# **PUBLIC HEALTH**

# The impact of abortion restrictions on American mental health

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The overturning of *Roe* v. *Wade* has led to numerous states enacting new abortion restrictions. However, limited empirical evidence exists regarding the general mental health impact of these bans. Leveraging the nationwide Household Pulse Survey, we evaluate the impact of emergent gestational limits and outright bans on self-reported mental health status between July 2021 and June 2023 using a difference in difference approach. Responses indicate a significant increase in reports of mental distress after the institution of such restrictions. These effects appear to persist at least 4 months following a ban and are moderated by household income and education but not by sex, race, age, marital status, or sexual orientation. Less educated and less wealthy subjects reported greater mental health distress compared to wealthier, more educated groups. These results suggest that the institution of abortion restrictions has had broad negative implications for the mental health of people living in the US, particularly those of lower education and personal wealth.

#### INTRODUCTION

On 24 June 2022, the Supreme Court of the United States overturned the constitutional right to abortion access in *Dobbs* v. *Jackson Women's Health Organization* (1). The consequences have been pronounced, with many states banning abortion outright (often without exceptions for rape or incest) and numerous others implementing more restrictive gestational limits (2). Since that time, the medical and academic communities have scrambled to understand the implications of these bans, with confusion emerging surrounding the conditions under which abortion remains permissible (3), giving rise to legal action (4, 5). To date, received research indicates that these restrictions increase projected patient travel costs (6), change patterns of health information seeking (7), decrease trust in clinicians (8), contribute to excess mortality (9), and even alter firm recruitment strategies to cover out of state travel if an abortion is needed by an employee (10).

Empirical attention to abortion is not new. Prior work has examined a host of factors, up to and including the effect abortion access can have on mental health, the focus of this investigation. Prior scholarship from the Turnaway Study, for example, uncovered negative effects on self-reported anxiety, at least in the short term, when pregnant women were denied access to abortion services (11, 12). This scholarship has subsequently led to calls to examine the mental health effects of reproductive care restrictions more systematically (13). Recent work opens the door to such an effect, finding that the leak of the Alito draft opinion in *Dobbs* led to short-term spikes in mental distress among women of childbearing age in states where bans were likely to occur (14), and subsequent research has shown sustained negative effects on mental health in states with bans that were triggered by the Dobbs decision (15). Here, we build on past research not only by investigating state-level statutory restrictions on abortion access that were introduced in the months following Dobbs but also by investigating potential heterogeneity in effects across personal factors like race, sex, marital status, and personal

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wealth. Such moderations are important to consider, because abortion and associated restrictions have previously been found to affect a broader array of individuals, extending beyond women of childbearing age (16). To the extent that these changes in state level policy may be perceived as leading indicators of other changes to policy that might erode individual rights, it is entirely plausible that any effect on mental health will be felt more broadly by persons living in the US.

We hypothesize that such restrictions will have a negative effect on mental health for at least four reasons. First, women seeking reproductive care in the form of abortion services regularly cite factors with clear implications for mental health as their reason for doing so [e.g., financial pressures, partner-related issues, and the need to focus on other children (17)]. Second, households of women who have been denied an abortion experience greater financial distress, and their existing children often suffer as a consequence (18). Third, a fear of being compelled to carry an unwanted pregnancy to term could harm mental health (19, 20). Given the relative danger of child birth in the United States, such concerns are not unfounded (9), notably given the comparative safety of a legally induced abortion as compared with childbirth (21, 22). Last, inasmuch as recent work has also found effects of emergent restrictions on both men and women post-Dobbs [e.g., rates of rates of tubal sterilizations and vasectomies (16)], it stands to reason that groups other than women of childbearing age might also be affected. In other words, insofar as overturning of a half century held Constitutional right may signal instability in institutions and the possibility of future shifts in policy, it is plausible that mental health as a whole might deteriorate, particularly among more vulnerable populations (23).

#### RESULTS

Results from Eq. 1, which includes two-way fixed effects (TWFE) for the state and survey wave, are in Table 1. Note that these estimations also include a vector of binary indicators reflecting each level of the control variables, i.e., a flexible dummy coding. Results indicate that the institution of abortion restrictions led to significantly increased reports of anxiety [0.0069; 95% confidence interval (CI) of 0.0013 to 0.0126; Table 1] and disinterest (0.0061; 95% CI of 0.0016

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Table 1. Average effect of abortion ban or gestational limit (TWFE). SEs in parentheses clustered by state; control variables include flexible dummies capturing sex, age, income, marital status, sexual orientation, and race, including dummies for nonresponse where appropriate. Observation counts differ across estimations due to variation in nonresponse across each mental health survey outcome.

Anxious	Disinterested	Worried	Down		
0.0069** (0.0028)	0.0061*** (0.0022)	0.0029 (0.0029)	0.0035 (0.0024)		
Yes	Yes	Yes	Yes		
1,407,754	1,404,812	1,405,252	1,405,480		
0.083	0.071	0.076	0.079		
	State and survey wave				
-	Anxious 0.0069** (0.0028) Yes 1,407,754 0.083	Anxious Disinterested   0.0069** (0.0028) 0.0061*** (0.0022)   Yes Yes   1,407,754 1,404,812   0.083 0.071	Anxious Disinterested Worried   0.0069** (0.0028) 0.0061*** (0.0022) 0.0029 (0.0029)   Yes Yes Yes   1,407,754 1,404,812 1,405,252   0.083 0.071 0.076   State and survey wave		

\*\**P* < 0.05. \*\*\**P* < 0.01.

to 0.0105). For reference, the average level of these outcomes (anxious, worry, down, and disinterest) are 0.263, 0.214, 0.193, and 0.182, respectively, in states and periods where no ban or gestational limit is present. Findings are similar when using a two-stage difference-indifference (DID-2S) (24) approach (table S3), with anxiety (0.0071; 95% CI of 0.0026 to 0.0115) and disinterest (0.0063; 95% CI of 0.0027 to 0.0098) again rising significantly. Further, reported feelings of being down also rose to a marginally significant degree (0.0036; 89% CI of 0.0001 to 0.0071). These estimates are notable, as they translate to a 0.35 to 0.7 percentage point (pp) increase in the average prevalence of mental health concerns in a state following the institution of an abortion restriction, an approximate 3% relative increase over the baseline. Repeating our estimations using the Patient Health Questionnaire 2 (PHQ-2), Generalized Anxiety Disorder 2 (GAD-2), and Patient Health Questionnaire 4 (PHQ-4) composite survey screens (25) yields consistent and statistically significant results (tables S4 and S5 of the appendix).

To examine these effects over time and to assess the plausibility of the key assumption of difference-in-differences regression (viz., parallel pretreatment trends), we next consider an event-study specification. This is done to ensure that treatment is not preceded by an unobserved, approximately coincident change, such as the implementation of other state level policies, which are also correlated with our outcomes. Assessing equivalence of pretreatment trends in the outcome measures between treated and untreated states is critical to ensure that states enacting restrictions are not trending differently in mental health, as compared with states not enacting such restrictions, before the restriction. To execute the event-study, we replace treatment with a vector of dummies reflecting an observation's temporal distance from the first full survey wave conducted after the restriction's enactment in a state. The estimates show the average effect of an abortion restriction on all four outcomes. We plot the coefficients associated with 15 survey waves before, and 15 waves after, restriction enactment in Fig. 1. The estimator is a DID-2S, which, as noted earlier, is robust to potential bias stemming from staggered treatments (24). We include the same controls as in our regressions above and cluster the SEs by state. Before enactment of abortion restrictions, states with and without abortion restrictions had similar trends for each measure, with significant differences emerging post treatment. In short, Fig. 1 is consistent with the assumption of parallel pretreatment trends and indicates that the effects persist over the course of the posttreatment sample.

We observe no consistent moderation of the relationship between mental health measures and the enactment of restrictions by race, sex, marital status, sexual orientation, or age (tables S8 to S12). We do observe significant moderation by self-reported income (Tables 2 and 3) and a similar pattern of moderation by level of education (table S13). Focusing here on income, in Tables 2 and 3, the main effect (*abortion ban*) reflects the average change in mental health among persons whose states implement an abortion restriction and who report a household income below \$25,000 (the omitted group). Among those with an income below \$25,000, abortion restrictions were associated with increased anxiety (0.0152; 95% CI of 0.0059 to 0.0245; Table 2), disinterest (0.0180; 95% CI of 0.0096 to 0.0264), worry (0.0114; 95% CI of 0.0018 to 0.0211), and feeling down (0.0100; 90% CI of 0.0014 to 0.0186). These estimates are larger and equate to between a 1.0 and 1.8 pp increase in reports of mental health concerns among low-income respondents.

The interaction between abortion ban and income in Table 2 reflects the relative change in responses to the mental health items for each 1-U increase on the income ordinal scale. A graphical representation of effects over income levels can be found in fig. S1. Replication with the DID-2S estimator is in table S7. All point estimates of the interaction terms are negative, exhibiting statistical significance in the case of anxious (-0.00196; 95% CI from -0.0039 to -0.00004), disinterest (-0.0034; 95% CI from -0.0054 to -0.0014), and worry (-0.0026; 95% CI from -0.0044 to -0.0007). These negative interactions imply that as income increases, mental health concerns are attenuated (i.e., the negative effects on mental health become weaker as wealth increases). These results are corroborated by Table 3, which indicates an attenuation of the negative impact as wealth increases. We observe similar results when moderating the effect by education (table S13), such that the main effect of the abortion ban is stronger on persons who did not complete their high school education. These effects attenuate as a person's level of education rises.

## DISCUSSION

In this work, we add to the growing body of evidence demonstrating that the institution of abortion restrictions is associated with increased mental distress of persons living in the US (11, 12, 14, 15). In doing so, we respond to calls by practitioners to address this potential link (13, 19, 26) and materially expand on prior work [which has only considered reactions to potential bans and trigger bans among women of childbearing age (14, 15)]. Results indicate significant increases in multiple measures of mental distress for persons living in abortion restricting states for the duration of the sample. We further find that these effects are concentrated among respondents of lesser financial means at their time of response and the less educated. While speculative, two possible explanations exist for why abortion restrictions would have larger effects on individuals of lesser means. On the one hand, this may be due to increases in the cost of travel to reach a state where abortion remains legal or increased costs of carrying a pregnancy (e.g., lost wages, costs of raising a child, etc.) (27, 28). Individuals of lesser means may experience distress tied to these higher costs, above and beyond any negative effects stemming from a perceived loss of individual rights. On the other hand, this effect may reflect the disproportionate use of abortion services by people of lesser financial means (29, 30). We leave determination of the specific mechanism to future research.

We observe no significant differences across other social factors, including sex, sexual orientation, marital status, or race, suggesting that the effects we identify may be consistently experienced across the populace. This could partly reflect greater levels of baseline mental distress in already marginalized groups, leaving less room for them to report further increases in distress. It also raises the possibility that the observed effects are not specifically or only attributable to reduced abortion access among women of childbearing age. To the extent that prior work had demonstrated the impact of *Dobbs* on permanent contraception for both men and women (*16*), an effect on individuals who are connected to affected women is not implausible. While speculative, changes in mental health may also be driven by broader perceptions of shifts in policy, the loss of long-standing individual rights, and the changing of institutions, such as those signaled by Justice Thomas's concurrence in *Dobbs*, which people may view negatively.

Numerous implications stem from these findings. As scholars have pointed out (26), intense disinformation exists about the relationship between abortion and mental health. Much of the scholarship which claims that having a first trimester abortion can have a negative effect on mental health is characterized by severe methodological flaws



**Fig. 1. Event-study estimates of abortion restrictions.** Estimated increase (95% CI) in each mental health domain in restriction states relative to changes observed in nonrestriction states, across time (survey waves), from 15 study waves before through 10 study waves after enactment of abortion restrictions (DID-2S). Models are adjusted for sex, age, race, education, sexual orientation, and marital status, with SEs clustered by state. Note that the *x* axis is indexed at 0, such that survey wave 0 is the first full survey wave conducted following institution of an abortion restriction.

Table 2. Income-moderated effect of abortion ban or gestational limit (TWFE). SEs in parentheses clustered by state; control variables include flexible dummies capturing sex, age, education, marital status, sexual orientation, and race, including dummies for nonresponse where appropriate. Income is the raw ordinal survey measure, with 0 implying a reported annual income of less than \$25,000. Responses that lack income information are omitted from the estimation. Observation counts differ across estimations due to variation in nonresponse across each mental health survey item.

DV	Anxious	Disinterested	Worried	Down	
Abortion ban	0.01517*** (0.00463)	0.0180**** (0.00417)	0.0114** (0.00478)	0.0100* (0.00512)	
Income level	-0.02731**** (0.00076)	-0.0254**** (0.00076)	-0.0271**** (0.00066)	-0.0252**** (0.00061)	
Abortion ban $\times$ income level	-0.00196* (0.00115)	-0.0034** (0.00120)	-0.0026** (0.00109)	-0.0020 (0.00124)	
Controls	Yes	Yes	Yes	Yes	
Observations	1,286,889	1,285,214	1,285,301	1,285,841	
R <sup>2</sup>	0.087	0.073	0.079	0.081	
Fixed effects	State and survey wave				

\*P < 0.10. \*\*P < 0.05. \*\*\*P < 0.01. \*\*\*\*P < 0.001.

Table 3. Income-moderated effect of abortion ban or gestational limit. SEs in parentheses clustered by state; control variables include flexible dummies capturing sex, age, marital status, income, sexual orientation, education, and race, including dummies for nonresponse where appropriate. The income dummy associated with a reported annual income of <\$25,000 is omitted and thus serves as the reference group. Moderated estimates associated with nonresponse to the income question are omitted for brevity. Observation counts differ across estimations due to variation in nonresponse across each mental health survey item. TWFE, flexible income dummies.

DV	Anxious	Disinterested	Worried	Down	
Abortion ban	0.0062 (0.0074)	0.0162** (0.0072)	0.0124* (0.0064)	0.0119* (0.0068)	
Abortion ban × income: \$25,000–34,999	0.0082 (0.0083)	-0.0043 (0.0083)	-0.0056 (0.0062)	-0.0065 (0.0076)	
Abortion Ban × income: \$35,000–49,999	0.0083 (0.0075)	-0.0817 (0.0083)	-0.0075 (0.0066)	-0.0129** (0.0057)	
Abortion ban × income: \$50,000–74,999	0.0009 (0.0095)	-0.0112 (0.0093)	-0.0145* (0.0080)	-0.0101 (0.0081)	
Abortion ban × income: \$75,000–99,999	-0.0015 (0.0070)	-0.0140 (0.0088)	-0.0118 (0.0080)	-0.0147* (0.0078)	
Abortion ban × income: \$100,000–149,999	0.0123 (0.0093)	-0.0074 (0.0077)	-0.0054 (0.0074)	0.0005 (0.0086)	
Abortion ban × income: \$150,000–199,999	-0.0288*** (0.0078)	-0.0268*** (0.0099)	-0.0300**** (0.0081)	-0.0243*** (0.0077)	
Abortion ban × income: \$200,000+	-0.0074 (0.0074)	-0.0250*** (0.0091)	-0.0137 (0.0090)	-0.0167* (0.0086)	
Controls	Yes	Yes	Yes	Yes	
Observations	1,407,754	1,404,812	1,405,252	1,405,480	
R <sup>2</sup>	0.084	0.071	0.077	0.079	
Fixed effects	State and survey wave				

(e.g., selection issues and omitted variable biases) (31, 32). As a result, the medical and scientific communities have vigorously pushed back and illustrated the flaws in such claims (11, 33-36). Our findings go one step further, showing that restricting access to abortion services, which many—including the American College of Obstetrics and Gynecology (37)—consider a cornerstone of reproductive health care, may have the negative effects on mental health.

Our work has implications for public health officials and physicians. Physicians must be aware that patients living in jurisdictions with enacted restrictions may experience heightened levels of mental distress, especially the less wealthy and less educated. This underscores the need for information campaigns communicating how and where individuals may access mental health services. To the extent that our findings replicate using clinically validated screening tools (e.g., PHQ-2, PHQ-4, and GAD-2), our results also suggest that the medical community may lean on these measures to identify patients at risk. The psychologists and psychiatrists who care for such patients should also be aware that these effects may not manifest exclusively among women or the heterosexual community (14).

Implications also exist for lawmakers. Numerous paths have been taken across the country to either restrict or liberalize access to abortion services in the wake of *Dobbs*. These include trigger bans, legislation, and state constitutional amendments [see the Center for Reproductive Rights for a complete discussion (2)]. While it is now the purview of the electorate in each state to make decisions regarding the legality of abortion (the central holding in *Dobbs*), legislators should be aware of the effects such changes can have on constituents.

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This work offers opportunities for future scholarship. Specifically, the lack of moderating effects across various social factors (e.g., sex, marital status, race, and sexual orientation) raises the possibility that the reaction to abortion restrictions may result from more general changes in the political environment rather than an exclusive reaction to changes in reproductive health care. We leave the determination of the mechanism behind the observed effects, be they general or abortion specific, to future scholarship.

This work is subject to limitations. First, although we observe self-reported mental health across a variety of respondents, we are unable to access clinical diagnoses by trained professionals, despite replication with the PHQ-2, PHQ-4, and GAD-2 measures. This is a limitation of available data. To our knowledge, no large-scale administrative data exist that capture such information, apart from Medicaid and Medicare records which have obvious sampling restrictions. Second, the states that have enacted restrictions tend to be more conservative, meaning that restrictions are not assigned at random. Although our results indicate that mental health measures were comparable across states, once conditioned upon controls, and only diverged after institution of restrictions, a finding consistent across estimators (24), the potential remains that the effects we estimate might manifest differently were bans to be instituted in more liberal states. Accordingly, the generalizability of our estimates to more liberal states should be considered cautiously. In addition, we evaluated changes in mental health before and after the date of abortion restriction enactment in each state. As a result, it is possible that other changes, unknown to the team, are driving the changes in mental health rather than the institution of abortion restrictions, i.e., an omitted variable bias. However, these other changes would need to have occurred in near perfect concert with changes to each state's abortion restriction, which seems unlikely. Last, while we observe no consistent effect across factors like sex or race, it is possible that these estimations are simply underpowered.

In conclusion, in this work, we examined the relationship between the institution of abortion restrictions, stemming from the *Dobbs* decision, and mental health. Results from a multiyear difference in difference estimation involving survey responses from than 1.4 mm people living in the US indicate that restrictions give rise to increases in anxiety, worry, disinterest, and feeling down, particularly among the socioeconomic lower class. These effects are consistent across race, sex, and sexual orientation and persist over the course of the sample.

#### MATERIALS AND METHODS Data sources

Consistent with prior work, we measure the effects of abortion restrictions on mental health using data from the national Household Pulse Survey (HPS) conducted by the US Census Bureau (14, 15). The HPS is a 20-min survey developed to study the economic impact of the COVID-19 pandemic on labor markets. To be included in the study, respondents must be at least 18 years of age, have a fixed address in the Census Bureau's master address file, and have internet access through either a mobile phone or home internet provider. Use of the survey has subsequently broadened to address a host of other issues, including financial well-being (38), education (39), and the mental health of those facing abortion bans (14, 15). Respondents to the survey are contacted via email or short message service (SMS) and receive three follow-up reminders to improve response rates. Summary statistics can be found in table S2.

Our sample encompasses HPS waves 34 to 58 (July 2021 through June 2023), yielding a relatively balanced window of observations around the first state abortion restrictions (the bulk of state restrictions were introduced between June and September of 2022). Each wave of the HPS includes a representative sample of more than 60,000 respondents with a response rate between 5 and 7%. As with all Census surveys, the survey includes weights to adjust for nonresponse, enabling us to obtain accurate state-level estimates (40).

We combine the HPS data with information on the timing of abortion restrictions. We focus on a joint indicator of bans or gestational limits which would not be permitted under *Roe*. Results are consistent if we consider bans and gestational limits separately. These data are drawn from the Center for Reproductive Rights. A treatment schedule is in table S1. We do not consider restrictions that have been enjoined by courts, as those changes have not yet taken effect.

# **Dependent variables**

We consider four outcomes, all of which are survey items related to respondent mental health on the HPS. For each, the survey preamble states "Over the last 2 weeks, how often have you been bothered by the following problems ..." followed by the relevant wording. For the four items, *anxiety* ("feeling nervous, anxious, or on edge"), *disinterest* ("having little interest or pleasure in doing things"), *worry* ("not being able to stop or control worrying"), and *depression* ("feeling down, depressed, or hopeless"); subjects reply on a four-value scale: not at all, several days, more than half the days, or nearly every day. We dichotomize the response, coding a response of either "more than half the days" or "nearly every day" as one and zero otherwise. To ensure consistency with established mental health screening measures (*25*), we also consider effects on three outcomes which are composites of our four survey items, namely, PHQ-2, PHQ-4, and GAD-2. These are commonly used to screen patients for clinical depression and general anxiety disorder in clinical settings. Consistent effects are observed.

## Independent variable

We regress our outcomes on a binary indicator, *abortion ban*, reflecting whether the respondent's state instituted either an outright ban on abortion or a more restrictive gestational limit before the start of the focal survey wave. Given the disproportionate use of abortion services by women of lower socioeconomic status, we also consider household *income*. Omitting responses where *income* was not reported, we interact the ordinal income response with our treatment. We also consider an alternative specification, constructing a vector of dummies reflecting each possible income response, including nonresponse. We then interact these dummies with our treatment, income below \$25,000 serving as the base.

## **Control variables**

We control for a variety of respondent characteristics. *Sex* (female) is a binary indicator based on self-reported "gender assigned at birth." *Age* is measured using a vector of dummies reflecting self-reported year of birth. We discretize the age measure into the following ranges: 18 to 24, 25 to 49, 50 to 74, and 75 to 99, allowing for the capture of flexible, nonlinear effects of age, due to variation in fertility. *Marital status* is a vector of dummies reflecting categorical responses from the survey, as are *race, education*, and *sexual orientation*.

## Statistical analysis

To identify the effect of abortion bans on mental health, we use a difference-in-differences design, exploiting the staggered implementation of restrictions across states over time (41). Respondents are designated as treated if a restriction is enacted in their state. We repeat each of our estimations four times, once per mental health survey item: *anxious, disinterest, worry*, and *down*. Estimations include state and survey-wave fixed effects. Formally

$$Y_{i,s,t} = \text{Abortion Ban}_{s,t} + C_i + \delta_s + \tau_t + \epsilon_{i,s,t}$$
(1)

This regression is a variant of a two-way fixed effect estimator, where Y reflects mental health survey response. Subscripts, i, s, and t index survey respondents, states, and survey wave, respectively. Abortion ban is our treatment of interest, reflecting the implementation of either an outright ban or a gestational limit. C is the set of controls,  $\delta$  indexes state fixed effects,  $\tau$  indexes surveywave fixed effects, and  $\epsilon$  is the error term. SEs are clustered by state (42). We then interact the abortion ban dummy with a variety of moderating factors, including sex, sexual orientation, marital status, race, and income. We focus on the income-moderated regressions; most other factors did not significantly moderate treatment, the exception being a respondent's level of education, which exhibited a pattern of moderation similar to income. For brevity, we report these other regressions in the statistical appendix. Last, in deference to recent work on DIDs involving staggered treatments (43), we replicate our estimations using a DID-2S

estimator, which is robust to potential biases arising from the combination of staggered treatments and heterogeneous treatment effects (24). Results remain consistent and are in the statistical appendix (table S3).

# **Supplementary Materials**

This PDF file includes: Supplementary Text Fig. S1 Tables S1 to S14 References

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