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Essays on Health, Health Care Utilization, and Public Insurance

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Abstract

Inadequate prenatal care has long been associated with low birth weight as well as adverse health and economic outcomes. As such, public policy and public health interventions have focused on increasing access to prenatal care with the goal of increasing birth weight. Despite this focus, research has generally found little conclusive evidence supporting a causal relationship between increased access to prenatal care and substantial gains in birth weight. A small but growing body of work suggests prenatal care may improve future health outcomes independently of birth weight by directly influencing a mother's health or how she interacts with the health care system. In the first chapter of my dissertation, I use unique data combining all Medicaid financed births with all subsequent Medicaid claims in South Carolina between 2001 and 2012, to find that prenatal care increases the probability of an infant receiving routine well-child care and decreases the probability of requiring inpatient care within the first year of life. In spite of these findings, my results suggest that prenatal care has only a small, marginally significant impact on an infant's birth weight I find that a portion of the causal mechanism through which prenatal care acts is by providing health knowledge and that prenatal care and formal education are substitutes in the production of health knowledge.

Although the impacts of prenatal care on birth weight and health outcomes at birth have been extensively studied, little is known about the long term impacts

of prenatal care on prospective measures of health. In the second chapter of my dissertation, I restrict my data to include all Medicaid financed births between 2001 and 2007 and find that a mother's utilization of prenatal care increases the probability of her child receiving treatment for an asthma-related diagnosis. Using a count model that allows for unobserved heterogeneity at the individual level, I find evidence that a mother's usage of prenatal care increases the number of times her child receives primary care and does not impact the frequency with which resource intensive care is utilized for asthma-related conditions. These results suggest that prenatal care may be more likely to impact the prospective management, rather than the underlying presence or severity, of chronic health conditions.

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Chapter 1

Prenatal Care and Infant Health Care Utilization

1.1 Introduction

Improving access to prenatal care has long been a major focus of the Medicaid program. In 1986, the program was expanded to allow states to cover pregnant women and infants whose family incomes were up to 100 percent of the federal poverty level (FPL). Shortly thereafter, states were given the option to expand eligibility to those with incomes up to 185 percent of the FPL, and coverage at income levels exceeding the FPL was gradually mandated. South Carolina was one of the earliest states to voluntarily expand coverage to pregnant women, having done so as early as October 1987. As a result of this early expansion, Medicaid was already paying for well over 40 percent of all births in South Carolina by the mid-1990s (National Governors Association, 1997).

Ostensibly, the focus on improving access to prenatal care was undertaken not just for the sake of care itself, but rather because of the purported health benefits of prenatal care. One of the primary benefits of prenatal care has been posited to be its propensity to increase the birth weights of infants. Low birth weight has long been identified as a leading indicator of poor health, and several studies have shown that infants with low birth weights tend to have lower educational achievement and worse employment outcomes than those with normal birth weights (Black et al., 2007; Royer, 2009). Because a number of indicators for adequate prenatal care are associated with increased birth weight, the conventional thinking assumes that prenatal care is most likely to impact an infant's health through this channel.

Despite these facts, the evidence supporting a causal relationship between prenatal care and birth weight is inconclusive. While a number of medical studies have documented an association between prenatal care and birth weight, the majority of these studies have not accounted for the potential endogeneity of prenatal care. Mothers that are more concerned about the health and well-being of their infant may be more likely to seek prenatal care. If they also engage in other unobserved behaviors that produce healthier, heavier infants, the estimated impact of prenatal care will be biased upwards. Conversely, if mothers who are likely to have complicated or difficult pregnancies are more likely to seek prenatal care, the estimated impact of prenatal care will be biased downwards. Illustrating these conflicting forces, Cox et al. (2011) find that inadequate prenatal care and intensive prenatal care are *both* associated with increased risks of preterm birth and low birth weight. Given the appearance of endogeneity, a number of researchers have attempted to identify a causal link between prenatal care and birth weight using an instrumental variables approach. The results of these studies have generally been mixed, with some finding positive effects and others finding insignificant effects.

The majority of work on the impacts of prenatal care uses outcomes at birth to proxy for health, and there are relatively few studies that focus on the impacts of prenatal care on more prospective measures of health. Miller and Wherry (2014) estimate long-term impacts of prenatal care due to expanded Medicaid eligibility and find improvements in adult health for those individuals who gained coverage while in-utero. Although there is a broad consensus in the medical community that prenatal care is likely to improve subsequent health outcomes, the underlying causal mechanism is not well understood. One alternative explanation that has emerged on a limited basis is that prenatal care may directly influence the health of the mother, how she cares for her child, and, potentially, how she interacts with the health care system following birth (Conway and Kutinova, 2006; Reichman et al., 2010).

In this paper, I extend on the work of Reichman et al. and investigate the relationship between prenatal care and health care utilization within the first year of life. I overcome potential endogeneity by utilizing an instrumental variables technique where the concentration of prenatal care providers within a county is used as a proxy for access to care. I find evidence suggesting that increased utilization of prenatal care increases the probability of routine well-child pediatric visits within the first year of life. Well-child visits (WCVs) are doctors' visits that generally involve providing preventive care and immunizations. Hakim and Bye (2001) find a significant negative association between WCVs and preventable hospitalizations within the first two years of life in Medicaid populations across three states, although they do not establish a causal link between the two. Results in De La Mata (2012) suggest a significant positive relationship between Medicaid eligibility and preventive health care utilization in children, but no significant relationship between eligibility and future measures of health.

I also investigate the relationship between prenatal care and health care utilization within the first year of life in the context of two types of acute care: care provided in emergency department (ED) and inpatient settings. I find that increased access to prenatal care decreases the utilization of inpatient treatment and may potentially lead to large reductions in future health care expenditures. According to Dafny and Gruber (2005), over 40 percent of pediatric health care costs were attributable to hospitalizations in 2000 and 26 percent of pediatric hospitalizations were avoidable between 1983 and 1996. Under the assumption that more severe illnesses and infants in worse health will be more likely to require treatment in an inpatient setting, fewer hospitalizations may also be suggestive of improvements in infant health. Although I do not find a significant relationship between prenatal care and ED usage in the general population, I find evidence that is consistent with the hypothesis that prenatal care influences future ED utilization by providing education and information to an expectant mother, and that this education is a substitute for formal education in the production of health knowledge.

1.2 Background and Motivation

This paper relates to three main strands of health economics literature. Although the primary focus of the paper is the impact of prenatal care on the health and health care utilzation of infants, it also investigates how access to health care providers affects the health care utilization of pregnant mothers eligible for Medicaid. I look at two distinct types of infant health outcomes: birth weight and future care utilization. This paper adds to the relatively large literature that has studied the impact of prenatal care on birth weight and uses Medicaid claims data to expand on the much smaller set of literature that has looked at health outcomes beyond birth.

Both medical and economic research has focused on assessing the impacts of prenatal care on health outcomes. Prenatal care has long been thought to improve infant health primarily through its claimed ability to improve birth outcomes such as birth weight. Despite this fact, the causal mechanisms and whether care actually impacts birth weight remain something of a mystery. McDuffie et al. (1996) use a controlled randomized trial to study how a reduced regimen of prenatal care impacts birth outcomes in low-risk pregnancies and find no significant differences between the experimental or control groups along several dimensions. Several different meta-analyses find no evidence that reducing the recommended number of prenatal care visits in developed countries is associated with increases in any adverse outcomes (Fiscella, 1995; Dowswell et al., 2010), suggesting that, for low-risk pregnancies in developed countries, the marginal benefit of prenatal care on birth weight at the recommended number of visits is effectively zero.[1]

Although the estimated impact of prenatal care on birth weight should be unbiased in a randomized controlled trial, a number of medical studies fail to control for the potential endogeneity of prenatal care. If prenatal care is endogenous, it is possible that the estimated impact of prenatal care on birth weight could either be too large or too small. To overcome this potential for endogeneity, most economic studies have used a measure of access to prenatal care as an exogenous source of variation in order to identify a causal impact of prenatal care on birth weight.

Some of the more common sources of variation used as proxies for access to care are differences in reimbursement rates to doctors who provide prenatal care services or measures of care availability such as provider concentration or the distance from the closest provider. Currie et al. (1995) show that higher relative reimbursement fees for Medicaid patients are associated with a small but statistically significant reduction in infant mortality. Both Gray (2001) and Sonchak (2014) show that higher reimbursement rates are associated with increased utilization of prenatal care. Gray

[1] Dowswell et al. also note that the basis of modern prenatal care came from guidelines developed during the early 20th century. Since the inception of modern prenatal care, few of the components or recommendations for prenatal care have been subject to rigorous scientific testing.

outlines several possible theoretical reasons that higher reimbursement fees might increase access to care, including that higher fees may induce a greater number of physicians to accept Medicaid patients. Rosenzweig and Schultz (1982) as well as Warner (1998) each use several measures of provider concentration, such as physicians, registered nurses, and hospital beds per capita at the state or county level, as measures of the availabilty of prenatal care. Because I directly observe the number of providers treating Medicaid insured pregnant women, I am able to construct a more direct measure of the availability of prenatal care for this subpopulation and use this measure to proxy for access to care.

Gray (2001) also finds that increased Medicaid reimbursement fees are associated with improvements in several birth outcomes, including a reduced probability of low birth weight. Sonchak (2014) uses an instrumental variables approach to estimate the causal impact of prenatal care on birth weight and finds that additional prenatal care visits increase birth weight for white mothers, but have no significant effect for black mothers. Warner (1998) finds that additional prenatal care visits actually yield a larger impact on birth weight for black mothers than they do for white mothers. Currie and Gruber (1997) employ infant mortality as a measure of health and use variations in Medicaid eligibilty to find evidence suggesting that increases in Medicaid eligibility may decrease infant mortality, though these results are not statistically significant in some of their specifications.

Evans and Lien (2005) utilize a quasi-experiment following a 1992 transportation strike in the greater Pittsburgh area to estimate the impact of prenatal care on birth weight. Using an instrumental variables strategy, they estimate a positive impact of prenatal care on birth weight, but this impact is not precisely estimated and the estimates are not statistically different from zero. However, they find a significant impact of prenatal care on maternal smoking cessation behavior. Smoking during a pregnancy has long been identified as a cause of poor birth outcomes (Kramer, 1987; Murray and Bernfield, 1988; Butler et al., 1972), and clinical recommendations include smoking cessation programs within the scope of prenatal care as a cost-effective way to improve birth outcomes (Marks et al., 1989). Fertig (2010) provides evidence suggesting that the estimated impact of smoking during pregnancy has increased over time due to selection bias and that, for the group of mothers that continue to smoke during pregnancy, tobacco use may be related to other unobserved characteristics that negatively influence birth outcomes.

Because of the contradictory results that have been obtained when estimating the impact of prenatal care on birth weight in a linear two-stage least squares framework, several recent studies have estimated the impact of prenatal care in a more flexible, non-linear framework. Abrevaya and Dahl (2008) allow prenatal care to impact birthweight in different ways across the birth weight distribution using quantile estimation techniques. They find that prenatal care tends to have a larger impact on birthweight at lower quantiles of the birth weight distribution. Conway and Deb (2005) recognize the bimodal nature of the birth weight distribution and use a mixture model to allow for prenatal care to have a differential impact on normal versus complicated pregnancies. Their results suggest that prenatal care has a significant impact on normal pregnancies that is masked when these pregnancies are grouped with complicated ones.

Perhaps due to the difficulty of obtaining data linking measures of prenatal care to future health or health care utilization, relatively few studies have looked at how prenatal care impacts these prospective outcomes. Kogan et al. (1998) show that less than adequate prenatal care is associated with a decreased number of subsequent well-child visits and Freed et al. (1999) show that late initiation of prenatal care is a leading risk factor for a low number of WCVs before two years of age. Lu et al.

(2000) estimate the potential increase in perinatal medical costs among undocumented immigrant mothers in California from eliminating public funding for prenatal care and find that access to prenatal care leads to significantly lower future expenditures on health care. None of these studies, however, control for the possible endogeneity associated with the decision to seek prenatal care and Noonan et al. (2013) find there is no impact of early prenatal care on observed health at age five.

To my knowledge, Reichman et al. (2010) is the only study investigating the causal impact of prenatal care on pediatric health care utilization. Using survey data that is augmented with hospital record data, they find that prenatal care is associated with an increased probability of making at least four WCVs and a decreased probability of maternal postpartum smoking. Additionally, their study makes use of several approaches to control for the potential endogeneity associated with prenatal care suggesting that the effect of prenatal care on postpartum behaviors is causal. Their work is closely related to my study as I augment data from birth certificates with future Medicaid claims in order to identify future health care utilization, although I observe a wider range of outcomes. In a related strand of literature, Wherry et al. (2015) investigate the impact of expanded medicaid eligibility during childhood on health care utilization during early adulthood. They find evidence that a longer duration of Medicaid eligibility in childhood is associated with decreaseed utilization of both inpatient and emergency department services in adulthood for blacks.

One potential pathway through which prenatal care could impact both future health and health care utilization is by providing information to mothers. Providing information about healthy behaviors during pregnancy is seen as a linchpin of comprehensive prenatal care (Kirkham et al., 2005); Reichman et al. (2010) show that prenatal care is associated with changes in maternal behaviors that are likely to be influenced by information provided during prenatal care visits. Rosenzweig and Schultz (1982) model a health production function where education is a complement to health by providing information about the relationship between health inputs and health outputs. Grossman (2006) provides an overview of several theoretical approaches where education may impact health through an allocative efficiency mechanism. Among the papers mentioned by Grossman, Thomas et al. (1991) shows that much of the impact of education on health indicators is explained by information, while Glewwe (1999) finds that education primarily improves health outcomes by increasing a mother's health knowledge and that a mother's health knowledge alone is a primary determinant of her child's health.

1.3 Model

1.3.1 Theoretical Framework

The theoretical model presented here follows closely from the family health production function modeled by Rosenzweig and Schultz (1982). It is assumed that household utility is a function of market goods X that can be directly purchased and the health H of the household. Utility at time t is given by

$$U_t = U(X_t, H_t), \tag{1.1}$$

with $U_X > 0$, $U_H > 0$, $U_{XX} < 0$, and $U_{HH} < 0$. Health can not be directly purchased; it must be produced with inputs Z that may be purchased directly. Assume that health production is

$$H_t = F(Z_t, H_{t-1}, \mu | K_t(\sum_{\tau=1}^{t-1} Z_\tau, \kappa)), \tag{1.2}$$

where $F(\bullet)$ is a function of health inputs, prior health, and a vector μ of household specific characteristics, at least some of which are unobserved. This function is conditional upon the household's health knowledge K, which is developed through both formal education κ and prior health care utilization, and is strictly increasing in both arguments.[2]

As Rosenzweig and Schultz note,

> ...it is doubtful that schooling can affect the production of H without it being associated with some alteration in an input. Instead, education, by augmenting information, may be thought to affect parental *perceptions* of the relationships between inputs and outputs. Parents maximize utility subject to production relations which they think exist (p. 59).

In this framework, it is assumed that health knowledge does not directly enter into the health production function, but instead makes the household's production of health more efficient at a constant rate so that $F_{ZK} > 0$ but F_K is undefined and $F_{HK} = F_{ZKK} = 0$. Although this framework assumes constant returns to health knowledge, I assume that $K_{ZZ} < 0$ and $K_{\kappa\kappa} < 0$, i.e. that there are decreasing returns in the production of health knowledge. I do not place any formal restrictions on the sign of $K_{Z\kappa}$, allowing for the possibility that education and health care could be complements or substitutes in the production of health knowledge. Previous empirical work suggests they may be substitutes in this setting, implying $K_{Z\kappa} < 0$. Similarly, there are no restrictions placed on the sign of F_{HZ}, which allows for the possibility that improved health at the start of a period may either increase or decrease the marginal productivity of care. Assume, however, that $F_H + F_{HZ} > 0$ so that the net

[2] In their original model, Rosenzweig and Schultz allow for a subset of market goods to directly impact both utility and health production, but for the ease of exposition I assume health is independent of consumption goods.

effect of improved health is always positive.

In the context of prenatal care, it is natural to restrict the general framework above to a case where there are only two periods. During period 1, Z_1 consists of prenatal care, which will directly impact the production of health H_1 at birth, and will also indirectly impact the production of health in period 2 through its effect on K_2. In the second period, health is impacted both by H_1 and Z_2, where Z_2 consists of care delivered directly to the infant and whose productivity is augmented by increases in K. Using variations in children's eligibility for Medicaid across states, Currie et al. (2008) find that impacts of increased health care utilization are not visible until future time periods, thus it is reasonable to think that much of the impact of prenatal care will be observed in period 2 health H_2.

In this two-period framework, individuals maximize utility

$$U = U(X_1, H_1) + U(X_2, H_2), \tag{1.3}$$

where H_1 and H_2 are given by

$$H_1 = F(Z_1, \mu | K_1(\kappa)) \tag{1.4}$$

and

$$H_2 = F(Z_2, H_1, \mu | K_2(Z_1, \kappa)). \tag{1.5}$$

In addition, the maximization in (3) is subject to a resource constraint

$$I_t = P_X X_t + P_Z Z_t, \ \forall t = 1, 2.^3 \tag{1.6}$$

[3]It is assumed that initial health status and knowledge of the health care system from utilization that precedes period 1 are both part of μ. Because a majority of the cost of care, P_z, borne by Medicaid recipients is non-monetary, I do not allow for any inter-temporal substitution.

The first order conditions imply that in period one,

$$\frac{U_{X_1}}{P_X} = \frac{U_{H_1}F_{Z_1} + U_{H_2}(F_{H_1}F_{Z_1} + F_{Z_2H_1}F_{Z_1} + F_{Z_2K_2}K_{Z_1})}{P_Z}. \tag{1.7}$$

However, following the interpretation of Rosenzweig and Schultz that parents maximize family utility subject to relationships that they think exist, assume that individuals are somewhat myopic and believe that $K_{Z_t} = 0$. This amounts to assuming that in each period, parents maximize utility subject to their health knowledge at the start of that period.[4] Thus, the full set of first order conditions for utility maximization are given by

$$\frac{U_{X_1}}{P_X} = \frac{U_{H_1}F_{Z_1} + U_{H_2}(F_{H_1}F_{Z_1} + F_{Z_2H_1}F_{Z_1})}{P_Z} \tag{1.8}$$

and

$$\frac{U_{X_2}}{P_X} = \frac{U_{H_2}F_{Z_2}}{P_Z}. \tag{1.9}$$

The primary aim of this study is to estimate the impact of prenatal care on health outcomes of infants, i.e. $\frac{dH_1}{dZ_1}$ and $\frac{dH_2}{dZ_1}$. Rosenzweig and Schultz show that the observed relationship between H_1 and Z_1 will be biased if μ and Z_1 are correlated because

$$\frac{dH_1}{dZ_1} = F_{Z_1} + F_{\mu}\frac{d\mu}{dZ_1}. \tag{1.10}$$

Since H_2 is a function of H_1, the observed relationship between H_2 and Z_1 will also be biased under the same conditions. Consistent estimates of the relationship between health and health care can be obtained using two-stage least squares, by exploiting

[4] This assumption rules out the possibility that individuals with less education will be more responsive to price changes if κ and Z are substitutes in the production of health knowledge. Without this assumption, it would be possible that individuals with less education may see a larger impact on future health following a decrease in P_Z due to a larger substitution effect between care and consumption goods in period 1.

variation in Z due to changes in P_Z. Empirically, I estimate this impact under the assumption that a higher concentration of prenatal care providers increases access to care and therefore decreases P_Z.

The relationship between prenatal care and future health outcomes,

$$\frac{dH_2}{dZ_1} = F_{H_1}F_{Z_1} + F_{Z_2H_1}F_{Z_1} + F_{Z_2K_2}K_{Z_1} + F_{H_1}F_{\mu}\frac{d\mu}{dZ_1}, \tag{1.11}$$

is subject to the same bias. After using variation in the price of prenatal care to obtain an unbiased estimate of (11), there are two distinct impacts of prenatal care on future health: a direct effect, given by $F_{H_1}F_{Z_1} + F_{Z_2H_1}F_{Z_1}$, and an indirect effect, given by $F_{Z_2K_2}K_{Z_1}$, which occurs because of the impact of care on a mother's health knowledge K_{Z_1}. Differentiating (11) with respect to education also yields two distinct impacts. Though both effects are always weakly positive, the magnitude of the direct impact is increasing in education. The magnitude of the indirect impact may either be increasing or decreasing in education, depending on the relationship between education and prior care in the production of health knowledge. If Z and κ are substitutes in the production of health knowledge so that $K_{Z\kappa} < 0$, then the final term in (11) will be decreasing in education.

If the observed health impact of prenatal care is larger in more educated populations, then the sign of $K_{Z\kappa}$ cannot be determined. If, however, the observed impact of prenatal care is larger in less educated populations, then $K_{Z\kappa} < 0$ and the decreasing magnitude of the final term at higher education levels must be large enough to outweigh the increasing magnitude of the first two terms. If prenatal care is observed to have a larger impact on health in less educated populations, theory implies that Z and κ must be close enough substitutes in the production of health knowledge for the impact from the indirect effect to outweigh those from the direct effect in (11).[5]

1.3.2 Empirical Framework

Typically, prenatal care is assumed to improve health by increasing birth weight. This relationship can be modeled by

$$W_i = \alpha Z_i + S_i \beta + \epsilon_i, \tag{1.12}$$

where W_i represents an infant's birth weight, Z_i represents the number of prenatal care visits, S_i represents a vector of controls, and ϵ_i is a stochastic error term. The controls included in the S_i vector are the sex and gestational age of the infant, the mother's age, race, and, in some specifications, additional controls related to maternal risk factors that precede the pregnancy.[6] Taken together, these controls comprise the observed components of μ from equation (2). The parameter of interest, α, can be interpreted as the impact of prenatal care on an infant's birth weight.

[5]The direct impact on health $\frac{dH_2}{dZ_1}$ is never directly observed. Instead, $\frac{dZ_2}{dZ_1}$ is observed, although in the case of inpatient care it may be reasonable to think that care utilization is a proxy for health status. The impacts of prenatal care on future care utilization will be analogous to those discussed above for types of care where the direct effect and indirect effect both impact utilization in the same manner. If, for instance, improved health weakly decreases care in acute, resource intensive settings (i.e. emergency department and inpatient settings,) and $F_{ZK} > 0$ occurs because of more efficient usage of health care resources, then a larger observed impact of prenatal care in less educated populations still implies that education and health care are substitutes in the production of health knowledge.

[6]Although an infant's gestational age is an outcome of pregnancy, evidence suggests the direction of causality flows from gestational age to prenatal care. Fiscella (1995) discusses the spurious relationship between preterm delivery and "a reduced opportunity for any prenatal care." In a review article, Goldenberg and Rouse (1998) cite five studies that investigate the impact of prenatal care on gestational age and note that none of them find evidence suggesting prenatal care is able to influence gestational age. They also cite ten randomized trials regarding enhanced prenatal care interventions and note that the majority of them find no impact of enhanced prenatal care on gestational age. Additionally, before 2004 the data do not distinguish between gestational diabetes and diabetes that precedes pregnancy. I choose to include the presence of any diabetes as a control in these specifications because of its close association with birth weight and because a review of the medical literature suggests prenatal care may effectively mitigate the adverse effects of, but does not impact the presence of, diabetes during pregnancy (Korenbrot et al., 2002).

As discussed previously, there may be several reasons to believe that the number of prenatal care visits a mother seeks is endogenous with respect to other unobserved factors which might also influence her pregnancy and her child's birth weight. If visits are actually endogenous, the estimated impact of prenatal care from equation (12) will be biased and will not reflect the true, causal impact of prenatal care. In order to consistently estimate the causal impact of prenatal care on birth weight, I employ an instrumental variables approach. The number of prenatal care visits is described by

$$Z_i = \delta P_i + S_i \Gamma + \upsilon_i, \tag{1.13}$$

where prenatal care visits are a function of S_i, the same vector of controls appearing in (12), and P_i, the concentration of prenatal care providers serving mothers in the Medicaid population in the county and year of birth for child i. After estimating equation (13), the *causal* impact of prenatal care on birth weight can be estimated by

$$W_i = \alpha \widehat{Z}_i + S_i \beta + \epsilon_i, \tag{1.14}$$

where $\widehat{Z}_i$ is the predicted number of prenatal care visits from equation (13).[7]

Estimation of equation (14) will be consistent provided that P_i influences the number of prenatal care visits women receive and is uncorrelated with the error term ϵ_i. It is possible to assess the former condition by looking at the significance of P_i when estimating equation (13), but because provider concentration is the lone candidate instrument, it is only possible to determine in theory whether the restriction condition

[7]It is possible to predict a negative number of visits for some observations when modeling prenatal care as a linear function of parameters and a continuous error term, but no observations have a negative predicted number of visits in my data.

is valid. It seems likely that concentration of prenatal care providers does not directly impact birth weight except through the channel of increased access to care. Therefore, as long as provider location is exogenous and unrelated to unobserved factors that influence pregnancy, the provider concentration will be uncorrelated with the error term in equation (12) and will be a valid instrument.

One might worry that provider location is not truly exogenous to the location of pregnancies. Although a doctor's location is presumably chosen before the onset of each individual pregnancy, it is still possible that some mothers may at least partially base their location choice on access to prenatal care or may move to obtain better access to care once they become pregnant. In this case, provider concentration may not be a valid instrument if the types of mothers who are likely to move to areas with more doctors have other unobserved characteristics that will also influence their child's birth weight. This worry is mitigated by Schwartz and Sommers (2014), who find no significant cross-state migration in response to the expansion of Medicaid under the Affordable Care Act, which presumably created more variation in relative prices than the cross-county variation in provider concentration used here.

As an additional test of the hypothesis that a mother's location choice is determined by access to prenatal care, I compare the impact of provider concentration on prenatal care visits by mothers who give birth at 16 or 17 years of age to the impact of concentration on visits by mothers who give birth at 19 or 20 years of age. If mothers systematically choose their locations as a function of access to prenatal care and the instrument is not valid, there should be a larger relationship between prenatal care and provider concentration for the older group under the assumption that mothers who are older than 18 are more likely to live independently and are therefore more mobile than are those under 18. I find no significant difference in the relationship between provider concentration and visits between the two groups, suggesting that this type of endogeneity is not a concern.[8]

Evidence suggests that provider location is in fact exogenous to the location of each individual pregnancy, but one may still be concerned that provider location is systematically related to unobserved factors that also impact pregnancies. If providers systematically locate in areas where there are poor birth outcomes or in geographic areas with other factors that impact birth outcomes (e.g. pollution, social services), then the concentration of providers will not be a valid instrument. Although there is some within county variation in provider concentration, by and large the smallest concentrations of providers are found in populous urban counties across all observed years. This suggests that systematic location choice may be an issue.

To address this issue, I include an indicator for whether each birth occurs in a rural county in all specifications. As the cut-off between urban and rural is somewhat arbitrary, I check to make sure all results are robust to different groupings of rural and urban counties. Finally, I also estimate all models after excluding Charleston, Greenville, and Richland counties.[9] In all cases, the results are robust to each possible classification of rural and urban counties and the exclusion of the three largest counties, suggesting that this type of endogeneity is also not a concern.

It is possible that prenatal care may impact health independently of its effect on birth weight. To analyze whether this is the case, I look at infant health care utilization in the year following birth for three distinct types of care: well-child visits, care in inpatient settings, and emergency department visits. All three types of care

[8] I observe 20,666 births to mothers aged 16 or 17, 61,597 births to mothers aged 19 or 20, and over one quarter of my sample has birth mothers between 16 and 20 years of age. The point estimate for younger mothers is actually slightly larger, but this difference is not statistically significant. These results are robust to using several different cutoffs and groupings of age, as well as to a specification including an interaction between provider concentration and age across all births.

[9] These three counties are the largest by population in South Carolina and account for 23.9 percent of observed births in my sample. The difference in population between the third largest (Charleston) and fourth largest (Spartanburg) counties is larger than the total population in 30 of South Carolina's 46 counties.

can be viewed through the same lens of a latent variable framework, where

$$Y_i^* = \gamma Z_i + D_i\Theta + \eta_i, \tag{1.15}$$

and Y_i^* is an individual's propensity to seek care beyond a certain threshold.

In this case, the propensity to seek care within the first year of life is influenced by prenatal care, a vector D_i of other observed factors (which is a subset of the same factors that influenced birth weight,) and a stochastic error term η_i. Of course, Y^* is never observed. Instead, only

$$Y_i = \begin{cases} 0 & \text{if } Y_i^* \leq 0; \\ 1 & \text{if } Y_i^* > 0 \end{cases} \tag{1.16}$$

is observed. Assuming the error term in (15) is normally distributed, the probability that $Y_i = 0$ is $1 - \Phi(\gamma Z_i + D_i\Theta)$ and the probability that $Y_i = 1$ is $\Phi(\gamma Z_i + D_i\Theta)$, where $\Phi(\bullet)$ is the standard normal distribution function. The impact of prenatal care on the probability of utilizing a certain type of care can be estimated by maximing the likelihood function

$$LF = \Pi[1 - \Phi(\gamma Z_i + D_i\Theta)]^{(1-Y_i)}\Phi(\gamma Z_i + D_i\Theta)^{Y_i}. \tag{1.17}$$

The concerns about the potential endogeneity of prenatal care discussed above, however, are still valid when considering future health outcomes. If prenatal care truly is endogenous, the estimated impact $\widehat{\gamma}$ will be biased and an instrumental variables approach may again be appropriate. Using equation (13) as a source of exogenous variation in the number of visits, the causal impact of prenatal care on health care utilization can be estimated using

$$LF = \Pi[1 - \Phi(\gamma\widehat{Z}_i + D_i\Theta)]^{(1-Y_i)}\Phi(\gamma\widehat{Z}_i + D_i\Theta)^{Y_i} \qquad (1.18)$$

where $\widehat{Z}_i$ is the predicted number of prenatal care visits from equation (13).

When the outcome variable of interest is health care utilization within the first year of life, it is plausible to think that the exclusion restriction of prenatal care provider concentration discussed above may not be entirely appropriate. Currie and Reagan (2003) show that the probability of an inner city child receiving WCVs is inversely related to the distance they live from the closest hospital. Therefore, if provider concentration serves as a proxy for the general availability of or access to other health care services, it may belong in the second stage regression. There is also a moderate amount of overlap between doctors that provide prenatal care and those that provide care to infants. In the data, approximately 22.8 percent of prenatal care providers are also observed providing pediatric services, while 59.8 percent of doctors that provide pediatric services are also observed delivering prenatal care. To address these additional concerns about the validity of the instrument, I include an additional variable in the D_i vector to control for the concentration of pediatric health care providers available to child i in every county-year pair. In all cases, the control is the concentration of all providers observed to provide any well-child care to infants within a given county and year.

1.4 Data used for Estimation

Data from two sources, South Carolina birth certificates and South Carolina Medicaid claims, are used for estimating the models in section 3. The birth certificate data contain all observed births in the state of South Carolina between 2001 and 2012 where Medicaid was identified as the source of payment on the birth certificate, providing a total of 426,319 births. Over this same time period, there were 701,903 total births in South Carolina (CDC National Vital Statistics Reports); hence Medicaid financed births accounted for just over 60 percent of all births in South Carolina.[10] The birth certificate records reflect the month of pregnancy during which prenatal care was initiated as well as the total number of prenatal care visits. Additionally, they contain an infant's year of birth, sex, birth weight, and gestational age, as well as information regarding the mother. Maternal characteristics include her county of residence, age, educational achievement, race, and certain risk factors that could impact a pregnancy such as tobacco use, diabetes, and hypertension. South Carolina made significant changes to their birth certificates in 2004. Some new data elements were collected, including whether a mother used tobacco prior to her pregnancy, while others data elements, such as a mother's educational achievement, were aggregated differently following the change. To the extent possible, I recategorize the data from earlier years to permit comparison to data from 2004 onward.

Medical billing claims submitted for reimbursement under South Carolina's Medicaid program provide the second source of data. These administrative data contain Current Procedural Terminology (CPT) codes reflecting the procedures that were performed, the patient's age at the time of service, unique provider identifiers, and indicators for whether or not the claim originated in the ED. Using all claims submitted between 2001 and 2012, I construct a measure of the concentration of doctors providing prenatal care by counting the unique providers who both deliver a child and bill Medicaid for at least one ordinary doctor's visit made by a mother

[10]This is somewhat higher than the percentage of Medicaid financed births that is generally reported, but may be due to differences in data collection as the numerator and denominator of this measure come from entirely separate sources.

in each county-year pair. This is a similar procedure to that described by Currie et al. (1995) who impute the reimbursement rate for prenatal care in states such as South Carolina that do not use a global obstetric care fee. I then normalize this count by dividing the raw number of providers by the total number of Medicaid financed births in a county-year as a proxy to measure access to care. The tenth and ninetieth percentiles of this concentration correspond to roughly one doctor for every 16.6 births and one doctor for every 5.3 births, respectively.[11] Concentrations for pediatricians providing well-child services to infants are generated in a similar manner. In addition to these measures, I construct count variables measuring the number of WCVs as well as the number of claims within the first year of life that originate in the ED or are submitted with an inpatient care code. Over the 12-year time period studied, I observe 7,801,731 total claims made by infants in my sample within the first year of their life, averaging 20.1 claims per infant.

I match data on 389,391 birth certificates with Medicaid claims made in the first year of life. While most of these records are complete, a small number of birth certificates are missing some information; excluding these observations leaves 384,605 observations.[12] I use this remaining sample when estimating the impact of prenatal care on birth weight. In order to obtain unbiased estimates of the impact of prenatal care on health outcomes within the first year of life, I restrict the sample to births occuring between 2001 and 2011 to allow for an entire year of observation following

[11]These represent the tenth and ninetieth percentiles of births, therefore counties with more births are more heavily weighted. The unweighted percentiles correspond to one doctor for 15.3 and 3.7 births, respectively.

[12]A large number of birth certificates are missing information regarding tobacco usage before the pregnancy, including all those before 2004 when the data were not collected. For these observations, I assume that the mother used tobacco before pregnancy if I observe tobacco usage during pregnancy, and include a separate indicator to differentiate between a missing value when no tobacco usage is documented and those that clearly state tobacco was not used before the pregnancy. For the subset of my sample that I observe using tobacco during their pregnancy and also observe pre-pregnancy behaviors, over 99 percent used tobacco before pregnancy.

birth. For this portion of the estimation, 351,850 births are used. Table 1 shows selected summary statistics of the sample used for estimation.

One potential shortcoming of the data is that they do not reflect any deaths. This could be problematic for this analysis as mortality will truncate the period of time in which infants could have made subsequent medical claims, and could lead to some bias in the predicted impact of prenatal care on outcome variables of interest that measure future care. Under the assumption that prenatal care weakly decreases the infant mortality rate, it is possible to sign the direction of this bias and discuss its implications for inference. Additionally, if there is bias due to unobserved infant mortality, then differences in the rates of medical care utilization between the majority of low-risk infants and those infants who are at highest risk of mortality should be apparent.

In the presence of some infant mortality, the observed counts of all types of doctor visits will be weakly lower than it would be without any mortality. For example, if a child is observed to have zero inpatient claims there are two possible explanations: the first being the child was simply healthy and did not have any conditions that required acute care, the second being the child died before being admitted for inpatient care. If prenatal care decreases infant mortality, some additional surviving infants may be hospitalized. In the case where additional prenatal care visits are associated with an increased life expectency and an increased life expectency inflates the probability of receiving care in an inpatient setting, the estimated impact of prenatal care will be biased upwards. In fact, for all variables measuring the probability of receiving care or receiving care beyond a certain threshold, the direction of this bias will always be upwards. Therefore, the estimated impact of prenatal care on future health care utilization provides an upper bound.

Although estimating the impact of prenatal care on infant mortality is well beyond the scope of this study, infant mortality is significantly higher in infants who are born at less than full gestation (MacDorman and Mathews, 2009) and in infants that are born at weights less than 2,500 grams (Centers for Disease Control and Prevention, 2002). Moreover, infant mortality rates increase as the distance from full gestation and normal birth weight increases. Because of these factors, the expected infant mortality rate should be significantly higher in the full population than it would be in a restricted sample of full term, normal birth weight infants. As such, if any bias exists, one would expect to see differences in the estimated impacts of prenatal care between these two samples. As a robustness check, I restrict my sample to infants born between 37 and 42 weeks of gestational age and 2,500 and 4,000 grams inclusive, and re-estimate equation (18) for each measure of future health care utilization. In every case, there is no statistical difference in the estimated impact of prenatal care, suggesting this bias is not a large concern.

1.5 Estimation Results

The empirical results can be divided into three main categories: the estimated determinants of prenatal care utilization, the estimated impact of prenatal care on birth weight, and the estimated impact of prenatal care on future health care utilization. Table 2 contains estimation results from equation (13), showing the estimated impacts of several factors, including the concentration of prenatal care providers, on maternal prenatal care utilization. Both regressions include an indicator for whether the updated post-2003 birth certificate was used, and the estimates in column 2 also include controls for the number of previous live and still births, risk factors related to prior methods of delivery, abnormal birth conditions, and additional maternal risk factors. In both cases, the standard errors are clustered to allow for the possibility of correlation across observations in each county-year pair.

The estimated coefficient on provider concentration is positive and precisely estimated in both specifications, though the estimated impact of provider concentration on care utilization is slightly smaller when including additional maternal controls in column 2. The magnitude of the estimate shown in column 2 suggests moving from a county in the 10th percentile to one in the 90th percentile of provider concentration is associated with an expected increase of approximately 0.7 prenatal care visits. This represents an increase of just over 5 percent at the median number of visits. In both cases, the F-statistic for provider concentration is above 10, the threshold that is traditionally used in order to assess whether a candidate instrument is a strong enough predictor of an endogenous outcome.[13]

The estimated impacts of prenatal care on birth weight, which are obtained using an instrumental variables approach by estimating equation (14), are contained in Table 3. The estimated coefficient on visits in column 1, with no controls for potential maternal choice variables, has a point estimate suggesting that prenatal care actually causes decreased birth weight. This negative association, however, disappears in the estimated coefficient on visits shown in column 2 when additional maternal controls are added. In column 2, the estimated impact of an additional prenatal care visit on birth weight is 8.4 grams, a result that is statistically significant at the 10 percent confidence level. These results suggest that prenatal care may have a modest positive impact on birth weight.

The estimated coefficients of the covariates contained in both columns of Table 3 are similar across both specifications. Additionally, in column 2 the estimated coef-

[13] These results are similar qualitatively to the point-estimates obtained when estimating equation (13) using an indicator for whether prenatal care was started in the first trimester as the dependent variable, but the estimates obtained when using this alternative measure of care are not precisely estimated. The estimates obtained in subsequent analyses using this measure of care are similarly noisy and are therefore not robust to this alternative specification.

ficients related to maternal age, education, and risk factors are strongly statistically significant and of the expected sign. Mothers with lower levels of education tend to give birth to lower birth weight infants, as do those that smoke before pregnancy and those who give birth at a younger age. On the other hand, mothers who are white, live in urban counties, or have diabetes all tend to give birth to heavier infants.[14]

Despite the fact that prenatal care does not appear to substantially increase birth weight in this sample, it is possible that prenatal care confers health benefits in other ways. I test this hypothesis by looking at three types of care utilized in the first year of life: well-child doctor visits, inpatient medical claims, and emergency department visits. The concern that prenatal care could potentially be endogenous are still present when future health care utilization is the outcome variable, and so an instrumental variables probit model is estimated (equation 18) to identify the causal impact of prenatal care on future health care utilization.

Table 4 contains estimates where the dependent variable is an indicator of well-child care. In columns 1 and 2, the dependent variable is whether an infant received any WCVs, in columns 3 and 4 the dependent variable is whether an infant received four or more WCVs and in columns 5 and 6 the dependent variable is whether an infant received six or more WCVs. The threshold of four is used following Reichman et al. in the only comparable work to date, and the threshold of six is tested because the American Association of Pediatrics (2014) recommends that infants have six WCVs within the first year of life. Columns 1, 3, and 5 provide estimates when maternal controls are excluded, and in all cases the estimated coefficient on prenatal care visits is smaller than the corresponding estimates when the maternal controls

[14]The association between maternal weight, diabetes, and heavier infants is well documented in the medical literature (Boney et al., 2005; Gillman et al., 2003). These studies have found that the joint association between gestational diabetes and infants that are large for gestational age are associated with adverse health outcomes and this large positive coefficient should not be interpreted as a beneficial outcome of diabetes.

are included. Much like prenatal care, well-child care is a form of preventive care and it seems likely that these additional maternal controls would at least partially explain the decision to seek care in both prenatal and well-child settings and should be included.

The result in the second column of Table 4 suggests that additional prenatal care visits increase the probability that a child will make at least one WCV within the first year of life, although this result is only marginally statistically significant. The probit coefficient reported is statistically significant at the 10 percent level, but the estimated average marginal effect, which is reported in Table 8, falls just short of this threshold.[15] The point-estimated average marginal effect of an additional prenatal care visit on the probability of an infant receiving any WCVs is 1.35 percent.

The result in the fourth column of Table 4 suggests that additional prenatal care visits are also associated with an increased probability of making four or more WCVs. This is consistent with results found by Reichman et al. (2010), and the estimated average marginal impact of an additional prenatal care visit on the probability of having at least four WCVs is 2.61 percent. The result in column 6 of Table 4 shows that additional prenatal care visits are not estimated to have any significant impact on the probability of meeting the recommended number of six WCVs within the first year of life.[16] The estimated positive impacts of prenatal visits reported in columns 2 and 4 are robust to the usage of a linear probability model, alternative definitions of rural counties, the exclusion of data that utilizes the pre-2004 birth certificates, and

[15]Tables 4 through 6 report probit coefficients and Table 8 provides a compact summary of the average marginal effects of prenatal care on well-child, emergency, and inpatient care when additional maternal controls are included. The marginal effect for each observation is calculated using the estimated probit coefficients and observed values for all included parameters.

[16]This could be driven by misrepresentations of well-child visits in the claims data if, for instance, doctors bill for services that are classified as "well-child" on visits that are primarily due to illness. If this problem were to exist, it seems likely to be most prevalent for children with higher observed counts of WCVs.

the exclusion of Charleston, Greenville, and Richland counties, but are robust to the inclusion of neither year nor county fixed effects.

Inpatient medical claims are the second health outcome I investigate. The vast majority of infants in my sample (about 95 percent) have at least one inpatient claim within their first year of life, which is likely driven by claims made immediately following birth. Since the data do not distinguish between claims made immediately following birth but before an initial discharge and those made later, I estimate the impact of prenatal care on the probability of an infant having more than one inpatient visit within the first year of life.[17] Any observed changes in health care utilization may also provide a valid measure of changes in health if the initial inpatient claim made by most infants is independent of prenatal care and if more severe illnesses are more likely to require treatment in an inpatient setting.

Table 5 shows results of estimating equation (18) using inpatient claims as the dependent variable. Column 1 contains estimates when excluding maternal controls, and column 2 shows the estimates when maternal controls have been included. The estimated impact of an additional prenatal visit on the expected probability of requiring inpatient care is negative and strongly significant in both columns and is statistically indistinguishable regardless of whether maternal controls are included. When additional maternal controls are included, the average marginal effect of an additional prenatal care visit is an approximately 4.0 percent decrease in the expected probability of requiring care in an inpatient setting on multiple occasions. These results are robust to use of a linear probability model, alternative definitions of rural counties, the exclusion of data that utilizes the pre-2004 birth certificates,

[17] To ensure that there is nothing systematically different about the small subset of infants with zero inpatient medical claims, I also estimate these models after excluding all infants with exactly zero inpatient claims. The estimated signs and magnitudes of the results are similar across both specifications.

the exclusion of Charleston, Greenville, and Richland counties, and year fixed effects. They are not, however, robust to the inclusion of county fixed effects, though this is somewhat expected due to limited within county variation of provider concentration.

Emergency department visits are the final outcome I investigate. Roughly half of the infants in my sample make at least one ED visit within the first year of their life. It is possible that prenatal care could influence ED utilization directly by improving infant health, or indirectly by improving maternal knowledge of the health care system and the availability of alternative sources of care. If infants in better health are less likely to require emergency services, then prenatal care should be associated with a decreased probability of visiting the ED. On the other hand, if ED visits do not reflect the underlying health of an infant and are instead determined by short-term health shocks and access to other forms of care, then prenatal care will only impact ED utilization through increased health knowledge. Supporting the view that ED usage is relatively price inelastic and may therefore be driven by health shocks, Finkelstein et al. (2012) do not find a statistically significant impact on the probability of utilizing emergency care in the adult Medicaid population following Oregon's expansion of Medicaid.

Table 6 shows the results of estimating equation (18) using emergency department care as the dependent variable. The estimated impact of prenatal care on ED utilization in column 1 is small, and not statistically different from zero. After controlling for maternal characteristics, the results contained in column 2 show that prenatal care has no estimated impact on the probability of receiving care in an ED. As discussed above, there are good reasons to believe that ED utilization is at best a noisy measure of health, and these results do not provide any evidence of prenatal care's ability to influence ED utilization through health knowledge.

To test the hypothesis that prenatal care and education are substitutes in the production of health knowledge, I estimate the impact of prenatal care on both birth weight and future health care utilization when including interaction terms between indicators for the number of prenatal care visits and whether a mother has either less than a high school degree or a high school degree with no post-secondary education. The omitted category is a mother with any form of post-secondary education. If prenatal care is endogenous, these interactions will also be endogenous and so I generate additional instruments by interacting the education indicators with the concentration of prenatal care providers. This procedure means there is a fair amount of collinearity between my instruments, and so I am only able to estimate whether there is a differential impact of prenatal care across education levels within the framework of a linear probability model. An additional concern in this specification is that a mother's education level may be endogenous, particularly if she became pregnant before having the chance to finish high school. As such, these estimations exclude all mothers who are 19 years of age or younger at the time they give birth.

Table 7 shows the results of estimating equation (14) with birth weight as the dependent variable in column 1 and three distinct indicators of care as the dependent variable in columns 2 through 4: any WCVs in column 2, multiple inpatient claims in column 3, and any ED claims in column 4. In all specifications, the coefficients of interest are those of the interactions between education levels and the number of visits. In column 1, neither interaction term is statistically different from zero. These results suggest that health knowledge does not directly impact the productivity of prenatal care as measured by birth weight. It is also some evidence that education is not serving as a proxy for an omitted variable, such as income, that is causing an observed differential impact of prenatal care on future care utilization. In column 2, both interaction terms are small and not statistically different from zero, suggesting there is not a larger effect of prenatal care visits for less educated mothers.[18] In both columns 3 and 4, the estimated differential impact of an additional prenatal care visit for a mother with less than a high school education compared to a mother with some post-secondary education is large and strongly statistically significant, while the estimated differential impact for a mother with a high school degree compared to one with some post secondary education is small and statistically significant. In both columns 2 and 3, the difference between the estimated impacts for mothers with no high school degree and those with a high school degree but no post-secondary education is statistically significant at the 99 percent confidence level.

These results suggest that additional prenatal care visits have a larger impact in less educated populations, and that the magnitude of this impact monotonically decreases as education increases. This is consistent with the hypothesis that prenatal care and formal education are substitutes in the production of health knowledge. It also appears that prenatal care may actually decrease the utilization of ED care in less educated populations. The point estimate of an additional prenatal care visit for a mother with less than a high school degree implies that an additional visit will decrease the probability of her child receiving care in an ED by 2.23 percent, however, this effect is not statistically significant at traditional levels with a p-value of 0.111.

1.6 Conclusions

Prenatal care has potential to influence an infant's health through multiple dimensions. The dimension that has received the most attention in the literature has

[18] I choose to report the threshold of one WCV instead of four or six due to potential concerns over measurement error at higher observed counts of well child visits. Although not reported, the negative differential effects when the threshold for WCVs is set at four or six visits become significant. This is consistent with the theoretical possibility that improved health from prenatal care may outweigh any effect due to knowledge, but also supports the hypothesis that there may be some misrepresentations in the data at high counts of WCVs.

been its potential to increase birth weight. Although there is an established correlation between prenatal care and increased birth weight, studies that have controlled for the endogeneity of care have not always found evidence of a causal relationship or have only found a causal relationship in certain subpopulations or types of pregnancies. Consistent with these results, I find a small positive effect of prenatal care on an infant's birthweight in South Carolina's Medicaid eligible population between 2001 and 2012.

I also estimate the impact of prenatal care on the utilization of three distinct types of health care within the first year of life. Despite the fact that prenatal care appears to have a limited impact on birthweight, I find that it does increase the probability of an infant making well-child visits and decreases the probability of requiring care in an inpatient setting. Typically, severe illnesses are more likely to require inpatient treatment, so this utilization pattern suggests that increasing access to prenatal care may improve the health of an infant beyond any direct impact on birth weight. These results persist even when the sample is restricted only to normal birth weight, full gestational age infants, and are robust to a variety of specifications. Additionally, the magnitude of this impact increases for mothers with lower levels of education. I do not find any significant relationship between prenatal care and emergency department utilization, but I find some evidence that additional prenatal care visits may decrease ED utilization for children of less educated mothers, though this effect is not precisely estimated and is therefore not statistically significant at traditional confidence levels.

There are potentially large cost savings attributable to the changes in future health care utilization patterns that arise following prenatal care. For the mothers in my sample, the average amount paid per claim by Medicaid for a prenatal care visit was $61.56. Increased utilization of prenatal care appears to increase the number of

WCVs at an average amount paid per claim of \$76.97. These costs, however, are dwarfed by the average paid claim amount of \$1,022.42 for inpatient claims made by infants within the first year of life. In a linear framework, I estimate that an additional prenatal care visit will decrease the expected number of inpatient claims made by an infant within the first year of life by 0.52 visits, saving \$528.98 in expected future medical costs. It must be noted that this effect is not precisely estimated, and a 95 percent confidence interval implies that the expected decrease in future expenditures from an additional prenatal care visit is somewhere between \$40.86 and \$1,017.09.

The instrumental variables approach employed in this study suggests that the relationship between prenatal care and future health outcomes is causal. The full extent of the causal mechanisms is not identified, however, meaning the exact policy implications are unclear. If prenatal care directly improves the health of infants or mothers, then it may be desireable for Medicaid to continue to expand access to and increase the utilization of prenatal care. I find evidence that is consistent with the hypothesis that prenatal care influences health care utilization at least partially through providing information and by influencing a mother's interactions with the health care system. If prenatal care impacts future health outcomes and health care utilization primarily through this mechanism, then there may be more cost-effective ways to provide this information.

Table 1.1: Summary Statistics

	Mean	Std. Dev.
weight	3164.223	585.052
prenatal visits	11.649	4.963
gestational age	38.334	2.109
mother's age	24.254	5.466
black	0.453	0.498
hispanic	0.115	0.319
other race	0.017	0.130
no hs degree	0.330	0.470
hs grad only	0.348	0.476
rural county	0.403	0.490
tobacco	0.192	0.394
missing tobacco	0.424	0.494
diabetes	0.051	0.220
any WCV	0.908	0.290
four WCV	0.611	0.487
six WCV	0.118	0.323
any ED	0.503	0.500
inpatient	0.563	0.496
observations	384,605	

Table 1.2: Determinants of Prenatal Care Utilization

	(1)	(2)
VARIABLES	Visits	Visits
providers per birth	6.963***	5.254***
	(1.198)	(1.339)
gestational age	0.346***	0.364***
	(0.00695)	(0.00690)
male	−0.0508***	−0.0571***
	(0.0154)	(0.0150)
mother's age	0.0476***	0.0715***
	(0.00260)	(0.00239)
black	−0.483***	−0.551***
	(0.0534)	(0.0539)
hispanic	−2.445***	−2.235***
	(0.0871)	(0.0859)
other race	−1.140***	−1.208***
	(0.0915)	(0.0936)
no hs degree		−0.417***
		(0.0406)
hs grad only		−0.0437
		(0.0294)
rural county		0.504***
		(0.138)
tobacco		−0.192***
		(0.0655)
diabetes		2.215***
		(0.0684)
Observations	384,605	384,605

Note: Table 2 presents the results of estimating a linear model with the number of prenatal care visits as the dependent variable. Column 2 contains controls for observed maternal risk factors, including additional controls not reported. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 1.3: IV on Birth Weight

VARIABLES	(1) Weight	(2) Weight
visits	−5.741**	8.442*
	(2.864)	(4.890)
gestational age	185.298***	180.917***
	(1.052)	(1.828)
male	117.138***	117.896***
	(1.441)	(1.449)
mother's age	7.833***	1.553***
	(0.185)	(0.373)
black	−155.214***	−182.055***
	(2.478)	(3.135)
hispanic	−18.588**	−6.469
	(7.653)	(11.460)
other race	−93.672***	−95.645***
	(6.099)	(7.612)
no hs degree		−56.367***
		(3.270)
hs grad only		−27.928***
		(1.794)
rural county		−23.034***
		(5.176)
tobacco		−105.763***
		(3.170)
diabetes		168.679***
		(12.007)
Observations	384,605	384,605

Note: Table 3 presents the results of estimating a 2SLS model with birth weight as the dependent variable. Column 2 contains controls for observed maternal risk factors, including additional controls not reported. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 1.4: IV on Probability of Exceeding Well-Child Visit Thresholds

VARIABLES	(1) Any	(2) Any	(3) Four	(4) Four	(5) Six	(6) Six
visits	0.0635*	0.0774*	0.0452	0.0741**	−0.0403	0.0521
	(0.0361)	(0.0436)	(0.0324)	(0.0360)	(0.0861)	(0.0910)
gestational age	0.0359**	0.0289	0.0285**	0.0152	0.0408	0.00646
	(0.0149)	(0.0195)	(0.0121)	(0.0151)	(0.0266)	(0.0347)
male	0.00175	0.00247	−0.0116**	−0.0101*	−0.00912	−0.00565
	(0.00655)	(0.00661)	(0.00490)	(0.00516)	(0.00644)	(0.00759)
mother's age	−0.0141***	−0.0145***	−0.00768***	−0.0115***	−0.00734	−0.0116***
	(0.00156)	(0.000730)	(0.00153)	(0.000667)	(0.00455)	(0.000992)
black	0.0124	0.0370	−0.0764***	−0.0608**	−0.0859**	−0.00427
	(0.0234)	(0.0310)	(0.0208)	(0.0290)	(0.0381)	(0.0596)
hispanic	0.0179	0.0650	0.0486	0.132	0.00196	0.186
	(0.114)	(0.124)	(0.0860)	(0.0892)	(0.221)	(0.201)
other race	0.103**	0.120**	0.0539	0.0682	0.0433	0.128
	(0.0477)	(0.0549)	(0.0418)	(0.0471)	(0.0986)	(0.0924)
pediatricians	−0.518	−0.354	−0.654**	−0.416	−0.552	0.158
	(0.399)	(0.352)	(0.307)	(0.278)	(0.610)	(0.584)
no hs degree		−0.00950		−0.117***		0.0422
		(0.0376)		(0.0332)		(0.0556)
hs grad only		0.0332***		−0.0160		0.0388***
		(0.0126)		(0.0108)		(0.0148)
rural county		−0.101**		−0.179***		−0.444***
		(0.0455)		(0.0375)		(0.0670)
tobacco		0.0214		−0.0935***		−0.0482
		(0.0268)		(0.0215)		(0.0388)
diabetes		−0.134		−0.124		−0.136
		(0.111)		(0.0920)		(0.219)
Observations	351,850	351,850	351,850	351,850	351,850	351,850

Note: Table 4 presents the results of estimating an IV Probit model with well child visits as the dependent variable. All columns are restricted to births before 2012. Columns 2, 4, and 6 contain controls for observed maternal risk factors, including additional controls not reported. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 1.5: IV on Probability of Multiple Inpatient Claims

VARIABLES	(1) Inpatient	(2) Inpatient
visits	−0.124***	−0.117***
	(0.0299)	(0.0353)
gestational age	0.0222*	0.0221
	(0.0135)	(0.0157)
male	0.0617***	0.0650***
	(0.0123)	(0.0134)
mother's age	−0.0112***	−0.0117***
	(0.00402)	(0.00297)
black	0.0618*	0.0721
	(0.0344)	(0.0448)
hispanic	−0.216**	−0.198**
	(0.0869)	(0.0890)
other race	−0.172***	−0.155***
	(0.0397)	(0.0462)
pediatricians	0.465	0.561*
	(0.341)	(0.306)
no hs degree		0.0956*
		(0.0507)
hs grad only		0.0696***
		(0.0212)
rural county		0.0692*
		(0.0413)
tobacco		0.107***
		(0.0341)
diabetes		0.343***
		(0.0770)
Observations	351,850	351,850

Note: Table 5 presents the results of estimating an IV Probit model with multiple inpatient claims as the dependent variable. All columns are restricted to births before 2012. Column 2 contains controls for observed maternal risk factors, including additional controls not reported. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 1.6: IV on Probability of any Emergency Department Visits

VARIABLES	(1) Any ED Care	(2) Any ED Care
visits	0.0237	0.00984
	(0.0226)	(0.0239)
gestational age	−0.00474	0.00204
	(0.00799)	(0.00868)
male	0.0762***	0.0765***
	(0.00430)	(0.00440)
mother's age	−0.0328***	−0.0259***
	(0.00100)	(0.000739)
black	0.301***	0.327***
	(0.0145)	(0.0192)
hispanic	0.220***	0.128**
	(0.0588)	(0.0589)
other race	−0.132***	−0.125***
	(0.0349)	(0.0365)
pediatricians	−0.166	−0.202
	(0.213)	(0.198)
no hs degree		0.305***
		(0.0175)
hs grad only		0.165***
		(0.00734)
rural county		0.0549**
		(0.0219)
tobacco		0.121***
		(0.0132)
diabetes		0.0400
		(0.0585)
Observations	351,850	351,850

Note: Table 6 presents the results of estimating an IV Probit model with any emergency department claims as the dependent variable. All columns are restricted to births before 2012. Column 2 contains controls for observed maternal risk factors, including additional controls not reported. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 1.7: Interactions Between Prenatal Care and Education

VARIABLES	(1) Weight	(2) One WCV	(3) Inpatient	(4) Any ED
visits	9.311*	0.0160*	−0.0295	0.0163
	(5.435)	(0.00902)	(0.0257)	(0.0108)
no hs degree×visits	3.313	−0.00184	−0.0877***	−0.0386***
	(5.870)	(0.00748)	(0.0277)	(0.0110)
hs grad only×visits	−6.580	−0.00605	−0.0178*	−0.0140***
	(4.015)	(0.00422)	(0.00935)	(0.00537)
gestational age	182.5***	0.00773***	0.0102	0.00349
	(1.788)	(0.00274)	(0.00943)	(0.00275)
male	121.2***	7.13e-05	0.0290***	0.0311***
	(1.771)	(0.00124)	(0.00287)	(0.00202)
mother's age	0.932***	−0.00220***	−0.00601***	−0.00971***
	(0.357)	(0.000224)	(0.000588)	(0.000255)
no hs degree	−93.98	0.0233	1.052***	0.565***
	(66.97)	(0.0870)	(0.305)	(0.125)
hs grad only	50.51	0.0778	0.253**	0.232***
	(48.49)	(0.0516)	(0.114)	(0.0653)
rural county	−23.45***	−0.0153*	0.0392	0.0244***
	(5.642)	(0.00828)	(0.0239)	(0.00888)
black	−183.6***	0.00820*	0.0415***	0.130***
	(3.025)	(0.00449)	(0.0154)	(0.00568)
hispanic	0.695	0.0151	−0.154**	0.0260
	(14.13)	(0.0214)	(0.0731)	(0.0259)
other race	−102.1***	0.0232**	−0.0731**	−0.0484***
	(8.479)	(0.0106)	(0.0359)	(0.0148)
tobacco	−111.4***	0.00526	0.0499***	0.0456***
	(3.627)	(0.00449)	(0.0126)	(0.00533)
diabetes	175.0***	−0.0228	0.173**	0.0229
	(12.88)	(0.0200)	(0.0686)	(0.0241)
pediatricians		−0.0630	0.253	−0.0613
		(0.0624)	(0.162)	(0.0769)
Observations	280,243	280,243	280,243	280,243

Note: Table 7 presents the results of estimating a 2SLS model allowing for interaction between prenatal care and education levels. Column 1 presents the results with birth weight as the dependent variable while Columns 2, 3, and 4 present the results of a linear probability model with dependent variables of any well child visits, multiple inpatient claims, and any emergency department care respectively. All columns are restricted to include only mothers 20 years of age and older at the time of birth and contain controls for observed maternal risk factors, including additional controls not reported. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 1.8: Marginal Effects

	(Visits)	
	Avg. Marginal Effect	SE
any WCV	0.0135	0.00942
four WCV	0.0261**	0.0118
six WCV	0.0106	0.0197
inpatient	−0.0400***	0.0100
any ED care	0.00378	0.00918
Observations	351,850	

Note: Table 8 presents the average marginal effect of an additional prenatal care visit on future care utilization when calculated at the observed covariate values for each observation. All coefficients correspond to the specification with controls for observed maternal risk factors and are restricted to births before 2012. All standard errors are calculated using the delta-method, and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Chapter 2

Prenatal Care and Childhood Asthma

2.1 Introduction

Prenatal health care has long been thought to improve health outcomes. Most research in this area has focused on changes in health status at birth, but those studies that attempt to identify a causal relationship between prenatal care and health status at birth have produced mixed results regarding the efficacy of prenatal care. More recently, literature focused on estimating the long-run impacts of prenatal care on health and health care utilization has developed. These studies have also produced mixed results, with some finding no long term impacts of prenatal care on self-reported health status and others estimating that prenatal care produces positive long term changes in the future utilization of health care.

Miller and Wherry (2014) and Wherry et al. (2015) use variations in Medicaid eligibility and find improvements in adult health and decreased utilization of care in emergency departments (EDs) for those individuals who gained coverage while in-utero. On the other hand, Noonan et al. (2013) find that prenatal care has no impact on self-reported health status, including whether or not a child has ever been diagnosed with asthma, at the age of five. Although it is generally tacitly assumed that the underlying causal mechanism through which prenatal care is likely to act is by improving health status, it has also been posited that prenatal care may influence the trajectory of future decisions related to health independently of any direct impact on health (Reichman et al., 2010).

In this paper, I extend on the work of Noonan et al. and investigate the relationship between prenatal care and utilization of health care for asthma within the first five years of life. While their study relied on self-reported survey data, I employ a unique set of comprehensive administrative Medicaid claims data from the state of South Carolina to identify future health care utilization. I find that prenatal care is associated with an increased probability of receiving health care for an asthma-related diagnosis, but that it has no significant impact on the probability of requiring care in an inpatient setting or utilizing care in an emergency department (collectively referred to as resource intensive care) among the subset of children that are diagnosed with asthma. Using a truncated negative binomial model that allows for unobserved individual heterogeneity, I find that prenatal care increases the frequency of utilization of non-resource intensive care (primary care) for asthma without having any significant impact on the frequency of resource intensive care utilization.

It is important throughout this paper to keep in mind that a child's actual health status is never directly observed. Observed changes in utilization patterns may be driven by changes in the underlying health status of a child, but they may also be driven by changes in how individuals utilize health care resources with given health status. The finding that prenatal care has a different impact on primary care utilization than intensive care utilization lends credence to the hypothesis that observed changes are driven by changes in utilization patterns rather than changes in underlying health status. To further examine this hypothesis, I also test if prenatal care has a differential impact for children of mothers with different levels of educational achievement. I find the positive relationship between prenatal care and primary care utilization is driven by mothers with at least a high school degree and that there may be a weak, negative relationship between prenatal care and the consumption of resource intensive care for children of mothers with less than a high school degree.

2.2 Background and Motivation

"Asthma is a common chronic disorder of the airways that involves a complex interaction of airflow obstruction, bronchial hyperresponsiveness and an underlying inflammation." (National Heart, Lung, and Blood Institute, National Institutes of Health, 2007) Asthma is also one of the most widespread chronic illnesses in the United States, affecting an estimated 7 million children younger than 18 years of age (Akinbami et al., 2012). According to Ash and Brandt (2006), the direct annual health care costs of treating asthma in the United States are nearly $13B. Asthma is the cause of one sixth of all pediatric ED usage and frequently requires treatment in a hospital setting. However, a large portion of asthma related hospitalizations are preventable, suggesting that improved treatment and management of asthma could reduce the costs of care (Flores et al., 2005).

Although asthma widely effects all demographic groups, there is variation in the reported prevalence of asthma across demographic groups. Blacks are significantly more likely, while hispanics are significantly less likely, than whites to have asthma. Prevalence varies even within the hispanic population; individuals of Puerto Rican heritage are more likely, while individuals of Mexican heritage are less likely, than whites to have asthma (Akinbami et al., 2012). Despite these differences in prevalence across races, both black and hispanic children are significantly more likely than white children to require hospitalization for asthma (Ash and Brandt, 2006), and are more likely than white children to be readmitted to a hospital for asthma following an initial discharge (Chabra et al., 1998; Ash and Brandt, 2006).

Conventional measures of asthma prevalence are all constructed using a diagnosis of asthma as a proxy for whether an individual has ever had asthma. This means that any reported differences in prevalence could be partially or wholly attributed to differences in the probability an individual with asthma is properly diagnosed. Holguin et al. (2005) estimate that Mexican-Americans born in the United States are between two and three times more likely to be diagnosed with asthma than Mexican-Americans born in Mexico. They posit that social and environmental factors are responsible for this gap in disease prevalence, but they cannot rule out the hypothesis that other factors —such as immigration status or familiarity with the U.S. health care system —systematically change the likelihood of receiving an asthma diagnosis without any underlying difference in disease prevalence.

Familiarity with the health care system may in fact be an important factor in the management of chronic diseases such as asthma. Harrington et al. (2015) find that a parent's health literacy is associated with a higher degree of asthma knowledge and better child asthma control. They do not, however, find any statistically significant relationship between a parent's health literacy and the care utilized by their child. Harrington et al. also provides a review of the literature linking health literacy to asthma management.

In the context of prenatal care, the majority of work has focused on estimating whether prenatal care impacts birth weight and other measures of health at birth. Evans and Lien (2005) provide an overview of these mixed results, with some studies finding positive impacts of prenatal care on birth weight and others finding insignificant results. While most of this work assumes that prenatal care impacts health through a clinical mechanism, some recent work has investigated the hypothesis that prenatal care may influence a mother's health behaviors and how she interacts with the health care system (Conway and Kutinova, 2006; Reichman et al., 2010). Noonan et al. (2013) do not find any significant impacts of prenatal care on the prevalence of childhood asthma at age 5, but Miller and Wherry (2014) and Wherry et al. (2015) find long run impacts on health and health care utilization from gaining insurance eligibility when a child is in-utero or early in life.

I hypothesize that the mechanism through which prenatal care acts is by providing health specific knowledge to mothers. Providing information about healthy behaviors during pregnancy is an important part of prenatal care (Kirkham et al., 2005), and is likely to help produce what is termed "health literacy" in the medical literature. In this paper, I follow the theoretical health production model outlined by Rosenzweig and Schultz (1982) where education is a complement to health by providing information about the relationship between health inputs and health outputs.

2.3 Model

2.3.1 Theoretical Framework

The theoretical model presented here follows closely from the family health production function modeled by Rosenzweig and Schultz (1982). Assuming that household utility is a function of market goods X that can be directly purchased and the health H of the household, utility is given by

$$U = U(X, H), \tag{2.1}$$

with $U_X > 0$, $U_H > 0$, $U_{XX} < 0$, and $U_{HH} < 0$. Health cannot be directly purchased; it must be produced with inputs Z_1 and Z_2 that may be purchased directly. Assume that health production is

$$H = G\left(Z_1, Z_2, \eta | K\left(Z_{PNC}, \kappa\right)\right), \tag{2.2}$$

where $G(\bullet)$ is a function of two types of health inputs, Z_1 and Z_2, which can be thought of as primary care and resource intensive care respectively. I assume $G_{Z_1} > G_{Z_2} > 0$, or that primary care is a more efficient producer of health than resource intensive care. This function is conditional upon the household's health knowledge K, which is developed through both formal education κ and prenatal care, Z_{PNC}, along with household specific characteristics η, at least some of which may be unobserved.[1]

In their original formulation, Rosenzweig and Schultz argue that "education, by augmenting information, may be thought to affect parental *perceptions* of the relationships between inputs and outputs." Following their lead, I assume that health knowledge does not directly enter into the health production function, but instead reveals information about the true nature of the production function, and the magnitude of the differential in productivity between the two types of care. In the absence of health knowledge, it is possible that parents underestimate the productivity of primary care or overestimate the productivity of resource intensive care. In the ex-

[1]In their original model, Rosenzweig and Schultz allow for a subset of market goods to directly impact both utility and health production, but for the ease of exposition I assume health is independent of consumption goods.

treme, they may even perceive primary care and resource intensive care to be perfect substitutes in the production of health, or $\widehat{G_{Z_1}} = \widehat{G_{Z_2}}$ where $\widehat{G}$ denotes the perceived marginal product.

I do not place any formal restrictions on the sign of $K_{Z\kappa}$, allowing for the possibility that education and prenatal care could be complements or substitutes in the production of health knowledge. Differences in the underlying health of a child that influence health and health care utilization will be captured by η. In this framework, I assume that prenatal care does not directly impact the prevalence of asthma in children. Relaxing this assumption would allow for the possibility that η were a function of prenatal care and that at least some of the observed impacts of prenatal care may be acting through the channel of improving the health of children.

In this simple framework, individuals maximize utility given in equation (1) subject to a resource constraint[2]

$$I = P_X X_t + P_{Z_1} Z_1 + P_{Z_2} Z_2, \tag{2.3}$$

which implies a first order condition of

$$\frac{U_X}{P_X} = \frac{U_H \widehat{G_{Z_1}}}{P_{Z_1}} = \frac{U_H \widehat{G_{Z_2}}}{P_{Z_2}}. \tag{2.4}$$

Rearranging equation (4) implies that parents will choose which types of care to consume to produce health by setting

$$\frac{\widehat{G_{Z_1}}}{\widehat{G_{Z_2}}} = \frac{P_{Z_1}}{P_{Z_2}}. \tag{2.5}$$

[2]Because a majority of the cost of obtaining medical care for individuals with Medicaid is non-monetary (i.e. time costs), the relative prices of care may differ across individuals.

In other words, they will consume care so that the perceived ratio of the marginal product of primary and resource intensive care is equal to the price ratio between the two goods. It is important, however, to keep in mind that utility maximization is subject to the *perceived* productivity of both primary and resource intensive care. If individuals either underestimate the productivity of primary care or overestimate the productivity of resource intensive care, then $0 < \frac{G_{Z_1}}{G_{Z_2}} - \frac{\widehat{G_{Z_1}}}{\widehat{G_{Z_2}}} \equiv \Delta$, where Δ is a measure of the gap between perception and reality.

The primary aim of this study is to estimate how prenatal care impacts the management of and health care utilization for asthma. All else equal, a mother with more prenatal care visits will have higher health knowledge. An increase in health knowledge will decrease Δ, and subsequently shift healthcare utilization towards primary care and away from resource intensive care. Moreover, if the observed impacts of prenatal care are larger in more educated populations, it suggests that $K_{Z\kappa} > 0$ and that prenatal care and formal education are complements in the production of health knowledge. If, however, the observed impact of prenatal care is larger in less educated populations, then $K_{Z\kappa} < 0$ and theory implies that Z and κ are substitutes in the production of health knowledge.

2.3.2 Empirical Framework

The observed consumption of health care can be viewed as arising from multiple tiered decisions. First, individuals choose whether or not to seek treatment for a given condition, a decision which is dependent on both health status as well as prior consumption of health care.[3] If individuals seek treatment for a condition, they are

[3]Although it seems likely that claims data will have few false positives where individuals who do not have asthma are diagnosed with asthma, it is likely that there are some false negatives where individuals have an underlying condition of asthma but choose not to treat it. Because of this factor, there is not a one to one relationship between the decision to seek treatment and the prevalence of

then faced with a second dichotomous choice: to consume resource-intensive care or to consume only primary care. Only after making the decision to seek treatment, and choosing the types of care to consume, do individuals choose *how* much of each type of care to seek.

In the case of asthma, it is possible that the determinants of whether to seek care are different from the determinants of how much care to seek. To that end, I model each step of the decision making process independently. As the first two decisions are binary, both can be viewed through a latent variable framework, where

$$Y_i^* = \gamma Z_i + H_i\beta + \epsilon_i, \tag{2.6}$$

and Y_i^* is an individual's propensity to seek care beyond a certain threshold.

In this case, the propensity to seek care is influenced by both prior consumption of health care in the form of prenatal care, and a vector H_i of other observed factors that partially account for an individuals health status as well as a stochastic error term ϵ_i. Of course, Y^* is never observed. Instead, only

$$Y_i = \begin{cases} 0 & \text{if } Y_i^* \leq 0; \\ 1 & \text{if } Y_i^* > 0 \end{cases} \tag{2.7}$$

is observed. Assuming the error term in (12) is normally distributed, the probability that $Y_i = 0$ is $1 - \Phi(\gamma Z_i + H_i\beta)$ and the probability that $Y_i = 1$ is $\Phi(\gamma Z_i + H_i\beta)$, where $\Phi(\bullet)$ is the standard normal distribution function. The impact of prenatal care

disease.

on the probability of seeking care for asthma can be estimated by maximizing

$$LF = \Pi[1 - \Phi(\gamma Z_i + H_i\beta)]^{(1-Y_i)}\Phi(\gamma Z_i + H_i\beta)^{Y_i}. \tag{2.8}$$

The framework for estimating the impact of prenatal care on the probability of utilizing resource intensive care for asthma is identical to that discussed above except it is conditional upon an initial decision to seek care. To estimate how prenatal care impacts this decision, equation (8) is estimated with Y_i as an indicator for the presence of any resource intensive care and the sample restricted to individuals who seek treatment for asthma.

As discussed previously, once individuals have decided whether to seek care and which types of care to utilize they must then choose how much care to consume. In this case the count, T_i, of health care encounters an individual has is likely to be a function of both health status and prior health care consumed. In the context of health care, however, it is natural to think that there may be a number of unobservable factors such as individual preferences for health that influence health care utilization across time periods. In this case, employing a model that allows for unobserved individual heterogeneity will allow for consistent estimates of how prior health care utilization influences future decisions to be obtained.

Following the framework outlined in Cameron and Trivedi (1998), I employ a negative binomial model to allow for gamma-distributed unobserved individual heterogeneity. Then the density of visits is given by

$$f(T|\mu,\alpha) = \frac{\Gamma(T+\alpha^{-1})}{\Gamma(T+1)\Gamma(\alpha^{-1})}\left(\frac{\alpha^{-1}}{\alpha^{-1}+\mu}\right)^{\alpha^{-1}}\left(\frac{\mu}{\alpha^{-1}+\mu}\right)^{T}, \tag{2.9}$$

with mean μ and dispersion parameter α. Estimates are obtained by parameterizing

$\mu = \exp(\delta Z_i + H_i \Theta)$ and maximizing the log-likelihood function

$$LLF(\delta, \Theta, \alpha) = \sum_{i=1}^{n} \Bigg(\Bigg(\sum_{j=0}^{T_i - 1} \ln(j + \alpha^{-1}) \Bigg)$$
$$- \ln T_i! - (T_i + \alpha^{-1}) \ln(1 + \alpha \exp(\delta Z_i + H_i \Theta)) + T_i \ln \alpha + T_i(\delta Z_i + H_i \Theta) \Bigg). \quad (2.10)$$

In a number of the applications discussed in this paper, measuring the count of visits is only of interest for those individuals who decide to seek care. In these cases, the above model is further extended by utilizing a zero-truncated negative binomial model and restricting the sample to individuals who utilize care at least once. In this case, the density is given by

$$h(T|\mu, \alpha, T > 0) = \frac{f(T|\mu, \alpha)}{1 - F(0|\mu, \alpha)}, \quad (2.11)$$

and estimates are obtained in by maximum likelihood estimation an identical fashion to that described above.

2.4 Data Used for Estimation

Data from two sources, South Carolina birth certificates and South Carolina Medicaid claims, are used for estimating the models in section 3.[4] The birth certificate data contain all observed births in the state of South Carolina between 2001 and 2012 where Medicaid was identified as the source of payment on the birth certificate, providing a total of 426,319 births. The birth certificate records reflect the total number of prenatal care visits made by a mother during pregnancy and her demographic

[4]Reimbursements for South Carolina's Medicaid program and the corresponding claims data are processed and housed by Clemson University's department of Computing and Information Technology (CCIT).

information such as age, race, and tobacco use during pregnancy. Additionally, the birth certificate records contain information about the infant at birth such as gender, birth weight, and gestational age.

Medical billing claims submitted for reimbursement under South Carolina's Medicaid program provide the second source of data. These administrative data contain codes for International Statistical Classification of Diseases and Health Related Problems (ICD-9), as well as the patient's age at the time of service, and indicators for whether or not the claim originated in the Emergency Department or resulted in hospital admission. I observe 247,971 claims between 2001 and 2012 for children within the first five years of life with an ICD-9 diagnosis code relating to asthma, the seventh most frequently occuring diagnosis code in my data. The only diseases I observe more frequently in children within the first five years of life are common childhood maladies (such as fussy infant, cough, nausea, and ear infections) as well as bronchitis and pneumonia. Bronchitis and pneumonia, like asthma, are respiratory diseases, though they are typically acute illnesses while asthma is a chronic disease.

The birth certificate and Medicaid billing data contain a unique person identifier allowing them to be matched by infant. I observe 389,391 unique births for children that make subsequent Medicaid claims. I further restrict the sample to 217,496 births between 2001 and 2007 to allow for five full years of observation and exclude observations with missing birth certificate data leaving a sample of 212,097 observations for estimation. Table 1 contains summary statistics for the final sample. Mothers have an average of 11.3 prenatal care visits and an average age of 24 years. 34.7 percent of mothers have not completed their high school degree at the time they give birth, while an additional 36.7 percent have completed high school but received no post-secondary education. 20 percent of the children in my sample are observed to have at least one claim with an asthma diagnosis during their first five years of life and, conditional upon a diagnosis, have an average of 4.1 asthma related claims. Resource intensive care makes up approximately 13 percent of all asthma related claims, and 28 percent of children diagnosed with asthma utilize resource intensive care at least once within their first five years of life.

One potential shortcoming of the data is that they do not reflect any deaths, migration, or transitions out of Medicaid eligibility. This could be problematic as any of these factors may lead to measurement error in the observed counts of future health care utilization, and could lead to some bias in the predicted impact of prenatal care on outcome variables of interest that measure future care. An additional complicating factor is that it is plausible that prenatal care could either be positively or negatively related to these factors if, for instance, prenatal care were associated with a reduction in infant mortality and an increase in a family's probability of transitioning out of medicaid eligibility. Despite the widespread prevalence of asthma, however, the annual mortality rate from childhood asthma is approximately only 3 per 100,000 children with asthma suggesting this may not be a large concern (Akinbami et al., 2012). I assume that any missing data are missing at random, as relaxing this assumption would complicate the analysis beyond the scope of this study.

2.5 Estimation Results

The results can be loosely divided into two main categories: those that impact the incidence of asthma and those that impact the utilization of care conditional upon a diganosis. Table 2 contains estimation results from a probit model with an indicator variable for whether or not a child is ever observed to have an asthma related diagnosis as the dependent variable. The estimates in column 1 are obtained from the full sample, while the estimates in columns 2 through 4 are stratified by the mother's education level; i.e., estimates for children of mothers with less than a high school degree, those with a high school degree with no post-secondary education, and those with at least some post-secondary education are contained in columns 2 through 4, respectively. This convention is maintained in all subsequent tables.

In each column of Table 2 the estimated impact of an additional prenatal care visit on the probability of a child being diagnosed with asthma is positive and strongly statistically significant. While this result may seem to suggest that prenatal care actually increases the incidence of asthma, it is also possible that prenatal care merely increases the likelihood that a child with asthma will be diagnosed. Although the results are all statistically different from zero, the average estimated marginal impact of an additional prenatal care visit is small, with one additional visit estimated to increase the probability of diagnosis by between 1.4 and 2.0 percent. The estimated coefficients of the other included covariates are generally similar in sign and significance across education levels and are of the expected sign as children born at a younger gestational age and males, as well as those born to mothers who smoke or are black are all more likely to be diagnosed with asthma. while children of hispanic mothers are significantly less likely than children of white mothers to be diagnosed with asthma.

As a relatively small proportion of children diagnosed with asthma utilize any resource intensive care related to asthma, I allow for the possibility that prenatal care and other observed factors may influence the decision of whether or not to utilize resource intensive care independently of how they influence the frequency with which resource intensive care is utilized.

Table 3 contains the estimation results from estimating a probit model with an indicator variable for whether or not a child is ever observed to utilize resource intensive care with an asthma diagnosis. In all four columns, the sample has been restricted to only include children who are diagnosed with asthma at some point, and columns 2 through 4 are further restricted by the mother's education level. The estimated coefficient on prenatal care visits in column 1 suggests that prenatal care is unrelated to the decision to seek resource intensive care for asthma. The estimated coefficient in column 3 is actually positive and statistically significant at the 10 percent level. This suggests that prenatal care may increase the probability of utilizing resource intensive care for children of mothers with a high school degree and no post-secondary education. However, the result is economically small as the estimated marginal impact of an additional prenatal care visit only increases the probability of using resource intensive care by 0.7 percent. This, coupled with the fact that the point estimated signs for prenatal care visits in both columns 2 and 4 are negative and that prenatal care has no statistically discernable impact on the number of times resource intensive care is utilized within this subsample is suggestive that this finding is simply due to chance.

In Table 3, as in Table 2, the estimated coefficients of the other included covariates are generally consistent across different levels of maternal education and of the expected signs. In all four columns, male children, children with mothers who smoke or are black, and children born at a younger gestational age are all significantly more likely to utilize resource intensive care for asthma. The estimated coefficient for a hispanic mother in column 1 shows that children of hispanic mothers are more likely to utilize resource intensive care, but the estimated coefficients in columns 2 through 4 show that this effect is driven entirely by children of hispanic mothers with post-secondary education. This result is perhaps somewhat unexpected given that it has been previously documented that both black and hispanic children are more likely to utilize resource intensive care than white children despite variation in disease prevalence across races.

Table 4 shows results from estimating a truncated negative binomial model with the observed count of all asthma related claims for a child within the first five years of life as the dependent variable. Because 80 percent of all children are not observed to have any asthma related diagnoses, this model is only estimated for those children who have at least one asthma related claim and the counts are therefore truncated at zero. The estimated coefficient for prenatal care visits is positive across all education levels, and is strongly significant except for when the sample is restricted to the children of mothers with less than a high school degree in column 2. These results suggest that prenatal care either increases the severity of asthma or increases health care utilization of asthma, and that the effect increases at higher levels of education. As expected and across all education levels males, blacks, and children born at a younger gestational age are likely to have a higher number of asthma related claims, but there is no relationship between the frequency of asthma claims and a mother's tobacco usage during pregnancy.

The approaches discussed to this point cannot identify whether an increase in claims frequency is reflecting poorer health status or merely changes in utilization patterns. Splitting asthma related claims into resource intensive claims that are made in an emergency department or inpatient setting and all other claims (primary care claims) may shed some light on the sources of the increase in health care utilization. Primary care claims likely reflect how well a chronic disease is managed, while resource intensive claims are more likely to reflect the severity of the disease (which may be due to poor management of a chronic condition). Table 5 contains results from estimating a truncated negative binomial model with the observed counts of asthma related primary care claims for children within the first five years of life as the dependent variable, conditional upon having at least one primary care claim. The estimated coefficients for prenatal care contained in Table 5 look remarkably similar to those contained in Table 4. Notably, in columns 3 and 4 an additional prenatal care visit increases the frequency of future primary care doctor visits related to asthma for children of mothers with a high school degree or post-secondary education, while the point-estimated impact of prenatal care for children whose mothers have not graduated from high school is not statistically different from zero. Although these results are precisely estimated, the estimated marginal impact is small as an additional prenatal care visit is only estimated to increase the number of future primary care visits by 0.1 percent. Again, across all education levels males, blacks, and children born at a younger gestational age are likely to have a higher number of asthma related primary care claims.

Table 6 contains estimates from a negative binomial model with the total number of resource intensive asthma claims as the dependent variable. In all columns, the sample is restricted to include only children who are diagnosed with asthma, but not all children who are diagnosed with asthma utilize resource intensive care. The estimated impact of an additional prenatal care visit on the frequency of resource intensive care utilization is not statistically different from zero, and this result holds across all education levels. The estimated coefficients on other included covariates are again of the expected sign and largely similar across educational levels, as children born at younger gestational age, males, and children of black mothers are all significantly more likely to have a higher observed count of resource intensive asthma claims. Taken together, these results suggest that prenatal care may be unrelated to the severity of asthma, and provide some additional evidence that the positive association between prenatal care and utilization of primary care documented in Table 5 is reflecting improved disease management.

It seems plausible that including children who utilize no resource intensive care and employing a negative binomial model may be a misspecification. Although prenatal care has no statistically discernable impact on the decision of whether to seek resource intensive care for asthma, it is possible that it may be related with how frequently intensive care is employed by those who utilize this type of care. If this is the case, estimating a truncated negative binomial model may be more appropriate. Table 7 contains results from estimation of a truncated negative binomial model with the count of resource intensive asthma claims as the dependent variable and the sample restricted to individuals who make at least one resource intensive claim. The estimated impact of an additional prenatal care visit is not statistically different from zero in all four columns. In column 2 the point estimated impact of an additional prenatal care visit for children of mothers with less than a high school degree is negative and falls just short of statistical significance at the 10 percent level, with a p-value of 0.117, however the point estimated marginal impact of an additional visit is not economically different from zero even within this subgroup.

2.6 Conclusions

Despite strong emphasis of the Medicaid program on ensuring "adequate" prenatal care and recommendations that encourage mothers to seek frequent prenatal care, surprisingly little is known about the efficacy or long term impacts of prenatal care. Prior studies that have estimated the causal impact of care on health outcomes at birth have produced mixed results, and few studies have estimated the longer term impacts of care.

Using Medicaid claims data, I document a positive relationship between prenatal care and the probability of receiving treatment for asthma within the first five years of life. This increased probability is also associated with an increased frequency of primary care visits but no change in the probability nor frequency of resource intensive care for asthma related diagnoses. These seemingly contradictory results are in fact suggestive that prenatal care may impact the management of asthma, rather than the underlying presence or severity of the disease.

Additionally, it appears as though prenatal care has a differential impact on the frequency of future care utilization across education levels. Notably, the positive relationship between prenatal care and the future frequency of primary care utilization is entirely concentrated amongst children of mothers with at least a high school degree, while there may be a weak, albeit statistically insignificant, negative relationship between prenatal care and the future frequency of resource intensive care utilization for children of mothers with less than a high school degree. These results suggest that prior health care utilization and education may in fact be complements in the production of health knowledge when it relates to certain types of care, such as preventive care, and substitutes in the production of health knowledge as it relates to other types of care, such as that delivered in an emergency department. Although many of the results are statistically significant, economically the estimated marginal impact of increasing prenatal care is small. More work is needed to fully identify the informational mechanisms at play and to better understand the channels through which health care utilization impacts the future trajectory and management of chronic health conditions.

Table 2.1: Summary statistics

Variable	**Mean**	**(Std. Dev.)**
PNC visits	11.328	(4.461)
gestational age	38.332	(2.215)
male	0.506	(0.5)
mother's age	23.976	(5.434)
no HS degree	0.347	(0.476)
HS degree only	0.367	(0.482)
mother black	0.461	(0.499)
mother hispanic	0.111	(0.314)
mother's tobacco use	0.175	(0.38)
rural county	0.413	(0.492)
pediatricians	0.059	(0.03)
any asthma	0.2	(0.4)
any intensive asthma	0.056	(0.229)
count asthma visits	0.820	(2.927)
count pc asthma visits	0.716	(2.561)
count intensive asthma visits	0.104	(0.6)
observations	212,097	

Table 2.2: Impact on probability of any asthma diagnosis

VARIABLES	(1) Full Sample	(2) No HS degree	(3) HS degree only	(4) Some post-HS
PNC visits	0.0124***	0.0151***	0.0102***	0.0116***
	(0.000995)	(0.00152)	(0.00133)	(0.00158)
gestational age	-0.0203***	-0.0205***	-0.0176***	-0.0234***
	(0.00162)	(0.00246)	(0.00237)	(0.00280)
male	0.213***	0.219***	0.203***	0.222***
	(0.00616)	(0.0107)	(0.00965)	(0.0121)
mother's age	-0.00576***	0.00160	-0.00376***	-0.00931***
	(0.000657)	(0.00111)	(0.000951)	(0.00131)
rural county	0.0966***	0.0764***	0.0952***	0.109***
	(0.0248)	(0.0253)	(0.0270)	(0.0294)
mother hispanic	-0.0991***	-0.233***	-0.0922***	0.0413
	(0.0163)	(0.0210)	(0.0270)	(0.0311)
mother black	0.237***	0.139***	0.225***	0.352***
	(0.0102)	(0.0158)	(0.0130)	(0.0144)
mother's tobacco use	0.120***	0.0626***	0.0796***	0.182***
	(0.00835)	(0.0137)	(0.0139)	(0.0193)
pediatricians	0.550*	0.549*	0.199	1.021***
	(0.307)	(0.326)	(0.360)	(0.368)
Constant	-0.386***	-0.465***	-0.453***	-0.333***
	(0.0640)	(0.0990)	(0.0931)	(0.116)
Observations	212,097	73,514	77,818	60,765

Note: Table 2 contains the results of a probit model with an indicator for any diagnosis of asthma as the dependent variable. Columns 2 through 4 are separated based on the educational attainment of the mother at birth. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 2.3: Impact on probability of any resource intensive asthma treatment

VARIABLES	(1) Full Sample	(2) No HS degree	(3) HS degree only	(4) Some post-HS
PNC visits	-6.78e-05	-0.000794	0.00471*	-0.00274
	(0.00161)	(0.00266)	(0.00278)	(0.00272)
gestational age	-0.0223***	-0.0269***	-0.0222***	-0.0173***
	(0.00339)	(0.00540)	(0.00498)	(0.00601)
male	0.159***	0.148***	0.164***	0.171***
	(0.0124)	(0.0206)	(0.0199)	(0.0265)
mother's age	-0.00996***	-0.00203	-0.00764***	-0.00681**
	(0.00132)	(0.00236)	(0.00204)	(0.00287)
rural county	-0.0894**	-0.152***	-0.0568	-0.0703
	(0.0381)	(0.0463)	(0.0439)	(0.0435)
mother hispanic	0.0792**	-0.0311	0.0875	0.220***
	(0.0341)	(0.0425)	(0.0693)	(0.0768)
mother black	0.286***	0.345***	0.271***	0.260***
	(0.0215)	(0.0278)	(0.0304)	(0.0345)
mother's tobacco use	0.111***	0.0720***	0.0795***	0.0853**
	(0.0184)	(0.0267)	(0.0296)	(0.0424)
pediatricians	-0.680	-0.760	-0.401	-0.916*
	(0.448)	(0.594)	(0.605)	(0.544)
Constant	0.239*	0.327	0.101	-0.0357
	(0.141)	(0.216)	(0.202)	(0.247)
Observations	42,519	14,827	16,335	11,357

Note: Table 3 contains the results of a probit model with an indicator for any resource intensive treatment of asthma as the dependent variable. All columns are restricted to only include children who receive a diagnosis of asthma within the first 5 years of life. Columns 2 through 4 are separated based on the educational attainment of the mother at birth. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 2.4: Impact on count of asthma treatments

VARIABLES	(1) Full Sample	(2) No HS degree	(3) HS degree only	(4) Some post-HS
PNC visits	0.00825***	0.00458	0.0116***	0.0108**
	(0.00221)	(0.00423)	(0.00364)	(0.00446)
gestational age	-0.0229***	-0.0226***	-0.0225***	-0.0259***
	(0.00474)	(0.00850)	(0.00706)	(0.00833)
male	0.237***	0.240***	0.218***	0.262***
	(0.0195)	(0.0323)	(0.0327)	(0.0402)
mother's age	0.00210	0.00774**	0.00113	-0.000185
	(0.00199)	(0.00342)	(0.00307)	(0.00386)
mother black	0.214***	0.237***	0.189***	0.222***
	(0.0268)	(0.0369)	(0.0367)	(0.0488)
mother hispanic	0.112	0.0812	-0.0773	0.334**
	(0.0743)	(0.0849)	(0.0884)	(0.134)
mother's tobacco use	0.0180	0.00436	0.0108	-0.000692
	(0.0260)	(0.0402)	(0.0392)	(0.0596)
rural county	-0.0501	-0.0670	-0.00350	-0.0964*
	(0.0376)	(0.0476)	(0.0459)	(0.0564)
pediatricians	0.579	-0.540	0.417	1.946***
	(0.489)	(0.743)	(0.636)	(0.744)
Constant	0.269	0.399	0.260	0.151
	(0.230)	(0.393)	(0.315)	(0.390)
Observations	42,522	14,828	16,337	11,357

Note: Table 4 contains the results of a truncated negative binomial model with the total count of asthma treatments as the dependent variable. All columns are restricted to only include children who receive a diagnosis of asthma within the first 5 years of life. Columns 2 through 4 are separated based on the educational attainment of the mother at birth. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 2.5: Impact on count of primary care asthma treatments

VARIABLES	(1) Full Sample	(2) No HS degree	(3) HS degree only	(4) Some post-HS
PNC visits	0.00974***	0.00576	0.0125***	0.0123***
	(0.00240)	(0.00443)	(0.00390)	(0.00462)
gestational age	-0.0222***	-0.0223**	-0.0223***	-0.0237***
	(0.00514)	(0.00938)	(0.00738)	(0.00883)
male	0.228***	0.245***	0.205***	0.239***
	(0.0209)	(0.0344)	(0.0350)	(0.0420)
mother's age	0.00541***	0.0112***	0.00352	0.000326
	(0.00208)	(0.00370)	(0.00324)	(0.00389)
mother black	0.151***	0.152***	0.125***	0.179***
	(0.0294)	(0.0412)	(0.0394)	(0.0500)
mother hispanic	0.0683	0.0621	-0.137	0.258**
	(0.0802)	(0.0910)	(0.0935)	(0.125)
mother's tobacco use	-0.00337	-0.0133	-0.000960	-0.0145
	(0.0269)	(0.0432)	(0.0401)	(0.0612)
rural county	-0.00594	0.0104	0.0319	-0.0731
	(0.0415)	(0.0520)	(0.0500)	(0.0603)
pediatricians	0.768	-0.531	0.606	2.203***
	(0.544)	(0.791)	(0.685)	(0.818)
Constant	-0.721*	-0.701	-0.646	-0.636
	(0.388)	(0.604)	(0.488)	(0.554)
Observations	41,596	14,466	15,997	11,133

Note: Table 5 contains the results of a truncated negative binomial model with the total count of primary care asthma treatments as the dependent variable. All columns are restricted to only include children who receive primary care treatment within the first 5 years of life. Columns 2 through 4 are separated based on the educational attainment of the mother at birth. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 2.6: Impact on count of resource intensive care asthma treatments

VARIABLES	(1) Full Sample	(2) No HS degree	(3) HS degree only	(4) Some post-HS
PNC visits	-0.00164	-0.00596	0.00575	-0.000380
	(0.00261)	(0.00503)	(0.00431)	(0.00492)
gestational age	-0.0259***	-0.0208***	-0.0297***	-0.0302***
	(0.00485)	(0.00800)	(0.00866)	(0.00975)
male	0.251***	0.192***	0.253***	0.342***
	(0.0246)	(0.0370)	(0.0391)	(0.0442)
mother's age	-0.0141***	-0.00694**	-0.0106***	-0.00590
	(0.00244)	(0.00351)	(0.00366)	(0.00547)
mother black	0.480***	0.570***	0.460***	0.414***
	(0.0391)	(0.0511)	(0.0499)	(0.0679)
mother hispanic	0.242***	0.102	0.143	0.561***
	(0.0691)	(0.0813)	(0.114)	(0.191)
mother's tobacco use	0.114***	0.0849*	0.0818	0.0105
	(0.0335)	(0.0455)	(0.0539)	(0.0696)
rural county	-0.268***	-0.366***	-0.209***	-0.235***
	(0.0622)	(0.0738)	(0.0712)	(0.0769)
pediatricians	-0.366	-0.588	-0.200	-0.158
	(0.667)	(0.992)	(0.835)	(0.958)
Constant	0.290	0.141	0.208	0.125
	(0.197)	(0.329)	(0.329)	(0.405)
Observations	42,522	14,828	16,337	11,357

Note: Table 6 contains the results of a negative binomial model with the total count of resource intensive care asthma treatments as the dependent variable. All columns are restricted to only include children who receive an asthma diagnosis within the first 5 years of life. Columns 2 through 4 are separated based on the educational attainment of the mother at birth. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

Table 2.7: Conditional impact on count of resource intensive asthma treatments

VARIABLES	(1) Full Sample	(2) No HS degree	(3) HS degree only	(4) Some post-HS
PNC visits	-0.00368	-0.0131	0.00103	0.00963
	(0.00477)	(0.00876)	(0.00682)	(0.0105)
gestational age	0.000605	0.0187	-0.00873	-0.0204
	(0.00862)	(0.0138)	(0.0165)	(0.0185)
male	0.138***	0.0425	0.133*	0.315***
	(0.0501)	(0.0710)	(0.0780)	(0.0950)
mother's age	-0.00707	-0.0114*	-0.00372	0.00573
	(0.00496)	(0.00643)	(0.00731)	(0.0110)
mother black	0.362***	0.462***	0.333***	0.279**
	(0.0549)	(0.0805)	(0.0856)	(0.121)
mother hispanic	0.384***	0.356**	0.143	0.741**
	(0.116)	(0.139)	(0.222)	(0.320)
mother's tobacco use	-0.0479	0.00731	-0.0476	-0.299**
	(0.0585)	(0.0845)	(0.109)	(0.134)
rural county	-0.431***	-0.502***	-0.392***	-0.395***
	(0.0731)	(0.0988)	(0.0949)	(0.112)
pediatricians	1.494	0.576	1.153	3.608**
	(0.912)	(1.499)	(1.231)	(1.500)
Constant	-21.03***	-19.76***	-19.47***	-17.24***
	(2.158)	(0.579)	(0.6474)	(0.881)
Observations	12,141	4,590	4,667	2,884

Note: Table 7 contains the results of a truncated negative binomial model with the total count of resource intensive care asthma treatments as the dependent variable. All columns are restricted to only include children who utilize some resource intensive care for asthma within the first 5 years of life. Columns 2 through 4 are separated based on the educational attainment of the mother at birth. All standard errors are clustered at the county-year level and statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and *, respectively.

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