# MATERNAL BED REST AND INFANT HEALTH

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## ABSTRACT

Negative shocks to early child health influence both short- and long-run outcomes. Interventions prior to birth, rather than early in childhood, may reduce the probability or the severity of the shock and may be relatively cost effective. One such intervention, bed rest during pregnancy, is frequently prescribed to reduce the likelihood of preterm birth. Premature infants experience worse average initial infant health than full-term infants and these differences may persist into adulthood without mediation. Recent medical literature on the efficacy of bed rest is mixed, but tilts towards a negative or zero relationship between bed rest and infant health, suggesting that this intervention does not improve initial health status. This is not surprising given the difficulty of navigating a crucial threat to identification: conditions leading to bed rest recommendations are correlated with infant health. We investigate this question using Pregnancy Risk Assessment Monitoring System (PRAMS) data. Among women who experience a medical issue during pregnancy, we compare outcomes of infants born to mothers who are observationally similar yet differ in whether they are recommended bed rest. Using OLS and matching methods, we find a positive relationship between bed rest and the likelihood of low birth weight and prematurity (consistent with prior medical literature). Yet our results also show that bed rest decreases the incidence of very low birth weight (at least −15.4 percent), very premature outcomes (at least −7.7 percent), and a reduction for infant death. We provide additional evidence that suggests bed rest shifts initial health status upward. Our back-of-the-envelope calculations suggest that when accounting for cost savings over both the short and the long run, bed rest may be more cost-effective treatment than post-birth interventions.

**KEYWORDS:** infant health, bed rest, pregnancy
**JEL CLASSIFICATION:** I12, J13

## I. Introduction

Negative shocks to early child health influence both short- and long-run outcomes (Almond and Currie 2010). Fortunately, while these effects can be substantial, the total effect can be mediated when additional investments in human capital are made. Work by Cunha and Heckman (2007) suggests that an earlier investment may actually be more cost effective than an investment achieving the same effect later, since investments are

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doi:10.1162/ajhe_a_00021
American Journal of Health Economics 1(3): 345–373

self-productive and exhibit dynamic complementarities. Interventions prior to birth, rather than early in childhood, potentially exhibit even greater cost effectiveness. One such intervention is medically prescribed bed rest during pregnancy, which is a treatment intended to reduce premature births, improve infant health, and reduce the time infants spend in the costly neonatal intensive care unit (NICU). While the medical literature often focuses on immediate health outcomes, improvement at this early stage is also extremely important for later outcomes. In this paper, we explore whether maternal bed rest during pregnancy improves infant outcomes at the usual margins (premature; low birth weight) and at more extreme margins (very premature; very low birth weight).

In the United States in 2009, just over 12 percent of all babies were born prematurely (before 37 weeks). Of these, 70 percent were late preterm births (34–36 weeks) and about 16 percent were very preterm (before 32 weeks) (Martin et al. 2011). Infant mortality is highly related to prematurity. Thirty-six percent of the infant deaths in the United States in 2007 were "preterm" related (Matthews and MacDorman 2011). The likelihood and degree of adverse health outcomes, including death, rises nonlinearly the earlier the infant is born. Although very preterm infants only account for approximately 2 percent of births, they comprise over 50 percent of infant deaths, suggesting this is a very important health margin to consider. When babies who are born prematurely survive, they are more likely than babies born full term to experience worse short- and long-run health outcomes including jaundice, respiratory distress syndrome and other breathing problems, bleeding in the brain, infection, problems controlling body temperature, trouble communicating and making sounds, vision and hearing problems, cerebral palsy and other neurological problems, intellectual disabilities, developmental delays, and feeding and digestive problems (ACOG 2011; CDC 2015). Due to these health issues, premature infants often have longer hospital stays in the NICU than full-term infants. Furthermore, it is costly to care for a child with one or more of these conditions. Lifetime costs for an individual with cerebral palsy alone, for example, are over $1 million (MMWR 2004).

There are a number of medical risk factors for premature birth, including multiple gestation, having a previous preterm birth, problems with the uterus or cervix (such as premature rupture of membrane (PROM), or incompetent cervix), preeclampsia, certain infections during pregnancy, chronic health problems in the mother (such as high blood pressure, diabetes, or clotting disorders), and adverse health behaviors before or during pregnancy (such as cigarette smoking, alcohol use, or illicit drug use) (Bigelow and Stone 2011; CDC 2015). There is additional variation in prematurity by mothers' demographic factors including race. African American women have the highest risk of premature birth. Approximately 17.5 percent of their babies are born prematurely, compared with 10.9 percent of white women, and 12.0 percent of Hispanic women (Martin et al. 2011). Despite these known correlative factors, what causes preterm birth and who will be afflicted by it is not well understood (Maloni 2010). Even so, factors that are correlated with preterm birth are used to assign individuals to treatments thought to reduce the incidence of prematurity, like bed rest.

Birth weight has also been identified as a factor in short- and long-run health and other human capital–related outcomes; individuals with lower initial birth weights exhibit greater mortality, as well as diminished educational and labor force outcomes (Behrman and Rosenzweig 2004; Black, Devereux, and Salvanes 2007; Oreopoulos et al. 2008; Royer 2009). Furthermore, infants who are born preterm and who are on the lower end of the observed birth weights for their gestational age often experience even worse outcomes, including higher infant mortality (Alexander et al. 2003).

The biggest barrier to studying the relationship between bed rest and infant health is that the very characteristic(s) that would lead a doctor to recommend bed rest may also be correlated with an infant's health. In other words, a mother's diminished health status may lead to both poor infant health and adverse pregnancy conditions that would lead her doctor to recommend bed rest. It is misleading to examine the simple difference in average infant health between infants born to women recommended and not recommended bed rest. Results of such analyses could lead researchers to conclude that bed rest is harmful when in fact women who are recommended bed rest may have, on average, infants with worse outcomes regardless of treatment. We address this potential threat to identification in several ways. First, we utilize a large set of observable characteristics, some of which are not often used in the medical literature. Second, we limit our sample to women who are at risk of a bed rest recommendation: specifically, women who report at least one of a substantial number of medical problems experienced during pregnancy. Importantly, women who experience the same pregnancy problems, and are otherwise similar, vary in whether they are recommended bed rest. This variation in assignment is likely due to ambiguity in the medical literature regarding bed rest effectiveness, which leads physicians to make different recommendations even when patients might be otherwise identical, as we further describe in our background section. Under the assumption that assignment to bed rest is random conditional on our observables, we use ordinary least squares (OLS), propensity score matching, inverse probability weighting, and entropy balancing (a preprocessing method) to compare infants born to observationally similar women who vary by whether they were recommended bed rest.

We first show that omitting individual pregnancy characteristics (pregnancy problems) from our analysis results in a strong estimated association between bed rest and negative infant outcomes. We then show that this estimated relationship changes dramatically once we condition on the pregnancy problems a woman may experience during pregnancy. Regardless of method, our main results show that bed rest decreases the incidence of very low birth weight (<1,500 grams) or very premature (<33 weeks) while at the same time increasing the likelihood of low birth weight (<2,500 grams) and prematurity (<37 weeks). We also show evidence that bed rest decreases infant death and shifts outcomes from a more severe into a less severe state across the birth weight and gestation distributions.

We begin by providing a brief background of antenatal bed rest in the United States, then document the relationship between observable characteristics and likelihood that a woman has bed rest during pregnancy. Next, we examine the relationships between bed rest and gestation or birth weight. Additionally, we offer some back-of-the-envelope calculations on potential cost savings. Last, we discuss the implications of these findings.

## II. Background on Antenatal Bed Rest

Bed rest during pregnancy varies by location, intensity, and duration. Women can be recommended bed rest to be had at home or in the hospital and it can be prescribed for a few days, a week, or longer. The degrees of bed rest typically include restricted activity (no traveling, no lifting, limited work, frequent rests, light housework), modified bed rest (allowed to be up during meals or showers), strict bed rest (allowed to go to the bathroom, short showers, lie flat on bed on left or right side).[1] In addition to varying degrees and lengths women experience, we note that for the same medical condition, doctors do not necessarily make the same bed rest recommendation (Maloni, Cohen, and Kane 1998).[2] In the data we utilize (described in more detail below), we are able to measure the prevalence and location (in the hospital or at home) but unable to discern degree of bed rest (low, moderate, strict), total length of bed rest, or to control for physician heterogeneity.

Medical professionals considering prescribing antepartum bed rest or restricted activity for a patient face a trade-off. Bed rest may improve infant health and consequently decrease infant medical care costs. At the same time, it has an adverse effect on maternal health and potentially imposes other (pecuniary or nonpecuniary) costs on the mother both during and after pregnancy.[3] In this paper, we consider whether bed rest improves *infant* health as measured by birth weight and gestation.

Up until the 1990s, almost every obstetrics textbook recommended bed rest to prevent preterm delivery (Goldenberg et al. 1994; Schroeder 1998). Common reasons for the recommendation are premature rupture of membrane; threatened abortion (vaginal bleeding); multiple gestation; hypertensive diseases of pregnancy including preeclampsia; preterm labor; fetal growth restriction; and edema (Bigelow and Stone 2011; Crowther and Han 2010; Goldenberg et al. 1994).[4] Recent figures suggest that approximately 95 percent of obstetricians use bed rest in their practice (Bigelow and Stone 2011; Sciscione 2010). Although not every woman who is pregnant would be a candidate for bed rest, prior evidence suggests that it is recommended in 18.2 percent of pregnancies (Goldenberg et al. 1994).

1 This description matches the general description in Maloni (2010) and accords with the information contained in a pamphlet given to high-risk-pregnancy patients of a Maternal Fetal Medicine unit that was provided to one of the co-authors.

2 Maloni, Cohen, and Kane (1998) conducted a survey of doctors that reveals variation across physicians in the conditions for which bed rest recommendations are made. While the survey was conducted several years before our sample period begins, it is reasonable to assume that physician behavior continues to vary in similar ways as during their survey.

3 Beginning with World War II and, more formally, with the aerospace program, research has shown bed rest (for any reason) leads to a variety of negative health consequences such as muscle atrophy, reduced bone density, weight loss, and diminished cardiovascular capacity (Bigelow and Stone 2011; Dunn, Handley, and Carter 2006; Maloni 2010; Sprague 2004; Sandler and Vernikos 1986). Additionally, research suggests that pregnant mothers also pay psychological and economic costs (Goulet et al. 2001; May 2001; Schroeder 1998). Although the PRAMS data contain a rich set of health measures during pregnancy, we are unable to examine post-birth maternal outcomes.

4 We observe these and related pregnancy problems in our data. See Table 1.

According to the large body of research we reviewed, almost every study cites Goldenberg et al. (1994) when claiming that one in five women in the United States is placed on bed rest during pregnancy.[5] Their analysis is conducted using the National Maternal and Infant Health Survey (NMIHS), a nationally representative subsample of the universe of births occurring in the United States in 1988. Since 1988, there have been large changes in the fertility of US women, in medical technology and practice, and in female labor force participation.[6] If any of these are related to the rate of bed rest, the effects on infant health could have changed, possibly dramatically, and we view an analysis utilizing more contemporary data to be relevant to this area of research.

Using the best data available, we document the rate of bed rest from the late 1970s through 2008 using three survey data sets. We begin by replicating Goldenberg's widely cited study. Using the NMIHS data, we find 18.1 percent of women who had live births in 1988 were recommended bed rest, a number very similar to the widely cited findings of Goldenberg et al. (1994). We also find that 13.0 percent quit working and 6.4 percent were hospitalized during pregnancy. These categories are not mutually exclusive, and taking into account the overlap, we find that 20.4 percent of these women experienced some sort of restriction on their activity while pregnant.

We then identified questions in the National Health Interview Survey (NHIS) completed in 1981, which asked women to report whether they were recommended bed rest during pregnancy. This information was collected for mothers of children ages five and under, and so the data correspond to births occurring from roughly 1976 to 1981. These data indicate that women experienced bed rest for approximately 11.9 percent of these births and were hospitalized during pregnancy for just under 7.3 percent of these births.

Finally, we examined bed rest for a more recent era using the Pregnancy Risk Assessment Monitoring System (PRAMS) data. Over the period 2000–08, we find an average of 19.1 percent of all women (aged 20 and older) reported experiencing bed rest (see Table 1). During this time, we observe a drop from approximately 20 percent to 16 percent for singleton births (see Figure 1).[7] Among women giving birth to singletons who reported at least one pregnancy problem (the group we focus on in this paper), approximately 29.3 percent experienced bed rest. Although these three data sets are not perfectly comparable, they suggest that in the recent period bed rest appears to be the same as or even higher than in the 1980s.[8]

Early work showed that bed rest improved infant health, but during the 1990s studies such as Maloni and Kasper (1991) began to question its efficacy. Increasingly, the

5 For example, Bigelow and Stone (2011) and Sprague (2004).

6 Some examples of the big changes in fertility include the following: the teen birth rate declined and this drop was different by race/ethnicity; the incidence of higher-order multiple births exploded, with a peak around 1998 and somewhat steady decline since; recently, the fastest growth in birth rates has occurred for women over the age of 40; the percentage of women experiencing hypertension during pregnancy has climbed noticeably during the 2000s (Martin et al. 2011).

7 This decline may be due to changes in medical practice. Despite the apparent decline, our results are quite similar when we examine the earlier (2000–03) and later (2004–08) periods of our data.

8 We note that the measurement of bed rest differs between the data sets. NMIHS and NHIS measure one week or more of bed rest, while the PRAMS Core measures two days or more of bed rest.

**TABLE 1.** Descriptive statistics

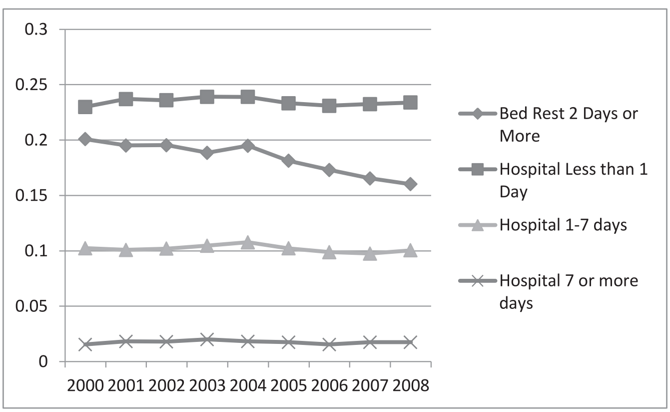
| Variable | Unconditional (1) Full sample Mean | Unconditional (2) Singleton Mean | Unconditional (3) Multiples Mean | Conditional on pregnancy problems (4) Singletons Mean | Conditional on pregnancy problems (5) Multiples Mean |
|---|---|---|---|---|---|
| *N* | 288,062 | 273,009 | 15,053 | 189,251 | 13,500 |
| Home bed rest 2 days or more | 0.191 | 0.186 | 0.431 | 0.293 | 0.508 |
| Hospital less than 1 day | 0.240 | 0.238 | 0.356 | 0.375 | 0.419 |
| Hospital 1–7 days | 0.107 | 0.103 | 0.326 | 0.162 | 0.384 |
| Hospital 7 or more days | 0.020 | 0.018 | 0.137 | 0.028 | 0.161 |
| Premature | 0.090 | 0.080 | 0.626 | 0.116 | 0.689 |
| Low birth weight | 0.069 | 0.059 | 0.590 | 0.082 | 0.632 |
| Very preterm | 0.024 | 0.020 | 0.230 | 0.030 | 0.261 |
| Very low birth weight | 0.012 | 0.010 | 0.112 | 0.016 | 0.126 |
| Maternal age 20–24 | 0.273 | 0.275 | 0.179 | 0.296 | 0.187 |
| Maternal age 25–29 | 0.310 | 0.311 | 0.273 | 0.310 | 0.273 |
| Maternal age 30–34 | 0.261 | 0.260 | 0.323 | 0.246 | 0.322 |
| Maternal age 35–39 | 0.128 | 0.127 | 0.176 | 0.121 | 0.172 |
| Maternal age 40 + | 0.027 | 0.027 | 0.049 | 0.026 | 0.046 |
| Education less HS | 0.136 | 0.136 | 0.099 | 0.149 | 0.100 |
| Education HS | 0.294 | 0.295 | 0.263 | 0.313 | 0.267 |
| Education some college | 0.249 | 0.249 | 0.239 | 0.260 | 0.242 |
| Education college or more | 0.321 | 0.320 | 0.399 | 0.278 | 0.390 |
| Married | 0.701 | 0.700 | 0.742 | 0.672 | 0.738 |
| Black | 0.149 | 0.149 | 0.166 | 0.165 | 0.172 |
| Medicaid | 0.350 | 0.351 | 0.296 | 0.385 | 0.302 |
| Private insurance | 0.567 | 0.566 | 0.640 | 0.532 | 0.640 |
| Previous birth | 0.626 | 0.627 | 0.590 | 0.634 | 0.581 |
| Previous LBW birth | 0.063 | 0.062 | 0.076 | 0.073 | 0.079 |
| Previous premature birth | 0.071 | 0.070 | 0.082 | 0.089 | 0.090 |
| Maternal smoking during pregnancy | 0.103 | 0.103 | 0.098 | 0.119 | 0.098 |
| Maternal drinking during pregnancy | 0.069 | 0.069 | 0.054 | 0.062 | 0.050 |
| Multiple | 0.018 | 0 | 1 | 0 | 1 |
| Male | 0.507 | 0.507 | 0.501 | 0.505 | 0.500 |

**TABLE 1.** *Continued*

| Variable | Unconditional | | | Conditional on pregnancy problems | |
|---|---|---|---|---|---|
| | (1) Full sample Mean | (2) Singleton Mean | (3) Multiples Mean | (4) Singletons Mean | (5) Multiples Mean |
| Labor pains | 0.231 | 0.225 | 0.538 | 0.355 | 0.634 |
| High BP | 0.147 | 0.145 | 0.250 | 0.229 | 0.295 |
| Vaginal bleeding | 0.158 | 0.156 | 0.245 | 0.246 | 0.289 |
| Placenta problems | 0.054 | 0.054 | 0.080 | 0.085 | 0.094 |
| Nausea/vomiting | 0.274 | 0.272 | 0.356 | 0.430 | 0.419 |
| Gestational diabetes | 0.097 | 0.097 | 0.123 | 0.152 | 0.145 |
| Infection | 0.171 | 0.171 | 0.176 | 0.270 | 0.208 |
| PROM | 0.052 | 0.048 | 0.227 | 0.076 | 0.268 |
| Cervix sewn shut | 0.018 | 0.017 | 0.053 | 0.027 | 0.062 |
| Blood transfusion | 0.005 | 0.005 | 0.024 | 0.008 | 0.029 |
| Car accident | 0.017 | 0.017 | 0.012 | 0.027 | 0.015 |

Data source: PRAMS Core, 2000–08.
Notes: Descriptive statistics are limited to women aged 20 and older and weighted using survey weights. Includes observations from 35 states and NYC covering the 2000–08 period (but not all states participate in every year). Columns 1–3 represent the unconditional sample of all births, singleton births, and multiple births, respectively. Columns 4 and 5 are conditional on pregnancy problems for singletons and multiples, respectively.

**FIGURE 1.** Types of bed rest over time, PRAMS Core




Data source: PRAMS Core, 2000–08.
Note: Figures are conditional on maternal age 20 and older and singleton births, and include sampling weights.

academic and medical literature has questioned the evidence on the effectiveness of bed rest in reducing prematurity (Allen, Glasziou, and Del Mar 1999; Bigelow and Stone 2011; Crowther and Han 2010; Josten et al. 1995; Maloni, Cohen, and Kane 1998; Maloni and Kasper 1991; Schroeder 1998; Sosa et al. 2004). Despite the large body of research questioning its effect, reviews of the literature continue to uncover contradictions in findings. Most prominently, in 2003, the American Congress of Obstetricians and Gynecologists (ACOG) Practice Bulletin contained a recommendation (reaffirmed in 2008) that bed rest should not be routinely used to delay preterm birth (ACOG 2003). This recommendation, however, was a "type B" recommendation, which indicates that it is based on *limited or inconsistent* scientific evidence, suggesting that further exploration of this topic is warranted. More recently, in a critique of bed rest, Sciscione (2010) acknowledges that the benefits of bed rest are unknown. While some providers, like Maloni, are of the opinion that bed rest is not effective in improving infant health, other doctors feel that bed rest is an effective treatment and some have even gone as far as obtaining a court order to have a woman take bed rest in the hospital.[9] The great uncertainty regarding the benefits (or harms) to the fetus results in a situation where two providers, faced with the same patient, would make different bed rest recommendations, which is also what Maloni et al. (1998) found in their survey.[10]

The mixed findings in the medical literature, which fuels the uncertainty regarding the effectiveness of this treatment, are not at all surprising when considering the confounding effect of mother's health, which makes cleanly identifying the effect of bed rest so difficult. That the same patient may receive different recommendations, even when presenting with the same observable problems during pregnancy, suggests we may be able to tease out the causal effect of bed rest on infant health using common econometric techniques. Furthermore, as Maloni (2010) points out, there are few studies on US pregnancies that compare outcomes for women who have high-risk pregnancies and do not have bed rest with those who do (either at home or in the hospital). In this paper, we leverage the rich individual-level information available in the PRAMS to compare women who do not experience bed rest who are observationally similar to women who do experience bed rest.

## III. Data

We use the Pregnancy Risk Assessment Monitoring System (PRAMS) data for the period 2000–08 in this analysis. It is a population-based data set that records information on maternal experiences before, during, and after pregnancy, including maternal bed rest during pregnancy. We rely on maternal responses to survey questions regarding bed rest rather

9 Source: http://www.floridachildinjurylawyer.com/2010/01/florida_court_orders_pregnant.html, accessed on July 30, 2014.

10 We describe their survey in footnote 2. Several other reasons we may see variation in recommendations are the following: (1) defensive medicine, which could lead some doctors to recommend bed rest, fearing a medical malpractice lawsuit if they do not; (2) no direct financial benefit from prescribing bed rest and doctors may be more inclined to ignore the additional harms (to the mother from an employment or family perspective) or more inclined to prescribe more profitable medical interventions; (3) recommendations made so that the patient can do "all she can" (we thank an anonymous referee for identifying this).

than medical records because there is not a specific diagnostic code assigned to home bed rest. As with the use of any survey, we acknowledge that responses may be subject to reporting bias.

The PRAMS is a subsample of the National Vital Statistics (VS) data. The VS data contain information available on the standard birth certificate for the universe of US births. VS data include many useful measures including maternal and paternal characteristics, and birth outcomes. The PRAMS data include the standard birth certificate data along with additional pregnancy-related information. Some measures we use are PRAMS-specific questions and some are birth certificate measures. For example, both the birth certificate data and the PRAMS data collect information on maternal smoking and maternal drinking, but these measures are reflective of different periods of time (before, during, or after pregnancy).

The PRAMS data are collected by individual states, but not all states participate in PRAMS (and not all of those that participate do so in every year). PRAMS-participating states sample approximately 1,300–3,400 women who give birth in the survey year. We utilize data primarily between 2000 and 2008 for two reasons: (1) this maximizes the number of participating states, and (2) questions on maternal bed rest began in the 2000 survey questionnaire and ended in the 2008 survey questionnaire. PRAMS collects specific information about maternal bed rest during pregnancy and hospital stays during pregnancy, and the data are representative of their respective state and comparable across states.

The PRAMS survey is conducted using two different questionnaires. The Core questionnaire is identical across states within each survey wave. The Standard questionnaire is customized by each state and varies across states within and between waves. We utilize questions from the Core survey in our analysis. We considered using some PRAMS Standard questions in our analysis. A small subset of the PRAMS Standard (two states) asked additional bed rest questions during the 2000–08 period that would have specifically allowed us to measure physician-recommended bed rest and patient compliance. These included Maryland (2001–03) and New York (2000–08).[11] Because these data do not span a wide enough time or geographic dimension, in particular the small number of years with overlap (2001–03), and measure bed rest differently (the Standard measure is bed rest of at least a week which could be prescribed at home or in the hospital), we felt that there may be limited external validity in these results and decided against using these data for the analysis of bed rest on infant health.

PRAMS Core questions allow us to identify several types of bed rest during pregnancy, conditional on having experienced at least one problem during pregnancy. Women are first asked whether they experienced any of the following problems: preterm/early labor, high blood pressure, vaginal bleeding, early water breaking, problems with placenta, nausea/vomiting/dehydration, gestational diabetes, kidney or bladder infection, PROM, cervix needing to be sewn closed, blood transfusion, or involvement in a car accident. Women who report any of these problems are then asked whether they experienced the following: home bed rest for at least two days, hospital bed rest for less than one day,

11 Vermont also asked these questions in the PRAMS Standard. Vermont, however, does not report maternal race information, which we use in our main models.

hospital bed rest for between one and seven days, and hospital bed rest for seven days or more.[12] In what follows, we focus on the measure of home bed rest for at least two days.[13] For this measure, because women who did not indicate that they experienced any pregnancy problems were not asked about bed rest recommendations, our estimate of population prevalence of bed rest recommendations is a lower bound on the true prevalence, since it is possible that some women who did not report experiencing problems during their pregnancy may have been recommended bed rest.[14] Using the PRAMS Standard, a small subset of the main PRAMS Core data which asks all women in the subsample about bed rest of one week or more (not conditionally on pregnancy problems), we find that among women who report zero pregnancy problems, approximately 3.5 percent report bed rest recommendations.[15,16] This suggests that this survey questionnaire issue is of limited concern.

The PRAMS data also include infant outcomes. We observe gestational age at birth and birth weight, but PRAMS reports these measures as ranges, not as continuous measures. Gestational age is reported in the following ranges: less than 27 weeks, 28–33 weeks, 34–36 weeks, 37–42 weeks, and 43 or more weeks. We define a binary variable "premature" as having a gestation of less than 37 weeks, and "very premature" as having a gestation of less than 33 weeks.[17] Birth weight is recorded as the midpoint of the corresponding range.

12 Conditional on answering yes to any of the pregnancy problems in the prior question, the bed rest question in the PRAMS Core reads, "Did you do any of the following things because of these problems? I went to the hospital or emergency room and stayed less than 1 day; I went to the hospital and stayed 1 to 7 days; I went to the hospital and stayed more than 7 days; I stayed in bed at home more than 2 days because of my doctor's or nurse's advice."

13 We are unable to consider the total length of bed rest using the PRAMS data. This dimension would be an interesting area for future research.

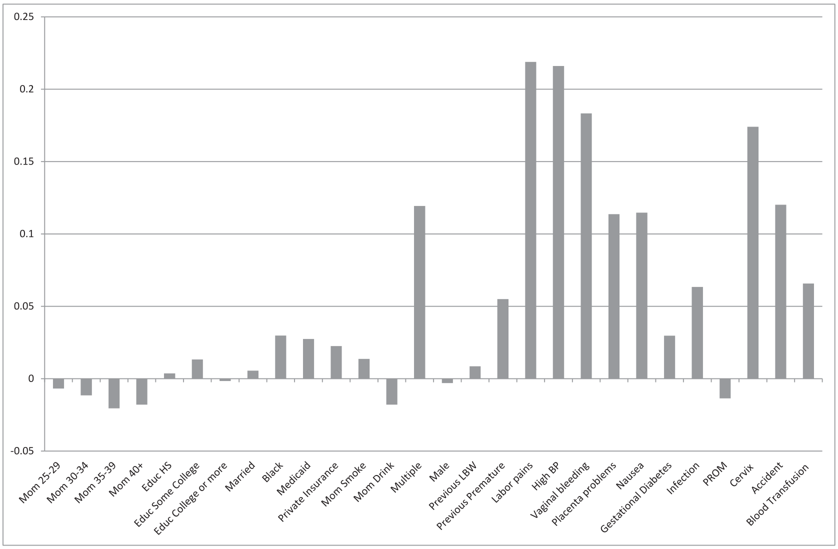
14 In the PRAMS Core, only mothers who report experiencing pregnancy problems are asked about bed rest. When we compute our summary statistics (Table 1) and examine the factors correlated with bed rest (Figure 2) using the PRAMS Core, we assign women without pregnancy problems, who were not asked about bed rest, as having no bed rest. We acknowledge that this may introduce bias into our analysis, but given the small percentage of women who have bed rest without reporting pregnancy problems, this bias is likely too small to greatly influence the estimates. Furthermore, it is unclear which direction the bias would be. If these women experienced bed rest but we do not observe it and bed rest negatively affects health, we would be overestimating the effect in our present analysis. On the other hand, if they were recommended bed rest proactively and it kept them from having pregnancy problems, we would be underestimating the effect in our present analysis.

15 The PRAMS Standard data that we use contain information for only two states: New York and Maryland. Since New York represents approximately 70 percent of the Standard data, the value presented here may have limited external validity.

16 In the National Maternal Infant Health Survey (1988), we observe 17.7 percent of mothers over age 20 being recommended bed rest. On a sample as similar as possible to the unrestricted PRAMS Core, among women having nonmultiples and experiencing no pregnancy problems, 6.0 percent report bed rest. We caution that this data set is not directly comparable to the PRAMS Core since the collection of pregnancy problem information in the NMIHS occurs only if bed rest is reported, the opposite issue as in the PRAMS Core.

17 A "very premature" birth is typically defined as less than 32 weeks, but the PRAMS data require us to use less than 33 weeks because of the grouped nature of the data.

**FIGURE 2.** Correlates of bed rest during pregnancy




Data source: PRAMS Core, 2000–08.

Notes: Universe is all births (singletons and multiples) for mothers aged 20 and older. Bars represent coefficient estimates from a linear probability model using survey weights. The omitted age group is women age 20 to 24. In the PRAMS Core, only women who indicated at least one problem during pregnancy are asked about subsequent bed rest and/or hospitalizations. For this figure, women who did not respond as well as women who were not asked these questions are coded as not having bed rest. All coefficients are statistically significant (at 10 percent level or higher) with the exception of educ HS, educ college or more, and male.

For example, the reported value "2,625" corresponds to the actual birth weight falling in the range of 2,500–2,749 grams. We define a binary variable "low birth weight" as having a birth weight of less than 2,500 grams, and "very low birth weight" as less than 1,500 grams. The available sample using the PRAMS data (given nonmissing information on our variables of interest) for the Core data includes 35 states over the 2000–08 period (depending on the state).[18] Summary statistics and sample sizes are contained in Table 1, with separate columns for the sample of all women (singletons and multiples) and for the sample who report at least one pregnancy problem.

In our main analysis, we limit the sample to women experiencing at least one pregnancy problem. Clearly, the PRAMS Core data we use for our analysis forces us to limit the data in this way. We are not overly concerned by this because, as we mention above, the likelihood of receiving a bed rest recommendation is very small unless some pregnancy problem is observed. As we show in Figure 2, described below, pregnancy problems (and

18 The sample includes 35 states and New York City. Not all states participate in every year, but the included states participated during some year(s) between 2000 and 2008.

multiple births) are the main factors associated with bed rest. Even if we had bed rest information on women who do not report pregnancy problems, we would most likely continue to focus our main analysis on women with pregnancy problems since including women without pregnancy problems makes it more difficult to identify the effect of bed rest on infant health. For example, in one set of estimates we utilize propensity score methods, which require common support for observations being compared. If we include women without pregnancy problems (and assign them zeroes for bed rest), we would have little (or no) common support for the non-pregnancy-problem observations, and hence less confidence with analysis performed in this way.

We additionally limit the sample to respondents aged 20 and older and to women with singleton births. Births are more likely on average to be unintended (74 percent versus 39 percent) for teens relative to older women, and, consequently, teens are more likely to initiate prenatal care after the first trimester (28 percent versus 11 percent).[19] Multiple birth pregnancies are treated differently, and are more likely to result in premature gestation and low birth weight (Cleary-Goldman et al. 2004). Furthermore, a large fraction of respondents with multiple birth pregnancies were recommended bed rest, which makes it difficult to find common support among these respondents (see Table 1).

## IV. Methods

This analysis uses the previously described PRAMS Core data set in a multipart analysis. We start by considering the correlates of bed rest and then examine the effects of bed rest on infant health using several different methods.

### A. CORRELATES OF BED REST

First, we examine the factors correlated with bed rest among all women (singletons and multiple births) available to us in the PRAMS Core as described in equation 1:

$$BedRest_{ist} = \beta_0 + \beta_1 X_{ist} + \theta_s + \tau_t + \varepsilon_{ist} \qquad (1),$$

where $i$ indexes the individual mother, $s$ indexes the state, and $t$ indicates the year. $BedRest_{ist}$ represents maternal report of bed rest at home. The vector $X_{ist}$ contains covariates suggested by the prior literature including maternal age, maternal education, marital status, race, insurance status, pregnancy problems, drinking and smoking behavior during pregnancy, multiple birth, infant sex, previous birth, previous low birth weight birth, and previous premature birth.[20] We use state fixed effects to control for regional differences

19 Authors' tabulation using the PRAMS Core data and appropriate survey weights.

20 As footnoted previously, in our PRAMS data Vermont does not release information on maternal race, and therefore, we have chosen not to include Vermont in our analysis. All other PRAMS participating states in our data set provided information on maternal race.

in bed rest and year fixed effects to control for general trends in bed rest. Equation 1 is estimated using OLS and appropriate survey weights.[21]

### B. INFANT HEALTH OUTCOMES

We then consider the effect of bed rest on infant outcomes including gestation at birth and birth weight using the PRAMS Core. The PRAMS Core allows us to look at maternal reported bed rest at home using a large sample of women over the 2000–08 period. As described in the data section, we limit our sample to women experiencing problems during pregnancy, who give birth to singletons, and who are age 20 or older.[22] We recognize that answering our research question could be problematic because of confounding factors. Women who are more likely to receive bed rest recommendations may also be more likely to have infants born at earlier gestations or lower birth weights. Any regression of bed rest on infant health outcomes may suffer from bias if the recommendation of bed rest is correlated with an unobserved variable, presumably mother's health, which is also correlated with worse infant health. If one can proxy for mother's health using a rich set of observables, the bias on the bed rest coefficient estimate will be reduced or even eliminated, meaning that a standard regression analysis may give reasonable estimates of the causal effect of maternal bed rest on infant health. In our analyses, we utilize a large set of maternal characteristics and pregnancy problem measures, factors that studies of infant health and health behavior often include.

We first estimate the effect of bed rest on infant health using a standard ordinary least squares regression of the following form:

$$Y_{ist} = \beta_0 + \beta_1 BedRest_{ist} + \beta_2 X_{ist} + \theta_s + \tau_t + \varepsilon_{ist} \qquad (2),$$

where $i$ indexes the individual mother, $s$ indexes the state, and $t$ indicates the year. The variable $Y_{ist}$ represents one of four binary infant outcomes: premature (<37 weeks); very premature (<33 weeks); low birth weight (<2,500 grams); very low birth weight (<1,500 grams). $BedRest_{ist}$ represents maternal report of bed rest at home for two days or more. The vector $X_{ist}$ contains covariates as described for equation 1 except multiple birth, since we limit our focus to singleton births. As with our correlates regression, we include state and year fixed effects and utilize appropriate survey weights.[23]

We next estimate the effect of bed rest on infant health using matching methods. Matching requires finding a control observation that closely matches the treatment observation in every observable way except for the assignment of the treatment. Ideally, we would match covariate by covariate using a large number of covariates. In practice,

21 In these models, we utilize the *svy* command in Stata and the appropriate weights and strata as specified by CDC PRAMS.

22 We also estimated a model (not shown) where we additionally conditioned on whether the woman had any insurance (private insurance or Medicaid), and the results were similar. We thank an anonymous referee for this suggestion.

23 For equation 2, the results were similar when we ran the model with a linear time trend, a quadratic time trend, or state-specific time trends.

however, this is often computationally intractable with a large number of covariates. Fortunately, as shown by Rosenbaum and Rubin (1983), when considering the average effect of a treatment on the treated (ATT), as long as treatment is assigned randomly conditional on observables, an unbiased estimate of the treatment effect can be obtained using a propensity score (instead of the full set of covariates) to match treatment and control variables.

We utilize several propensity score matching strategies for obtaining an estimate of the causal effect of bed rest on infant health. The propensity score model is desirable in our case because it is likely that treatment (bed rest) is assigned based on observable medical conditions but that women with the same conditions may or may not be assigned bed rest, as we described in our background section. The propensity score model generates a score based on observable characteristics and matches treatment and control observations based on this score such that after conditioning on the score, assuming the conditional independence assumption holds, the assignment to treatment, in our case bed rest, is effectively random. Using the PRAMS Core data, we estimate the effects of maternal bed rest on infant outcomes using several propensity score methods (PSM). PSM can offer causal estimates of a treatment effect, conditional on the researcher having access to the observable characteristics that physicians also observe and upon which they decide treatment, and that those observables are important for infant outcomes. PRAMS allows us to use reported pregnancy problem indications as predictors of bed rest, which certainly include the key medical factors that physicians use to make their bed rest recommendations.[24]

In what follows, we first predict the likelihood of bed rest as a function of observable maternal and other individual-level characteristics according to a logit regression (i.e., the treatment model) of the following form:

$$\Pr(BedRest_{ist} = 1|X_{ist}) = F(\beta_0 + \beta_1 X_{ist}) \qquad (3),$$

where variables are defined similarly to equation 1 except that we slightly alter the vector of covariates ($X_{ist}$) here to match those characteristics that are particularly important for the application of treatment, that is, a bed rest recommendation. These predictors include maternal age, maternal education, marital status, race, insurance status, pregnancy problems, drinking and smoking behavior during pregnancy, previous birth, previous low

24 The PRAMS data do not contain any information about the physicians who provided care to the women in our sample. We do not believe this materially affects our identification since our identification relies on two physicians providing different recommendations when seeing an identical patient (which we show to be the case in Figure 3). If we assume that higher-quality physicians are more likely to follow recent developments in the literature, and if these physicians are then less likely to recommend bed rest (due to the recent medical literature) and more likely to recommend other treatments that would be health improving (because they are higher quality), then omitting these physician characteristics would actually attenuate our estimates (if the better-quality care increases the average health of the population not receiving bed rest recommendations). Even so, there is a degree of uncertainty regarding the effectiveness of bed rest, such that high-quality physicians may recommend bed rest in similar ways as low-quality physicians, in which case there would be no bias from unobserved physician characteristics.

birth weight birth, and previous premature birth.[25, 26] Estimates of equation 3 are used to produce a propensity score, which we then use to match mothers. Next, we estimate the treatment effect (i.e., the outcome model) using a regression similar to equation 2 but where the observations are matched (or weighted) by the score computed as in equation 3.

We first use the nearest neighbor propensity score matching approach (with one nearest neighbor). Using Stata 13 and *teffects*, we are able to calculate corrected Abadie-Imbens standard errors, since bootstrapping procedures are inappropriate in the context of nearest neighbor matching.[27] This model yields an estimate of the average treatment effect on the treated (ATT). In addition to the treatment effect obtained using this procedure, we also provide information on the propensity score estimation, establishment of common support, covariate balance, and bias reduction when comparing the matched and unmatched samples.

Second, we present a variant of the propensity score model: specifically an inverse probability weighted model, with regression adjustment (IPWRA).[28] The IPWRA model utilizes the propensity scores to essentially create a weighting scheme based on one respondent's likelihood of treatment. The IPWRA allows us to further control for differences in the treatment and control group in a regression framework. In what follows, our IPWRA models utilize logit treatment models for estimation of the inverse probability weight. Additionally, we present both linear and logit outcome models with respect to infant health outcomes. We include the same covariates used in the treatment model in the IPWRA model.

Finally, we employ a relatively new alternative approach known as entropy balancing (Hainmueller 2011) that is a generalization of the classic propensity score matching method.[29] Entropy balance is similar in concept to inverse probability weighting, but is a data preprocessing procedure that reweights the data prior to outcome analysis. While PSM techniques are difficult in practice because the researcher manually manipulates the treatment equation in the hopes of achieving covariate balance, entropy balancing achieves covariate balance in a different way. Entropy balance "directly incorporates covariate balance into the weight function that is applied to the sample units" (Hainmueller 2011, 26).

25 We are not able to measure maternal employment in the PRAMS. When we explore the importance of education (our closest proxy to employment), we find that our estimates are nearly identical whether we include or exclude education. If we were able to measure employment directly, we would only be concerned if women who are employed are more likely to experience bed rest, since women who participate in the labor force are inherently healthier than women who do not (Cai 2010).

26 Propensity score estimation and balance is often improved by including higher-order terms and/or interactions. We are unable to include higher-order terms because of the discrete nature of our data. We explored the inclusion of a variety of interaction effects in the propensity score estimation, such as interactions between each pregnancy problem and maternal age, race, smoking behavior, and drinking behavior, and found that balance was not further improved with these additional controls. We instead use the model described above in equation 3 in what follows.

27 Stata 13, *teffects psmatch nneighbor(1)*; Abadie and Imbens (2006, 2008, 2011).

28 Stata 13, *teffects ipwra.*

29 User-written Stata code *ebalance* by Jens Hainmueller and Yiqing Xu, downloaded from http://ideas.repec.org/c/boc/bocode/s457326.html on August 15, 2013.

In this way, since covariate balance is determined in the first step, there is no need to confirm covariate balance later. Similar to inverse probability weighting, entropy balance allows the researcher to reweight the control group data, while using information about the moments of covariate distribution (in our case, the mean).[30]

## V. Results

### A. DESCRIPTIVE DATA

Table 1 contains the summary statistics for relevant variables in the PRAMS Core, reflective of the populations and subpopulations we utilize for analysis. Columns 1 to 3 present summary statistics for the full sample, singletons, and multiples, respectively.[31] Approximately 19.1 percent of the full sample, 18.6 percent of the singletons sample, and 43.1 percent of the multiples sample experienced at least two days of bed rest during pregnancy.[32] Columns 4 and 5 present summary statistics for singletons and multiples, conditional on experiencing at least one problem during pregnancy. The prevalence of bed rest is higher among this group; approximately 29.3 percent and 50.8 percent, respectively. Additionally, Table 1 confirms our decision to consider singleton births separately from multiple births; multiples experience higher likelihoods of bed rest, lower birth weight, and most pregnancy problems.

### B. EMPIRICAL FINDINGS

Figure 2 presents the correlates results in graphical form, and highlights factors that are predictive of home bed rest. The sample utilized in Figure 2 includes all women age 20 or older who give birth and are observed in the PRAMS Core, unlike our main results where we additionally limit our sample to women with pregnancy problems who had singleton births.[33,34] The PRAMS correlates for home bed rest of at least two days suggest that mothers of multiples, mothers with pregnancy problems, relatively more educated mothers, and

30 "The preprocessing consists of a reweighting scheme that assigns a scalar weight to each sample unit such that the reweighted groups satisfy a set of balance constraints that are imposed on sample moments of the covariate distributions. The balance constraints ensure that the reweighted groups match exactly on the specified moments" (Hainmueller 2011, 30).

31 All of these statistics are limited to women aged 20 and older.

32 These figures are comparable to what we find using similar variables available in two other data sets. For example, in the PRAMS Standard data, approximately 15.9 percent of the singletons sample and 51.0 percent of the multiples sample were recommended at least one week of bed rest during pregnancy, while 17.7 percent of the singletons sample and 52.6 percent of the multiples sample were recommended at least one week of bed rest during pregnancy in the NMIHS data.

33 When we limit the sample to women age 20 and older who experience at least one pregnancy problem and who experience singleton births (our main analysis sample), the covariates have similar predictive power.

34 In an analysis of the factors correlated with bed rest using the PRAMS Standard data, where answers to bed rest questions are not conditioned on having pregnancy problems, we see that pregnancy problems and multiple births are again the most prominent determinants (results available upon request). Additionally, assuming the primary reason a bed rest recommendation is given is to avert a preterm birth, the key factors

**TABLE 2.** The effect of bed rest on infant health outcomes, OLS

| | Premature | Very premature | Low BW | Very low BW |
|---|---|---|---|---|
| | PANEL A: State and year FEs only | | | |
| Bed rest | 0.0604[a] | 0.0140[a] | 0.0398[a] | 0.0071[a] |
| | [0.0024] | [0.0008] | [0.0014] | [0.0005] |
| | PANEL B: Demographic Xs, state and year FEs | | | |
| Bed rest | 0.0495[a] | 0.0102[a] | 0.0307[a] | 0.0051[a] |
| | [0.0024] | [0.0008] | [0.0014] | [0.0005] |
| | PANEL C: Demographic Xs, pregnancy problems, state and year FEs | | | |
| Bed rest | 0.0174[a] | −0.0025[a] | 0.0078[a] | −0.0024[a] |
| | [0.0023] | [0.0009] | [0.0015] | [0.0006] |
| *N* | 189,535 | 189,535 | 190,198 | 190,198 |
| Outcome mean | 0.116 | 0.030 | 0.082 | 0.016 |

Data source: PRAMS Core, 2000–08.

Notes: Standard errors in parentheses. Coefficients are derived from an ordinary least squares model with survey weights. Panel A includes state and year fixed effects only. Panel B adds demographic controls including maternal age, education, marital status, race, prenatal care insurance status, previous LBW birth, previous premature birth, male, maternal smoking during pregnancy, and maternal drinking during pregnancy. Panel C adds pregnancy problem indicators including labor pains, high BP, vaginal bleeding, placenta problems, nausea, gestational diabetes, infection, PROM, problems with cervix, accident, and blood transfusion. The analysis sample used for results reported in panels A, B, and C includes women aged 20 and older giving birth to singletons who experienced at least one pregnancy problem. [a]$p < 0.01$; [b]$p < 0.05$; [c]$p < 0.10$.

insured mothers have a greater likelihood of bed rest. Additionally, drinking during pregnancy is negatively related to bed rest, and most of the maternal age bands relative to the reference group (women 20–24) experience lower likelihoods of bed rest. This same set of variables contained in the correlates tables is utilized in our models predicting infant outcomes using OLS methods.

### C. ORDINARY LEAST SQUARES

In this and subsequent sections, we focus on the measure of home bed rest of two or more days for singleton births to mothers aged 20 and older who experience at least one pregnancy problem. We first estimate the effects of bed rest during pregnancy on infant health outcomes using ordinary least squares and report these results in Table 2. Important for interpreting these as causal estimates is the assumption of conditional independence. This assumption means that conditional on the observed controls we include in the

associated with a preterm birth, and those that would lead a medical practitioner to recommend bed rest, are problems experienced during pregnancy like the ones we observe in the PRAMS (see background section).

regression (of which we have many), the assignment to bed rest is random. If we have missed an unobserved covariate that is correlated with both bed rest and infant health, our estimates *could* be biased in a way that would preclude our interpreting them as causal. We argue that it is the uncertainty regarding the treatment that leads two providers to make different recommendations in a plausibly random way.[35] We also argue, based on the medical literature (discussed in the background section), as well as through our analysis of the correlates of bed rest (Figure 2), that pregnancy problems and multiple births are the key factors related to bed rest. We limit our sample to singleton births. Excluding pregnancy problem measures from a regression, however, is likely to severely bias the estimates.

We first report results from estimation like equation 2, but where we examine bed rest, and only control for state and year fixed effects (panel A of Table 2). All coefficients are large and positive, suggesting that bed rest leads to worse infant health. Next, we add demographic controls (variables listed in Figure 2, except for pregnancy problems). Estimates from these regressions (panel B of Table 2) are slightly smaller, but very similar to estimates in panel A. Finally, we add pregnancy problems in our regression. Results reported in panel C of Table 2 reveal two things. First, we demonstrate that the ability to control for the types of observable characteristics that physicians rely upon when making a recommendation for bed red rest is important. In particular, the inclusion of pregnancy problems dramatically changes the results, by reducing bias due to some pregnancy problems being correlated both with bed rest and with infant health. Even so, as with any study based on observational data, if unobserved factors remain, our estimated effects may still suffer some bias.[36] Because of the rich set of individual-level characteristics and pregnancy problems included in the PRAMS data, we believe this potential source of bias is minimal. Second, although there is a positive relationship between bed rest and the likelihood of prematurity or low birth weight (1.74 and 0.78 percentage points, respectively), there is a negative relationship between bed rest and the likelihood of very premature or very low birth weight (−0.25 and −0.24 percentage points, respectively).

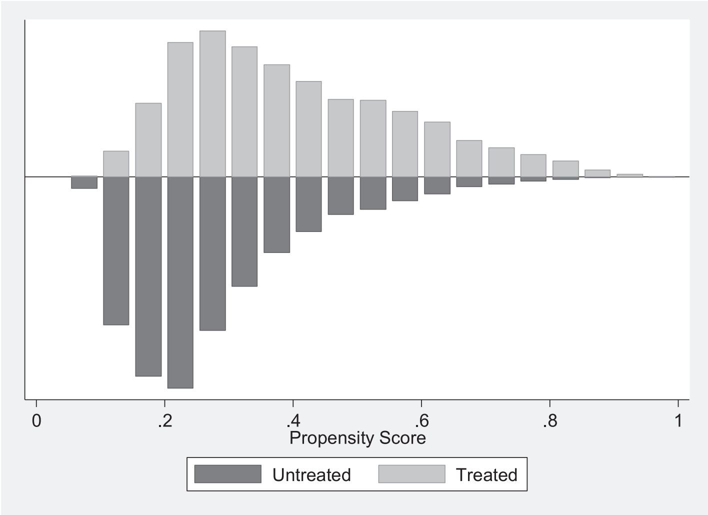
## D. MATCHING METHODS

In this section, we utilize matching methods to construct alternative estimates of the causal effect of bed rest on infant health. We estimate the effects of bed rest during pregnancy on infant health outcomes using several matching methods described previously, including propensity score nearest neighbor matching, regression-adjusted inverse probability weighting, and entropy balance. We first consider propensity score nearest neighbor matching. Online Appendix Table 1 (see http://www.mitpressjournals.org/doi/suppl/10.1162/AJHE_a_00021) shows the results of the logit model predicting bed rest, which is the basis for the propensity score estimation. Almost all of the covariates are highly

35 Specifically, it appears that the ambiguity regarding the effectiveness of bed rest improving infant health is the primary reason we see differences in physician recommendations.

36 Although we have controlled for a large set of pregnancy problems and other maternal characteristics, it is possible that we have omitted an important observable characteristic. For example, one such source of bias might be any additional pregnancy problems that we do not observe. Including these would likely further lessen the bias (as we showed in Table 2) and would strengthen our estimates.

**FIGURE 3.** Common support over estimated propensity scores




Note: Estimated propensity scores for the treated and untreated groups based on Stata 13 *psgraph*.

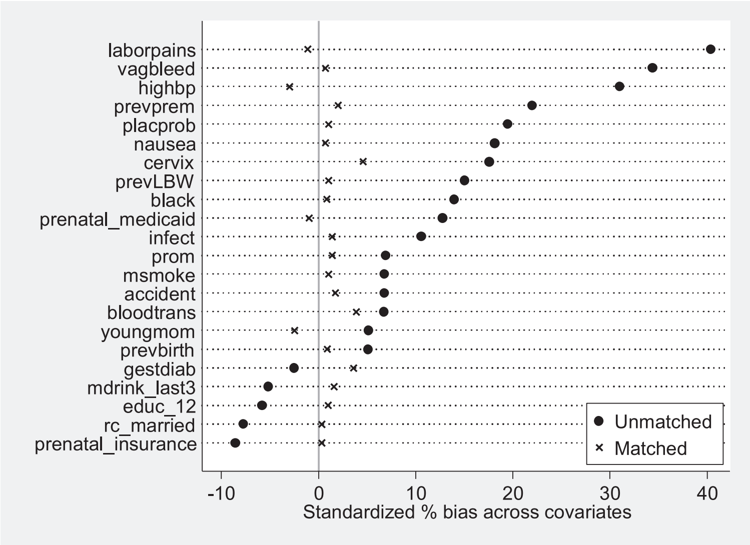
predictive of bed rest during pregnancy. Figure 3 displays the distribution of estimated propensity scores, indicating the presence of common support.[37] Figure 4 shows the reduction in bias attributed to the use of propensity score matching by comparing the unmatched and matched samples. This figure is quite striking; there is a substantial amount of bias (due to selection) in the naïve sample that is reduced with matching. Although our matching procedure does not completely eliminate the bias, it reduces bias considerably and improves confidence in our interpretation of the effect of bed rest on infant health outcomes as causal. This can be seen in Online Appendix Table 2 (covariate balance) as well.[38]

Table 3 (panel A) displays the results for infant health outcomes using nearest neighbor propensity score matching estimation. In this model, we find average treatment on the treated (ATT) effects that are positive for the likelihood of prematurity and low birth weight, but negative for very premature and very low birth weight. For example, the likelihood of delivering a premature infant increases by 1.1 percentage points for those women who were recommended bed rest. However, the likelihood of delivering a very premature infant decreases by 2.3 percentage points for those women who were recommended bed rest. This same pattern at the margins holds with respect to low birth weight and very low birth weight. Comparing results in Table 3 (panel A) with our OLS results presented in

37 In fact, all observations are on common support; no observations were removed from the analyses for this reason.

38 Our covariate balance (Online Appendix Table 2) shows that we have improved balance substantially as evidenced by the bias reduction statistics. Although we recognize that we have some possible imbalance present, we believe this gives us an estimate closer to the true effect of bed rest on infant outcomes.

**FIGURE 4.** Bias reduction from propensity score matching




Note: Bias reduction resulting from use of propensity score matching, based on Stata 13 *pstest, graph*.

Table 2 (panel C), we see that our coefficients are consistent with a further reduction in bias.

Our comparisons are similar if we consider Table 3 panels B and C using regression-adjusted inverse probability weights (IPWRA) or panels D and E using entropy balancing.[39] Panels B and D use linear outcomes models, while panels C and E use logit treatment models and report marginal effects. The findings are consistent with the PSM results shown in panel A, and are consistent across linear and logit outcome models. The similarity of these results to the results presented in Table 3 (panel A) as well as the comparison with the results in Table 2 (panel C) support the position that we have reduced the bias considerably through the propensity score estimation, and that further adjustment on those covariates does not result in considerable differences in the ATT.[40]

Overall, the ATTs contained in Table 3, as well as the OLS estimates in Table 2, tell a consistent story about the effect of bed rest on infant health at birth. Because we are able to control for a number of observable characteristics, and those which we believe specifically influence the treatment of bed rest assigned by the physician, we argue that these results are an estimate of the causal effect of bed rest. Our results suggest that bed rest is positively associated with the likelihood of low birth weight and preterm birth but negatively associated with the likelihood of more severe outcomes, very low birth weight

39 In these regression-adjusted models, we include the same covariates in the outcome equation that we do in the treatment equation.

40 These results are also qualitatively similar if we add additional controls to the outcome model such as the sex of the baby and state and year indicators.

**TABLE 3.** The effect of bed rest on infant health outcomes, matching methods

| | Premature | Very premature | Low BW | Very low BW |
|---|---|---|---|---|
| Panel A: Nearest neighbor propensity score matching (PSM) | | | | |
| Bed rest | 0.011[a] | −0.023[a] | 0.016[a] | −0.019[a] |
| | (0.003) | (0.002) | (0.003) | (0.002) |
| *N* | 189,487 | 189,487 | 190,148 | 190,148 |
| Panel B: Inverse probability weighting with regression adjustment (linear outcome model) | | | | |
| Bed rest | 0.019[a] | −0.017[a] | 0.024[a] | −0.015[a] |
| | (0.003) | (0.002) | (0.003) | (0.002) |
| *N* | 189,487 | 189,487 | 190,148 | 190,148 |
| Panel C: Inverse probability weighting with regression adjustment (logit outcome model) | | | | |
| Bed rest | 0.027[a] | −0.015[a] | 0.032[a] | −0.011[a] |
| | (0.003) | (0.002) | (0.003) | (0.002) |
| *N* | 189,487 | 189,487 | 190,148 | 190,148 |
| Panel D: Entropy balance weights (linear outcome model) | | | | |
| Bed rest | 0.018[a] | −0.018[a] | 0.023[a] | −0.015[a] |
| | (0.002) | (0.002) | (0.003) | (0.002) |
| *N* | 189,487 | 189,487 | 190,148 | 190,148 |
| Panel E: Entropy balance weights (logit outcome model) | | | | |
| Bed rest | 0.026[a] | −0.014[a] | 0.031[a] | −0.010[a] |
| | (0.003) | (0.002) | (0.003) | (0.002) |
| *N* | 189,487 | 189,487 | 190,148 | 190,148 |
| Outcome mean | 0.263 | 0.127 | 0.309 | 0.083 |

Data source: PRAMS Core, 2000–08.

Notes: Standard errors in parentheses. The analysis sample includes women over the age of 20 who experienced at least one pregnancy problem and had a singleton birth. The coefficients represent the average treatment on the treated (ATT). The X vector includes pregnancy problems including labor pains, high BP, vaginal bleeding, placenta problems, nausea, gestational diabetes, infection, PROM, problems with cervix, accident, and blood transfusion; maternal age (aged 34 and younger), maternal education (at least HS), marital status, race, prenatal care insurance status, previous LBW birth, previous premature birth, maternal smoking during pregnancy, and maternal drinking during pregnancy. Outcome means are means conditional on maternal age 20 or older, singleton birth, experience of at least one pregnancy problem but no bed rest, unadjusted for survey weights. Panel A: Abadie-Imbens adjusted standard errors are in parentheses. We utilize Stata 13 and the *teffects psmatch* command for nearest neighbor matching, employing one nearest neighbor. The treatment model uses a logit model. Panels B and C: regression-adjusted IPW model with a linear and logit outcomes model (marginal effects at the mean), respectively. We use Stata 13 and the *teffects ipwra* command. Panels D and E: entropy balance model with a linear and logit outcomes model (marginal effects at the mean), respectively. The weights applied in these models are calculated using the STATA user-written command *ebalance*. In this table, weights are calculated as the *ebalance* weight only. [a]$p < 0.01$; [b]$p < 0.05$; [c]$p < 0.10$.

and very preterm birth. By considering these margins separately in the outcomes analysis, we observe a pattern consistent with shifting of births from one category to another. We hypothesize that bed rest is effective in marginally shifting a birth from the more severe outcome (very low birth weight or very preterm) to the less severe outcome (low birth weight or preterm).

In Table 4, we present estimates where we have analyzed our outcome measures slightly differently to explore whether there is evidence of a shift in the distribution of birth weight or gestation. We define our dependent variable as an indicator for whether $y \leq y_b$, where $y$ is the outcome of interest and $y_b$ designates the bin of interest. Recall that we have birth weight and gestation measured by range in the PRAMS data. For birth weight, we have created dependent variables defined as the upper end of the bin and fewer grams for each interval. For example, the measure 1,375 grams in the data indicate the interval of 1,250 and 1,500, where 1,375 is the midpoint of the range. Our indicator is equal to one if a measured birth weight is in any bin corresponding to a birth weight less than or equal to 1,500 grams. The results in panel A suggest that bed rest reduces the likelihood of being in a lower birth weight interval up to 1,750 grams, while at the same time increasing the likelihood of being at or below low birth weight (2,500 grams) or normal birth weight. This pattern is consistent with bed rest shifting birth weight upward. Panel B reports analogous results for gestation, where the indicators are defined as at or below the top gestation in the bin. While there are fewer bins, the pattern suggests similar shifts for prematurity. The positive coefficients in the midrange of these measures suggest that on net bed rest is associated with an increased likelihood of low birth weight or preterm birth. This does not necessarily mean that bed rest is ineffective or harmful, but that statistically (on net), bed rest is not able to reduce the likelihood of experiencing a low birth weight or premature birth. Lastly, we explored the relationship between bed rest and infant death and find that it leads to a decrease in the likelihood of death of 0.16 percentage points.[41] Taken together, these results provide additional evidence that bed rest reduces the likelihood of the most extreme shocks to initial infant health.

## E. SURVEY WEIGHT ADJUSTED ANALYSIS

The PRAMS sample is drawn using a complex survey design, and is provided to researchers with survey weights. While we utilize survey weights in our OLS analysis (Table 2), we chose not to utilize survey weights for the matching results presented in Table 3 because the matching literature does not offer clear guidance on this point. We acknowledge that employing survey weights might be the more appropriate choice, and in this section we present several models that account for the complex survey design of the PRAMS data that we employ. We will compare our matching results (Table 3) to models estimated similarly except with the survey weights incorporated. Recall, our matching results, regardless of the method employed (nearest neighbor propensity score matching, inverse probability weighting with regression adjustment, or entropy balancing), yield similar findings with respect to infant health outcomes. Although it is not straightforward to incorporate complex survey weights using nearest neighbor propensity score

41 The infant death result (not shown in Table 4) is statistically significant at the 1 percent level.

**TABLE 4.** The effect of bed rest on birth weight and gestation across the distribution, OLS

| Panel A: Birth weight | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | BW ≤ 500 g | BW ≤ 750 g | BW ≤ 1,000 g | BW ≤ 1,250 g | BW ≤ 1,500 g | BW ≤ 1,750 g | BW ≤ 2,000 g | BW ≤ 2,250 g | BW ≤ 2,500 g |
| Bed rest | −0.0005[a] | −0.0011[a] | −0.0017[a] | −0.0025[a] | −0.0024[a] | −0.0022[a] | −0.0013 | 0.0018 | 0.0078[a] |
| | [0.0002] | [0.0003] | [0.0004] | [0.0005] | [0.0006] | [0.0007] | [0.0009] | [0.0011] | [0.0015] |
| *N* | 190,198 | 190,198 | 190,198 | 190,198 | 190,198 | 190,198 | 190,198 | 190,198 | 190,198 |
| | BW ≤ 2,750 g | BW ≤ 3,000 g | BW ≤ 3,250 g | BW ≤ 3,500 g | BW ≤ 3,750 g | BW ≤ 4,000 g | BW ≤ 4,250 g | BW ≤ 4,500 g | BW ≤ 4,750 g |
| Bed rest | 0.0169[a] | 0.0233[a] | 0.0360[a] | 0.0288[a] | 0.0201[a] | 0.0083[a] | 0.0029[c] | 0.0000 | 0.0000 |
| | [0.0026] | [0.0036] | [0.0042] | [0.0041] | [0.0034] | [0.0025] | [0.0017] | [0.0011] | [0.0006] |
| *N* | 190,198 | 190,198 | 190,198 | 190,198 | 190,198 | 190,198 | 190,198 | 190,198 | 190,198 |
| Panel B: Gestation | | | | | | | | | |
| | G ≤ 27 wks | G ≤ 33 wks | G ≤ 36 wks | G ≤ 42 wks | | | | | |
| Bed rest | −0.0019[a] | −0.0025[a] | 0.0174[a] | 0.0002 | | | | | |
| | [0.0004] | [0.0009] | [0.0023] | [0.0002] | | | | | |
| *N* | 189,535 | 189,535 | 189,535 | 189,535 | | | | | |

Data source: PRAMS Core, 2000–08.

Notes: Coefficients are derived from an ordinary least squares model with survey weights. Standard errors in parentheses. The analysis sample includes women over the age of 20 who experienced at least one pregnancy problem and had a singleton birth. The outcome variable is an indicator for whether $y \leq y_b$, where $y$ is the outcome of interest and $y_b$ designates the bin of interest. Regressions include controls for maternal age, maternal education, marital status, race, prenatal care insurance status, previous LBW birth, previous premature birth, male, maternal smoking during pregnancy, maternal drinking during pregnancy, labor pains, high BP, vaginal bleeding, placenta problems, nausea, gestational diabetes, infection, PROM, problems with cervix, accident, and blood transfusion. Panel A reports the estimates for all birth weight bins available in the PRAMS Core. Birth weight is measured by range of birth weight in 250-gram intervals in the PRAMS data. We have created dependent variables defined as the upper end of the bin and fewer grams for each interval. The omitted groups are birth weight greater than 4,750 grams. Panel B reports estimates for using the five birth weight bins available in the PRAMS Core: less than 27 weeks; up to 33 weeks; up to 36 weeks; and up to 42 weeks. The omitted group is 43 weeks or more. Each cell reports the estimate from a separate regression. [a]$p < 0.01$; [b]$p < 0.05$; [c]$p < 0.10$.

matching, it is relatively simple to do so for the estimation of the ATT with IPWRA and entropy balance approaches.

Here, we consider two ways to incorporate survey weights into the empirical design. First, because the survey weights assigned to observations are constructed the same way as an inverse probability weight, we can combine the survey weight and the inverse probability weight to form a combined weight used in the inverse probability weighting regression framework (IPWRA). Second, entropy balance is also another option to incorporate survey weights, because it allows the researcher to identify a base weight during the preprocessing procedure (i.e., the survey weights), and then calculate weights designed to reweight the sample based on the probability of treatment. In what follows, we incorporate survey weights into our design using both of these approaches. Our estimation of the ATT for each of our outcomes should now arguably reflect the population-ATT rather than the sample-ATT.

The results of survey-weighted regressions are presented in Online Appendix Table 3 and suggest the same qualitative pattern that we found in Table 3: positive coefficients on low birth weight and prematurity, and negative coefficients on very low birth weight and prematurity. The magnitudes, however, are smaller. While there is no clear and established way to incorporate survey weights into a propensity score design, we compare our incorporation of the weights with the effects found in Table 3. Using Online Appendix Table 3 (population-ATT) and Table 3 (sample-ATT), we present bounds on the estimated effects. Although the magnitudes are different, when we examine the change on the appropriate underlying sample means, the effect of bed rest on very premature ranges from −13.3 to −7.7 percent and the effect on very low birth weight ranges from −18.1 to −15.4 percent.[42] The results from OLS (Table 2, panel C), which account for survey weights, are similar: −8.3 percent and −15.0 percent for very premature and very low birth weight, respectively.[43]

## VI. Back-of-the-Envelope Calculations

Our results suggest that bed rest decreases the probability of very premature and very low birth weight outcomes. Below, we use readily available data to create back-of-the-envelope measures of the potential cost savings of averting these extreme adverse health

42 These magnitudes were estimated by comparing the coefficient estimates with and without accounting for the complex survey design (Online Appendix Table 3, panel A, and Table 3, panel B, respectively) against the mean of each respective infant health outcome conditional on no treatment (i.e., no bed rest), singleton birth, pregnancy problems, and maternal age of at least 20. For example, to calculate the upper bound of the very premature effect (−7.7 percent), we divided the very preterm coefficient (−0.002) from Online Appendix Table 3, panel B, by the survey-weighted conditional mean of very premature incidence (0.026) to get an estimated effect of −0.077 or −7.7 percent. The other conditional means (survey-weighted) for these calculations were 0.098 for premature, 0.070 for low birth weight, and 0.013 for very low birth weight. Similarly, estimates were calculated using the coefficients which do not account for survey weighting in Table 3, panel B, and used conditional means unadjusted for survey weights for premature (0.263), very premature (0.127), low birth weight (0.309), and very low birth weight (0.083).

43 These are computed using the coefficient estimates and sample means reported in Table 2.

outcomes and the costs (to the mothers) of experiencing bed rest during pregnancy. These calculations rely on a number of assumptions and they should be interpreted with these limitations in mind.[44]

Using National Hospital Discharge Data from 2004, about the middle of our sample period, we observe that among infants born prematurely, the average hospital stay is about 10.5 days. Infants born between 29 and 30 weeks, however, experience an average hospital stay nearly 10 times higher than that of infants born between 35 and 36 weeks (40 days versus 4.5 days). Assuming daily NICU costs of $3,700, delaying preterm birth by six weeks would save approximately $131,350 per infant.[45] In 2004, 2 percent of births occurred before 32 weeks, which translates to 82,240 infants (Martin et al. 2012). According to our estimates, women who experience bed rest during pregnancy are less likely to give birth at these early gestations (and at very low birth weights). If we assume bed rest moves just 5 percent of very premature infants (4,100 babies) to late preterm (to 36 weeks), this would save $540 million in NICU costs alone.

These costs savings, however, should be weighed against the costs a mother faces when taking bed rest. The median weekly earnings of women working full time in 2004 were $715 (BLS 2014). If we assume that the women on bed rest (19.1 percent in our sample) did not work while on bed rest and that the average length of bed rest is 1.5 weeks, then at the median wage, working women would be forgoing around $508,000, suggesting that improving infant health on the NICU-lost wages margin is cost effective.[46, 47] There are other margins, however, that are also important to consider.

While an infant born at 35 weeks is considered preterm, there is a world of difference between an infant born at 35 weeks and one at 30 weeks. NICU costs reflect only the immediate post-delivery expense of a preterm infant. As we discuss in the introduction, infants born at 30 weeks are much more likely to experience short- and long-term disabilities, leading to increased medical expenditures and parental or caregiver time and psychic costs. An Institute of Medicine report estimates that premature infants lead to annual costs of $718 million for early intervention, $1.3 billion for special education for disabilities common among premature infants, and $6.7 billion in lost household and labor productivity (IOM 2006). Reducing time in the NICU reduces both immediate non-monetary costs as well as lifetime costs related to the care of a disabled preterm infant. If, due to bed rest, a quarter of a percentage of the infants who would have been born extremely preterm do not suffer these common disabilities (206 infants), then the annual cost savings for special education services alone is around $625,000. In just five years, this

44 For ease of discussion, all figures referenced in the paper have been converted to $2012 using the CPI-U.

45 Source of the daily NICU cost figure is drawn from Muraskas and Parsi (2008).

46 We compute this figure based on 19.1 percent of women giving birth in 2004 and assuming that 60.3 percent of these women are working (for a total of 473,597 women).

47 Some women may be eligible for the Family Medical Leave Act (FMLA), but this time is unpaid and only available for up to 12 weeks, which women typically use after a birth. A large fraction of women in the United States, however, are not eligible for FMLA; individuals are not protected by FMLA unless they work for an employer with at least 50 employees, have worked at that employer for at least one year, and have worked at least 1,250 hours in the last 12 months (http://www.dol.gov/whd/fmla/1995Report/summary.htm, accessed August 9, 2014).

corresponds to \$3.13 million. The long-term costs to care for conditions related to prematurity are staggering. We acknowledge that women on extended periods of bed rest may also experience a number of costs in addition to forgone wages such as unexpected childcare costs, physical ailments, and psychological costs as a result of being physically isolated and disconnected from friends, family, and work. We suggest that the long-term cost savings of improving the initial health status of even an extremely small number of infants is overwhelmingly higher than costs to the mother and her family, which are more likely to be short-term in nature.

## VII. Conclusions

Health at birth is a crucial input to lifetime human capital production. Infants who are most at risk of early negative shocks as measured by birth weight or gestation are substantially disadvantaged because of the lower health trajectory on which they start. Recent work has shown that interventions made to compensate for negative shocks to this initial health status are more cost effective when made earlier. In this paper, we explore whether a common intervention *before birth*, home bed rest during pregnancy, is effective in improving initial infant health.

Bed rest during pregnancy is frequently prescribed as an attempt to reduce the likelihood of premature birth. The existing literature in this area, however, is inconclusive as to the efficacy of bed rest in improving infant outcomes. This is not surprising given the difficulty of navigating a crucial threat to identification: conditions leading to bed rest recommendations are correlated with the infant health outcomes. Using the Pregnancy Risk Assessment Monitoring Survey (PRAMS), our study improves on the existing literature by using population-based data with a rich set of controls on the mother, infant, and birth. We employ a large set of observable characteristics and obtain estimates using OLS, propensity score matching, inverse probability weighting, and entropy balancing.

Our estimates hinge on the ability to control for the types of observable characteristics that physicians would rely upon when making a recommendation for bed rest or not. The most important of these are pregnancy problems, which are often mentioned in the medical literature and which we identify as the key correlates of bed rest recommendations. We show that a large number of other individual-level covariates that are often included in studies of infant health are less substantial predictors of a bed rest recommendation. In our analysis of infant health, we show that the bias from omitting the other individual-level variables is fairly small but that the bias from omitting pregnancy problems is substantial. Because of this rich set of individual-level characteristics and pregnancy problems included in the PRAMS data, we view any remaining potential for bias to be minimal.

Our results show that bed rest is positively associated with infant health for the more extreme gestation and birth weight margins, but the opposite for less extreme margins. We find a negative relationship between bed rest and the incidence of very low birth weight (<1,500 grams) or very premature (<33 weeks). Yet we find a positive relationship between bed rest and the likelihood of low birth weight (<2,500 grams) or prematurity (<37 weeks) outcomes. We provide further evidence that these changes appear to be a result of upward shifts along the birth weight distribution. Taken together, these results suggest that the margin of infant health examined is important and that examining only the low birth weight or prematurity margin may be misleading. Furthermore, our results are robust; we find the same qualitative pattern using several different methods and performing estimation differently within each method. They also offer more external validity than most of the prior work since our analysis uses a large-scale population-based data set, unlike many of the studies on the efficacy of bed rest that rely on small samples in narrow geographic areas.

## ACKNOWLEDGEMENT

We are grateful for comments received from Angela Fertig, Jeremy Moulton, Ahmed Rahman, Joseph Sabia, and participants of the ASHE meetings, the Southern Economic Association meetings, the University of South Florida seminar, and the University of Central Florida seminar. Any remaining errors are ours.

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