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COMMENTARY

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# Drawing Plausible Inferences about the Impact of State Dependent Coverage Expansions

*Alan C. Monheit, Joel C. Cantor, and Derek DeLia*

In a recent paper published in *Health Services Research*, Burgdorf (2014) critiqued our prior research (Monheit et al. 2011) demonstrating that state laws expanding young adult dependent coverage led to an increase in the likelihood of such coverage.<sup>1</sup> Using Current Population Survey (CPS) data similar to our earlier study, applying a comparable difference-in-difference estimation framework, implementing several falsification tests, and most important, drawing a distinction between parental and spousal dependent coverage, Burgdorf asserts that our conclusion regarding the impact of this coverage expansion is flawed. In reaching this conclusion, Burgdorf asserts that the increase in dependent coverage of young adults through the state reforms was driven by enrollment in *spousal dependent coverage* rather than in *parental dependent coverage*.<sup>2</sup> As a consequence, Burgdorf raises serious doubts about our conclusion that the state reforms had the intended effect of increasing young adult dependent coverage through a parent's private insurance plan.

While we welcome further research that helps to clarify the role of these state insurance reforms and further quantifies their impact on young adult coverage and other outcomes, we believe that Burgdorf's conclusion regarding our work is implausible. Our assertion is based on the following points.

### THE NEW FINDINGS LACK FACE VALIDITY

Burgdorf provides no compelling explanation—in fact no explanation at all—as to why one would expect to observe a differential impact on spousal

coverage for young adults in states that implemented parental dependent coverage reforms compared to nonreform states. That is, the intent of the expansion legislation was to expand young adult coverage through enrollment in a parent's private health plan and, as such, there is no plausible mechanism through which these laws would result in an increase in spousal dependent coverage. Burgdorf appears to recognize this point, noting on page 2109 that "...one should not expect to find a positive effect on the two forms of spousal coverage that 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." Thus, without identifying the reasons why spousal dependent coverage would differentially increase in reform states compared with nonreform states, it is difficult to accept Burgdorf's findings as credible.

Finally, Burgdorf finds that reform was associated with both marital status and living with parents (a negative relationship between reform and these outcomes in his replication of our work for young adults less than age 26). On this basis he suggests that our estimation results may reflect "influences other than the state dependent coverage expansions. . ." (p. 2112). However, without offering any hint as to the potential omitted variables, this appeal to bias is unsatisfactory.<sup>3</sup>

## DETAILS OF STATE REFORMS REINFORCE THE IMPLAUSIBILITY OF THE NEW FINDINGS

As noted, Burgdorf's ancillary empirical work based on our model indicates that reform was associated with a reduction in the likelihood of being married (p. 2112). This is not surprising as 28 of the 31 reform states in our study made married young adults ineligible for reform (Cantor et al. 2012). Thus, it raises the issue of how one could obtain a finding of increased spousal coverage in reform states when such reform precludes marriage by otherwise eligible young adults. In other words, by reducing the likelihood of marriage by young adults, these reforms would limit opportunities for young adults to obtain spousal dependent coverage.

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Address correspondence to Alan C. Monheit, Ph.D., Rutgers University School of Public Health, 683 Hoes Lane West, Room 329, Piscataway, NJ 08554; e-mail: monheiac@sph.rutgers.edu. Joel C. Cantor, Sc.D., and Derek DeLia, Ph.D., are with the Center for State Health Policy, Rutgers University, New Brunswick, NJ.

## STRONGER FALSIFICATION TESTS SUPPORT OUR ORIGINAL FINDINGS

While Burgdorf implemented a number of falsification tests to challenge our findings, he has neglected to implement the most basic falsification test: whether the expansions had an impact on the dependent coverage of *unmarried* young adults who, by definition, have no access to spousal coverage. If such a test fails to produce the hypothesized impact of reform on young adult dependent coverage, then his assertion that reform failed to have the intended effect would have greater face validity.

Using the dataset from our published paper, we now have implemented such a test by restricting our data to unmarried young adults. In general, this test is consistent with our earlier findings that the state reforms resulted in an increased likelihood that a young adult would obtain dependent coverage. More specifically, for unmarried young adults, aged 19–25, who live with their parents (the group for which we expect the expansions to have the strongest effect), we find that the state reforms resulted in a 4.19 percentage point increase in dependent coverage ( $p < .01$ ); for all unmarried young adults, aged 19–25, the reforms resulted in a 2.48 percentage point increase in dependent coverage ( $p < .01$ ); and for unmarried young adults, aged 19–29, living with parents, we find a 2.87 percentage point increase in dependent coverage ( $p < .05$ ). The only group for which our model fails to obtain a statistically significant reform effect is for all unmarried young adults aged 19–29. Thus, the balance of the evidence for this more rigorous test of the impact of the coverage expansions supports our earlier findings that the state reforms increased the likelihood of young adult dependent coverage in the intended manner.

## ADDITIONAL PEER-REVIEWED RESEARCH SUPPORTS OUR ORIGINAL FINDINGS

In his introductory comments, Burgdorf cites unpublished work by Depew (2013) on the state dependent coverage expansions. As Burgdorf notes, Depew used the Survey of Income and Program Participation, applied a difference-in-difference-in-differences (DDD) estimation framework, and found that reform resulted in an increase in *parental* dependent coverage for both males and females. Curiously, Burgdorf provides no further commentary on this cited work. In more recent work in the peer-

reviewed *Journal of Health Economics*, Depew (2015) obtains findings comparable to our earlier work. As in our analysis, Depew uses data from the CPS for a comparable time period. He employs a DDD estimation framework and tests the impact of reform on *parental dependent coverage* for all young adults aged 19–29 and by gender. Additionally, Depew’s findings are obtained by defining eligibility only on the basis of age, avoiding potential selection into treatment that, arguably, might be associated with other eligibility criteria such as marital status. In doing so, he addresses an objection to our work raised by Burgdorf. Depew finds significant and positive impacts of the state reforms on young adult parental dependent coverage for all young adults and separately by gender, and despite his somewhat different approach, notes that his findings are consistent with our earlier study.

## AN IMPORTANT SUBGROUP ANALYSIS IS MISSING FROM THE NEW FINDINGS

Burgdorf also does not apply our model to the subset of young adults living with their parents, arguing that such a variable represents an endogenous outcome. However, we believe it is legitimate to consider analysis of this subgroup as a supplement to the main models, as this subgroup would be most likely to take advantage of a parent’s private health plan. This is especially likely to be the case among young adults aged 19–25 for which the transition to dependent coverage under the reforms would very likely be seamless given the likelihood of their prior status as dependents and parental knowledge of their insurance status. While this may raise the issue of selection into treatment, it is important to recall that Depew’s approach which adjusts for this issue obtains results comparable to our earlier finding.

## THE NEW FINDINGS MAY BE SENSITIVE TO QUESTION WORDING AND CODING RULES USED BY THE CPS TO IDENTIFY SPOUSAL DEPENDENT COVERAGE IN COMPLEX HOUSEHOLDS

In contrast to our work and that of Levine, McKnight, and Heep (2011), Burgdorf has attempted to distinguish parental dependent coverage from

spousal dependent coverage, drawing upon information in the CPS that, in theory, can link the source of coverage (e.g., parental or spousal) to an individual CPS observation (Burgdorf, p. 2110). Despite this effort, we remain concerned as to whether this process can accurately identify spousal dependent coverage in households with multiple health plans, as may be the case when young adults reside with their parents, and both parent and young adult are policyholders. For example, it may be challenging for respondents to the CPS to accurately identify dependent coverage status, and the editing and imputation strategies to address missing data and inconsistencies in responses are not entirely clear.<sup>4</sup> Moreover, if the CPS editing routines were to falsely code parental dependent coverage as spousal coverage, then an actual increase in parental coverage in reform states would appear as an increase in spousal coverage. Further, as young adult dependent coverage was new during the years of our study and varied greatly by state, it is unlikely that CPS editing algorithms could adequately account for this form of coverage. Thus, the challenge of coding appropriate dependent coverage in such complex households could add a measure of statistical noise to an estimated relationship that is not grounded in any a priori theory of causality.

In sum, we welcome the work of other researchers that seek to further investigate the impact of the state dependent coverage reforms on the insurance status of young adults. However, concerns should be raised when such research obtains implausible results that cannot be easily explained, is used as a basis to critique earlier work, and when other credible research confirms the prior finding that reform had its intended effect.

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## NOTES

1. More specifically, our findings indicate the reforms enhanced young adult dependent coverage but at the expense of reduced coverage of young adults as policyholders of employment-related coverage. We found no overall impact on young adult coverage rates. Burgdorf also took issue with research on the state reforms by Levine, McKnight, and Heep (2011), but we restrict our comments to issues raised with regard to our own work.
2. Our study did not make such a distinction since as we note in the text, one could not plausibly expect the reform legislation to operate through spousal dependent coverage. Our models did control for young adult marital status and residence with parents. In models subset to young adults residing with parents, we also controlled for marital status and assigned dependent coverage to young adults if there was at least one parent-policyholder in the household.
3. In his analysis of Levine, McKnight, and Heep (2011), Burgdorf finds a positive relationship between reform and the probability of living with one's parents and similarly appeals to omitted variable bias (p. 2126).
4. We thank Michel Boudreaux of the University of Maryland, for his insight on this issue. The CPS questionnaire is available at <http://www.census.gov/prod/techdoc/cps/cpsmar13.pdf>.

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