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# DISENTANGLING SOURCES OF VARIATION IN C-SECTION RATES

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**ABSTRACT**

Cesarean section rates vary widely across U.S. counties, yet it remains unclear how much of this variation reflects demand-side factors (such as patient risk or preferences) versus supply-side factors (such as physician practices or hospital incentives). We develop a new empirical strategy to isolate the influence of supply-side forces. Exploiting hospital obstetric unit closures from 1989–2019 that reallocate some mothers to counties with different C-section rates, we find that a one–percentage-point increase in the delivery county's rate raises a mother's likelihood of a C-section by roughly one point. The results point to a dominant role for provider behavior and local practice norms in driving geographic variation in C-section use, the most common major surgery in the United States.

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# 1 Introduction

The share of births delivered surgically, known as the Cesarean section (C-section) rate, varies dramatically across U.S. counties, revealing striking geographic disparities in childbirth practices (Figure 1). In 2019, counties at the 95th percentile had C-section rates of 40.6 percent, more than 23 percentage points higher than those at the 5th percentile, where rates were 17.5 percent. These pronounced disparities have remained remarkably stable over the last three decades, even as the national C-section rate has risen (Robinson et al., 2024). Despite the more intensive use of surgical delivery in high-rate areas, neonatal mortality is no lower than in counties with much lower C-section rates (Baicker et al., 2006; Robinson et al., 2024).[1]

The notable geographic variation in C-section rates coupled with its limited association with improved health outcomes has raised concerns about potential overuse of C-section. To design effective policy, researchers and policymakers must understand the sources of this geographic variation: Do differences in C-section rates primarily reflect supply-side factors (such as provider practices or hospital incentives) or demand-side factors (such as patient risk profiles and preferences)? If demand-side considerations dominate, interventions aimed at patients may be effective in reducing unnecessary C-sections. For example, providing information that shifts beliefs or preferences could help patients make more informed choices about delivery methods. Conversely, if supply-side factors play the central role, provider-focused incentives may be more effective. Notably, both types of interventions are embedded in recent policy initiatives, such as the California Maternal Quality Care Collaborative, a program highlighted by the Centers for Disease Control as a "success story" (Centers for Disease Control and Prevention, 2015).[2]

The objective of this paper is to identify the causal effect of the place of care, which we term —what we term the "provider effect" —on the likelihood that a mother delivers by C-section. In particular, we estimate the impact of a birth county's C-section rate on a mother's probability of undergoing a surgical delivery. Distinct from much of the existing literature (Chyn and Shenhav, forthcoming; Ding, forthcoming; Finkelstein et al., 2016), which relies on mover designs that combine place of care with place of residence to capture a total place effect, our research design isolates the care setting itself. We ex-

[1]This weak relationship between C-section rates and health outcomes, such as maternal and infant mortality, also holds across World Health Organization countries (Molina et al., 2015).

[2]Rosenstein et al. (2021) study the effect of this California program on C-section rates, finding a 3.2% reduction in the probability of a C-section birth among lower risk first birth mothers. Miller et al. (2025) leverages the decline in C-sections to study the relation with mothers' postpartum labor market outcomes.

ploit the widespread and frequent closures of maternity wards across the United States, which shift where mothers deliver without altering where they live, thereby providing sharp variation in the place of care.

Distinguishing supply-side influences from demand-side influences is difficult as mothers living in different areas face different underlying maternal risk factors for C-sections. For example, maternal obesity, a key risk factor, is more prevalent in the Southern states (Obisesan et al., 2000). To distinguish the place of care effects, we implement an instrumental variables (IV) approach that leverages county-level closures of hospital-based obstetric (OB) units. Our design focuses on counties that lose all obstetric capacity, moving from $\geq 1$ OB unit to zero. These events shift where mothers give birth, reallocating them to neighboring counties with different baseline C-section rates. We exploit heterogeneity in the "C-section gap" —the pre-closure difference between the home (closure) county's C-section rate and the exposure-weighted rate of its receiving counties —to generate variation in the county of birth C-section rate. This design allows us to control for the main effect of closures while being identified off variation in the effects of closures correlated with the C-section gap.

As a stylized and simplified example, consider a county with a 25 percent C-section rate that, following a closure, reallocates mothers to a county with a 35 percent rate (a C-section gap of +10 percentage points).[3] In parallel, a county with a 30 percent rate, following a closure, shifts births to a county with a 15 percent rate (a C-section gap of -15 percentage points). In the first case, the closure leads mothers to experience a 10 percentage point higher C-section rate and in the second, a 15 percentage point lower rate. We then leverage variation in the size and direction of these C-section gaps in an instrumental variables framework. This variation is meaningful: the median C-section gap, measured in absolute value, is 5.4 percentage points. This gap is equivalent to a 17 percent change relative to the national average C-section rate of 32 percent in 2022.[4]

Our analysis draws on individual-level birth data from the National Center for Health Statistics (NCHS) covering the universe of U.S. births from 1989 to 2019. We identify 326 closures of OB units that resulted in the complete loss of hospital-based obstetric services within a county. Crucially, in the data we observe both the county of maternal residence and the county of birth, allowing us to infer closures by identifying a sharp and sustained drop in the number of births occurring in a county, an approach based on an algorithm developed and validated by Fischer et al. (2024). Between 1989 and 2019,

[3]One simplification in this illustration, not carried into our empirical framework, is that we abstract from maternal choice following a closure by assuming a single receiving county.

[4]Centers for Disease Control and Prevention, 2022.

widespread closures of obstetric units left over half of rural counties without a hospital-based OB unit (Fischer et al., 2024). In earlier work, Fischer et al. (2024) and Battaglia (2025) use a difference-in-differences design to show when a county loses its last OB unit, nearly all deliveries are reallocated to hospitals in neighboring counties. Using information on maternal county of residence, we track outcomes for mothers who resided in counties affected by these closures. We restrict our main sample to first births, as the likelihood of a C-section increases markedly for subsequent births following a first-birth C-section.[5]

Our first stage estimate reveals that a 10-percentage-point gap in C-section rates between the closing and receiving counties yields a 2.5-percentage-point increase in the average C-section rate in the county of delivery. This attenuation reflects the intent-to-treat nature of the design: prior to closure, 28.3% of resident births occur in the soon-to-close county, with higher-risk mothers already delivering elsewhere; thus, compliers are likely those with less complicated deliveries (Fischer et al., 2024).[6]

Our main finding is that the instrumental variables estimate of the effect of a county's C-section rate on the probability of a C-section delivery among first-time, singleton birth mothers is 1.052 (standard error = 0.132). This implies that when a mother is reallocated to give birth in a county with a C-section rate that is 10 percentage points higher, her own likelihood of receiving a C-section increases by 10 percentage points, a one-for-one relationship. Because the variation in our design comes from changes in delivery location, we interpret the result as evidence that geographic differences in C-section rates reflect provider practices more than patient preferences or medical need. Our estimated provider effect exceeds the median response from an expert survey (N = 225) of health economists, obstetricians, and obstetrics researchers, who attributed 60% of county-level variation in C-section rates to providers. We also find that closure-induced shifts toward counties with better-resourced hospitals, as proxied by the share of medical-school–affiliated hospitals and NICU availability, reduce C-section rates.

The causal interpretation of our estimates hinges on the validity of the exclusion restriction: that hospital obstetric unit closures affect C-section use only through changes in the county-level provider environment, not through changes in maternal composition or other confounding factors. We assess this assumption using several tests. First, we present event study figures that track how C-section probabilities evolve over time.

[5]This pattern follows the widespread recommendation restricting vaginal births after a previous C-section (VBACs) (American College of Obstetricians and Gynecologists Committee on Practice Bulletins—Obstetrics, 2017).

[6]Closures induce only a small change in the probability of an out-of-hospital birth. 98.7% of mothers deliver in a hospital in our sample, which falls by 0.14 percentage points as a result of the closures.

To account for the continuous nature of our instrument, involving C-section gaps, we stratify the sample at different points in the C-section gap distribution (i.e., 25th and 75th percentiles) and examine dynamic effects separately. Second, we test for correlations between the instrument and pre-determined maternal characteristics, such as maternal age, as a balance test and find little evidence of such correlations.

The post-closure choice of birth county for mothers residing in closure counties may not be random, complicating the interpretation of our IV estimates. For this reason, we invoke an additional identifying assumption beyond the standard relevance and exclusion requirements: the "fallback" condition. Abaluck et al. (2021) formalize this condition in their analysis of how Medicare Advantage plans affect mortality, exploiting variation in plan choice induced by plan exits. The fallback condition requires that mothers from closure counties with different pre-closure C-section rates do not differentially select into post-closure counties based on unobserved determinants of C-section risk. We provide evidence supporting this assumption using two strategies: (1) applying the fallback test developed by Abaluck et al. (2021), and (2) by showing that the distribution of birth county choices outside the closure county before the closure closely resembles the distribution of birth county choices after the closure.[7]

We assess the robustness of our analysis to several other potential concerns. First, one might be concerned that the C-section gaps are correlated with the distance to the nearest provider. To address this, we control for distance and find that our estimates are largely unchanged. Second, given that our empirical strategy is based on a difference-in-differences framework, we examine the sensitivity to alternative estimators in light of potential biases from heterogeneous treatment effects. Third, we consider alternative parameterizations of our instrumental variables model. One specification interacts the instrument with the pre-closure share of mothers who gave birth in their county of residence. This exploits heterogeneity in exposure: closures should have the largest effect in counties where most mothers previously delivered in their county of residence, and little effect where many were already delivering elsewhere. Another model weights counties by their propensity to experience a closure ensuring better comparability across treated and untreated counties.

Our results speak to the policy debates over the persistent overuse and uneven distribution of Cesarean delivery in the United States. In response to growing concerns, public health efforts have increasingly targeted reductions in C-section rates, particu-

[7]This second test is not feasible in Abaluck et al. (2021), as their treatment occurs at the plan level where all individuals in a plan are treated as the plan ceases to exist. In contrast, our treatment is at the county of residence level; some mothers are unaffected because they would have delivered out of county regardless of the closure.

larly among low-risk, first-time mothers. For example, Healthy People 2030 sets a national goal of lowering the C-section rate for this group by 10 percent, from 26.3 to 23.7 percent. Achieving this target could also yield substantial financial savings: Cesarean deliveries cost, on average, $26,280 compared to $14,768 for vaginal births,[8] implying potential savings of nearly $1 billion.[9] With Medicaid financing over 40 percent of U.S. births,[10] the implications for public spending are significant.

This study underscores the central role of supply-side factors, particularly provider behavior, in driving geographic variation in C-section use. This finding complements earlier work documenting a meaningful role of providers in racial differences in C-section rates (Corredor-Waldron et al., 2024), which leverages hospital delivery capacity constraints. The large influence of providers in our context suggests that policies targeting provider practices may be more effective in reducing unnecessary C-sections than those aimed at patients. For example, while California's Maternal Quality Care Collaborative (CMQCC) includes both provider- and patient-focused strategies, our results highlight especially large potential gains from provider-side efforts.

More broadly, our findings contribute to understanding the consequences of maternity ward closures. Organizations such as the March of Dimes have highlighted the growing prevalence of 'maternity care deserts' and called for targeted responses.[11] During our sample period, over 20% of counties lost their only maternity ward. At the hospital level, between 2010 and 2022, a sub-period of our analysis, the share of short-term acute hospitals without a maternity ward rose from 35.2% to 42.4% (Kozhimannil et al., 2025). These closures heighten concerns about access to maternal care, particularly in rural and underserved areas (Fischer et al., 2024; Andrews and News, 2016), and raise questions about potential adverse health effects. Our results show that closures not only restrict access but also shift C-section rates, an often overlooked consequence, in ways that depend on local practice patterns and the destinations to which mothers are reallocated. Policymakers should therefore weigh not only whether mothers can access care after a closure and the direct health consequences of reduced access, but also how differences in local practice environments influence the type of care mothers ultimately receive, especially the likelihood of a C-section.

We extend the rich literature on place-based differences in health care use and out-

[8] See https://www.healthsystemtracker.org/brief/health-costs-associated-with-pregnancy-childbirth-and-postpartum-care/

[9] These cost figures do not adjust for selection into C-section, which could change the cost advantage of vaginal delivery. However, part of the C-section premium is mechanical: hospital clinical protocols often recommend longer postpartum hospital stays after a C-section birth than after a vaginal birth.

[10] Source: https://www.kff.org/medicaid/state-indicator/births-financed-by-medicaid/

[11] See https://www.marchofdimes.org/maternity-care-deserts-report.

comes, clarifying how provider behavior versus patient composition shapes these gaps.(Wennberg and Gittelsohn, 1973; Baicker et al., 2006; Chandra and Staiger, 2007; Epstein and Nicholson, 2009; Skinner, 2011; Finkelstein et al., 2016; Molitor, 2018; Deryugina and Molitor, 2020; Chyn and Shenhav, forthcoming; Robinson et al., 2024). Our approach diverges from much of this work in two important ways. First, we focus specifically on maternal and infant health, an area less commonly addressed in this literature,[12] and second, we leverage quasi-experimental variation from OB unit closures. Because the timing of OB unit closures is determined at the hospital level, this variation is especially likely to be exogenous to both patients and physicians. In contrast, prior studies have relied on patient or physician relocation (Finkelstein et al., 2016; Molitor, 2018; Chyn and Shenhav, forthcoming; Ding, forthcoming), but the underlying motives for such moves are typically unobserved and may introduce selection concerns. Our findings point to substantial provider-driven effects, with magnitudes notably larger than those reported by Finkelstein et al. (2016). They attribute approximately 40 percent of geographic variation in health care spending to place-based supply factors. Their estimate likely reflects a combination of place of residence effects and the place of occurrence of health care services, the latter of which we refer to as provider effects, the focal parameter of interest in our study.

## 2 Geographic Differences in Health Care Utilization and Health

Geographic variation in health care use and outcomes has long attracted attention from researchers and policymakers alike.[13] Studies typically attribute such variation to two broad sources: demand-side factors related to patients and supply-side factors related to providers or place.

For C-sections specifically, demand-side factors include medical risks, maternal preferences, and patient beliefs. Certain medical risks, such as hypertension and obesity, are known to vary across regions (Kershaw et al., 2010; Ford et al., 2005). In addition, cultural norms and personal preferences shape maternal demand for C-sections. Phrases like “too posh to push” or a desire for a “natural birth” illustrate how some women request a C-section or a vaginal delivery, and providers often honor these preferences. Indeed, Declercq et al. (2014) reports that 22% of women requested a C-section from

[12] An exception is Chyn and Shenhav (forthcoming), who examine migration across zipcodes using California natality records.

[13] See, for example, the reviews and discussions in Skinner (2011); Zuckerman et al. (2010).

their provider.

On the supply side, provider behavior is influenced by a complex interplay of institutional factors, economic incentives, and individual preferences. Physicians and hospitals may be more likely to perform C-sections due to higher reimbursement rates (Gruber and Owings, 1996), the structure of insurance arrangements such as fee-for-service versus managed care (Koroukian et al., 2001), and concerns over medical liability (Dubay et al., 1999). Provider characteristics —such as training (Dranove et al., 2011), surgical skill (Currie and MacLeod, 2017), and the type of birth attendant (e.g., physician versus midwife) (Davis et al., 1994) —can further influence delivery decisions. Beyond these institutional and financial factors, physicians' preferences, including a desire for greater control over their schedules and reduced uncertainty around birth timing, may also play a role. For example, some may schedule C-sections to align deliveries with more convenient or predictable hours or days (Jacobson et al., 2021). Finally, physicians' beliefs about the appropriate type of care (Cutler et al., 2019), learned through experience or shaped by past experiences that form heuristics (Singh, 2021). Epstein and Nicholson (2009), examining within-market physician variation in New York and Florida, conclude that considerable variation in C-section propensities across physicians reflects unobserved provider-specific factors beyond experience, gender, race, or residency training.

Understanding the causal contributions of demand-side and supply-side factors is critical for interpreting geographic disparities. Policymakers often view differences in treatment received by "identical" individuals across locations as prima facie evidence of inefficiency, prompting calls for reform. However, such conclusions can be misguided. Individuals across regions may differ in unobservable ways that affect their health care use and treatment decisions (e.g., rates of obesity in the United States are higher in the South where C-section rates are also higher). Alternatively, different geographic areas may operate via different health care production functions, justifying their use of different levels of medical intervention. Where to direct policy requires better knowledge of the importance of both demand-side and supply-side influences.

Documenting and understanding geographic variation in health care in the United States is challenging. The data demands alone are substantial. To credibly describe nationwide geographic patterns, researchers need comprehensive, population-level data that span all regions. Many standard health care datasets (e.g., hospital claims data) are limited because they only capture care conditional on insurance coverage, which itself influences whether individuals seek hospital care. Even with ideal data, identification poses a major hurdle. People are not randomly assigned to the locations where they live and seek care. Their location choices are shaped by preferences, constraints, and selection processes that often correlate with both health behaviors and provider practices. As a result, simple geographic correlations are difficult to interpret in a causal way. Disentangling supply-side drivers from patient-side characteristics requires quasi-experimental variation such as exogenous shifts in where people live or which providers they see that are plausibly exogenous. However, such variation is rare in practice, limiting the settings in which separate contributions of patient and provider factors can be cleanly identified.

These complications have led many researchers to focus on the Medicare population, which offers nearly universal insurance coverage for older adults and a relatively uniform policy environment across the country (Card et al., 2008). Within this setting, "mover" designs, which track individuals or physicians as they relocate between health care markets, have become a leading strategy for isolating supply effects from demand effects.[14] Within this literature, estimates of place effects on health care inputs (e.g., 54% in Finkelstein et al. (2016) for Medicare spending) tend to be larger than those for health outcomes (e.g., 15% for mortality among Medicare beneficiaries (Finkelstein et al., 2021), and 16% for birth weight (Chyn and Shenhav, forthcoming)).

Obstetric care offers a uniquely powerful setting for studying geographic disparities in health care. First, it satisfies the data requirements for characterizing variation. Detailed natality files provide a near census of U.S. births, exceeding three million each year, irrespective of insurance coverage, minimizing selection concerns when measuring geographic differences. Obstetric care exhibits striking geographic disparities: even hospitals located in close proximity can differ sharply in C-section rates (Epstein and Nicholson, 2009; Kozhimannil et al., 2013). Our study contributes new causal evidence by leveraging a novel quasi-experimental design based on obstetric unit closures.

# 3 Data

Our core data are the natality files from the National Vital Statistics System (NVSS) for 1989-2019. Each record in these data represents a birth. These files are derived from birth certificates, and cover the near universe of births in the United States. We use the restricted-access version of the NVSS files which include information on the county of birth occurrence and the mother's county of residence (National Center for Health

[14] Examples of mover designs in Medicare include Finkelstein et al. (2016); Ding (forthcoming); Molitor (2018).

Statistics, 1989-2019).[15]

The data include information on a wide range of birth outcomes and procedures, including our main outcome: whether the delivery occurred via C-section. The data do not distinguish directly between planned versus emergency C-sections. Often this distinction is inferred by examining whether a trial of labor preceded the C-section and then classifying those births as emergency C-sections (Corredor-Waldron et al., 2024). Because of potential measurement error in indicators for trial of labor in our data, we refrain from separately analyzing planned versus emergency C-sections. Our main analysis centers on first births, as hospitals often require subsequent deliveries after a C-section to also be performed via C-section due to perceived risks associated with vaginal birth after C-section (VBAC).[16] The dataset also contains information on medical risk factors useful for calculating risk-adjusted C-section rates, including maternal age, breech presentation, eclampsia, chronic and pregnancy hypertension, gestational diabetes, and gestational age in weeks. Using these factors to predict C-section delivery in a logistic regression for first births yields a pseudo-$R^2$ of 0.106.[17,18]

As described in more detail in the next section, our identification strategy exploits closures of hospital-based OB units. In our context, a closure refers to the loss of all hospital-based obstetric units in a county. We identify closures using an algorithm developed in Fischer et al. (2024) using the natality data. In this context, a closure occurs when the number of hospital-based births occurring in a county experiences a large drop, and remains near zero thereafter.[19] Alternatively to code closures, one could use American Hospital Association (AHA) hospital data but such geolocated hospital data cover a narrower time period. Our cross-validation with the AHA data reveals our algorithm works well. Our data prevent us from identifying whether the closures are a complete hospital closure or only an OB unit closure. However, in our cross-check with the AHA data, we note that the modal closure is an OB unit closure as opposed to a full hospital closure. This distinction may be important, as hospital closures can nega-

[15]The data do not identify the hospital of birth.

[16]VBAC remains relatively uncommon; 85% of mothers with a prior C-section deliver by C-section again.

[17]For higher-order births, adding prior C-section delivery as a risk factor increases the pseudo-$R^2$ to 0.452.

[18]The revision of the birth certificate that began in 2003 changed the set of medical risk factors that were recorded. There were a wider range of medical risk factors recorded in the data prior to the 2006 data year, however including these adds only a small amount of predictive power, yielding a pseudo-$R^2$ of 0.121 in the pre-2006 sample.

[19]Specifically, we flag a county as experiencing a closure in year y if the number of hospital-based births falls by at least 75% between year y and year y+1, with at least six births in year y and fewer than six in year y+1. Some births may still occur in hospitals without active maternity wards due to emergency deliveries.

tively impact local economies (Alexander and Richards, 2023). In total, we identify 605 counties that experienced a closure throughout the 31-year sample. These closures are widespread over the sample period and geographically dispersed (Figure A1).

For our analysis sample, we adopt the three county-level sample restrictions used by Fischer et al. (2024) to define our closure and control counties. First, to ensure a standard staggered difference-in-differences setup (treatment turns on once, and not off), we exclude 149 counties that experienced an OB unit opening (117 experienced both opening and closing, and 32 experienced an opening without a closure). Second, we exclude 348 "receiving counties" from the control group. This is because non-closure counties serving a sizable size of residents from closure counties prior to the closure as those counties may be also treated. These are counties where at least 30% of mothers from the closure county deliver in that county prior to the closure. Third, we exclude 886 counties that never had an operational OB unit as the inclusion of these "always-treated" counties can exacerbate negative weighting concerns in two-way fixed effects specifications (de Chaisemartin and D'Haultfoeuille, 2020). While these exclusions are theoretically important, Fischer et al. (2024) find very similar results when including all counties. We make one additional restriction that is specific to our setting: we exclude 154 closure counties that did not offer C-sections (closure counties reporting zero C-sections in the three years prior to closure). We make this exclusion because the empirical-Bayes procedure, discussed later, needs an estimated sampling error for each county.[20] Our results are similar, as we show later, when this restriction is removed. Our final sample consists of 326 closure counties (1,171,360 first births) and 1,095 control counties (29,429,318 first births).

Table 1 provides summary statistics for the main outcomes, risk factors, demographics, and a number of county characteristics. This shows that, in the cross-section, mothers in closure counties tend to be different from mothers in non-closure counties. Specifically, closure county mothers tend to have more risk factors for C-section delivery, give birth younger, are less educated, and more likely to be unmarried. Furthermore, closure counties tend to be much smaller and more rural. In Column 4, we re-weight control counties by the propensity to experience a closure, which makes mothers in treatment and control counties more similar. To do this, we calculate the propensity weights using a logistic regression that predicts whether a county ever experienced a closure, based on county characteristics measured in 1989, the first year of our sam-

[20]When the observed C-section rate is exactly zero, that sampling error is also zero, so the shrinkage weight cannot be meaningfully computed.

ple.[21] While cross-sectional differences between closure and non-closure counties do not directly threaten our identification since we rely on within-county variation, the difference-in-differences approach assumes parallel trends, which is more plausible when the two groups are more comparable. To address this, we also present propensity score-weighted difference-in-difference estimates that adjust for observable differences across counties. The results are broadly consistent with our main findings, as we demonstrate below.

# 4 Empirical Strategy

## 4.1 Objective

Our goal is to estimate how the location of birth influences the likelihood of a C-section. With this objective in mind, we introduce a statistical model of provider effects following other work in the place-based health effects literature (e.g., Finkelstein et al. (2016); Molitor (2018); Deryugina and Molitor (2021); Chyn and Shenhav (forthcoming); Badinski et al. (2023)). We assume an additively linear framework as common in this literature where outcomes are a function of place effects and person effects:[22]

$$C_{irot} = \underbrace{\underbrace{\lambda_{ot}}_{\text{Place of birth effect}} + \underbrace{\phi_r}_{\text{Place of residence effect}}}_{\text{Supply effects}} + \underbrace{\underbrace{\omega_i}_{\text{Person effect}}}_{\text{Demand effect}} + \kappa_{irot} \tag{1}$$

where $C_{irot}$ is an indicator for C-section delivery for first-time mother $i$, who resides in county $r$ in time $t$ if they give birth in county $o$. The set of $\lambda_{ot}$, $\phi_r$, and $\omega_i$ are the effects of place of occurrence, place of residence, and individual, respectively. $\lambda_{ot}$ are what we will call the 'provider effects' (e.g., effects operating through health care system such as hospital or physician practices), that may evolve over time. $\phi_r$ are supply-side effects operating through the place of residence (e.g., pollution could affect fetal development leading to higher risk of complications and thus higher risk of a C-section). For

[21] These characteristics include fertility rate, employment-to-population ratio, per capita earnings, per capita government transfers, female population share by 5-year age bands (15–19, 20–24, 25–29, 30–34, 35–39, and 40–44), total population, population density, and the percentage of urban area in the county.

[22] The literature uses both log-linear and linear specifications, with no single standard approach. Finkelstein et al. (2016) estimate a log-linear model, using the log of utilization (in dollars) as the dependent variable. In contrast, Molitor (2018) use a linear probability model with a binary outcome indicating catheterization. Similarly, Abaluck et al. (2021) rely on a linear additive model to estimate the mortality effects of Medicare Advantage plans.

simplicity and to be consistent with our empirical specification, we consider the place of residence effects to be time-invariant. $\omega_i$ are person effects, capturing both clinical risk factors and personal preferences and beliefs regarding C-sections. $\kappa_{irot}$ captures factors outside individual and place-varying determinants (e.g., national clinical guidelines) that affect C-section utilization. $\lambda_{ot}$ are the objects of interest.

We argue that our design separates $\lambda_{ot}$ from $\phi_r$, in contrast to the movers design, which identifies composite place effects (i.e., $\lambda_{ot} + \phi_r$). Following conventions in the health economics literature, we use "place effects" to refer to the combined supply-side variation driven by place of residence and place of birth occurrence and "individual effects" to capture demand-side variation driven by patient characteristics, beliefs, and preferences (Zeltzer et al., 2021).

Ordinary least squares estimation of Equation (1) faces several challenges. First, endogenous location choice induces selection bias: the location of care and location of residence may be correlated with the error term, $\kappa_{irot}$. For example, counties with elevated C-section rates also have disproportionately high Medicaid-funded birth shares (Robinson et al., 2024). Second, identification rests on comparing individuals across multiple births, yet delivery type is highly persistent across births, raising endogeneity concerns. A modified and less demanding specification that replaces individual demand effects $\omega_i$ with observed individual characteristics avoids relying on cross-birth comparisons. However, this is unlikely to resolve the endogeneity problem as unobservable or difficult-to-measure characteristics captured in $\kappa_{irot}$ are likely to be correlated with locational choices.

To fix ideas about how the place of birth occurrence effects might be identified, consider an ideal experiment in which for a given county of residence, mothers are randomly assigned their county of birth. This induces independence between county of birth and $\kappa_{irot}$. In other words, it removes the opportunity for mothers to select their county of birth. Our closure design approximates this ideal experiment. The closures remove the option for mothers to deliver in their county of residence and then they must seek care elsewhere. As long as the closures do not reallocate mothers to new counties in a systematic way (i.e., their selection abides by a fallback condition discussed later) and the characteristics of mothers do not change in response to the closures, we can identify the effect of county of birth on birth.

Our objective is to estimate the effect of county of birth on a mother's risk of C-section and attribute it to differences between counties in provider practice intensity, proxied by the county C-section rate. Following the place-effects literature, we relate our county-of-birth-occurrence causal effects to birth county C-section rates via a projection of $\lambda_{ot}$ on $R_{ot}$. That is,

$$\lambda_{ot} = \beta R_{ot} + \mu_{ot}. \tag{2}$$

The forecast residual $\mu_{ot}$ is orthogonal to $R_{ot}$ by construction. Equation (2) cannot be estimated directly in part as $\lambda_{ot}$, the causal place of birth effects are not observed.

Our main equation of interest, delivered via substitution of the expression for $\lambda_{ot}$ from Equation (2) into Equation (1) and conditioning on place of occurrence:

$$C_{irt} = \beta R_{irt} + \phi_r + X'_{irt}\theta + u_{irt}, \tag{3}$$

where

$$\begin{aligned}
C_{irt} &= \sum\nolimits_o C_{irot} D^{occurrence}_{irot}, \\
R_{irt} &= \sum\nolimits_o R_o D^{occurrence}_{irot}, \\
u_{irt} &= \sum\nolimits_o \mu_{ot} D^{occurrence}_{irot} + \sum\nolimits_o \kappa_{irot} D^{occurrence}_{irot}
\end{aligned}$$

where $C_{irt}$ is the observed C-section for mother i, residing in county r at time t conditional on their choice of birth county $D^{occurrence}_{irot}$, and $R_{irt}$ is observed C-section rate in the birth county given the mother's choice of birth county $D^{occurrence}_{irot}$. Note in Equation (3), given concerns about the reliance of comparisons across births when including individual fixed effects, we have replaced the individual fixed effects with $X$s, intended to capture individual characteristics that may affect the probability of a C-section. Conditions on the forecast error, $\mu_{irt} = \sum_o \mu_{ot} D^{occurrence}_{irot}$, later deliver the fallback condition.

The goal of our analysis is to estimate $\beta$. Abstracting from endogeneity issues, $\beta$ measures how a mother's C-section probability changes when they deliver in a county whose C-section rate differs from that of their county of residence. For example, if $\beta = 1$ then giving birth in a county with a 10 percentage points higher C-section rate would increase a mother's probability of C-section by a full 10 percentage points on average. On the other hand, if $\beta = 0.5$, then giving birth in a county with a 10 percentage points higher C-section rate would increase a mother's probability of C-section by only 5 percentage points on average. A larger value for $\beta$ reveals a stronger role for providers than patient in driving cross-county variation in C-section rates.

We treat $R_{irt}$ as given and, in line with the existing literature, do not microfound its determinants. Its current values may reflect the dynamic interaction of historical provider practices and patient preferences.

## 4.2 Use of Hospital-Based Obstetric Unit Closures to Identify Effects of Providers

We argue that hospital-based obstetric unit closures can provide useful variation in $\mathrm{R}_{irt}$ to identify $\beta$. The central idea is that when a county no longer has an operational OB unit, residents of that county expecting to give birth are reallocated to hospitals in nearby counties.[23] As C-section rates are not uniform across geographic space (Robinson et al., 2024), closures likely induce women to give birth in counties with different C-section rates.

Our study builds on Fischer et al. (2024) and Battaglia (2025), who use a difference-in-differences design to examine how the loss of a county's last hospital-based obstetric unit affects maternal and infant outcomes. Drawing on national birth data, they exploit within-county changes as obstetric units close. Fischer et al. (2024) find that closures reduce C-section rates on *average* without severe harms to maternal or infant health. We extend this work by leveraging the heterogeneity in these closures to isolate the role of providers in driving C-section use.

To build intuition for our approach, consider Equation (4) which describes the difference-in-differences framework utilized by Fischer et al. (2024) to estimate the average effects of OB unit closures:

$$Y_{irt} = \phi \text{Closed}_{rt} + \text{X'}_{irt}\theta + \delta_r + \tau_{ut} + \varepsilon_{irt}. \tag{4}$$

$Y_{irt}$ is an outcome for an individual i residing in county r at time t. The treatment variable, $\text{Closed}_{rt}$, is an indicator equal to one if the mother's county of residence does not have a hospital OB unit and 0 otherwise. The controls include county-of-residence fixed effects ($\delta_r$), urban group-by-year fixed effects ($\tau_{ut}$),[24] and county-level aggregated time-varying covariates ($\mathrm{X}_{irt}$).[25] $\phi$, the focal parameter of Fischer et al. (2024) and Battaglia

[23]Residents could also be contemplating giving birth outside of a hospital setting as a result of the closure, but Fischer et al. (2024) estimate small effects of a closure on the probability of a non-hospital birth (0.1 percentage points off of a base of 98.7%).

[24]As our analysis includes counties that vary in size and urbanicity, we incorporate urban group-by-year fixed effects to allow for geographic variation in time effects. While theoretically important, Fischer et al. (2024) find similar results when using a common set of year fixed effects across geographies. The urban group classification is implemented using the 2013 NCHS urban-rural classification, which classifies counties in six groups based on urbanicity; the fixed effects are interactions of year fixed effects and these six groups.

[25]Specifically, we include population shares for 5-year age bands, per-capita personal income, per-capita government transfers, and the employment-population ratio.

(2025), represents the *average effect* of a closure on the outcome.

The reduced-form health effects could operate through 3 main channels: reallocation to alternative health care providers (the focus of this work), increased distance to a provider, and crowding at facilities receiving the displaced mothers. Fischer et al. (2024) argue that crowding is unlikely to play a significant role, as displaced mothers make up only 6% of the population in the receiving counties. To separate the reallocation channel from the distance channel, we directly control for travel distance in our robustness checks.

***Construction of the C-Section Gap Measure* (Gap$_r$)** Our estimation strategy differs from Equation (4): rather than relying solely on the closure indicator, we exploit heterogeneity across closures by interacting Closed$_{rt}$ with our measure of geographic variation in C-section rates between closure and receiving counties. Our approach characterizes this heterogeneity by exploiting the difference in C-section rates between closure and receiving counties. This disparity, what we call the "C-section gap," is the pre-closure difference in C-section rates between the counties where mothers are likely to be reallocated post-closure (i.e., the receiving counties) and the closure county. More formally,

$$\text{Gap}_r = \text{R}_{\text{Receiving},h(r)-3\leq y<h(r)} - \text{R}_{\text{Closure},h(r)-3\leq y<h(r)} \tag{5}$$

In Equation (5), $\text{R}_{\text{Closure},\ h(r)-3\leq y<h(r)}$ denotes the average C-section rate in the closure county over the three years prior to the closure year, $h(r)$. Using a three-year window helps produce a more stable and potentially less biased measure of the C-section rate, which is particularly important given that many closures in our sample occur in small counties. Similarly, $\text{R}_{\text{Receiving},\ h(r)-3\leq y<h(r)}$ denotes the average C-section rate in the receiving counties over the same three-year pre-closure period. Measuring rates over the 36 months prior ensures that mother $i$'s outcome is excluded from the calculation.

Our simplified example of the C-section gap measure in the Introduction assumed a single receiving county per closure, though in practice mothers face multiple alternatives. To account for this, we construct a composite 'receiving' county for each closure county using a weighted average of potential destination counties, with weights based on pre-closure market shares.[26] The resulting measure, Gap$_r$, is a time-invariant

[26]We calculate pre-closure market shares by computing the probability of giving birth in each non-closure county, conditional on delivering outside the closure county before the closure. For example, suppose 100 mothers gave birth in the closure county the year before closure, and 50 delivered outside the county: 10 in county A, 20 in county B, and 20 in county C. The resulting pre-closure market shares would be 0.2, 0.4, and 0.4, respectively. We also construct receiving-county C-section rates using post-closure market shares, and our main estimates of $\beta$ are qualitatively similar.

county-level value representing the expected change in birth county C-section rates for each closure county. We take seriously the concern that non-random selection of birth county following a closure could threaten identification, and we later impose and justify a fallback condition that restricts the form of allowable selection.

To account for estimation error, both $\text{R}_{\text{Receiving},h(r)-3\leq y<h(r)}$ and $\text{R}_{\text{Closure},h(r)-3\leq y<h(r)}$ are adjusted using a standard empirical Bayes shrinkage procedure (Chandra et al., 2016). Although shrinkage makes little difference for the distribution of $\text{R}_{\text{Receiving},h(r)-3\leq y<h(r)}$, it does meaningfully shrink the distribution of $\text{R}_{\text{Closure},h(r)-3\leq y<h(r)}$ due to the fact that closure counties have a relatively small number of births and, therefore, less precisely estimated C-section rates. Figure 2A plots the distributions of $\text{R}_{\text{Closure},h(r)-3\leq y<h(r)}$ and $\text{R}_{\text{Receiving},h(r)-3\leq y<h(r)}$. Both exhibit similar overall shapes. But there are two notable differences: (1) the distribution of closure county C-section rates is more dispersed, and (2) the average closure county C-section rate is higher.

The distribution of C-section gaps in Figure 3 further emphasizes these distinctions between closure and receiving counties. C-section rates are higher in closure counties, suggesting that most mothers are reallocated to counties with lower C-section rates. The median of the distribution is -3.1 percentage points. Nevertheless, there is mass on both sides of zero. These gaps are substantial. For example, 22 closure counties have a gap of more than 20 percentage points in absolute value. To put this in perspective, a 20 percentage point shift in the distribution of county C-section rates is roughly equivalent to moving from the 95th to the 5th percentile (see Figure 2B).[27,28]

Since our identification strategy relies on variation in C-section rates between closure and receiving counties, one concern is whether this variation is representative of the national distribution. Figure A2 compares the distribution of C-section gaps in our closure county sample to that from randomly matched counties drawn from the full set of U.S. counties. While our sample exhibits a slightly lower standard deviation (0.081 vs. 0.099), our C-section gap distribution has heavier tails, suggesting that it still captures substantial and meaningful variation in C-section rates nationwide.

***Construction of Our Main Independent Variable of Interest*** ($R_{irt}$) Analogous to the construction of Gap, we define our focal regressor $R_{irt}$ as the C-section rate in the county

[27] The difference between the 95th and 5th percentile of the risk-unadjusted (risk-adjusted) C-section rate is 20.6 (22.9) percentage points.

[28] For comparison, the interquartile range—defined as the difference between the 75th and 25th percentiles—across Hospital Referral Regions in Molitor (2018) is 0.099 for cardiac catheterization use among Medicare patients, whereas in our context, the interquartile range across counties for C-section use is 0.085.

of birth $o$ chosen by individual $i$, measured over the 36 months preceding $t$. Thus, individual $i$'s own birth is excluded from this calculation.

### 4.3 Formalizing Our Instrumental Variables Approach

To utilize the heterogeneity captured by $\text{Gap}_r$, we define our instrument $Z_{rt}$ for $R_{irt}$ as the interaction of $\text{Closed}_{rt}$ and $\text{Gap}_r$ as defined by Equation (6).[29]

$$\text{[Instrument]} \qquad Z_{rt} = \text{Closed}_{rt} \times \text{Gap}_r \tag{6}$$

As we detail below, identification could be threatened if $\text{Gap}_r$ is correlated with other factors that affect C-section rates, such as maternal health risk or access to care. One key concern is that hospital closures increase the travel distance to a hospital-based obstetric unit, and this increased distance could itself influence C-section rates. For example, a provider, knowing that a patient needs to travel far, may induce the birth, which itself could change in the probability of a C-section.[30] To address this, our robustness checks include controls for changes in distance from each county's population-weighted centroid to the nearest county with an operating obstetric unit.

As an alternative parameterization of our instrument, we could, in principle, use $\text{Closed}_{rt} * \text{Pre-Closure C-section Rate}_r$ as an instrument for $\text{R}_{irt}$. This is akin to the strategy of Abaluck et al. (2021). The logic of this approach builds on regression to the mean. If a county's C-section rate is high, then residents of that county should be exposed to lower C-section rates in the presence of a closure. On the other hand, if the C-section rate is low, the C-section rate for the county of birth occurrence should rise on average. We prefer the $\text{Closed}_{rt} \times \text{Gap}_r$ for at least three reasons. First, it leverages heterogeneity in a mother's choice set following the closure. Second, it allows us to control for any direct common effects of closures through the inclusion of $\text{Closed}_{rt}$. It is plausible that closures affect C-sections through mechanisms other than reallocation to counties with different C-section rates (e.g., travel distance). Third, our preferred parameterization provides more variation.

The first-stage and reduced-form equations are described in Equations (7) and (8). The regression equations include all the same controls as Equation (4). Additionally, because the instrument $Z_{rt}$ exploits heterogeneity in the effects of closures, we control

[29] In never-treated counties, $\text{Z}_{rt} = 0$ for all observations.

[30] In Fischer et al. (2024), they probe this mechanism and find that it explains roughly 10% of the closure-induced change in C-section rates.

for the direct effects of closures that do not vary by $\text{Gap}_r$, captured by $\phi_2$ and $\pi_2$.

$$\text{[First Stage]} \qquad R_{irt} = \phi_1 Z_{rt} + \phi_2 \text{Closed}_{rt} + X'_{irt}\theta^{FS} + \delta_r^{FS} + \tau_{ut}^{FS} + \varepsilon_{irt}^{FS} \tag{7}$$

$$\text{[Reduced Form]} \qquad C_{irt} = \pi_1 Z_{rt} + \pi_2 \text{Closed}_{rt} + X'_{irt}\theta^{RF} + \delta_r^{RF} + \tau_{ut}^{RF} + \varepsilon_{irt}^{RF} \tag{8}$$

# 5 Assumptions Underlying Our Empirical Approach

We lay out the identification conditions for our IV —relevance, exogeneity, and a fallback condition (Abaluck et al., 2021).

## 5.1 Instrument Relevance

The relevance of the instrument requires that

$$Cov(Z_{rt}, R_{irt} | \text{Closed}_{rt}, \text{X}_{irt}, \delta_r, \tau_{ut}) \neq 0$$

In words, closures must change birth county C-section rates and the closure-driven changes in the C-section rates must be correlated with the pre-closure C-section gap between closure and receiving counties, $Gap_r$.

Column (1) of Table 2 presents the estimate of $\phi_1$, the effect of the instrument on the county-of-birth-occurrence C-section rate from Equation (7), providing direct evidence in support of the relevance assumption. The estimate implies that in a county with a 10 percentage point pre-closure C-section gap, the average woman gives birth in a county with a 2.52 percentage point higher C-section rate following the closure. The first stage is strong, with an F-statistic of 124.3. The estimate of $\phi_1$ is less than 1 because not all mothers change their delivery location in response to a closure. Some were already giving birth outside their county of residence before the closure. After a closure, the probability of delivering outside the county increases by 27 percentage points. Assuming C-section rates remain stable, this implies that the expected first stage coefficient would be 0.27, which is close to the estimate we obtain.

In Figure A3, as complementary evidence, we present visual diagnostics for the alternative IV but less preferred design leveraging regression to the mean (i.e., $Z = \text{Closed}_{rt} * \text{Pre-Closure C-section Rate}_r$). Panel A of Figure A3 presents a binned scatter plot illustrating the relationship between current C-section rates in the county of birth occurrence (Y-axis) and lagged C-section rates (X-axis).[31,32] Among mothers in closure counties, we expect a negative relationship: higher pre-closure C-section rates should be associated with larger declines post-closure. In contrast, for mothers in non-closure counties, due to persistence in C-section rates, the plot should lie near the 45 degree line. These patterns accord with the data.

## 5.2 Exogeneity

The standard exclusion restriction states that the instrument is conditionally uncorrelated with unobserved determinants of C-section delivery. That is,

$$Cov(Z_{rt}, \varepsilon_{irt} | \text{Closed}_{rt}, \text{X}_{irt}, \delta_r, \tau_{ut}) = 0$$

While this assumption is not directly testable, we offer indirect evidence to support its plausibility. First, because our identification relies on a difference-in-differences framework, we examine the viability of the parallel trends assumption by providing a series of event studies. Second, we conduct balance tests by treating potential confounders such as maternal age as outcomes, and test whether the variation in C-section gaps generated by closures is systematically correlated with these characteristics.

### 5.2.1 Assessing Parallel Trends

**Event Studies Showing Reduced-Form Impacts of Obstetric Unit Closures**

To test the credibility of the parallel trends assumption, we begin with event-study versions of Equation (4), which do not consider the heterogeneity in C-section gaps and estimate average effects. We then later extend the analysis by presenting event studies that incorporate this heterogeneity. In these first event study specifications, we replace the single post-treatment indicator ($\text{Closed}_{rt}$) with a vector of ten indicators for year relative to closure ($\sum_{j=-5}^{-2} \psi_j \text{Closed}_{r,t+j} + \sum_{j=0}^{5} \psi_j \text{Closed}_{r,t+j}$) in Equation (4):[33]

[31] All binned scatter plots in Figure A3 control for a common set of county-level, time-varying covariates: the female population share by 5-year age group, per-capita personal income, per-capita government transfers, and the employment-population ratio. In addition, we include county-of-residence fixed effects and urban group-by-year fixed effects in all specifications.

[32] For mothers residing in non-closure counties, the lagged C-section rate is the county of residence C-section rate for the previous year. For mothers in closure counties, the lagged C-section rate is defined as the average rate in the closure county over the three years prior to the closure ($R_{\text{closure},h(r)-3\leq y<h(r)}$).

[33] The two end points ($j = -5$ and $j = 5$) represent five *or more* years prior to treatment and five *or more* years post-treatment and, as such, the specification is fully saturated. Because the end points are not comparable with the other estimates, the end points are omitted from the figures displaying the results.

$$Y_{irt} = \sum_{j=-5}^{-2} \psi_j \text{Closed}_{r,t+j} + \sum_{j=0}^{5} \psi_j \text{Closed}_{r,t+j} + \text{X'}_{irt}\theta + \delta_r + \tau_{ut} + \varepsilon_{irt} \tag{9}$$

Figure 4 presents event study estimates for the first stage (C-section rate in county of birth occurrence with $Y_{irt} = C_{irt}$) and reduced-form outcomes (a mother's probability of a C-section (i.e., $Y_{irt} = R_{irt}$)). We first estimate the effect of closures on outcomes *on average*, presented in Figure 4A and Figure 4B. Importantly, for the plausibility of the parallel trends assumption, the average effects show little evidence of pre-closure differential trends in the outcome, and show a discrete change in both outcomes that coincides with the timing of treatment. The reported "Avg. Effect" represents an estimate of $\phi$ from Equation (4); Figure 4A shows that on average closures reallocate women to counties with 1.3 percentage points lower C-section rates, and Figure 4B suggests that these women are 1.3 percentage points less likely to have a C-section. While suggestive of strong provider effects, this evidence shows that closures affected overall C-section rates but does not exploit variation in the C-section gap, the core focus of this study.

**Event Studies Leveraging Variation in C-Section Gap: The Focus of this Study**

As our treatment is continuous, it falls outside the standard difference-in-differences framework. To more directly assess the parallel trends assumption underlying our empirical strategy, we follow the recommendation of Callaway et al. (2024) and disaggregate the continuous treatment. Specifically, in the bottom panels of Figure 4, we present event study estimates for treated counties in the first and fourth quartiles of the $\text{Gap}_r$ distribution, shown separately in Figure 4C, the first stage estimates, and Figure 4D, the reduced-form estimates. All untreated counties are included in the regressions for both groups.

In Figure 4C, we plot event study estimates of changes in the C-section rate of the county where mothers give birth. At the time of closure, mothers living in counties in the first quartile (i.e., those with the largest negative C-section gaps) experience a sharp drop of about 3 to 4 percentage points in the C-section rate of their birth county. This reflects a strong first-stage relationship, as previously documented. While this change is smaller than the pre-treatment gap of –17.1 percentage points, this is expected because the estimates reflect intent-to-treat effects as only 27% of mothers are compliers (see Figure A4).[34] For mothers in the fourth quartile (those from counties with higher base-

[34] In addition, pre-closure C-section rates are not perfectly predictive of post-closure C-section practices.

line C-section rates than the receiving counties), the C-section rate in the county where they give birth increases by approximately 1 to 2 percentage points.

There are two key takeaways from the combined event study plots shown in Figure 4C and Figure 4D. First, the figures reinforce our main finding: mothers reallocated to counties in the bottom quartile of the C-section gap ($\text{Gap}_r$ < -7.6 percentage points, mean -13.5 percentage points) experience a drop in their C-section probability, whereas those reallocated to counties in the top quartile ($\text{Gap}_r$ > 1.9 percentage points, mean 7.1 percentage points) experience an increase. Second, both plots show clear breaks in outcomes that align precisely with the timing of hospital closures, echoing the patterns in Figure 4A and Figure 4B. This sharp discontinuity supports a causal interpretation rather than one driven by pre-existing or gradual trends.

However, a closer look at the pre-closure periods might raise some potential concerns about differential trends between treated and control counties. Specifically, Figure 4C suggests that the first-stage outcome was rising for counties in the lower end of the $\text{Gap}_r$ distribution. However, the trends are modest though compared to the treatment effect. To address the concern about potential bias caused by these trends, our primary IV estimates restrict the sample of treated counties to a 4-year window around each closure (two years before and two years after) excluding the partially treated year of closure. This restriction ensures that the estimates are primarily driven by the sharp changes in the outcome at the time of closure, reducing the potential bias from slow-evolving differential trends. Furthermore, we assess the impact of these trends on the IV estimates by varying the treatment window for closure counties. Our main results are qualitatively unchanged by varying the window.

### 5.2.2 Balance Tests

Our interpretation of the IV estimates as provider effects would be flawed if closures led to changes in C-section rates that varied with the size of the C-section gap, and if those gaps were correlated with other factors influencing C-sections. For example, if high-gap counties experienced more selective fertility and thereby changing the composition of mothers giving birth, then any post-closure changes in C-section rates correlated with the gap might reflect shifts in patient characteristics rather than provider behavior. We are less concerned about correlations between C-section gaps and provider characteristics, as we consider those part of the provider effect. To assess the exogeneity of the instrument, we conduct a balance test to see whether it is correlated with observable maternal characteristics.

To do this, we conduct a summary balance test by calculating the predicted probability of C-section delivery for each mother, based on various medical risk factors and demographic variables, and use that predicted probability as the outcome in Equation (7).[35] The results, presented in column (3) of Table 2, confirm that there is little evidence of imbalance across our set of observed characteristics. Specifically, for a county with a 10 percentage points higher pre-closure C-section gap, closures are associated with a statistically insignificant 0.05 percentage points decrease in predicted C-section risk. Furthermore, in Figure 5, we perform this balance test separately for each of the 20 predictors and find that only one is statistically significant at the 5% level.

Evidence of balance for the complementary regression-to-the-mean IV design is shown in Panel B of Figure A3. This substitutes the predicted C-section rate, estimated as described above, for the county-level rate used in Panel A. Both scatterplots of nonclosed and closed counties are flat suggesting that health characteristics and risk factors related to C-section are not related to lagged C-section rates, providing support for the exclusion restriction.

#### 5.2.3 Potential Biases Due to Heterogeneous Treatment Effects

Lastly, since our approach is based on a difference-in-differences framework with a two-way fixed effects specification, it is crucial to ensure that our estimates are robust to potential treatment effect heterogeneity (Roth et al., 2023). In Figure A5, we present an alternative version of Figure 4C and Figure 4D, using the de Chaisemartin and D'Haultfoeuille (2020) estimator, which is robust to heterogeneous treatment effects. The results are quite similar to those obtained from the two-way fixed effects estimates in our main specification, which is expected given the substantial share of never-treated units. Nonetheless, these supplementary analyses increase our confidence that negative weights are not significantly impacting our IV estimates.

### 5.3 Fallback Condition

The last assumption is the "fallback" condition as articulated in Abaluck et al. (2021). This condition addresses the concern that even if closures are as-good-as randomly assigned (conditional on controls), the choice of birth county after a closure might not

[35] The medical risk-factors we include are: 5-year age bins (15-19, 20-24, 25-29, 30-34, 35-39, 40-44, 45+), breech presentation, eclampsia, chronic hypertension, gestational hypertension, and gestational diabetes. The demographics we include are: marital status (an indicator for unmarried), education (an indicator for some college or more education), and race (indicators for White non-Hispanic, Black non-Hispanic, and Hispanic).

be.

Formally, this condition is described as

$$Cov(Z_{rt}, \mu_{irt}|\text{Closed}_{rt}, \text{X}_{irt}, \delta_r, \tau_{ut}) = 0$$

where $\mu_{irt}$ is the forecast residual of the chosen birth county for individual $i$ residing in county $r$ at time $t$ (see Equation (2) and the discussion surrounding Equation (3)). Mechanically, this requires that the unforecastable component of the relationship between the causal county of birth effects and the county C-section rate, and the instrument be uncorrelated. This assumption has no structural interpretation, as $\mu_{irt}$ comes from a statistical relationship (Abaluck et al., 2021).

The fallback condition implies that individuals in a closure county with a particular C-section rate do not systematically change the way they select counties in which to give birth differently from individuals in closure counties with a different pre-closure C-section rate. In this setting, we believe that since the analysis is restricted to first births, the fallback condition is more likely to hold *a priori*. Since these mothers are giving birth for the first time, there is limited scope for selective sorting based on prior delivery experiences.

The fallback condition offers a "non-nested" alternative to the standard IV monotonicity assumption (Abaluck et al., 2021). As Abaluck et al. (2021) emphasize, a key advantage is that the fallback condition can be microfounded within standard discrete choice models.

For higher-order births, the fallback assumption is less likely to hold. A prior C-section is a strong predictor of a repeat C-section, which may lead mothers to place greater weight on local C-section practices when selecting a delivery location after a closure. As a result, their post-closure county choice may systematically depend on their original county's C-section rate, potentially violating the fallback assumption.

To evaluate the plausibility of this condition, we implement the fallback test proposed by Abaluck et al. (2021). Although we cannot directly observe $\mu_{irt}$, the test relies on constructing a proxy for it.

To perform the fallback test, we begin by regressing $R_{irt}$ on a set of provider-level characteristics for that county. We included provider-level characteristics we expected ex ante to affect C-section rates. These include the share of hospitals with a neonatal intensive care unit (NICU), the share affiliated with a medical school, the share using an integrated salary model,[36] the share for-profit, and the share of births attended by

[36]In integrated-salary systems, doctors are salaried (i.e., not paid per procedure) so compensation is independent of delivery type, weakening incentives for C-sections.

midwives. All variables are aggregated to the county level. The midwife measure is averaged over a three-year period using NVSS data, where reporting is more consistent. The remaining variables are constructed using data from the previous three years in the American Hospital Association database. This step produces fitted values, the predicted county-level C-section rate based on provider characteristics. We then retrieve the residuals, which become our proxy for $\mu_{irt}$, from a projection of these fitted values on the county of birth occurrence C-section rates, $R_{irt}$. The fallback test assesses whether these residuals are correlated with the instrument. A lack of correlation provides support for the fallback assumption. The fallback test reports the coefficient from regressing the fallback residuals on the instrument. In column (4) of Table 2, the estimated coefficient, a value of -0.0053, is small and statistically insignificant.

As a further test of the fallback condition, we evaluate the stability of birth-county choices after a closure. In particular, we compare pre- and post-closure birth shares across receiving counties in Figure A6.[37] A strong correlation between these shares indicates that mothers' delivery-location choices remain largely unchanged with a closure. The observed correlation of 0.97 provides further support for the plausibility of the fallback assumption.

For the complementary IV design leveraging regression to the mean, Panel C of Figure A3 demonstrates the relationship between the fallback residuals (y-axis) and lagged C-section rates. The fitted line is essentially flat in closure counties and only weakly sloped in non-closure counties, consistent with the fallback restriction for the complementary IV design.

# 6 Results

## 6.1 IV Estimates

In Tables 3–4, we present several versions of the IV estimates based on Equations (7) and (8). Our preferred IV estimate is presented in Column (1) of Table 3 and is labeled "4-Year," indicating the sample is limited to four years of data for treated counties: two years before and after closure. We exclude the closure year because treatment status in that year is ambiguous. The direct interpretation of our preferred estimate of 1.052 is that when a first-time mother is reallocated to give birth in a county with a 10 percentage points higher C-section rate, the mother's own probability of a C-section increases by

[37] The pre-closure share is calculated as the probability of a birth in a given receiving county, conditional on delivering outside the closure county.

10.5 percentage points.

This strong provider effect is also visually evident in the similarity between Panels A and D of Figure A3 for the complementary IV regression-to-mean design. Panel A shows that mothers from closure counties with high (low) C-section rates tend to be reallocated to receiving counties with lower (higher) rates. Panel D demonstrates that these reallocations result in corresponding changes in actual C-section probabilities: mothers from low-rate closure counties, who are likely to deliver in higher-rate counties, indeed experience higher C-section rates.

Our estimated provider effect exceeds expectations from a survey we conducted with 225 experts, including health economists, obstetrics researchers, and practicing obstetricians. The median respondent predicted that providers account for 60 percent of the geographic variation in C-section rates (see Figure A7). Our estimate is larger than the 0.5 to 0.6 supply-side contribution to health care utilization reported by Finkelstein et al. (2016), which they identify using variation in Medicare utilization arising from beneficiary migration.

Columns (2)-(5) of Table 3 present estimates from models using extended sample windows for treated counties. The estimates change little when using sample windows up to 10 years (the 10-year estimate is 1.058), suggesting that slow-moving differential trends are not contributing substantial bias. Even the use of the unrestricted sample, which allows up to 30 years of pre- or post-treatment data, only changes the preferred estimate by 3.4 percentage points (to 1.086).[38] Across all main specifications, the county-level C-section rate is not risk-adjusted, since risk adjustment would remove some of the variation attributable to patient composition. In Table A1, we present results with the risk-adjusted county level C-section rate. The effects are moderately smaller but remain consistent with the main conclusion that provider effects are substantial and exceed the experts' predictions.[39]

Table 4 reports further robustness checks for the IV estimates using the four-year

[38]We also consider a 2 year treatment window, using only one year of data on either side of the closure, which yields an estimate of 0.862. We prefer a slightly larger treatment window for two reasons. First, using only one year of data on either side of the closure means the estimates are more vulnerable to idiosyncratic variation; the stability of the estimates for treatment windows between 4 and 10 years suggests the 2-year estimate is likely to be an outlier caused by idiosyncratic variation rather than caused by trends we have failed to account for. Second, the years immediately surrounding the closure year could be considered transition years, reflecting transitory rather than more permanent behaviors.

[39]Theoretically, the effect of risk adjustment on the direction of the estimates is ambiguous and depends on the correlation between the risk adjusted C-section gap and the demand-driven component of the birth county C-section rate and the correlation between the risk adjusted C-section gap and the supply-driven component of the birth county C-section rate. Each of these correlations can be of opposite sign.

treatment window. First, we instrument with the interaction of $Z_{rt}$ and the share of mothers who delivered in their county of residence during the three years before the closure. This allows treatment intensity to vary: where pre-closure reliance on local hospitals is higher, the closure-induced gap affects more mothers.[40] This tests robustness to an alternative parameterization of treatment. "P-Weighted" in column (2) weights the sample from non-closure counties by the propensity to experience a closure. "Median Instrument" in column (3) uses a version of the instrument that replaces the continuous measure of $\text{Gap}_r$ with an indicator for the gap being above the median. In column (4), "$R_{\text{closure}}$ Instrument" uses the alternative version of the instrument using the regression-to-the-mean argument (i.e., $Z_{rt} = \text{Closed}_{rt} * \text{Pre-Closure C-section Rate}_r$) which is more similar in structure to the instrument used in Abaluck et al. (2021). Finally, "Distance Controls" in column (5) adds distance to the nearest county with a hospital-based obstetric unit from the county of maternal residence and its square as controls. The minimum value for the IV estimate across these robustness checks is 1.025, suggesting robust evidence of dominant provider effects.

We consider the sensitivity of our results to the inclusion of counties with a C-section rate of 0 in Table A2. In our main specifications, we apply empirical Bayes shrinkage to the C-section gap measure. However, as this excludes counties with a zero C-section rate, we consider what happens if the zero C-section rate counties are included. In this robustness specification, we apply empirical Bayes shrinkage for closure counties with non-zero C-section rates but use $R_{\text{closure},h(r)-3\leq y\leq h(r)} = 0$ for closure counties with an observed zero C-section rate prior to closure. As a result, the distribution of $R_{\text{closure},h(r)-3\leq y\leq h(r)}$ exhibits a mass point at zero. The resulting estimate is 1.110, compared to 1.052 in our main sample.

Table 5 presents estimates for different subsamples. The first two columns are subsampled by predicted C-section need based on observable risk factors available in the vital statistics data.[41] "Low Need" ("High Need") represents births with a predicted probability of C-section below (above) the 75th percentile.[42,43] Among low-need mothers, the IV coefficient relating county C-section rate to the probability of a C-section is 1.146, slightly larger than the overall sample coefficient. For high-need mothers, the IV estimate is only 0.663. Taken at face value, this suggests provider effects matter less when

[40] We thank Jacob Light for suggesting this alternative IV parametrization.

[41] The variables entering the model to predict C-section need include weeks of gestation, maternal age, breech, eclampsia, chronic hypertension, pregnancy hypertension, and diabetes.

[42] The predicted probability of C-section at the 75th percentile is 0.28.

[43] Because C-section rates reflect provider behavior, we focus on the overall rate rather than subgroup rates based on predicted C-section risk. The predicted risk is itself influenced by provider behavior, as it is derived from regressions of C-section incidence on maternal characteristics.

mothers present observable C-section risk factors. Consistent with this interpretation, a breech presentation typically results in a C-section regardless of where the birth occurs.

Column (3) of Table 5 presents the estimate for higher-order births.[44] The effect of county C-section rate is reduced for higher order births to 0.423. This is likely due to the extremely strong persistence in C-section probabilities across birth order. For mothers who had a prior C-section, nearly all providers will recommend a C-section for subsequent births. In this case, a mother's risk factors (i.e., prior C-section) dominates, leaving little role for providers to determine variation in C-section rates.

Columns (4) and (5) present subsample analyses based on the level of OBGYN market competitiveness in the receiving county. Specifically, we stratify counties by their Herfindahl-Hirschman Index (HHI), a measure of market concentration. For these calculations, we directly compute the HHI for each county using the number of hospital beds reported in the American Hospital Association (AHA) Annual Survey as a proxy for market share.[45] When mothers are reallocated to a county with only one OBGYN provider (i.e., HHI = 10,000), the provider effect tends to be larger at 1.244, suggesting that limited provider choice amplifies the provider effect.

## 6.2 Unpacking C-Section Variation: The Supply-Side Role of Hospitals

In this section, we take a closer look at how provider effects on C-section rates correlate with observable hospital characteristics. We begin with a descriptive look at how these characteristics relate to county-level C-section rates. Then, using our IV framework, we use the closure design to ask how closure-induced changes in county-level hospital and provider attributes map into changes to C-section rates. The hospital data are drawn from the AHA Annual Survey (American Hospital Association, 1989-2019), which we aggregate to the county-year level using the number of births at each hospital as weights. To focus on relevant providers, we restrict the sample to hospitals that offer obstetric care.

We examine a set of hospital characteristics that may capture heterogeneity in clinical practice styles and institutional capacity. Specifically, we consider whether the hospital operates on a for-profit basis, whether physicians are compensated under a fully

[44]For these estimates, $R_{irot}$ and $Gap_r$ are calculated using higher order (second or higher) births (rather than first births only as in the main specification).

[45]For example, if there are two hospitals with OBGYN services in county A, one with 100 beds and the other with 300 beds, the HHI is calculated as $(100/(100+300))^2 + (300/(100+300))^2 = 0.0625 + 0.5625 = 0.625$, or 6,250 when scaled by 10,000. If there is only one hospital with OBGYN in the county, HHI is 10,000 by its construction.

integrated salary model (as opposed to fee-for-service), whether the hospital is affiliated with a medical school, and whether it has a neonatal intensive care unit (NICU).[46] We also incorporate two variables reflecting labor and delivery capacity. The first is the share of births attended by midwives over the previous three years, calculated using natality data from NVSS. The second is the number of births per bassinet (per 100 births), which serves as a proxy for delivery capacity and is calculated from AHA data. Together, these measures allow us to assess whether profit motive, quality and resources, and labor and capacity change C-section probabilities.

County-level aggregated characteristics and instruments are computed in the same manner as the C-section gap, that is, measured over the three years prior. One difference is that we do not apply empirical Bayes shrinkage to the instruments. This is because many closure counties have only a single hospital prior to the closure, making it infeasible to calculate a within-county standard error.

The first column of Table 6 labeled as 'Correlation' reports the raw correlations between county-level C-section rates and aggregated hospital characteristics. The remaining columns present IV estimates labeled as 'Hospital Characteristics IV Regression'. The IV estimates are derived from Equations (7) and (8) after substituting $R_{irt}$ with each hospital characteristic and constructing $Z_{rt} = Closed_{rt} \times Gap_r$, with $Gap$ defined separately for each hospital characteristic. To increase power, given that the interacted instrument (i.e., the interaction between $Z_{rt}$ and the share of mothers giving birth in their county of residence in the three years prior to the closure) has a strong first stage (F-statistic = 199) as shown in Column (1) of Table 4, we use this instrument for the hospital characteristics regression.[47] Each IV coefficient measures how a closure-induced reallocation toward providers with higher values of a given attribute shifts C-section risk. For example, a closure that redirects births to counties with a 10–percentage-point higher share of for-profit hospitals increases the C-section rate by 2.24 percentage points.

County C-section rates are most strongly associated, in raw correlations, with the share of for-profit hospitals (positive) and the share of births attended by midwives (negative). In our IV design, indicators of hospital quality and resources as measured by the shares of medical-school-affiliated hospitals and of hospitals with a NICU, have statistically significant (5 percent level) negative effects: a closure-induced reallocation to a county with a 10-percentage-point higher share of medical-school-affiliated hospitals (NICU availability) reduces the C-section rate by 0.834 (0.728) percentage points.

[46] These characteristics were used in our fallback test.

[47] Appendix Table A3 presents results using the standard gap IV without the interaction. We confirm that the first-stage F-statistic is mostly weaker under this alternative specification.

These IV estimates are opposite in sign to the corresponding raw correlations, consistent with selection into higher-resource markets. Other attributes yield smaller, less precise effects; the midwife share suggests a reduction of comparable magnitude but is imprecisely measured. Taken together, the evidence indicates that stronger quality-of-care infrastructure enables providers to manage deliveries without relying on surgical intervention.

While the IV estimates are causal for closure-induced changes in exposure to provider attributes, they do not by themselves identify the causal effects of specific hospital features such as medical-school affiliation or NICU availability on C-section use. That stronger interpretation would require these attributes to be orthogonal to unobserved determinants of C-section rates. Nonetheless, the patterns offer useful insights into the institutional correlates of lower C-section rates.

## 6.3 Effects on Maternal and Infant Outcomes

Having established that provider-side factors play a major role in determining C-section rates, we next examine whether these differences translate into changes in maternal and infant health outcomes, as shown in Table A4. We anticipated limited statistical power with this analysis because of the noise in the reporting of some outcomes (e.g., maternal morbidity)[48] and the rarity of others (e.g., infant mortality). All of the estimates are imprecise. Among them, the most precise is for low APGAR (<7), a binary indicator of poor newborn health, with a p-value of 0.137. The point estimate implies that a 10 percentage point increase in the county C-section rate reduces the incidence of low APGAR by about 1 percentage point. This effect size is comparable to the IV estimates from Card et al. (2023). Card et al. (2023) study the health effects of a C-section. They use a research design based on relative distance to high- vs. low-C-section hospitals in California. They report a two-stage least squares estimate of -0.0079 for the effect of delivering at a high-C-section hospital on the likelihood of a low APGAR score among low-risk first births. Given the reported mean C-section rates of 0.150 at high-C-section hospitals and 0.106 at low-C-section hospitals, a back-of-the-envelope calculation implies that a full (0 to 1) shift in hospital C-section rate is associated with a -0.179 change in the probability of a low APGAR score (i.e., -0.0079 / (0.150 - 0.106)). This figure is within the 95% confidence interval of our estimate.

[48] Northam and Knapp (2006) note that the information on tobacco and alcohol use, prenatal care, maternal risk and pregnancy complications are less reliable than that for birth weight, APGAR score or delivery method.

# 7 Discussion

## 7.1 Understanding IV Estimates Through the Lens of Changes in the Health Care Environment

We argue that the primary channel through which obstetric unit closures affect C-section rates is the change in the set of providers (both medical medical professionals and hospitals) attending deliveries. A key assumption underlying this argument is that providers in closure counties do not simply relocate to nearby counties after a closure. If they did, we would expect little response in C-section rates to the C-section gap.

To assess potential resorting, we use Health Resources and Services Administration data (Health Resource & Service Administration, 1995-2013), which incorporates information from the American AMA Physician Professional Data on the number of obstetricians/gynecologists by county. As expected, closures reduce OB/GYN presence in affected counties, from 1.452 to 0.633 on average (Panel A of Figure A8). Receiving counties experience only a modest increase, with an average gain of 0.956 providers on a base of 35.021 (2.7 percent; see Panel B of Figure A8). This suggests that the impact of provider reallocation on provider practices in receiving counties is small and unlikely to be driving our results.

## 7.2 Identification in Related Work

Our approach builds on a growing literature that uses quasi-random reallocation of patients to study the effects of health care organizations or locations on outcomes. These studies exploit plausibly exogenous shifts in where patients receive care to estimate causal impacts of health care settings. For example, Doyle et al. (2015) use variation in ambulance referral patterns to examine the returns to hospital spending for elderly emergency patients, while Chan et al. (2023) apply a similar strategy to study how ambulance routing affects whether veterans are treated at VA hospitals or Medicare-funded private hospitals.[49] Abaluck et al. (2021) leverage Medicare plan terminations to assess the effect of insurance plans on mortality. Most similar in terms of research design, Chandra et al. (2023) exploit hospital closures to study the relationship between observed and causal hospital-level mortality for Medicare patients. Like these studies, we use plausibly exogenous variation in care location, driven by obstetric unit closures, to isolate the influence of provider characteristics on clinical practices.

[49] This strategy is not applicable in our context, as most mothers do not arrive at hospitals by ambulance.

Another prominent strategy for identifying place effects is the mover design (Finkelstein et al., 2016; Deryugina and Molitor, 2021; Chyn and Shenhav, forthcoming). These designs typically examine how individual outcomes change when people move from one location to another and correlate those changes with differences in average outcomes across the two places. For example, Deryugina and Molitor (2021) leverage heterogeneity in geographic displacement following Hurricane Katrina to show that individuals who relocated to lower-mortality areas experienced lower subsequent mortality than those who moved to higher-mortality regions.

These studies identify place effects that reflect a bundled influence of both residential and provider environments. For instance, when someone moves from Michigan to Florida, their health may be affected by environmental factors such as climate or pollution, as well as by differences in the quality or style of medical care. These combined effects are useful for estimating the overall impact of place but do not isolate the role of providers.

Our objective differs. Rather than estimating the overall place effect, we attempt to isolate the provider component from other location-specific factors, such as residential environment and social context. Obstetric unit closures prompt mothers to change where they deliver without changing where they live, offering a setting in which provider effects can be identified separately from other place-based influences.

# 8 Conclusion

Exploiting variation from obstetric unit closures in an instrumental variables framework, we find that provider behavior plays a dominant role in driving geographic differences in C-section rates. The provider effect we estimate is larger than experts anticipated and exceeds place effects found elsewhere (e.g., Ding (forthcoming); Finkelstein et al. (2016); Chyn and Shenhav (forthcoming)). Importantly, those studies leveraging residential moves across geographies capture the effect of where people live and receive care, while our estimates isolate the effect of the place of care.

Our estimated effects are especially strong for low-risk mothers, exactly the group for whom unnecessary C-sections are most concerning and where policy efforts have focused. These results point to the central role of supply-side factors in shaping obstetric care and suggest that targeting provider practices may be key to improving maternal health and reducing avoidable medical interventions.

Our analysis does not take a position on the welfare implications of geographic variation in C-section rates. These differences may or may not reflect efficient practice patterns. Higher rates could result from greater surgical expertise, and reducing them may not necessarily improve outcomes (Currie and MacLeod, 2017). Evaluating the welfare effects of C-sections is difficult: for some mothers and infants they are clearly beneficial, for others unnecessary or even harmful (Card et al., 2023). Clinical appropriateness is hard to determine ex ante, limiting our ability to distinguish necessary from unnecessary procedures. Beyond physical risks, C-sections may also entail psychological costs that are harder to measure (Sega et al., 2021).

The findings of this study may help to inform ongoing efforts to reduce unnecessary C-sections, particularly among low-risk mothers. Initiatives such as those led by the California Maternal Quality Care Collaborative disseminate information on C-section rates to both patients and providers. Our results suggest that provider-facing components of these programs, when effective in changing provider behavior, may meaningfully affect the likelihood of a surgical delivery. While prior research finds limited effects from patient-targeted information campaigns alone (Gourevitch et al., 2019), the sizable provider effect we document underscores the importance of understanding how clinicians respond to information and incentives in obstetric care. In addition, our results indicate that hospital quality and resources are important considerations for C-section policy design, and that tailoring interventions based on these factors may enhance their effectiveness.

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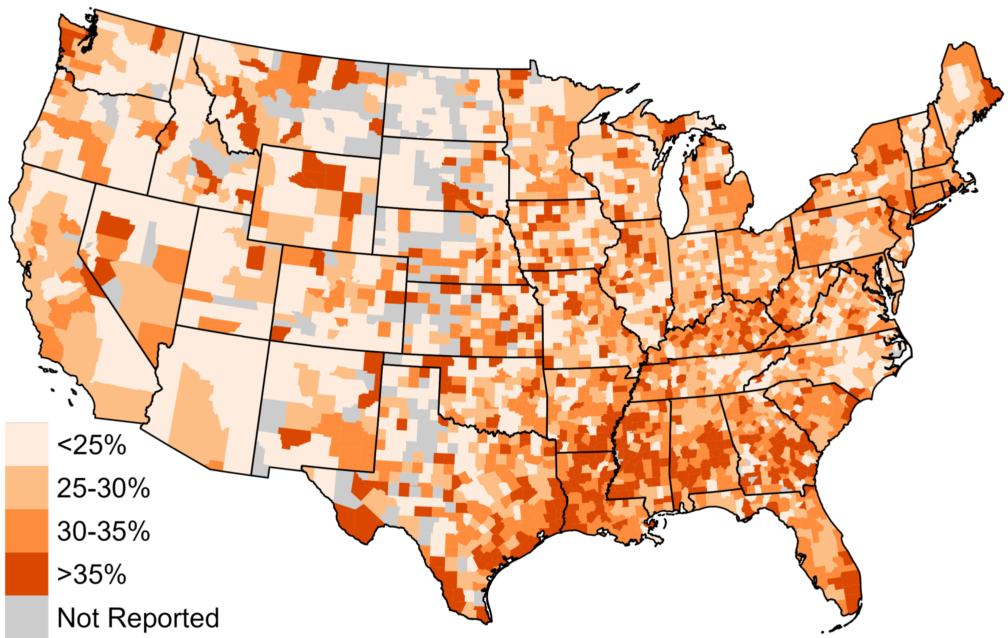
# Figures and Tables

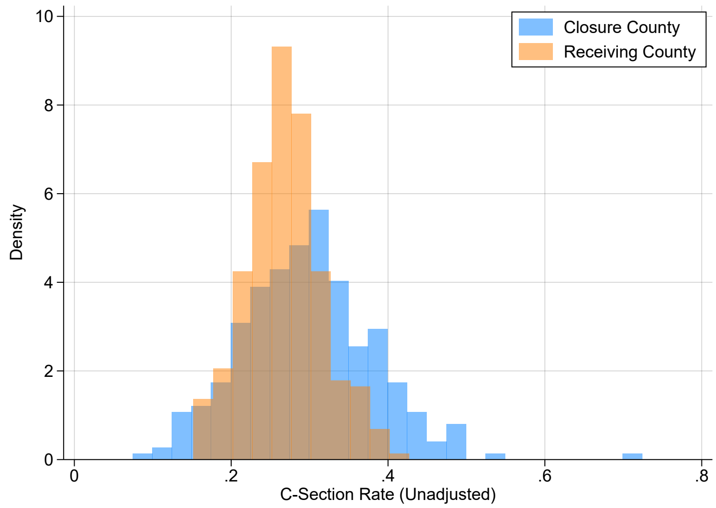
Figure 1: C-Section Rates, 2019 (Map)



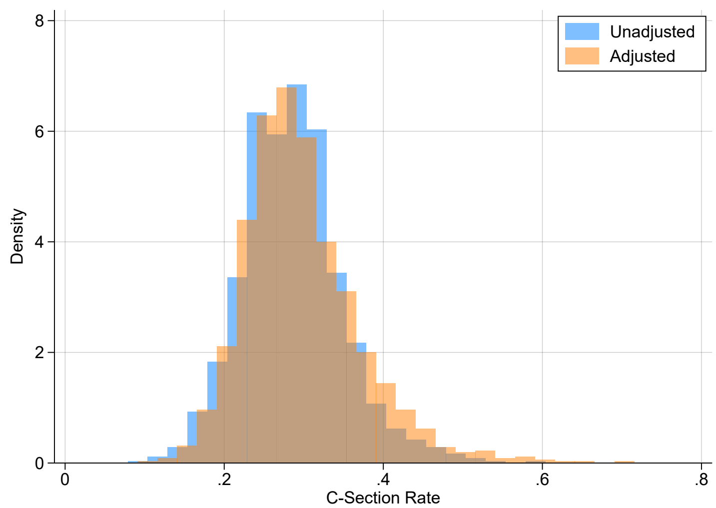

Notes: C-section rates in 2019 are reported by mother's county of residence. Counties with fewer than 10 births are not reported. The source is the National Vital Statistics System Natality File.

Figure 2: Variation in C-Section Rates

(A) C-Section Rates: Closure/Receiving Counties

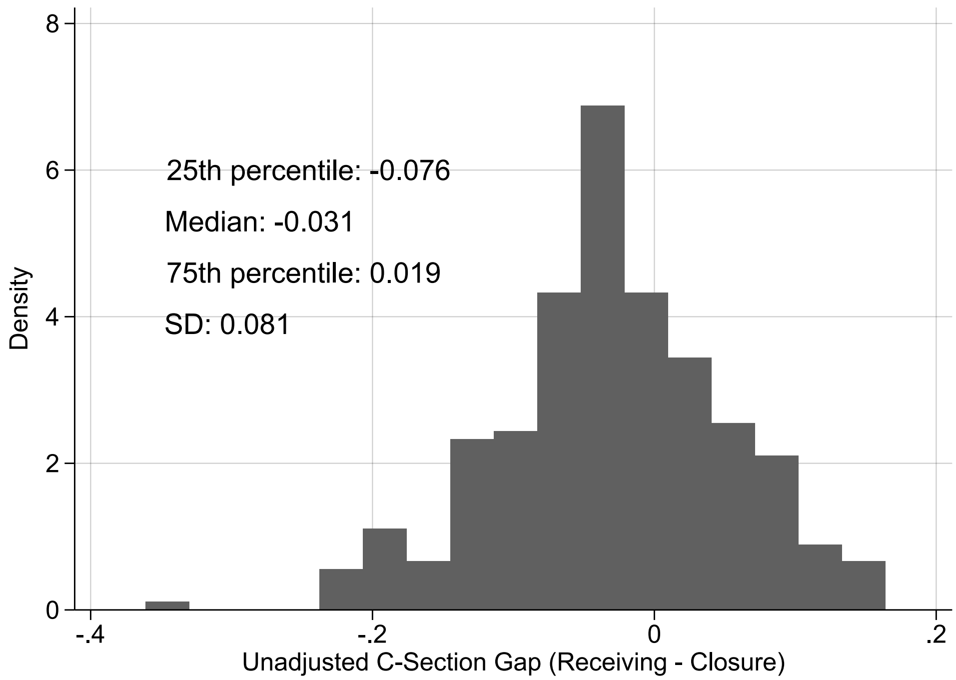



(B) C-Section Rates: All Counties



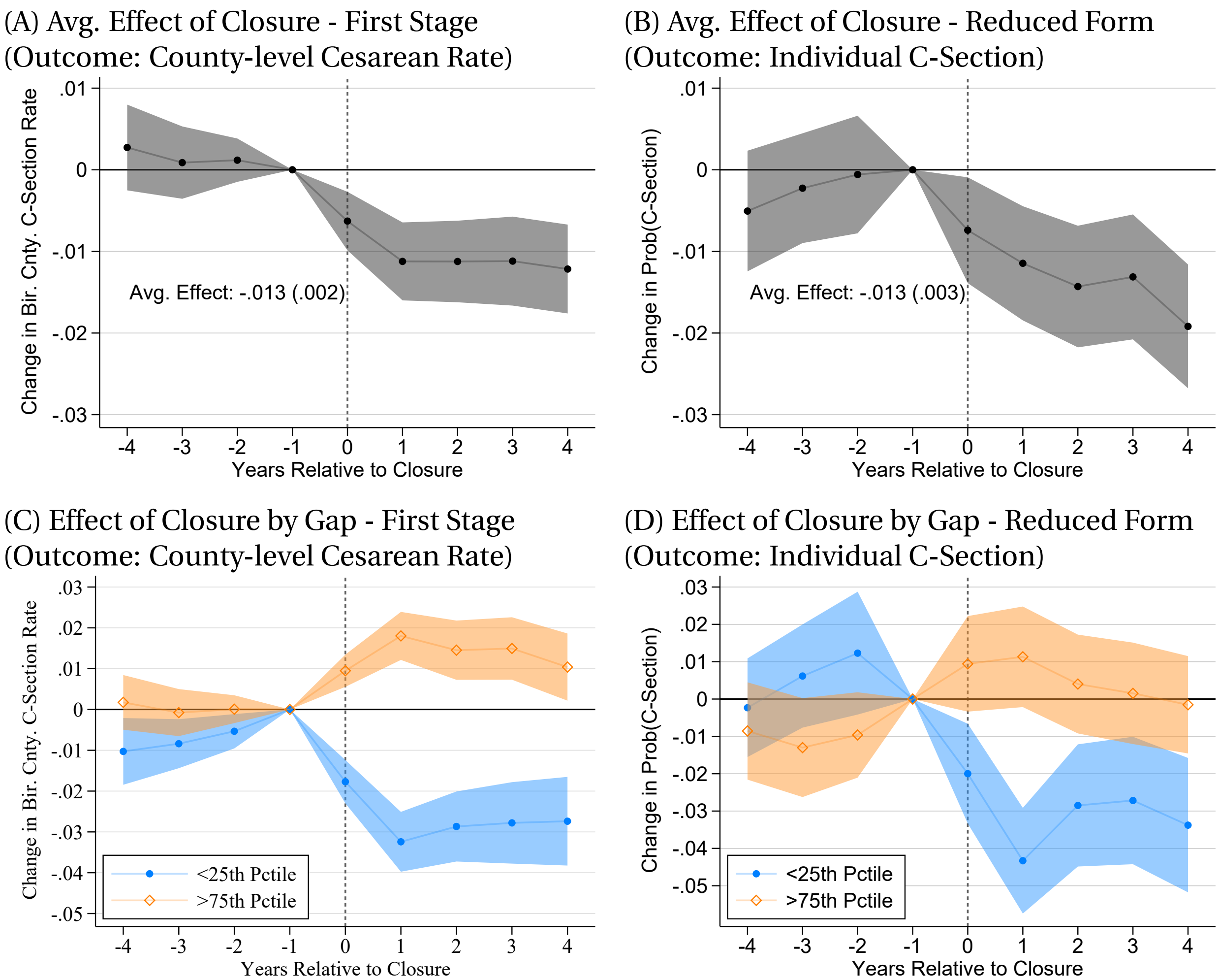

Notes: The sample is first-birth singleton births for both panels. Panel A plots C-section rates separately for counties that experienced a loss of all hospital-based obstetric units (i.e., closure counties) and for neighboring counties that subsequently received patients from these closure counties (i.e., receiving counties). Rates are measured over the three years prior to the closure year. Receiving counties are defined as those that had any pre-closure market share among mothers residing in the closure county, and their rates are averaged across receiving counties, weighted by their pre-closure market shares. In Panel B, county-level risk-unadjusted (crude) and risk-adjusted C-section rates in 2019 are plotted. We exclude counties that experienced fewer than 10 births within their borders in 2019. Risk adjustment is performed using inverse probability weighting, where the probability of undergoing a C-section is predicted based on maternal age, breech birth, eclampsia, chronic hypertension, pregnancy-related hypertension, pregnancy-related diabetes, and gestational age. The weighted rates are then standardized by multiplying by the average C-section rate observed in the full sample.

Figure 3: C-Section Gaps



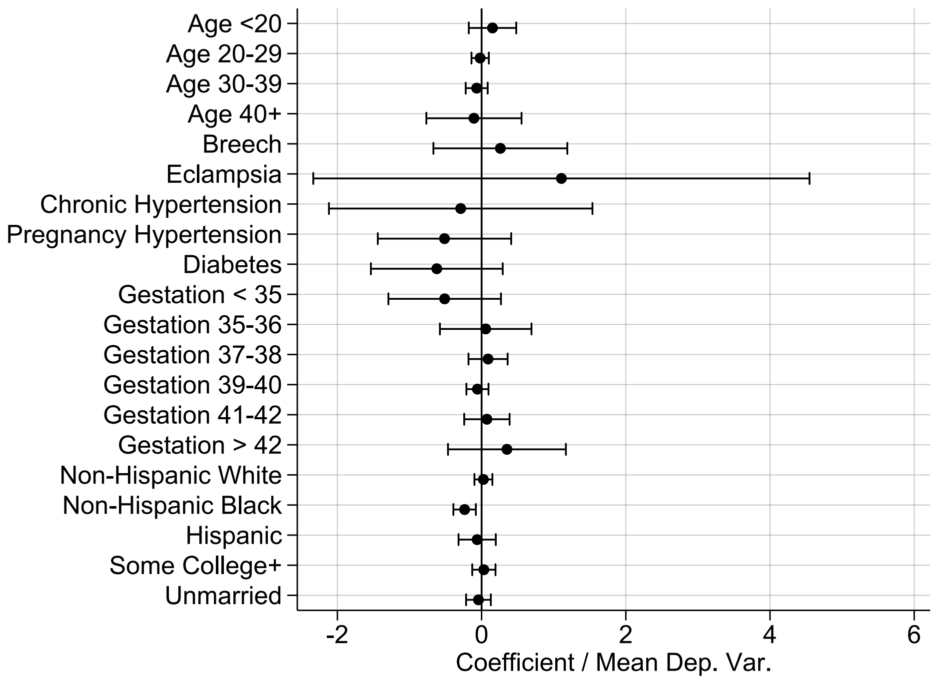

Notes: C-Section gap ($Gap_r$) between closure and receiving counties, which is the difference of $\mathrm{R}_{Closure,h(r)-3\leq y<h(r)}$ and $\mathrm{R}_{Receiving,h(r)-3\leq y<h(r)}$, is plotted. $\mathrm{R}_{Closure,h(r)-3\leq y<h(r)}$ represents the C-section rate in the county experiencing the closure in the three years prior to the closure year, $h(r)$. $\mathrm{R}_{Receiving,h(r)-3\leq y<h(r)}$ is the equivalent measure for the "receiving" counties. Receiving counties are defined as those with any pre-closure market share among mothers residing in the closure county, and $\mathrm{R}_{Receiving,h(r)-3\leq y<h(r)}$ is calculated as an average across all receiving counties, weighted by their pre-closure market share.

Figure 4: Hospital-Based Obstetric Unit Closure Event Study




Notes: These figures plot estimated $\psi_j$ from Equation (9) using canonical two-way fixed effects estimator. The outcome is C-section rate in county of birth occurrence in Panel (A), whereas the outcome is the probability of C-section in Panel (B). In these subfigures, the average treatment effects (standard error clustered at the county of residence level in parentheses) are displayed. The average treatment effect estimate represents the estimate of $\phi$ from Equation (4). Panel (C) and (D) leverage heterogeneity in C-section gaps: treated counties in the first (blue) and fourth (orange) quartile of the $\text{Gap}_r$ are used. Circles represent the point estimates of dynamic treatment effect and the shade represents their 95% confidence intervals. All the regressions include controls for county of residence fixed effects, urban group-by-year fixed effects, county-level time varying covariates (female population shares for 5-year age bands, per-capita personal income, per-capita governmental transfers, and the employment-population ratio).

Figure 5: Disaggregated Balance Tests




Notes: We estimate Equation (7) but replace the outcome with each predictor used in the balance test (in the third column of Table 2). For comparability across outcomes, all coefficient estimates in interest ($\widehat{\phi_1}$) are divided by the mean of the dependent variable. Circles represent the point estimate and spikes are their 95% confidence interval. All the regressions include residential county fixed effects, urban group-by-year fixed effects, and county-level time varying covariates (female population shares for 5-year age bands, per-capita personal income, per-capita governmental transfers, and the employment-population ratio). Standard errors are clustered at the county of residence.

Table 1: First-Birth Summary Statistics

| | All Counties in Our Main Sample | Closure Counties | Non-Closure Counties Unweighted | Non-Closure Counties P-Weighted |
|---|---|---|---|---|
| Panel A: C-Sections | | | | |
| C-Section Delivery Rate | 0.2814 | 0.2859 | 0.2812 | 0.2776 |
| C-Section Rate in Birth County | 0.2683 | 0.2706 | 0.2682 | 0.2611 |
| Panel B: Risk Factors | | | | |
| Singleton | 0.9816 | 0.9843 | 0.9815 | 0.9848 |
| Breech | 0.0485 | 0.0506 | 0.0485 | 0.0501 |
| Eclampsia | 0.0038 | 0.0050 | 0.0037 | 0.0051 |
| Chronic Hypertension | 0.0098 | 0.0130 | 0.0097 | 0.0117 |
| Pregnancy Hypertension | 0.0562 | 0.0756 | 0.0555 | 0.0749 |
| Diabetes | 0.0373 | 0.0382 | 0.0373 | 0.0374 |
| Premature | 0.1092 | 0.1174 | 0.1088 | 0.1139 |
| Panel C: Demographics | | | | |
| Non-Hispanic White | 0.5615 | 0.7962 | 0.5521 | 0.7586 |
| Non-Hispanic Black | 0.2115 | 0.0765 | 0.2168 | 0.1090 |
| Hispanic | 0.1389 | 0.0983 | 0.1405 | 0.0867 |
| Age <20 | 0.1914 | 0.2634 | 0.1886 | 0.2583 |
| Age 20-29 | 0.5411 | 0.5928 | 0.5390 | 0.5903 |
| Age 30-39 | 0.2529 | 0.1375 | 0.2575 | 0.1446 |
| Age 40+ | 0.0146 | 0.0063 | 0.0150 | 0.0068 |
| Some College+ | 0.5420 | 0.4388 | 0.5462 | 0.4534 |
| Unmarried | 0.4188 | 0.4708 | 0.4167 | 0.4734 |
| Panel D: County Characteristics | | | | |
| Fertility Rate | 67.9852 | 65.5381 | 68.0826 | 67.9465 |
| Population | 1,446,651 | 48,152 | 1,502,315 | 73,455 |
| Employment/Pop | 0.5775 | 0.4392 | 0.5830 | 0.5024 |
| Rural | 0.1800 | 0.6343 | 0.1619 | 0.7571 |
| Female 15-44 Share | 0.4185 | 0.3798 | 0.4201 | 0.3790 |
| # Counties | 1,421 | 326 | 1,095 | 1,095 |
| N | 30,600,678 | 1,171,360 | 29,429,318 | 29,429,318 |

Notes: For Panels A-C, the sample is first births. We exclude counties that experienced an OB unit opening (149), counties receiving >30% of deliveries from closure counties pre-closure (348), counties that never had an OB unit (886), and closure counties that did not offer C-sections prior to closure (154) (as in our main specification). The fourth column ("Non-Closure P-Weighted") weights by the propensity to experience a closure. Specifically, this propensity is estimated via a probit regression using county-level fertility rates, employment to population rate, earnings per-capita, transfers per-capita, female population share aged 15-19, female population share aged 20-24, female population share aged 25-29, female population share aged 30-34, female population share aged 35-39, and female population share aged 40-44, total population, population density, and percent urban measured in the first year of the sample —1989. Weighting forces similarity between treated and untreated counties. It ensures, for example, that the comparison group for the largely rural treated counties is also largely rural. Rural counties are those classified as non-core or micropolitan in the 2013 NCHS urban/rural classification.

Table 2: Instrument Relevance, Reduced-Form, and Identifying Assumptions

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dep. Var. | C-section Rate in Birth Cnty. (First Stage) | Individual C-section (Reduced Form) | Predicted C-Section (Balance Test) | Fallback Residual (Fallback Test) |
| $\text{Closed}_{rt} \times \text{Gap}_r$ | 0.252$^{***}$ | 0.265$^{***}$ | -0.00592 | -0.00533 |
| | (0.0226) | (0.0343) | (0.0134) | (0.00853) |
| F Statistic | 124.3 | | | |
| N | 29,024,851 | 29,024,851 | 29,024,851 | 23,580,498 |

Notes: This table reports regression coefficients and standard errors for 4 separate regressions used to assess the validity of the identifying assumptions. All estimates use singleton first births as the sample. County-level C-section rate is risk-unadjusted (i.e., crude C-section rate). Standard errors clustered at the county of residence level. Column 1 (Column 2) provides the first stage (reduced form) regression in which the outcome is county-level C-section rate (individual-level indicator of C-section birth). Column 3 is an indirect test of the exclusion restriction, and the outcome is a predicted C-section probability from a regression on the following set of balance variables: fertility rate, marital status, race indicators, birth order indicators, age bands, and risk factors. Column 4 represents a test for the "fallback condition" described in Abaluck et al. (2021). All the regressions include controls for county of residence fixed effects, urban group-by-year fixed effects, county-level time varying covariates (female population shares for 5-year age bands, per-capita personal income, per-capita governmental transfers, and the employment-population ratio). $^{***}$, $^{**}$, $^{*}$, $^{+}$ indicate significance at the 0.1%, 1%, 5%, and 10% levels.

Table 3: IV Estimates with Varying Time-Window

| Dep. Var. | Individual C-Section | | | | |
|---|---|---|---|---|---|
| | (1) 4-Year | (2) 6-Year | (3) 8-Year | (4) 10-Year | (5) Unrestricted |
| County C-Section Rate | 1.052$^{***}$ | 1.118$^{***}$ | 1.087$^{***}$ | 1.058$^{***}$ | 1.086$^{***}$ |
| | (0.132) | (0.120) | (0.124) | (0.125) | (0.142) |
| First-Stage F | 124.3 | 89.32 | 66.11 | 44.30 | 28.19 |
| N | 29,024,851 | 29,090,584 | 29,153,133 | 29,214,107 | 29,947,411 |

Notes: This table reports IV regression coefficients and standard errors from five separate regressions estimating the relationship between county-level C-section rates and an individual's probability of receiving a C-section, each using a different time window around the OB unit closures. All estimates use singleton first births as the sample unless stated otherwise. County-level C-section rate is risk-unadjusted (i.e., crude C-section rate). Standard errors are clustered at the county of residence level. IV estimates with varying time windows are reported. The preferred estimate "4-Year" includes four years of data (2 years prior to the closure and 2 years after the closure, excluding the year when the closure happened) for treated counties. The rest of the columns present estimates from models using extended sample windows for treated counties. All the regressions include controls for county of residence fixed effects, urban group-by-year fixed effects, county-level time varying covariates (female population shares for 5-year age bands, per-capita personal income, per-capita governmental transfers, and the employment-population ratio). $^{***}$, $^{**}$, $^{*}$, $^{+}$ indicate significance at the 0.1%, 1%, 5%, and 10% levels.

Table 4: Robustness of IV Estimates

| Dep. Var. | Individual C-Section Prob. | | | | |
|---|---|---|---|---|---|
| | (1) Interaction Instrument | (2) P-Weighted | (3) Median Instrument | (4) $R_{Closure}$ Instrument | (5) Distance Controls |
| County C-Section Rate | 1.027*** | 1.046*** | 1.159*** | 1.198*** | 1.025*** |
| | (0.119) | (0.127) | (0.173) | (0.194) | (0.120) |
| First-Stage F | 199.0 | 124.0 | 66.12 | 79.58 | 100.0 |
| N | 29,024,851 | 23,648,779 | 29,024,851 | 29,028,267 | 29,024,851 |

Notes: This table reports IV coefficients and standard errors from five separate regressions of individual C-section probability on county-level C-section rates, each corresponding to a different robustness specification. All estimates use singleton first births as the sample unless stated otherwise. County-level C-section rate is risk-unadjusted (i.e., crude C-section rate). Standard errors clustered at the county of residence level. We use $Z_{rt} \times s_r$ (where $s_r$ is a fraction of mothers giving birth in county of residence within 3 years prior to the closure) as the instrument in Column 1. In Column 2, the sample from non-closure counties is weighted by the propensity to experience a closure. To implement this, we predict the probability of ever experiencing a closure in a cross-sectional county-level logistic regression based on a set of county-level characteristics observed in the first year of the sample, 1989 (i.e., fertility rate, employment-to-population ratio, per-capita earnings, per-capita government transfers, female population shares by age group (15–44, divided into five-year bins), total population, population density, and the percentage of the population living in urban areas). We then weight the untreated counties by $\frac{\widehat{p}}{1-\widehat{p}}$, where $p$ is the predicted probability of experiencing a closure from the logit (treated observations receive weight equal to one). In Column (3), we replace $Gap_r$ in Equation (6) with $1\{Gap_r > \text{Median}\}$. We use only the pre-closure C-section rate in closure counties as IV instead of the gap between closure and receiving counties in Column 4 (closer specification to Abaluck et al. (2021)). Finally, we add distance to the nearest hospital-based obstetrics unit and its square as a control in Column (5). All the regressions include controls for county of residence fixed effects, urban group-by-year fixed effects, county-level time varying covariates (female population shares for 5-year age bands, per-capita personal income, per-capita governmental transfers, and the employment-population ratio). ***, **, *, + indicate significance at the 0.1%, 1%, 5%, and 10% levels.

Table 5: Heterogeneity of IV Estimates

| Dep. Var. | Individual C-Section | | | | |
|---|---|---|---|---|---|
| | (1) Low Need | (2) High Need | (3) 2nd+ Birth | (4) Cnty. Occurrence HHI $< 10,000$ | (5) Cnty. Occurrence HHI $= 10,000$ |
| County C-Section Rate | 1.146$^{***}$ | 0.663$^{+}$ | 0.423$^{**}$ | 0.890$^{***}$ | 1.244$^{***}$ |
| | (0.143) | (0.386) | (0.147) | (0.179) | (0.197) |
| First-Stage F | 117.2 | 80.19 | 114.4 | 46.77 | 39.43 |
| N | 21,809,058 | 7,215,793 | 41,535,922 | 23,026,076 | 3,781,049 |

Notes: This table reports IV coefficients and standard errors from five separate regressions of an individual's probability of receiving a C-section on county-level C-section rates, with each regression estimated on a different subsample. All estimates use singleton first births as the sample unless stated otherwise. County-level C-section rate is risk-unadjusted (i.e., crude C-section rate). Standard errors clustered at the county of residence level. In Columns 1 and 2, the sample is split based on the propensity to receive a C-section predicted by the set of maternal risk factors and demographic variables. The sample is classified as Low-Need if a mother's predicted probability of receiving a C-section falls below the 25th percentile; otherwise, the mother is classified as High-Need. The sample in Column 3 is 2nd or higher order singleton births instead of the first-births. Columns 4 and 5 are heterogeneity analyses based on the obstetric hospital's market competitiveness in a receiving county: we compare the cases where (1) mothers have at least two choices within the county (i.e., oligopoly; column 4) and (2) mothers have virtually only one choice (i.e., monopoly; column 5). All the regressions include controls for county of residence fixed effects, urban group-by-year fixed effects, county-level time varying covariates (female population shares for 5-year age bands, per-capita personal income, per-capita governmental transfers, and the employment-population ratio). $^{***}$, $^{**}$, $^{*}$, $^{+}$ indicate significance at the 0.1%, 1%, 5%, and 10% levels.

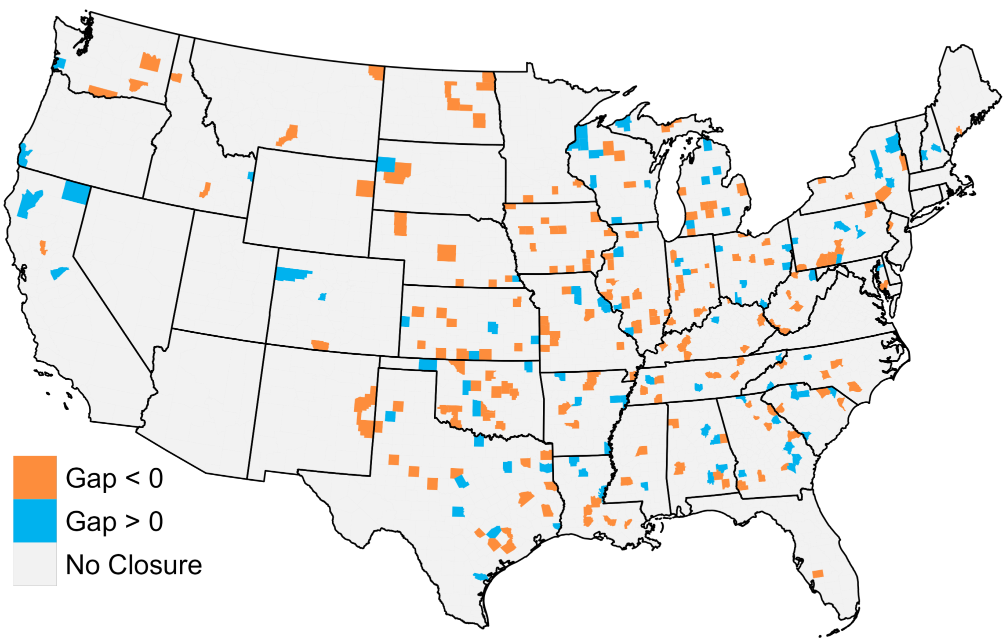
Table 6: Hospital Characteristics

| | (1) Correlation | (2) Hospital Characteristics IV Regression | | | | | |
|---|---|---|---|---|---|---|---|
| Dep. Var. | C-section Rate in Birth Cnty. | Individual C-section | | | | | |
| | | Profit Motive | | Quality & Resources | | Labor & Capacity | |
| For Profit | 0.167 | 0.0224<br>(0.0244) | | | | | |
| Full Integration Salary Model | 0.027 | | -0.0107<br>(0.0290) | | | | |
| Med-School Affiliation | 0.028 | | | -0.0834*<br>(0.0336) | | | |
| NICU Hospital | 0.068 | | | | -0.0728*<br>(0.0361) | | |
| Share of Birth Attended by Midwives | -0.186 | | | | | -0.0851<br>(0.109) | |
| (Birth/Bassinets)*100 | 0.001 | | | | | | -0.0581<br>(0.0394) |
| First-Stage F | | 218.5 | 88.5 | 74.5 | 172.2 | 30.0 | 64.7 |
| N | | 23,685,260 | 23,685,260 | 23,685,260 | 23,572,655 | 29,017,139 | 23,679,092 |

Notes: Column (1) reports the raw cross-sectional correlation between county-level hospital characteristics and county-level C-section rates. The remaining columns present IV estimates from regressions where the dependent variable is a binary indicator for whether an individual had a C-section, and the main independent variable is the hospital characteristic listed in the corresponding row. All hospital characteristics, except for the share of births attended by midwives, are sourced from the AHA Annual Survey. The midwife share is constructed using the NVSS Natality files, following the same three-year averaging approach used for the C-section rate. To increase statistical power, we scale the instrument by the share of mothers who delivered in their county of residence during the three years prior to the closure. Formally, the instrument is $Z_{rt} \times s_r$, where $Z_{rt}$ is the pre-closure gap in the hospital characteristic and $s_r$ is the in-county delivery share. We do not apply empirical Bayes shrinkage because many closure counties have only one hospital, making shrinkage-based standard errors infeasible. Hospital characteristics are averaged over the three years preceding the birth. All regressions include county-of-residence fixed effects, urban group-by-year fixed effects, and county-level time-varying covariates (female population shares in 5-year age bands, per-capita personal income, per-capita government transfers, and the employment-population ratio). Standard errors are clustered at the county of residence level. $^{***}$, $^{**}$, $^{*}$, $^{+}$ indicate significance at the 0.1%, 1%, 5%, and 10% levels.

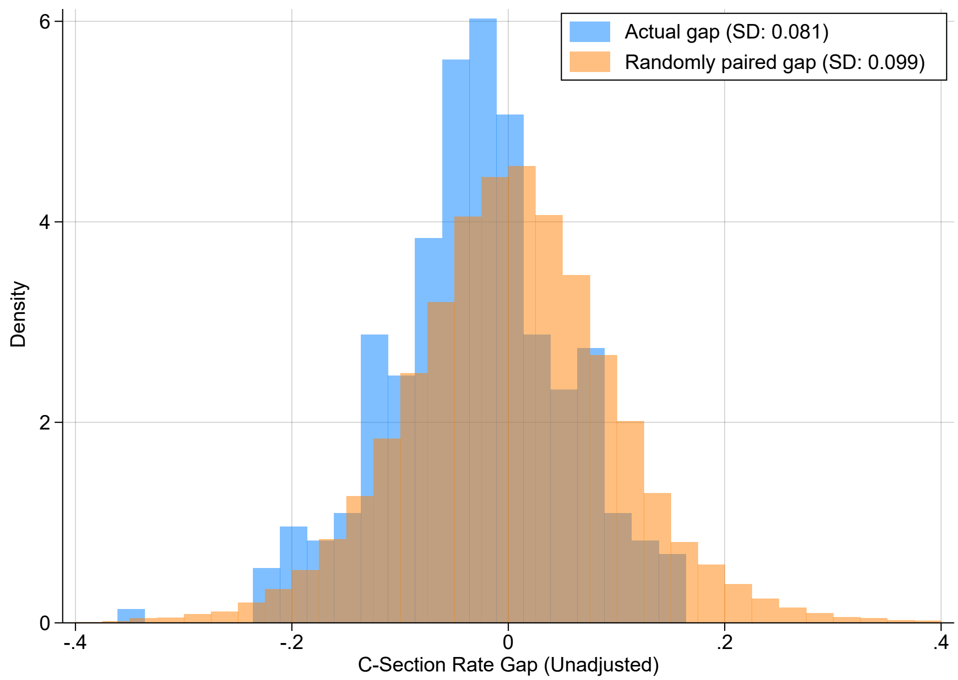
# A Supplemental Figures and Tables

Figure A1: Map of C-Section Gaps & Closures



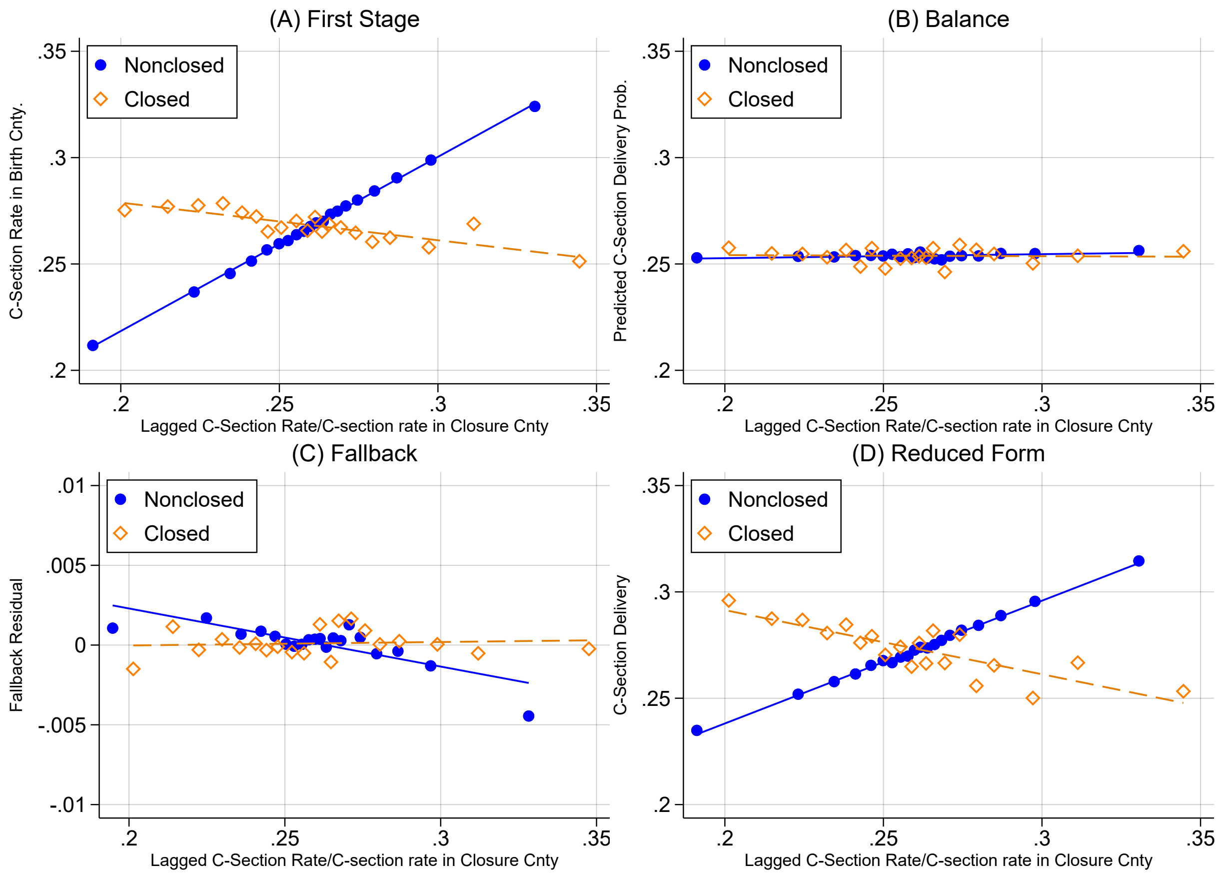

Notes: The map displays the C-section gaps for closure counties. See Section 4.2 for details about the construction of the C-section gap. Closure counties with negative (positive) C-section gap are colored in orange (blue).

Figure A2: Distributions of Actual and Randomly Paired C-Section Gaps



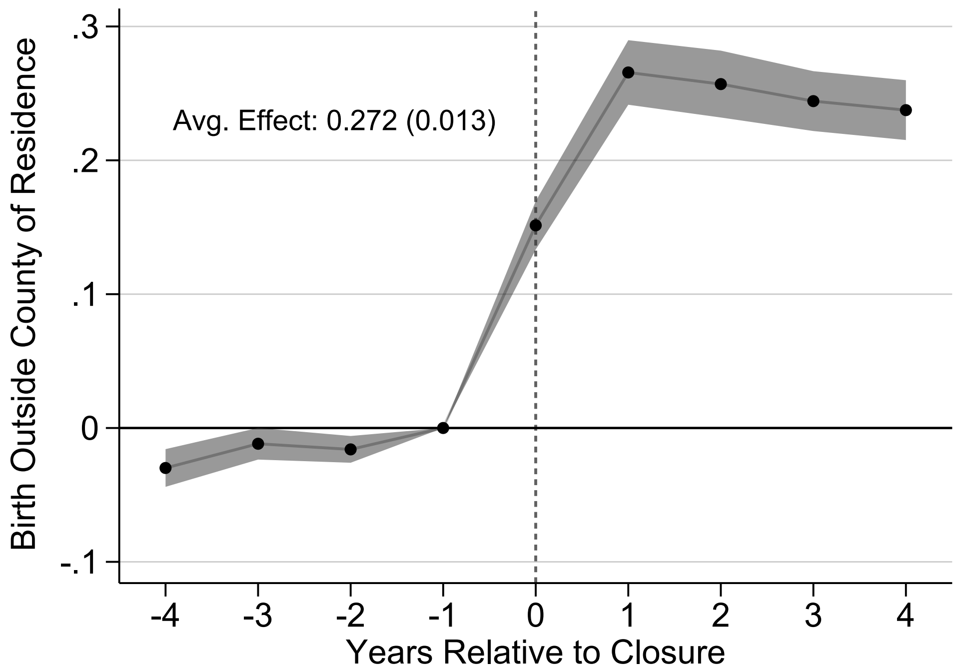

Notes: The blue-colored plot shows the actual distribution of C-section rate gaps between closure and receiving counties (as in Figure 3). The orange-colored plot shows the distribution of C-section rate gaps from randomly-paired counties. To construct this counterfactual, we calculate the average C-section rate for each county-year over the three years prior, and then randomly pair counties within each year (without replacement) to compute the gap between each pair.

Figure A3: Binned Scatter Plots for Graphical Representation of First Stage, Balance Test, Fallback Condition, and Reduced Form



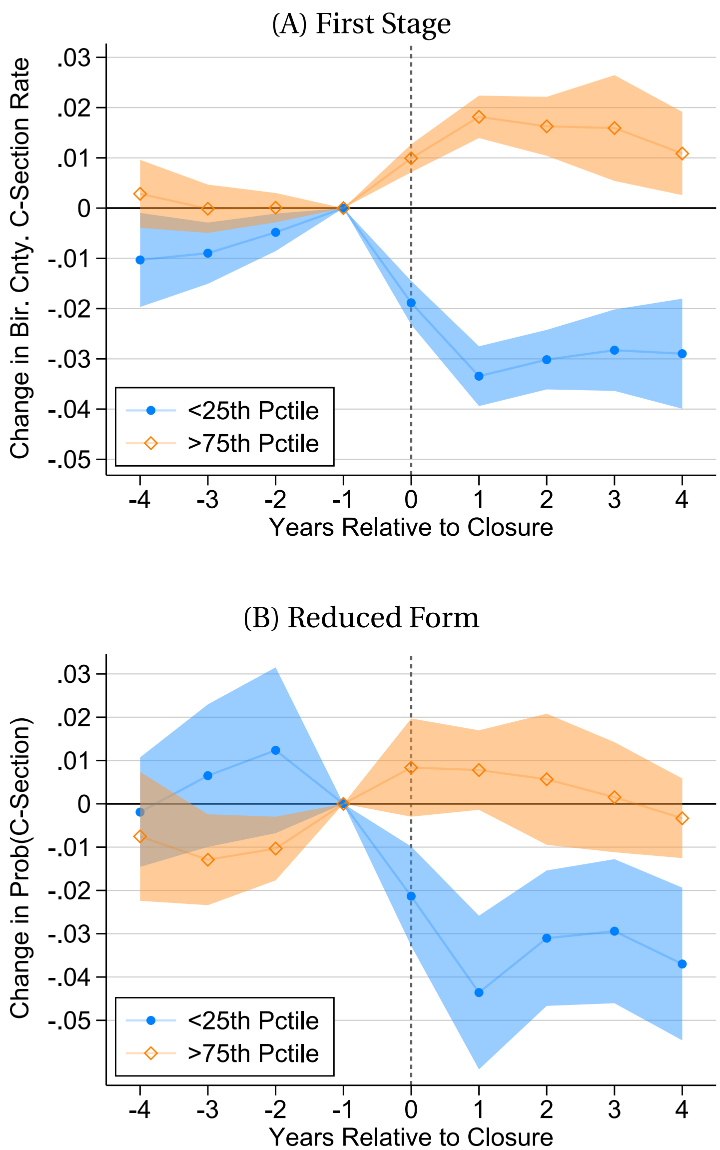

Notes: This figure illustrates the three assumptions underlying the complementary IV approach using $Z = \text{Closed}_{rt} * \text{Pre-Closure C-section Rate}_r$ (i.e., strong first stage, exclusion restriction, and the fallback condition) and the reduced form using binned scatter plots. In all figures, the X-axis represents the lagged C-section rate: for non-closure counties, this is the rate in the previous year; for closure counties, it is the rate measured in the three years prior to the closure. We control for county-level time varying covariates (female population shares for 5-year age bands, per-capita personal income, per-capita governmental transfers, and the employment-population ratio), county of residence fixed effects, and urban group-by-year fixed effects in all plots. [Panel A] The Y-axis represents county-level C-section rate. [Panel B] The Y-axis represents C-section rate predicted by demographic variables and risk factors (See Section 5 "Exogeneity" for the choice of variables). [Panel C] The Y-axis represents the fallback residual (See Section 5 "Fallback Condition" for the construction of this measure). [Panel D] The Y-axis is the indicator of whether a mother actually receives a C-section.

Figure A4: Effect of Closure on Probability of Birth Outside County of Residence



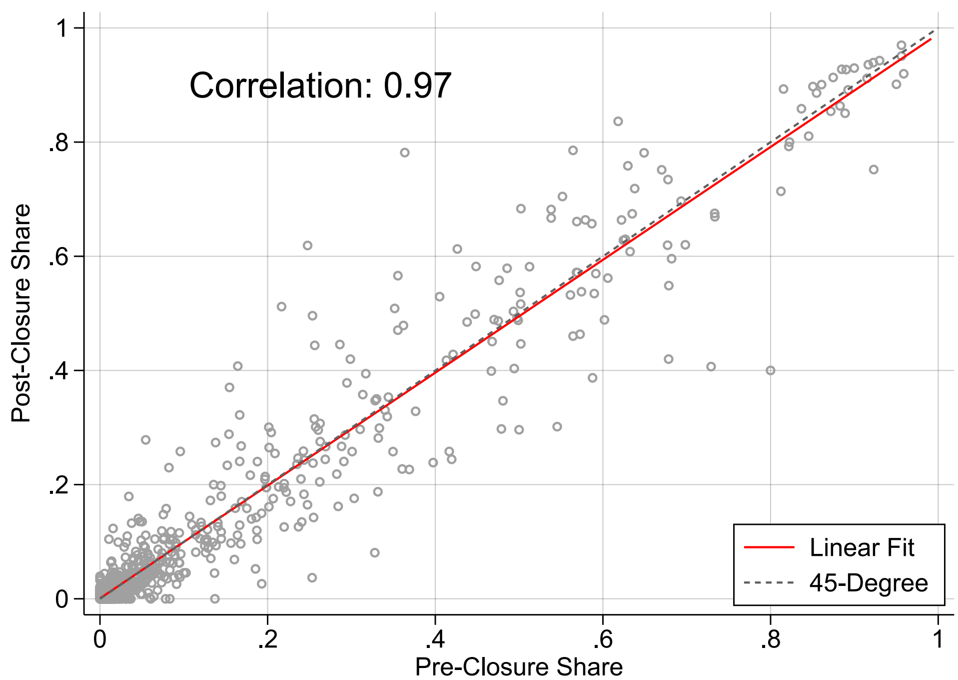

Notes: This event study plots estimated $\psi_j$ from Equation (9), but the outcome is replaced with the indicator of birth occurrence outside county of residence. Black circles represent the point estimates of dynamic treatment effect and the shade represents their 95% confidence intervals. It also displays the average treatment effect (standard error clustered at the county of residence in parentheses).

Figure A5: Effects of Closures by Gap: de Chaisemartin and D'Haultfoeuille (2020) Estimator



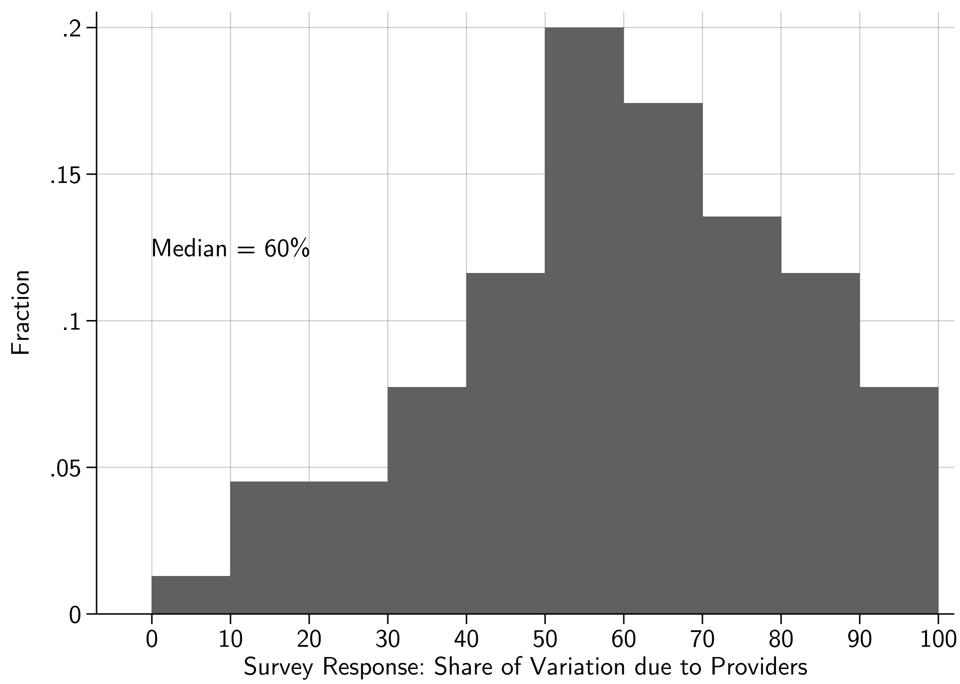

Notes: These figures plot estimated $\psi_j$ from Equation (9) using the de Chaisemartin and D'Haultfoeuille (2020) estimator. The outcome is C-section rate in county of birth occurrence in Panel (A), whereas the outcome is probability of C-section in Panel (B). In each subfigure, treated counties in the first (blue) and fourth (orange) quartile of the $\text{Gap}_r$ are used. Circles represent the point estimates of dynamic treatment effect and the shade represents their 95% confidence intervals. All the regressions include controls for county of residence fixed effects, urban group-by-year fixed effects, county-level time varying covariates (female population shares for 5-year age bands, per-capita personal income, per-capita governmental transfers, and the employment-population ratio).

Figure A6: Correlation between Pre- and Post-Closure Receiving County Shares (Indirect Test of Fallback Condition)




Notes: The figure plots the share of births occurring in each receiving county for a given closure county, measured three years before (pre-closure share on the x-axis) and three years after (post-closure share on the y-axis) the closure event. Pre-closure shares are calculated conditional on mothers giving birth outside the closure county. The red line shows the linear fit between pre- and post-closure shares, while the dotted 45-degree line connects (0,0) and (1,1) for reference. For visual clarity, we display a 20% random sample of all closure–receiving county pairs.

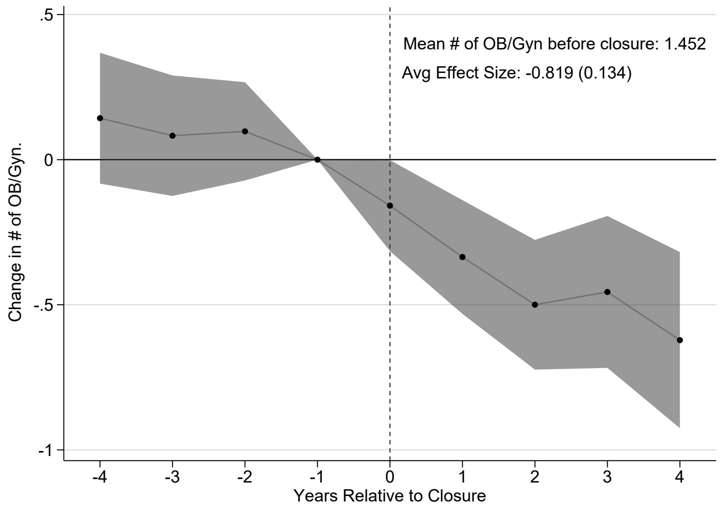
Figure A7: Experts' Prediction of the Fraction of Variation Due to Providers



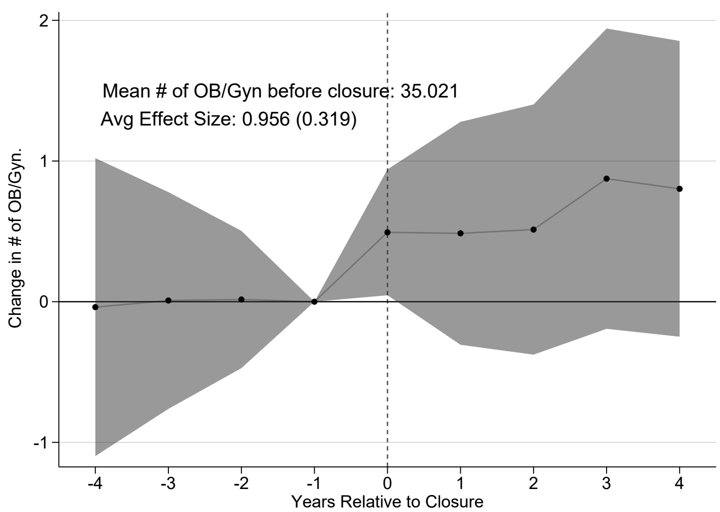

Notes: This histogram shows the distribution of responses to a survey question asking how much of the geographic variation in C-section rates is attributable to providers. Responses are based on predictions from 225 experts, including practicing obstetricians, obstetrics researchers, and health economists. The survey is provided in Appendix B.

Figure A8: Changes in Numbers of OB/GYN Facilities in Closure and Receiving Counties

(A) Closure counties




(B) Receiving counties




Notes: These figures plot estimated $\psi_j$ from Equation (9) for the number of OB/GYN in county, sourced from Health Resources and Services Administration (HRSA). Panel A (Panel B) shows the results of event study specification using closure counties (receiving counties) and control counties. Circles represent the point estimates of dynamic treatment effect and the shade represents their 95% confidence intervals. All the regressions include controls for county fixed effects, urban group-by-year fixed effects, county-level time varying covariates (female population shares for 5-year age bands, per-capita personal income, per-capita governmental transfers, and the employment-population ratio). Standard errors are clustered at county level. Because HRSA data are available since 1995, closure counties in which the closure event happened prior to 1995 and their receiving counties are not included in this analysis.

Table A1: Assumptions and IV Estimates (Risk-Adjusted)

| **Panel A**: Assumptions | (1) C-section Rate in Birth Cnty. (First Stage) | (2) Individual C-section (Reduced Form) | (3) Predicted C-Section (Balance Test) | (4) Fallback Residual (Fallback Test) | |
|---|---|---|---|---|---|
| Closed × Gap | 0.288*** | 0.228*** | -0.093 | -0.015 | |
| | (0.054) | (0.043) | (0.071) | (0.012) | |
| First-Stage F | 28.6 | | | | |
| N | 29,028,188 | 29,028,359 | 29,028,359 | 23,583,823 | |
| **Panel B**: IV Estimate with Varying Time-Window | (1) 4-Year | (2) 6-Year | (3) 8-Year | (4) 10-Year | (5) Unrestricted |
| County C-Section Rate | 0.793*** | 0.827*** | 0.770*** | 0.763** | 0.971* |
| | (0.180) | (0.191) | (0.202) | (0.232) | (0.478) |
| N | 29,028,188 | 29,095,583 | 29,159,791 | 29,222,280 | 29,969,594 |
| **Panel C**: Robustness | Interaction IV | P-Weighted | Median Instrument | $R_{Closure}$ Instrument | Distance Controls |
| County C-Section Rate | 0.919*** | 0.788** | 0.812*** | 0.736*** | 0.791*** |
| | (0.174) | (0.176) | (0.197) | (0.221) | (0.178) |
| N | 29,028,188 | 23,652,116 | 29,028,188 | 29,028,188 | 29,028,188 |
| **Panel D**: Heterogeneity | Low Need | High Need | 2nd+ Birth | HHI $< 10,000$ | HHI $= 10,000$ (Monopolized) |
| County C-Section Rate | 0.965*** | 0.662 | 0.404* | 0.615*** | 0.882** |
| | (0.235) | (0.569) | (0.189) | (0.162) | (0.333) |
| N | 21,907,585 | 7,665,990 | 41,555,814 | 23,500,836 | 3,829,186 |

Notes: This table presents regression estimates and standard errors from several analyses. Panel A reports tests of identifying assumptions. Column (1) presents the first stage, where the outcome is the county-level C-section rate. Column (2) shows the reduced form, where the outcome is an indicator for whether an individual had a C-section. Column (3) provides an indirect test of the exclusion restriction, using as the outcome a predicted C-section indicator based on a regression of balance variables (marital status, race indicators, birth order, age bands, and risk factors). Column (4) tests the "fallback condition" as described in Abaluck et al. (2021). Panel B reports IV estimates using different time windows, with the individual likelihood of C-section as the dependent variable and the county-level C-section rate as the main independent variable. Panel C presents robustness checks of the IV estimates. Column (1) uses $Z_{rt} \times s_r$ (where $s_r$ is the share of mothers giving birth in their county of residence in the three years prior to the closure) as the instrument. Column (2) weights observations by the propensity to experience a closure. Column (3) replaces $Gap_r$ in Equation (6) with an indicator for whether the gap exceeds the median. Column (4) uses the pre-closure C-section rate in closure counties as the instrument, rather than the gap. Column (5) adds controls for distance to the nearest hospital-based obstetrics unit and its square. Panel D presents heterogeneity analyses for the IV estimates. Columns (1) and (2) split the sample by predicted C-section likelihood, based on maternal risk factors and demographics. Individuals with $Pr(\widehat{\text{C-section}}) < 0.25$ are classified as Low-Need; all others are classified as High-Need. Column (3) restricts the sample to second or higher-order singleton births. Columns (4) and (5) explore heterogeneity by hospital market competitiveness in the receiving county: Column (4) includes mothers with at least two hospital options; Column (5) includes those with effectively only one. All regressions control for county-of-residence fixed effects, urban group-by-year fixed effects, and county-level time-varying covariates: female population shares in 5-year age bands, per-capita personal income, per-capita government transfers, and the employment-population ratio. County-level C-section rates are risk-adjusted using inverse probability weighting. The probability of a C-section is predicted using maternal age, breech presentation, eclampsia, chronic hypertension, pregnancy-related hypertension, pregnancy-related diabetes, and gestational age. Weighted rates are then standardized by multiplying by the average C-section rate in the full sample. Standard errors are clustered at the county of residence level. ***, **, *, + indicate significance at the 0.1%, 1%, 5%, and 10% levels, respectively.

Table A2: Results without Empirical Bayesian Shrinkage

| Dep. Var. | Individual C-Section | | | | |
|---|---|---|---|---|---|
| | (1) 4-Year | (2) 6-Year | (3) 8-Year | (4) 10-Year | (5) Unrestricted |
| County C-Section Rate | 1.174*** | 1.206*** | 1.166*** | 1.126*** | 1.007*** |
| | (0.133) | (0.122) | (0.116) | (0.110) | (0.113) |
| First-Stage F | 159.6 | 117.6 | 92.89 | 66.88 | 52.00 |
| N | 29,045,770 | 29,121,384 | 29,193,663 | 29,263,654 | 30,113,025 |

Notes: This table presents IV estimates and standard errors using a sample that includes all closures, not just those with non-zero C-section rates. The dependent variable is a binary indicator for whether an individual had a C-section, and the main independent variable is the county-level C-section rate. Each column corresponds to a different time window around the closure event. The preferred specification, labeled "4-Year," uses data from two years before and two years after the closure (excluding the year of the closure) for treated counties. The remaining columns report estimates from models using longer time windows. All estimates use singleton first births. County-level C-section rates are unadjusted (i.e., crude rates without risk adjustment). We do not apply a Bayes Empirical shrinkage adjustment to these estimates. Standard errors are clustered at the county of residence level. All regressions include county-of-residence fixed effects, urban group-by-year fixed effects, and county-level time-varying covariates: female population shares by 5-year age bands, per-capita personal income, per-capita government transfers, and the employment-population ratio. ***, **, *, $^{+}$ indicate significance at the 0.1%, 1%, 5%, and 10% levels, respectively.

Table A3: Hospital Characteristics Regression (Gap Instrument)

| Dep. Var. | Individual C-section | | | | | |
|---|---|---|---|---|---|---|
| | Profit Motive | | Quality & Resources | | Labor & Capacity | |
| For Profit | 0.0234 | | | | | |
| | (0.0260) | | | | | |
| Full Integration | | -0.0158 | | | | |
| | | (0.0322) | | | | |
| Med-School Affiliation | | | -0.0902* | | | |
| | | | (0.0408) | | | |
| NICU Hospital | | | | -0.0458 | | |
| | | | | (0.0460) | | |
| Share of Birth Attended by Midwives | | | | | -0.0641 | |
| | | | | | (0.128) | |
| (Birth/Bassinets)*100 | | | | | | -0.00612 |
| | | | | | | (0.0615) |
| First-Stage F | 86.6 | 44.8 | 59.2 | 69.9 | 38.1 | 12.0 |
| N | 23,685,260 | 23,685,260 | 23,685,260 | 23,572,655 | 29,017,139 | 23,679,092 |

Notes: This table presents IV estimates from regressions where the dependent variable is a binary indicator for whether an individual had a C-section, and the main independent variable is the hospital characteristic listed in the corresponding row. Unlike Table 6, the instrument is $Z_{rt}$ rather than $Z_{rt} \times s_r$. All hospital characteristics, except for the share of births attended by midwives, are sourced from the AHA Annual Survey. The midwife share is constructed using the NVSS Natality files, following the same three-year averaging approach used for the C-section rate. We do not apply empirical Bayes shrinkage because many closure counties have only one hospital, making shrinkage-based standard errors infeasible. Hospital characteristics are averaged over the three years preceding the birth. All regressions include county-of-residence fixed effects, urban group-by-year fixed effects, and county-level time-varying covariates (female population shares in 5-year age bands, per-capita personal income, per-capita government transfers, and the employment-population ratio). Standard errors are clustered at the county of residence level. $^{***}$, $^{**}$, $^{*}$, $^{+}$ indicate significance at the 0.1%, 1%, 5%, and 10% levels.

Table A4: IV Estimates: Maternal and Infant Health Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dep. Var. | Mat. Morbidity Revised | Mat. Morbidity Unrevised | Inf. Morbidity Unrevised | Low Birthwt. (< 2,500g) | Preterm (< 37 weeks) | Low APGAR (< 7) | Infant Death |
| County C-Section Rate | -0.239 | 0.629 | 0.0206 | 0.0604 | -0.0740 | -0.103 | -0.00938 |
| | (0.286) | (0.499) | (0.349) | (0.0710) | (0.101) | (0.070) | (0.0240) |
| Mean Dep. Var. | 0.0114 | 0.0398 | 0.0139 | 0.0714 | 0.0994 | 0.0189 | 0.00559 |
| First-Stage F | 63.86 | 27.64 | 66.70 | 124.3 | 124.5 | 118.9 | 97.46 |
| N | 9,971,315 | 14,431,779 | 18,745,808 | 29,004,148 | 28,862,600 | 24,806,628 | 22,991,363 |

Notes: This table reports IV coefficients and standard errors from seven separate regressions of maternal and infant health outcomes on county-level C-section rates. All estimates use singleton first births as the sample. County-level C-section rates are unadjusted (i.e., crude rates). Standard errors are clustered at the county of residence level. All regressions use a 4-year window around the closure (two years before and after, excluding the year of closure), consistent with the main specification reported in Column 1 of Table 3. Columns (1) and (2) report estimates for composite measures of maternal morbidity. Higher values indicate worse maternal health. Because the components of these composite measures differ based on whether a state has adopted the 2003 revision of the birth certificate, we construct two separate versions: "Maternal Morbidity (Unrevised)" (available 1989–2006) includes maternal fever, excessive bleeding, and maternal seizures and is estimated only for state-years using the unrevised birth certificate; "Maternal Morbidity (Revised)" (available 2009–2019) includes maternal transfusion, 3rd–4th degree perineal laceration, ruptured uterus, unplanned hysterectomy, and ICU admission and is estimated only for state-years using the revised birth certificate. "Infant Morbidity (Unrevised)" is a composite outcome that includes meconium staining, birth injury, infant seizures, and ventilator use. As several components were phased out with the 2003 revision, this measure is constructed using only state-years with unrevised birth certificates (last available in 2006 for some states). Higher values indicate worse infant health. The APGAR score measures the infant's condition five minutes after birth and ranges from 0 to 10, with higher scores indicating better health. It is based on skin color, heart rate, reflexes, muscle tone, and breathing effort. All regressions include county-of-residence fixed effects, urban group-by-year fixed effects, and county-level time-varying covariates: female population shares in 5-year age bands, per-capita personal income, per-capita government transfers, and the employment-population ratio. $^{***}$, $^{**}$, $^{*}$, $^{+}$ indicate significance at the 0.1%, 1%, 5%, and 10% levels, respectively.

# B Online Survey



UC SANTA BARBARA

**Version 1**

By checking the "Yes" box below, I understand that my participation in this survey is voluntary and that I am free to withdraw at any time, without giving a reason and without penalty.

☐ Yes

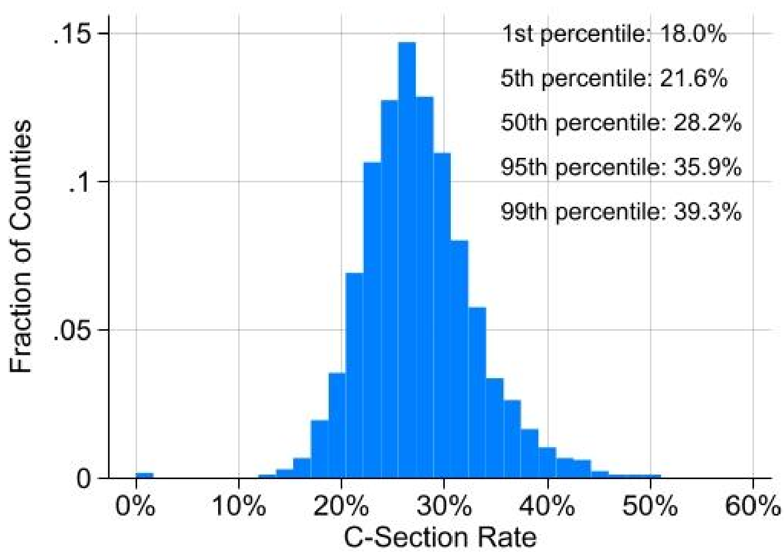
First and Last Name

Email Address

Primary Affiliation

**Background**: There is substantial variation in C-section rates across counties. The causes of these differences are unclear.

**First Birth C-Section Rates Across US Counties - 2019 (Counties with More 100 Births)**

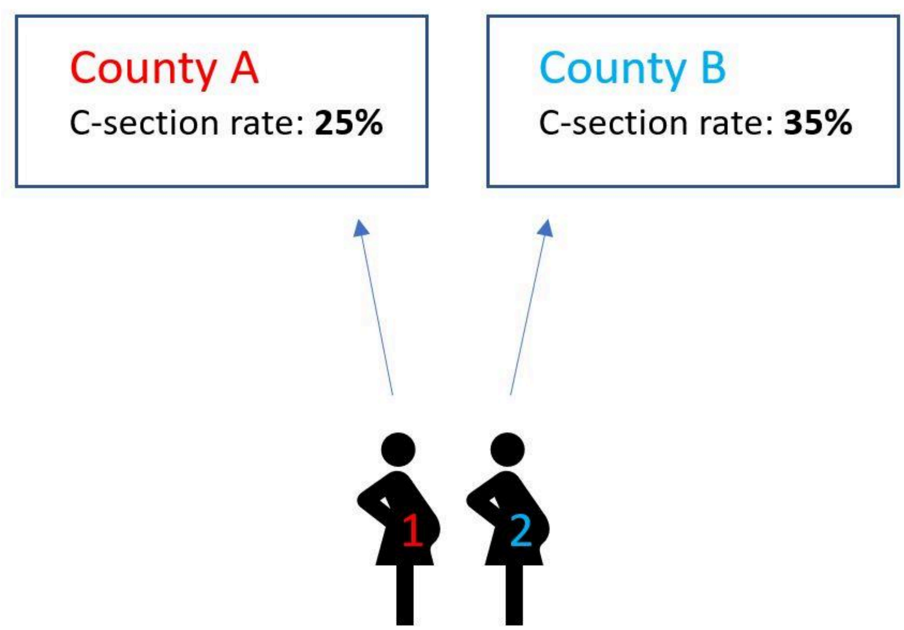


Consider the following scenario with 2 identical mothers who live in the same place. Mother 1 is randomly assigned to give birth in County A and Mother 2 is randomly assigned to County B.

The C-section rates between County A and B may differ due to many factors, including

(a) **healthcare providers (hospitals, healthcare personnel)**, including hospital ownership structure, physician training, reimbursement rates, etc.

(b) **patient characteristics**, including preferences and risk factors



On average, how much more likely do you believe Mother 2 is to have a C-section compared to Mother 1? Please give a response ranging from 0 to 10 percentage points.

Choosing the **"0 percentage points"** option implies that the 2 women are equally likely to have a C-section, and thus **none of the C-section rate variation** across counties is attributable to healthcare providers.

Choosing the **"5 percentage points"** option implies that the probability that Woman 2 has a C-section is 5 percentage points higher than Woman 1, and thus **half of the C-section rate variation** across counties is attributable to healthcare providers.

Choosing the **"10 percentage points"** option implies that the probability that Woman 2 has a C-section is 10 percentage points higher than Woman 1, and thus **all of the C-section rate variation** across counties is attributable to healthcare providers.

0 1 2 3 4 5 6 7 8 9 10

Percentage point difference: 0.5

Why did you give this answer? Briefly explain.

**Version 2**

By checking the "Yes" box below, I understand that my participation in this survey is voluntary and that I am free to withdraw at any time, without giving a reason and without penalty.

☐ Yes

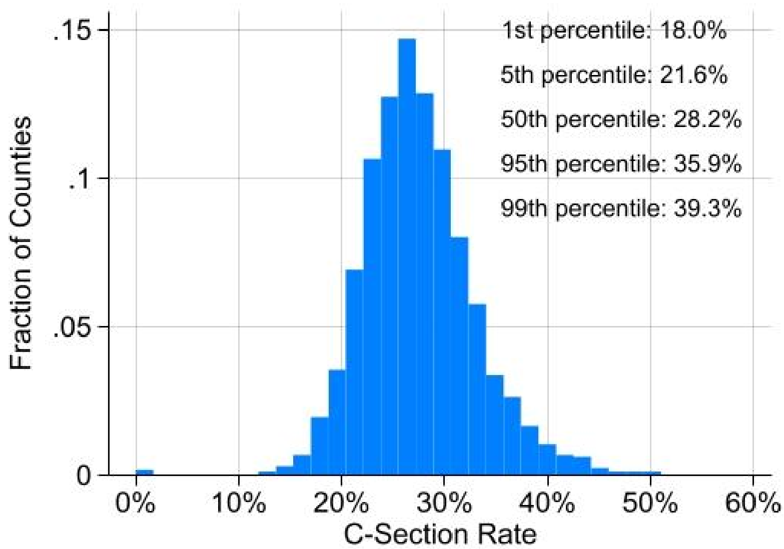
First and Last Name

Email Address

Primary Affiliation

**Background**: There is substantial variation in C-section rates across counties. The causes of these differences are unclear.

**First Birth C-Section Rates Across US Counties - 2019 (Counties with More 100 Births)**

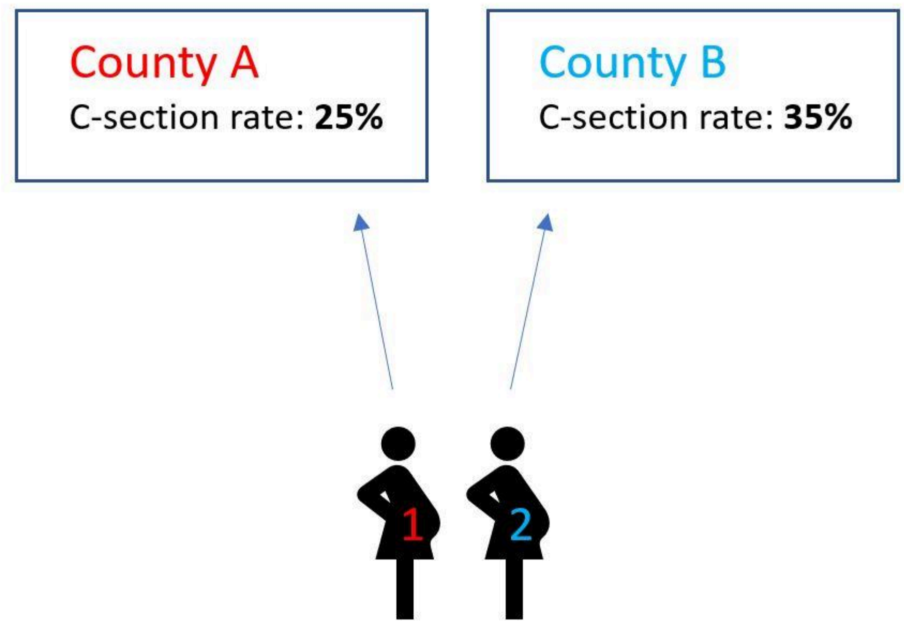



Consider the following scenario with 2 identical mothers who live in the same place. Mother 1 is randomly assigned to give birth in County A and Mother 2 is randomly assigned to County B.

The C-section rates between County A and B may differ due to many factors, including

(a) **healthcare providers (hospitals, healthcare personnel)**, including hospital ownership structure, physician training, reimbursement rates, etc.

(b) **patient characteristics**, including preferences and risk factors




On average, how much more likely do you believe Mother 2 is to have a C-section compared to Mother 1? Please give a response ranging from 0 to 10 percentage points.

Choosing the **"0 percentage points"** option implies that the 2 women are equally likely to have a C-section, and thus **none of the C-section rate variation** across counties is attributable to healthcare providers.

Choosing the **"10 percentage points"** option implies that the probability that Woman 2 has a C-section is 10 percentage points higher than Woman 1, and thus **all of**

**the C-section rate variation** across counties is attributable to healthcare providers.

| | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Percentage point difference: | | | | | | | | | | | | 0.5 |

Why did you give this answer? Briefly explain.

**Version 3**

By checking the "Yes" box below, I understand that my participation in this survey is voluntary and that I am free to withdraw at any time, without giving a reason and without penalty.

☐ Yes

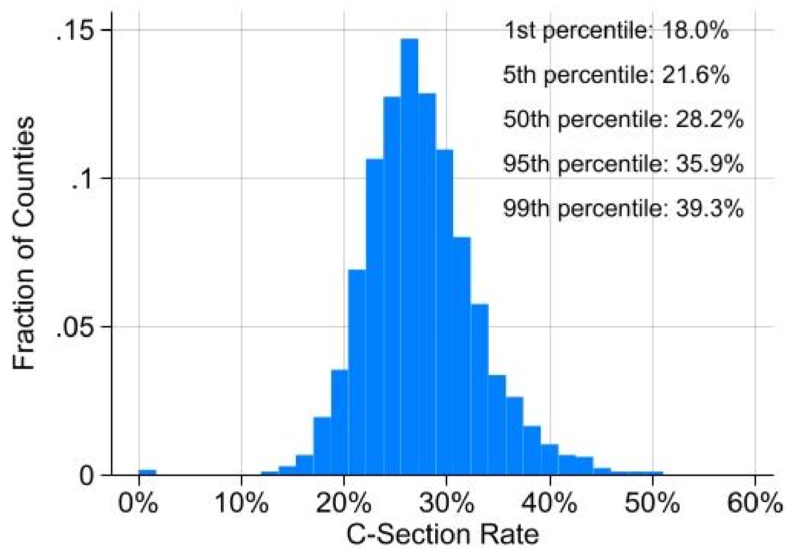
First and Last Name

Email Address

Primary Affiliation

**Background**: There is substantial variation in C-section rates across counties. The causes of these differences are unclear.

**First Birth C-Section Rates Across US Counties - 2019 (Counties with More 100 Births)**




What percentage of the differences in county C-section rates do you believe is **causally** due to **healthcare providers** (hospitals, healthcare personnel)?

| | 0 | 10 | 20 | 30 | 40 | 50 | 60 | 70 | 80 | 90 | 100 | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Percentage due to providers? | | | | | | | | | | | | 5 |

Why did you give this answer? Briefly explain.