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Kelly M. Jones
Mayra Pineda-Torres

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ABSTRACT

Many public policies aim to support (or change the behavior of) a specific, targeted sub-group of the population. To measure the causal impacts of such policies, social scientists use quasi-experimental methods of policy evaluation, but are often hampered by data availability issues, especially regarding targeted populations, who are often disadvantaged and/or vulnerable. We use power calculations to quantify the challenge of detecting a policy's impact on a small minority population based on population level data, using the impact of abortion bans on economic welfare as an example. We provide insight into this challenge and a roadmap for how researchers can calculate, ex-ante, whether their study is sufficiently powered to detect reasonably meaningful effect sizes.

Kelly M. Jones
American University
kmjones@american.edu

Mayra Pineda-Torres
Georgia Institute of Technology
and NBER
mpinedat@gatech.edu

Introduction

Many public policies in the U.S. aim to support (or change the behavior of) a specific, targeted sub-group of the population. Changes such as welfare reform and the expansion of the Earned Income Tax Credit were primarily relevant for low-income, single mothers. Summer feeding programs and universal school meals target food-insecure children. Tuition grant programs target potential college students. Childcare subsidies target low-income mothers with young children. Similar targeting occurs in other realms of public policy, including criminal justice, immigration, agriculture, urban, environmental, and veterans' policies.[1]

To understand the causal impacts of such policies, social scientists use quasi-experimental methods of policy evaluation but are often hampered by data availability issues. Data that can identify a niche population must have relevant details on marital status, fertility, income, food security, college intention, etc. Yet, data that include such details are often for small samples, which might not generate sufficient statistical power to identify impacts. In contrast, large "population-level" datasets such as the American Community Survey, which includes an annual 1% sample of the entire U.S. population, offer ample sample sizes but do not always include sufficient detail to identify the target population of interest. Even if the target population is identifiable (for example, low-income single mothers), a 1% sample of the target population may still not be a sufficient sample size for analysis.

Further, researchers and policymakers may be interested in the "downstream" impacts of a policy. For example, not only whether college grants increase college enrollment, but whether they also increase college completion; or whether welfare reform not only affects income but also whether it affects poverty and/or employment. Such downstream impacts are even more difficult to identify, as they require sufficiently large "first stage" effects (i.e., impacts on the primary outcome) and even more statistical power.

While certain public policies may target small population groups, these are often populations that are particularly disadvantaged and/or vulnerable. Therefore, the impact of policies on these populations is priority knowledge for policymakers. Yet, the relatively small size of some of these groups may hamper researchers' ability to identify such impacts. Indeed, studies that fail to find statistically significant impacts of social welfare policies may be merely reflecting an issue of low statistical power due to the limited nature of publicly available data in the U.S. In this paper, we use power calculations to examine whether a policy's impact on a small minority population can be detected in population-level data. In doing so, we provide insight into the challenge of estimating policy impacts on minority populations and a roadmap for how researchers can calculate, *ex-ante*, whether their study is sufficiently powered to detect reasonably meaningful effect sizes.

We focus our analysis on an investigation of whether abortion bans impact women's economic welfare. This is an excellent example of the challenges discussed above because (i) relatively few women will experience an unintended birth as a result of an abortion ban and (ii) economic impacts are likely to be primarily driven by impacts on births, so this is an example of a "downstream" outcome. In the next section, we provide contextual background. Then, we undertake this exercise in four discrete steps. First, we approximate a reasonable range for the impact of the policy on the primary outcome (here, the impact of abortion bans on birth rates). Second, we use existing data (that other researchers are most likely to use)

[1] Some of the largest examples include: VA disability compensation & pensions targets veterans, Section 8 housing vouchers target very low-income families, HIV/AIDS programming and Violence Against Women programming target specific patients/victims, and EPA Community Change Grants target low-income communities burdened by pollution.

to choose outcomes of interest, estimate the variance of these outcomes, and predict sample sizes. Third, we use the information from steps 1 and 2 as inputs into power calculations to calculate the minimum detectable effect (MDE) for each outcome of interest under a range of reasonable assumptions for years of data employed, and the desired power and significance of a hypothesis test. This step is conducted for several outcomes across several different demographic groups. Finally, in our fourth step, we examine the MDEs to understand whether such effect sizes are realistic.

Background

Since the repeal of *Roe v. Wade* by the Supreme Court in the case *Dobbs vs. Women's Health Organization* (henceforth, *Dobbs*) in June 2022, 14 states have banned abortion access. Recent studies have emerged that estimate the causal impact of these bans on births, mental health, child abuse/neglect, intimate partner violence, WIC participation, internet searches, voter attitudes, migration patterns, and medical school applications (Dave et al., 2025; Dench et al., 2024; Dench, Lifchez, et al., 2025; Dench, Myers, et al., 2025; Ferguson et al., 2024; Gupta et al., 2023; Hess, 2024; Mukhopadhyay & Katsikas, 2024; Piette Durrance et al., 2025; Springer, 2026; Thornburg et al., 2024). Given the documented role of bans in increasing fertility, it is natural to expect this to result in downstream impacts on economic welfare. After all, historical evidence confirms that other forms of reproductive control technology, such as contraception, reduced fertility and resulted in numerous positive economic outcomes (Bailey, 2006; Bailey et al., 2012, 2018; Browne & LaLumia, 2014; Edlund & Machado, 2015; Goldin & Katz, 2002; Steingrimsdottir, 2016).

Though evidence from contraceptive availability does suggest that unintended births have economic consequences for women, such evidence does not necessarily have implications for the impact of abortion access, as the populations that contraception and abortion access impact may not be the same. Indeed, evidence suggests they are not. In the context of the U.S., while contraception access improved outcomes disproportionately for privileged women (Bailey et al., 2012; Goldin & Katz, 2002; Steingrimsdottir, 2016) abortion access (or conversely, abortion restrictions) has improved (or worsened) outcomes more for disadvantaged women (Abboud, 2025; Angrist & Evans, 1999; Borelli, 2011; Jones & Pineda-Torres, 2024; Kalist, 2004). While research from the post-*Roe,* pre-*Dobbs* era does indicate that abortion access affects abortion use and/or births (Altındağ & Joyce, 2022; Arnold, 2022; Fischer et al., 2018; Gius, 2019; Hall, 2026; Joyce et al., 2020; Levine, 2003; Lindo et al., 2020; Lindo & Pineda-Torres, 2021; C. K. Myers & Ladd, 2020; Venator & Fletcher, 2021), evidence on downstream economic impacts is limited (Bahn et al., 2020; Jones & Pineda-Torres, 2024; Miller et al., 2023). Likewise, one might expect abortion bans to have even greater impacts on economic welfare than abortion restrictions, but so far, evidence of the economic impacts of post-*Dobbs* abortion bans is limited (Springer, 2026).

We argue that a lack of evidence of impact should not necessarily be interpreted as a lack of impact, especially when considering downstream outcomes. **We demonstrate that reasonably large individual impacts may remain undetected when data availability is insufficient for statistical analyses to document them.** In this demonstration, we conduct power calculations to test whether reasonably sized economic effects of abortion bans are detectable given expected impacts on births and current data availability.

Analysis and Results

Step 1. Approximating the impact in the first stage (the impact of abortion bans on births)

We rely on existing published research to approximate the impact of past related policies (abortion restrictions and current abortion bans) on the front-line outcome, birth rates. First, we search journals in economics and public health and working paper series to identify papers that estimate an arguably causal effect of a change in abortion access on births.

We classify papers according to the type of change in access (i.e., expansion or restriction of access), and we focus on studies that examine changes in access most similar to abortion bans: closure of clinics and/or an increase in travel distance to providers. We do not focus on papers examining other changes, such as the implementation of parental involvement laws, mandatory waiting periods, or changes to insurance coverage, welfare benefits, or subsidization of costs, because, although restrictive, these policies affect access for specific abortion seekers and, therefore, they do not prevent everyone in an area from obtaining an abortion such as abortion bans potentially do. In contrast, clinic closures, similarly to abortion bans, affect access for anyone requiring abortion services from such facilities, no matter their age, gestational age, insurance status, etc.

We then classify papers according to whether the study period is historical or modern. Study periods are classified as modern if they include more years in the 21st century than years in the 20th century. This excludes papers focused on 19th-century criminalization and pre-*Roe* repeals or reforms. Finally, we exclude otherwise relevant studies if they are set outside the U.S. (including studies set in Norway, Spain, Eastern Europe, Mexico, Colombia, and Nepal). We note that papers that are otherwise relevant are excluded if they do not report impacts on births.

We rely on estimates of impacts on births from 9 studies in total: 2 studies of Texas House Bill 2 (2013), which closed about half of the state's clinics, 1 study of clinic closures in Wisconsin, 3 studies of TRAP laws nationally, 1 study of changes in distance nationally, and 2 studies of post-*Dobbs* abortion bans. The estimates from these studies are presented in Table 1. For half of these studies, impacts on births are estimated nonlinearly, for example, separate impacts for a change in driving distance from 0 to 50 miles than for a change from 100 to 150 miles.

Next, we identify which findings from these papers are most relevant for our purposes by approximating the change in access occurring under an abortion ban. For the 14 states with post-*Dobbs* bans, we use Myers (2024) to compile the pre-*Dobbs* average driving distance and calculate a population-weighted average. We find that the average driving distance for individuals in these states prior to bans was 46 miles. We then use the Myers Abortion Access Dashboard to compile the average driving distance for each state as of October 1, 2025. Again, using population-weighted averages, we calculate that the average woman aged 15 to 44 in ban states has faced an increase in driving distance of 279 miles (from an average of 47 miles to 326 miles). These findings suggest that we should refer to evidence on the impacts of the highest categories reported for increases in driving distance. The estimated impacts of such changes on births range from not statistically different from zero to increases as high as 2.8% for all women, 5% for teens, 3.8% for women aged 20 to 24, and 3.6% for Black women.

Estimates of impacts of post-*Dobbs* bans on births (in early 2023) range from 2.1% to 2.7% for all women of reproductive age, with higher estimates for Black women (3.8%) and Hispanic women (4.7%). Because these geographic-based (rather than distance-based) estimates come from only one study, we also consider other geographic-based studies of TRAP law exposure. These estimates indicate a range of

impacts on births between 1.8% and 5.6% for all women of reproductive age and estimates between 3% and 6% for teens.

Step 2. Identifying and understanding outcomes (economic welfare indicators)

To generate the largest sample possible, we use the American Community Survey (ACS), which represents a 1% sample of the U.S. population. We select possible outcomes of interest from available ACS data by considering which economic indicators could reasonably be affected in the short run by an unintended birth. A key outcome of interest is **total family income** (measured as the total pre-tax income earned by one's family from all sources in the previous year). We focus on family income rather than individual income because non-participation in the labor market is a luxury for some individuals – and this is more prevalent among women – so family income is potentially a better economic welfare indicator. We also employ a version of family income that is scaled by household composition: income as a percentage of the household's official poverty line. In addition, we employ two measures of **poverty**: a binary indicator of official poverty status (being below the household composition-specific poverty line) and receipt of public assistance (food stamps).

We also select indicators of economic activity and investment in human capital that may be impacted in the short run and, therefore, identify these for specific age groups. To capture college enrollment, we focus on the age group 18-22 and use an indicator of being enrolled in any educational program. To capture recent college completion, we focus on the next 5-year age group (23-27) and use an indicator for having completed a bachelor's degree. For employment, we focus on the remaining women above these age groups (ages 28-44) and use an indicator for being employed.

For each age group, we consider aggregate outcomes across all races and ethnicities. We additionally consider possible analyses that focus specifically on Black women or on Hispanic women. As discussed in the *Background* section, prior evidence suggests that abortion access has larger impacts on economic welfare in these groups. Larger treatment effects may increase statistical power, yet smaller available sample sizes for these groups reduce statistical power; it is important to calculate the net effect of these counteracting forces for the analysis of these important subgroups.

For each of these demographic groups, we measure the average sample size per year, $\bar{n}$, based on the ACS data in 2019, 2021, 2022, and 2023. These are reported in Table 2 and range from a high of 547,798 for all women aged 15-44 to a low of 59,749 for Black women aged 15-44. For each outcome and for each relevant group, we measure the mean and standard deviation in these data. For the calculation of the mean, we employ the Census-provided inverse-probability sampling weights to ensure population representativeness. These statistics are provided in Table 3. For all women aged 15-44, the mean family income is $107,114, and the poverty status is 15%. Among college-age women, 63% are enrolled in school. Among women aged 23-27, 40% have completed a bachelor's degree, and among women aged 28-44, 75% are employed.

Step 3. Calculating minimum detectable effects

We aim to calculate the minimum effect size that would be statistically distinguishable from zero, given the expected sample size, outcome variance, and proportion of sample treated.

To calculate the minimum detectable effect (MDE), we first set assumptions about the parameters of the planned hypothesis test. Let $\alpha \in (0,1)$ be the probability of a Type I error (incorrectly detecting an effect), known as the significance of the test. Let $\kappa \in (0,1)$ be the probability of a Type II error (incorrectly failing to

detect an effect), so that $1-\kappa$ is the power of the test. Then, we can identify from the cumulative distribution function the critical values $t_{1-\kappa}$ and $t_{\alpha/2}$. We can calculate

$$MDE = \left(t_{1-\kappa} + t_{\alpha/2}\right) \times \sqrt{1/P(1-P)}\sqrt{\sigma^2/N} \quad (1)$$

based on sample size, N, the proportion of the sample that is treated, P, and σ^2, which is the variance of the error term, or the residual variance if including control variables (Glennerster & Takavarasha, 2013).

Under assumptions for κ and α, we can calculate the MDE based on N, P, and σ^2. Where applicable, we employ two values for a parameter: most conservative (MC) and least conservative (LC). We set power ($1-\kappa$) within the range of 90% (MC) to 80% (LC) and hold significance (α) constant at 5%.

To predict N, we assume that the researchers' calculations will rely on T years of data, where $T = \{10,20\}$ for most- and least-conservative, respectively. We estimate $N = T \times \bar{n}$. Values of N are determined at the group-by-assumption level and are shown in Table 4; these range from 597,492 to more than 11 million observations.

To predict P, we calculate the share of observed individuals that reside in states with post-*Dobbs* abortion bans and are observed in post-*Dobbs* years. The optimal value of P for minimizing MDE is $P = 0.5$, as this maximizes the quantity $P(1-P)$. In our MC version, we identify 11 states as treated, and as a result, treated individuals comprise 19.5% to 22.9% of the sample, depending on demographics. The treated states include AL, AR, ID, KY, LA, MS, OK, SD, TN, TX, and WV, which are states that have had a complete abortion ban enforced since 2022. However, this includes states that have had very short interruptions to the enforcement (of a few months or less) due to injunctions, and states that have exceptions for cases of rape, incest, or risk of death. Our LC version includes a larger number of treated states, which yields a lower MDE, increasing the ability to identify effects. The larger set of states includes the 11 above, plus 9 additional states that have either passed an abortion ban after 2022, had a post-*Dobbs* ban that is no longer in place, or have a 6-week gestational limit. [2] These additional states are FL, GA, IA, IN, MO, ND, SC, WI, WY, and together the LC group of states comprises between 36.1% and 46.6% of the sample, depending on demographics.

P is then calculated as

$$P = \frac{N_B}{N} \times \frac{T_{post}}{T}$$

where N_B is the sample size from ban states and T_{post} is the number of data years post-*Dobbs*. We assume $T_{post} = 5$ (for future analyses), which is either 50% or 25% of the data years, depending on the value of T. Values for P are determined at the group-by-assumption level, and the values are shown in Table 4.

We could approximate σ^2 using the square of the standard deviation for each outcome; however, we assume that most estimates would include a basic set of control variables that would absorb some variation in the outcome and reduce the variance of the estimate. Therefore, we estimate

$$O_{istaebh} = \nu_s + \nu_t + \nu_a + \nu_e + \nu_b + \nu_h + \varepsilon_{istaebh}$$

[2] We acknowledge that, in addition to reducing the MDE, expanding the definition of a ban (for example, to include 6-week bans or interrupted bans) may also reduce the actual *average* impact of a ban, reducing the probability that the true average treatment effect is at least as large as the MDE. Such considerations are left to the researchers aiming to estimate effects, as our purpose here is to set upper and lower bounds for MDEs.

where $O_{istaebh}$ is the outcome of interest for individual i in state s in year t of age a in education group e from race group b and ethnicity group h, and each ν is a set of fixed effects as indicated by its subscript. ν_a is for single-year age groups, ν_e uses 4 categories of education, and ν_b and ν_h are binary indicators of Black race and Hispanic ethnicity, respectively. The education fixed effect is omitted from the estimations of the education-related outcomes. We then approximate σ^2 using the square of the residual error, σ, from this regression. The values for σ, are determined at the group-by-outcome level and are reported in Table 3. These values are generally 2% to 10% smaller than the standard deviation of the outcome.

Table 5 reports the results of the calculation of the MDE for each outcome at the group-by-assumption level; these results are discussed in detail in step 4. The units in which the MDE is reported are the same as the units in which the outcome is measured; we also report the MDE as a percentage of the mean from Table 3.

MDE among the affected sub-group

These calculated MDEs are the smallest size *population-level* effects for which we could reject a null hypothesis that the effect is zero. By population-level effects, we mean the average effect among women age 15-44. To determine whether population effects of at least this size are realistic, we estimate what effect size would be required *upon those actually affected by the abortion ban* in order to generate a population effect of this magnitude.

We define those affected by the ban as those who have an unintended birth that would not have occurred in the absence of the ban. There are several important caveats to this. First, an unintended birth is not the only possible impact of an abortion ban. For many people, even if they do eventually access care, a ban will cause economic hardship due to additional requirements of travel, lodging, time away from work, childcare, and/or higher cost of abortion care. Second, even in the absence of any unintended pregnancy, perceiving reductions in access to abortion may cause distress, impacting mental health, and/or shift an individual's decisions regarding investment in education or career, which may have economic implications. Nonetheless, to estimate realistic population-level impacts on economic outcomes, we must identify what share of the population is most likely to feel such effects, which is not the majority. We therefore estimate what share of the population of women age 15-44 (henceforth, population) will have an additional birth as a result of a ban.

To estimate the affected share of the population, we rely on several inputs. First, we use the assumed range of impacts on births discussed in step 1, e, reported in percentages of births. e is determined at the group-by-assumption level, and values are reported in the fourth row per group of Table 4; these range from 2% to 6%. Second, we use an assumed baseline birth rate per year for the relevant demographic or age group, b, reported in the second column of Table 2 as births. These are the group-specific numbers of births per 1,000 women aged 15-44 (or relevant age) in 2021, as reported by the National Vital Statistics.[3] For the non-standard 5-year age groups, the reported birth rate is a weighted average of the birth rates for the standard 5-year age groups included. For example, the birth rate for the age 18-22 group is the 18-19 rate multiplied by two-fifths, plus the 20-24 rate multiplied by three-fifths. The birth rates are reported for each group in Table 2 and range from 47.5 to 80.4 births per 1,000 women per year.

[3] We note that the following calculations are based on recent birth rates from 2021, which are considerably lower than in decades past. As such, these calculations do not provide information about the MDEs of abortion access on economic outcomes in previous decades.

We calculate the affected share of the population of women aged 15-44 (or the relevant age group), S, as

$$S = e \times \frac{b}{1000} \times T_{post}$$

because $b/1000$ is the share of women with a birth, e is the proportion by which it is increased each year, and T_{post} is the number of years of increase. If we again assume $T_{post} = 5$, then S is the assumed percentage of reproductive-age women with an unintended birth resulting from the ban over a 5-year period. S is determined at the group-by-assumption level and is reported in the fifth row per group of Table 4. These values range from 0.56% to 2%.

Finally, we use the population-level MDEs and the estimated share of the population affected to calculate the MDE among the affected, τ, where $\tau = \frac{MDE}{S}$. τ is determined at the group-outcome-assumption level and is reported in the third row per group in Table 5. In the fourth row, we additionally report τ as a share of the population mean for the relevant outcome. These are interpreted as the minimum effect of an unintended birth on the outcome of interest that would be required in order for researchers to statistically identify impacts of abortion bans on that outcome at the population level.

Step 4. Comparing MDEs to reasonably expected effect sizes

In this section, we examine the estimates of τ and consider whether such magnitudes are realistically possible. We move chronologically through the outcomes, beginning with school enrollment at ages 18-22.

Under the least conservative assumptions, the impact of an abortion ban on school enrollment would require that an unintended birth reduce a woman's probability of school enrollment at these ages by at least 40%, which certainly seems possible. However, under the most conservative assumptions, a decrease of 122% would be required, which is clearly impossible. Similarly, for college completion (among women aged 23-27), the MDE on the affected ranges from 49% to 125%. Impacts on women's employment (at ages 28-44) are somewhat easier to detect (primarily due to the larger age range generating a larger sample), with the MDE on the affected ranging from a 20% decrease in employment to a 62% decrease, depending on the assumptions employed.

Changes in education and/or employment may contribute to changes in family income, and this, combined with changes in household composition, may contribute to changes in poverty and receipt of food assistance. We find that the impacts of an unintended birth on family income would need to be at least 23% to 88% to detect an effect of abortion bans on this outcome at the population level. While 23% seems reasonable, 88% seems unrealistic. However, the impacts on family income would be easier to detect using the measure of family income as a percent of the poverty line; this is due to the lower variance of this outcome generated by the Winsorization of outliers. For this outcome, MDEs on the affected are as low as 11% to 43%.

Changes in poverty status and receipt of food assistance would need to be quite large in order for the impacts of abortion bans on these outcomes to be detected at the population level. Given the high correlation between these outcomes, the findings are very similar. An unintended birth would need to increase the probability of these outcomes by at least 50% to 200%, depending on the assumptions employed, which seem unrealistic at the high end.

Taken together, these findings suggest that if (a) impacts of bans on births are large relative to previous estimates and (b) impacts of unintended births on these outcomes are reasonably large,

then detecting population-level economic effects of bans is feasible, at least for the aggregate population. Are these conditions realistic?

Condition (a) seems possible. One would expect that the impact of bans on births would be larger than the 2%-6% estimated impacts of less severe abortion restrictions, such as TRAP laws. Further, though estimates of the early impact of abortion bans on births have been modest (2%-3%), these are based on 2023-24 data. This was a time when significant donor funding in response to *Dobbs* was supporting abortion access, which, paired with increased telemedicine abortion access, prevented significant reductions in abortion rates. The donor response to *Dobbs* largely expired in late 2024 (Peters, 2025), so it may be possible that the impacts of bans on births will increase in 2025 and subsequent years, though sustained increases in telemedicine provision may prevent this(Norris et al., 2026).

Condition (b) also seems likely. Miller et al (2023) find that women who were denied a wanted abortion due to gestational limits experience a 78% increase in past-due debt and an 81% increase in negative public records (i.e., bankruptcy, evictions, and tax liens) relative to women who sought an abortion at the same gestational stage but were not denied. It seems reasonable that 40% of students with an unintended birth drop out, given that nearly 50% of teen moms do not finish high school (Manlove & Lantos, 2018). It also seems possible that at least 20% of women with unintended births would leave the labor market and that this might result in an average reduction in family income of at least 11%-23%. Given that women with low incomes are the ones most likely to be "trapped" by state bans, such reductions in family income, combined with a larger household size, could reasonably increase the probability of poverty/food assistance by more than 50%.

However, if condition (a) is not met and/or analyses include smaller sample sizes, the likelihood of detecting effects drops dramatically. Under the most conservative assumptions, which assume smaller impacts on births and fewer years of data, detecting economic impacts would require that an unintended birth reduce the probability of school enrollment by at least 122%, or reduce the probability of college completion by at least 125%, decrease the probability of employment by at least 62%, reduce family income by at least 43% to 88% (depending on the measure), and/or increase the probability of poverty/food assistance by at least 200%. These minimum effect sizes may be unrealistic, suggesting that if the impacts of post-*Dobbs* bans on births do not increase, or analyses employ insufficiently large data sets, there is a high chance of failing to detect economic impacts at the population level that are, nonetheless, very much occurring for affected individuals.

This finding is primarily driven by the fact that **unintended births arising from abortion bans, while potentially hugely impactful for the individuals experiencing them, nonetheless affect a relatively small share of the population, even when accumulated over 5 years, limiting the ability to identify statistically significant effects of abortion bans on population-level economic outcomes**. However, increasing the post-*Dobbs* years included in the analysis would increase the accumulation of impacts and increase the likelihood of detecting effects. For example, under the most conservative assumptions, an increase to 8 post-*Dobbs* years would increase the share of the sample affected from 0.59% to 0.94%. Even this small increase in population affected would substantially decrease τ as a share of the mean, from a 63% reduction in the probability of employment to a much more realistic 39% reduction.

Discussion

We have endeavored to provide an example of ex-ante power calculations to determine whether the downstream impacts of a public policy on a targeted sub-group would be identifiable in population-level

data. To do this, we have (i) relied on existing evidence to approximate the expected front-line impact of the policy, (ii) relied on population-level data to identify outcomes of interest and garner useful statistics on them, (iii) calculated the MDE on these outcomes based on available data and a reasonable set of assumptions, and (iv) identified the magnitude of effects on the affected that would be required to observe impacts at the population level.

One takeaway from this exercise is that it is possible that large impacts that affect only a small share of the population can be detected in population-level data. In our example, the share of women who will have an unintended birth as a result of the post-*Dobbs* ban is quite small: between 0.56% and 1.4% of women aged 15-44 (though the share may be as high as 2% for women in their 20s). Nonetheless, we know from existing research that the economic impacts on an individual of having an unwanted birth can be very large (for example, increasing bankruptcy/evictions by 81%). Our calculations indicate that if the impacts of bans on births increase at all (relatively to early estimates from 2023-24) and if the impacts of unwanted births on economic outcomes are comparable to the largest previous estimates, then population-level effects could be detected. In addition, our calculations indicate that the number of post-ban years included in the analysis is an important factor for statistical power, affecting both the sample size and the share of the population affected. This suggests that it may take several more years for researchers to be able to accurately identify the impacts of post-*Dobbs* bans on women's economic welfare.

A second takeaway from this exercise is that large – and potentially devastating – impacts at the individual level may go unnoticed in statistical analyses. In our example, if impacts on births remain at a moderate 2% level (or if smaller sample sizes than assumed here are employed), even very large effects on the affected will not be detectable in population-level analyses. Narrowing the focus of the analysis to a relevant sub-population would substantially increase the treated share of observations (P), thereby reducing MDEs. For example, focusing on women who experience an unintended pregnancy would increase P from 0.56% to 3.1%.[4] Unfortunately, such a focus would also drastically reduce the available sample size, N. The ACS is the largest survey in the US, covering 1% of the population. It does not include information on unintended pregnancy, but even if it did, narrowing the sample to women with this experience would reduce the sample size (in the most conservative case) from 5.5 million to less than 200,000. The increased P and decreased N would counteract each other, resulting in almost no change in required MDE on the affected (from a 62% reduction in the probability of employment to 58%). Combining this sub-sample focus with an increase in the ACS population coverage from 1% to 5% would allow researchers to detect effects on employment as small as 26%, but clearly, quintupling the size of the ACS is highly unlikely (as might be incorporating information on unintended pregnancy in the ACS).

Data sets that do include details allowing a researcher to identify the targeted or affected population have other challenges that may make them unusable for the analyses described here. In the case of our example, there are existing data sets that can allow researchers to identify women who have experienced

[4] This calculation is based on an assumption that losing abortion access increases births by 2%, which, applied to an annual birth rate of 56 per 1,000 women aged 15-44, is an increase of 1.12 births per 1,000 per year. However, these births occur only to women who experience an unintended pregnancy, so if these women are observable in the data, then the analysis can focus on this important sub-group. The CDC estimates an annual unintended pregnancy rate in 2019 of 35.7 per 1,000 women aged 15-44 per year (CDC, 2025). This would suggest that, among women with unintended pregnancies, 1.12/35.7=3.1% have an unintended birth as a result of losing abortion access. Over a 5-year period, additional births per year are 1.12*5=5.6, and the number of women is not changing, so this is 5.6 births per 1,000 women or 0.56% of women affected over 5 years. Over this same period, the number of women with unintended pregnancies is 35.7 per 1,000 per year, so 1.12/35.7=3.1% of women with unintended pregnancies are affected regardless of the number of years considered.

an unintended pregnancy. The National Survey of Family Growth, for example, includes about 5,000 women per cycle, which would include fewer than 200 women with unintended pregnancies per cycle, a sample far too small for these analyses. Pregnancy Risk Assessment Monitoring System (PRAMS) data include more than 100,000 women per year across states, likely including 3,500 or more women with unintended births. However, the PRAMS sample includes only women with a birth, so women who have a birth as a result of lost abortion access could not be compared to their best counterfactual (women who *were* able to abort); they could only be compared to other women with unintended births (who chose not to abort for other reasons).

In sum, if the impacts of our example policy increase over time – or simply accumulate over time – it may be possible to statistically detect their downstream impacts by using large-scale 1% samples of the US population. This seems like the most feasible pathway for estimating such impacts, as datasets that allow researchers to identify impacted women have sample issues that prevent their usability for this purpose.

The example of estimating the economic impacts of post-*Dobbs* bans has many parallels with other public policies that may suffer from similar challenges. While abortion legalization under *Roe* changed abortion access for the vast majority of American women, post-*Dobbs* bans have changed access in ways that are strongly aligned with geographic and economic inequality. The women most likely to be impacted by recent bans are not the majority; they are a disadvantaged and vulnerable population, much like the target populations for many social welfare policies and other public policies. Impacts upon these populations, while difficult to identify, should, nonetheless, be a research priority. Yet even the largest "population-level" data in the U.S. (the ACS) might not be large enough to identify impacts on such minority populations. Such challenges should always be considered when relying on the body of evidence regarding the impact of policies. Many benefits of supports (or harms of restrictions) may only be identifiable with much larger sampling frames than currently exist in U.S. data.

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Tables

Table 1. Existing evidence of impacts of abortion restrictions on births

Policy evaluated	Paper	Description of non-time dimension of variation	Change in aggregate births	Change in births by age group	Change in births by race/ethnicity	Change in abortions
HB2	Cunningham, Lindo, Myers, and Schlosser (2020)	provider 50-100 mi vs w/in 50mi	1.6%[a]	not robust	not robust	16%
		provider 100-150 mi vs w/in 50mi	ns	not robust	not robust	28%
		provider 150-200 mi vs w/in 50mi	ns	not robust	not robust	38%
HB2	Fischer, Royer, and White (2018)	clinic 25+mi vs <25mi	1.60%	ns	1.9% Hisp	16.60%
		clinic 50+ mi vs <25mi	2.80%	2.9% 30-39, 8.5% 40-44	1.9% Hisp	16.70%
		clinic 100+mi vs <25mi	1.70%	3.1% 30-39, 9.6% 40-44	ns	22.10%
Wisconsin clinic closures	Venator and Fletcher (2020)	counties with driving distance 100+ mi relative to those < 25 mi	2.80%	nr	nr	36.0%
		counties with driving distance 50-100 miles relative to those < 25 mi	1.51%	nr	nr	25.2%
		counties with driving distance 25-50 miles relative to those < 25 mi	1.17%	nr	nr	16.1%
National distance	Myers (2024)	counties with changes in driving distance from 0 to 100 miles relative to those with no change	2.20%	5% 15-19; 3.8% 20-24, 1.7% 25-29	2.1% W, 3.6% B, ns H	19.40%
		counties with changes in distance from 100 to 200 miles	1.60%	nr	nr	12.80%
Ban-induced distance	Dench et al (2025)	counties under ban with no change in distance	0.80%	nr	nr	nr
		counties under ban where distance increases from 10 to 50mi	1.15%	nr	nr	nr
		counties under ban where distance increases from 50 to 300mi	2.06%	~2% 15-19; ~3% 20-34	~2.5% for W and Hisp	nr

Continued on next page.

Table 1 continued

Policy evaluated	Paper	Description of non-time dimension of variation	Change in aggregate births	Change in births by age group	Change in births by race/ethnicity	Change in abortions
State bans	Dench, Pineda-Torres, and Myers (2024)	Ban states relative to protective states	2.1 to 2.7%	ns 15-19, 3.3% 20-24, 2.8% 25-29, 2.1% 30-44	3% W, ns B, 4.7% H	nr
TRAP	Arnold (2022)	State with TRAP laws vs other states; average over first 9 years	3.20%	nr	nr	nr
TRAP	Jones and Pineda-Torres (2024)	State with TRAP laws vs other states; average over first 5 years	1.81%	5.92% 15-19	teen W 3.52%; teen B 3.24%	4.2% all; 4.6% teens
TRAP	Gardner (2024)	State with TRAP laws vs other states	5.6%[a]	nr	nr	nr

Notes: "nr" indicates the results were not estimated or not reported. "ns" indicates the estimate was not statistically different from zero. "a" indicates this finding is incidental and used for a back-of-the-envelope calculation of prevalence of health conditions among marginal births.

Table 2. Characteristics at the group level

Demographic group		Average sample size per year	Births per 1,000 women aged 15-44 (2021)
All women	age 15-44	547,798	56.3
Black women	age 15-44	59,749	57.4
Hispanic women	age 15-44	106,628	63.4
All women	age 18-22	101,639	47.5
All women	age 23-27	88,538	80.4
All women	age 28-44	337,147	59.0

Table 3. Parameters at the group-outcome level

	Age 15-44				Age 18-22	Age 23-27	Age 28-44
	Total Family Income (TFI)	TFI as pct of poverty line	Below poverty line	Receives food assistance	In school	Completed college	Employed
All women							
Mean	107,114	317.2	0.150	0.161	0.630	0.404	0.754
Std. dev.	116,083	161.9	0.340	0.350	0.463	0.495	0.432
Sigma	102,410	147.6	0.346	0.349	0.458	0.472	0.417
Black women							
Mean	76,743	262.3	0.218	0.302			
Std. dev.	83,553	165.4	0.408	0.453			
Sigma	75,046	150.3	0.401	0.440			
Hispanic women							
Mean	87,358	270.7	0.189	0.217			
Std. dev.	89,276	157.6	0.380	0.404			
Sigma	80,244	145.8	0.383	0.402			

Table 4. Parameters at the group-assumption level

		Most conservative assumptions	Least conservative assumptions
All women (15-44)			
	N: sample size	5,477,980	11,000,000
	P: treated share of sample	0.098	0.094
	kappa: Pr(Type II error)	0.10	0.20
	e: Assumed effect on births	0.02	0.05
	S: Share of treated affected	0.0056	0.0141
Black women			
	N: sample size	597,493	1,194,985
	P: treated share of sample	0.114	0.116
	kappa: Pr(Type II error)	0.1	0.2
	e: Assumed effect on births	0.03	0.05
	S: Share of treated affected	0.0086	0.0144
Hispanic women			
	N: sample size	1,066,283	2,132,565
	P: treated share of sample	0.113	0.090
	kappa: Pr(Type II error)	0.1	0.2
	e: Assumed effect on births	0.02	0.05
	S: Share of treated affected	0.0063	0.0159
Age 18-22			
	N: sample size	1,016,393	2,032,785
	P: treated share of sample	0.100	0.095
	kappa: Pr(Type II error)	0.1	0.2
	e: Assumed effect on births	0.03	0.06
	S: Share of treated affected	0.0071	0.0143
Age 23-27			
	N: sample size	885,378	1,770,755
	P: treated share of sample	0.098	0.093
	kappa: Pr(Type II error)	0.1	0.2
	e: Assumed effect on births	0.03	0.05
	S: Share of treated affected	0.0121	0.0201
Age 28-44			
	N: sample size	3,371,465	6,742,930
	P: treated share of sample	0.098	0.093
	kappa: Pr(Type II error)	0.1	0.2
	e: Assumed effect on births	0.02	0.04
	S: Share of treated affected	0.0059	0.0118

Table 5. Minimum Detectable Effects

	Total Family Income (TFI)		TFI as percent of poverty line		Below poverty line		Receives food assistance	
	A	B	A	B	A	B	A	B
All women								
MDE	529	344	0.76	0.50	0.0018	0.0012	0.0017	0.0011
MDE as % of mean	0.0049	0.0032	0.0024	0.0016	0.0119	0.0078	0.0109	0.0071
MDE on affected	94,030	24,469	135.54	35.27	0.318	0.083	0.310	0.081
MDE on affected as % of mean	0.88	0.23	0.43	0.11	2.12	0.55	1.93	0.50
Black women								
MDE	1100	694	2.20	1.39	0.0059	0.0037	0.0061	0.0039
MDE as % of mean	0.0143	0.0090	0.0084	0.0053	0.0270	0.0170	0.0202	0.0127
MDE on affected	127,773	48,363	255.95	96.88	0.682	0.258	0.709	0.268
MDE on affected as % of mean	1.66	0.63	0.98	0.37	3.13	1.19	2.35	0.89
Hispanic women								
MDE	886	622	1.61	1.13	0.0042	0.0030	0.0043	0.0030
MDE as % of mean	0.0101	0.0071	0.0059	0.0042	0.0224	0.0157	0.0200	0.0140
MDE on affected	139,764	39,216	253.94	71.25	0.667	0.187	0.684	0.192
MDE on affected as % of mean	1.60	0.45	0.94	0.26	3.54	0.99	3.15	0.88

	Enrolled in school (age 18-22)		Completed college (age 23-27)		Employed (age 28-44)	
	A	B	A	B	A	B
All women						
MDE	0.0055	0.0036	0.0061	0.0040	0.0028	0.0018
MDE as % of mean	0.0087	0.0056	0.0151	0.0098	0.0037	0.0024
MDE on affected	0.767	0.249	0.505	0.198	0.467	0.152
MDE on affected as % of mean	1.22	0.40	1.25	0.49	0.62	0.20