	0.003 (0.022)	0.002 (0.006)	−0.003 (0.012)	0.001 (0.09)	0.030 (0.013)
	Shift state codes down	n/a	0.017 (0.011)	−0.001 (0.002)	0.000 (0.001)	0.000 (0.003)	0.001 (0.001)	−0.010 (0.008)	0.002 (0.002)	0.030 (0.011)***	−0.001 (0.007)	0.002 (0.002)	−0.002 (0.004)	0.026 (0.009)***
	Shift state codes up	n/a	0.009 (0.016)	0.017 (0.010)*	0.002 (0.003)	−0.004 (0.002)	−0.002 (0.001)	−0.006 (0.010)	−0.001 (0.002)	−0.002 (0.012)	0.000 (0.005)	0.014 (0.008)*	0.019 (0.007)***	−0.004 (0.010)

Note. Difference-in-differences coefficient estimates shown; multiply by 100 for percentage point change estimates. LMH's estimates are reported in their Table 5.
***$p < .01$; **$p < .05$; *$p < .10$.

indicators and living with parent indicators as predictors, respectively). The second panel of Table 2 displays the results of a series of falsification tests of this model, first by shifting all implementation years 1 year backwards or forwards (so that a reform that truly started in 2003 is assumed to have started in 2002 or 2004), then by shifting all state policy codes by up or down (so that Alabama assumes the reform policy coding of Wyoming or Alaska, and Alaska assumes that of Alabama or Arkansas, etc.). I continued to find significant "effects" for employment-related spousal coverage, not being married, and not living with one's parents in most of these models even when estimating the effects of these false reforms.

Levine, McKnight, and Heep (2011)

Table 3 displays the results of my attempt to replicate LMH's findings from their Table 5, showing estimates of the effect of the parental coverage laws on private insurance coverage. The first column of results is taken directly from the original paper and indicates that the reforms are significantly associated with increased access to private coverage when the sample is restricted to eligible individuals, as well as when the "triple-difference" model is used. The second column shows the results of my attempt to replicate these results regarding private coverage. My estimated effects are exactly the same in terms of magnitude and significance, as evidenced by a comparison of the first two columns.

However, "private" coverage is a broad category that includes private and employment-related coverage held by a parent, a spouse, an unknown person, or a respondent, as well as insurance received from outside of one's own household. Of these nine categories, only privately purchased parental coverage, employment-related parental coverage, and coverage received through someone outside of one's own household should be encouraged by a parental coverage expansion, and of these three categories, employment-related insurance is much more common in the sample. Thus, if the reform truly did increase access to private coverage, it is most likely to have occurred through employment-related parental coverage.

The third through eleventh columns of results in Table 3 consider results for the nine subcategories of private insurance considered in this study. These results make it clear that the effects observed by LMH on "private" coverage are in fact being driven by employment-related parental coverage (column three), which is similarly associated with the reform laws in terms of magnitude and significance of the estimated effect. Consistent with

expectations, no significant effects were found on privately purchased parental coverage, on either type of spousal coverage, or on either type of own-name coverage.

The second panel of Table 3 shows that when conducting falsification tests, the three forms of the LMH model tend to produce fewer significant false positives than the MCDB model, and in fact, fewer than would be expected by random chance given a 0.05 level of significance (with four significant results out of 9*12 = 109).

Table 4 further investigates the association between reform and parental employment-related coverage by showing separate results for each year of age, rather than pooling all ages together as in LMH. Across the three models, significant associations were observed among individuals who were of 19, 23, and possibly 21 years of age during March of the survey year—and 18, 22, and 20 (or younger) at some point during the reference year. The transitions from 18 to 19 and 22 to 23 are ages at which one might have typically lost coverage, due to aging typical out due to adulthood or the completion of schooling. The strongest effects estimated were among 19-year olds using the "triple-difference" model, with an increase of 15.5 percentage points, an increase greater than one-third from the rate of 44.0 percent in nonreform states. No significant effects were estimated on individuals of age 22 or 24, and a significant effect on individuals of age 21 was only observed in one of three models.

While the plausibility of these results gives confidence that LMH's findings are valid, one is left wondering why the LMH model produces the "right" results, while the MCDB model does not.

Reconciling the Results

If both approaches seem reasonable prior to running the models, why does one give plausible results consistent with research hypotheses, while the other gives seemingly nonsensical results? In answering this question, I experimented with altering features of the basic MCDB and LMH models (starting with the fullest samples in each group) in ways that made it more similar to the competing model. Among these are as follows: the coding of the states and the states' eligibility requirements; the explicit use of a control for eligibility or target group status (and the interaction of this indicator with a reform indicator); age ranges; excluded states; control variables; and adjustments for complex survey sample design. The results are shown in Table 5. Panel 1 reviews model changes that could be made to the MCDB model that would help it

Table 4: Effects on Employment-Related Parental Insurance by Age (in Survey Year)

Sample	*Age 19*	*Age 20*	*Age 21*	*Age 22*	*Age 23*	*Age 24*
Baseline rate if *Lst* = 0 (coef., SD)	0.440 (0.496)	0.345 (0.475)	0.244 (0.429)	0.167 (0.373)	0.091 (0.288)	0.050 (0.217)
Full sample	0.030 (0.017)*	0.018 (0.012)	0.006 (0.011)	0.012 (0.011)	0.011 (0.007)	0.003 (0.005)
Eligible sample	0.044 (0.016)***	0.023 (0.014)*	0.008 (0.012)	0.017 (0.015)	0.020 (0.008)**	0.010 (0.009)
Triple-difference full sample	0.155 (0.025)***	0.031 (0.021)	0.041 (0.015)**	0.015 (0.019)	0.024 (0.011)**	0.010 (0.009)

Note. Difference-in-differences coefficient estimates shown; multiply results by 100 for percentage point change estimates.
***$p < .01$; **$p < .05$; *$p < .10$.

Table 5: Reconciling the MCDB and LMH Results

		MCDB (2011)	*My Results*									
				Parental Source		*Unknown Source*		*Spousal Source*		*Own Name*		*From Outside of One's Own Household*
Unique Feature of Run	*Sample*	*Employer-Sponsored Dependent*	*Employer-Sponsored Dependent*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	
Panel 1												
Original run	Ages 19–29	1.52 (0.69)**	1.62 (0.65)**	0.31 (0.57)	0.24 (0.18)	−0.07 (0.19)	0.14 (0.14)	1.37 (0.26)***	0.04 (0.07)	−0.53 (0.81)	0.04 (0.36)	−0.65 (0.46)
(1) Cluster @ state, ignore strata and weights	Ages 19–29			1.02 (1.05)				1.72 (0.74)**				
(1) Ages 19–24	Ages 19–24			1.58 (0.76)**				1.17 (0.32)***				
(1) Include UT	Ages 19–24			1.94 (0.73)**				0.74 (0.31)**				
(1) Cluster at state, ignore strata and weights; (2) Ages 19–24; (3) Include UT	Ages 19–24			2.18 (1.09)***				0.77 (0.74)				

continued

Table 5. *Continued*

Unique Feature of Run	*Sample*	*MCDB (2011)* *Employer-Sponsored Dependent*	*My Results* *Employer-Sponsored Dependent*	*Parental Source* *Employment-Related*	*Parental Source* *Privately Purchased*	*Unknown Source* *Employment-Related*	*Unknown Source* *Privately Purchased*	*Spousal Source* *Employment-Related*	*Spousal Source* *Privately Purchased*	*Own Name* *Employment-Related*	*Own Name* *Privately Purchased*	*From Outside of One's Own Household*
(1) Use E, L, E*L (not T, P, T*P)	Ages 19–29			−1.78 (0.46)***				2.05 (0.28)***				

Unique Feature of Run	*Sample*	*LMH (2001)* *Private Insurance*	*My Results* *Private Insurance*	*Parental Source* *Employment-Related*	*Parental Source* *Privately Purchased*	*Unknown Source* *Employment-Related*	*Unknown Source* *Privately Purchased*	*Spousal Source* *Employment-Related*	*Spousal Source* *Privately Purchased*	*Own Name* *Employment-Related*	*Own Name* *Privately Purchased*	*From Outside of One's Own Household*
Panel 2												
Original run	Full sample ages 19–24	0.010 (0.007)	0.010 (0.007)	0.014* (0.007)	0.000 (0.002)	−0.001 (0.002)	0.001 (0.001)	−0.002 (0.002)	0.001 (0.001)	−0.004 (0.007)	0.003 (0.002)	−0.002 (0.003)
(1) Use *E*, *L*, *E**L*, not just *L*	Full sample ages 19–24			0.016 (0.025)				0.018 (0.022)				

continued

Table 5. *Continued*

Unique Feature of Run	*Sample*	*LMH (2001)*	*My Results*									*From Outside of One's Own Household*
				Parental Source		*Unknown Source*		*Spousal Source*		*Own Name*		
		Private Insurance	*Private Insurance*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	*Employment-Related*	*Privately Purchased*	
(1) Drop UT	Full sample ages 19–24			0.012 (0.007)*				−0.002 (0.002)				
(1) Use E, L, E*L, not just L; (2) Drop UT	Full sample ages 19–24			−0.007 (0.013)				0.041 (0.008)***				
(1) Use *P*, not *L*	Full sample ages 19–24			0.010 (0.006)				−0.001 (0.002)				
(1) Use *T*, *P*, *T***P*, not just *L*	Full sample ages 19–24			0.023 (0.016)				0.003 (0.009)				

Note. Difference-in-differences coefficient estimates shown; multiply LMH's results by 100 for percentage point change estimates.
***$p < .01$; **$p < .05$; *$p < .10$.

produce the results more consistent with LMH's findings as well as with the research hypotheses. Panel 2 alters the initial LMH model in ways that would result in MCDB-like findings, that is, significant estimates of increases in employment-related spousal coverage without detecting significant increases in employment-related parental coverage. (For the sake of brevity, only results on parental and spousal employment-related coverage are considered.)

As shown in panel 1, just three simultaneous modifications to the original MCDB model could result in positive estimates of the impact of reform on employment-related parental coverage, without significantly predicting employment-related spousal coverage. These are as follows: clustering standard errors at the state level (as LMH, while ignoring weights or strata), limiting the sample to ages 19–24, and including Utah in the analysis. But if these modifications are in fact preferred, it is hard to argue that this would obviously be the case prior to running the models. As the final row of panel 1 shows, substituting LMH's state policy and individual eligibility coding assumptions into the MCDB model (by replacing T_{is} with E_{is}, an individual eligibility indicator based on the LMH assumptions, P_{ist} with L_{st}, an indicator of policy implementation based on the LMH assumptions, and $T_{is}*P_{ist}$ with $E_{is}*L_{st}$), the new interaction term continued to predict positive increases in employment-related spousal coverage, as well as, for the first time, a significant decrease in employment-related parental coverage.

Panel 2 shows that even fewer modifications have to be made to the basic LMH model before it starts to yield positive estimates of the impact on spousal employment-related coverage, while failing to predict changes in parental employment-related coverage. As the second row shows, the use of a more typical setup which controls for eligibility alongside policy implementation, while focusing on their interaction, does not result in significant predictions of either parental or spousal employment-related coverage. The third row shows that simply dropping Utah does not result in qualitatively different results than the original LMH model. However, if a typical DD setup is used and Utah is excluded from the analysis, the LMH model starts to become predictive of spousal rather than parental employment-related coverage. Though the use of an interaction term (alongside controls for reform and individual eligibility) brings the model closer to typical DD forms, and excluding Utah is justifiable as its implementation predated the study period, these simple modifications result in estimated policy effects that seem nonsensical on their face. For those interested, the final row of panel 2 shows that replacing L_{st} with MCDB's eligibility and policy coding (using T_{is}, P_{ist}, and $T_{is}*P_{ist}$) does not result in significant predictions of either type of employment-related dependent coverage.

DISCUSSION

This study provides new and compelling information regarding the uncertainty surrounding the effectiveness of state-level dependent coverage expansions. It demonstrates that under certain specifications of the LMH models, reforms are indeed associated with higher levels of employment-related parental dependent coverage, especially among individuals at ages at which one would traditionally be dropped from a parental plan due to reaching adulthood (ages 19 and 20 in the survey year) or the completion of college (age 23 in the survey year). In only one of three models was a significant effect observed among individuals of 21 years of age, while no models showed a significant effect among individuals of age 22 or 24 in the survey year. This would be more consistent with a "passive" effect that prevents young adults from being dropped from parental coverage, rather than an "active" effect that enrolls young adults in parental coverage after having gone without it for a period of time. Overall, these results imply that further state-level limiting age increases may have their strongest effect among individuals near the new, federally mandated cutoff point of 26—the "new 19"—where one would have lost parental insurance but for some new state-level reform.

Unfortunately, findings from difference-in-differences models regarding employer-related parental coverage do not seem to be robust to a range of alternative model specifications. While the original LMH model produces plausible results, this assessment changes completely when seemingly reasonable adjustments are made, such as excluding Utah while controlling for individual eligibility status and focusing on the interaction of eligibility and reform. Similarly, by using recommended adjustments for ASEC's complex sample design, excluding Utah from the analysis for having a reform that predated the study period, and including as controls individuals ages 25–29, the original MCDB model predicts increases in spousal coverage, but not parental coverage.

Under no specifications did the reforms seem to affect insurance received through someone outside of one's own household, suggesting that if they did in fact work, the effect was concentrated among young adults living with their parents. But the LMH models that predicted positive increases in employment-related parental coverage also predicted nominally larger increases in the probability of living with ones parents. This raises the additional question of whether the reforms really "caused" the increase in parental coverage, which is possible considering the eligibility requirements of some

states, or if an omitted variable is behind the apparent increases in both living with parents and using parental insurance.

So did these reforms in fact increase the holding of employment-related parental coverage among young adults? Perhaps, and the original LMH results certainly seem to suggest that that is the case. However, there seems to be no ready explanation for why significant and plausible results are only estimated with a somewhat unconventional model that does not follow the standard DD setup (which would control for treatment group and policy reform, while focusing on the interaction of those indicators), and when the sample includes a state that arguably should have been excluded. This is not to say that the policies did not work; perhaps, there is insufficient power to detect what are likely to be, at best, subtle shifts in coverage status among particular subsets of young adults. However, use of these policies to examine other phenomena, such as the link between insurance access and labor market participation, should be undertaken with caution.

ACKNOWLEDGMENTS

Joint Acknowledgment/Disclosure Statement: This study is based on research conducted by James Burgdorf for his Ph.D. dissertation at the Pardee RAND Graduate School. The author thanks Susan Gates, Paul Heaton, and David Neumark for providing early inspiration and advice; Rosalie Pacula, Mireille Jacobson, and Todd Gilmer for their guidance as dissertation committee members; Joel Cantor and Briggs Depew for their generous assistance in interpreting their work; and the anonymous reviewers for their helpful comments on an earlier draft. Any views expressed are those of the author, and any errors are the sole responsibility of the author.

Disclosures: None.

Disclaimers: None.

NOTES

1. Illinois, Maryland, Minnesota, Missouri, and Montana are excluded as reform states by LMH, and included by MCDB, while the reverse is true for Washington. The two papers disagree on when the expansions were implemented in the states of New Mexico, South Dakota, Texas, and Utah. Beyond these two empirical papers, other sources, including the National Conference of State Legislatures (NCSL) (2009), Collins and Nicholson (2010), and Cantor et al. (2012) introduce even more

disagreement as to the timing and limiting ages of state dependent coverage expansions.

2. Using data from the Survey on Income and Program Participation (SIPP) fitted to difference-in-difference-in-differences models, Depew (2012) reports a 5.71 percentage point increase in dependent coverage among women and a 2.99 percentage point increase among men when eligibility is estimated based on age, marital status, student status, and presence of children. The author has confirmed that this "dependent" coverage is in fact parentally based.
3. Some argue, based on comparisons with other surveys that respondents tend to report on their current insurance status despite being asked about coverage in the previous year (Assistant Secretary for Planning and Evaluation [ASPE] 2005). This might explain why the CPS indicates lower rates of calendar year coverage than other sources (Swartz 1986; ASPE 2005). While many argue that the estimates should be interpreted as they are intended, as calendar year estimates (e.g., Kronick 1989; Bennefield 1996; and Fronstin 1996), others argue that they are best interpreted as point-in-time estimates (e.g., Swartz 1986; Bilheimer 1997). A third camp believes that the estimates should be interpreted as lying somewhere in between point-in-time and previous calendar year estimates (Rosenbach and Lewis 1998).
4. Note that my version does not include a noninteracted temporal "trend" variable, although Monheit et al.'s does. This "trend" variable was excluded because it would be redundant to include alongside year indicators.

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