RESEARCH LETTER

Changes in Health and Medical Spending Among Young Adults Under Health Reform

Beginning September 23, 2010, the Affordable Care Act allowed young adults to be covered under their parents’ plans until 26 years of age. This dependent coverage provision increased insurance coverage and access among young adults.1,2 However, the association between implementation of the provision and medical spending, health care use, and overall health is unknown.

Methods | Our sample included adults aged 19 to 34 years in the 2002-2011 Medical Expenditure Panel Survey, an annual household survey of the US civilian population conducted by the Agency for Healthcare Research and Quality.3 The study used deidentified, publicly available data and was exempted from institutional board review. Based on previous research,1 we conducted a differences-in-differences analysis, defining the “intervention” group as adults aged 19 to 25 years and the control group as adults aged 26 to 34 years. We excluded 2010 as a washout period; 2011 was the postimplementation period.

Binary outcomes were having health insurance; having any outpatient visit, primary care physician visit, emergency department visit, or prescription medicine fill within the past 12 months; and reporting excellent physical and mental health. Continuous outcomes were inflation-adjusted annual health care expenditures, annual out-of-pocket expenditures, and percentage of expenditures paid out-of-pocket.

We fitted models predicting outcomes as a function of intervention group status, postimplementation year status, and their interaction (the population-level differences-in-differences estimate). We used linear models for binary outcomes and percentage of expenditures paid out-of-pocket. For dollar-value expenditure outcomes, we used a 2-part model: a linear model predicting the probability of any expenditures and a linear model predicting log-transformed expenditures among individuals with any expenditures.

We tested for diverging preimplementation trends in outcomes between groups. Regressions controlled for sex, self-reported race/ethnicity, marital status, Census region, and urban residence. We used SAS version 9.3 (SAS Institute Inc), sampling weights, and robust design-based variance estimators. We considered 2-sided P<.05 to indicate statistical significance.

Results | The sample included 26 453 individuals in the intervention group and 34 052 in the control group. Overall, the sample was 46.6% male and 73.9% white. Group demographics were similar, except fewer adults in the intervention group were married (17.6% vs 56.1% in the control group).

Table 1. Differences-in-Differences Estimates for Insurance, Health Care Use, and Health Outcomes After Implementation of the Dependent Coverage Provision

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Intervention Group (Ages 19-25 y)</th>
<th>Control Group (Ages 26-34 y)</th>
<th>Estimated Change in Policy-related Outcomes, % (95% CI)*</th>
<th>P Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Any health insurance at the end of the year</td>
<td>Before Implementation 62.7</td>
<td>After Implementation 69.2</td>
<td>Before Implementation 72.7</td>
<td>After Implementation 91.5</td>
</tr>
<tr>
<td>≥1 Outpatient visit</td>
<td>57.1</td>
<td>56.3</td>
<td>63.9</td>
<td>62.9</td>
</tr>
<tr>
<td>≥1 Primary care physician visit</td>
<td>32.9</td>
<td>32.7</td>
<td>38.5</td>
<td>35.8</td>
</tr>
<tr>
<td>≥1 Emergency department visit</td>
<td>15.0</td>
<td>15.0</td>
<td>12.8</td>
<td>12.8</td>
</tr>
<tr>
<td>≥1 Hospitalization</td>
<td>6.1</td>
<td>5.8</td>
<td>7.6</td>
<td>7.3</td>
</tr>
<tr>
<td>≥1 Prescription medicine</td>
<td>49.2</td>
<td>48.3</td>
<td>54.7</td>
<td>54.6</td>
</tr>
<tr>
<td>Excellent physical health</td>
<td>26.9</td>
<td>30.9</td>
<td>23.4</td>
<td>21.1</td>
</tr>
<tr>
<td>Excellent mental health</td>
<td>36.6</td>
<td>39.1</td>
<td>35.0</td>
<td>33.2</td>
</tr>
</tbody>
</table>

* Adjusted coefficient of the interaction between postimplementation status and intervention group.

Defined as outpatient visits to a physician whose specialty was internal medicine, pediatrics, family practice, general practice, or osteopathy. Visits to nurse practitioners and physician assistants were excluded because Medical Expenditure Panel Survey databases do not include specialty information for these types of clinicians.
Compared with the control group, the dependent coverage provision was associated with an increase of 7.2 (95% CI, 4.2-10.2) percentage points in the probability of insurance coverage among adults aged 19 to 25 years ($P < .001$), no statistically significant changes in health care use, an increase of 6.2 (95% CI, 3.2-9.3) percentage points in the probability of reporting excellent physical health ($P < .001$), and an increase of 4.0 (95% CI, 0.6-7.5) percentage points in the probability of reporting excellent mental health ($P = .02$) (Table 1).

Compared with the control group, implementation of the provision was associated with a decrease of 3.7 (95% CI, 0.9-6.4) percentage points in the percentage of expenditures paid out-of-pocket among adults aged 19 to 25 years with any expenditures ($P = .009$; Table 2). Annual out-of-pocket expenditures declined by approximately 18% (95% CI, 5%-31%) in the intervention group (from an unadjusted mean of $546.11$, relative to the control group ($P = .006$). Results were similar after additionally controlling for household income, education, and employment. Preimplementation trends did not differ significantly between groups.

### Discussion

The dependent coverage provision was associated with improved self-reported health and protection against medical costs among adults aged 19 to 25 years compared with older adults unaffected by the law. One recent study indicated that the provision was associated with improved protection against emergency care costs; we found this financial protection extended to overall medical expenditures. Previous research documented rapid improvements in self-reported health among low-income and elderly adults gaining Medicaid and Medicare coverage, respectively. In one study, these gains occurred before any changes in health care use, suggesting that insurance may improve peace of mind and perceptions of health.

We did not detect significant changes in health care use; however, only 1 year of postimplementation data was available for our study, which limited statistical power and prevented examination of longer-term changes. Another limitation is that other factors during the postimplementation period could have differentially affected outcomes between groups.

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**Study concept and design:** Both authors.

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**Drafting of the manuscript:** Chua.

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**Study supervision:** Sommers.

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**COMMENT & RESPONSE**

**Number of Human Papillomavirus Vaccine Doses and Condyloma**

**To the Editor** Dr Herweijer and colleagues1 presented short-term data on the association of quadrivalent human papillomavirus (HPV) vaccine with the risk of condyloma by number of vaccine doses. Their primary analysis showed that fewer doses may be associated with a reduction in condyloma risk, albeit with less reduction than 3 doses. Yet their supplemental data suggested that 1 and 2 doses may be associated with comparable risk reduction as 3 doses. We would like to address this inconsistency.

Reduced-dose schedules could diminish disparities related to access to HPV vaccines by easing costs and logistics; the hope is this would translate to greater uptake in resource-poor settings. If 1 dose allows for greater vaccine uptake, it may be able to prevent more deaths than 2 doses, even if modestly inferior. Data from the Costa Rica Vaccine Trial, which used the bivalent HPV vaccine, showed comparable vaccine efficacy against incident persistent HPV 16/18 infections after 4 years for women who received 1, 2, or 3 doses2; within the same study, immunogenicity data bolstered this observation by showing stable antibody levels by dose against HPV 16/18.3 Responses for 1 dose were lower although higher than those observed for natural immunity. Immunogenicity data showing comparable antibody titers for 2 and 3 doses have also been published.4

Ideally, risk reductions or vaccine efficacy should be assessed among vaccine recipients prior to sexual debut, to avoid misclassifying as vaccine failure HPV infections prevalent at time of vaccination, which are not affected by vaccination.5 In the current analysis, prevalent infections may have disproportionately affected girls receiving fewer than 3 doses: sensitivity analyses that accounted for cases occurring within months immediately after vaccination by lengthening the “buffer period” between vaccination and case counting showed a decrease in risk of condyloma, especially among the youngest girls most likely to have been recently exposed. Use of the 12-month buffer made the incident rate ratios (IRRs) for girls who received 1, 2, and 3 doses nearly identical (IRRs, 0.24, 0.19, and 0.19, respectively) compared with unvaccinated girls (eTable 1 in the Supplement for the article). One potential conclusion is that individuals who did not receive the full 3-dose schedule obtained similar risk reduction.

We look forward to future work with more person-time that is restricted to girls vaccinated before sexual debut, for whom risk or vaccine efficacy estimates will be nearly unaffected by prevalent HPV infections, and the durability of protection can be assessed.

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**In Reply** Dr Kreimer and colleagues point out that there appears to be little difference in the risk of condyloma with different numbers of doses of HPV vaccine if a 12-month buffer period is used. We appreciate the thoroughness with which they studied the supplementary material and regret that space limitations did not allow a more detailed explanation of the sensitivity analysis in question.

In short, the buffer period and delayed case counting were introduced to account for prevalent cases diagnosed shortly after vaccination, which would otherwise bias the estimated risk of condyloma upward. In Figure 1 in the article, which...