An Application of the Dummy Test of Identification in the Impact of Prenatal Care on Infant Health

by

Eric Gluckman

(Under the Direction of Carolina Caetano)

Abstract

Low birth weight (LBW) infants experience severe health and developmental difficulties that can impose large social costs. However, estimates of the effect of prenatal visits on the birth weight of the infant range from no effect to a small positive effect. These estimates are somewhat at odds with conventional wisdom that prenatal visits should make one's baby healthier when compared to their absence. This paper applies a test of the main identification assumption in models where the treatment variable takes multiple values and has bunching, known as the dummy test of identification from Caetano et al., 2021, in order to detect endogeneity in the prenatal visits model. The dummy test fails to reject in the preferred specification at the no prenatal visits bunching point with an estimated impact of over 26 grams per prenatal visit. However, a different prenatal visit bunching point fails the test, implying further endogeneity.

Index words: [birth weight, bunching, dummy test of identification, endogeneity test, infant health, pregnancy, prenatal visits, prenatal care]

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Chapter 1

Introduction

Infants born with low birth weight (LBW), defined as birth weight less than 2500 grams, impose substantial costs on society through their health and developmental difficulties. Issues faced by families with low birth weight infants include tens of thousands of dollars in medical bills, a high risk of infant death, and increased lifelong physical and mental developmental problems such as high blood pressure, cerebral palsy, deafness, blindness, asthma, and lung disease among children, in addition to worse cognitive development and poorer IQ and test scores.[1] Due to the myriad social costs associated with birth weight outcomes, birth weight is a the primary measure of a baby's health in most economic analyses of infant health and welfare. Birth weight is also a direct target of birth policy, with aims to increase average birth weights and decrease the incidence of LBW.

With such high stakes for families and policy, birth weight outcomes are well studied in economics. Modifiable causes such as maternal smoking while pregnant and prenatal care utilization are extensively studied with the goal of quantifying their effects so that families can have healthier and heavier babies. However, regardless of the behavior being studied, endogeneity is an omnipresent concern. This potential for correlation between unobserved inputs and maternal characteristics such as genetics that affect infant health outcomes makes prenatal care difficult to study. Recently, Caetano, 2015 and Caetano et al., 2021 introduced new methods for dealing with endogeneity by leveraging bunching in the treatment variable of interest and applied these methods to the effect of maternal smoking on birth weight. However, these methods have not yet been applied to measures of the effect of prenatal care on birth weight.

This paper seeks to address the fundamental issue of endogeneity in the effect of the number of prenatal visits on birth weight outcomes by applying the dummy test of endogeneity, as proposed by Caetano et al., 2021. Producing accurate measures of the impact of factors relating to prenatal care outcomes is the subject of a longstanding debate, the stakes of which have grown over time as the costs associated with healthcare now total 18.3% of GDP in the United States (CDC, 2022). Intuitively, prenatal care should improve birth outcomes, but the literature does not always find that increasing the number of prenatal care visits results in improved birth weight outcomes. And, without an accurate measure of the effect of prenatal

[1] Almond et al., 2005, Barker, 1990, Black et al., 2007

visits, policymakers cannot accurately assess the benefits of additional prenatal care when constructing prenatal protocols.

The dummy test of endogeneity is implemented in order to attempt to identify endogeneity in the effect of the number of prenatal visits on birth weight. The dummy test from Caetano et al., 2021 is a test of exogeneity that yields an objective statistical basis to help identify misspecification and endogeneity in a given model. For the analysis in this paper, the treatment of interest is the total number of prenatal visits a mother attends over the course of their pregnancy. This analysis investigates the population of nearly a half-million births in Pennsylvania from 1989 to 1991, as used in Caetano, 2015 and Almond et al., 2005. While the results of the dummy test in the preferred specification fail to detect endogeneity in the zero prenatal visits bunching point, it is unlikely that this paper identifies the true causal effect of prenatal visits on birth weight, but it does provide a more straightforwards and testable method of addressing endogeneity than other papers on the subject. This suggests that it will be easier to address endogeneity concerns and get more accurate estimates of the effect of prenatal visits on birth weight.

The simple dummy test detects endogeneity in the simple regression of prenatal visits on birth weight, but it fails to reject in specifications with a more detailed set of controls. While this paper presents a preferred controls specification, all specifications that pass the dummy test arrive at estimates of the effect of prenatal visits on birth weight in grams that are within a standard deviation of one another. That these estimates are sufficiently similar to each other provides some credibility to those analyses as all models are potentially producing the same inferred parameter value because they are all identified. However, further analysis suggests that there is still latent endogeneity in the preferred specification.

The estimate of the effect of an additional prenatal visit is an additional 26.545 grams. This estimate is in line with other statistically significant estimates of the effect of prenatal visits in the literature. This effect corresponds to an expected increase of 345.30 grams in the birth weight of the child between a mother that does not attend a single prenatal care visit and a mother that attends a complete schedule of prenatal care visits, all else equal. For mothers with infants near the 2500 gram LBW threshold, this estimate of the effect of prenatal visits on birth weight predicts that an additional prenatal visit would result in over 2,000 fewer LBW births in the sample, which is 7% of all LBW births.

This paper is the first to use the dummy test of endogeneity to investigate the effect of prenatal care on birth weight outcomes. Because endogeneity is of high concern in this field and prenatal care inputs may be correlated with unobserved inputs and maternal characteristics that are associated with the health of the infant, this test is a boon to the prenatal care research landscape. Recently, prenatal care standards and practices changed alongside the COVID-19 pandemic, so continued research of the present effects of prenatal visits are an important topic to continue to study in the near future. As healthcare systems transition out of the COVID-19 pandemic, medical protocols are being thoroughly reconsidered. Therefore after nearly a century, policy makers are retooling the prenatal visit schedule originally proposed in 1930 (Peahl & Howell, 2021), and that makes issues regarding the efficacy of prenatal care as important as ever.

Chapter 2

Literature and Context of the Issue

For the purposes of this paper, we focus on the United States healthcare system and birth weight outcomes in the United States. Rich literature focusing on birth outcomes in other countries exists, but its implications are often not applicable to the United States healthcare system. Often, those papers focus on situations where access to prenatal care is drastically reduced when compared to the U.S.. For instance, in developing country contexts, Gajate-Garrido, 2013 notes that the standards for adequate levels of prenatal care often constitute much fewer total visits. Therefore, the applicability of those papers to this one are limited.

It is generally recognized that low birth weight (LBW) is governed by two factors: a short duration of gestation (i.e., prematurity), and a reduced fetal growth rate at a given gestation length, also known as intrauterine growth retardation (IUGR). Furthermore, Almond et al., 2005 and Mukhopadhyay and Wendel, 2008 make it clear that there are high social costs associated with LBW, and that some of these costs may be reduced through prenatal healthcare. While the factors governing LBW and its high social costs are well documented, there is not a definitive protocol in place to prevent LBW from occurring, and it is difficult to pin down which factors actually contribute to low birth weight outcomes.

Prevailing wisdom in the discussion of the effectiveness of prenatal care is that prenatal care improves birth outcomes. In a widely cited 1985 report, the Institute of Medicine estimated that for every dollar spent on more adequate prenatal care for low-income, low-educated women, overall medical expenditures and social costs from low birth weight infants in the first year of life would be reduced by $3.38. These savings were attributed to the effects of prenatal care on the birth weight, and on a reduction in low birth weight outcomes (Brown, 1988). That report was based on a weak evidence base of 30 prenatal care programs and surveys of mothers that did not receive prenatal care. Subsequently, economics research has been more rigorous, but the benefits of prenatal care are not as clear.

Economic literature on prenatal care and birth outcomes originates with Michael Grossman's work on the demand for health and health production (Grossman, 1972), whose ideas are first applied to prenatal outcomes in a study by Rosenzweig and Schultz in 1983. A simplified version of Grossman's framework can be expressed as a three equation system, as seen in Reichman et al., 2009. Under this framework, mothers maximize their utility according to their own factors and infant health, which is an argument in the parent's utility function. The parental utility function is constrained by the process underlying the production of infant health, which is a function of various inputs and the maternal health endowment. Therefore, this paper's focus on prenatal care's effect on birth weight serves as an investigation of the role of prenatal care in producing infant health.

A key methodological challenge throughout the empirical literature estimating this effect is endogeneity (Corman et al., 2018). Direct estimation of the infant health production function may produce biased estimates of the effects of its various inputs because the demand for prenatal care and other inputs may be correlated with unobserved inputs or maternal characteristics that are associated with the infant health outcome. As a result, much of the prenatal care economics literature to this point has implemented various strategies to address this potential for endogeneity. This paper addresses these concerns using novel methods in the prenatal care literature by applying the dummy test of identification from Caetano et al., 2021.

Attempts to address the potential for endogeneity in prenatal care started with Rosenzweig and Schultz, 1983 and have since evolved towards increasingly sophisticated ways. Using data from the National Natality Followback Survey (1967-69), the authors did a two stage regression on four potentially endogenous inputs–prenatal care delay, smoking, parity, and maternal age. The first stage estimated all four inputs using a set of individual-level and state or local area-level instrumental variables, including availability of medical services, health expenditures, prices, and economic characteristics. Under the identifying assumptions of two-stage estimation, some of the authors' specifications estimate a 50 gram reduction in birth weight, on average. Rosenzweig and Schultz, 1988 later replicated these results with data from a different year, which estimated a larger effect of delaying prenatal care (91 grams).

Instrumental Variables (IV) methods such as those in Rosenzweig and Schultz, 1983 are frequently used in the literature to attempt to address endogeneity in birth weight outcomes from delays in prenatal care. Corman et al., 1987, Rosenzweig and Schultz, 1988, Grossman and Joyce, 1990 (addressed the issue of self-selection of pregnant women into live births versus terminating their pregnancies), Warner, 1995 (no significant effects of number of prenatal care visits, but prenatal care delay reduces birth weight), Currie and Gruber, 1996, Reichman et al., 2009, and Liu, 1998 all apply IV methods to measure the effects of prenatal care delay. Results range from a large significant effect of a delay in prenatal care leading to a reduction in birth weight upwards of 130 to 150 grams per month of delay, to non statistically significant estimates of the effect of a delay in prenatal care. More recently, Conway and Deb, 2005 implements finite mixture models with endogeneity correction to identify an effect of one week of prenatal care delay of 30 to 35 grams. Generally, these results are much lower than the effects of other behaviors that are thought to affect birth weight, such as smoking (Corman et al., 2018).

Estimates for the effect of the number of prenatal care visits on birth weight are also variable. Interestingly, for a healthy pregnancy, a one month delay in initiating care would result in an overall reduction of one total prenatal visit using the modern guidelines for prenatal care. Thus, one might expect that estimates of the effect of a single prenatal visit would be similar to estimates of the effect of delaying prenatal care. However, while estimates for the effect of a delay in prenatal care initiation vary between no effect and 130 to 150 grams per month of delay, estimates of the effect of the number of prenatal care visits on birth weight range from no effect to just 46 grams per visit. While this range is similar to some of the more recent range of studies of prenatal care delay, it is still much smaller than the overall established picture.

Reichman et al., 2009 investigated the effects of enhanced prenatal care programs for women on Medicaid that provided more intensive services compared to standard Medicaid prenatal care. Specifically, Reichman et al., 2009 investigate the role of New Jersey's HealthStart program, which included an increased number of prenatal visits, increased provider reimbursement, health support services, and community outreach. Their OLS models found that these enriched services increased birth weight by 55.7 grams and decreased the probability of low birth weight and very low birth weight outcomes. While 5-15 grams of this increase could be explained by prenatal care delay, much of the change was attributed to the program itself and its increased number of prenatal care visits and other features. However, the favorable effects of the program could potentially reflect effects of services other than prenatal medical care because of its increased access to other assistance programs.

Natural experiments have also been used to study the effect of prenatal visits on birth weight. Evans and Lien, 2005 analyzes birth records from Allegheny County, PA from 1990–1994 by exploiting plausibly exogenous variation in care as a result of a bus strike in Pittsburgh, Pennsylvania. The argument follows that increased transportation costs leads to decreased access to prenatal care services. While the number of prenatal visits did not significantly affect birth weight for all pregnant women, the paper suggests an effect of 57 grams for women early in their pregnancies due to the strike. Therefore, one less prenatal care visit in the first trimester due to the bus strike led to a 57 gram reduction in birth weight. This paper also showed that results were worst for the group hurt most by the strike, namely black women, which underlines the importance of public transportation in medical care. However, the one month bus strike in Allegheny County is likely not a mechanism that impacts a large enough population so that the second stage outcomes of their model can be measured with precision.

Sonchak, 2015, whose work builds on Gray, 2001, investigates the effects of Medicaid reimbursement rates on birth outcomes. These papers draw inferences about the effects of prenatal care on birth outcomes. Gray, 2001 applies a difference in differences approach to conclude that Medicaid reimbursement rates increased the use of prenatal care in 1988. Sonchak, 2015 built on this result using individual level data from 2001-2010 U.S. birth records and an IV framework using state reimbursement rates as identifiers for prenatal care visits. This paper finds that an one additional prenatal visit increased birth weight for white women with less than a high school education (including teens) by between 21 and 25 grams, but that it had no significant effect on low birth weight, gestational age, or preterm birth. While this result was statistically significant for white mothers, it was not statistically significant for black women with similar education levels. These results provide evidence that improved prenatal care access for disadvantaged pregnant women can potentially improve prenatal care outcomes.

There is also work considering heterogeneity in the effects of prenatal care on birth outcomes along the birth weight distribution (Abrevaya, 2001; Abrevaya & Dahl, 2008). Abrevaya finds that the impact of prenatal care varies substantially across the distribution of birth weight, and that OLS masked this heterogeneity. Specifically, Abrevaya, 2001 found that no prenatal care reduced birth weight by 389 grams (compared to first trimester care) in the lowest decile of the birth weight distribution, and that effects were monotonically declining. However, this paper does not account for endogeneity in prenatal care. Abrevaya and Dahl, 2008 takes this heterogeneity a step further by allowing prenatal care effects to vary by the distribution of birth weights and by the trimester of care. This specification found that no prenatal care reduced birth weight by 323 grams at the 10^{th} percentile of birth weight and increased birth weight by 271 grams at the 90^{th} percentile, but that there were no statistically significant effects for the bulk of the population from the 25^{th} to 90^{th} percentiles. Unlike Abrevaya, 2001, this study attempts to control for unobserved heterogeneity by embedding correlated random effects (at the level of the mother) within the quantile regression framework. Heterogeneity of effects may be important, but such variations have not been sufficiently explored to allow for generalized inferences (Corman et al., 2018).

These attempts to address potential bias from unobserved heterogeneity and selection underline the empirical issues in identifying the causal effects of prenatal care and other maternal inputs on infant health. Caetano, 2015 addressed some of these concerns in their study of the effect of cigarette use on birth weight. Those results suggest that there is substantial endogeneity remaining for the birth weight outcome, even after controlling for the most complete covariate specification in Almond et al., 2005, but that the evidence of endogeneity is considerably weaker for the probability of low birth weight outcome. However, Caetano et al., 2021 provides a falsification test to detect endogeneity in the effect of prenatal visits on birth weight through bunching methods which leverage discontinuities in the distribution of unobservable confounders with respect to the explanatory variable of interest. The result in Caetano, 2015 provides compelling evidence for these discontinuities in the distribution of unobservable confounders, which allows this paper to apply the test set forth in Caetano et al., 2021. This paper is the first to attempt to address the endogeneity issue for the effect of prenatal care visits on birth weight using such a test.

Chapter 3

The Data

This paper utilizes the data set from Almond, Chay, and Lee (2005). This data represents the population of singleton births between 1989 and 1991 in the state of Pennsylvania. This data set originates from the linked birth and infant death micro data, which is produced annually by the National Center for Health Statistics (NCHS). These micro data files provide detailed information on the universe of births occurring each year in the United States; this time of birth data is linked to death certificate information for infants with mortality occurring within the first year of life. For the purposes of this paper, the universe of births in the state of Pennsylvania totals $497,139$ births over the three year period.

The natality portion of the data provides detailed information about the mother, father, and infant. For each mother giving birth, there is detailed socioeconomic and demographic information including maternal age, race, educational attainment, marital status, history of previous births, prenatal care utilization, and tobacco use during pregnancy. Information about the father includes age, race, and educational attainment. Finally, medical information about the infant at the time of birth rounds out the data. This includes the baby's sex, race, birth weight, gestational age, APGAR scores, and ventilator use.

Of these nearly half a million births, $488,105$ or 98.18% contain complete information for maternal smoking habits. Of these $488,105$, $480,240$ also have full prenatal visit information, which constitutes 96.60% of the population. These births form the basis for the analysis in this paper.

Table 3.1 presents summary statistics for the mother and infant for the population of births in Pennsylvania from 1989 to 1991. The table is broken up into three columns, with column (1) representing summary statistics for the full sample of births, column (2) representing summary statistics for births where the infant weighed fewer than 2500 grams at the time of birth, and column (3) representing summary statistics for births where the infant weighed fewer than 1500 grams at the time of birth.[1] Figure 3.1 shows various birth weight distributions for infants in the sample.

Low birth weight (LBW) and very low birth weight (VLBW) births are separated because of their increased social costs and worse outcomes (Almond et al., 2005). Of the near half-million births included in the analysis, only $29,706$ were LBW births, representing 6.09% of all births in the sample. This incidence rate is several standard deviations below the national rate from 1989 to 1991, which was on

[1]Note that the births in column (3) are included in column (2) because LBW births include very low birth weight births.

Table 3.1: Summary Statistics, NCHS Natality Files Pennsylvania 1989 - 1991

	(1) Full Sample	(2) LBW Births (<2500 grams)	(3) VLBW Births (<1500 grams)
Mother's Age	26.79	25.94	25.99
	(5.646)	(6.040)	(6.200)
Black	0.15	0.34	0.43
	(0.359)	(0.475)	(0.494)
Unmarried	0.29	0.51	0.55
	(0.455)	(0.500)	(0.497)
Mother's Education	12.79	12.19	12.28
	(2.184)	(2.037)	(2.064)
Cigarettes	2.55	4.85	3.98
	(6.248)	(8.144)	(7.403)
Drinks	0.06	0.26	0.30
	(0.893)	(2.038)	(2.287)
Prenatal Visits	10.74	7.80	5.32
	(3.780)	(4.725)	(4.244)
Month of First Visit	2.64	2.67	2.24
	(1.517)	(1.799)	(1.645)
Prenatal Care Adequacy	0.70	0.50	0.47
	(0.458)	(0.500)	(0.499)
Premature Births	0.02	0.23	0.85
	(0.132)	(0.419)	(0.361)
Birth Weight (g)	3366.17	1964.92	960.85
	(590.3)	(560.6)	(350.1)
N	488105	29706	5641

mean coefficients; sd in parentheses

average 7.00% from 1989 to 1991 (CDC, 1994). However, the LBW incidence is still substantial and the state-level heterogeneity should not limit the applicability of the results of this paper since this paper focuses on an intrastate sample.

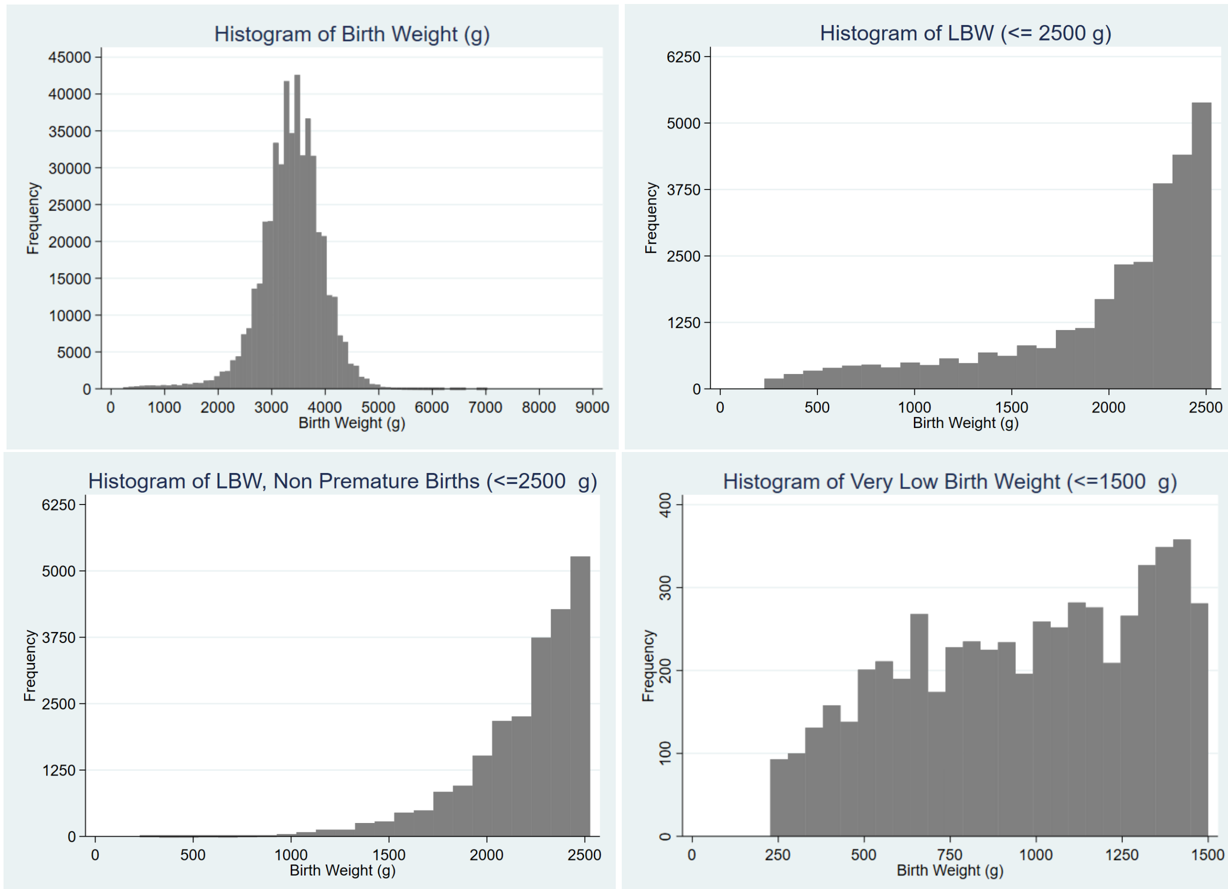
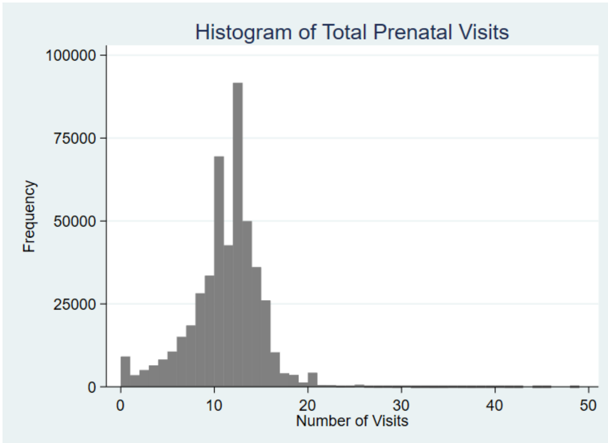


Figure 3.1: Histograms of Birth Weight Distributions

Results from Table 3.1 highlight key demographic information about birthing mothers in the sample. The average age of mothers is 26.79 years, 15% of mothers are black, and 29% of mothers are unmarried. Cigarettes and drinks are both defined in terms of units consumed per day, either cigarettes per day or drinks per day. The incidence of mothers reporting tobacco use during pregnancy is 29.69% of the sample, and the incidence of maternal alcohol use during pregnancy is 3.65%. For the month of the first prenatal visit, we see that on average mothers initiate care near the end of their first trimester.

In terms of the infant, the average birth weight in the sample is 3,366.17 grams and 1.77% are born prematurely. Being born prematurely is defined as a gestational period less than 37 weeks (Mayo Clinic, 2023). Since a short duration of gestation and IUGR are the two main factors affecting low birth weight, it is important to consider these premature births as an important factor in birth weight analyses even though they represent a small proportion of the overall population.

In order to explore factors affecting birth weight outcomes, this paper focuses on prenatal visits. The data contains several observations that are relevant in considering the overall prenatal care experience of



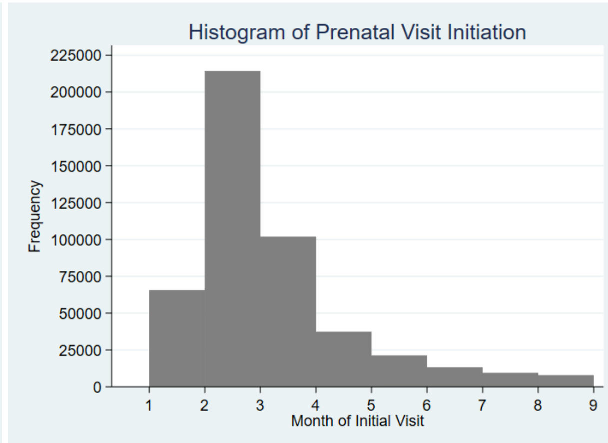


Figure 3.2: Histogram of Prenatal Visits (left)

Figure 3.3: Histogram of Prenatal Visit Initiation (right)

each mother in the sample. The first is the number of prenatal visits, which is distributed according to Figure 3.2.

Three key points in this distribution are the bunching points at 0, 10, and 12 prenatal visits. Applying the framework of Grossman, 1972 and Rosenzweig and Schultz, 1983, this means that a portion of mothers are choosing a corner solution for the number of prenatal visits as an input in the infant health production function. Bunching at zero is especially important because confounders tend to be discontinuous at bunching points (Caetano, 2015). Figure 3.4 is a plot of the simple average birth weight at each level of prenatal visits. The largest discontinuity is the 149 gram difference between average birth weights between mothers with zero and one total prenatal visits.

The other aforementioned bunching points at 10 and 12 prenatal visits correspond to healthy mother prenatal visit protocols. For most of the 20^{th} century, the American College of Obstetricians and Gynecologists (ACOG) set the protocol for the number of prenatal visits, and a healthy mother that is in their first pregnancy and without preexisting conditions would expect to have 12 prenatal visits contingent on full 40 week term pregnancy without complications (Riley et al., 2012). This is backed in the data, as we see a significant decrease in the incidence of prenatal visits above 15. As shown in Figure 3.3 the majority of mothers initiate prenatal care in the latter half of the first trimester which results in the 12 or so visits that we see in the protocol. [2] However, in 1989 The National Institutes of Health (NIH) Public Health Service Expert Panel on the Content of Prenatal Care recommended a prenatal visit schedule for

[2] The full recommended schedule of obstetric visits for an uncomplicated pregnancy is as follows: once every 4 weeks for the first 28 weeks of gestation, every 2 weeks until 36 weeks of gestation, and weekly thereafter.

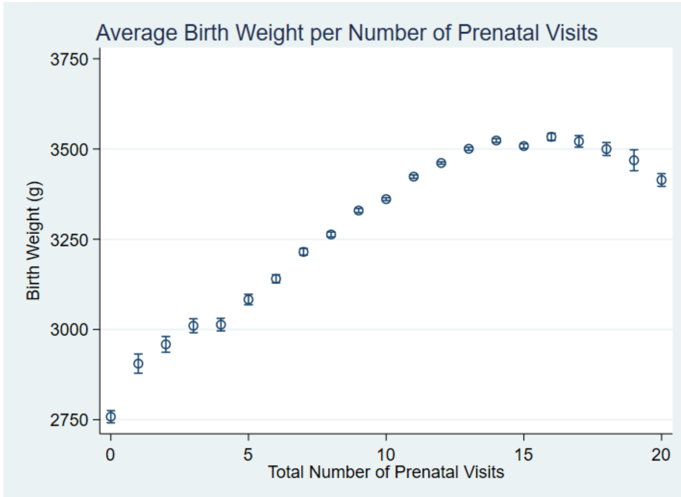


Figure 3.4: Average Birth Weight by Prenatal Visits

nulliparous mothers of just 10 total visits (Peahl & Howell, 2021). [3] Thus, these two bunching points coincide with the two leading prenatal visit protocols at the time.

Figure 3.5 shows the average incidence of low birth weight outcomes that correspond to each number of total prenatal visits attended. These results demonstrate that low birth weight outcomes are not uniformly distributed across the total number of prenatal visits in the sample. Intuitively, since the heart of the distribution from 10 to 15 visits is where we would expect to find patients that attend full prenatal visit schedules with minimal complications, these pregnancies would naturally have the lowest incidence of low birth weight. In the right tail of the distribution, patients are likely going to so many prenatal visits because of complications in their pregnancy, which naturally would lead to worse outcomes for the child. In the left tail, there are a large proportion of mothers with premature births that would not have had the time to go to the full schedule of prenatal visits due to the truncated nature of their pregnancies. Even infants that are born a couple of weeks early but do not meet the definition of prematurity would miss out on the additional birth weight benefits of gestation and due to the nature of the prenatal care schedule would have fewer total prenatal visits. There are also mothers in this tail that through their demographics and other risk factors may be selecting into a lower total number of prenatal visits. Therefore, it is not unreasonable to see higher proportions of low birth weight outcomes in either tail of the distribution.

Looking at the left tail of the distribution of total prenatal visits in figure 3.5, there are some discontinuous differences in birth weight related outcomes for mothers that do not attend a single prenatal visit versus mothers that attend at least one prenatal visit. Mothers that do not attend a single prenatal visit

[3] This schedule of prenatal visits that was proposed by the NIH recommended an emphasis on medical and social risk factors to provide a more tailored standard of care. It also coincided with an expansion in Medicaid to improve prenatal care access. In 2017, ACOG reaffirmed their commitment to their 12 week visit schedule, and it was once again seen as the regular standard of care, but further changes have occurred since with the COVID-19 pandemic.

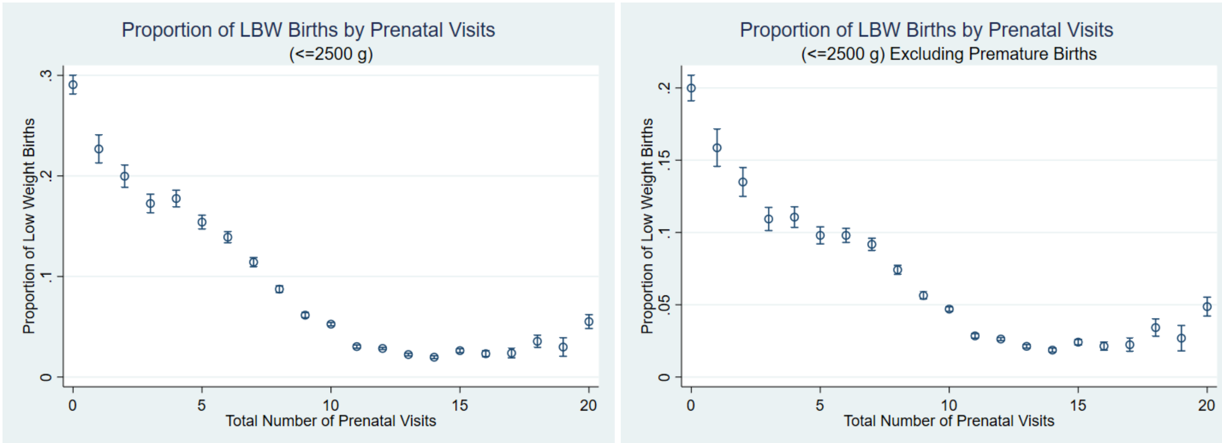


Figure 3.5: Average Incidence of Low Birth Weight Outcomes by Prenatal Visits

face a 29.07% incidence of LBW compared to just 22.68% among mothers that attend a single prenatal visit. Excluding premature births, there is still a substantial difference. 19.99% of births are LBW and not premature for mothers with no prenatal visits, whereas just 15.86% of births are LBW and not premature among mothers with a single prenatal visit. There is also bunching at zero prenatal visits. This discontinuity and the bunching at zero prenatal visits forms the basis for later analysis in this paper.

Chapter 4

The Dummy Test of Identification in Models with Bunching

As mentioned earlier, bunching in a treatment variable is important because the distribution of the confounders conditional on the treatment tends to be discontinuous at the bunching point (Caetano, 2015). This section provides an overview of the dummy test of identification and its application in the literature. Next, it explores the bunching behavior seen in the total number of prenatal visits, specifically investigating mothers bunching at 0 prenatal visits. Finally, this section leverages the bunching in the treatment variable for the total number of prenatal visits to apply the dummy test of identification from Caetano et al., 2021 in order to test for violations of the main conditions for the identifications of average treatment effects.

The following is an overview of the setup and assumptions of the dummy test in linear specifications, as applied to birth weight outcomes (Y) and prenatal visits (X). A complete analysis, including uses of the test in nonlinear specifications can be found in Caetano et al., 2021. In order to identify β as the coefficient of the total number of prenatal visits, β is estimated by an OLS regression of birth weight on the total number of prenatal visits and Z, where Z represents a vector which includes a constant and optionally a set of control variables. Identification of β is therefore guaranteed by a rank condition and the following assumption:

$$(1)\mathbb{E}[\varepsilon|X,Z] = \mathbb{E}[\varepsilon|Z] = Z'\lambda$$

where ε is the remainder of the linear regression of Y on X. This assumption can be tested by adding $\mathbb{1}(X = 0)$ to the regression of Y on X and Z. If a t-test of the null hypothesis that the coefficient of the dummy indicator in the regression is zero fails to reject the null, then one cannot rule out that the assumption holds, and β can be identified. While a failure to reject would not guarantee causality, it would mean that the test is not detecting endogeneity across this threshold and its discontinuous distribution of individuals. [1]

[1] A failure to reject the null hypothesis of exogeneity for a specification does not guarantee that the specification is exogenous. For more information, see the discussion of power in Caetano et al., 2021.

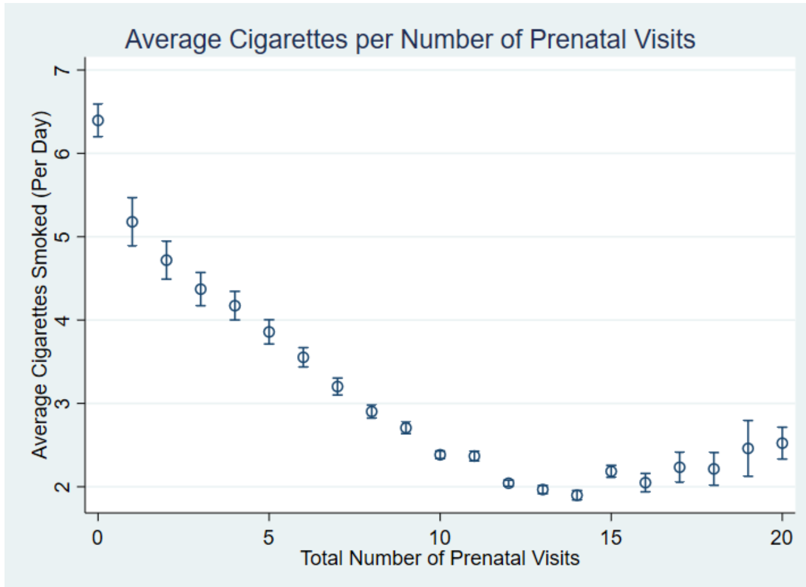


Figure 4.1: Average Cigarettes Per Number of Prenatal Visits

Because the dummy test from Caetano et al., 2021 is a test of exogeneity that yields an objective statistical basis to detect endogeneity in measures of the parameters of interest in an empirical model, the test of identification has already been applied in several contexts. See e.g. Ferreira et al., 2018 Lavette and Schmutte, 2016 DeVito et al., 2019 Caetano et al., 2019 Kaneko and Noguchi, 2020 Caetano et al., 2022 Jurges and Khanam, 2021 Hussein, 2021 Fe and Sanfelice, 2022. Generally, the test requires that unobservables vary discontinuously at a known threshold of the causal variable of interest, which frequently accompanies bunching points in the data. In the context of this paper, unobservables vary discontinuously around the zero prenatal visits threshold.[2] For example, strongly religious mothers in several faiths refuse medical care for their children entirely (Swan, 2020), and their overall religious convictions would be different than the religious convictions of a mother that attends even a single prenatal care visit. Further, because the total number of prenatal care visits cannot be negative, unobserved characteristics of the mother and the pregnancy tend to accumulate at zero. Therefore, parents and pregnancies with no prenatal visits are likely discontinuously different than parents and pregnancies with positive prenatal care utilization.

Maternal tobacco use is also an influential factor in the birth weight literature. Among maternal smokers, an average of 12.93 cigarettes per day are smoked during pregnancy (standard deviation 8.00) which corresponds to slightly more than a half pack per day. Cigarette use in this data set has been extensively covered by Almond et al., 2005 and Caetano, 2015. Additionally, Yeager and Krosnick, 2010

[2] For instance, mothers that go to eight prenatal visits are similar to mothers that go to seven prenatal visits, and mothers that go to seven prenatal visits are similar to mothers that go to six prenatal visits. However, mothers that do not go to any prenatal visits are quite different from mothers that go to a singular prenatal visit, and the notion of similarity breaks down.

provides evidence that measurement error in cigarette usage affects less than 1 percent of the population. To summarize briefly, Almond et al., 2005 estimate that maternal smoking decreases intrauterine growth rates by -200 grams using propensity score methods, but that maternal tobacco use does not affect gestation durations. Caetano, 2015 applies bunching methods which precisely identify the effect of a marginal amount of smoking at zero cigarettes as an expected decrease in birth weight of -137 grams. These estimates and the evidence of endogeneity in the measurement of the effect of smoking on birth weight challenge previous notions in the literature.

Figure 4.1 plots the average cigarette use for mothers at each number of prenatal visits. There is a discontinuity of 1.21 cigarettes on average going from mothers that did not attend a prenatal visit over the course of their pregnancy to mothers that attended a single prenatal visit during their pregnancy. After that, the number of prenatal visits decreases steadily towards an average slightly above 2 cigarettes for the heart of the distribution between 10 and 15 prenatal visits. Past 20 visits, the data is increasingly sparse and variance increases. However, these mothers at 15 prenatal visits and above are most likely to have health complications that merit the additional prenatal visits, which could be positively correlated with an increase in smoking due to the effects of smoking on the mother's health. To summarize, on average mothers that are not going to the doctor frequently enough are more likely to be smoking large quantities whereas mothers that are going to the doctor in the normal range are least likely to be smoking cigarettes.

Importantly, there is a large discontinuity in the average cigarettes per number of prenatal visits going from 0 to 1 total prenatal visits. This discontinuity is important because it provides evidence supporting the idea that the distribution of confounders conditional on the treatment is discontinuous at the bunching point.

This relationship between the number of cigarettes smoked and the total number of prenatal visits is intuitive. Take for example a mother that does not care at all about their own health or the health of their child. This hypothetical mother would be unlikely to modify their smoking behavior for the sake of their child's health and they are least likely to visit the doctor over the course of their pregnancy. On the opposite extreme, a mother whose utility is highly impacted by their health and the health of their child

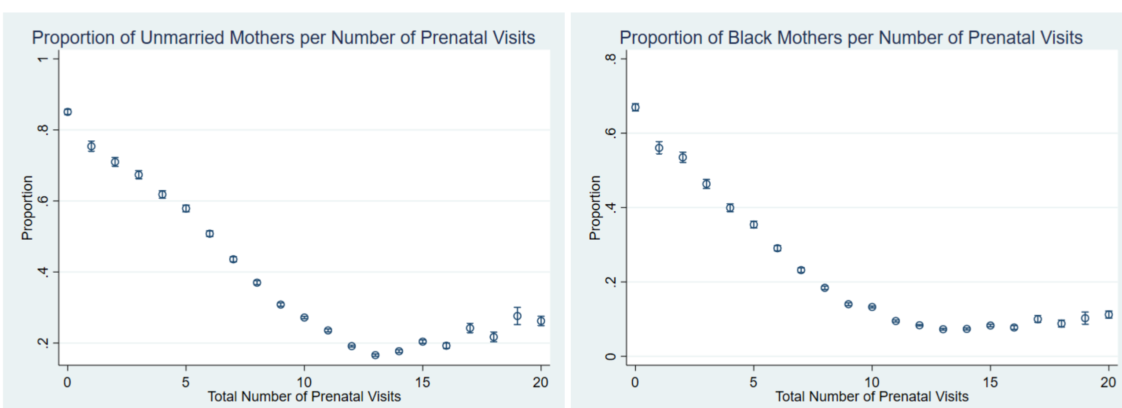


Figure 4.2: Mother's demographic characteristics. Dots represent average values among pregnant mothers for each level of total prenatal visits attended, with 95% confidence intervals.

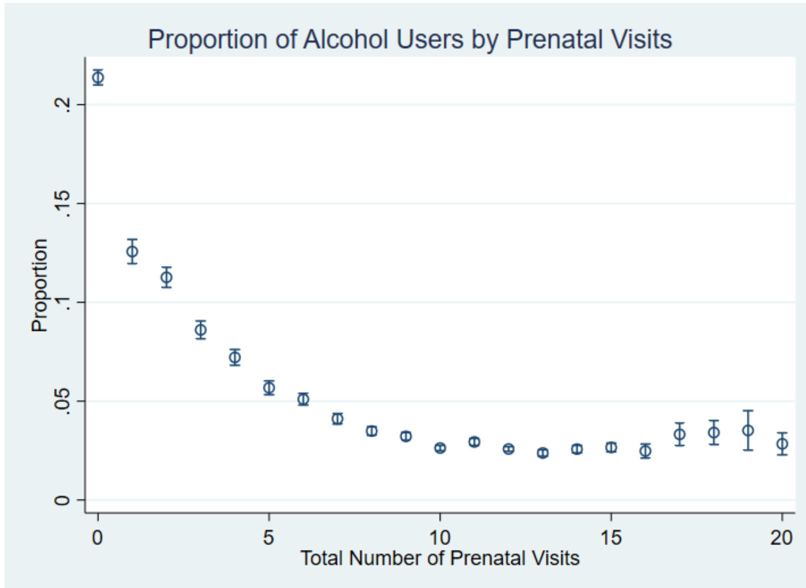


Figure 4.3: Proportion of mothers that regularly consumed alcohol. Dots represent average values among pregnant mothers for each level of total prenatal visits attended, with 95% confidence intervals.

would go to great lengths to decrease their smoking consumption and to attend every prenatal visit that their doctor recommends.[3] The relationship between cigarettes and prenatal visits is explored further in the application of the dummy test.

Figure 4.2 and Figure 4.3 plot average values of the mother's demographic characteristics and alcohol use, broken down by the total number of prenatal visits. Similarly, Figure 4.4 plots the proportion of infants that are born prematurely, again broken down by the total number of prenatal visits.These plots provide heuristic evidence of discontinuities at $X = 0$ for several variables in the data set. The dots are estimates of $\mathbb{E}[Z|X]$, and all reported discontinuities are at the 95% confidence level. These plots serve to demonstrate that in some dimensions, the average of the distribution of the data is discontinuously different at zero prenatal visits when compared against low positive values of prenatal visits. These are just some of the plots that have the described discontinuities. However, this is not true in all dimensions. For example, the sex of the infant is as good as random, therefore intuitively there is not a discontinuity in the average proportion of male children about the bunching point, as it is not clear that this variable is a possible confounder in the data set.

Overall, these plots provide strong evidence in favor of the assumption that pregnancies with zero prenatal visits are discontinuously different from pregnancies with positive prenatal visits. Among mothers

[3]Note that the highly impacted mother would not be maximizing their utility by maximizing the number of prenatal visits that they attend directly. The dynamic of the doctor's recommended number of prenatal visits would affect the total number of prenatal visits that the mother can attend. Therefore, the most caring mother would not necessarily have the highest number of prenatal visits unless their doctor recommended and carried out those visits.

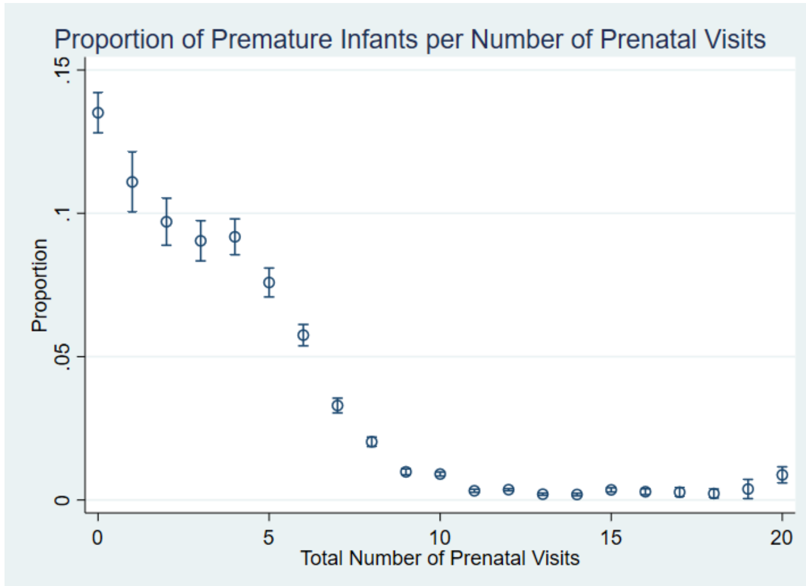


Figure 4.4: Proportion of premature births. Dots represent average values among pregnant mothers for each level of total prenatal visits attended, with 95% confidence intervals.

that attend zero prenatal visits, 85% are reported as unmarried, versus just 75% among mothers with just a single prenatal visit. For the proportion of black mothers, the proportion is a full tenth higher at zero prenatal visits in this case too, with 67% of all mothers being black at zero prenatal visits versus 0.56% among mothers with a single visit. The discontinuity in Figure 4.4 is not nearly as impressive graphically, but less than 2% of all mothers have babies that are premature. 13.5% of infants at no prenatal visits are premature compared to just 11.1% of infants at one prenatal visit, which marks a significant 20% difference. Therefore, since the distribution of observable characteristics is discontinuously different at zero prenatal visits, it stands to reason that unobservable characteristics are similarly discontinuous at this bunching point. This makes this bunching point important for applying the dummy test to detect endogeneity.

To apply the dummy test of identification, this paper implements an indicator variable for prenatal visits $\mathbb{1}(X = 0)$ for prenatal visits that is equal to one if the mother did not attend a single prenatal visit during their pregnancy. Then this indicator variable is incorporated as a regressor in OLS regressions of birth weight on prenatal visits with varying controls specifications. The full controls specification utilizes the most complete control specification of Caetano, 2015 and Almond et al., 2005 with adjustments to include cigarette consumption. Details of the full controls specification are in footnote 36 of Almond et al., 2005.

The results of these dummy test regressions are presented in Table 4.1. Specification (1) represents the simple regression dummy test which regresses Y on X and $\mathbb{1}(\text{Prenatal}= 0)$ and including no controls.

Specifications (2)–(4) represent regressions of Y on X and Z with Z the $\mathbb{1}$(Prenatal$=0$) and including varying controls specifications. Specifically, specification (2) includes the full control specification without cigarettes, while (3) adds cigarettes and cigarette interaction terms to the control set, and (4) adds cigarette bins[4] and cigarette interaction terms.

For the purposes of the test, the most important coefficient is the coefficient of the $\mathbb{1}$(Prenatal$=0$) term. The simple specification in column (1) has a statistically significant coefficient of the $\mathbb{1}$(Prenatal$=0$) term, meaning that it rejects the null hypothesis that there is no endogeneity or misspecification. The models in columns (2)-(4) all fail to reject the null hypothesis. This result is a credible objective indicator which supports the conclusion that the endogeneity in the relationship between prenatal visits and birth weight is captured by the controls in the model. Thus, according to the assumptions laid out in this chapter, including that the distribution of unobservables among pregnancies with zero prenatal visits are discontinuously different from pregnancies with positive prenatal visits, and that equation (1) holds, this paper fails to reject the dummy test of endogeneity in its preferred specification at zero prenatal visits.

Table 4.1: The Dummy Test of Identification
Regression of Birth Weight on Prenatal Visits

Birth Weight (grams)	(1)	(2)	(3)	(4)
Prenatal Visits	35.139***	26.670***	26.305***	26.502***
	(0.30)	(0.36)	(0.36)	(0.36)
$\mathbb{1}$(Prenatal$=0$)	-240.204***	8.543	7.847	9.800
	(9.32)	(7.98)	(8.27)	(7.97)
Dummy Test Result	Rejects	Fails to Reject	Fails to Reject	Fails to Reject
Controls	No	Yes	Yes	Yes
Smoking	No	No	Yes	Yes
Smoking Dummies	No	No	No	Yes
R-sqr	0.065	0.246	0.257	0.256
N	480,240	480,240	480,240	480,240

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

[4]The cigarette bins are as follows: No cigarettes (default), 1 cigarette, 2 cigarettes, 3 cigarettes, 4 cigarettes, 5 cigarettes, 6 cigarettes, 7 cigarettes, 8 cigarettes, 9 cigarettes, 10 to 15 cigarettes, and above 15 cigarettes. The purpose of this cigarette specification structure is to capture the varying marginal effects of daily cigarettes use near zero as reported in Caetano, 2015.

Chapter 5

Results

After applying the dummy test of identification with several controls specifications, it is possible to fail to reject the dummy test for OLS regressions on the effect of the total prenatal visits on the birth weight of the child during pregnancy at zero prenatal visits. Table 5 reports the regression results of the various specifications for which the dummy test is applied in Table 4.1.

To improve clarity, Table 5 reports the t-value and its corresponding probability under the null hypothesis that the coefficient of the dummy indicator in the regression is zero below the coefficients of each model. The $\mathbb{1}(\text{Prenatal}=0)$ term is not included in the regressions in this table, but the statistical significance of this components is still important for understanding if endogeneity is detected in the effect of prenatal visits in each specification. Specifications (2) - (5) fail to reject the dummy test null hypothesis for the $\mathbb{1}(\text{Prenatal}=0)$ term. Even though many of these specifications fail to reject the dummy test in this manner, these specifications are not all preferred.

While several specifications in Table 5 provide similar estimates for the effect of prenatal visits, these specifications are not all equal. The specification in column (2) is a poor choice because it omits smoking, and smoking is an important behavior to consider when it comes to birth weight outcomes. As seen in 4.3 cigarettes usage is correlated with the total number of prenatal visits. The specification in column (3) is also a poor choice because while it includes scalar cigarette controls, Caetano, 2015 demonstrates that the effects of cigarette smoking are non-linear. Specifications (4) and (5) are fundamentally the same, but since specification (5) contains slightly more information it is the preferred specification for the model.

The main effect of prenatal visits on birth weight as estimated by the regressions is an increase of roughly 26.5 grams in the birth weight of a child per prenatal visit. The simple regression in column (1) of birth weight on prenatal visits estimates effects that are 1.5 times as large, but this model rejects the dummy test at the $\mathbb{1}(\text{Prenatal}=0)$ bunching point. All controls specifications that pass the dummy test arrive at a coefficient estimate within one standard deviation of this effect. To contextualize this effect, a mother following a 10 prenatal visits protocol would expect their baby to weigh an additional 265 grams at the time of birth when compared against mothers that did not attend a single prenatal visit, all else

Table 5.1: Regression Estimates for Effects on Birth Weight

Birth Weight (grams)	(1)	(2)	(3)	(4)	(5)
Prenatal Visits	38.560***	26.622***	26.257***	26.443***	26.545***
	(0.28)	(0.36)	(0.36)	(0.35)	(0.37)
Black Mother		51.536	231.532**	209.206*	209.157*
		(92.67)	(103.65)	(92.62)	(92.63)
Mother's Age		0.655*	-1.055***	-0.087	-0.118
		(0.26)	(0.26)	(0.26)	(0.26)
Unmarried		-412.819***	-368.362***	-479.730***	-487.345***
		(103.24)	(96.97)	(99.88)	(103.68)
Alcohol Use		-70.330***	-13.569	-45.386***	-45.516***
		(10.68)	(15.55)	(10.55)	(10.54)
Cigarettes per day			-4.747***		
			(0.31)		
$\mathbb{1}$(Prenatal$=10$)					1.246
					(2.31)
$\mathbb{1}$(Prenatal$=12$)					26.762***
					(1.91)
Dummy Test t-value	-25.78	1.01	1.03	1.23	1.15
$P > \|t\|$	0.000	0.315	0.302	0.219	0.252
Controls	No	Yes	Yes	Yes	Yes
Smoking	No	No	Yes	No	No
Smoking Dummies	No	No	No	Yes	Yes
Other Bunching Points	No	No	No	No	Yes
R-sqr	0.062	0.246	0.257	0.256	0.256
N	480,240	480,240	480,240	480,240	480,240

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

equal.[1] This is a large significant effect when considering that this effect is 7.8% of the median birth weight in the sample.

One interesting result is the coefficient of being unmarried in the preferred regression specification. In the preferred specification, being unmarried is associated with a -497.345 gram decrease in birth weight. Biologically, there is not a known mechanism through which being married affects pregnancy outcomes. Therefore, marriage advantages in prenatal health enter either through the marriage selection hypothesis or the marriage protection hypothesis (Kane, 2016). In either case, the estimated effect of marriage is larger than the estimated effect of prenatal care.

[1]Using the specification in column (5), a mother that attends 12 prenatal visits would expect a birth weight increase of an additional 79.8 grams over the mother that only attended 10 prenatal visits, all else equal.

Specification (5) of Table 5 implements the same controls strategy as (4) with the addition of indicators for the other two common bunching points in the distribution of prenatal visits at 10 and 12 visits respectively. In effect, this is similar to applying the dummy test again, this time looking at these two other bunching points in the data. While the 10 prenatal visits fails to reject with its not statistically significant effect on birth weight outcomes, 12 prenatal visits increases birth weight outcomes by an additional 26.76 grams. Thus, by testing at the second and third bunching points, this paper can falsify its own specification. Applying an F-test of the hypothesis that the coefficients of the indicators $\mathbb{1}(\text{Prenatal}=0)$, $\mathbb{1}(\text{Prenatal}=10)$, and $\mathbb{1}(\text{Prenatal}=12)$ are all zero gives an F-statistic of 70.65 and a Probability $> F = 0$. Interestingly, even though the results of this additional testing falsify specification (4), the estimate of the effect of prenatal visits on birth weight is still quite similar between specification (4) and specification (5). Intuitively, the majority of pregnancies at both of these bunching points follow prenatal care protocols set forth for uncomplicated pregnancies, so it is interesting that on their own one but not both of these bunching points fails this test of endogeneity. So, this specification indicates that there is still endogeneity in the estimate of the effect of prenatal visits on birth weight, but it also estimates a 26.545 gram increase in birth weight from an additional prenatal visit, all else equal.

Chapter 6

Discussion

The preferred specification of the prenatal outcomes model in this paper is column (5) of Table 5. This model estimates that an increase of one additional prenatal care visit will increase the expected birth weight of the infant by 26.545 grams. Applying an F-test of the hypothesis that the coefficients of the indicators $\mathbb{1}(\text{Prenatal}=0)$, $\mathbb{1}(\text{Prenatal}=10)$, and $\mathbb{1}(\text{Prenatal}=12)$ are all zero gives an F-statistic of 70.65 and a Probability $> F = 0$. So, even though this estimate fails to reject using the dummy test with singular bunching, it fails the dummy test with multiple bunching. While this estimate is small and is not without its limitations, the implications of the estimate are still important in the grand scheme of low birth weight outcomes.

It is important to consider the implications of the 26.545 gram estimate of the effect of an additional prenatal visit on birth weight as it relates to low birth weight (LBW) outcomes. Of the $29,706$ infants in the sample that are born with LBW (6.09% of all infants), $2,077$ would no longer be classified as LBW babies if they benefited from an additional prenatal visit. This corresponds to 6.99% of all LBW infants in the sample. This several thousand infant LBW incidence reduction would decrease the proportion of LBW babies in the sample to 5.66%. While the LBW threshold is somewhat arbitrary in the sense that infants born just above the threshold likely experience many of the same complications of infants born just below it, this potential reduction in the incidence of LBW is still rather significant.

Furthermore, of the $9,104$ mothers that did not attend a single prenatal visit, $2,647$ or 29.08% had LBW babies. This proportion is substantially larger than the overall population. If these mothers with LBW infants and no prenatal visits had instead attended 10 prenatal visits, we would expect that 875 of them would no longer have LBW infants. This would reduce the incidence of LBW among these mothers by 33.06%.[1] Due to the aforementioned self selection factors affecting the types of mothers that may choose not to go to a prenatal visit during their pregnancy, it is intuitive that those mothers would have the most to gain from increasing the amount of prenatal care that they receive.

To begin to address some limitations, this model implements a simple linear functional form. Because Abrevaya and Dahl, 2008 found that the impact of prenatal care varied substantially across the distribution

[1] Increasing to just a single prenatal visit would accompany an expected decrease in the incidence of LBW among these mothers of 116 births, which is 4.38% of all LBW births among mothers that did not attend a single prenatal visit.

of birth weight, and since investigations such as that of Sonchak, 2015 only find evidence of a causal effect of prenatal visits for specific subsets of the population, there is credible potential that the linearity assumption is too simplistic to describe the infant health production function. The dummy test of endogeneity does not allay these concerns either. Even though this control specification is quite detailed and is used in Almond et al., 2005 and Caetano, 2015 to assess the effect of smoking on birth weight, its application to prenatal visits in this OLS model does not do enough to fully address endogeneity issues with estimation of this effect. Therefore, the results of the dummy test do not allow for causal conclusions in terms of the true effect of prenatal visits on birth weight. Further efforts to address this endogeneity detected through the dummy test are outside of the scope of this paper, but are important to explore going forwards because this specification has been falsified.

To situate this paper in the broader field of prenatal care research, the estimates of the effects of prenatal care on birth weight reported by this paper are in line with other publications that estimate statistically significant effects for prenatal care visits on birth weight (Corman et al., 2018). These estimated effects support the idea in the literature that prenatal visits are an important factor for pregnancy outcomes, but that overall they do not currently play an outsized role in the development of healthy infants.

Additionally, this paper is the first to use the dummy test of endogeneity to investigate the effects of prenatal care on pregnancy outcomes. Because endogeneity is of high concern in this field, and prenatal care inputs may be correlated with unobserved inputs and maternal characteristics that are associated with the health of the infant, this test is a boon to the prenatal care research landscape. Prenatal care standards and practices changed alongside the COVID-19 pandemic, so continued research of the present effects of prenatal visits are an important topic to continue to study in the near future. Further efforts to study this topic should look to apply this test when applicable in the continued battle against endogeneity and to have properly specified models.

Chapter 7

Conclusion

As healthcare systems emerge from the COVID-19 pandemic and begin to reconsider medical protocols such as the prenatal visit protocol that defined most of the 20^{th} century (Peahl & Howell, 2021), it is important to understand how the total number of prenatal visits affects birth weight outcomes. By employing the dummy test of identification, this paper addresses the looming issue of endogeneity in the prenatal care utilization literature. Even though the dummy test with a complete control specification fails to reject with single bunching at zero prenatal visits, by testing for endogeneity with multiple bunching in prenatal visits the dummy test rejects the null. This means that there is still endogeneity in estimates of the effect of prenatal visits on birth weight.

This paper estimates a birth weight increase of 26.545 grams per additional prenatal visit under the preferred linear specification. This result should not immediately serve as an endorsement for one prenatal visit protocol over the other, however, because factors such as the additional costs of two extra prenatal visits should be carefully considered in policy decisions. Merely, an appropriate conclusion is that the data provides some evidence of a small additional birth weight benefit for the 12 prenatal visit schedule. These estimated effects mirror the literature in that prenatal visits factor into pregnancy outcomes, but other modifiable behaviors may still have larger effects.

Most of the literature in this field applies IV regression models in an attempt to address endogeneity. This paper demonstrates that other methods may also be applicable, and that strong controls specifications can also address endogeneity issues in health outcomes. This idea can even be applied to models that use heterogeneous treatment effects (Caetano et al., 2021). The applicability of the test still depends on meeting various assumptions, but these assumptions make a lot of sense in the context of prenatal care utilization.

And so, while the results suggest that increasing the number of prenatal care visits has as positive effect on the birth weight of a child, there is more work to be done. Economics researchers must continue to study the effects of prenatal care visits, especially considering the shifting healthcare landscape and COVID-19. Doctors and policymakers face a difficult decision when it comes to the prenatal care protocol that is best for patients, and it is incumbent upon them to find a protocol that is effective for a broad number of women but that still allows for personalization in cases with extenuating or abnormal circumstances. For both researchers and policymakers, it is important to apply a framework that addresses endogeneity when considering the effects of prenatal care on birth weight.

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