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Miscarriage, self-harm, and psychiatric disorders in first-time pregnant women: Evidence from a linkage study

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ABSTRACT

We leveraged the biological evidence that a first pregnancy ending in miscarriage is considered a quasi-exogenous shock to fertility and linked electronic health records to estimate adjusted associations between miscarriage and self-harm and psychiatric outcomes. In a random cohort of 1.2 million women aged 16 to 50, all first recorded pregnancies between 01/01/2004 and 31/12/2017 were identified using data linked from health registries in England, UK. Each first pregnancy was subsequently categorized into one of two mutually-exclusive groups: miscarriage vs continued pregnancy using valid medical definitions. For each outcome-specific model, we excluded women with a prior recorded diagnosis of the same outcome before first pregnancy. Our empirical strategy relied upon methods under the selection on observables assumption (logistic regression, the augmented-inverse probability weighting estimator, and entropy balancing) to estimate the effects of miscarriage. Miscarriage was associated with higher adjusted odds of self-harm at 6 months (OR 2.30), depression at 6 months (OR 1.50), and anxiety at 6 months (OR 1.25), with the self-harm association persisting up to three years (OR 1.60). Associations with self-harm differed by area-level deprivation: no statistically significant association was observed in the least deprived quintile, whereas elevated odds were observed in more deprived quintiles. Targeted interventions such as counselling aimed at ensuring that women who miscarry have access to healthcare services are required to mitigate possible harms caused by early pregnancy losses.

1. Introduction

Miscarriage, defined as the loss of a pregnancy before fetal viability (Quenby et al., 2021), is the most common adverse outcome of pregnancy (Jurkovic et al., 2013). It affects between 15 and 30% of all pregnancies (Linnakaari et al., 2019; Quenby et al., 2021; Stephenson and Kutteh, 2007) and can cause deleterious clinical consequences (Coomarasamy et al., 2021; Quenby et al., 2021; Rai and Regan, 2006). While miscarriage has been linked to adverse mental health outcomes in cohort studies (Bolbocean et al., 2026; Quenby et al., 2021), its effect on *new* mental health or self-harm diagnoses across primary and secondary care remains unclear (Coomarasamy et al., 2021; Quenby et al., 2021).

Miscarriage may lead to self-harm or mental health or psychiatric disorders because the loss of a pregnancy often leaves women, their partners, and families grieving, thereby increasing susceptibility to prolonged or excessive grief reactions (Adolfsson et al., 2004; Keefe-Cooperman, 2005). First documented over sixty years ago (Benedek, 1959; Benedek and Liebman, 1958; Bowlby, 1973, 2018; Cranley, 1981), miscarriage abruptly and irreversibly ends the maternal–fetal relationship. It is often stigmatized, leading to unwarranted guilt and self-blame (Banno et al., 2020; Bardos et al., 2015; San Lazaro Campillo et al., 2018). In addition, only a minority of women share their experiences with their broader social networks or seek treatment and support (Bellhouse et al., 2018), and many experience a range of negative emotions, such as loss of confidence, grief, and despair (Adebayo et al., 2019; Adolfsson et al., 2004; Gerber, 2017; Keefe-Cooperman, 2005; Swanson, 1999) which might directly impact self-harm outcomes.

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Recent literature documents large earnings penalties (35–74% lower earnings) for individuals with psychiatric conditions in the United States (Biasi et al., 2021). Self-harm outcomes are particularly costly to the healthcare system, women, and their social networks (Corredor-Waldron and Currie, 2023; Marcotte and Hansen, 2023). For instance, a study in England found that more than 200,000 people attend hospital for self-harm each year, at a cost exceeding £128 million (Tsiachristas et al., 2020). Beyond these economic costs, self-harm carries profound social and emotional consequences, including lasting psychological distress for affected individuals, strain on family and interpersonal relationships, stigma, social isolation, and broader community impacts through loss of social participation (Hoven et al., 2010; Townsend et al., 2001). Identifying those at risk is critical because each episode of self-harm increases the likelihood of future episodes (Colman et al., 2004) and can ultimately lead to suicide (Zahl and Hawton, 2004).

Self-harm and psychiatric outcomes are closely related because the risk of self-harm intensifies in the presence of multiple psychiatric conditions and risk factors. Depression or anxiety (Busch et al., 1993; Fawcett et al., 1993; Frank and McGuire, 2000; Hansen et al., 2022; Orbach et al., 2003; Rees et al., 2021) are among the strongest predictors of self-harm. Negative emotions stemming from miscarriage may further interact with suicidal cognitions and increase self-harm risk (Townsend et al., 2001). Thus, accurate data on the effect of miscarriage on self-harm and psychiatric outcomes is vital to inform preventative efforts against mental ill-health and suicide (Hoven et al., 2010).

Importantly, both miscarriage and self-harm are shaped by social determinants that are not uniformly distributed across the population. Miscarriage risk is associated with socioeconomic deprivation, ethnicity, maternal age, occupational exposures, and barriers to accessing timely healthcare (Coomarasamy et al., 2021; Quenby et al., 2021). Similarly, the social determinants of self-harm extend beyond individual psychological factors to include poverty, social isolation, housing instability, exposure to adverse childhood experiences, and relationship instability (Townsend et al., 2001; Tsiachristas et al., 2020). These overlapping social determinants mean that women who are already socially and economically disadvantaged may face a disproportionate burden from both miscarriage and its psychiatric sequelae. Consequently, it is important not to treat women as a homogeneous group; pronounced social and economic inequities exist among them that shape both their risk of miscarriage and their vulnerability to adverse self–harm and psychiatric outcomes. This study examines heterogeneity in self-harm and psychiatric outcomes by area-level socioeconomic deprivation through stratified analyses.

Although numerous studies have reported an increased risk of adverse psychiatric health outcomes following miscarriage (Brier, 2008; Broen et al., 2005; DeMontigny et al., 2017; Farren et al., 2020; Jacob et al., 2017; Kersting and Wagner, 2022), none have specifically examined new psychiatric diagnoses in detail or analyzed potential impacts on self-harm. Moreover, miscarriage may serve as a traumatic stressor associated with post-traumatic stress disorder (PTSD), bipolar disorder, obsessive-compulsive disorders (OCD), or psychosis, although evidence in these areas is sparse (Di Florio et al., 2015; Geller et al., 2001; Magnus et al., 2021; Neziroglu et al., 1992; National Health Service, 2024; Sham et al., 2010).

Despite these documented associations, the overall evidence on miscarriage and subsequent psychiatric outcomes is constrained by several methodological limitations, including weak research designs and restricted use of population-based health care records. There are significant knowledge gaps in our understanding of the relationship between early pregnancy loss and the subsequent risk of self-harm or psychiatric disorders. For example, research in the United States has struggled to accurately quantify early pregnancy losses because patients' lifetime health records are not stored in a single unified database, and universal patient identifiers are prohibited by law(Payne et al., 2019). Consequently, studies have not been able to differentiate sporadic (up to two miscarriages) from recurrent (at least three consecutive) miscarriages (Coomarasamy et al., 2016; Quenby et al., 2021), or to distinguish outcomes of a first pregnancy from subsequent ones. In contrast, UK population-based health registries can reliably capture miscarriages through hospital records (often required for severe cases) or via primary care (Quenby et al., 2021).

Many published studies also rely on associations that do not address pre-existing risk factors or prior pregnancies (Brier, 2008; Broen et al., 2005; DeMontigny et al., 2017; Farren et al., 2020; Kersting and Wagner, 2022), nor do they fully address the issue of reverse causality (i.e., a prior outcome). Most have employed small samples (Cubo Nava et al., 2019; Farren et al., 2020; Hughes et al., 1996; Petrou and McIntosh, 2009; Petrou et al., 2006; Prior et al., 2017; Rausch et al., 2012), emphasized selected outcomes over short-term periods, or relied on retrospective recall methods (Coomarasamy et al., 2016; Cubo Nava et al., 2019; Dalton et al., 2006, 2015; Farren et al., 2020; Graziosi et al., 2005; Hughes et al., 1996; Lemmers et al., 2018; Petrou and McIntosh, 2009; Petrou et al., 2006; Prior et al., 2017; Rausch et al., 2012; Tasnim et al., 2014, 2011; van den Berg et al., 2015; You and Chung, 2005), which are prone to recall bias. Few have leveraged large population-based cohorts, and many are limited to a single geographic region or sociodemographic group, raising concerns about external validity, recall bias, social desirability bias, and reverse causality. Understanding whether miscarriage increases the risk of self-harm or psychiatric disorders is crucial for improving clinical awareness and preventative efforts in antenatal and community care. It also highlights the significance of exploring biological mechanisms underlying miscarriage. These methodological concerns underscore the need for rigorous investigations into how miscarriage affects self-harm and psychiatric outcomes.

This study aimed to overcome these challenges and fill gaps in the literature by providing novel evidence on how miscarriage affects first-time pregnant women. The primary aim of this study was to estimate the effect of a first-pregnancy miscarriage on the incidence of new self-harm episodes and new psychiatric diagnoses — depression, anxiety, PTSD, OCD, psychosis, and bipolar disorder — at 6-month, 1-year, and 3-year follow-up, using linked NHS primary- and secondary-care records in England. We used data from a random cohort of 1.2 million women in the QResearch database, which covers over 1500 general practices across the UK and is broadly representative of the population in England in terms of age, ethnicity, and region; all pregnancies that involved contact with the National Health Service (NHS) in England between 2004 and 2017 were identified. We exploited the unique structure of QResearch's linked primary- and secondary care records to identify each woman's first-ever NHS recorded pregnancy and to compare those whose first pregnancy ended in miscarriage against those whose pregnancy continued. The focus on *first* pregnancies allows us to treat miscarriage as a plausibly random shock to fertility, since approximately 80% of all miscarriages are caused by random, non-viable chromosomal abnormalities (Banno et al., 2020). Full details of the cohort, identification strategy, and the robustness and sensitivity tests are reported in the Methods and Sensitivity sections.

Throughout this study, we use the term "women" to refer to individuals recorded as female in NHS electronic health records, which classify patients by sex registered at birth. We acknowledge that transgender men and non-binary individuals assigned female at birth may also experience pregnancy and miscarriage, and the findings of this study may be relevant to these groups.

2. Methods

2.1. Study background and sample

The UK's publicly funded healthcare system, the NHS is a service available universally that cares for people on the basis of need and not ability to pay and is primarily funded by taxes and national insurance contributions (Grosios et al., 2010; Light, 2003). Health care in the UK is

provided via primary care (community care, general practitioners (GPs), dentists, pharmacists, etc.), secondary care (hospital-based care largely accessed through GP referral) and tertiary care (specialist hospitals). We identified all registered pregnancies between 1 January 2004 and 31 December 2017 in a random cohort of 1.2 million women in England aged 16 to 50 years through the QResearch database and followed them until 31 December 2020. QResearch is derived from the anonymised health records of over 35 million patients from more than 1500 general practices across the UK. The practices record data using the Egton Medical Information Systems. Recorded information includes data on patients' diagnoses, symptoms, consultations, referrals, test results, prescriptions and socioeconomic variables. Electronic primary health care data reported in the QResearch database are linked to Hospital Episode Statistics (HES) at the patient level and provide information on all contacts with primary and secondary health care services during pregnancy, including contacts during the first trimester. We linked patients' records across the health registries using unique NHS numbers.

Eligible women had to have all their health histories continuously recorded with QResearch and were required to have at least three years of follow up time following the start of their first pregnancy. Continuous registration means that any consultations, prescriptions, referrals, or hospital contacts in the window would have been captured in the woman's record had they occurred; women without continuous registration were excluded because, for them, the absence of a recorded diagnosis could not be distinguished from non-observation. The QResearch database records sex as registered at birth in NHS records. Our study sample comprises individuals recorded as female in these records. The database does not separately capture gender identity; therefore, the term “women” in this study refers to individuals classified as female based on their registered sex.

2.2. *Identification of unique pregnancies and definitions of miscarriage and outcomes*

Robust clinical definitions using Read, Systemised Nomenclature of Medicine (SNOMED-CT) and ICD-10 codes were employed to ascertain all clinical variables utilised in the study: miscarriage, continued pregnancy, self-harm and psychiatric outcomes. In our dataset, a total of 582,313 women out of the random sample of 1.2 million. A total of 315,546 women were excluded because the pregnancy record was prior to January 1, 2004, which resulted in a sample of 266,767. Furthermore, 22,725 women were excluded because the first pregnancy was recorded from January 1, 2018 onwards resulting in a sample of 244,042 first pregnancies. Finally, 1599 records were excluded because age at first pregnancy fell outside the boundaries of 16 to 50 years, yielding a final analytic sample of 242,443.

We identified the first pregnancy for each woman in our sample and categorized women into two mutually-exclusive groups: first-time women who miscarried vs first-time women with a continued pregnancy. We defined miscarriage as a loss of a wanted pregnancy and we used diagnostic Read, SNOMED-CT and ICD-10 codes to identify the following clinical conditions: blighted ovum and non-hydatidiform mole (O02.0); missed abortion (O02.1); other specified abnormal products of conception (O02.8); abnormal product of conception, unspecified (O02.9) and spontaneous abortion (O03). A missed abortion refers to an in utero death of the embryo or fetus before the 20th week of gestation, where the products of conception are not immediately expelled. Appendix A provides a list of the diagnostic Read, SNOMED-CT and ICD-10 codes used to define miscarriage within this study. Appendix Table A.1 summarises the exposure, outcome, prescription, covariate, and eligibility variables. Because miscarriage is personal and traumatic, some cases may not be captured in medical records(Adolfsson and Larsson, 2006; Li et al., 2016; Linnakaari et al., 2019; Rasmark Roepke et al., 2017). Using the primary care records we found the following prevalence rates for the outcomes considered broadly matched previous reports from the literature (Magnus et al., 2021): self-harm (2.6%), depression (11.9%), anxiety (15.9%), bipolar disorder (0.1%), psychosis (0.8%), PTSD (0.6%), obsessive compulsive disorder (OCD) (0.6%) and eating disorders (1.9%)) experienced pregnancies which did not end in abortion and were identified using Read, Systemised Nomenclature of Medicine (SNOMED-CT) and ICD-10 codes. Panic and eating-disorder diagnoses were identified at low frequency in primary care and are not reported as separate outcomes in Table 3.

In this study, we excluded all first-time pregnancies that ended in medical abortion because these represent elective terminations rather than spontaneous, non-elective pregnancy losses. Terminations for medical reasons (TFMR) were not separately identifiable in the available data extract. In practice, medical abortion codes (e.g. ’Z’ in ICD-10) are distinct from spontaneous abortion codes (O03). Cases with ambiguous codes were excluded to avoid misclassification. Stillbirths, which occur at or beyond the point of fetal viability, were classified within the continued pregnancy group. This approach aligns with our definition of miscarriage as a spontaneous loss before viability and ensures that only non-elective, early pregnancy losses are considered in the miscarriage category.

We identified diagnostic codes (Read, SNOMED-CT, and ICD-10) for the following outcomes in this study: any episode of self-harm irrespective of suicidal intent, depression, anxiety, PTSD, OCD, psychosis and bipolar disorder. We grouped antidepressant drugs for analysis according to the major classes of antidepressants as described in the British National Formulary (Committee, 2012): tricyclic and related antidepressants (TCAs), monoamine oxidase inhibitors (MAOIs), selective serotonin reuptake inhibitors (SSRIs/SNRIs), and atypical/other antidepressants that have been also been used in previous studies (Coupland et al., 2011; Jack et al., 2020; Vinogradova et al., 2019). Outcomes were considered within the following time frames: 6 months, 1 year, and 3 years follow-up after study entry. We focused on 6months, 1year, and 3years because they represent short-, medium-, and longer-term postpartum intervals, commonly used to monitor mental health changes after pregnancy-related events (Jurkovic et al., 2013; Linnakaari et al., 2019; Quenby et al., 2021; Stephenson and Kutteh, 2007). This approach captures both acute and more sustained impacts.

Women who miscarried and women with a continued pregnancy entered the study at the earliest recorded date of the first pregnancy according to the electronic health records. Because of the known stigma associated with psychiatric, mental health and self-harm diagnoses (Gaiha et al., 2020; Rossler, 2016), the electronic health registries might not fully capture all cases of these conditions (Clement et al., 2015; Schomerus and Angermeyer, 2008).

Our data do not allow us to identify the exact medical indication for each antidepressant prescription. This is important because, in the UK, as in other developed countries, TCAs, MAOIs, and SSRIs may be prescribed for reasons other than the psychiatric and self-harm outcomes considered in this study, including pain management, gastrointestinal disorders, urological conditions, and migraine (Birkinshaw et al., 2023, Burch, 2019, Ilari et al., 2022, Koch and Jürgens, 2009, Mercier et al., 2013; National Health Service, 2024; Papandreou et al., 2009; Törnblom and Drossman, 2018). For example, the NHS notes that antidepressants may also be used for chronic non-neuropathic pain, including fibromyalgia, chronic back pain, and chronic neck pain (National Health Service (NHS), 2024). In our study, among eligible women, 30,790 or 12.7% had an antidepressant prescription prior to the first pregnancy. Excluding all women with any prior antidepressant exposure would therefore remove a clinically heterogeneous group whose exposure may have been unrelated to the outcomes considered in this study, and could introduce selection bias. Furthermore, excluding women on the basis of any prior antidepressant prescription would condition the analytic sample on a pre-pregnancy history characteristic that might be itself associated with the outcomes of interest, since prior antidepressant exposure may predict subsequent psychiatric morbidity. We therefore retained women with prior use of TCAs, MAOIs, or SSRIs in the main analysis. However, to assess whether this decision influenced the

findings, we also conducted a planned sensitivity analysis in which the main models were repeated after restricting the sample to women with no documented antidepressant prescription prior to the first pregnancy.

2.3. Estimation of treatment effects under selection on observables

We adopt the standard potential outcomes setup (Holland, 1986), where $Y_i(1)$ and $Y_i(0)$ denote the potential outcomes for woman i under miscarriage ($M = 1$) and no miscarriage ($M = 0$), respectively. The estimation of average treatment effects ($ATE = E[Y(1) - Y(0)]$ (ATE), the average treatment effect on the treated $ATT = E[Y(1) - Y(0)|M = 1]$ (ATT), and the average treatment effect on the controls $ATC = E[Y(1) - Y(0)|M = 0]$ (ATC)) require selection on observables assumption:

$$Y_i(1), Y_i(0) \perp M_i \mid X_i,$$

where X_i is a vector of observed covariates. In addition, overlap (i.e., $0 < P(M = 1 \mid X) < 1$) and no interference are assumed. While these conditions justify a causal interpretation of the miscarriage effect, any violation—particularly unobserved confounding—may bias estimates. We view the empirical approach given the data we employ as providing more robust measures of association rather than causal effects. Section 3 describes additional tests and sensitivity analyses to address such concerns.

Before formal modeling, we compared baseline characteristics of women who experienced a miscarriage versus those whose pregnancy continued. Continuous variables were evaluated using the Student t-test (with unequal variances); Fisher's exact test was employed for categorical variables. We additionally ran Kolmogorov–Smirnov tests to examine distributional differences in key covariates (e.g., age, year of pregnancy, prior hospital admissions).

Potential Endogeneity of Miscarriage. We assessed whether miscarriage might be systematically related to observable characteristics by estimating a logistic model:

$$P(M_i = 1 \mid X_i) = \Lambda(\gamma_0 + \gamma_1 X_i),$$

where X_i includes age (with a squared term), year of pregnancy, area deprivation (Townsend Index quintile), self-reported ethnicity, region of England, and total prior hospital admissions. This model underlies our propensity score calculations for matching methods and informs which factors most strongly predict treatment status.

We use logistic regression as our main approach, supplemented by augmented-IPW and regression adjustment matching to address model dependence, and partial identification methods to check sensitivity to unobservables. Below, we briefly outline each method:

Logistic Regression Specification To estimate the effect of miscarriage on each binary outcome (e.g., new onset of self-harm or a specific psychiatric disorder), we employed logistic regression:

$$\ln\left(\frac{Y_{i,t}}{1 - Y_{i,t}} = \beta X_i + \tau M_i + \varepsilon_i\right), \quad (1)$$

where $Y_{i,t}$ represents the new onset of a particular adverse outcome for pregnant woman i at time t (6 months, 1 year, or 3 years after the first pregnancy), and is only defined for women with no prior history of that same outcome before their first pregnancy. For example, when examining miscarriage effects on depression, we exclude women who already had a depression diagnosis but do not exclude those with other diagnoses (e.g., self-harm or PTSD or anxiety etc.).

We used the Pregibon link test (Pregibon, 1980) to confirm the suitability of the logit link. Random effects at the general practice level address potential correlation within practices, while preserving sample size (see Section 3 for further discussion).

Augmented Inverse-Probability Weighting (AIPW) and Regression Adjustment Matching (RA) Although logistic regression with rich covariates can help mitigate confounding, it remains sensitive to functional form and model misspecification. We therefore implemented two additional approaches—AIPW and RA matching—to reduce reliance on a single parametric specification.

We utilised AIPW to weight linear outcome regression models to construct the so-called double robust (DR) estimator (Wooldridge, 2007). DR methods combine models for the propensity score and for the outcome/endpoint. DR estimators are unbiased if one specifies at least one of the models correctly, i.e. the propensity score model or the outcome model. The DR estimators are consistent if either (but not necessarily both) the propensity score or the regression model is correctly specified (Bang and Robins, 2005). If both components are correct, the DR estimator is a semiparametric efficient estimator (Robins et al., 2007). We used the augmented-inverse probability weighting estimator (Basu et al., 2011; Funk et al., 2011), which augments the IPW by a weighted average of the outcome model (Kurz, 2022).

We used RA matching because the literature suggests that matching should be followed by regression adjustment (Abadie and Imbens, 2006) as the regression is used to "clean up" any remaining imbalances between treatment groups after matching (Stuart, 2010). We used regression-adjusted matching undertaken as a two stage process: matching was followed by applying parametric regression models to the matched data (Ho et al., 2007). This estimator is also DR. This approach can reduce finite sample bias and increase efficiency compared to matching alone (Ho et al., 2007).

We specify identical covariates in both the propensity score and outcome regression steps. These alternative estimators allow us to assess the robustness of our findings to different modeling approaches; detailed results appear in Results Section. Finally, given that socioeconomic status may moderate the effect of miscarriage on self-harm, we also perform a stratified logistic regression by Townsend Index quintile. This approach explores heterogeneity in the miscarriage-outcome relationship across different deprivation levels.

Analyses were conducted in the statistical package STATA (Version 17) and p-values of 0.05 or less were considered statistically significant.

2.4. Covariates

Covariates included in the multivariable models were motivated by their documented associations with both the likelihood of experiencing a miscarriage (the "treatment") and subsequent mental health outcomes, drawing on previous literature (Coomarasamy et al., 2016; Jacob et al., 2017; Quenby et al., 2021). Specifically, our model accounts for: age at first pregnancy and a squared term for age, year of pregnancy, self-reported ethnic group, region of England, patient-level area deprivation measure, Number of hospital admissions prior to the first pregnancy and general practice effects. Below we provide the rationale for each variable.

Age is strongly correlated with the biological risk of miscarriage and may also shape mental health risks. Including a squared term allows for non-linear age effects, as miscarriage rates and mental health outcomes can vary non-linearly with age. Temporal trends in healthcare access, societal attitudes, and diagnostic practices can affect both miscarriage detection and mental health diagnoses. Prior research identifies differences in both miscarriage rates and mental health outcomes across ethnic groups, possibly due to genetic predispositions, cultural factors, or differential access to health services. Regional factors capture variations in healthcare infrastructure and socio-cultural norms that might jointly influence miscarriage rates and mental health outcomes. Patient-level area deprivation assessed using the Townsend Index captures community-level socioeconomic conditions. Prior findings (Fletcher and Wolfe, 2009) indicate that miscarriage rates can correlate with certain unobserved community-level factors. To mitigate this, we control for socioeconomic conditions at the census-tract level using the Townsend Index of Socioeconomic Deprivation, which includes unemployment rates among economically active adults, the prevalence of households without vehicle access, the proportion of rented households, and the

degree of overcrowded housing (Townsend et al., 1988). Each component is standardized, and their sum provides a composite measure of deprivation, thus helping us adjust for local contexts that might influence both miscarriage and mental health outcomes. The higher the Townsend Index score, the more deprived and disadvantaged an area is thought to be. Specifically, quintile 1 indicates the least deprived and quintile 5 indicates the most deprived. Hospital admissions serve as a proxy for underlying health status and healthcare engagement. Individuals with frequent hospital contacts may have pre-existing conditions placing them at higher risk for complications, including miscarriage and mental health disorders. General practice (random effects) accounts for practice-level unobserved heterogeneity. Certain practices may record or diagnose miscarriages or mental health conditions differently, and patient populations can vary by practice. Fixed effects specifications are explored as a robustness check.

By including these covariates, we aim to reduce the risk that omitted variables simultaneously drive both the probability of miscarriage and subsequent mental health outcomes; yet, residual unobserved confounding cannot be entirely ruled out. Section 4 details further sensitivity checks.

3. Robustness checks and sensitivity analyses

Our identification strategy relies on the selection-on-observables assumption: the causal effect of miscarriage on mental health and self-harm is identified only if all relevant confounders are observed.

Miscarriage is commonly regarded as a plausible exogenous biological shock to fertility, unlikely to be correlated with unobserved variables that directly influence self-harm or mental health outcomes (Hardy et al., 2016; Kaandorp et al., 2014; Kolte et al., 2014; Philipp and Kalousek, 2002; Quenby et al., 2021). Nevertheless, many early pregnancy losses arise from unknown causes (Adolfsson and Larsson, 2006; Lund et al., 2012; Quenby et al., 2021). To address these potential threats to identification, we undertake a series of robustness checks and sensitivity analyses. Specifically, we (i) implement partial identification bounds to test for selection on unobservables, (ii) conduct permutation tests and correct for multiple hypothesis testing, (iii) carry out placebo tests with outcomes that should not be affected by miscarriage, and (iv) employ alternative estimation strategies using entropy balancing and fixed effects models.

3.1. Tests for selection on unobservables: bounds under partial identification approach

The complete elimination of omitted variable bias caused by possible non-random assignment of miscarriage is unlikely despite our novel study design, rich data and empirical methods employed. However, we are able to provide bounds for $\widehat{\beta_M}$ using the partial identification approach Oster (2017). Specifically, we implemented a formal test advocated by Altonji et al. (2005) and formalized by Oster (2017) to assess the degree to which omitted unobservable factors might possibly explain away the observed relation between β_M and self-harm, and psychiatric disorders following adjustment for X covariates.

The idea of the test is based on the assumption that the bias from observed variables contains useful data regarding the bias from unobserved variables. The coefficient of proportionality (δ) is understood as how substantial the impact of unobserved variables needs to be relative to the impact of observed variables for the miscarriage effect to be zero (i.e. $\beta_M = 0$). Thus, a $\delta = 2$ implies that the unobserved variables would need to be two times as substantial as the variables used in analysis to cancel the identified effect (i.e. for $\beta_M = 0$). A value of $\delta > 1$ suggests that the effect is likely robust Altonji et al. (2005).

To apply this test, it is required to run a linear probability regression model and to set up a maximum attainable value of R^2 called $R_{\max}$ that measures the maximum variance explained by both observed and unobserved variables Oster (2017). However, the empirical evidence suggests that $R_{\max} = 1$ is too conservative, and Oster proposed to set $R_{\max} = 1.3R^2$ Oster (2017), where R^2 measures the variability explained by observed covariates. We implemented this test with $R_{\max} = 2R^2$ and with $R_{\max} = 3R^2$.

3.2. Permutation tests and multiple hypothesis testing

We used permutation tests where miscarriage status was randomly reassigned 1000 times and tested how many times the placebo estimates exceeded observed estimates under the null hypothesis. Under the random assignment of miscarriage status there should be no statistically significant relationship between miscarriage and self-harm and psychiatric disorders. Thus, under the null hypothesis of random assignment of miscarriage status $\beta_M = 0$ (i.e. no true miscarriage effect) and the proportion of resampled $|\beta_{Ms}|$ that are greater or equal in absolute value than the observed $|\beta_M|$ provides a p-value for the null hypothesis. Given the multiple outcomes we consider for self-harm and psychiatric disorders, we also correct for multiple hypothesis testing using the Benjamini– Hochberg procedure (Benjamini and Hochberg, 1995). This approach controls the false discovery rate, ensuring that our inferences remain valid even with multiple comparisons.

3.3. Placebo tests

In order to explore the internal consistency of estimated effects of miscarriage on mental health and self-harm outcomes we performed placebo tests. We regressed outcomes that are plausibly not expected to be impacted by β_M because these precede the first pregnancy. The outcomes selected for the placebo test were: number of hospitalisations during the two-year period prior to the first pregnancy and number of mental health drugs two years prior to the first pregnancy. If our model yields no significant effect of miscarriage on these pre-pregnancy placebo outcomes, it offers additional reassurance that the main effects are not driven by model artifacts or spurious correlation.

3.4. Sensitivity analyses using entropy matching and fixed effects models

As sensitivity analyses, we used entropy balancing (Hainmueller, 2012) to estimate the impact of miscarriage on self-harm and psychiatric disorders. Entropy balancing is a matching strategy that reduces the impact of confounding on observational causal inference; specifically it is a quasi-experimental design that matches cases and controls using the reweighting of covariates based on the propensity for treatment, i.e. miscarriage. Entropy balancing has been shown to outperform propensity score matching and coarsened exact matching (Black et al., 2020; Parish et al., 2018; Zhao and Percival, 2017). Moreover, this method is doubly robust (Zhao and Percival, 2017). We used entropy balancing to make the two groups (first time pregnant women who miscarried vs first time pregnant women with a continued pregnancy) statistically equivalent, based on a number of observable factors: age at pregnancy, ethnicity, region, quintile of the Townsend Index and count of previous hospitalisations prior to the first pregnancy.

As an additional sensitivity analysis, we modelled general practices with fixed effects as a sensitivity analysis to control for individual heterogeneity across practices. Thus, we implemented logistic regression to estimate equation (1) using indicator variables for each general practice.

4. Results

4.1. Baseline characteristics

Table 1 reports the baseline characteristics of the study population by miscarriage status. The mean age at first pregnancy for women who miscarried (29.19 years) was slightly higher than for those who had a continued pregnancy (28.69 years). The average pregnancy age in our

Table 1
Demographic characteristics of study participants.

	Continued pregnancies	Women who miscarried	KS Test p-value
N (%)	182451 (75.3)	59992 (24.7)	
Age at First Pregnancy, Mean (SD)	28.69 (6.33)	29.19 (6.86)	<0.001
Age Group, N (%)			
≤30	109017 (59.8)	33505 (55.8)	
>30 and ≤ 40	65606 (36.0)	22113 (36.9)	
≥40	7828 (4.3)	4374 (7.3)	<0.001
Ethnicity, N (%)			
White	112645 (75.4)	36664 (74.9)	
Indian	6190 (4.1)	1805 (3.7)	
Pakistani	5241 (3.5)	2118 (4.3)	
Bangladeshi	4123 (2.8)	1391 (2.8)	
Other Asian	4437 (3.0)	1239 (2.5)	
Caribbean	2438 (1.6)	810 (1.7)	
Black African	5775 (3.9)	2180 (4.5)	
Chinese	1403 (0.9)	412 (0.8)	
Other	7094 (4.8)	2320 (4.7)	<0.001
Ethicity Missing Indicator, N (%)			
0	149346 (81.9)	48939 (81.6)	
1	33105 (18.1)	11053 (18.4)	0.12
Region, N(%)			
East Midlands	4302 (2.4)	1415 (2.4)	
East of England	7928 (4.3)	2560 (4.3)	
London	48711 (26.7)	16017 (26.7)	
North East	5855 (3.2)	1787 (3.0)	
North West	34752 (19.0)	11989 (20.0)	
South Central	22289 (12.2)	7166 (11.9)	
South East	14625 (8.0)	4530 (7.6)	
South West	17354 (9.5)	5719 (9.5)	
West Midlands	18611 (10.2)	6178 (10.3)	
Yorkshire & Humber	8024 (4.4)	2631 (4.4)	0.06
Quintile of Townsend, N (%)			
1 (Least Deprived)	39706 (21.9)	13205 (22.1)	
2	38005 (21.0)	12329 (20.7)	
3	37458 (20.7)	12171 (20.4)	
4	34381 (19.0)	11180 (18.8)	
5 (Most Deprived)	31761 (17.5)	10741 (18.0)	0.219
4l: Townsend quintile percentages computed among non-missing observations (1140 continued and 366 miscarried with missing quintile).			
Count of Previous Hosp Prior First Preg, N (%)			
No Previous Hospitalisations	64825 (35.5)	25013 (41.7)	
One Hospitalisation	42365 (23.2)	13408 (22.3)	
Two Hospitalisations	27561 (15.1)	7903 (13.2)	
Three Hospitalisations	17129 (9.4)	4740 (7.9)	
Four Hospitalisations	10323 (5.7)	2926 (4.9)	
Five or more Hospitalisations	20248 (11.1)	6002 (10.0)	<0.001
Status at end of follow-up, N (%)			
Died GP record	470 (0.3)	141 (0.2)	
Left	50423 (27.6)	16636 (27.7)	
Still registered	131558 (72.1)	43215 (72.0)	0.58

Notes: KS Test stands for Kolmogorov-Smirnov Test.

sample broadly mirrored national figures for the UK population provided by the Office of National Statistics. In the sample, 59,992 women (24.7%) experienced miscarriage during the first pregnancy and 182,451 women (75.3%) had a continued pregnancy. The Kolmogorov-Smirnov test for the equality of distribution functions strongly rejected the null of no differences by miscarriage status (p-value < 0.001) for the following variables: age at first pregnancy, age group, self-reported ethnicity, prior hospitalisations, and year of the first pregnancy. However, this test did not reject the null for quintile of the Townsend Index, and geographical region. Thus, unadjusted comparisons should not be interpreted as causal; inference rests on adjusted, matched, and sensitivity-analysis estimates.

Overall, the evidence was not fully conclusive regarding the comparability of covariate distributions across miscarriage status. This imbalance driven primarily by self-reported ethnicity and prior hospital admissions implies that an unadjusted contrast would be biased, and it directly motivates the analytic strategy used here. Three features of that strategy address the imbalance directly: regression adjustment, entropy balancing, and coarsened exact matching.

4.2. Randomness of the first miscarriage vs continued pregnancy

Table 2 provides results from a regression model that aimed to predict whether the first pregnancy ends in miscarriage as a function of covariates within a linear probability model framework. This way we evaluated whether the first pregnancy that ended in miscarriage was correlated with observable characteristics. We used a linear probability model to assess whether the miscarriage status of the first pregnancy was correlated with covariates. Results show that age at first pregnancy, selected self-reported ethnicity categories (Indian, Pakistani, Other Asian, Black African, and Chinese), and prior hospital admissions were predictive of whether the first pregnancy ended in miscarriage. This observation is also consistent with previous literature in the field of miscarriage (Adolfsson and Larsson, 2006; Lund et al., 2012; Quenby et al., 2021). Unlike previous studies, our large data set allowed us to do some subclassification of Asian ethnicity with Pakistani women being at increased miscarriage risk and Indian and Chinese women at reduced risk However, previous healthcare service use was found to slightly decrease the adjusted odds of miscarriage. Across all other covariates, the estimated coefficients were not statistically significant. The overall evidence presented in Tables 1 and 2 suggest that miscarriage status is not a purely exogenous shock to fertility given the covariates described in Section 2.4.

4.3. Effect estimates

Table 3 shows the estimates of impact of miscarriage on self-harm

Table 2
Linear probability model of factors to predict that the 1st pregnancy results in miscarriage.

	First Pregnancy Ended in Miscarriage vs First Continued Pregnancy	p-value
Age at First Pregnancy	0.002	<0.001
Townsend Index 2	−0.002	0.451
Townsend Index 3	0.002	0.400
Townsend Index 4	0.002	0.450
Townsend Index 5	0.006	0.051
Indian	−0.018	<0.001
Pakistani	0.040	<0.001
Bangladeshi	0.006	0.114
Other Asian	−0.030	<0.001
Caribbean	0.000	0.832
Black African	0.023	0.044
Chinese	−0.024	0.037
Other	0.000	0.732
East of England	−0.003	0.849
London	−0.002	0.650
North East	−0.008	0.574
North West	−0.006	0.804
South Central	−0.001	0.641
South East	0.001	0.572
South West	0.003	0.951
West Midland	0.000	0.897
Yorkshire & Humber	0.002	0.724
Counts of Previous Hosp	−0.014	0.002
Pseudo R^2	0.006	

Notes: This linear probability model predicts miscarriage as a function of observed covariates. The outcome variable is an indicator which denotes that the first pregnancy ended in miscarriage vs first pregnancy continued. Coefficients reflect changes in the probability of miscarriage relative to the base category. The base category for region is East Midlands. The base category for ethnicity is White group.

Table 3
Matching estimates of the treatment effects.

	Logistic Regression	Matching - RA	Matching - AIPW
	βMisc	βMisc	βMisc
Self-harm at 6M	2.30	2.28	2.28
	(<0.001)	(<0.001)	(<0.001)
Self-harm at 1Y	1.95	1.94	1.94
	(<0.001)	(<0.001)	(<0.001)
Self-harm at 3Y	1.60	1.60	1.60
	(<0.001)	(<0.001)	(<0.001)
Depression at 6M	1.50	1.48	1.48
	(<0.001)	(<0.001)	(<0.001)
Depression at 1Y	1.23	1.23	1.22
	(<0.001)	(<0.001)	(<0.001)
Depression at 3Y	1.12	1.14	1.15
	(<0.001)	(<0.001)	(<0.001)
Anxiety at 6M	1.25	1.25	1.25
	(<0.001)	(<0.001)	(<0.001)
Anxiety at 1Y	1.12	1.12	1.12
	(0.01)	(<0.001)	(<0.001)
Anxiety at 3Y	1.10	1.10	1.11
	(0.060)	(0.020)	(0.020)
PTSD at 6M	1.15	1.15	1.15
	(0.343)	(0.005)	(0.005)
PTSD at 1Y	1.05	1.04	1.04
	(0.430)	(<0.001)	(<0.001)
PTSD at 3Y	1.08	1.08	1.08
	(0.296)	(<0.001)	(<0.001)
Psychosis at 6M	1.46	1.46	1.46
	(0.058)	(<0.001)	(<0.001)
Psychosis at 1Y	1.71	1.72	1.72
	(0.001)	(<0.001)	(<0.001)
Psychosis at 3Y	1.49	1.51	1.51
	(<0.001)	(<0.001)	(<0.001)
OCD at 6M	1.45	1.44	1.44
	(0.170)	(0.009)	(0.009)
OCD at 1Y	1.05	1.03	1.03
	(0.413)	(<0.001)	(<0.001)
OCD at 3Y	1.34	1.32	1.32
	(0.020)	(<0.001)	(<0.001)
Bipolar at 6M	0.75	0.69	0.75
	(0.396)	(0.674)	(0.875)
Bipolar at 1Y	1.84	1.76	1.74
	(0.198)	(0.172)	(0.172)
Bipolar at 3Y	0.96	0.94	0.97
	(0.458)	(0.014)	(0.014)
MH Drugs at 6M	1.13	1.15	1.15
	(<0.001)	(<0.001)	(<0.001)
MH Drugs at 1Y	0.98	1.00	1.00
	(0.226)	(<0.001)	(<0.001)
MH Drugs at 3Y	1.00	1.01	1.01
	(0.460)	(<0.001)	(<0.001)

Notes: Stata 17 nlcom command was used to compute log odds ratios after teffects command. p-values in parenthesis. Logistic estimators were computed adjusting for age at pregnancy study entry date, age squared, year of pregnancy, patient level area deprivation index (based on Townsend index), self-reported ethnic group, UK region and total count of prior hospital admissions, general practice. Matching RA and Matching AIPW estimators were computed by matching upon the following covariates: age at pregnancy, year of pregnancy, Townsend Index, self-reported ethnic group, UK region and total count of prior hospital admissions. Adjusted p-values adjusted for multiple hypothesis testing comparisons (computed using the Benjamini and Hochberg procedure). Finding were robust to these additional inference methods for self-harm, depression, anxiety, and prescription drugs because empirical p-values were less than 5%.

and psychiatric disorders. Miscarriage was associated with higher adjusted odds of self-harm and depression at all three follow-up time points; anxiety was significant at 6 months and 1 year and borderline at 3 years; anti-depressant prescription use was elevated at 6 months. The results show that miscarriage had the strongest impact on self-harm and the null is strongly rejected ($p < 0.001$) at all follow-up time points considered. Compared to women who had a continued pregnancy, women who experienced a miscarriage were at higher odds of self-harm at 6 months (odds ratio (OR) 2.3), at 1 year (OR 1.95) and at 3 years (OR 1.60). At the same time, women who had a miscarriage were at increased odds of being diagnosed with depression at 6 months (OR 1.50), with the effect attenuating to OR 1.23 at 1 year and OR 1.12 at 3 years; the null was strongly rejected at all three time points ($p < 0.001$). Anxiety followed a similar pattern of attenuation, with OR 1.25 at 6 months ($p < 0.001$), OR 1.12 at 1 year ($p = 0.01$), and OR 1.10 at 3 years ($p = 0.060$). Women who had a miscarriage were at increased odds of antidepressants prescription drugs use at 6 months (OR 1.13; $p < 0.001$). We additionally re-estimated the main logistic models with and without exclusions of women with prior antidepressants drug history (Appendix B, Table B.5; underlying logistic and Poisson estimates in Tables B.6–B.8). The consistency of effect direction and magnitude across model form and exclusion rule indicates that the central inclusion of women with prior antidepressant exposure is not driving the reported associations.

Evidence for PTSD, OCD, psychosis, and bipolar disorder was less consistent across estimators.

In the baseline logistic model, psychosis was significant at 1 and 3 years and borderline at 6 months; OCD was significant at 3 years but not at 6 months or 1 year; PTSD and bipolar disorder were not statistically significant. Matching-based estimates suggested additional associations for some sparse outcomes, which should be interpreted cautiously. Table 3 shows that results remained robust regardless of the use of logistic regression or augmented-inverse probability weighting estimator and regression adjustment matching. While the coefficients between these models were largely consistent, however, matching methods generally were more efficient.

Furthermore, when we analyzed the effects of miscarriage on self-harm by quintile of Townsend Index of Socioeconomic Deprivation (Table 4), miscarriage was not associated with self-harm in the least deprived Townsend quintile, but elevated self-harm risk was observed in quintiles 2–5. The estimates did not show a strictly monotonic deprivation gradient — the largest point estimate at 6 months was in quintile 2 rather than quintile 5. Notably, for women from the least deprived quintile, the adjusted odds of experiencing self-harm at 6 months was 1.07 (95% CI: 0.43, 2.64; p = 0.89). In contrast, for women from the most deprived areas, the adjusted odds of experiencing self-harm at 6 months was 2.53 (95% CI: 1.32, 4.88; p = 0.01). We note the potential underreporting of mental outcomes and self-harm incidents in our data from electronic health registries may have resulted from the stigma of diagnosis and its deterrent effect on help-seeking behaviour (Clement et al., 2015; Gaiha et al., 2020; Rossler, 2016; Schomerus and Angermeyer, 2008).

5. Robustness checks

5.1. Permutation tests

Table B.1 reports *p*-values from a two-sided permutation test of zero effect ($\beta_M = 0$). Table B.1 also reports *p*-values adjusted for multiple hypothesis testing comparisons (computed using the Benjamini and Hochberg procedure (Benjamini and Hochberg, 1995)). The evidence demonstrates that the main finding was robust to these additional inference methods for self-harm, depression, anxiety and prescription drugs because empirical *p*-values were less than the 5% significance level. Furthermore, the results were significant for self-harm, anxiety, depression, and prescription drugs when we adjusted for multiple hypothesis testing as *p*-values computed using the Benjamini and Hochberg procedure are less than the 5% significance level.

5.2. Coefficient stability tests: entropy balance matching and fixed effects for general practices

We implemented entropy balance matching (Table B.2). Entropy-balancing estimates are reported in Table B.2, and estimates including GP-practice fixed effects are reported in Table B.3; coefficients were

Table 4
Stratified analysis.

	Adjusted			
	βMisc	Lower 95% CI	Upper 95% CI	p-val
Panel A: Index=1				
Self-harm at 3M	0.36	0.05	2.71	0.32
Self-harm at 6M	1.07	0.43	2.64	0.89
Self-harm at 1Y	1.21	0.64	2.29	0.56
Self-harm at 3Y	1.30	0.87	1.93	0.20
Panel B: Townsend Index=2				
Self-harm at 3M	5.78	2.04	16.39	0.00
Self-harm at 6M	3.31	1.61	6.80	0.00
Self-harm at 1Y	1.98	1.20	3.27	0.01
Self-harm at 3Y	1.64	1.20	2.25	0.00
Panel C: Townsend Index=3				
Self-harm at 3M	2.23	0.86	5.74	0.10
Self-harm at 6M	2.44	1.20	4.99	0.01
Self-harm at 1Y	2.74	1.61	4.67	0.00
Self-harm at 3Y	1.76	1.27	2.44	0.00
Panel D: Townsend Index=4				
Self-harm at 3M	2.29	1.14	4.58	0.02
Self-harm at 6M	2.51	1.45	4.36	0.00
Self-harm at 1Y	1.98	1.29	3.02	0.00
Self-harm at 3Y	1.55	1.18	2.03	0.00
Panel E: Townsend Index=5				
Self-harm at 3M	1.88	0.74	4.77	0.19
Self-harm at 6M	2.53	1.32	4.88	0.01
Self-harm at 1Y	2.18	1.32	3.62	0.00
Self-harm at 3Y	1.81	1.35	2.43	0.00
Demog Controls	No			Yes
Prior Hosp Adm	No			Yes
Mat Dep Controls	No			Yes
Year Effects	No			Yes
GP Practice Effects	No			Yes

Notes: First pregnancies recorded between 1.01.2004 and 31.12.2017. We excluded patients if they had a previous recorded diagnosis prior to the first pregnancy for each diagnosis. Demographic controls include age, age squared and ethnic background. "Prior Hosp Adm" is a count of the total number of hospital admissions prior to the first pregnancy. These are indicator variables for hospital admissions recorded prior the first pregnancy. "Material dep" stands for the patient-level area deprivation index (based on the Townsend Index). Year Effects denote dummies for year of the first recorded pregnancy. GP Practice Effects denote variables for general practice. Standard errors clustered by GP practice.

similar across both estimators, although inference differed for some sparse outcomes. Fixed-effects estimates for antidepressant-prescription outcomes are not reported because those models did not converge. Coarsened exact matching estimates are reported in Table B.10.

5.3. Tests for selection on unobservables

Table B.4 provides results for selection on unobservables tests. The tests suggest that the results are robust at a level of $R_{\max} = 2R^2$ and $R_{\max} = 3R^2$ from the specification with observable controls. Specifically, for the outcomes reported in Table B.4 (self-harm, anxiety, depression, PTSD, and psychosis), the selection-on-unobservables test excluded $\beta_M = 0$ under the specified $R_{\max}$ assumptions. All models excluded $\beta_M = 0$. Thus, the selection on unobservables test provides evidence that the reported results are robust.

5.4. Placebo tests

We regressed outcomes that are plausibly not expected to be impacted by miscarriage in order to test the internal consistency of the empirical model. Table B.9 shows the results of two placebo tests designed to exclude the possibility that the effects we observed are mechanical. Outcomes selected for the placebo test were: number of hospitalisations two years prior to the first pregnancy and number of prescriptions two years prior to the first pregnancy. Thus, a priori we expected to find no systematic relationship adjusted for covariates between miscarriage and outcomes considered for placebo tests. The evidence shows (Table B.9) that β_{cM} was virtually zero or not statistically significant from zero. The F-test results (Table B.9) suggest that β_M is excludable from both models. The overall statistical evidence demonstrates that the reported results are not mechanical.

6. Discussion

This is the first study, to our knowledge, to identify miscarriages recorded in NHS primary care and hospital records in England, allowing us to examine the association between miscarriage and *new* diagnosis of self-harm and psychiatric disorders among first-time pregnant women. We use the biologically plausible case and clinical evidence that the first recorded pregnancy that results in miscarriage is a quasi-random fertility shock. This implies that the first miscarriage is unlikely to be correlated with unobserved variables that directly impact the outcomes considered. Our comparison is plausible because the two groups of women compared were pregnant for the first time and this circumscribed by default the endogeneity between treatment and outcomes considered.

The evidence from sensitivity and robustness tests shows that the results remained robust. The results also remained robust regardless of the use of logistic regression that modelled general practices using random or fixed effects or matching methods (RA, AIPW or entropy). While the coefficients between these models were largely similar, however, matching methods generally were more efficient which is consistent with the methodological literature in the field (Rothman, 2012; Stuart, 2010).

Miscarriage was associated with higher adjusted odds of self-harm at 6 months, 1 year, and 3 years; depression at 6 months, 1 year, and 3 years; and anxiety at 6 months and 1 year, with the 3year anxiety estimate borderline in the baseline logistic model. Evidence for PTSD, OCD, psychosis, and bipolar disorder was less consistent across estimators. Our results show that miscarriage had the strongest effect on self-harm outcomes and the null was strongly rejected ($p < 0.001$) at all follow-up time points considered. Specifically, compared to women who had a continued pregnancy, women who experienced a miscarriage had increased odds of self-harm at 6 months (OR 2.30), at 1 year (OR 1.95) and at 3 years (OR 1.60). At the same time, women who had a miscarriage were at higher odds of being diagnosed with depression at 6 months (OR 1.50), and with anxiety (OR 1.25). However, this OR decreased to 1.16 at 1 year and at 1.14 at 3 years. Women who miscarried during the first pregnancy had higher odds of PTSD (OR 1.15 at 6 months) and psychosis (OR 1.46 at 6 months, rising to OR 1.71 at 1 year), with the null strongly rejected for psychosis at 1 year ($p = 0.001$) and 3 years ($p < 0.001$). Furthermore, our results show that miscarriage was a significant predictor of the total count of adverse outcomes considered. In particular, miscarriage increased the total count of diagnoses by 47% at 6 months follow-up; however, the impact of miscarriage on total counts of diagnoses also decreased over time.

We identified disparities in outcomes by socioeconomic deprivation, with the outcomes being more prevalent among women residing in the most deprived areas compared to those residing in the least deprived areas. This suggests that there may be social determinants of health that are contributing to disparities in outcomes for pregnant women, such as access to healthcare, education, and other resources. While the results do indicate that the least deprived has the lowest risk for women there is no clear gradient for women residing in areas with Townsend equal 2, 3 or 4. Future research should explore the direction and shape of the gradient.

It will be important to address these disparities in order to improve and promote health equity in England. Overall, this highlights the importance of considering the social determinants of miscarriage when assessing health outcomes, as women who are more socially and economically disadvantaged may face greater health risks and barriers

to accessing care. Identifying and addressing factors that contribute to disparities in self-harm outcomes is an important area for future research. This will help to inform the development of interventions and policies that aim to improve health equity around miscarriage and its effects.

Overall, our results are in stark contrast with the psychological, health services research and economics literature that has examined the impact of abortion following a diagnosis of medical necessity on mental health outcomes. This literature generally found either no effect (Biggs et al., 2017, 2018) or a minimal impact of abortion of pregnancy on mental health (Broen et al., 2005; Charles et al., 2008; Heikinheimo et al., 2017; Horvath and Schreiber, 2017; Janys et al., 2019; Major, 2008; Major et al., 2009; Miller et al., 2020; Reardon, 2018, Thorp Jr et al., 2005). Our results suggest that miscarriage might have contrasting consequences compared to abortion. However, variations in study designs, populations, and outcome measurements between our research and previous studies on abortion might partly explain the contrasting outcomes observed. Furthermore, our study findings provide further evidence to support the presence of the maternal–fetal relationship widely documented during the last century (Benedek, 1959; Benedek and Liebman, 1958; Bowlby, 1973, 2018; Cranley, 1981).

By default, our study examined the impact of a single miscarriage experienced during the first pregnancy. The observed magnitude of the relationship between miscarriage and self-harm and psychiatric outcomes suggests that recurrent miscarriages might exert a significantly heightened burden on women's physical and emotional well-being, as well as on their partners. Indeed, recent longitudinal evidence shows that HRQoL deficits compound with successive pregnancy losses, with recurrent miscarriage producing clinically significant HRQoL decrements relative to a single loss (Bolbocean et al., 2026). Consequently, investigating the effects of multiple pregnancy losses on self-harm and psychiatric outcomes will likely emerge as an important area for future research.

The study's findings suggest that miscarriage may not constitute a completely exogenous shock to fertility, as factors such as ethnicity, prior health service usage, and deprivation levels are associated with the likelihood of a first pregnancy resulting in miscarriage. Future research should address the socioeconomic determinants of fertility. Exploring disparities in healthcare access and developing strategies to ensure equitable healthcare provision for all pregnant women in the UK represents a promising direction for future research.

Overall, these findings point to several implications for clinical practice and intervention research. First, because the elevated odds of self-harm, depression and anxiety we identify arise after a *first* miscarriage – in women with no prior psychiatric history by construction of our sample – case-finding approaches that rely on pre-existing mental-health contact will systematically miss the at-risk group. We recommend routine, universal assessment of mental-health symptoms and self-harm risk for all first-time pregnant women experiencing a miscarriage, offered in primary care, early-pregnancy assessment units, and emergency departments. Second, the concentration of elevated self-harm risk within the first six months is consistent with recent longitudinal evidence that health-related quality of life trajectories following miscarriage show their steepest decrements at three and six months and recover toward baseline by twelve months (Bolbocean et al., 2026), reinforcing the case for structured follow-up contact during that window rather than reliance on self-referral. Third, the same longitudinal evidence shows clinically meaningful compounding of HRQoL and anxiety/depression deficits with each additional loss, which provides empirical support for tiered or Graded Models of Care that escalate the intensity of psychosocial and specialist mental-health input with loss number (Bolbocean et al., 2026; Royal College of Obstetricians and Gynaecologists, 2023).

6.1. Strengths and limitations

A key strength of this study is the linkage of primary care and hospital care records for a large, population-based cohort, which to our knowledge has not been employed in previous miscarriage research. To our knowledge, no previous study has combined primary care and hospital records at this scale to examine miscarriages; our approach thus provides a valuable extension of existing research by ensuring near-complete capture of pregnancy histories. We focused exclusively on known pregnancies because this allowed us to circumvent the issues that might confound the relationship between miscarriage and outcomes considered such as: prior pregnancies, recurrent miscarriage and endogeneity of treatment and outcomes considered. This was essential to minimise biases that impact the exposure-outcome relationship. We also, uniquely, used pregnancies that did not result in miscarriage as controls so that effects of the miscarriage rather than the pregnancy *per se* were studied. Furthermore, because the structure of our data enabled us to examine the temporal relationship between miscarriage and outcomes considered we were able to address the issue of reverse causality. We were able to consider ethnicity which is particularly important since ethnic minority women are at higher risk of adverse outcomes (Knight et al., 2017;Knight et al., 2018) compared to white women including self-harm outcomes (Cooper et al., 2010; MacDonald et al., 2020; Vance et al., 2023).

The use of medical records instead of surveys is another strength of this study because primary care and hospital records do not suffer from inherent biases that characterise these data, and allows comprehensive capture of clinically verified outcomes in a diverse population, enhancing both internal and external validity. Furthermore, we applied clear and consistent clinical definitions to identify miscarriage, self-harm and psychiatric outcomes and covariates.

We were able to observe pregnancy histories and identify the first pregnancy record for each woman in our sample. However, it seems plausible that miscarriages registered with the GP occur early during the pregnancy, without particular physical complications, because that would involve a standard examination at the hospital (and a registered diagnosis from the hospital if treated there). Because we used electronic primary care data and HES records there is no concern that those registering their miscarriage with their GP are a selected group.

We used standardized coding systems to ensure consistency in outcome classification, minimizing misclassification bias and enhancing the reliability of our estimates for subsequent analyses. This approach enables replication and validation of our findings across diverse healthcare settings, strengthening the study's internal and external validity. Finally, the data from the study sample was diverse in terms of ethnicity, and socioeconomic status as measured by the Townsend Index. Thus, the reported results are likely to be generalisable to the UK maternity population.

We acknowledge the limitations of our study. The data used does not necessarily capture all miscarriages because pregnancy loss is a very personal and traumatic issue and some experiences related to miscarriage might not be recorded in either primary care or hospital records (Adolfsson and Larsson, 2006; Li et al., 2016; Linnakaari et al., 2019; Rasmark Roepke et al., 2017). Additionally, the electronic health registries might not have fully captured all cases of self-harm or psychiatric outcomes because of the known stigma associated with the diagnoses (Gaiha et al., 2020; Rossler, 2016), and because of the negative impact of mental health-related stigma on help-seeking behaviours (Clement et al., 2015; Schomerus and Angermeyer, 2008). This suggests that the results presented likely display the lower bound of the true effects. Although linkage to both primary and hospital records improves capture of clinically recorded outcomes, we cannot quantify the extent of under-recording due to stigma, non-help-seeking, or private care use. To the extent that women who miscarried were more likely to seek private support, NHS-only outcome ascertainment would likely bias estimates toward the null.

A further coverage limitation is that QResearch captures contacts within the NHS only. Mental health support obtained privately e.g. counselling, psychotherapy, psychiatric consultation, or private prescriptions is not recorded in a woman's QResearch record. If women who miscarried were systematically more likely than the continued-pregnancy comparator to seek private support (plausible given the elevated psychological burden documented here and long NHS waiting times for psychological therapies), our NHS-only outcomes would under-count true incidence asymmetrically in the miscarriage group, biasing our estimates toward the null.

We classified stillbirths within the continued-pregnancy comparator because our exposure of interest is biologically and clinically distinct from late pregnancy loss. We acknowledge that women experiencing stillbirth (Heazell et al., 2016; Kersting and Wagner, 2022) or TFMR, a profoundly different experience from elective termination for non-medical reasons, are themselves at elevated risk of adverse mental-health outcomes. Because stillbirth and TFMR are themselves associated with adverse mental-health outcomes, their inclusion in the continued-pregnancy comparator would be expected to raise the comparator outcome rate and attenuate miscarriage-versus-comparator estimates. Thus, if present, this bias would likely be conservative, moving estimates toward the null. The magnitude of this bias is bounded by two design features. First, the analytic cohort is restricted to women's *first-ever* recorded pregnancies, and stillbirths and TFMR are substantially rarer than miscarriage at first pregnancy. Second, the comparator group also includes the much larger population of pregnancies that progressed without adverse outcome, further diluting the influence of stillbirth and TFMR cases on the comparator's outcome rate. Future work using cohorts powered to treat stillbirth and TFMR as distinct exposures is needed.

It is important to note that while there is overlap, self-harm and suicidality are distinct constructs that do not perfectly align. Not all incidents of self-harm behaviour are motivated by suicidal intent, and conversely, not all suicidal ideation manifests in self-injurious acts. Our operationalization of these outcomes based on clinical documentation may not fully capture this nuance. Future research that carefully disentangles effects on suicidal ideation versus non-suicidal self-injury could provide additional insights.

We recognise that our study did not include all known behavioural and clinical factors related to miscarriage, although we requested data on all known health behavioural risk factors (e.g. substance use, smoking) and clinical risk factors (e.g. thyroid disease) prior to the start of this research project. Additionally, the absence of marriage or partnership status in healthcare records prevented our models from accounting for this individual-level factor. Our study utilised hospital admissions as a potential marker for prior healthcare use. Unfortunately, many of those variables had very high rates of missingness or have not been collected and the data provider chose not to transfer these poorly recorded measures to avoid introducing significant amounts of measurement error.

Thus, it would appear that the omission of these variables indicate that our primary regressor may be endogenous (Kennedy, 2008). However, this is unlikely to bias our findings because our identification strategy is based on the biological evidence that a first pregnancy that results in miscarriage is largely an unexpected shock to fertility. Therefore, the first miscarriage unlikely to be correlated with unobserved variables that directly impact the outcomes considered in this study (Hardy et al., 2016; Kaandorp et al., 2014; Kolte et al., 2014; Philipp and Kalousek, 2002; Quenby et al., 2021).

Our data does not allow us to identify the medical reason for the prescription of antidepressants. Because in the UK, these medications might be prescribed for reasons other than the outcomes we considered in the study, in our baseline analysis we included women with prior use of antidepressants. The prior-history sensitivity analysis confirmed that the principal association is preserved whether the analytic sample included all eligible women or was restricted to women with no prior antidepressant drug exposure.

To ensure a complete medical record for capturing any first diagnosis considered in this study, we only included women who remained with the same GP practice for the entire three-year follow-up period after the first pregnancy. While this approach enhances the reliability of our findings, it also introduces a potential limitation in terms of generalisability. Women who frequently relocate or change GP practices are likely to be underrepresented or entirely missing from our sample. However, it is important to note that patients typically do not change GP practices frequently (Empel et al., 2023), mitigating the extent of this limitation to some degree. Empel et al. (2023) found that only 1% of patients change their general practice each year in the UK. Moreover, since miscarriage is a personal issue, some cases may not be captured in NHS records(Adolfsson and Larsson, 2006; Li et al., 2016; Linnakaari et al., 2019; Rasmark Roepke et al., 2017) leading to potential underestimation of the true incidence and, possibly, conservative estimates of miscarriage effects.

Furthermore, an additional limitation is that the econometric methods we use here are second-best methods, which may not fully control for potential unobserved heterogeneity compared to a prospective randomized controlled trial that is not pragmatically possible. Thus, the reported empirical relationships do not necessarily represent causal relationships.

7. Conclusions and implications

Our findings suggest miscarriage may increase odds of self-harm, depression, anxiety, and PTSD up to one year, and psychosis at 1 and 3 years. The implications are especially important for pregnant women who face barriers to equitable health care further endangering women that face high pregnancy-related morbidity and mortality. This is essential in the light of recent research, which shows that close to 50% of all disability benefits in OECD countries are due to mental illness, and in the UK as a consequence of broad mental health conditions, the reduction in the national income is close to 7% (Layard, 2017). Moreover, the consequences of miscarriage extend beyond economic measures to encompass lasting emotional distress, social isolation, relationship strain, and diminished well-being for affected individuals and their families.

Given that up to 30% of pregnancies end in miscarriage (Linnakaari et al., 2019; Quenby et al., 2021; Stephenson and Kutteh, 2007), clinical pathways should ensure that all women who miscarry are offered routine mental-health assessment, low-barrier access to counselling and specialist perinatal services and follow-up contact during the six-month window (Bolbocean et al., 2026). Because we find elevated self-harm risk across all but the least deprived Townsend quintiles, with the largest point estimate at 6 months in quintile 2, commissioning decisions should prioritise equitable provision of these services rather than rely on self-referral.

Ethics approval statement

This study used an anonymised, patient-level extract from the QResearch database. At the time of approval for this project, QResearch was a Research Ethics Approved Research Database under East Midlands–Derby Research Ethics Committee approval (REC reference 18/EM/0400; IRAS 257790). No additional project-specific NHS Research Ethics Committee application was required beyond the QResearch governance approvals. The study protocol was reviewed and approved by the QResearch Scientific Committee (project reference OX90).

The research team received only the approved anonymised/de-identified data subset required for the analysis and did not access direct patient identifiers. Individual consent was not sought because the study used anonymised/de-identified records under the REC-approved QResearch research-database governance framework.

The study was conducted in accordance with applicable UK research-governance requirements, including the UK Policy Framework for

Health and Social Care Research.

CRediT authorship contribution statement

Corneliu Bolbocean: Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing. **Arri Coomarasamy:** Conceptualization, Validation, Writing – review & editing. **Julia Hippisley-Cox:** Data curation, Methodology, Software, Validation, Writing – review & editing. **Catia Nicodemo:** Validation, Writing – review & editing. **Siobhan Quenby:** Conceptualization, Methodology, Validation, Writing – review & editing. **Stavros Petrou:** Conceptualization, Funding acquisition, Investigation, Methodology, Resources, Validation, Writing – review & editing.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.socscimed.2026.119481.

Data availability

The authors do not have permission to share data.

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