Does Preventive Care Reduce Usage of Curative Services?

David M. Zimmer
Western Kentucky University

September 4, 2019

Abstract

Existing research offers somewhat mixed evidence on whether preventive care leads to reductions in usage of curative services. But do those findings reflect the somewhat limited dynamic considerations in existing studies? Would offsets become evident if researchers allowed more explicit dynamic relationships over longer time horizons? This paper contributes to the existing literature in three ways. First, it uses relatively long individual-level panel surveys. Second, it employs a formal lag adjustment estimator that accommodates the potentially complicated dynamic relationship between preventive and curative care. Third, it focuses on relatively young individuals, the group of most interest for the Affordable Care Act. Results find that, following a preventive checkup, individuals do not appear to reduce future curative usage or spending.

JEL Codes: I18; C23

Keywords: offsets; cumulative effects; dynamic adjustment

1. Introduction

Although health policy and medical research mostly agree that usage of preventive health care services helps to avert or delay serious medical problems, considerable disagreement exists about whether preventive care actually reduces subsequent usage of costly curative services. If preventive care does reduce future usage of curative services, then such "offsets," as they are sometimes called, might generate cost savings. However, if offsets do not exist, then preventive care might still offer health benefits, but policymakers should be more circumspect when using offsets to promote policies that encourage usage of preventive care.

Several empirical obstacles complicate easy detection of offsets. First, usage of a preventive service might affect *contemporaneous* usage of curative services, if, for example, physicians detect health problems while conducting physicals. But statistical approaches designed to quantify those contemporaneous effects differ from those needed to uncover potentially more important *dynamic* offsets, in which preventative care (hopefully) reduces usage of curative services during future time periods. Most existing studies of offsets emphasize contemporaneous channels, in part due to a lack of data sources that track individual-level health care usage over time, and in part due to analytical complications inherent in dynamic models.

But the dynamic channels are arguably more important from a policy perspective.

A second complication is that a one-time usage of a preventive service might create an offset during *any and all* subsequent time periods. Thus, a dynamic model that seeks to detect offsets must allow for accumulated effects over time, without undue restrictions on when those offsets may occur. A third complication is that individuals might possess unmeasured attributes, such as risk aversion or attitudes toward health care, that simultaneously affect usage of preventive *and* curative services. Such endogeneity potentially biases estimates of offsets.

The paper uses two large individual-level household panel surveys to investigate whether offsets exist. In doing so, it focuses on young adults (18-29 years old). Because that age group had relatively low insurance coverage prior to passage of the Affordable Care Act, and because insurance policies regulated by the Affordable Care Act include provisions designed to reduce point-of-service costs of preventive care, young adults are expected to increase their usage of preventive services in the coming years. Therefore, having an understanding of possible offsets among young adults is crucial in order to anticipate the impacts of the law. To allow for unrestricted dynamic offsets, the empirical approach adopts a lag adjustment model that accommodates the evolution of the effects of preventive care over time. The model also addresses potential endogeneity of preventive service usage.

The main conclusion is that preventive care does not appear to lead to economically meaningful reductions in curative usage or spending, whether in office-based or hospital settings. That conclusion may be interpreted in a negative light, in that preventive usage does not appear to lead to cost reductions. On the other hand, that conclusion also points to little evidence of cost *increases*. Therefore, if preventative usage leads to significant improvements in health, policies that emphasize preventative usage still might appear attractive once subjected to formal cost/benefit analyses.

2. Background

In September, 2009, as debate over the Affordable Care Act was intensifying, President Barack Obama delivered an address to a joint session of Congress in which he described a key component of his health care plan. During that address he said,

> "Insurance companies will be required to cover, with no extra charge, routine checkups and preventive care, like mammograms and colonoscopies – because there's no reason we shouldn't be catching diseases like breast cancer and colon cancer before they get worse. That

> makes sense, it saves money, and it saves lives."

In February, 2012, the President reiterated those themes during a White House press briefing.

> "As part of the health care reform law that I signed last year, all insurance plans are required to cover preventive care at no cost. That means free checkups, free mammograms, immunizations and other basic services. We fought for this because it saves lives and it saves money – for families, for businesses, for government, for everybody. That's because it's a lot cheaper to prevent an illness that to treat one."

President Obama is not alone among American politicians claiming links between preventive care and medical spending. Senators Hillary Clinton and John Edwards and Arkansas Governor Mike Huckabee all made similar statements during the 2008 presidential election.

Most health policy and medical research concurs that increasing usage of preventive services helps to avert or delay serious health problems (Maciosek, Coffield, Flottemesch, Edwards, and Solberg, 2010). But whether preventive care actually reduces subsequent usage of costly curative services remains ambiguous. The widely-publicized RAND health insurance experiment of the 1970s randomly assigned subjects to different insurance plans with varying levels of coverage for preventive services. The results of that experiment fail to uncover evidence that increased usage of preventative services results in lower usage of subsequent curative services (Manning et al., 1987). Gruber (2006) reviews the economic literature since the RAND experiment, reaching the same conclusion. A meta analysis of medical studies by Cohen, Neumann, and Weinstein (2008) reaches a similar consensus, with little evidence of offsets. By contrast, Maciosek et al. (2010), using a more aggregated approach, report relatively large reductions in health spending following increases in preventive utilization.

A smaller strand of research focuses on the more specific question of whether prescription drug usage results in reduced usage of nondrug care. Among those studies, Gaynor, Li, and Vogt (2007) find evidence of offsets from prescription drugs, while Stuart et al. (2007) do not. Deb, Trivedi, and Zimmer (2014), using a limited dynamic approach, find evidence of trivially small offsets from prescription drugs.

In sum, existing research offers somewhat mixed findings, with the balance tilted somewhat toward the conclusion of no offsets. But do those findings reflect the somewhat limited dynamic considerations in existing studies? Would offsets become evident if researchers allowed more explicit dynamic relationships over longer time horizons? This paper contributes to the existing literature by using panel surveys with relatively long time dimensions, and it places a greater emphasis on dynamic relationships as opposed to contemporaneous ones. The following section describes the two panel surveys, followed by a formal description of the empirical model.

3. Data

This paper considers data drawn from two large nationally representative individual-level surveys, each with separate advantages and disadvantages for the topic at hand. The 1997 National Longitudinal Survey of Youth provides a relatively long panel (7 years in this paper), but somewhat limited information on health care usage. The Medical Expenditure Panel Survey, by contrast, offers a shorter panel with higher periodicity (5 approximate six-month "rounds"), but more detailed information on health care usage. Using two large panel surveys, each with different lengths and periodicities, and each with different measures of health care usage, allows one to explore whether findings of offsets, or lack thereof, owe to survey-specific features.

3.1. NLSY

The 1997 National Longitudinal Survey of Youth (NLSY) consists of a series of annual surveys of approximately 9,000 youths who were 12 to 16 years old as of December 31, 1996. The estimation sample used in this paper considers subjects present in the years 2002, 2003, 2004, 2005, 2006, 2007, and 2008, with no missing information for key health care usage measures, yielding an estimation sample of 30,877 person/year observations. Focusing on those years means that all subjects were between 18 and 29 years old, which represents one of the key demographics targeted by the Affordable Care Act.

In each year, the NLSY asks respondents (1) if they have had a routine checkup in the past year, and (2) how many times they have visited a doctor in the past year for treatment for an injury or illness. Table 1, which reports sample means, labels those two measures of health care usage "checkup," which is binary, and "curative doctor visits," which is a discrete count.

3.2. MEPS

The second survey is the publicly-available Medical Expenditure Panel Survey (MEPS), conducted by the Agency for Healthcare Research and Quality, a unit of the U.S. Department of Health and Human Services. The MEPS consists of a series of on-going surveys of families and individuals, employers, and health care providers. Introduced in 1996 as a successor to the National Medical Expenditure Survey, the MEPS enjoys a reputation as the most complete source of data on healthcare usage and costs in the U.S.

To match as closely as possible to the NLSY sample, data are drawn from the 2002-2008 years. The sample is limited to subjects between 18 and 29 years old. All MEPS subjects are interviewed for five "rounds" over a two-and-a-half year period, with each round spaced approximately six months apart. Thus, the MEPS survey offers a panel structure, albeit a limited one. The estimate sample includes 69,820 person/round observations.

Like the NLSY, the MEPS includes information on annual checkups and the number of curative doctor visits. In contrast to the NLSY, which directly records the number of curative doctor visits, the MEPS measure is calculated by subtracting checkup visits from the total number of doctor visits. Moreover, whereas the NLSY topcodes visits at 4, the MEPS does not. However, the paper reached similar conclusions after forcing a topcode of 4 on the MEPS observations, perhaps because fewer than 5 percent of subjects in the MEPS sample have more than 4 visits.

A significant advantage of the MEPS is its highly detailed information on health care usage and spending. To that end, the MEPS sample includes three other measures of curative usage not available in the NLSY. Those are

- Total spending on doctor visits (in 2007 dollars using the medical CPI)
- Number of outpatient hospital visits
- Total spending on outpatient hospital visits (in 2007 dollars using the medical CPI)

Doctor spending captures the "intensity" of doctor usage, which might be missed in simple counts of doctor contacts, while hospital usage and spending captures potentially more serious types of curative-based health care.

3.3. Correlations between checkups and curative usage

Table 2 presents correlations between the binary checkup variables and the curative measures. All numbers presented in the table differ from zero with p-values less than 0.05. The most interesting pattern from the table is that all correlations are positive, even after lagging the checkup measure several periods. Those positive correlations might lead a policymaker to believe that preventive care does *not* reduce curative usage, but rather actually *increases* it.

However, if preventive usage is *contemporaneously* correlated with curative usage, as seems possible, and if preventive usage is serially correlated, which seems likely, then the positive correlations in Table 2 might simply be blending those two patterns in the data, even if economically-meaningful offsets in fact do exist. The following section presents an econometric model that seeks to isolate the impact of preventive care on subsequent curative utilization, while controlling for potential confounding patterns in the data.

4. Econometric Model

The model presented here is a variant of a lag adjustment specification originally proposed by Borenstein, Cameron, and Gilbert (1997) to investigate the effects of crude oil prices on downstream retail gasoline prices. This section includes enough detail to keep this paper relatively self contained, but the interested reader might wish to consult the Borenstein, Cameron, and Gilbert piece for a more detailed exposition.

Let y_t measure a person's usage of curative health care services in period t, and let x_t be a binary indicator for whether the person received a preventive checkup in period t. The empirical model seeks to determine whether a preventive checkup in period t affects curative usage, both in period t and in subsequent periods.

Defining the change in curative usage from the previous period as $\Delta y_t =$

$y_t - y_{t-1}$, the effects of preventive care on curative usage are expressed as

$$\begin{aligned} \Delta y_t^t &= \beta_0 x_t \\ \Delta y_{t+1}^t &= \beta_1 x_t \\ \vdots \quad & \quad \vdots \\ \Delta y_{t+n}^t &= \beta_n x_t \end{aligned}$$

where the superscript on Δy_t highlights that it is solely preventive care received in period t that drives current and future changes in curative usage. The subscript n represents the number of periods affected by preventive care received in period t. That number could stretch toward a person's end of life, but, for issues of data practicality, must be limited to a finite number.

Under those assumptions, the cumulative change in curative usage in period t depends on preventive care received in the previous n periods,

$$\Delta y_t = \Delta y_t^t + \Delta y_t^{t-1} + \cdots + \Delta y_t^{t-n} = \sum\nolimits_{i=0}^{n} \beta_i x_{t-i}.$$

To render this setup suitable for regression estimation, the cumulative change in curative usage is re-expressed as,

$$\Delta y_t = \phi + \sum\nolimits_{i=0}^{n} \beta_i x_{t-i} + \gamma \Delta y_{t-1} + \varepsilon_t \tag{1}$$

where ϕ represents a constant term, and ε_t denotes a white noise error. Note that equation (1) also includes the lagged change in curative usage, both to accommodate serially correlated patterns in curative usage, and to allow a richer dynamic specification. The dependent variable, Δy_t, is approximately symmetrically distributed about zero for all curative usage measures, allowing equation (1) to be estimated by ordinary least squares. (Although, see Cameron, Li, Trivedi, and Zimmer (2004) for an alternative approach for estimating differences in discrete counts.)

The additive lag structure in equation (1) places few restrictions on how curative usage responds to preventive care. Of particular importance, the setup allows temporal independence, in that the effects of a preventive checkup in period t need not be undone if the person does not receive a checkup in period $t + 1$.

The NLSY and MEPS include 7 and 5 time periods, respectively. Although longer than other studies of offsets, those finite time dimensions place practical limitations on the number of lags n. For both the NLSY and the MEPS, lagged preventive care became insignificant after the third lag. Therefore, for both estimation samples, the number of lags is set to $n = 3$.

Although the following section reports regression estimates from equation (1), the main message of this paper emphasizes the cumulative effects on curative usage of a preventive checkup, which is a nonlinear expression of the estimated parameters from equation (1). Specifically, let B_k denote the cumulative effect on curative usage k periods after a preventive checkup. Those cumulative effects in each period are calculated as

Periods after checkup	Cumulative effect on curative usage
0	$B_0 = \beta_0$
1	$B_1 = B_0 + \beta_1 + \gamma B_0$
2	$B_2 = B_1 + \beta_2 + \gamma(B_1 - B_0)$
3	$B_3 = B_2 + \beta_3 + \gamma(B_2 - B_1)$
$\vdots$	$\vdots$

Standard errors for the terms B_k are calculated by block bootstrap. The first step randomly draws (with replacement) a bootstrapped sample, keeping each individual's time observations "blocked" together. The second step re-estimates equation (1) and the associated B_k terms using the bootstrapped sample. After repeating those two steps 200 times, the standard deviations of the estimated B_k terms provide standard errors.

5. Results

Table 3 shows estimates from the lag adjustment equation given in equation (1). In contrast to the simple correlations between preventive checkups and curative usage presented in Table 2, all of which are positive, the more nuanced lag adjustment setup uncovers some statistically-significant negative relationships between checkups and curative utilization. The models also find statistically-significant negative coefficients for lagged changes in curative usage, indicating some amount of mean reversion following a change in usage of curative services.

Unfortunately, a cursory glance of Table 3 makes it difficult to determine whether offsets exist, as the total change in curative usage t periods after a preventive checkup is the sum of (1) the changes in curative usage through periods $t-1$, (2) the contemporaneous link between a possible checkup in period t and curative usage, and (3) the effects of lagged changes in curative usage. Therefore, this paper emphasizes cumulative effects on curative usage, calculated as described in the previous section.

Figure 1 graphs those cumulative effects, along with 95 percent confidence bands calculated by block bootstrap. Focusing on curative doctor visits in the NLSY (the top-left panel), a preventive checkup correlates with an *increase* of almost 0.2 curative doctor visits during the year of the checkup (illustrated by the large circle in the figure). The following year, however, cumulative usage (meaning summing the impacts in years 0 and 1) has decreased by approximately 0.05 visits (the large triangle). Despite the offset losing statistical significance two years after the checkup (the large square), three years later, the checkup appears to have reduced curative doctor visits by a cumulative 0.05 visits (the large diamond).

Thus, the top-left panel of Figure 1 presents evidence of a statistically significant offset, albeit a very small one. The mean number of curative doctor visits in the NLSY over a *four year* period is 3.3. That implies that a cumulative four-year offset of 0.05 represents a mere 1.5 percent reduction in curative doctor visits, relative to mean usage.

The other four panels in Figure 1 show estimates for the MEPS survey, where, in contrast to NLSY, a "period" represents approximately six months. Despite evidence of a contemporaneous offset of 0.2 visits for curative doctor visits, and a contemporaneous positive link with respect to doctor spending, all cumulative offsets become statistically indistinguishable from zero three periods (approximately 1.5 years) after a preventive checkup.

In sum, the cumulative effects calculations suggest a tiny, economically-insignificant reduction in curative doctor visits three years after a preventive checkup, but otherwise no evidence of offsets. Perhaps most importantly from a public policy perspective, preventive checkups do not appear to reduce subsequent doctor or outpatient hospital *spending*.

6. Robustness Checks

This section considers two robustness checks. The first partitions the sample according to several subsamples of potential policy interest. The second considers the possible endogeneity of contemporaneous preventive care with respect to curative usage.

6.1. Subsamples of policy interest

The baseline lag adjustment setup put forth in equation (1) does not include other explanatory variables, because the goal of that equation is to calculate mean offsets among the population of interest, an exercise that does not require eliminating variation due to socioeconomic characteristics. Nonetheless, one might consider whether the findings of the previous section – minimal to no offsets – apply to certain subsamples of policy interest.

To that end, Figures 2–4 present cumulative offset calculations for three subsamples of interest. Figure 2 examines females, who might have different preventive care needs than their male counterparts, especially among the young adult population considered in this paper. The figure indicates a cumulative reduction of approximately 0.08 curative doctor visits three years after a preventive checkup in the NLSY. That represents an approximately 2 percent reduction in doctor visits, relative to mean usage among females over a four year period. The MEPS sample, on the other hand, reveals no evidence of offsets.

Figure 3 examines black and Hispanics. None of the calculations show evidence of offsets among that group. Finally, Figure 4 examines poor subjects (family income less than 200 percent of the federal poverty line) in self-reported fair or poor health. This is a group for which previous studies have found some economically meaningful offsets (Gruber, 2008), yet Figure 4 fails to find any such evidence. (Figure 4 focuses on the MEPS sample, as the NLSY database does not include comparable information on self-reported health.)

6.2. Possible endogeneity of contemporaneous preventive care

There is reason to suspect that contemporaneous preventive usage, denoted x_t in the notation above, might correlate with the error term in equation (1). For example, a person who suspects he has health problems might seek a routine checkup, and if that checkup confirms his suspicions, he might then be referred toward curative care. Such endogeneity bias does not matter if one's primary interest is correlations between preventive care and changes in subsequent curative usage. But if one wishes to establish cause-and-effect relationships between preventive care and changes in curative usage, then one must address the endogeneity problem.

Addressing such endogeneity requires an instrument that correlates with contemporaneous preventive usage, but does not (directly) affect curative usage. Unfortunately, no obvious instrument is present in both surveys, but each survey offers a slightly different option. The NLSY sample includes a binary measure of whether the person claims to have an "organized" personality. The MEPS sample, by contrast, includes a binary measure of whether the person claims to be risk averse. (According to those measures, approximately 53 percent of the NLSY sample reports being organized, and approximately 45 percent of the MEPS sample claims to be risk averse.)

Instrument validity rests on two suppositions. The first is that organized or risk averse individuals are more likely to take the initiative to seek a preventive checkup. Indeed, in the NLSY sample, being organized associates with a statistically-significant 3.3 percentage point increase the probability of receiving a checkup, which represents a 6.3 percent increase relative to the mean likelihood of receiving a checkup. In the MEPS sample, risk aversion associates with a 2.1 percentage point increase in the probability of receiving a checkup, a 21 percent increase relative to the mean likelihood of having a checkup. Consequently, each survey's instrument seems to significantly and nontrivially affect the probability preventive care usage.

The second requirement for instrument validity is that the instruments cannot directly affect changes in curative usage, other than indirectly through their effect on preventive care. That implies that the instrument may be excluded from the main regression in equation (1). Indeed, when those instruments were included as explanatory variables in equation (1), their coefficients failed to come close to reaching statistical significance. (The p-values of the coefficient of the instruments in the five regressions reported in Table 3 are 0.78, 0.89, 0.85, 0.81, and 0.86.) Consequently, the instruments seem plausibly excludable from equation (1).

With those instruments, equation (1) is estimated by two-stage least squares, treating contemporaneous preventive care (x_t) as endogenous. Presented in Figure 5, two-stage least squares estimates do not find any statistically-significant evidence of offsets. The main effect of the instruments appears to be a loss of precision during period 0, but, more importantly, the cumulative offsets still appear to hug closely to the horizontal zero line.

7. Conclusion

The belief that relatively-inexpensive preventive care reduces expensive curative usage remains pervasive in the political arena. Existing research on the subject, however, offers more tempered findings, with the balance tilted somewhat toward the conclusion of no offsets.

One concern with existing research is the somewhat limited consideration of dynamic effects. That is, preventive care may require years before yielding any potential reductions in curative usage. This paper contributes to the existing literature by using panel surveys with relatively long time dimensions. The paper uses a lag adjustment estimator that isolates the impact of preventive usage and emphasizes its long-run dynamic effects of curative usage. In doing so, the model finds small, but economically meaningless, reductions in curative doctor visits three years after a preventive checkup. More importantly, preventive checkups do not appear to reduce subsequent *spending* on doctor services or outpatient hospital care.

A few caveats of the present study deserve mention. First, although this study uses longer panels than previous studies, along with a more formal treatment of dynamics, the length of the panels employed here still might be insufficient to detect offsets. For instance, if preventive checkups require decades before offsets materialize, then detecting such patterns remains beyond the scope of currently available data sources. A second caveat relates to the measure of preventive care, routine checkups, employed in this paper. From a policy perspective, routine checkups remain important, due to their emphasis in the Affordable Care Act. But perhaps such a broad measure of preventive care does not produce subsequent reductions in curative usage, but more specific, targeted preventive services, such as colonoscopies, might. A third caveat is this paper's emphasis on young adults. Although that group represents the most important demographic for the Affordable Care Act, perhaps, being relatively healthy, young adults already have relatively little need for curative care.

The main conclusion of this paper – that preventive checkups do not appear to reduce subsequent curative usage or spending – should not be interpreted as questioning the appropriateness of preventive care. Indeed, if preventive services yield even small improvements in health, then such care could pass formal cost/benefit tests, even without generating any offsets. Rather the main implication of this paper is that policymakers should be more circumspect when attempting to use offsets as a justification for policies that encourage usage of preventive care.

References

Borenstein, S., Cameron, A., and R. Gilbert (1997). "Do Gasoline Prices Respond Asymmetrically to Crude Oil Price Changes?" *Quarterly Journal of Economics*, 112, 305-339.

Cameron, A., Li, T., Trivedi, P., and D. Zimmer (2004). "Modeling the Differences in Counted Outcomes Using Bivariate Copula Models with Application to Mismeasured Counts." *Econometrics Journal*, 7, 566-584.

Cohen, J., Neumann, P., and M. Weinstein (2008). "Does Preventive Care Save Money? Health Economics and the Presidential Candidates." *New England Journal of Medicine*, 358, 661-663.

Deb, P., Trivedi, P., and D. Zimmer (2014). "Cost-offsets of Prescription Drug Expenditures: Data Analysis via a Copula-based Bivariate Dynamic Hurdle Model." *Health Economics*, 23, 1242-1259.

Gaynor, M., Li, J., and W. Vogt (2007). "Substitution, Spending Offsets, and Prescription Drug Benefit Design." *Forum for Health Economics and Policy*, 10, 1-31.

Gruber, J. (2006). "The Role of Consumer Copayments for Health Care: Lessons from the RAND Health Insurance Experiment and Beyond." Kaiser Family Foundation Report, 10/2006.

Gruber, J. (2008). "Covering the Uninsured in the United States." *Journal of Economic Literature*, 46, 571-606.

Maciosek, M., Coffield, A., Flottemesch, T., Edwards, N., and L. Solberg (2010). "Greater Use of Preventive Services in U.S. Health Care Could Save Lives at Little or No Cost." *Health Affairs*, 29, 1656-1660.

Manning, W., Newhouse, J., Duan, N., Keeler, E., Leibowitz, A., S. Marquis (1987). "Health Insurance and the Demand for Medical Care: Evidence from a Randomized Experiment." *American Economic Review*, 77, 251-277.

Stuart, B., Briesacher, B., Doshi, J., Wrobel, M., and F. Baysac (2007). "Will Part D Produce Savings in Part A and Part B? The Impact of Prescription Drug Coverage on Medicare Program Expenditures." *Inquiry*, 44, 146-156.

Table 1: Sample means

	NLSY	MEPS
Person/period observations	30,877	69,820
Checkup?	0.82	0.10
Curative doctor visits	0.52	0.57
Curative doctor visit spending (2007 dollars)	—	123.5
Outpatient hospital visits	—	0.06
Outpatient hospital spending (2007 dollars)	—	47.6

Table 2: Correlations between checkup and curative usage

	Checkup	Checkup$_{t-1}$	Checkup$_{t-2}$	Checkup$_{t-3}$
NLSY: Curative doctor visits	.172	.122	.116	.102
MEPS: Curative doctor visits	.057	.077	.081	.054
MEPS: Doctor visit spending	.126	.064	.053	.054
MEPS: Outpatient hospital visits	.024	.022	.016	.020
MEPS: Outpatient hospital spending	.039	.024	.011	.025

All correlations differ from zero at the .05 level.

Table 3: Estimates of lag adjustment equations
(Standard errors in parentheses)

Survey:	NLSY	MEPS	MEPS	MEPS	MEPS
Δy_t:	Δcurative visits$_t$	Δcurative visits$_t$	Δdoctor spending$_t$	Δhosp visits$_t$	Δhosp spending$_t$
checkup$_t$	0.182**	−0.198**	166.142**	0.025	35.110**
	(0.019)	(0.048)	(16.431)	(0.017)	(14.028)
checkup$_{t-1}$	−0.142**	0.161**	−100.883**	0.021	4.586
	(0.020)	(0.044)	(15.059)	(0.016)	(12.843)
checkup$_{t-2}$	−0.062**	0.141**	−111.085**	−0.035**	−52.502**
	(0.019)	(0.044)	(15.005)	(0.016)	(12.795)
checkup$_{t-3}$	−0.034*	−0.123**	−16.171	0.016	18.646
	(0.019)	(0.045)	(15.636)	(0.016)	(13.347)
Δy_{t-1}	−0.434**	−0.495**	−0.438**	−0.407**	−0.512**
	(0.007)	(0.006)	(0.005)	(0.007)	(0.005)
constant	−0.003	−0.064**	−21.974**	0.009*	−11.133**
	(0.015)	(0.015)	(5.280)	(0.005)	(4.508)

* significant at .10 level

** significant at .05 level

Figure 1: Cumulative adjustments to curative healthcare usage after checkup (with 95% confidence bands)

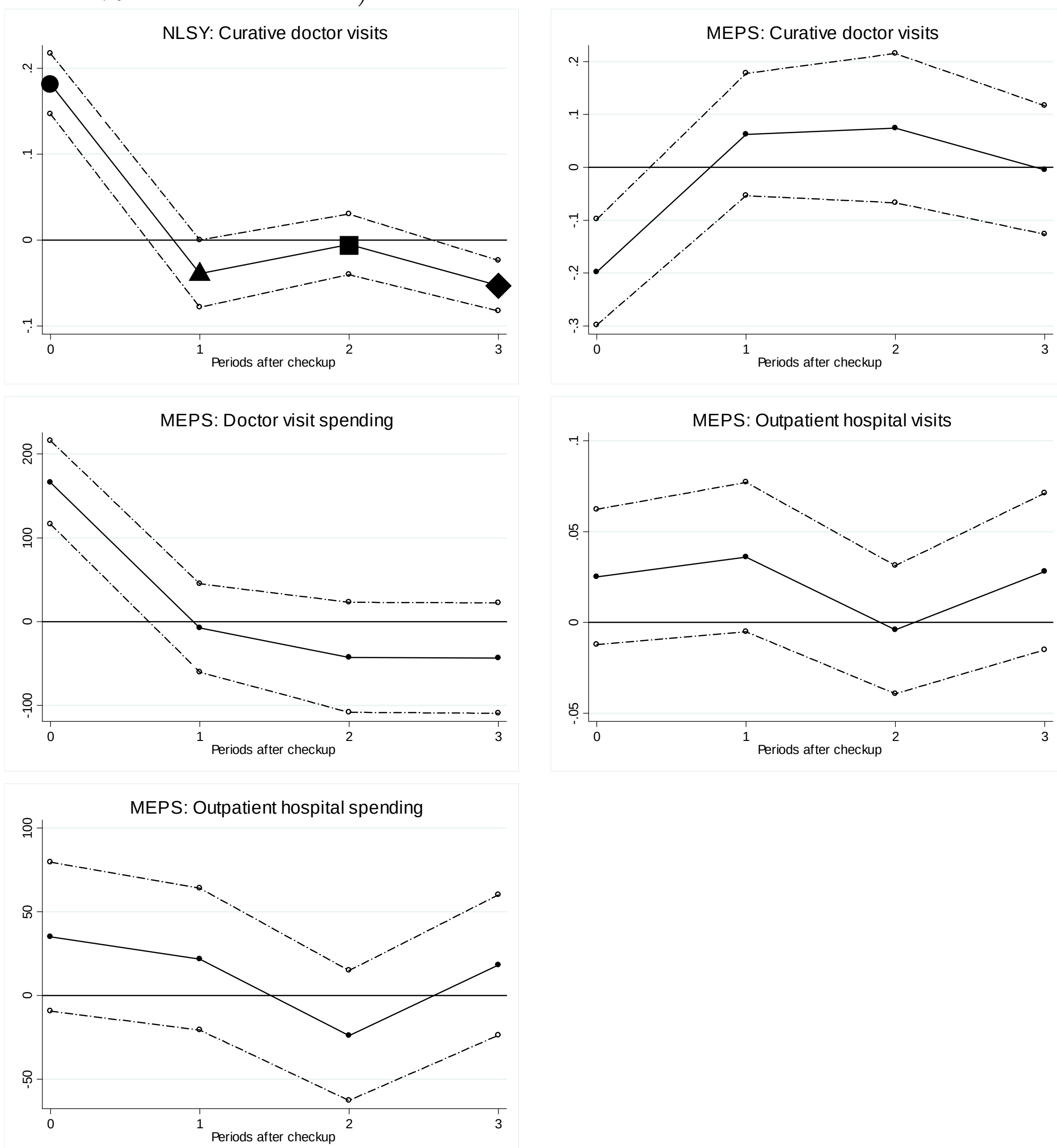

Figure 2: Adjustments to curative healthcare usage after checkup
(with 95% confidence bands)
FEMALES

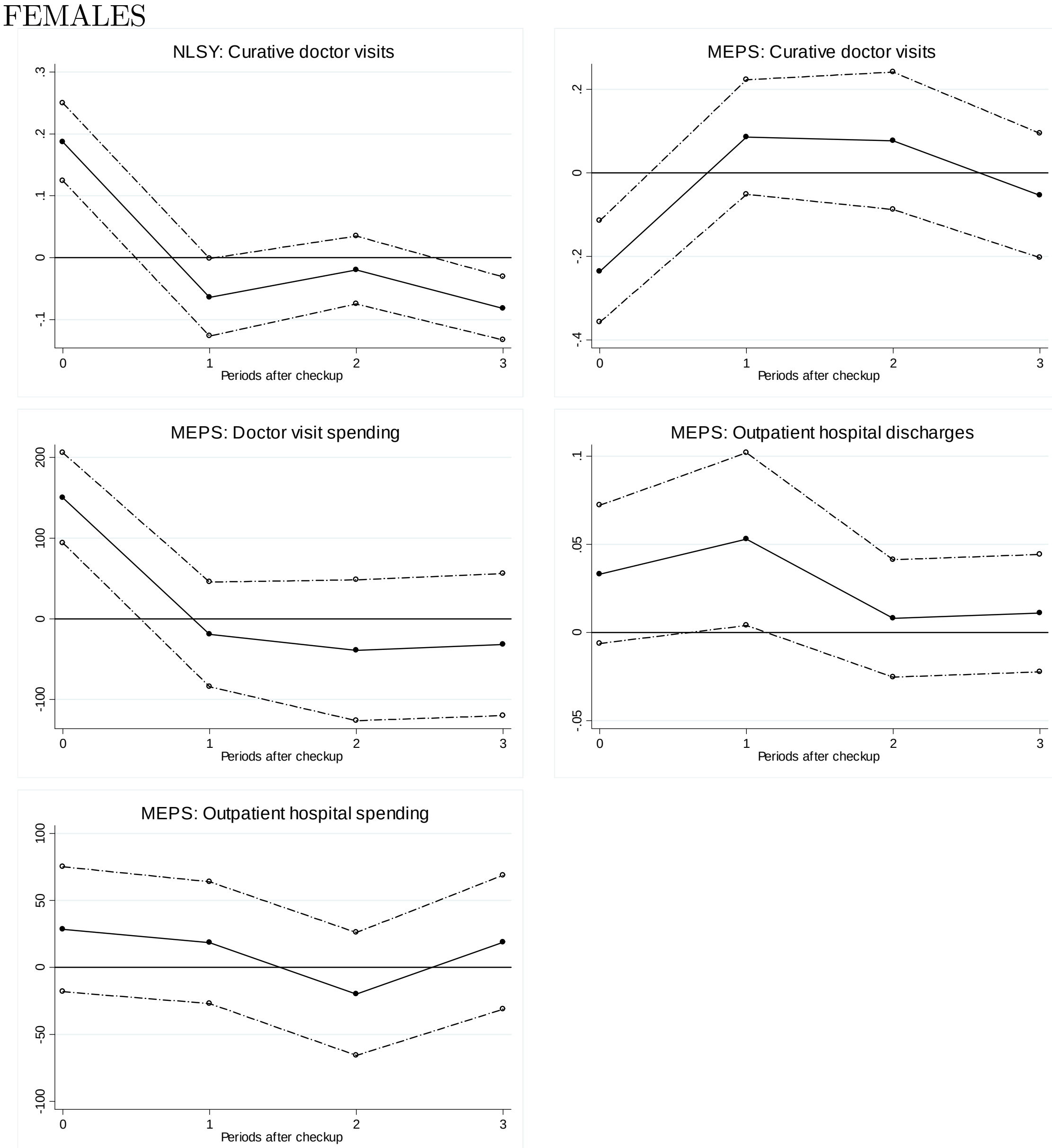

Figure 3: Adjustments to curative healthcare usage after checkup
(with 95% confidence bands)
BLACKS and HISPANICS

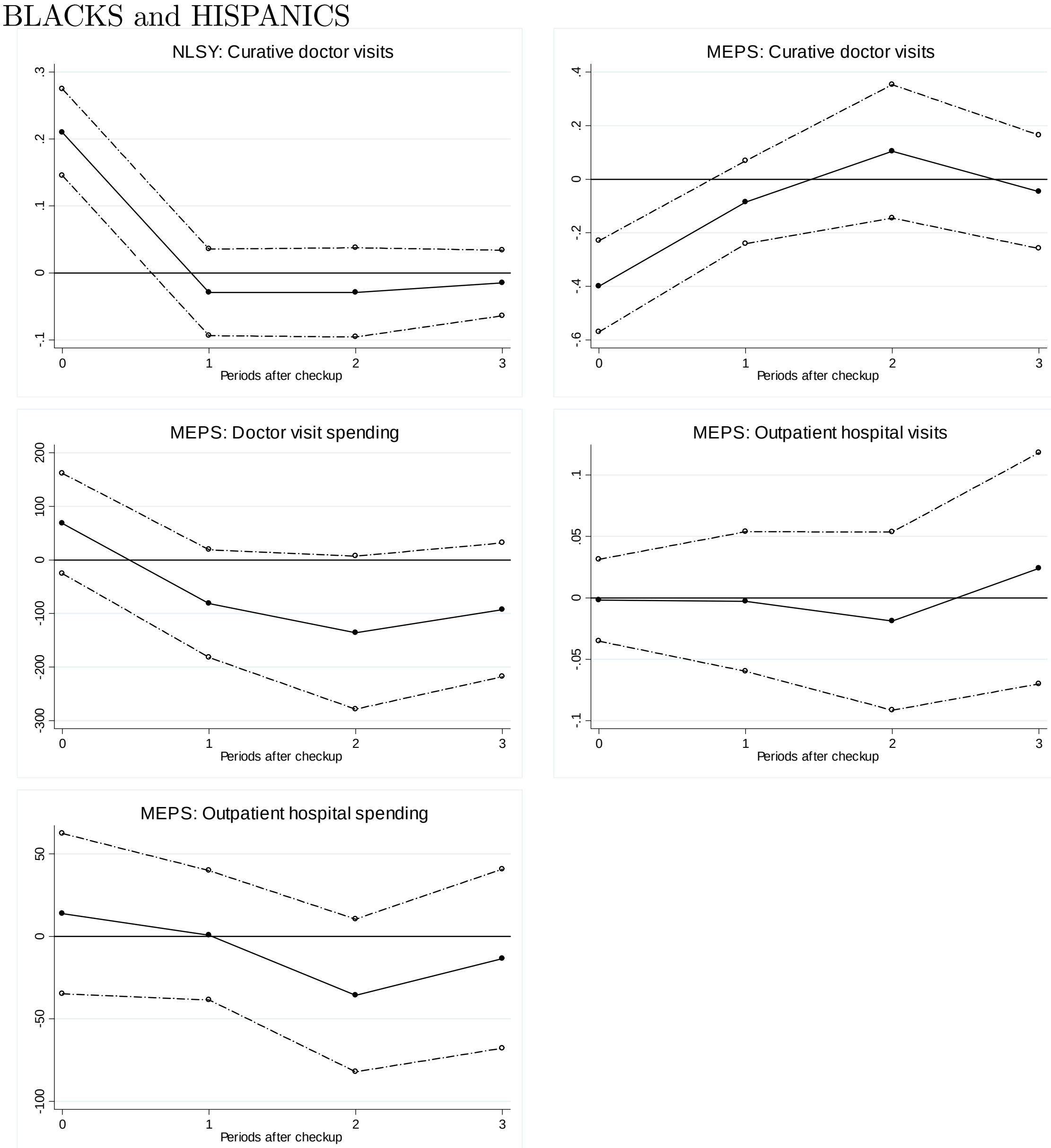

Figure 4: Adjustments to curative healthcare usage after checkup
(with 95% confidence bands)
<200% of FEDERAL POVERTY LINE and FAIR or POOR HEALTH

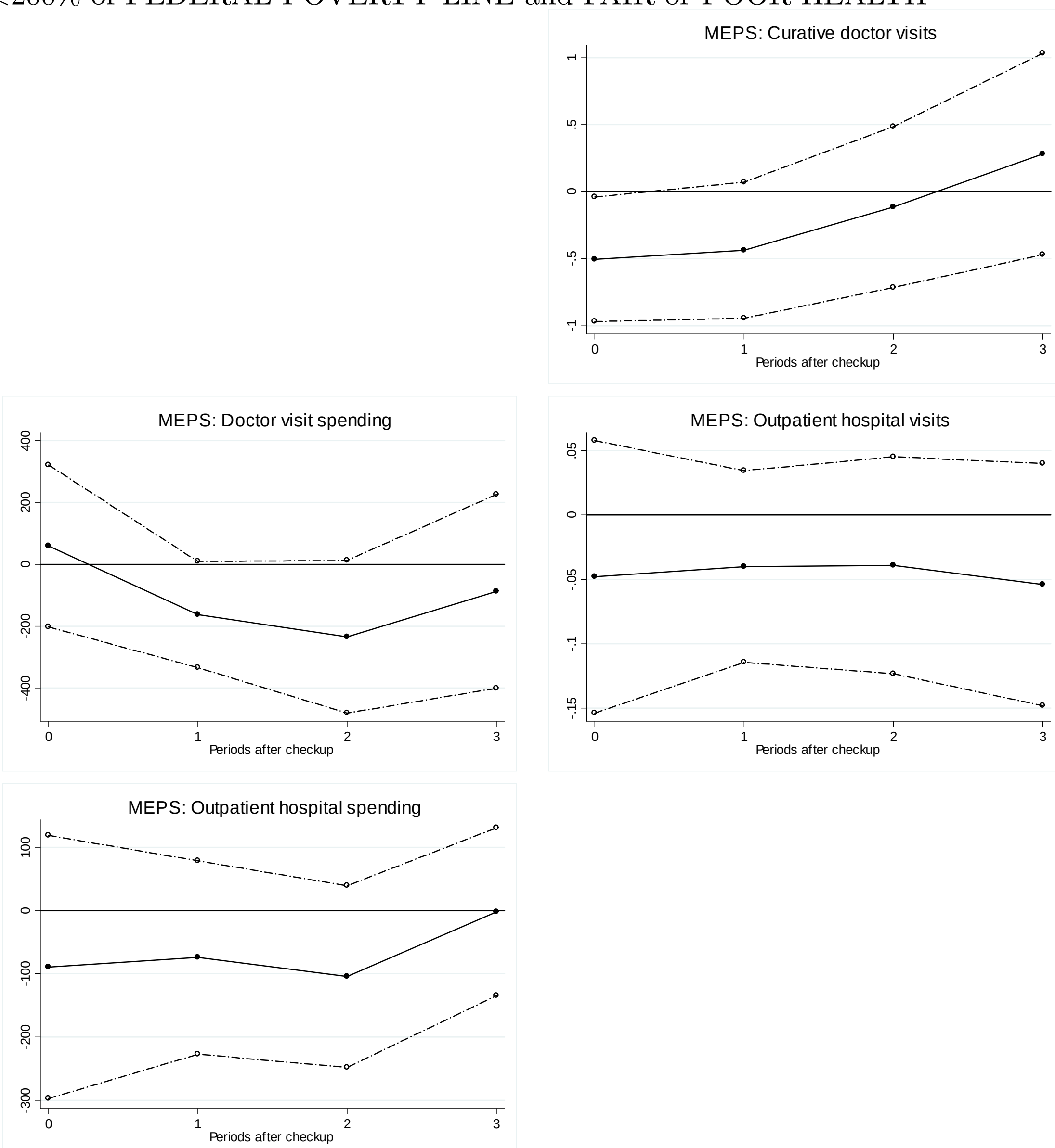

Figure 5: Adjustments to curative healthcare usage after checkup
(with 95% confidence bands)
TWO STAGE LEAST SQUARES