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DISCUSSION PAPER SERIES

IZA DP No. 16395

The Economics of Abortion Policy

Damian Clarke

AUGUST 2023

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The Economics of Abortion Policy

Damian Clarke

University of Exeter, University of Chile, IZA, MIPP and CAGE

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ABSTRACT

The Economics of Abortion Policy*

This article provides a review of the economics of abortion policy. In particular, it focuses on the determinants of abortion reform, as well as the effects of abortion reform on individual circumstances. The economic literature on abortion policy is broad, studying abortion reforms that have occurred over the past two centuries, although there is a concentration of studies examining policy reform over the 20th and 21st centuries. The literature has examined a range of policies: both those which restrict access and those which legalise elective abortion, but within these two broad classes, the precise nature of policy reform can vary greatly. Policy reforms studied range from particular types of limits or financial barriers restricting access for particular age groups, up to policies which entirely criminalise or legalise elective abortion. The economic literature on abortion reform has illuminated a number of clear links, showing that increased availability of abortion decreases rates of undesired births, and vice versa when access to abortion is limited. These effects have been shown to have downstream impacts in many domains such as family formation, educational attainment, labour market attachment, as well as impacts on health, empowerment and well-being. There is mixed evidence when examining the impact which abortion reform has on cohorts of children exposed to reform variation. Much of what is known in the economic literature on abortion is gleaned from country-level case studies and cohort variation in access, with this evidence generated from a relatively small number of countries in which reforms have occurred and data is available. In general, much of the literature available covers low fertility and industrialised settings. Additional evidence from other settings would allow for a more broad understanding of how abortion reform affects well-being.

JEL Classification: A33, I18, J10, K36, O57

Keywords: abortion, contraceptives, labour markets, fertility, child outcomes, crime, health, political economy

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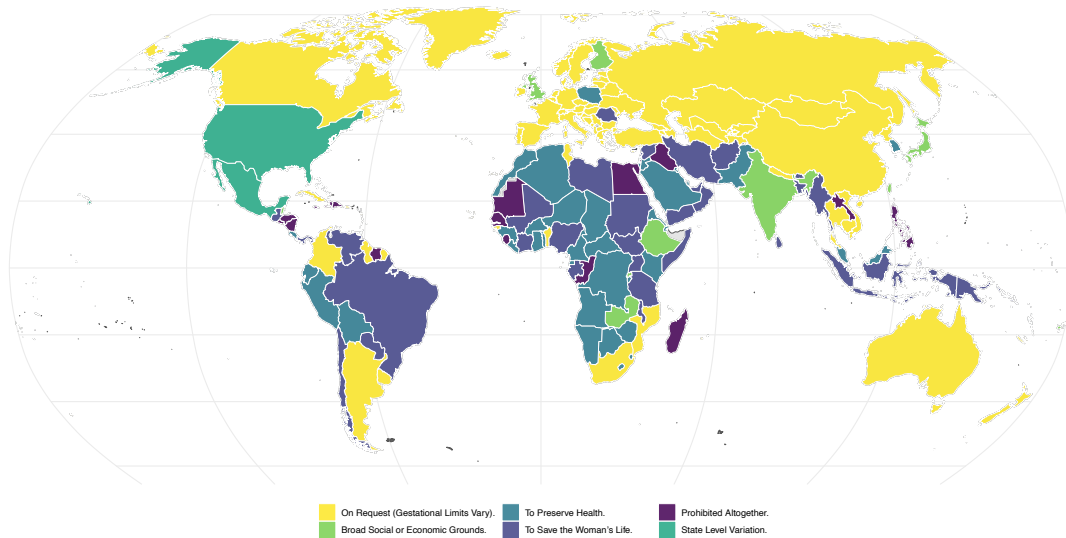
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1 Introduction

Few reproductive health policies, and arguably few health policies more generally, change with the frequency and polarity as laws related to abortion. By one measure, over the last 30 years there have been 64 national-level reforms to the rights that govern individual access to abortion, corresponding to around two reform changes per year (Center for Reproductive Rights, 2023). What’s more, these reforms cover quite similar ground, and move in both directions, at times liberalising and at times rolling-back access to abortion. The current state of abortion legislation worldwide suggests substantial differences in access across countries and by continent, as documented in Figure 1. Whilst the broad current over the past three decades has been in progressively increasing access to abortion when considering national-level legislative changes, there have similarly been movements to restrict access. These reforms have received substantial attention in both the academic literature and the popular press.

Figure 1: Exposure to National Level Abortion Laws (2023)



Notes: Classifications of abortion legality by country are compiled by the Centre for Reproductive Rights, and these figures refer to laws as at June 2022.

The considerable attention paid to these policies is warranted given the far-reaching impacts which abortion policy has on the lives of affected individuals. When women and families can optimally plan their desired number of children, the timing of these births, and, potentially, the spacing between births, this allows them to balance their own desires for economic security and career advancement, as well as

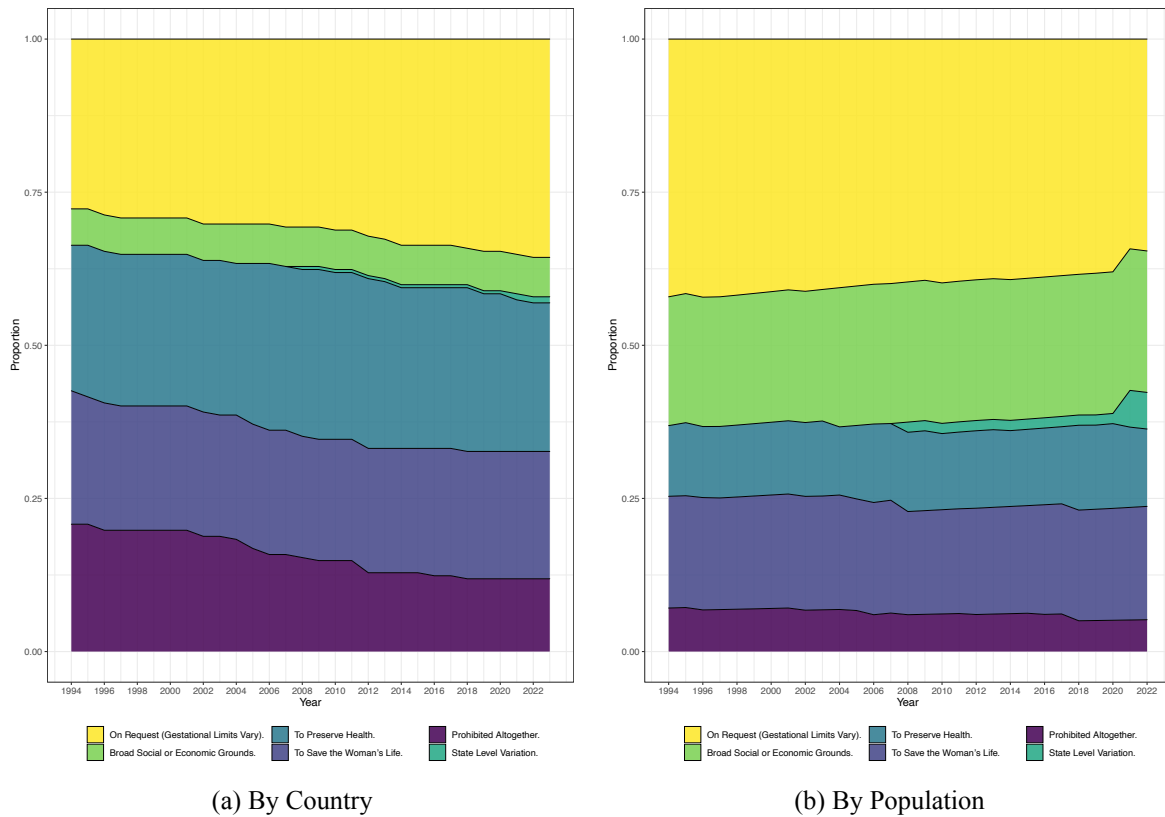
optimally plan investment in any desired children. And when children are desired, the environments in which they live, the types of care they receive, and their well-being across their entire life course is, on average, different to when they are undesired. Historical and recent policy decisions taken with regards to access to abortion have undeniably shaped the outcomes of generations of women, families, and children.

This review seeks to provide a broad overview of the economics of abortion policy. The economic literature on abortion offers a number of important lessons. Abortion reform is both a highly relevant input to many factors of central importance to economists including individual well-being, human capital accumulation, demographic structures, and labour force participation. Abortion policy is itself also shaped by economic factors (Blank et al., 1996; Gonzalez and Quast, 2022), suggesting that economic research on abortion can help us understand both how abortion policy is set, as well as how it shapes micro- and macro-level outcomes. The relevance of questions related to abortion and abortion reform in economics is reflected in its coverage in the economic literature. The quantity of papers published across economic journals suggests that understanding the impacts of abortion on individuals, families, and population cohorts is a question of general interest, as well as of particular interest to fields including demographic economics, labour economics, health economics, development economics, and law and economics.

This review is structured to first seek to synthesise how economic research shapes our understanding of the passage of abortion policy, as well as what we can learn from studying abortion reforms. It then covers a number of substantive questions related to the impacts of abortion policy on individual outcomes and well-being. Specifically, it covers three key themes in this regard: understanding how abortion policy shapes abortion *access*, understanding how abortion policy shapes the outcomes of women and families during their fertile years, and understanding how abortion policy shapes the composition of birth cohorts both at the time of reform, as well as during the lives of these individuals. Finally, some discussion is provided related to how policy makers and individuals may change their behaviour as a result of abortion policy decisions.

Despite the fact that the economic literature on abortion reform is large, many of the lessons which can be drawn from this literature flow from policy changes in a relatively small number of settings. This is a relevant limitation when examining the current body of knowledge from the economic literature focused on abortion policy. This is especially so when considering that many of the countries

Figure 2: Changes in Exposure to National Level Abortion Laws (1994-2023)



Notes: Figures present the proportion of countries classified as being subject to alternative policy regimes (Panel (a)), and the proportion of individuals world-wide classified as being subject to these policy regimes (Panel (b)). Figures are plotted based on the legal classification provided by [Center for Reproductive Rights \(2023\)](#).

which are currently exposed to the most restrictive abortion laws—and hence potentially most likely to see shifts in their abortion laws if recent trends in liberalisation continue—are located in high fertility environments in the developing world. The majority of the papers in economics relating to abortion focus on relatively lower fertility environments, often in industrialised countries.

Nevertheless, the economic literature on abortion policy offers relevant findings when considering both the effects of liberalising abortion policy as well as the effects of criminalisation. Despite the well-documented fact that policy changes over the past decades have been broadly progressive in nature, it may be surprising to note that the total world population living in countries with abortion available upon request is actually falling, given varying population growth rates world-wide. Figure 2 plots these trends where we can observe that in 1994 around 30% of countries allow abortion upon

request, while in 2023 this proportion has grown to around 40% of countries. However, due to differential population growth rates, in 1994 around 45% of the world lived in a country where abortion was available upon request, while today this figure stands at closer to 30%. These broad trends, combined with the frequency of changes in abortion policy, make clear the relevance of understanding how abortion policy decisions are made, and what these decisions imply for individual well-being for populations exposed to a range of policy regimes.

This article seeks to cover both early and more recent literature in the economics of abortion policy, laying out the broad scope of questions studied in this literature and key points of agreement and divergence within the literature. A number of additional texts provide overviews on the economics of abortion and are likely to be of relevance to interested scholars. This includes a book by [Levine \(2004\)](#) which comprehensively covers the literature up to that time with a focus on abortion reform and its impacts on abortion access and fertility, a scoping review by [van der Meulen Rodgers et al. \(2021\)](#) with a particular interest in the abortion reform and macro-economic outcomes, a review by [Bernstein and Jones \(2019\)](#) focused on abortion access and micro-level economic outcomes, and a handbook chapter by [Joyce \(2010\)](#) particularly focused on the line of research seeking to delineate links between abortion and crime. This present article aims to cut across themes, providing an overview of the state of the art of economic research as well as discuss implications for where the literature may go from here.

2 What Precipitates Abortion Reforms?

What can explain the policy landscape with regards to abortion access, and what precipitates changes in abortion policy? The economic literature on abortion reform has pointed to a number of determinants of *policy change*, though in general, the complexity of abortion legislation and the way which legal reform occurs depends on a country's specific institutions including legal origins, cultural norms, political systems and the interaction between law-makers and the judiciary. Nevertheless, studies which focus particularly on determinants of abortion policies have pointed to a number of important direct and proximate factors that precipitate voting and eventual legal reform.

2.1 Political Determinants and Politician Identity

Given that legal reform is, in many cases, an overwhelmingly political process, much of the literature focuses on political determinants. Indeed, politician identity and personal circumstance of politicians have been documented to affect both voting on laws related to abortion, as well as *de facto* access to abortion. Unsurprisingly, voting for abortion law is strongly polarised across political party lines. For example, [Bouton et al. \(2021\)](#) document that in the United States of America, Democratic Senators are much more likely to vote in favour of pro-choice positions on Senate roll-call votes, while Republican senators are less likely to support such positions in a sample of Senate votes between 1997-2012.

However, beyond party affiliation, individual politician identity has also been shown to affect voting behaviour. An influential paper by [Washington \(2008\)](#) documents that US Congress-people with daughters are substantially more likely to vote liberally than those with sons. This holds in particular with votes related to abortion and reproductive health. Using voting records from the members of four houses of US Congress (all houses between 1997-2004), She estimates, for example, that having an additional daughter makes a Congress-person 3.5pp and 3.2pp more likely to vote in favour of pro-choice positions on laws considering an abortion ban, and teen access to abortion (respectively), with this particularly the case for fathers.¹ [Van Effenterre \(2020\)](#), considering voting patterns in both France and the US, finds that these results are heterogeneous by country and party of politicians. In the case of France, she examines the decriminalization of abortion with the passage of the “Veil-law” in 1974,² finding that for a right-wing politician, having an additional daughter makes them less likely to vote in favour of the law, while for a left wing politician, no precisely estimated impact of a daughter is observed. However, in the US, she finds that left-wing politicians are substantially more likely to vote for a pro-choice stance in a law regarding teenage access to abortion for each additional daughter, while no such result is observed for right-wing politicians. [Bhalotra et al. \(2021\)](#) note that a politician’s religious identity can shape *de facto* rates of sex-selective abortion in India. The authors document in a sample of close elections that increasing the proportion of Muslim state legislators rather than Hindu legislators results in declines in use of sex-selective abortion, in line with greater aversion to abortion among Muslims rather than Hindus. Finally, politician gender has been documented as an

¹Recent work has pointed to evidence that these results may be time- and context-specific, with [Green et al. \(2023\)](#) not observing similar patterns if extending to earlier and later Congresses to those studied by [Washington \(2008\)](#).

²This law was named after Simone Veil, the Health Minister who drafted the bill, and legalised abortion, permitting abortion on request up to the tenth gestational week.

important determinant in voting patterns in certain contexts. For example, [Washington \(2008\)](#) documents that women politicians are much more likely to vote in favour of pro-choice positions, although [Bhalotra et al. \(2023\)](#), studying the passage of parliamentary gender quotas world-wide do not observe that reserving seats in parliament (and subsequent increases in female legislators) results in reform to abortion laws in the 10 subsequent years.

While the aforementioned evidence all refers to the impact of national politics on the passage of abortion reform, it is worth noting that international policy may also shape access to abortion. The Global Gag Rule, or Mexico City policy, is a US foreign policy which since 1984 has been invoked by Republican Presidents, and repealed by their Democratic counterparts. This policy directly limits how overseas NGOs can conduct reproductive health programs ([van der Meulen Rodgers, 2018](#)), and this has been shown to have substantial effects on access to contraceptives, as well as downstream outcomes in low-income countries ([Jones, 2015](#)).

2.2 Economic and Other Determinants

While much of the literature studying inputs to abortion policy and legislation is focused on political determinants, [Elías et al. \(2017\)](#) suggest that economic development may be a relevant determinant of the passage of more liberal abortion policy, additionally noting that democratic regimes and greater rights for women are associated with passage of more liberal abortion policy, as well as noting that certain legal origins (specifically Socialist legal origins) are associated with more permissive abortion policy. Descriptive evidence from [Medoff \(2002\)](#) also suggests that interest groups and characteristics of constituents are relevant correlates of the severity of abortion restrictions in the US, finding for example that state-level restrictions are less severe in states with a greater presence of the National Abortion Rights Action League, and more severe in states where a greater proportion of the population is Roman Catholic. While not being relevant for abortion reform but rather directly relevant for *de facto* access to abortion in a setting where abortion is legally available, [Jacobson and Royer \(2011\)](#) note that extremist activities may curb access of women seeking abortion. They find that acts of domestic terrorism in which violence is exercised against abortion providers or at abortion clinics both reduces provider availability limiting access to abortion, and geographically shifts patterns of access.

2.3 Context Dependence in Abortion Policy

Despite these precedents from certain settings, the nature of abortion policy reform is highly context specific. To see this, consider two emblematic examples studied in the abortion literature in economics: the first the many policy changes in the United States of America from the 1970s to the 2020s, and the second the case of Romania in the 20th century (Pop-Eleches, 2006, 2010; Mitrut and Wolff, 2011). In the United States, abortion policy is dictated by the interpretation of the Constitution, which depends on how the Supreme Court parses relevant passages of the Constitution at the federal level, and many state-level laws, which can eventually be referred to the Supreme Court if their validity is questioned. According to Myers's work on the last 60 years' history of contraceptive reform in the US:

State policies governing young women's legal and confidential access to abortion and prescription contraception have evolved for six decades, determined by a complex and varying interplay of U.S. Supreme Court rulings and state regulations.

Myers (2022, p. 1437)

What is more, this complex interplay between the Supreme Court and states at times has been lead far more by states, and at other times been lead far more by the Supreme Court. In practice, this has meant many disperse changes such as the imposition and repeal of parental access laws, the closure of clinics due to requirements that they have admitting privileges at hospitals, and sharp changes in gestational week limits given the passage of state-level laws. It has also meant large sharp changes such as the legalisation of abortion given the Supreme Court's decision in Roe vs. Wade in 1973. However, this complex interplay between politicians and the judiciary is not always observed. If we consider the case of Romania, in 1966 the government of Ceaușescu, the Romanian dictator, imposed Constitutional Decree 770, which immediately banned abortion and contraception, except in specific and very limiting circumstances. This near total ban remained in place until the Romanian Revolution in December 1989 and the fall of Ceaușescu, at which point the new government legalised abortion on January 1, 1990, on only their fifth day in power. Thus, while the economic literature points to a number of specific factors which are inputs to abortion policy in particular contexts, the vast differences in experiences of abortion reform across countries illustrated in these two-particular cases suggests that the evidence must be viewed through this lens.

3 What Can We Learn from Studying Abortion Reforms?

3.1 Identifying Variation

The large majority of empirical studies in economics seeking to estimate the impact of abortion availability draw identification from legal reforms. There are certain exceptions, such as evidence from the Turnaway Study (Miller et al., 2020, 2023) discussed in Section 4.2.5, but overwhelmingly, evidence is driven by natural experiments based on policy or legal reform. These nature of these natural experiments have key implications for what can be learned about abortion reform. Reforms may work at different margins: for example limiting access or liberalizing access, may deal with particular restrictions: for example changing access to abortion in particular circumstances, may work with particular age groups, and are naturally context dependent, both in terms of the time period studied as well as the particular country, countries or regions considered. These are points of *external validity* and in section 3.2 these are enumerated at more length, with a discussion of the types of settings in which the literature can inform us.

Additionally, the suitability of using policy reform to understand the impacts of abortion on individual outcomes presumes *internal validity*. In particular, if we wish to estimate the impact of abortion availability or restrictions for a particular group exposed to some policy reform, we must estimate an unbiased counterfactual for what would have happened to this group in the state of the world in which such a reform had not been passed. Assuming that such a counterfactual can be estimated, an average treatment effect on the treated (ATT) can be generated. This is challenging, as discussed for example by Joyce (2013), who notes that at a minimum if we wish to learn lessons about the impact of abortion access from such abortion reform we require that (a) the reform actually affect access to abortion; (b) the reform may not affect other relevant factors apart from abortion; (c) we must understand the assignment mechanism which results in certain populations being affected by the reform and others not; and (d) that a causal channel exists allowing for downstream results to be considered. This is also a point made clear by Besley and Case (2000). They note that policies are naturally endogenous decisions, and understanding the impact of any policy change requires separating the effect of the reform from any underlying processes which may have propelled the reform to occur, and at the same time be related to outcomes of interest. Particularly, in the case of abortion, we may be concerned that states which impose more draconian abortion reforms may be generally engaging in policies which

limit the rights of women, and vice versa in times of liberalising policy. In general, plausibly estimating the effect of abortion policy on outcomes of interest requires separating the specific effect of abortion reform from any suite of policy decisions. Typically, the empirical studies discussed in this review spend considerable time laying out the requirements for internal validity, and the identification strategy will be a key element of any such study. As discussed below, perhaps principally in Section 4.3.3, discussions of identification strategies themselves have spurred substantial strands of literature in work on the impacts of abortion policy.

Finally, it is worth noting that where within-country variation in abortion access is used to identify the impact of abortion policy on outcomes of interest, and where reforms of interest occurred long in the past, measurement of reform is not necessarily trivial. While in certain cases law changes are unambiguous; based on widely documented political events (for example, [Pop-Eleches \(2006, 2010\)](#)) or policy reforms (for example, [Brooks and Zohar \(2021\)](#); [Clarke and Mühlrad \(2021\)](#)), in other cases, multiple state-level reforms and contradictory legislative findings may mean that the exact dating of reforms is not straightforward and requires substantial archival work. The importance of measurement, as well as documentation that measurement errors may exist in the literature is discussed by [Myers \(2022\)](#), who additionally proposes a clear coding for US state-level reform in the 1960s and 1970s.

3.2 The Nature of Abortion Reforms Studied

3.2.1 Liberalizations or Restrictions

Likely the most salient element of abortion reform is the whether the reform acts to restrict access to abortion, or liberalize access to abortion. It is generally clear with a particular reform whether its passage results in increased access to abortion or decreased access to abortion. For example, the passage of *Roe vs. Wade* in 1973 codified access to abortion in the United States, implying that the reform was liberal in nature. Similarly, [Mølland \(2016\)](#) studies a reform of abortion law in Norway in which access was liberalised for teenagers in Oslo four years earlier than in the rest of the country. A particularly interesting case is that of Romania. In Romania, abortion was widely accessed before being immediately criminalized in 1966 and then legalized once again 34 years later in 1990. This reform has been influential in studies in economics given that it has provided two sharp changes

with considerable pre and post-study periods available (see for example [Pop-Eleches \(2006, 2010\)](#); [Hjalmarsson et al. \(2021\)](#); [Mitrut and Wolff \(2011\)](#)), all of which study one or both of these particular legal shifts.

Apart from the aforementioned cases, clear examples of liberalisations studied in the economic literature on abortion include Canada ([Sen, 2007](#)), the USA ([Myers, 2017](#), *inter alia*), England and Wales ([Kahane et al., 2008](#)), all during the period of the 1960s-'70s, and later liberalisations such as Spain and Taiwan in the 1980s ([González et al., 2018](#); [Lin et al., 2014](#)), a number of Eastern European countries in the 1980s and '90s ([Levine and Staiger, 2004](#)), Nepal in the early 2000s ([Valente, 2014](#)), Mexico in 2007 ([Clarke and Mühlrad, 2021](#)), and Uruguay in 2012 ([Antón et al., 2018](#)). Examples of restrictions have been studied in a broad range of time periods, ranging from anti-abortion laws in the US in the 1850s-1910s ([Lahey, 2014a,b](#)), the Romanian policy shift of 1966 discussed previously, to very recent state-level restrictions limiting both provision of abortion (for example [Arnold \(2022\)](#)) and individual access to abortion in the US (for example [Lindo and Pineda-Torres \(2021\)](#)), as well as certain state-level legal tightenings in Mexico examined in [Clarke and Mühlrad \(2021\)](#).

3.2.2 Causes

Abortion reform may impose or remove limits. In certain settings, abortion may be allowed in particular circumstances such as in cases of risk to the mother's life or health, in cases of fetal inviability, or in cases of rape, but not broadly upon request. Thus, reforms may move particular margins of these restrictions, or eliminate restrictions all together. This includes cases such as the passage of *Roe v. Wade* in the US which codified access to abortion upon request, similarly to abortion reform studied in Mexico, Uruguay, and Norway (among others). In other cases, abortion reform may be more moderate, for example the case of abortion reform in Spain in 1985 and the UK in 1967 where abortion was not available upon request, but was available in the other three specific clauses mentioned above ([González et al., 2018](#); [Kahane et al., 2008](#)).

However, it is important to note that both the causes and the interpretation of the causes may vary substantially. An interesting case in point is the abortion liberalisation in both Spain and in the UK, which, while nominally putting limits on access to abortion, in a *de facto* sense did not imply substantial limits, given that in both settings the maternal health criterion was widely used to include

mental health concerns. In other settings, the interpretation of abortion restrictions may also vary, with quite different implementations of criminal sanctions imposed in settings where abortion was illegal; see, for example, the case of Mexican states studied in [Clarke and Mühlrad \(2021\)](#), or Draconian measures to ensure compliance with the law in [Pop-Eleches \(2006\)](#), compared with state-level policy reform in the US, where it has been well documented that individuals with means to travel over state lines can access abortion in states with less strict policies ([Joyce and Kaestner, 2001](#); [Lindo et al., 2020a](#)).

3.2.3 Types of Restrictions

The nature of reforms varies considerably by countries and time periods. These include outright legalisations (eg [Myers \(2017\)](#); [Sen \(2007\)](#); [Antón et al. \(2018\)](#)) or criminalisations (for example [Pop-Eleches \(2006\)](#); [Lahey \(2014a\)](#)), but additionally include restrictions which are more piecemeal, making access harder by either reducing access to abortion at the supply side (providers) or demand side (individuals).

Examples of demand side policies include funding reforms such as restrictions that limit the coverage of abortion from public funds; see, for example, [Blank et al. \(1996\)](#) for discussion of a US Medicaid funding restrictions, or [Brooks and Zohar \(2021\)](#) which consider the converse setting, where reforms occur to increase the provision of free access to abortion without changing any underlying restrictions. Similarly, other restrictions on the demand side include the passage of laws which impose mandatory minimum waiting periods and which require women to receive information about abortion procedures and then wait some pre-stipulated amount of time before being able to access the procedure have been put in place, see, for example [Altındağ and Joyce \(2022\)](#); [Lindo and Pineda-Torres \(2021\)](#). More invasive laws in a similar vein have been studied; for example, [Gius \(2019\)](#) considers laws requiring ultrasounds prior to abortions being performed. Additional examples include the passage of parental consent laws requiring parents of women under 18 years of age to be informed or otherwise provide consent prior to an abortion being provided ([Joyce and Kaestner, 2001](#)). A number of papers, for example [Bitler and Zavodny \(2004\)](#) consider multiple such restrictions in a single setting.

Examples of supply side policies include laws which impose restrictions on providers such as hospital admitting privileges or that they meet specific medical or infrastructure criteria, as well as

other licensing criteria. These laws, known in the United States as TRAP laws (for Targeted Regulation of Abortion Providers) have been widely studied in a this setting, given the proliferation of such policies across US states over the last 2-3 decades (Fischer et al., 2018; Lindo et al., 2020a; Arnold, 2022; Jones and Pineda-Torres, 2021). A description of the full nature of these laws can be found in Jones and Pineda-Torres (2021). Later in this article, further detail is provided on the nature of these reforms, as well as their implications.

3.2.4 Age Limits

Policies may inherently target specific age groups, and in cases such as these, any findings will naturally be limited to the age groups affected by policies. Along with the aforementioned parental consent laws which affect women under the ages of 18, certain policies only affect other specific age groups such as a roll-out of free abortion provision in Israel to 20-32 year-olds (Brooks and Zohar, 2021), or abortion restrictions which were put in place in Romania for all women, with exceptions for women above certain age cut-offs (Pop-Eleches, 2006). Even within the country studied in such settings, results will provide lessons for the particular population groups which are affected by marginal reform changes, and as such, external validity to other groups *within the same country* cannot be assumed.

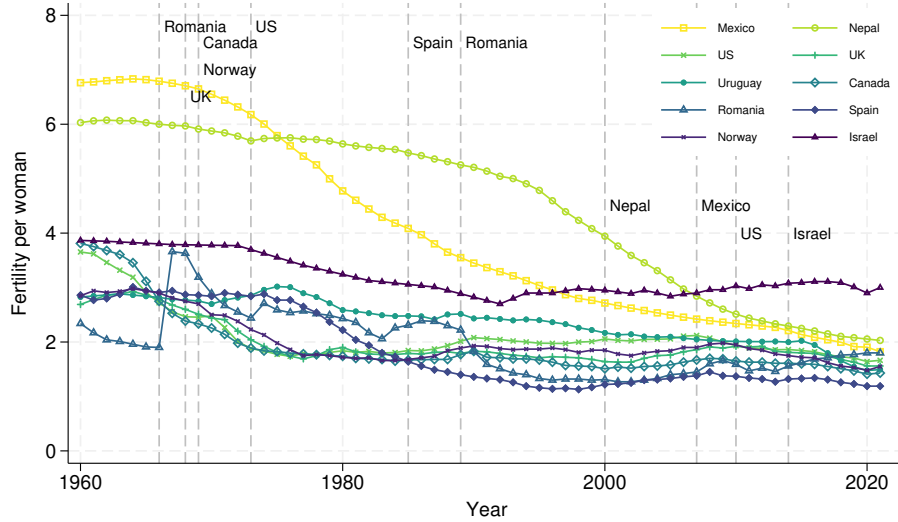
3.3 Limits to the Current Knowledge of Abortion Reform in Economics

More generally, it is important to note that the external validity of the reasonably large literature on abortion reform in economics may be limited. While quite a large number of papers have been published on abortion in the economic literature, the number of settings studied has been quite selected, in large part owing to the availability of reforms and data. Indeed, a large majority of the papers in the literature are from a single country – the United States – though from a number of policy reforms. While these papers provide a large body of evidence to understand the impacts of abortion reform in the US over the last 50 years, they may provide limited evidence on the implications for abortion reform in other settings.

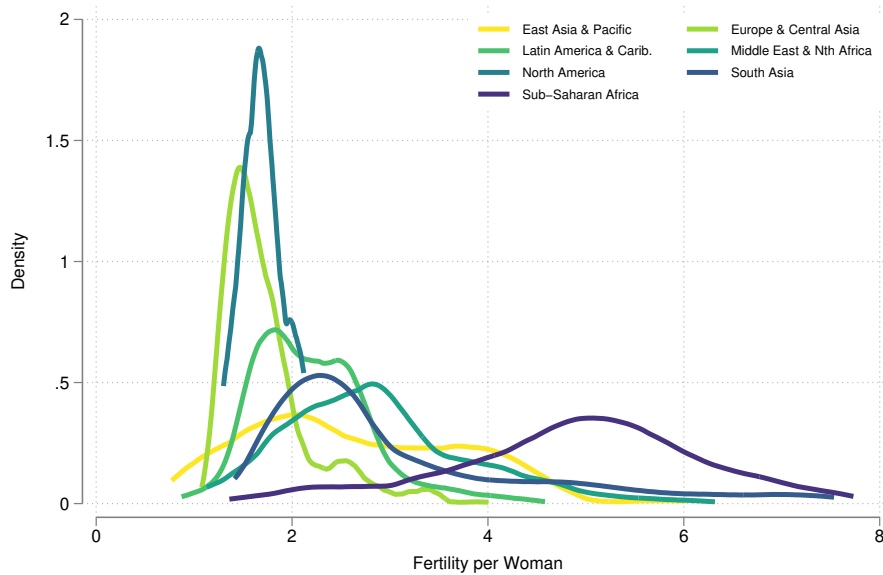
Throughout this paper abortion reforms are discussed which have been studied in papers published in academic journals in economics.³ Figure 3a provides a plot covering the period of 1960-2021

³This is a limit to this article, given that there is a substantial literature on abortion policy outside of economic journals,

Figure 3: Studied Abortion Reforms and Fertility Levels



(a) A Selection of Abortion Reforms Covered in Studies in Economics



(b) Fertility by World Region

Notes: Fertility rates are drawn from the World Bank Databank (indicator sp.dyn.tfrt.in). Reform dates in panel (a) refer to reforms discussed in this article, though at times refer to ranges of reform dates (such as state-level legal reforms in the US which occurred over a wide-period, though are signalled in 2010 in the plot). Fertility distributions in panel (b) are plotted for all countries in each region for years 2000-2020 only to provide a more recent snapshot of fertility levels, pooling all country by year observations.

indicating the dates when studied reforms took place (dashed grey lines), as well as the fertility rates of the countries in which reforms were studied. Despite the fact that these reforms occur over a substantial period of time, virtually all reforms occur in relatively low fertility environments. Indeed, the majority of reforms studied occur in countries and time periods in which fertility is close to the replacement rate (around 2 births per woman over her lifetime), though historical reforms such as those in the 1960s and 1970s in the US occurred in higher fertility environments, at closer to 3 births per woman. One exception to this is the reform studied by [Brooks and Zohar \(2021\)](#) in Israel in 2014, where the total fertility rate was slightly higher than 3 births per woman over her lifetime, however this reform is a financial shift where abortion was permitted both pre and post-reform.

When comparing fertility rates in these reform contexts to fertility rates worldwide (Figure 3b), it seems that these may provide limited lessons in certain contexts. This Figure provides density plots of country fertility rates over years 2000-2020 by world region, where a substantial portion of countries have fertility rates well in excess of those in reform countries, especially in Sub-Saharan Africa, though average fertility is well above 2 births per woman in all regions except North America, and Europe and Central Asia. When considering that the vast majority of countries which could conceivably liberalise abortion laws are located outside of North America, Europe and Central Asia, lessons from the reforms studied to date in the economic literature may be limited in terms of their predictive power for the impact of any reforms in these settings, at least if impacts depend on baseline fertility rates.

4 Evidence on the Nature and Magnitude of Effects of Abortion Reform

4.1 Abortion Reform and Abortion Access

A precursor to considering any impacts of abortion policy on outcomes of women and families is to understand how these policies affect access to abortion in the first instance. Such ‘first stage’ policy impacts are necessary for these policy changes to have any downstream effects ([Joyce, 2013](#)). Being able to convincingly document reform impacts on rates of abortion is, at times, challenging, as in many contexts, the policy reform implies that elective abortion is illegal in either the pre-policy or

which in the interests of space is not reviewed here.

post-policy period. In cases where abortion is illegal, measuring rates of abortion is very difficult⁴ and even in cases where abortion is legal, many of the reforms studied in the economic literature on abortion occurred more than 50 years ago, implying that access to data on abortion rates can be challenging (Blank et al., 1996).

Nevertheless, a range of papers have provided credible estimates of the impact of both historical and more recent abortion reforms on actual rates of abortion. In general, these studies leverage identifying variation from differences in access across geographic areas within countries over time (for example state-level, or even county-level variation in the United States), however in certain settings even time-series variation provides credible information on the impact of abortion laws on rates of abortion. Much of the evidence base on the impact of abortion policy on abortion rates comes from the United States covering the period from the late 1970s to the 2020s. This is a period in which US states were emboldened to enact their own laws related to abortion access as a result of a number of political events and legal findings; in particular, the passage of the Hyde Amendment in 1976 which restricts the use of federal funds such as Medicaid to finance abortion, as well as the Supreme Court decision in *Planned Parenthood vs. Casey* in 1992, which allowed states to impose restrictions on access to abortion, provided that those restrictions did not result in an “undue burden” on a woman’s access to abortion before a fetus became viable. Studies from the US thus examine the impact of reforms such as Medicaid funding changes, laws which seek to restrict individual access to abortion including parental involvement laws and waiting periods, and laws which seek to restrict providers’ ability to offer abortion, such as the need for clinics to have admitting privileges in hospitals.

Among studies to consider the impact of cuts to Medicaid funding on rates of abortion Blank et al. (1996) and Levine et al. (1996) provide early evidence of the relevance of changes in funding on access to abortion. Using data on abortion rates collected by the Guttmacher Institute Levine et al. (1996) find that funding restrictions reduce rates of abortion by 5.5% among women aged 15-44. Blank et al. (1996), who additionally considers data on abortion rates from the Center for Disease Control finds that rates of abortion decline by a similar rate if considering residents of states, but also have cross-state effects, with important reductions in the usage of abortion in state when spending restrictions are in place owing to declines in individuals travelling from across state lines. She finds that the total effect

⁴In certain settings, proxies have been proposed as a noisy way to measure access to abortion procedures. For example Pop-Eleches (2010) notes that in cases where abortion is illegal, and given risks inherent in clandestine abortion, maternal mortality is a proxy of access to clandestine abortion in the absence of safe alternatives.

of funding restrictions may be as high as 13%, and additionally notes that these effects are much higher among Medicaid eligible women: at between 19-25%. While Medicaid funding cuts were imposed at the Federal level, certain states made funds available at the state level to cover abortion for women on Medicaid. [Cook et al. \(1999\)](#) use a particular situation in North Carolina where state-level abortion funds ran out before the end of a number of financial years, and estimate that for affected women, around one third of abortions which would have been performed were actually taken to term given the lack of funding availability. Taken together, these findings suggest that the availability of financing is a highly relevant element in access to abortion.

The impact of laws that restrict access has also been documented in considering parental involvement laws for young women. Such laws, which were passed in a reasonably large proportion of US states from the late 1970s onwards and require that parents are notified where women under 18 seek an abortion, have been found to have substantial impacts on abortion rates for young women. [Haas-Wilson \(1996\)](#) finds that the laws decreased rates of abortion by between 13 and 25%, with [Levine \(2003\)](#) finding broadly similar effects. [Bitler and Zavodny \(2001\)](#) additionally note that declines in access to abortion owing to these laws result in increased rates of later-gestational abortion, with implications for the health and well-being of women. More recent analyses based on event study models ([Joyce et al., 2020](#)) suggest the impacts of such laws may be limited to earlier time-periods (pre-1990), while [Joyce and Kaestner \(2001\)](#) note that minors may travel out of state to blunt the impact of such reforms. A further review of these laws, as well as discussions related to data quality is provided by [Dennis et al. \(2009\)](#), and [Myers and Ladd \(2020\)](#) provides considerable further discussion related to challenges in measuring abortion rates, and suggests that these estimates should be viewed with caution.

A final set of demand-side laws considered are the imposition of mandatory waiting periods. Mandatory delay laws, which can require that individuals travel twice to abortion facilities to seek an abortion have been documented to have substantial impacts on abortion rates ([Altındağ and Joyce, 2022](#)), the proportion of abortions conducted in the second trimester ([Lindo and Pineda-Torres, 2021](#)), and, in certain contexts, to increase the rate of out-of-state abortions ([Joyce and Kaestner, 2001](#)). [Altındağ and Joyce \(2022\)](#), using a regression discontinuity design and a law change in Arkansas in 2015 which required that women make two trips to abortion providers prior to accessing abortion found that these mandatory delay laws reduced rates of abortion by 17%. While these studies suggest that these

laws can be highly relevant in affecting access to abortion, earlier evidence from [Bitler and Zavodny \(2001\)](#) suggests that this may not always be the case. In their setting, studying earlier mandatory delay laws, they found relatively little evidence on impacts of these laws on rates of abortion, potentially because unlike the laws studied in [Lindo and Pineda-Torres \(2021\)](#); [Altındağ and Joyce \(2022\)](#), the earlier laws principally required a single trip to abortion providers. This is supported by [Myers \(2021\)](#) who explicitly considers the differential impacts of mandatory waiting periods which imply two-trip versus one-trip to abortion clinics. She finds that while one-trip waiting period restrictions do not have substantial effects on rates of abortion or birth, in cases where two-trip limits are imposed, this results in substantial increases in rates of abortion occurring in the second trimester, declines in abortion rates among state residents by around 9%, and increases in birth rates of around 1.5%. Such results are larger among low income and among hispanic and black women, while also being larger in states with larger travel times to out of state abortion clinics.

An alternative set of studies examines recent laws in the US which seek to limit the provision of abortion from the *supply* side. These laws, referred to as Targeted Regulation of Abortion Providers (or TRAP laws), impose a series of restrictions on abortion providers including requiring that they meet the requirements of Ambulatory Surgical Centres (ASCs) which can be costly, especially for small clinics, or that they have admitting privileges at a nearby hospital. Such reforms have been studied by (among others) [Colman and Joyce \(2011\)](#) who focus on ASCs in Texas, [Fischer et al. \(2018\)](#); [Lindo et al. \(2020a\)](#) who study Texas' HB2 law, which imposed ASC and hospital admitting privileges (including maximum distances to nearby hospitals), [Venator and Fletcher \(2020\)](#), who study a series of laws in Wisconsin including ultrasound requirements and [Arnold \(2022\)](#); [Caraher \(2023\)](#); [Jones and Pineda-Torres \(2021\)](#) who consider state-level reforms nation-wide. All told, these papers suggest that demand side laws can have substantial effects on rates of clinic closures, the distance individuals must travel to reach their nearest clinic, and finally on rates of abortion. For example [Arnold \(2022\)](#) presents convincing event-study evidence which suggests that the passage of TRAP laws reduces rates of abortion by 5% in the short run, and by more than 10% in the medium run. Studies by [Fischer et al. \(2018\)](#); [Lindo et al. \(2020a\)](#); [Venator and Fletcher \(2020\)](#); [Myers \(2023\)](#) as well as earlier work by [Joyce et al. \(2013\)](#) all provide convincing evidence that a relevant mechanism is increasing travel times owing to clinic closures. For example, the estimates of [Fischer et al. \(2018\)](#) based on clinic closures in Texas, show that the when a county does not have an abortion clinic within

25 miles, abortion rates fall by 16.6% compared to counties which do. Recent work by [Caraher \(2023\)](#) provides an analysis based on rich data with abortion rates at the level of the county in the US and considering both demand side (mandatory waiting period) and supply side (TRAP) laws, and finds that supply side laws result in larger declines in abortion rates.

While the majority of the economic literature focusing on abortion reforms and abortion access is set in the US, [Brooks and Zohar \(2021\)](#) are also able to provide evidence from the expansion of free provision of elective abortion in Israel in 2014. In this settings, free provision is found to result in relatively smaller effects on abortion rates than those found based on funding cuts due to Medicaid in the US, with the authors documenting that free provision increases rates of abortion by 4.5-7%, however there is a socioeconomic gradient in this result, consistent with findings from Medicaid in the US.

4.2 How Does Abortion Reform Shape The Outcomes of Affected Cohorts of Women?

4.2.1 Fertility and Family Formation

In line with abortion reform affecting abortion rates, there is considerable evidence in a number of settings that changes in abortion availability have important impacts on fertility, with evidence that it also affects young women's marriage decisions. Results are observed from both liberalizations in abortion policy, which have been shown to result in declines in birth rates, as well as in the implementation of abortion restrictions, which have been shown to bring about sharp increases in birth rates.

Naturally, the effect of reforms is highly context-dependent. The magnitude of the effect depends upon how much *De Jure* legislative change generates *De facto* policy change. For example, certain reforms are radical changes, dramatically altering the nature of access to abortion when comparing pre-reform to post-reform circumstances. Situations of this nature include cases such as the Romanian criminalisation (1966) and legalization (1990), as initially prior to 1966 abortion was widely used, and post-1966 sanctions were extremely strict and highly monitored. A similar case, though slightly less extreme was that of *Roe v. Wade* in 1973 in the US. Prior to the reform abortion was available in only certain early-access states ([Myers, 2022](#)), while post-reform abortion was available nation-wide upon request. Both of these reforms could be conceivably expected to generate large changes in fertility

given that they reforms have substantial bite (see Section 4.1). Other reforms, on the other hand, are likely to have smaller effects on fertility given that their *De facto* impact is lower. For example, a number of state-level legal tightenings in Mexico are considered by [Clarke and Mühlrad \(2021\)](#), but given that these simply increase sanctions on (already criminalised) abortion, any fertility effects are likely to be modest.

In a land-mark study in the United States, [Myers \(2017\)](#) suggests that liberalisation in abortion access during the 1970s lead to large changes in rates of fertility as well as shotgun marriage among young women. Using state-level variation in abortion availability for young women during the period of 1970–1973 and a difference-in-difference design, she estimates that abortion liberalization is responsible for a three percentage point (or 34 percent) decline in first births, and 19 percent decline in first marriages prior to the age of 19, with even larger declines observed in so called “shotgun marriages”.⁵ These results condition on the availability of the contraceptive pill, with the effects of the contraceptive pill observed to be small in comparison to those of abortion availability, particularly when abortion is available to minors without parental consent restrictions.

These effects cohere with other estimates documented from liberalising policy changes which similarly resulted in large effective changes in abortion law. [Mølland \(2016\)](#) estimates that liberalising reform in Norway in 1969 resulted in a 3pp decline in teenage motherhood (a 16% decline), with no effect on completed fertility. [Pop-Eleches \(2010\)](#) finds even larger effects following the Romanian liberalisation of 1989: a decline in 30% in fertility rates, which *does* affect completed fertility. Liberalisation in Mexico in 2007 is similarly observed to produce declines in birth rates of around 8% for all women [Clarke and Mühlrad \(2021\)](#) with similar figures observed following liberalisation in Uruguay, at least among unplanned birth ([Antón et al., 2018](#)). What all these reforms share is that they increase access to abortion, and generate a substantial discontinuity in the nature of the policy environment moving from pre- to post-policy, in each case removing any limits (apart from gestational limits) in how abortion can be sought. It is important to note that even in cases where certain limits remain in place, liberalisations can still result in substantial declines in fertility: see for example the case of Spain studied in [González et al. \(2018\)](#), who document a 6% decline in fertility among young women exposed to legalised abortion despite the fact that abortion was limited to three particular cases (additional discussion of this case was provided in Section 4.1).

⁵These are defined as marriages which are followed by a birth within the next 8 months.

Such results are not limited to liberalising abortion reforms. Indeed, generally speaking, effects of approximately the same size (or larger) but the opposite direction are observed when abortion restrictions are put in place. One example of this in an historical setting is [Lahey \(2014a\)](#) who studies US state-level anti-abortion laws put in place between 1850–1910. Using census micro-data and difference-in-differences style modeling she finds that the existence of a state-level anti-abortion law increases the child to woman ratio by 12%. This finding is echoed in more recent literature. When abortion was criminalised in Romania in 1966, the number of births was immediately observed to more than double, with [Hjalmarsson et al. \(2021\)](#) estimating an increase in the number of births by 133% following the abolition of abortion. While this context resulted in the highest change in birth rates documented in the literature, this likely owes to (a) the relative frequency of abortion at baseline in Romania (b) the strict nature of the ban and its enforcement, and (c) the fact that use of clandestine abortion only grew in later years, rather than at the (surprise) announcement of the legal reform.

A number of papers document how impacts on fertility occur over the life-cycle. A key-question in this setting is whether any impacts of abortion reform on fertility are transitory, simply allowing individuals to time the births they would like to have in a more optimal way, or permanent, implying that the availability of abortion results in fewer births on average when individuals reach the end of their childbearing years. [Ananat et al. \(2007\)](#), studying the case of early legal access in the US in the 1970s find that in this setting, fertility declines are permanent: women not giving birth as a result of increased abortion availability in this period do not tend to go on to increase births later in life. What's more, they find that much of this result owes to women being more likely to remain childless. A similar effect from abortion restrictions is observed in Romania, with [Pop-Eleches \(2010\)](#) documenting that women who spent much of their fertile life under periods with sharp limits on abortion access had higher life cycle fertility than later cohorts after abortion bans were lifted. However, these results are not observed in all contexts. Both [Mølland \(2016\)](#) and [González et al. \(2018\)](#) find that while liberalising abortion reforms in Norway and Spain respectively resulted in declines in fertility early in life, these are transitory rather than permanent, principally affecting timing rather than total fertility.

While in many contexts including those discussed above the impact of abortion reform on fertility is clear, in other cases impacts are less clear cut. One such example are parental involvement laws in the US, where the literature suggests divergent results. Early work from [Kane and Staiger \(1996\)](#); [Levine \(2003\)](#) suggests that there is relatively little evidence that these laws have aggregate effects

on teen fertility, though more recent work suggests that the bite of these laws have increased, with significant increases in fertility observed with the passage of laws [Myers and Ladd \(2020\)](#). As [Myers and Ladd \(2020\)](#) note, these results may owe to the broader context in which reforms take place, as the growing frequency of parental involvement laws means that the existence of nearby states which offer abortion without parental involvement is limited, unlike in earlier periods where out-of-state travel may act as an imperfect substitute for in-state access to abortion without parental involvement.

Notwithstanding some particular settings in which the fertility effects of abortion reforms may be more moderate, the general picture painted from the economic literature is that abortion availability has important impacts on birth rates, fertility timing, and at times, total fertility. This is observed in a range of studies, covering abortion restrictions and liberalisations, a range of time periods from as early as 1850 to as late as the 2020s, and a range of countries and continents, pointing to the enduring importance of abortion as a birth control method across space and time. There is relatively little evidence in the economic literature on abortion to suggest that abortion access is irrelevant as a fertility control mechanism, even in settings where modern contraceptives such as the pill are available.

4.2.2 Women's Human Capital Attainment

The impact of abortion reform on fertility has myriad longer term impacts on women and families. Consider the legalisation of abortion and the corresponding changes in timing of fertility. If women's ability to control their fertility improves, this potentially allows delays in marriage and allows women to invest more in their own human capital ([Katz and Goldin, 2002](#)). Empirical evidence of the relevance of this relationship has been documented in a number of contexts.

In 1960s Norway, [Mølland \(2016\)](#) substantiates this link between abortion legalisation and women's education, finding that after the early passage of abortion reforms in Oslo, women were 1.8% more likely to graduate with a college degree, and 0.8% more likely to gain an advanced degree. She does not observe evidence of impacts on completion of high school. Educational effects are also documented by [Angrist and Evans \(1996\)](#) who study US state abortion reforms progressively legalising abortion between 1967 and 1973 (similar results are discussed by [Ananat et al. \(2007\)](#), though results are mixed). They document that these abortion reforms increase the likelihood that black women attend college by around 3pp, with no similar effect observed for white women (or for men). Finally,

evidence from Spain presented in [González et al. \(2018\)](#) suggests that greater availability of abortion reduces the likelihood of high-school dropout. As in the case of fertility, in the case of education effects are observed to be bi-directional: [Pop-Eleches \(2006\)](#) documents that mothers exposed to the 1966 Romanian abortion ban are 4.8pp less likely to have primary education as their highest level of education, and 4.5pp more likely to have a secondary education. [Jones and Pineda-Torres \(2021\)](#) document that abortion restrictions can translated into declines in educational attainment quite quickly. Studying black women in the USA, they find that TRAP laws implemented in the 1990s and 2000s resulted in declines in rates of college initiation of around 2% for women exposed to restrictions during their adolescence. All of these results point in the direction of greater agency in birth timing owing to abortion liberalisation being positive for human capital accumulation, and reduced agency compressing educational attainment.

4.2.3 Labour Market Outcomes

Effects similar to those on education are observed when considering labour market participation (both at the extensive and intensive margin), salaries, and financial security. Many papers which study women's human capital accumulation, also document follow on effects on labour market outcomes. This includes [Angrist and Evans \(1996\)](#) who find that as with educational attainment, employment rates for black women (but not white women) are higher by around 1.2pp when abortion is available, though they find no evidence of impacts on log earnings. [Kalist \(2004\)](#), who examines this context with different data and slightly modified design also finds that black women have substantially larger labour market effects than white women, though does find small effects of abortion legalization during the 60's and 70's in the US for white women. This results is expanded upon by [Mølland \(2016\)](#) who documents substantial effects of abortion legalisation on labour market participation across the life course for women: with around 2pp higher rates of participation up to around the age of 35, at which point effects become negative, in line with re-optimised fertility timing. Similarly, in a US setting [Lindo et al. \(2020b\)](#) document that access to abortion increases the likelihood that women work in jobs with Social Security coverage early in life (in their 20s and 30s), with negative impacts later in life, reminiscent of the cyclicalities documented by [Mølland \(2016\)](#). While the majority of this literature examines laws which liberalise access to abortion, [Bahn et al. \(2020\)](#) provide evidence from US "TRAP" laws which reduce access. They find results which suggest that, conversely to

the liberalising effect of abortion availability, the targeting of abortion providers results in depressed labour market opportunities for women, specifically by reducing the ability which women have to move between jobs, and access higher paying occupations.

While the principal channel which likely explains these effects is greater flexibility to accumulate human capital (Section 4.2.2) and participate more freely in the labour market when fertility is more optimally timed, an additional channel has been noted in the economic literature. Theoretically, [Chiappori and Oreffice \(2008\)](#) note that abortion legalisation will increase bargaining power of women, and via an income effect, lead to increases in men's labour supply, and declines in women's labour supply. [Oreffice \(2007\)](#) finds support for this model empirically, noting immediate changes in labour supply of men and women surrounding abortion legalisation in the US in the 1960s and 1970s. In the long-run, any empowerment effect will be combined with effects flowing from additional human capital gains, meaning that these short-run effects will no longer be able to be identified cleanly, however at least in the short run, [Oreffice \(2007\)](#) documents the existence of such effects.

4.2.4 Health Outcomes

There are a small number of studies considering the impacts of abortion availability on women's health outcomes. These are largely focused on reproductive health measures, with access to safe and legal abortion being convincingly found to reduce health complications and rates of maternal death. A clear example based on a recent reform is studied in [Clarke and Mühlrad \(2021\)](#), who consider the passage of legal abortion in Mexico City, in 2007. They document declines in hospitalisation both in examining rates of haemorrhage and abortion-related morbidity. They do not find evidence that contemporaneous increases in penalisations of abortion in other states in Mexico had any clear impact on rates of morbidity.

Evidence from an historical setting has been recently studied by [Farin et al. \(2022\)](#). Examining state-level changes in abortion law in the US between 1969-1971 (pre *Roe vs. Wade*) and in 1973 (*Roe vs. Wade*), they document that the availability of legal abortion brought about declines in maternal mortality, and abortion-related mortality in particular, and that this was driven by declines in mortality rates among non-white women. Both sets of results – those from Mexico considering morbidity and from the US considering mortality – point to substantially-sized impacts. [Farin et al. \(2022\)](#)

find declines in non-white maternal mortality rates of between 30-40%, while [Clarke and Mühlrad \(2021\)](#) find declines in morbidity of between 20-35%. Strikingly, as a headline figure [Farin et al. \(2022\)](#) estimate that nationwide in the US, 113 non-white maternal deaths were averted the first year abortion was legalised, suggesting major welfare implications which cannot be ignored in any policy considerations related to abortion reform.

4.2.5 Other Outcomes

Abortion policies have also been documented to affect a number of other dimensions when focusing on cohorts of women exposed to reforms. This includes empowerment, financial well-being, and life-satisfaction.

A particularly clean counterfactual for considering access to abortion is the Turnaway study, discussed in [Miller et al. \(2020, 2023\)](#). Based on gestational length limits and access to abortion, this study worked with a sample of women who requested abortions and were either just below the gestational length limit (a Near Limit group), or just over the gestational length limit (the Turnaway group). The Turnaway study followed these two groups over a substantial period of time (5 years), leveraging the vastly different trajectories owing to accessing or being turned away from abortion owing to the gestational length limit. In [Miller et al. \(2023\)](#), these data are additionally matched to data on financial outcomes from credit reports. The authors find that when compared to the Near Limit group, the Turn Away group was observed to have substantially worse financial outcomes, being more likely to have overdue debts or to have suffered adverse events such as bankruptcy or eviction. What's more, these effects are observed to persist for an extended period of time.

A small number of studies have documented impacts of abortion reform on empowerment of women. One such series of papers, mentioned above, is [Chiappori and Oreffice \(2008\)](#); [Oreffice \(2007\)](#) who note a theoretical channel whereby abortion legalisation will empower women, with [Oreffice \(2007\)](#) documenting empirical results in support of this model. A handbook chapter by [Bernstein and Jones \(2021\)](#) notes more generally a range of findings linking women's reproductive health – including access to abortion – to empowerment.

Finally, recent results from [González et al. \(2018\)](#) suggest that abortion access may be relevant for women's life satisfaction. Using household survey data and subjective measures of life satisfaction,

they find some evidence to suggest the women with greater access to abortion at younger ages are satisfied, at least when considering their living conditions and time devoted to leisure.

4.3 How Does Abortion Reform Shape the Composition of Birth Cohorts?

A series of influential papers in the late 1990s and early 2000s brought into focus the question of how access to abortion shaped the characteristics of birth cohorts. If access to abortion implies that women and families are more likely to take desired pregnancies to term, and not continue pregnancies which are unwanted, this will imply that children born after the legalisation are more likely to have been planned, and potentially exposed to different home environments and family investments. These ideas were formalised in [Gruber et al. \(1999\)](#) examining the circumstances of cohorts of children at young ages, with later work of [Donohue and Levitt \(2001\)](#) considering the implications of this for later life outcomes (namely, crime rates). These papers have been followed by a substantial literature with the modelling implications of “Abortion and Selection” laid out in [Ananat et al. \(2009\)](#).

This focus on abortion and selection has precedents in earlier work. [Kane and Staiger \(1996\)](#) document that restrictions in access to abortion in the US lead to declines in rates of teen births in the population, and [Lundberg and Plotnick \(1995\)](#) note potentially divergent impacts of abortion reform by race in the US, with both of these results suggestive that changes in abortion legislation will be reflected in changes in the mean characteristics of affected birth cohorts. [Currie et al. \(1996\)](#) additionally examine the impact of abortion restrictions on average health at birth of affected cohorts, suggesting that they observe weak evidence of a decline in birth weight. At an aggregate level [Blank et al. \(1996\)](#) document that rates of abortion correlate with state-level economic factors as well as demographic factors, again potentially suggesting that changes in abortion laws will map into changes in cohort characteristics.

Conceptually, the impacts of abortion reform on cohorts could owe to a number of mechanisms. [Pop-Eleches \(2006\)](#) notes three principal mechanisms by which abortion policy may affect the composition of children. The first is an “**unwantedness**” channel: if children are more likely to be planned, parents may invest differentially or have more to invest in their children. The second is a **compositional** channel: if certain groups are more or less likely request an abortion, legalisation of abortion will change the aggregate characteristics of families giving birth. And the third is a potential **crowding**

channel: if abortion reform affects total cohort size, this may have some general equilibrium effects, such as more overcrowding in schools.

4.3.1 Abortion and Living Conditions

The impact of changes in abortion law on living conditions early in life have been documented in a number of papers and across settings. Studying variations in access to abortion in the 1970s in the US, the work of [Gruber et al. \(1999\)](#) documents that the ‘marginal child’ affected by abortion policy would have been at least 60% more likely to live in single parent households had abortion not been available, 50% more likely to live in poverty, 45% more likely to receive welfare, and 40% more likely to have died in the first year after birth. The early-life impacts of abortion legalisation in this period is similarly documented in [Ananat et al. \(2009\)](#), who find that the immediate impacts of legalization in the US in the 1970s is to reduce rates of teen motherhood among affected cohorts by 8.5%, and reduce the likelihood of non-white births by 5.3%. Both of these sets of results can be considered to be estimates of the compositional channel, as they refer to how changes in usage of abortion across groups shapes the characteristics of cohorts. What’s more these changes have been documented to have effects on hard measures of child well being. [Bitler and Zavodny \(2001, 2002\)](#) show that the legalisation of abortion propelled declines in child abuse and neglect, with this result potentially owing to all three channels above.

Impacts of abortion law on child living conditions have been documented in other contexts. Specifically, [Mitrut and Wolff \(2011\)](#) document impacts of abortion legalisation in Romania on child abandonment. They document that following legalisation of abortion in 1989, rates of child abandonment fell by around four children per 10,000 births, evidence of quite extreme impacts of abortion legalization on the living conditions of the marginal child in certain situations. The earlier criminalisation of abortion in Romania has been comprehensively demonstrated to result in positive selection in terms of mothers’ characteristics. [Pop-Eleches \(2006\)](#) documents that prior to the criminalisation of abortion in 1966, mothers which were highly educated and who lived in urban areas were more likely to seek abortions, and as such, following criminalisation the proportion of children living in more educated and urban household increased (see also discussion in [Hjalmarsson et al. \(2021\)](#)). It is noteworthy that in this particular case, the compositional effects are quite different to those documented with the

passage of abortion laws in the US in 1970s. In the United States, when abortion was legal, it was more common among socioeconomically disadvantaged groups (Gruber et al., 1999). In contrast, in Romania, the situation was reversed, with abortion being more prevalent among socioeconomically advantaged groups. Below we return to discuss how impacts on characteristics at birth (a compositional channel) may shape outcomes in adulthood, where both composition, as well as other channels, may be relevant.

4.3.2 Characteristics of Cohorts in Adulthood

When considering outcomes later in life, a number of results have been documented. Early evidence from the US presents clear results consistent with abortion legalisation resulting in positive changes in mean life outcomes for cohorts when they reach maturity. Ananat et al. (2009) finds that at around the age of 30, individual from birth cohorts which were subject to legal abortion have a substantially lower likelihood of living in poverty, being a single parent, being on welfare, and not having graduated from college than cohorts in which abortion was not legally available. Charles and Stephens (2006), considering the same context and using data on substance abuse, document lower rates of drug abuse among cohorts where abortion was available when these cohorts reach adolescence. In a non-US context, Mølland (2016) finds that the legalisation of abortion in Norway similarly resulted in positive human capital outcomes for children of exposed cohorts at adulthood, both in terms of educational outcomes, labour market participation at age 30, and a reduced reliance on welfare. Gutierrez (2022) also documents that cohorts exposed to abortion at birth have impacts beyond human capital, finding that in Romania, children of mothers exposed to the abortion ban have significantly lower rates of fertility across their entire fertile life when they reach maturity.

A particularly clear case considering the multiple possible channels by which abortion may shape adult outcomes is Pop-Eleches (2006). Examining the criminalisation of abortion in Romania, he documents that at adulthood children of cohorts where sharp limits were in place restricting access to abortion had considerably *improved* outcomes in terms of education and labour market measures. This result is the opposite of what one may have suspected from the aforementioned studies, given that in the other contexts, following abortion legalisation cohorts of mothers were observed to be – on average – more positively selected on socio-economic status. However, Pop-Eleches (2006) documents that this

result also owes to a compositional channel. In the case of Romania, he documents that where abortion was legal, individuals who accessed abortion were selectively more highly educated, and more likely to live in urban areas. Thus, when abortion was legalised these individuals were relatively more likely to give birth, and as a result the composition of birth cohorts moved towards more highly educated mothers. A key result from the study by [Pop-Eleches \(2006\)](#) is that despite this selection channel, he can additionally document impacts of the unwantedness channel. In particular, he notes that when conditioning on the composition of mothers, effectively shutting off the compositional channel, he observes the individuals born following the abortion ban have worse outcomes than similar individuals born prior to the abortion ban. Thus, in this particular setting [Pop-Eleches \(2006\)](#) is able to document the relevance of both of these channels, additionally showing that the nature of any compositional effects is context-dependent.

4.3.3 Abortion, Child Circumstances and Crime

Arguably one of the most visible debates in the economic literature of abortion has been given to questions related to abortion legalisation and crime. [Donohue and Levitt's 2001](#) landmark study proposes the “abortion–crime” hypothesis. Their paper proposes the theory that a decline in rates of homicides and other violent crime in the early 1990s in the US may be largely explained by the legalisation of abortion two decades earlier. This explanation coheres with the literature discussed in this section which suggests that abortion legalisation resulted in substantial changes in characteristics of birth cohorts, and additionally, broadly lines up with macro-level trends in crime in the United States in the period in which the work was published. The original authors propose a quite clear link between abortion legalisation and crime (see for example [Donohue and Levitt \(2020\)](#)): namely (a) if children are not desired, they are at an elevated risk of facing unfavourable life circumstances including criminal involvement, (b) abortion reduces rates of undesired births, and (c) as a consequence, cohorts exposed to abortion should engage in less criminal activity. This argument makes clear that the selection channel, and in particular the argument relating to unwantedness, is key in mediating the proposed hypothesis.

Despite these broad facts in favour of the abortion–crime hypothesis, the estimated impact of abortion legalisation on crime rates is very large (the authors estimate that abortion legalisation may

explain around half of the total crime reduction observed in the 1990s), and the empirical results underlying this paper have been questioned both in terms of a coding mistake in specific models (Foote and Goetz, 2008), as well as substantive contextual questions (Joyce, 2004, 2009; Foote and Goetz, 2008; Belloni et al., 2013; Hjalmarsson et al., 2021, for example). Reading across studies and contexts, it appears that the weight of the evidence in the literature suggests that the abortion-crime hypothesis – and especially the argument that wantedness of births results in declines in rates of crime – may not hold up to substantial scrutiny. Nevertheless, the early work of Donohue and Levitt has propelled a considerable and still-active literature examining the abortion crime hypothesis, capturing both academic and popular interest. This literature is substantial, and indeed, a number of papers provide broad overviews of the main findings and controversies. To capture the full nuance of the discussion and academic debate regarding the abortion–crime hypothesis, these papers are likely worthwhile starting points. Specifically, Joyce (2010) provides a handbook-length discussion of the first 10 years or so of debate on the abortion–crime hypothesis, proposing a number of key facts which appear to disprove the posited causal link, while Donohue and Levitt (2020) revisit their original work 20 years later suggesting original models still hold up. Below, the broad lines of this research are laid out, as well as key questions which challenge the veracity of the abortion-crime hypothesis.

The original paper by Donohue and Levitt (2001) examines the impact of state-level abortion rates which particular cohorts were exposed to at birth on rates of crime per capita and on raw total arrests by age group. For the period covering 1985-1997, they present estimates which suggest that crime was between 15-25% lower in 1997 than it would have been in the absence of legalised abortion. Given the total decline in crime during the 1990s, this result leads to the striking headline that the legalisation of abortion can explain around half of the decline observed in violent crime over this period. This result has been questioned for a number of reasons. Foote and Goetz (2008) raise three concerns that range from coding errors in which state-year fixed effects were not included in regressions, to failure to account for rates in models examining total arrests. In Donohue and Levitt (2001), the log of total arrests by age group is regressed on abortion rates, and Foote and Goetz (2008) note that when crime rates rather than total crimes are regressed on abortion rates, significant effects often do not remain. This appears to be an important point, because the selection argument posits not that total criminal activity should fall, but rather rates of criminal activity should fall, or in other words, that crime should not simply scale with any declines in cohort size due to the legalisation of abortion. A

second argument put forward by [Joyce \(2004\)](#) raises additional concerns: namely the possibility that unmeasured exposure to the Crack-cocaine epidemic may explain a substantial portion of the affect attributed to abortion on crime. The original authors have responded to these points suggesting that revised estimates still point to evidence in favour of their original hypothesis [Donohue and Levitt \(2004\)](#); [Donohue III and Levitt \(2008\)](#); [Donohue and Levitt \(2020\)](#). Two other pieces of important evidence are raised in [Joyce \(2009\)](#) and [Belloni et al. \(2013\)](#). [Joyce \(2009\)](#) notes that if abortion availability alters cohort composition and crime rates, declines in crime should be observed differentially over time in cohorts progressively exposed to abortion legalisation. In [Joyce \(2009, 2010\)](#) he documents that such cohort-specific patterns are not observed, a point similarly noted by [Lott Jr and Whitley \(2007\)](#). Finally, [Belloni et al. \(2013\)](#) in a methodological paper introducing their post-double selection estimator consider the abortion-crime hypothesis as an empirical example with which to apply their methods. When conducting variable selection using double-debiased machine learning techniques, they fail to find statistically significant effects of abortion legalisation on changes in crime, noting that the hypothesis is thus sensitive to considerations of covariates included in [Donohue and Levitt \(2001\)](#)'s original analysis.

Given that there is substantial debate related to the specific empirical example studied by [Donohue and Levitt \(2001\)](#) (United States abortion legalisation in the 1970s), to further clarify thinking on this issues it is useful to examine results from other contexts. Recent work by [Hjalmarsson et al. \(2021\)](#) studies the impact of the large changes in Romania's abortion policies on crime rates as an additional test of the abortion crime hypothesis.⁶ In their setting, they consider both the criminalisation of abortion in 1966, and the legalisation of abortion in 1990. While they document a large increase in rates of crime with the abolition of abortion and a decline in rates of crime with its legalisation, it turns out that this effect simply owes to changes in the number of births, with smaller cohorts mechanically committing less crime (and vice versa with larger cohorts). When considering rates, they find no evidence of changes in crime *rates*, leading them to conclude that any effect of abortion on crime in this setting is simply mechanical, rather than owing to any compositional or wantedness channels. Interestingly, a similar conclusion was reached by [Ananat et al. \(2009\)](#) in the United States, suggesting that any abortion-crime link is likely largely mechanical rather than owing to selection. There are a number of additional studies which have examined international experiences including [Kahane et al.](#)

⁶[Pop-Eleches \(2006\)](#) also touches on this debate, however [Hjalmarsson et al. \(2021\)](#) is focused exclusively on the abortion-crime link and interacts closely with [Pop-Eleches \(2006\)](#), and so we focus on this study here.

(2008) (England and Wales), [Kahane et al. \(2008\)](#) (Canada) and [Buonanno et al. \(2014\)](#); [François et al. \(2014\)](#) (panel of European countries and US). Overall, these studies suggest mixed results with no clear consensus in these four settings.

In summary, despite a compelling hypothesis and multiple descriptive facts which appear to be consistent with the abortion–crime hypothesis, a number of highly credible recent papers suggest that this hypothesis should be viewed with considerable caution. While in multiple contexts there is strong evidence that abortion passage results in declines in the *absolute number* of crimes (consistent with smaller cohorts mechanically committing fewer crimes), the evidence in favour of a selection effect are much weaker, with a number of key tests which do not appear to suggest that changes in abortion rates drive down *rates* in crime.

5 Policy and Behavioural Interactions with Abortion Reform

A final point of interest in the economic literature on abortion reform is related to how individuals, policy makers and policy more generally may interact with abortion reform. The discussion provided up to this point suggests that abortion reform may have substantial implications for individual outcomes in a partial equilibrium sense. However, it is of interest to consider what the ripples of abortion policy may be in a more broad sense.

A small number of papers have studied how abortion reform interacts with available technologies and social norms. Two of these papers consider the routine availability of ultrasounds which are capable of reliably detecting fetal sex early in gestation. These papers ([Lin et al., 2014](#); [Anukriti et al., 2021](#)) both take place in settings where there is a well-documented preference for sons, namely Taiwan and India. [Lin et al. \(2014\)](#) document that the legalisation of abortion in Taiwan in 1985 in a setting where sex-detecting technology is widely available increased the rate of boys born at higher parities, and reduced rates of relative neonatal mortality among girls at higher parities. [Anukriti et al. \(2021\)](#) considers a similar interaction, however in this case studying the introduction of ultrasound in a setting where abortion was already available and where sex-detecting ultra-sound has been documented to substantially alter the gender composition of birth cohorts ([Bhalotra and Cochrane, 2010](#)). They find clear evidence that the interaction of these reforms improves the well-being of girls in a number of dimensions – most clearly reducing excess female mortality, but also reducing dispari-

ties in the duration of breastfeeding of boys compared to girls, and rates of vaccination of boys to girls. These policy interactions can be conceived as complementarities between abortion policy and ultra-sound technologies. However, [Goff et al. \(2023\)](#) document in quite a different setting that such complementarities do not occur when considering the interaction between abortion legalisation and access to improved school environments. While they separately find that abortion reform and higher quality schools improve test scores, they do not find evidence of complementary interactions between improved home environments owing to abortion reform and higher school quality.

In general, these studies suggest that the implications of abortion policy may play out in a number of ways depending on the specific context of countries in which reforms occur. At times, the impacts of abortion policy decisions may result in unexpected, and entirely undesired responses from individuals or policy makers. One such case is documented by [Jones \(2015\)](#) who studies the impact of the Global Gag Rule (GGR) in Ghana. While the GGR ostensibly aims to reduce rates of abortion by cutting US Aid funding for overseas NGOs which provide information related to abortion, [Jones \(2015\)](#) documents that the policy has the converse effect, given that it drives down access to a broad range of contraceptive methods, resulting in a corresponding increase in pregnancies, abortion and birth rates. In another context, [Clarke and Mühlrad \(2021\)](#) note that policy makers may respond to abortion policy outside their jurisdiction and themselves implement policy reform. They document that progressive abortion policy in one state in Mexico lead to a backlash in other states, at least in the short term, with criminal codes in other states being tightened to seek to further discourage access to clandestine abortion.

The effects of abortion policy have been shown to depend more generally on the way policy changes occur in space. A growing literature has shown that the way *local* (ie state-level) reforms work will depend more generally on interactions between each area and nearby localities. Specifically, where abortion restrictions are imposed in a sub-national setting, the travel distance to the nearest available abortion provider has substantial impacts on both access to abortion and birth rates. Some discussion of this is provided in [Section 4](#), and given a spate of state-level reforms in the US over the last two decades, this question has attracted considerable recent attention. Overall, a range of papers point to the costs of distance to the nearest abortion clinic being substantial, non-linear, relevant both historically as well as at present, and depend considerably on individual characteristics. Work by [Joyce et al. \(2013\)](#) shows that in the 1970s in the United States, distance to the nearest available abortion

clinic had an important impact on access to abortion. The authors suggest that a 100 mile increase in distance for women living around 180 miles from an abortion clinic reduces rates of abortion by 12.2%, whereas the same distance change for individuals living 830 miles away reduces rates of abortion 3.3%. Certain groups were found to be particularly sensitive to distance: in particular younger women, and non-white women. Across a range of studies examining recent policy reforms these results have been found to still be relevant (Fischer et al., 2018; Lindo et al., 2020a; Myers, 2023; Venator and Fletcher, 2020), both in terms of the magnitude of the effect on access to abortion, as well as in finding the existence of substantial non-linear effects. Using novel data on the location of abortion clinics as well as state-level reforms across the entire US, Myers (2023) documents the importance of distance in both explaining abortion rates, *as well* as explaining birth rates. Like impacts of distance on access to abortion, impacts on birth rates are found to be non-linear. Myers (2023) documents that increasing distance to the nearest clinic by 100 miles increases birth rates by 2.2% for the first 100 mile increase, and this figure declines to 1.6% for the next 100 mile increase. She estimates that the costs of these changes in distance are largest for younger women, especially teens and individuals in their early 20s.

Finally, a number of studies note that individuals themselves may react to abortion policy altering their behaviours over a range of dimensions. One consideration is that individuals will react to abortion laws by reducing rates of sex. In considering parental involvement laws in the US which increase the challenges which teenagers face when seeking abortion, Colman et al. (2013); Levine (2003); Sen (2006) do not find evidence consistent with teens reducing sexual behaviour, but both Levine (2003); Sabia and Anderson (2016) do find evidence consistent with these laws increasing usage of other birth control methods. In a related setting, Ananat and Hungerman (2012) document evidence both at a micro- and state-level consistent with the availability of the oral contraceptive pill reducing young women's use of abortion. More recently, when considering reductions in access to abortion clinics in Texas and all age contraceptive purchase, Fischer et al. (2018) do not find any substantial response in terms of purchases of condoms or the emergency contraceptive pill.

6 Conclusion

This article provides an overview of the state of economics research on abortion. It seeks to cover the range of abortion policies in place world-wide and provide a review of the reforms studied in the economic literature, which have generally occurred over the last 50+ years. It covers a number of key questions, ranging from a limited literature on the determinants of abortion reform, to a substantially broader literature on the impacts of abortion policy on women, families, and cohorts of children. This research has largely been motivated by policy changes which have occurred over the last half century, and so has been more focused on liberalising reforms to abortion policy in low fertility settings. Nevertheless, the literature covers a vast array of types of policy changes including a recent spate of restrictions limiting both demand for abortion and supply of abortion.

In the interests of space, this article has focused on the economic literature on abortion. While this is a large field of study with a number of key themes and questions, this restriction necessarily reduces the breadth of questions and methods. A comprehensive review of abortion policy could productively draw on links with studies from reproductive health, public health, sociology, anthropology, legal studies, gender studies, demography, philosophy, and psychology, among other fields, although this is outside the scope of this article.

Despite being a broad literature spanning multiple sub-fields of economics a number of open questions remain. Principally, and as discussed in this article, much of the extant literature in economics is driven off policy changes in a relatively small number of countries. Expanding the contexts studied will thus expand the frontier of knowledge in this area. Beyond expanding the scope of available evidence, the literature on the impacts of abortion policy will constantly evolve to the degree that policies and access to abortion evolve. This has been clear with the uptick in recent studies examining TRAP laws in the USA, in line with their growing relevance in limiting access in this setting. Future themes in abortion research will likely similarly track innovations and key changes in access. Thus, growing use of technologies such as abortion pills which can be taken at home and delivered by post, limits in access owing to global funding restrictions in aid directed to maternal and reproductive health, and the use of tele-medicine services to bypass within-country restrictions are all potential areas of future fruitful work.

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