Essays in Public Finance and Labor Economics

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Abstract

This thesis examines three questions of causality relevant to public finance and labor economics: the effect of racial segregation on city characteristics, the effect of divorce on women's economic outcomes, and the effect of abortion legalization on completed fertility.

Chapter one examines the effect of segregation on cities. There is a strikingly negative city-level correlation between residential racial segregation and population outcomes—particularly for black residents—but it is widely recognized that this correlation may not be causal. This chapter provides a novel test of the causal relationship between segregation and population outcomes by exploiting the arrangements of railroad tracks in the 19th century to isolate plausibly exogenous variation in a city's susceptibility to segregation. I show that, conditional on miles of railroad track laid, the extent to which track configurations physically subdivided cities strongly predicts the level of segregation that ensued after the Great Migration of African-Americans to northern and western cities in the 20th century. Prior to the Great Migration, however, track configurations were uncorrelated with racial concentration, income, education and population, indicating that reverse causality is unlikely. Instrumental variables estimates find that segregation leads to negative characteristics for blacks and high-skilled whites, but positive characteristics for low-skilled whites. Segregation could generate these effects either by affecting human capital acquisition of residents of different races and skill groups ('production') or by inducing sorting of race and skill groups into different cities ('selection'). I develop a model to distinguish between production and selection effects. The findings are most consistent with the view that more segregated cities produce better outcomes for low-skilled whites and that more segregated cities are in less demand among both blacks and whites, implying that Americans on average value integration.

Chapter two, coauthored with Guy Michaels, examines the effect of divorce on women's economic outcomes. Having a female firstborn child significantly increases the probability that a woman's first marriage breaks up. We exploit this exogenous variation to measure the effect of marital breakup on women's economic outcomes. We find evidence that divorce has little effect on a woman's average household income, but significantly increases the probability that her household will be in the lowest income quartile. While women partially offset the loss of spousal earnings with child support, welfare, combining households, and substantially increasing their labor supply, divorce significantly increases the odds of household poverty on net.

Chapter three, coauthored with Jonathan Gruber and Phillip B. Levine, examines the effect of abortion legalization on completed fertility. Previous research has convincingly shown that abortion legalization in the early 1970s led to a significant drop in fertility at that time. But this decline may have either represented a delay in births from a point where they were "unintended" to a point where they were "intended," or they may have represented a permanent reduction in fertility. We combine data from the 1970 U.S. Census and microdata from 1968 to 1999 Vital Statistics records to calculate lifetime fertility of women in the 1930s through 1960s birth cohorts. We examine whether those women who were born in early legalizing states and who passed through the early 1970s in their peak childbearing years had differential lifetime fertility patterns compared to women born in other states and in different birth cohorts. We consider the impact of abortion legalization on both the number of children ever born as well as the distribution of number of children ever born. Our results indicate that much of the reduction in fertility at the time abortion was legalized was permanent in that women did not have more subsequent births as a result. We also find that this result is largely attributable to an increase in the number of women who remained childless throughout their fertile years.

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And, of course, without Hubris, nothing is possible.

The Wrong Side(s) of the Tracks:
Estimating the Causal Effects of Racial Segregation on City Outcomes

Elizabeth Oltmans Ananat

Abstract

There is a strikingly negative city-level correlation between residential racial segregation and population outcomes—particularly for black residents—but it is widely recognized that this correlation may not be causal. This paper provides a novel test of the causal relationship between segregation and population outcomes by exploiting the arrangements of railroad tracks in the 19^{th} century to isolate plausibly exogenous variation in a city's susceptibility to segregation. I show that, conditional on miles of railroad track laid, the extent to which track configurations physically subdivided cities strongly predicts the level of segregation that ensued after the Great Migration of African-Americans to northern and western cities in the 20^{th} century. Prior to the Great Migration, however, track configurations were uncorrelated with racial concentration, income, education and population, indicating that reverse causality is unlikely. Instrumental variables estimates find that segregation leads to negative characteristics for blacks and high-skilled whites, but positive characteristics for low-skilled whites. Segregation could generate these effects either by affecting human capital acquisition of residents of different races and skill groups ('production') or by inducing sorting of race and skill groups into different cities ('selection'). I develop a model to distinguish between production and selection effects. The findings are most consistent with the view that more segregated cities produce better outcomes for low-skilled whites and that more segregated cities are in less demand among both blacks and whites, implying that Americans on average value integration.

I. Introduction

Residential segregation by race is one of the most visible characteristics of many American cities. Although African-Americans represent just over one-tenth of the U.S. population, the average urban African-American lives in a neighborhood that is majority black (Glaeser and Vigdor 2001). Cities vary in the extent to which their black populations live in black neighborhoods, and more segregated cities on average have worse characteristics than less segregated cities, on measures ranging from infant mortality to educational achievement (Massey and Denton 1993).

This correlation is difficult to interpret, however. In what ways, if any, racial segregation causally affects outcomes is a longstanding question in social science. Two conceptual obstacles complicate identifying the answer. First, some economic, political, or other attribute[1] may lead some cities to have more segregation and also more negative city characteristics. This will cause omitted variable bias when estimating the bivariate relationship between segregation and city outcomes. Instrumenting for a city's level of segregation can help address this problem, thereby allowing the effect of segregation on macro city-level outcomes to be estimated.

A second conceptual obstacle arises from the fact that, although segregation must affect aggregate city characteristics through some effect on individuals, there at least two different ways it can do so. First, segregated cities may be less productive, leading to lower accumulation of capital (human and otherwise) for its citizens. Second, people may respond to segregation itself, and to any effects segregation has on production, by sorting between cities in ways that alter average city characteristics. Both of these phenomena are of economic interest, but only the first can be considered a causal effect

[1] For example, greater political corruption or a more industrial economy.

of segregation on the outcomes of *people*. The combination of the two, which is easier to observe, must be considered an effect of segregation on *places*.

In this paper I address concerns about omitted variable bias by using 19th-century railroad configurations to instrument for the extent to which cities became segregated as they developed African-American populations during the 20th century. I show that the proverbial convention of the "wrong side of the tracks" is helpful in identifying segregation; the more subdivided a city was by railroads (i.e., the more total "sides" there were to the tracks) the more segregated the city became during the Great Migration.

As a city began to develop a significant black population during the Great Migration, African-Americans became isolated in ghettoes in part because demand among the broader community for residential segregation grew (Weaver 1955). As the black population continued to expand, the physical size of a ghetto had to increase if segregation was going to be maintained. Since railroads generate neighborhood divisions, cities that were subdivided by railroads into many small insular neighborhoods could expand a ghetto by one neighborhood at a time and still practice "containment," whereby the black population remained concentrated and contiguous. On the other hand, in cities where expanding a ghetto meant breaching a main divide, then as the black population increased segregation could no longer be as easily maintained.[2]

Figure 1 illustrates this concept. Binghamton, NY, and York, PA, were similar in total quantity of railroad tracks laid by 1900 (shown in red, circumscribed by a four kilometer-radius circle). They also had similar industrial bases and substantial changes in

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[2] The proverb does not explain why it is that railroads tend to define neighborhood boundaries, although in many cases it is self-evident that they do. One likely possibility is that a railroad provides a clear demarcation that facilitates collective agreement on neighborhood boundaries by residents, real estate agents, police, and others. When a community is interested in remaining separate from a certain group, railroads could facilitate collective action in enforcing segregation by reducing coordination costs.

African-American population (these characteristics are discussed in detail later in the paper). But York's railroads were configured such that they created many insular neighborhoods, particularly in the center of the city. Its Census tracts, in the year 2000, show a black population more concentrated in this set of small, railroad-defined neighborhoods (tract percent black is represented by darkness of tract shading). In Binghamton, on the other hand, railroads are tightly clustered, leaving some areas too long and narrow to encompass neighborhoods and others too wide open to create meaningful population restrictions. In contrast to York, Binghamton's year 2000 Census tracts show its black population dispersed lightly and evenly throughout much of the city.

My research design relies on the assumption that the variation I observe in the railroad subdivision of neighborhoods, conditional on total track laid in a city, is exogenous. I provide a variety of tests of the validity of this assumption. Under this assumption, instrumentation allows me to identify the extent to which cities that initially are randomly assigned greater segregation end up having more negative characteristics overall. That is, it identifies the causal effect of segregation on places.

To identify the effect of segregation on people, it is necessary to determine how much of a city's characteristics result from the production effects of segregation and how much from the migration effects. I develop a simple model in which cities' equilibrium characteristics are driven by race, tastes, production effects of segregation, and relative housing demand. This model produces implications that I am then able to test empirically, providing some speculative estimates that decompose the macro effects of segregation on places into the effects on people and the effects on migration.

A number of other papers have attempted to measure the effects of segregation on outcomes (cf. Massey and Denton 1993, Wilson 1996, Polednak 1997). An influential contribution by Cutler and Glaeser (1997), which includes a rich discussion of the problems of omitted variable bias and endogenous migration, is significantly limited in its ability to address those problems because they lack a plausible instrument. In place of an instrument they impose a variety of exclusion restrictions. Their main empirical estimates depend on the identifying restriction that segregation has no effect on whites, so that within cities white outcomes provide a counterfactual for what black outcomes would be in the absence of segregation. My results, by contrast, suggest that segregation has significant effects on whites and that assuming otherwise produces incorrect estimates of the effects of segregation on blacks.[3]

In some specifications, Cutler and Glaeser use an approach more similar in spirit to that used here, instrumenting for a city's level of segregation with its number of rivers. In practice, however, my approach, using railroads, is quite different. Railroads, unlike rivers, predict division on a small scale—at the neighborhood rather than municipal level. This means, first, that unlike rivers (Hoxby 1994) railroads do not separately predict confounding metropolitan characteristics such as intergovernmental competition. Secondly, it means that railroads identify neighborhood-level segregation, which is the level that the literature has generally considered relevant and is the level that segregation indices measure. Most importantly, railroad division strongly and robustly predicts segregation. Therefore, I argue that the use of railroad division provides ex-ante a more compelling answer to the question of the effects of segregation on places than has

[3] For example, I find that Cutler and Glaeser's results overstate the negative effects of segregation on low-skilled blacks because their strategy misses the positive effects of segregation on low-skilled whites.

previously been proposed. It can then allow research to proceed to the next important question—the effects of segregation on people.

My results on the overall effects of instrumented segregation suggest that, consistent with observed correlation, greater segregation causes cities to have black populations with worse present-day characteristics, both at the top and the bottom of the education/income distribution. Within the white population, segregation results in worse characteristics at the top of the skill distribution and better characteristics at the bottom. This relationship is obscured in ordinary least squares estimation, possibly because other omitted local characteristics that lead to broader inequality also lead to more segregation.

My results when looking separately at production and migration suggest that segregation actually leads to better outcomes for low-skilled whites. For other groups, it is not possible to distinguish between direct effects of segregation on individual outcomes and effects on average group characteristics due to selective migration. For all groups, there is no evidence of preferences for segregated cities, and there is some evidence that Americans on average have tastes for integration.

II. Some facts about U.S. segregation

The history of urban American racial segregation can be divided into four periods. In the 19^{th} century, very few African-Americans lived outside of the South. This changed rapidly during the Great Migration (roughly 1915 to 1950), when large numbers of African-Americans migrated into Northern and Western cities from the South. Cities became highly segregated as their urban black populations grew (Cutler et al. 1999). Much of this segregation resulted not from market forces but from deliberate government policies and collective action by white residents (Massey and Denton 1993). Government policy towards segregation then changed gradually during the civil rights era, and a clear break in housing policy came in 1968 with the Fair Housing Act. Subsequent stated government policies on housing segregation have been neutral or have explicitly endorsed integration, but cities today continue overwhelmingly to exhibit high degrees of neighborhood segregation (Cutler et al. 1999). Segregation appears to persist despite the fact that significant proportions of Americans now state preferences for integrated neighborhoods (Ananat and Siegel 2002) and that, at a macro level, the U.S. population has in recent years been migrating to less-segregated cities (Glaeser and Vigdor 2001).

Two explanations can reconcile the survey and macro evidence that people prefer integration with the micro-level evidence (Emerson et al. 2001, Bayer et al. 2005, Goering et al. 2002) that within cities they continue to choose segregated neighborhoods. First, attempts to provide integration for people who have tastes for it may suffer from a collective action problem (Schelling 1971, Ananat and Siegel 2002). Second, integrated cities may be more efficient than segregated cities, at least for some people. For example, if there are neighborhood effects on individual outcomes and the black and white populations differ in their distribution of skill types, then blacks and whites will have different outcomes in more segregated cities. If these types are substitutes in the production of neighborhood effects, then separating blacks from whites in neighborhood production will be inefficient, causing people to sort away from less-productive segregated cities.

In theory, the productivity of segregation relative to integration is ambiguous in sign. For example, Tiebout sorting suggests that, if demand for neighborhood public goods varies less within race than within an entire city, then segregation is efficient. Segregation of blacks (relatively low-skilled) from whites (relatively high-skilled) would also be efficient if the high-skilled are complements in the production of public goods.

Productivity effects of and tastes for segregation may act in tandem. For example, after initial assignment to a city with a given level of segregation, rational agents sort away from that city based both on tastes *and* on any efficiency cost or benefit of segregation. If, for example, people have tastes for integration and integration is efficient, then, since willingness to pay is a function of income, high types will select into integrated cities, which will further reinforce the correlation of integration and positive outcomes.

III. Research Design

The ideal approach to identifying the effects of segregation on people and on places would require actual random assignment within an experimental framework. The experimental design would involve two otherwise identical cities with small open economies, one that would be kept perfectly residentially segregated by race (the treatment city), and another that would be kept perfectly integrated (the control city).

At time zero, each city would receive the same total number of blacks and the same total number of whites, with individuals assigned randomly to one city or the other. By virtue of random assignment, each city's within-race and overall skill distribution would start out equal to that of the other city. Restricting individuals from moving, one could then observe, over generations, the overall and within-race outcomes in each city.[4] Absent migration, differences in these outcomes would reflect *both* the effect of segregation on individuals and the effect on the population characteristics of places—since population would be fixed, the two effects would not be conceptually different. Within-race outcomes would identify whether segregation was beneficial or harmful for each group; which city had better aggregate outcomes would identify whether segregation is more or less efficient than integration for society as a whole.

The quasi-experiment generated by railroad division

The actual quasi-experiment that railroads provide does not perfectly follow the framework described above. Instead of a technology that results in being either perfectly segregated or perfectly integrated, railroad technology for segregation varies by degrees. To accept the estimates of segregation effects I derive, it is necessary to assume that the effects are monotonic. In addition, whites and blacks were not randomly assigned to cities; it is necessary to assume that individuals did not sort selectively based on railroad division. However, as I will show, there is no evidence of pre-period differences in cities based on railroad division that would cause individuals to sort selectively; they would have had to predict the effects of railroad division subsequent to black inflows, which seems unlikely.

[4] The standard model (Roback 1982) of the way wages and rents adjust for city consumption amenities that are productive (e.g. temperate climate) or unproductive (e.g. clean air) is inapplicable here. It requires that an amenity have the same productivity effects for all residents, a restriction that is inappropriate in the case of segregation. It is much more plausible to assume, to the contrary, that segregation has the effect of concentrating resources in the white community, making segregation more productive for whites than for blacks. This will be true both because the skill and resource distribution of whites dominates that of blacks and because whites typically outnumber blacks, with the political result of redistribution towards the white community. Moreover, the clearest way to model the effects of differential resources over time is with a multigenerational model rather than the one-period model in Roback (1982). Therefore I opt to model the production of type in the next generation, with type-specific wages fixed across cities, rather than city-determined productivity conditional on type.

To believe that railroad division can identify the effect of segregation on people, it is necessary to assume that individuals do not move. This assumption is plainly not credible; however, it is also not necessary to identify effect on places.

IV. Empirical Strategy

Segregation can be modeled as a classic endogenous regressor affecting outcomes at the city level,

$$(1)\ Seg = \alpha_1 Z + \alpha_2 X + \mu$$

$$(2)\ Y = \beta_1 Seg + \beta_2 X + \varepsilon,$$

and then estimated using two-stage least squares analysis. The right-hand side variable of interest in equation (2), Seg, represents a city's current level of segregation. Segregation is captured by a dissimilarity index, which measures the difference between the distribution of blacks by neighborhood and their total representation in the metropolitan area as a whole. Dissimilarity is defined as

$$(3)\quad \text{Index of dissimilarity} = \tfrac{1}{2}\sum_{i=1}^{N}\left|\frac{black_i}{black_{total}} - \frac{nonblack_i}{nonblack_{total}}\right|$$

where $i = 1 \ldots N$ is the array of census tracts in the area. It can be considered the answer to the question, "What percent of blacks (or non-blacks) would have to move to a different census tract in order for the proportion black in each neighborhood to equal the proportion black in the city as a whole?" Note that an index of zero is improbable in the absence of central planning.

Outcomes, represented by Y in equation (2), include the proportions of a city's blacks and whites who are poor, unemployed, high-school dropouts, college graduates, or who have household incomes above \$150,000. The first three outcomes should reflect primarily characteristics of a city's low-skilled population, who are more likely to be on the margin of poverty, unemployment, and dropping out of high school. The last two outcomes should reflect primarily characteristics of a city's high-skilled population.

The instrument, Z, is a measure of a city's railroad-induced potential for segregation. Z quantifies the extent to which the city's land is divided into smaller units by railroads. I define a "railroad division index," or RDI, which is a variation on a Herfindahl index that measures the dispersion of city land into subunits.

$$(4) \quad RDI = 1 - \sum_i \left(\frac{area_{neighborhood\,i}}{area_{total}} \right)^2$$

If a city were completely undivided by railroads, so that the area of its single neighborhood was 100% of the total city area, the RDI would equal 0. If a city were infinitely divided by railroads, so that each neighborhood had area near zero, the RDI would equal 1. The more subdivided a city, the more "sides" there are to its tracks, and the more possible boundaries between groups are available to use as barriers enforcing segregation. In particular, if railroads created many small neighborhoods that adjoin each other, it would have been possible during the Great Migration to relieve pent-up housing demand by allowing a ghetto to expand into an adjacent neighborhood, while still maintaining a new railroad barrier between the ghetto and the rest of the city. This should have facilitated persistent segregation even as the black population increased.

For variation in track configuration to be a valid instrument for segregation, it must result from random factors, such as minor variations in gradient, natural resource location, or direction to the next city—factors that amount to noise in aggregate. Any factors that drive both railroad configuration and city outcomes must be included as controls. X denotes a vector of city characteristics that affect railroad configuration and city outcomes. The most important of these is total track length, because there is a mechanical correlation between the total length of track and the division of the city by track. If total length of track is related to other features of the city, such as industrial composition or land quality, then length of track may predict city outcomes on its own, not because of the way track divides the city. Therefore all regressions in the paper control for total track length.

Other possible confounding factors include: manufacturing share—more manufacturing-oriented cities may differ in their routing of railroads; region—cities in the Northeast or Midwest may have different geography and different outcomes from those in the West; and black population inflows—African-Americans may have chosen cities based on different tastes for railroad breakup. I perform specification checks to test for explanatory power of these possible confounders. I run regressions including either 1920 manufacturing share or region dummies. I also instrument for black population inflows using data from Dresser (1994). She demonstrates that during World War II, some cities received larger war contracts per capita than others, leading to larger labor shortages in some cities than in others. She further shows that larger per-capita war contracts predicted higher inflows of African-Americans during World War II. I draw on Dresser's work by using per-capita war contracts as an instrument for greater black inflows during the Great Migration and including it in reduced form as a control in the analysis.[5]

[5] Railroad-induced segregation technology should have proved differentially useful in cities with high exogenous inflows of blacks. In cities with large exogenous changes in African-American population, which therefore had high demand for segregation, available segregation technology would have made more of a difference in the resultant degree of separation of blacks and whites. In cities with low inflows, where segregation did not become a salient demand, differences in the technology for producing segregation would have been less relevant to the equilibrium dispersion of blacks and whites and to city outcomes. This has the empirical implication that railroad subdivision should matter more in cities where a rapidly

The chronology of the Great Migration provides a further specification check. The validity of railroad division as an instrument relies on the assumption that division affected cities only by facilitating segregation of significant African-American populations. Therefore there would be cause for concern about validity if railroad division predicted city outcomes prior to the Great Migration. To test this assumption, I estimate equations (1) and (2) using pre-Great Migration city characteristics as dependent variables. These pre-period "outcomes" include manufacturing, labor force participation rate, average income, population, physical city size, percent black, and literacy rate. The ideal year to measure these characteristics would be 1910, the last Census year before the beginning of the Great Migration, when nearly 90% of African-Americans still lived in slave states. Measuring later, however, should bias specification tests toward failure, to the extent that cities may have already begun to differ due to segregation; my estimates, which use 1920 characteristics, therefore provide an especially strong test of the validity of the instrument.

I examine the characteristics of cities' black and white populations separately, since these groups would not be expected to respond identically to segregation. The two-stage least squares estimate allows me to measure the effect of railroad-induced segregation on city outcomes. The difference between the two-stage and ordinary least squares estimates can provide a sense of whether segregation that occurs endogenously is obscuring or intensifying the observed correspondence between segregation and group characteristics.

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increasing black population increased whites' utility of segregation. Unfortunately, the railroad division index and the proxy for WWII labor shortage do not provide enough power to estimate that interaction. I therefore confine my estimates to the main effect of each, by controlling for per-capita war contracts in the standard regression specification.

I test whether the population flows of each race are positive or negative, and also whether individuals of each race face relatively higher or lower rent and mortgage costs in more segregated cities. Differences in housing costs would not be a valid measure of city demand if they are driven by variation in the cost of living or by the amount of housing consumed in more versus less segregated cities. To test for this possibility, I examine costs as a percent of income and I examine household crowdedness.

Differences between the overall change in a city's skill distribution and that explained by migration represent the city's production of skill. For example, the percent change in a city's population of young high school dropouts, conditional on net migration of young high school dropouts, can provide an upper bound for the effect of city production on the size of the low-skill population. A similar argument holds for college graduates. By breaking down these differences by race, the race-specific production effects of segregation can be bounded. I therefore examine population change and net migration by race and education.

V. Data

<u>Sample</u>

My major data sources are U.S. Census Bureau reports on metropolitan demographics (various years), information on 19th century railroad configuration extracted from archival maps, measures of metropolitan segregation from Cutler and Glaeser (1997), and a replication of Dresser's data using city-level total war contracts and total population information from the 1947 County Data Book.

My ideal sample would include all places outside the South that were incorporated prior to the Great Migration, so that they were potential destinations for

African-Americans leaving the South. Then the growth of the place itself into an MSA could be treated as an outcome of its potential segregation. Because the Census only provides data for large places, however, it is not possible to get pre-period information for places that were small at the time of the Great Migration.

My sample of cities is chosen as follows. Cutler and Glaeser (1997) provide data for MSAs with at least 1000 black residents. Of these MSAs, I include only those in states that were not slave-owning at the time of the Civil War, because these were the states that had few African-Americans prior to the Great Migration.[6] Further, my sample was limited by the set of historical maps held by the Harvard Map Library. The library depends on donations and estate purchases, etc., to collect maps, and therefore there are gaps in its collection. I have compared the full non-South Cutler and Glaeser (1997) sample to the sample available from the Harvard Map Library. The cities for which the library could not provide maps tend to have been smaller cities in the 19th century but otherwise do not appear different in either historical or current characteristics. My final sample consists of 134 urban areas.

Maps

The maps that provide railroad placement information were created by the U.S. Geological Survey as part of an effort to document the country's topography, beginning in the 1880s.[7] These maps display elevation, bodies of water, roads, railroads, and (in

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[6] Specifically, I exclude Delaware, Maryland, Washington, DC, Virginia, West Virginia, North Carolina, South Carolina, Georgia, Florida, Alabama, Mississippi, Louisiana, Tennessee, Kentucky, Missouri, Texas, and Arkansas. Nearly 90% of African-Americans resided in one of these states in 1910 (author's calculation from 1910 IPUMS data).

[7] The median map year in my sample is 1909, prior to the start of the Great Migration. The observations in the maps should primarily reflect 19th century railroads, since 75% of the total track laid in the United States was in place by 1900 (Atack and Passell 1994, p. 430)

many cases) individual representations of non-residential buildings and private homes.[8] The edges of a 15-minute map are exogenously defined in round 15-minute units, so that, for example, a map will extend from -90°30'00" longitude and 43°45'00' latitude (in the southeast corner) to -90°45'00' longitude and 44°00'00' latitude (in the northwest corner)

Because the Harvard Map Library collection is incomplete, there are 77 cities in non-South states available in the Cutler and Glaeser data for which I do not have the necessary map observations. In addition, in 15 cities I observe only some fraction of the four-kilometer-radius land area I wish to observe, since the cities overlap two or more 15-minute areas and I have maps only for some subset of those areas. Finally, in 40 cases the city overlaps multiple areas and I observe all of the areas.

The process of extracting railroad information from the maps is illustrated in Figure 2. For each city, its map or maps were used first to identify its physical size, shape and location at the time its map was made. A Geographic Information Systems program, ArcGIS, was used to create a convex polygon that was the smallest such polygon that could contain the entire densely inhabited urban area. Dense habitation, defined as including any area with houses and frequent, regular cross-streets, was identified by visual examination. ArcGIS was then used to identify the centroid of this polygon, and this point was defined as the historical city center. A four-kilometer radius circle around this point became the level of observation for the measurement of railroads. This approach meant that differences in initial city area would not distort the measurement of initial railroads: cities that were, at the time, very small would still be coded with railroads that affected later development, after the population had expanded;

[8] In some cities with relatively dense areas, some center-city blocks are marked as continuously inhabited, rather than showing individual structures. The outskirts of every inhabited area, however, do show individual structures.

cities that were already large would have only those railroads in their center cities included. It should be noted, however, that about 75% of the cities were smaller than 16π square kilometers when mapped, and many were much smaller, so for most cities this measure includes railroads that were laid on unoccupied land without need to consider habitation.

Visual examination reveals that the historical city center created in this way is typically quite close to what would be identified as the current city center if using a current map. Within this four-kilometer circle, every railroad was identified, its length measured, and the area of the "neighborhoods" created by its intersections with each other railroad calculated. Historical railroads predict the borders of current neighborhoods as identified by the Census quite well. The actual land area within the circle was also calculated, so that measurement could be adjusted for available observed land when working with maps that truncate city observations or include substantial bodies of water.

Segregation Indices

I use the Cutler/Glaeser/Vigdor segregation data provided online by Vigdor (2001). These data come from various decennial Censuses, and include 19th and 20th century historical segregation indices and metropolitan characteristics from Cutler and Glaeser (1997), 1990 GIS-dependent measures of segregation based on Census data from Cutler et al. (1999), and additional data from the 2000 Census from Glaeser and Vigdor (2001). These data include dissimilarity indices for every decade from 1890 to 2000. In addition, they provide four other measures of segregation, all based on those developed in

Massey and Denton (1988). These include an index of isolation, available for every decade from 1890 to 2000, which provides a different way to organize the same information contained in the dissimilarity index and is highly correlated with the dissimilarity index. Supplementary measures of clustering, concentration, and centralization—all of which rely on geographical data about the proximity, size, and location of a city's census tracts—are available for 1990. Dissimilarity is the standard measure of segregation in the literature, and I use the dissimilarity index throughout the paper, while also testing the robustness of my instrument to alternative segregation measures.

Census Measures of Urban Characteristics

I collect city outcomes from published Census reports (U.S. Census Bureau 2005). Although at the time that tracks were laid each of these cities was physically separated by open space from other cities, over the last century urban growth has meant that many once-distinct metropolitan areas are now conglomerates. To surmount this problem, I collect data for the reporting area which best centers on the original city center without containing other original city centers.

Thus I use MSA-level data for the 64 cities that have remained independent MSAs. For MSAs in which multiple city centers are each in a separate county, I assign to each city the characteristics for the county that holds that city's original urban center. Doing so allows me to differentiate between the effect of an original center on its county level outcomes and the combined effect of several centers on MSA-level outcomes (e.g. outcomes for the New York-Northern New Jersey-Long Island Consolidated MSA).

Fifty-three cities are in unique counties but share an MSA with at least one other city. Finally, for the 17 cities that share a single county with another city, I assign the characteristics of the politically-defined city itself to the observation.

I use Census data collected at these MSA, county, or municipal levels to derive city outcomes by race. These outcomes include education, labor force characteristics, poverty, distribution of income, median rent and mortgage costs, housing costs as a percent of income, percent of households with more than one person per room, and proportion of residents who are new to the area.

VI. Results

First stage

Table 1 shows that, controlling for track per square kilometer in the historical city center, the neighborhood RDI generated by the configuration of track strongly predicts the metropolitan dissimilarity index in 1990. Adding a control for pre-period manufacturing composition does not significantly affect the relationship between railroad division and current segregation.[9] Nor does adding per-capita war contracts as a proxy for exogenous black inflows. The regression in column 4 includes Census region dummies; the coefficient on the RDI remains positive and significant. As seen in Table 2, the RDI similarly positively predicts other aspects of segregation, including isolation, clustering, concentration, and centralization. The effects of the RDI on three of these four facets of segregation are highly significant.

[9] Regressions that instead use later measures of manufacturing share (1970 and 1998) produce similar results. These regressions should, if anything, bias the effect of manufacturing on outcomes toward greater significance (Acemoglu, Johnson, and Robinson 2001).

Specification checks

Table 3 displays the results of regressions that use railroad division to predict city characteristics prior to the time when cities experienced significant African-American inflows. I use data from 1920, which is just after the start of the Migration, but using data from such a late date should merely bias specification tests toward failure. As shown in Table 3, the RDI predicts neither 1920 labor force participation, average income, population, physical city size, nor percent black. The RDI has a marginally significant relationship with literacy rate in an unexpected direction—more railroad subdivision corresponds to higher literacy.

Main results: The impact of segregation on city outcomes

Table 4 shows ordinary least squares and two-stage least squares estimates of the effects of racial segregation on a variety of current urban characteristics. Black outcomes and white outcomes are shown separately. Table 4 also includes overall city outcomes, but these are strongly driven by white outcomes, since white populations numerically dominate black populations.

The top panel of Table 4 demonstrates that RDI-induced segregation causes a city's low-skilled whites to have better characteristics. White unemployment and poverty are lower, and whites are less likely to be high-school dropouts; the former two effects are significant. In contrast, RDI-induced segregation causes a city's black population to have worse characteristics; in particular, they have much higher poverty rates. The effects of segregation on black unemployment and proportion of adults who are high-school dropouts, however, are not significant.

The bottom panel of Table 4 shows that RDI-induced segregation has negative effects on the characteristics at the upper end of the skill distribution. Segregation significantly lowers the fractions of a city's blacks and whites who are college graduates, as well as the fractions of white and black households with more than $150,000 in income. The negative effects on these characteristics for whites are as large as or larger than for blacks.

Table 5 shows migration and housing market characteristics by race from 2000 Census statistics reported at the urban level. Cities with more RDI-induced segregation have significantly fewer new residents, both black and white. The effect on black in-migration is larger than the effect for whites.

Unfortunately, because the Census does not supply data on out-migration, I cannot distinguish between low demand and low supply as explanations for this result. It may be that there are fewer new residents because out-migration is lower, leading to few vacancies. However, the evidence on housing values in Table 5 suggests that segregated cities are in fact in less demand. First, more segregated places have significantly lower rents, lower mortgage costs, and lower home values. These effects do not appear to be driven by lower cost of living in more segregated cities, since rents are as low or lower as a fraction of income (significantly lower for whites). Second, lower expenditures on housing also do not seem to reflect lower consumption of housing in more segregated cities; blacks and whites in more segregated cities are significantly less likely to live in crowded homes (that is, homes with more than one person per room).

VII. The effects of segregation on individual outcomes

To what extent are these differences in present-day city characteristics driven by sorting of individuals across cities in response to segregation? To what extent are they driven by the direct effect of segregation on the production of health, human capital, and productivity? To distinguish between the effects of segregation on individuals and equilibrium sorting of individuals by race and skill, it is helpful to briefly examine the theoretical relationships between tastes, skills, production, and residential choice.

<u>Theory of Skill Production and Endogenous Migration</u>

Assume two small open-economy cities that exist for two generations. City I has railroad technology such that it will have two perfectly racially integrated tracts, while city S has railroad technology such that it will have two perfectly racially segregated tracts (See Figure 4a for an illustration). In all other ways, these two cities are identical.

At time zero, corresponding to the Great Migration, each city is randomly assigned the same population of measure one, β of which is black and $1-\beta$ white. There are two types of residents, high and low, such that the high types receive wage H, relative to a low-type wage that is normalized to 1. Even assuming some wage discrimination, it is appropriate to infer from the data that at time of the Great Migration the proportion of blacks who are high types, p_{hb}, is lower than the proportion of whites who are high types, p_{hw}.

Production of types in the next generation depends on the type mix of current neighborhood residents. This implies that type is a neighborhood-level public good. It could represent education, health, connectedness to a job network, political influence—anything that varies at the individual level but might depend on the mix of characteristics of a neighborhood's elder generation. In particular, consider the following public-good production function:

$$p_{H2} = \lambda p_{H1}^{\alpha}$$

where $\alpha \geq 0$, λ is a scaling parameter, and p_{H1} and p_{H2} are a neighborhood's proportion high-type in the first and second generations, respectively.

The parameter α reflects the complementarity or substitutability of types in the production of next-generation type. If $\alpha < 1$, the production of high-type offspring is concave in the percent high type in the current generation, meaning that types are substitutes. If $\alpha > 1$, the production of high-type offspring is convex in the percent high type in the current generation, meaning that types are complements. (See Figure 4b for an illustration.) The complementarity or substitutability of types has important implications for the economic efficiency of segregation.

If residents cannot move between cities—i.e., moving costs are greater than high-skilled income—we need go no further. Tastes or distastes for integration will be irrelevant, the high-skilled will not be able to migrate differently from the low-skilled, and housing demand will not differ by city. Observed differences in proportion high-skilled in the second generation can be interpreted as resulting from the relative productivity of segregation. As long as $\alpha > 0$, whites in S will have higher p_{H2} than whites and blacks in I, who in turn will have higher p_{H2} than blacks in S. If $\alpha > 1$, so that types are complements, then the weighted average p_{H2} in S will be greater than the weighted average p_{H2} in I—implying that segregation is more efficient than integration. If $\alpha < 1$, so that types are substitutes, then the weighted average p_{H2} in S will be smaller than the weighted average p_{H2} in I—implying that integration is more efficient than segregation.

However, if moving cost C is low enough that migration between cities occurs, then the picture becomes more complicated. Race, income, tastes or distastes for integration, intergenerational altruism, and the elasticity of housing supply will affect individuals' choice of city. To explore these complications, allow a parameter a to represent taste (if positive) or distaste (if negative) for integration, and assume that individual utility is:

$$U = \ln(wage) + (a \mid residenceinI)$$

Consistent with survey evidence, a varies continuously, takes on both positive and negative values, and is distributed differently by race. A simple parameterization that captures these attributes is to define a as distributed uniformly on the interval $[\underline{a}_w, \bar{a}_w]$ for whites and $[\underline{a}_b, \bar{a}_b]$ for blacks.

In the absence of housing discrimination and with a flexible neighborhood delineation, we can assume that the housing market clears on the city level. Initially, with equal population of measure 1 in each city, the price of housing in city I relative to city S can be normalized to 0. To avoid an unrealistic corner solution in which everyone resides in one city or the other, it is desirable that the relative price of housing in city I approach infinity as city population approaches 2 and approach negative infinity as city population approaches 0. A simple parameterization that captures these three characteristics of the housing market is:

$$R = \eta\left(\frac{\frac{1}{2} - T}{T(T-1)}\right)$$

where R is the rent that must be paid (or subsidy received) each generation, η is a scaling parameter, and T is the total population of city I (see Figure 4c for an illustration).

To capture individuals' interests in the difference in public goods production by segregation and race, assume that each individual is altruistic towards an offspring in the second generation who shares taste parameter *a*. The individual's problem, then, is to maximize, by choosing I or S, the value of his own utility plus the expected value of his offspring's utility, which is discounted at rate δ.

In equilibrium, the proportion of each of the four groups of individuals (high- and low-type blacks and whites) choosing I should be in the interval (0,1), since we never in fact observe cities entirely missing one of these demographics. Within race and type, individuals will sort by preferences, so that the individual with preference a^* is indifferent between the two cities, while those with $a>a^*$ choose city I and those with $a<a^*$ choose city S.

Thus we have four indifference equations and four unknowns—the proportions (b_H, w_H, b_L, and w_L) of each group that choose the integrated city—so we can solve for the equilibrium populations, generation 2 proportion high-type, and rent differential:

$$b_H = \tfrac{1}{2}\ln(H - C - R[T]) + \tfrac{1}{2}\ln(H - R[T]) + \frac{1}{1+\partial}\left(\frac{\beta p_{hb} b_H + (1-\beta) p_{hw} w_H}{T}\right)^{\alpha} \ln(H - R[T])$$
$$+ \frac{1}{1+\partial}\left(1 - \frac{\beta p_{hb} b_H + (1-\beta) p_{hw} w_H}{T}\right)^{\alpha} \ln(1 - R[T]) - \tfrac{1}{2}\ln(H - C) - \tfrac{1}{2}\ln(H)$$
$$- \frac{1}{1+\partial}\left(\frac{(1-b_H) p_{hb}}{1 - b_H p_{hb} - b_L(1-p_{hb})}\right)^{\alpha} \ln(H) + \left(\frac{2+\partial}{1+\partial}\right)(1-b_H)$$

$$w_H = \tfrac{1}{2}\ln(H - C - R[T]) + \tfrac{1}{2}\ln(H - R[T]) + \frac{1}{1+\partial}\left(\frac{\beta p_{hb} b_H + (1-\beta) p_{hw} w_H}{T}\right)^{\alpha} \ln(H - R[T])$$

$$+ \frac{1}{1+\partial}\left(1 - \frac{\beta p_{hb} b_H + (1-\beta) p_{hw} w_H}{T}\right)^{\alpha} \ln(1 - R[T]) - \tfrac{1}{2}\ln(H - C) - \tfrac{1}{2}\ln(H)$$

$$- \frac{1}{1+\partial}\left(\frac{(1 - w_H) p_{hw}}{1 - w_H p_{hw} - w_L(1 - p_{hw})}\right)^{\alpha} \ln(H) + \left(\frac{2+\partial}{1+\partial}\right)(1 - w_H)$$

$$b_L = \tfrac{1}{2}\ln(1 - C - R[T]) + \tfrac{1}{2}\ln(1 - R[T]) + \frac{1}{1+\partial}\left(\frac{\beta p_{hb} b_H + (1-\beta) p_{hw} w_H}{T}\right)^{\alpha} \ln(H - R[T])$$

$$+ \frac{1}{1+\partial}\left(1 - \frac{\beta p_{hb} b_H + (1-\beta) p_{hw} w_H}{T}\right)^{\alpha} \ln(1 - R[T]) - \tfrac{1}{2}\ln(1 - C)$$

$$- \frac{1}{1+\partial}\left(\frac{(1 - b_H) p_{hb}}{1 - b_H p_{hb} - b_L(1 - p_{hb})}\right)^{\alpha} \ln(H) + \left(\frac{2+\partial}{1+\partial}\right)(1 - b_H)$$

$$w_L = \tfrac{1}{2}\ln(1 - C - R[T]) + \tfrac{1}{2}\ln(1 - R[T]) + \frac{1}{1+\partial}\left(\frac{\beta p_{hb} b_H + (1-\beta) p_{hw} w_H}{T}\right)^{\alpha} \ln(H - R[T])$$

$$+ \frac{1}{1+\partial}\left(1 - \frac{\beta p_{hb} b_H + (1-\beta) p_{hw} w_H}{T}\right)^{\alpha} \ln(1 - R[T]) - \tfrac{1}{2}\ln(1 - C)$$

$$- \frac{1}{1+\partial}\left(\frac{(1 - w_H) p_{hw}}{1 - w_H p_{hw} - w_L(1 - p_{hw})}\right)^{\alpha} \ln(H) + \left(\frac{2+\partial}{1+\partial}\right)(1 - w_H)$$

Because these equations are of the form $x\ln(x)$, they do not have closed-form solutions. Instead, they can be solved numerically for given parameter values.

Without making parametric assumptions necessary to solve the model, several implications are clear. If, in equilibrium, rents are lower in cities with more exogenous segregation, then either segregation is unproductive or people on average have strong tastes for integration. If, in addition, it is observed that in equilibrium a racial group has more positive outcomes in more segregated cities and faces lower rent in these cities, it is evident that segregation is productive for that group, but that the group has average tastes in favor of integration. On the other hand, if a group has worse outcomes in more segregated cities and pays lower rents, it is unclear whether those with lower unobservable type are sorting into more segregated cities because of the lower costs or whether the cities actually produce worse outcomes. Finally, the model has a specific prediction in terms of the relative production effects by race: since blacks start out with a lower type distribution, segregation must produce relatively worse outcomes for blacks than for whites. Any effect on white characteristics that is more negative than the corresponding effect on blacks must be due to sorting.

Extrapolating to an multigenerational context, sorting recurs every generation, since type is not perfectly inherited. That is, migration persists even in equilibrium, as offspring who find themselves with different type than their parents re-sort so that the housing market clears. The signed equilibrium conditions for outcomes, rents, and migration can be used to make inferences from the patterns that emerge in the empirical results.

<u>Empirical Estimates of Outcomes by Race and Type</u>

To apply the equilibrium conditions concerning prices and housing demand by race and skill to the data, segregation is again treated as an endogenous regressor affecting outcome *Y*, as in equations (1) and (2). Here, however, these outcomes are city average housing prices and net population flows for particular demographic groups. I test whether the population flows of young people of each race or race and skill group are positive or negative, and also whether the group faces relatively high or low rent and mortgage costs.

The difference between the overall change in a city's skill distribution, estimated earlier, and that explained by migration represents the city's production of skill. For example, the percent change in a city's population of young high school dropouts, conditional on net migration of young high school dropouts, can provide an upper bound for the effect of city production on the size of the low-skill population. A similar argument holds for college graduates. By breaking down these differences by race, the race-specific production effects of segregation can be measured, which in turn allows the testing of the equilibrium conditions derived above.

Aggregate Census data do not allow me to distinguish racial migration patterns by education level. They also do not report out-migration, which is required to identify net migration (since population change data combine in- and out-migration with births and deaths). To identify the out-migration and characteristics of young people by race-skill group, I use Census microdata (Ruggles et al. 2004) on 22- to 30-year-olds that I aggregate to the urban level. However, Census microdata only identify a subset of urban locations, and represent a 5% sample of the population. Both of these limitations reduce the precision of my estimates.

Table 6 shows migration data by race and education for young adults age 22 to 30. Consistent with the aggregate data in Table 5, individual-level data also show lower rents in more segregated cities. Unfortunately, the microdata cannot give precise enough estimates to distinguish total population change from that change induced by net migration. Therefore, these data cannot separately identify production effects and the effects of general equilibrium sorting by race and education. I hope to further explore these separate effects using restricted-use Census microdata in future research.

Combining these results with the aggregate results derived earlier, low-skilled whites are better off in more segregated cities: they are less likely to be poor or unemployed, and they pay lower rent for better housing. Nonetheless, they do not appear to be migrating towards segregated cities—the estimates of the effects of RDI-induced segregation on migration of white high-school graduates and dropouts are negative and insignificant. This suggests that more segregated cities produce better outcomes for low-skilled whites. The mechanism or mechanisms through which this production occurs remains an open question.

More segregated cities have a higher percentage of blacks who are poor. Their white and black populations have fewer college graduates and fewer households with very high income. The data cannot distinguish with certainty between sorting and production explanations for these groups. However, the fact that the quasi-experimental estimates for high-skilled white education and income are as large as or larger in magnitude than those for high-skilled blacks suggest, according to the model, that at least some of the white effect is due to migration.

VIII. Discussion

To what extent does racial segregation cause worse city level outcomes? This question has been difficult to answer because of the confounding effects of endogenous segregation and endogenous migration. This paper addresses the first of these two obstacles: it separates endogenous relationships between segregation and city characteristics (such as their correlations with more manufacturing and larger black population) from relationships induced by quasi-experimental variation, and demonstrates thatRDI-induced segregation causes cities to have low-skilled whites with better characteristics and other populations with worse characteristics. It also sheds light on the second concern: it identifies the effects of segregation on low-skilled whites as results of differential production rather than migration; it suggests that at least some of the effects on high-skilled white characteristics occur through migration.

OLS estimates overstate the negative effects of segregation on low-skilled whites and blacks and overstate the positive effects on high-skilled whites. This suggests that other city characteristics that result in greater inequality also imply more endogenous segregation. Such a correlation could arise, for example, if cities that have lower tastes for redistribution also have lower tastes for neighborhood mixing. It does, however, appear that on average Americans have tastes for more integrated cities—such cities are more crowded, demand higher rents, and attract more new residents.

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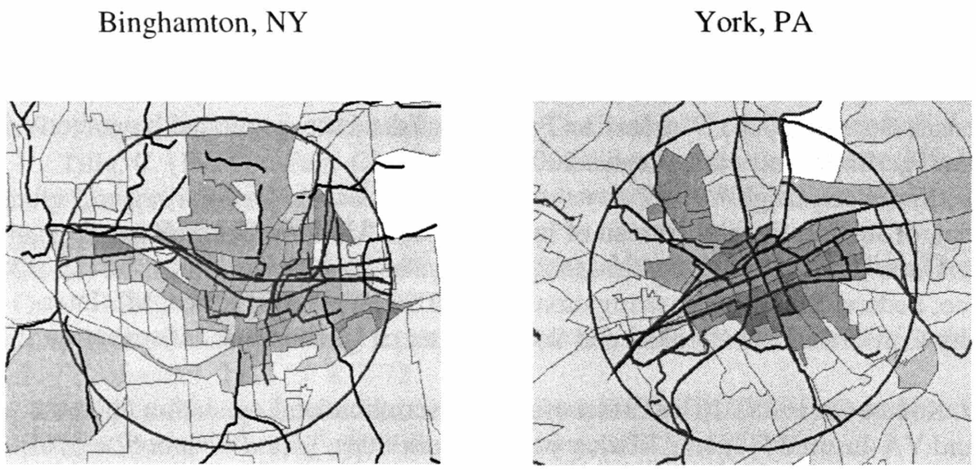
For figures in color, see http://econ-www.mit.edu/graduate/candidates/download_res.php?id=250

Figure 1.



19th century railroads, shown in red within the 4-kilometer radius historical city center, divide York, PA into a larger number of smaller neighborhoods than do the railroads in Binghamton, NY. Thus, even though the two cities had similar total lengths of track, similar World War II labor shortages, and similar manufacturing bases (in fact, Binghamton was somewhat more industrial than York), York became more segregated, as can be seen from the smaller, more concentrated area of African-Americans near the railroad-defined neighborhoods at the city's center. Rivers are shown in blue.

Figure 2. Measuring the railroads of Anaheim, CA

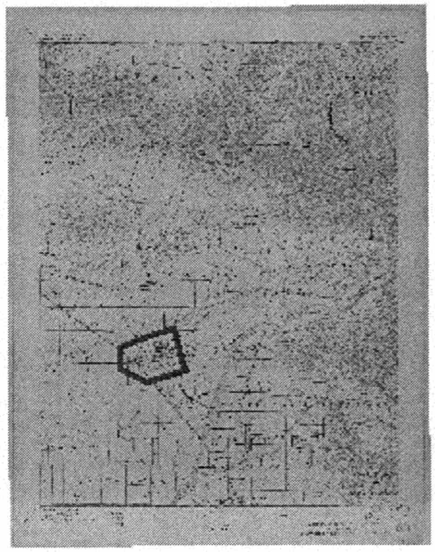


Figure 2a. 1894 15' map showing Anaheim, CA, which is marked in green.

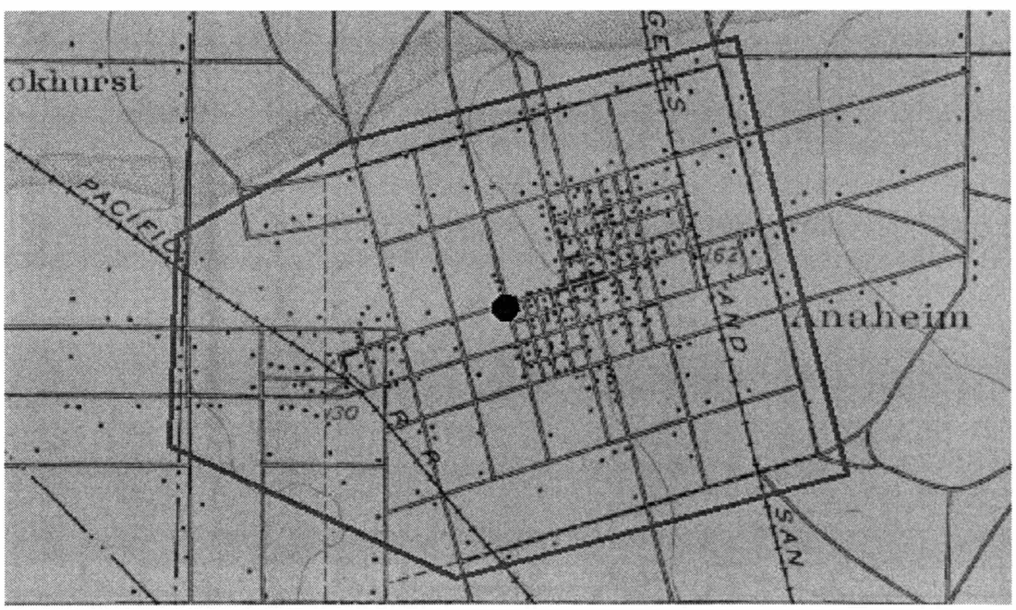


Figure 2b. The outline of the densely occupied area of Anaheim, defined as dense housing (each house is represented by a dot) and regular streets. The centroid of the occupied area is marked in blue.

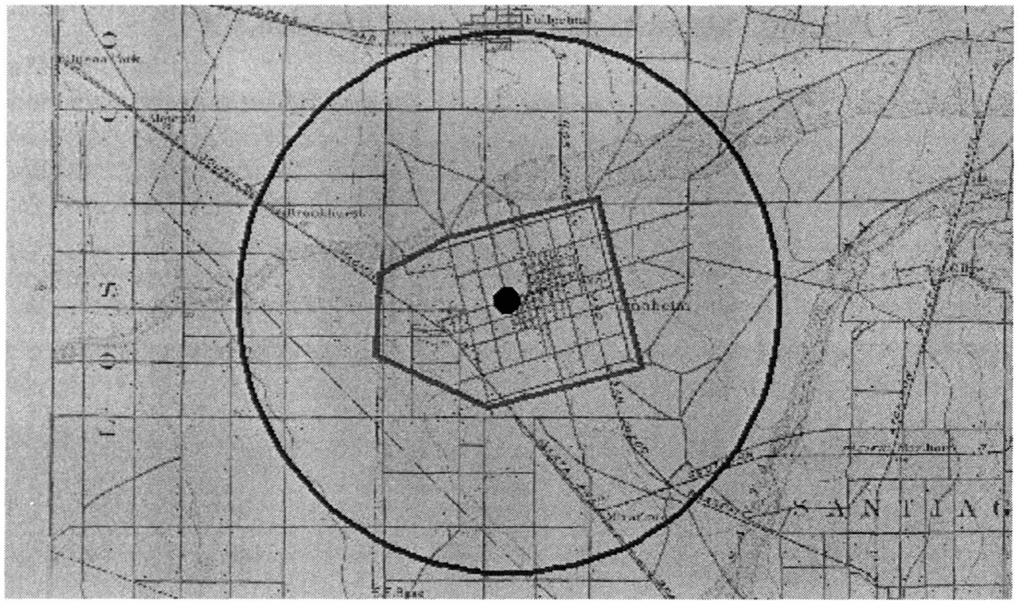


Figure 2c. The historical city center is defined as the 4 kilometer-radius circle around the centroid of the historical city, and is shown here in red.

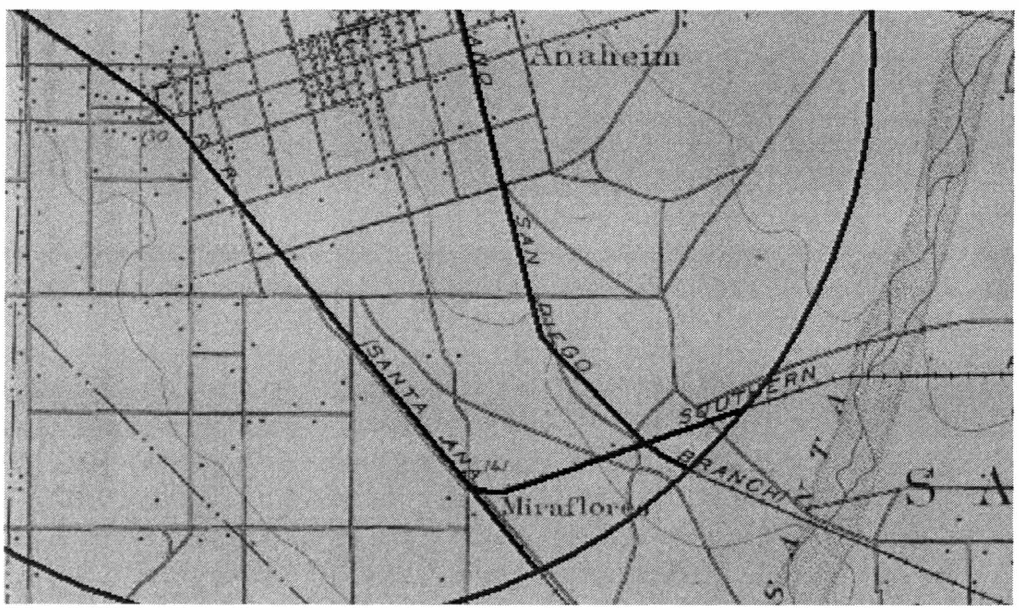


Figure 2d. Every railroad within the 4-kilometer circle is marked and measured—detail is shown here in violet.

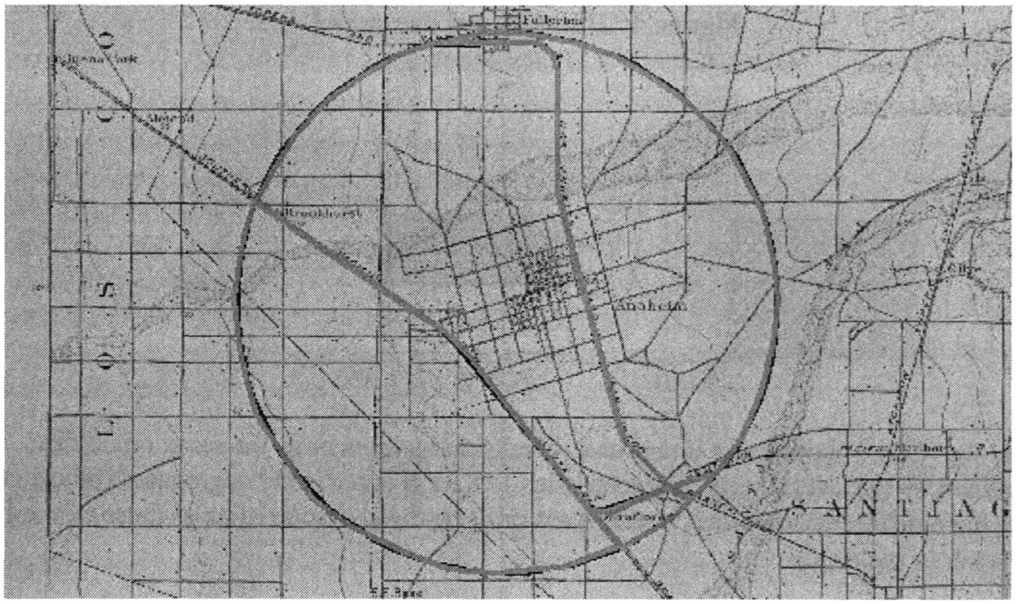


Figure 2e. Neighborhoods are defined as polygons created by the intersection of railroads with each other and with the perimeter. Anaheim contains five neighborhoods, shown here in orange. The area of each neighborhood is calculated and used to calculate a RDI measuring the subdivision of the historical city center.

Figure 2f. Year 2000 census tracts are shown in green. Note that current neighborhood borders, as defined by the US Census Bureau in 2000, closely follow historical railroad tracks.

Figure 4. Illustrations for model

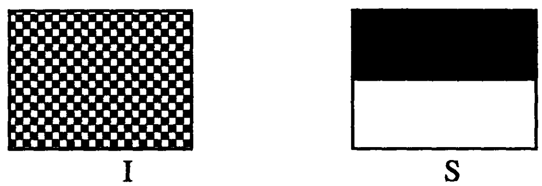
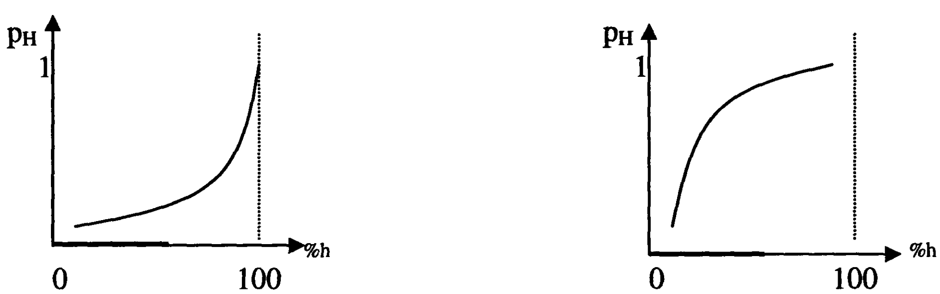


Figure 4a. City I is perfectly integrated (blacks and whites have the same outcomes, determined by the overall city characteristics). City S is perfectly segregated (blacks and whites have different outcomes, determined only by the characteristics of those of their race living in the city.)



n>1: segregation dominates **n<1: integration dominates**

Figure 4b. The convexity of the skill production function with regard to proportion high-skilled in the current generation will determine whether the separating (segregation) or pooling (integration) outcome is more efficient in producing second generation skill. Note, however, that the proportion of high-skill in the next generation is always increasing with the current proportion of skill, so that it is always desirable for an individual to be in a higher-skilled neighborhood. In the absence of perfect markets, then, low-skilled people may be unable to fully compensate high-skilled people to live with them even if pooling is more efficient.

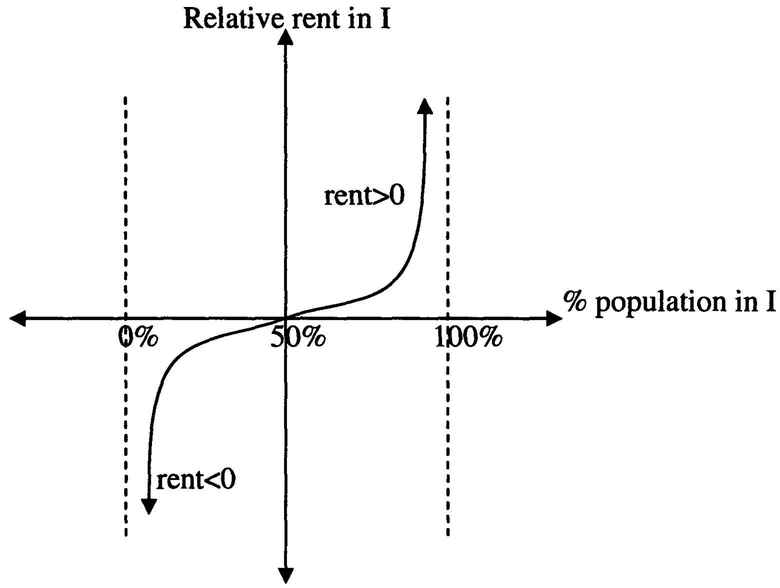


Figure 4c. A basic rent function. Note that the parameter η will determine for what range of populations rent will rise less than population (housing supply is elastic) and at what threshold level rent will begin rising faster (housing supply is inelastic). A smaller η implies elastic housing over a broader range of populations.

Table 1. First stage: Railroad division index as a predictor of current segregation (dissimilarity index)

	(1)	(2)	(3)	(4)
Railroad division index	0.3915**	0.3407**	0.3458**	0.2332**
	(0.081)	(0.083)	(0.073)	(0.076)
track length (km/km^2)	18.7881*	19.5858*	13.7753+	13.7068+
	(9.235)	(9.078)	(8.300)	(8.068)
Per-capita WWII war contracts		0.0101*		
		(0.004)		
% of employment in manufacturing 1920			0.2752**	
			(0.047)	
Region dummies				X
R-squared	0.21	0.28	0.37	0.42

Standard errors in parentheses. N=134.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 2. First stage with Alternative Segregation Measures: Railroad subdivision index as a predictor of current segregation

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Dissimilarity	Isolation	Clustering	Concentration	Centralization
Railroad division index	0.3407**	0.3596**	0.4299**	0.3875*	0.2335
	(0.0825)	(0.1046)	(0.1345)	(0.1529)	(0.1414)
Observations	134	134	121	121	121
R-squared	0.24	0.29	0.28	0.16	0.05

All regressions control for total track length per square kilometer and per-capita WWII war contracts.
Standard errors in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%

Table 3. Falsification Tests: 1920 Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Labor Force Participation Rate	Average Income Category	Literacy Rate	Population	Area (mi^2)	Percent Black
Railroad division index	0.0403	-0.0887	0.0429+	312,242	1,036	-0.0029
	(0.0279)	(0.1329)	(0.0241)	(305,958)	(27,741)	(0.032)
Observations	134	134	134	78	72	49
R-squared	0.03	0.01	0.05	0.03	0.00	0.49

All regressions control for total track length per square kilometer and per-capita WWII war contracts.
Standard errors in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%

Table 4. The Effect of Segregation on Current City Characteristics

	Overall		Blacks		Whites	
Dependent variable	OLS	2SLS	OLS	2SLS	OLS	2SLS
A. Lower-tail characteristics						
Poverty rate	-0.0165	-0.1711+	0.2291**	0.3573*	-0.0497*	-0.1721*
	(0.0287)	(0.0930)	(0.0511)	(0.1536)	(0.0217)	(0.0711)
Unemployment rate	0.0028	-0.0794+	0.1119**	0.0222	-0.0105	-0.0657+
	(0.0142)	(0.0467)	(0.0270)	(0.0824)	(0.0105)	(0.0338)
Fraction of adults who are high school dropouts	0.0480	-0.1751	0.3436**	0.0971	0.0602	-0.0701
	(0.0475)	(0.1509)	(0.0546)	(0.1724)	(0.0404)	(0.1232)
B. Upper-tail characteristics						
Fraction of adults who are college graduates	-0.1956**	-0.3303+	-0.3352**	-0.4035**	-0.1695*	-0.4614*
	(0.0557)	(0.1670)	(0.0440)	(0.1302)	(0.0619)	(0.1967)
Fraction of households with more than $150,000 in income	-0.0341*	-0.0960+	-0.0311**	-0.0496+	-0.0277	-0.1347*
	(0.0169)	(0.0521)	(0.0085)	(0.0255)	(0.0210)	(0.0675)

All regressions control for total track length per square kilometer and per-capita WWII war contracts.
Standard errors in parentheses. N=134. + significant at 10%; * significant at 5%; ** significant at 1%

Table 5. Effect of segregation on migration and housing by race, from aggregate Census data

	Blacks		Whites	
Outcome variable	OLS	2SLS	OLS	2SLS
Proportion of residents who are new to the city since 1995	-0.3783**	-0.5804**	-0.1705**	-0.2175**
	(0.0683)	(0.2070)	(0.0260)	(0.0774)
Median monthly rent	-412.6272**	-770.3659**	-348.2157**	-847.8479**
	(73.42)	(234.33)	(89.18)	(291.62)
Median home value	-198,549**	-404,682**	-137,381**	-463,138**
	(34,273)	(113,725)	(46,467)	(160,093)
Median home expenses w/mortgage	-1,007**	-2,243**	-490*	-1,972**
	(198)	(662)	(222)	(755)
Median percent of income that goes to rent	-3.3789	-3.1139	-7.8383**	-17.0297**
	(2.41)	(7.07)	(1.25)	(4.38)
Proportion of HHs w/more than 1 person per room	-0.0771**	-0.1577*	-0.0510**	-0.1166*
	(0.02)	(0.07)	(0.01)	(0.05)

All regressions control for total track length per square kilometer and per-capita WWII war contracts. Standard errors in parentheses. N=134. + significant at 10%; * significant at 5%; ** significant at 1%

Table 6: City Demand by Race and Education Among Young Workers (Aged 22-30), from 5% Census Microdata

	College Graduates				High-School Graduates				High School Dropouts			
	Black		White		Black		White		Black		White	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Average Rent	-220	-982*	-396**	-1,266**	-472**	-630*	-451**	-916**	-549**	-1,326**	-253*	-705*
	(151)	(453)	(134)	(472)	(89)	(262)	(109)	(347)	(175)	(475)	(97)	(313)
N	91		96		96		96		87		96	
% Pop Change '95-'00	0.9873	1.6145	0.0708	0.125	0.2112	-1.8971	0.1113	-0.4125	0.2603	-6.8406	0.1503	-0.7176
	(0.6121)	(1.4259)	(0.2914)	(0.8063)	(0.3661)	(1.1972)	(0.1368)	(0.4098)	(2.7668)	(7.6651)	(0.2466)	(0.7303)
N	78		89		88		89		81		89	
% Net Migration '95-'00	0.768	1.3315	-0.0385	0.3849	-0.1266	-0.5786	-0.2576*	-0.0418	-0.1296	-3.1076	0.0867	-0.1687
	(0.5595)	(1.3030)	(0.2363)	(0.6662)	(0.2631)	(0.7412)	(0.1025)	(0.2908)	(1.2898)	(3.5463)	(0.1763)	(0.4938)
N	78		89		88		89		81		89	

All regressions control for total track length per square kilometer and per-capita WWII war contracts. Standard errors in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%

The Effect of Marital Breakup on the Income and Poverty of Women with Children

Elizabeth O. Ananat
Guy Michaels

Abstract

Having a female firstborn child significantly increases the probability that a woman's first marriage breaks up. We exploit this exogenous variation to measure the effect of marital breakup on women's economic outcomes. We find evidence that divorce has little effect on a woman's average household income, but significantly increases the probability that her household will be in the lowest income quartile. While women partially offset the loss of spousal earnings with child support, welfare, combining households, and substantially increasing their labor supply, divorce significantly increases the odds of household poverty on net.

Introduction and Motivation

The poverty rate for single mothers fell substantially between 1974 and 2002. Over the same period, the poverty rate for married mothers remained virtually unchanged. Given these facts, one might assume that women with children are less likely to be poor than they were 30 years ago. In fact, however, the overall poverty rate for women with children rose slightly over this period (Figure 1).

A clue to resolving this puzzle may be found in the fact that divorce and single parenthood have been increasing dramatically over the past several decades throughout the developed world. In the United States, the proportion of mothers who are single rose from about 16 percent in 1974 to roughly 26 percent in 2002 (Figure 2). It appears that in the absence of this trend, overall poverty rates would have decreased, rather than increased.

In order to establish this argument, however, one would need to demonstrate a causal relationship between divorce and poverty. Current political discussions commonly assume that marriage has causal beneficial effects on women and children. In particular, recent welfare legislation encourages marriage as a method of increasing income and reducing the need or eligibility for welfare. President Bush's 2005 budget proposal earmarks $1.2 billion of its Temporary Assistance to Needy Families (TANF, or welfare) budget for a five-year initiative "supporting healthy marriages"[10]; five states allocate some of their general TANF budget to marriage promotion activities, and ten states provide cash marriage incentives in the structure of their TANF benefits[11].

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10 U.S. Department of Health and Human Services, "FY2005 Budget in Brief." http://www.hhs.gov/budget/05budget/acf.html

11 Gardiner, Karen, Michael Fishman, Plamen Nikolov, Asaph Glosser, and Stephanie Laud. "State Policies to Promote Marriage: Final Report." U.S. Department of Health and Human

Despite the assumptions made in popular debate, a causal relationship between economic well-being and marriage preservation has not been established. Indeed, there are reasons to suspect that the correlation between marital breakup and poverty overstates the causal effect of divorce. For example, negative household income shocks such as job loss or falling real wages may increase marital tension and thereby increase the likelihood of divorce, so that poverty causes divorce rather than divorce driving poverty. In addition, women with worse earnings opportunities may be less attractive marriage partners, which would mean that women who get divorced have lower income, but not simply because they got divorced. Of course, the possibility also exists that the causal relation is understated.

This paper uses an instrumental variables (IV) approach to separate the causal effects of divorce from its well-known correlations. Using the 1980 U.S. Census, we document that having a female first-born child slightly, but robustly, increases the probability that a woman's first marriage breaks up. We find that the likelihood that a woman's first marriage is broken is 0.63 percentage points higher if her first child is a girl, representing a 3.7 percent increase from a base likelihood of 17.2 percentage points. This result, which we will argue implies that sons have a stabilizing effect on the family unit, is consistent with the finding (established in the psychology and sociology literature) that fathers bond more with sons.

When we use child sex as an instrumental variable to measure the effect of divorce on economic outcomes, we find that the marital breakup driven by having a girl does not significantly affect mean household income. Marital breakup does, however, significantly affect the household income distribution; in particular, it dramatically increases the probability that a mother will end up with very low income. While child support, welfare, and an increase in her

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Services Assistant Secretary for Planning and Evaluation, 2002.
http://aspe.hhs.gov/hsp/marriage02f/report.htm

own earnings (due to a substantially increased labor supply) go some way towards alleviating the loss of her husband's earnings, they often do not prevent poverty status. We also find some evidence that women who divorce may be slightly more likely to end up at the top of the income distribution, possibly due to re-marriage.

Several concerns might reasonably be raised about causal interpretation of the relationships between child sex, marital breakup, and economic outcomes. First, one might be concerned that the observed relationship between child sex and divorce is in fact an artifact of differential custody rates; women are more likely to lose custody of their boys in divorce, leading to divorced women being observed with fewer boys. However, we find highly comparable estimates in Current Population Survey (CPS) supplements that provide detailed fertility histories, which lends evidence that our results are not due to bias in observed household composition. Second, biologists (Trivers and Willard 1973) argue that child sex is endogenous to resources—that is, that natural selection has favored the ability of parents to have more girls in bad economic times. We find, however, that in our sample there is no difference between the economic resources of mothers of boys and mothers of girls when their first child is born; rather, the difference in income builds up over time, as does the difference in divorce rates. Third, one might worry that sex of the first-born child affects economic outcomes through channels other than divorce. For example, parents may work harder when they have a son. But we find a negligible effect of child sex on economic outcomes in a sample of women with a low exogenous probability of divorce, which provides evidence that child sex is not affecting economic outcomes through a channel other than marital breakup.

After discussing our findings, we consider the potential macro implications of the relationship we identify between divorce and poverty. In recent decades, mothers' poverty rates have failed to decline as much as the overall rate. Can the increase in the proportion of divorced women in the U.S. explain this stagnation? We calculate that, in fact, the poverty rate of women with children today is substantially higher than it would be if divorce had not risen over the post-1980 period.

The paper proceeds as follows. In section 2, we set our findings in the context of previous research. In section 3, we describe the data and the sample we use. In section 4, we describe the estimation strategy for analyzing mean outcomes and describe the results. In section 5, we develop a framework for estimating distributional outcomes and discuss the results. In section 6, we conclude.

Divorce and Children's Gender

Previous research has established the need for instrumental variables when examining the effect of marital status on women's outcomes, both in theory (e.g. Becker, Landes, and Michael 1977; Becker 1985) and empirically (e.g. Angrist and Evans 1998). In particular, Gruber (2000) emphasizes the necessity of alternatives to cross-sectional analysis when measuring the effect of divorce on child outcomes.

We instrument for the breakup of a woman's first marriage using the sex of the first-born child. Having a first-born girl could be positively correlated with breakup of the first marriage for one or more reasons involving both the husband and the wife. First, a man with a daughter may be less interested in sustaining marriage than a man with a son. He may intrinsically value a son more, perceiving him as an "heir." Or he may have more in common with a boy than with a girl, and therefore bond better over time. Second, a woman with a daughter may be less interested in sustaining marriage than a woman with a son. For example, she may believe a son needs his father as a role model, and therefore work harder to retain her husband. Or she may get better companionship from a daughter, and therefore place less weight on her relationship with her husband.

The previous literature supports the idea that fathers tend to bond more with their sons. Aldous, Mulligan and Bjarnason (1998), using data from the U.S. National Surveys of Families and Households, find that fathers spend more time with boys than with girls, and that the gender gap in paternal attention increases as the child ages. This supports the idea of increased bonding over time. Harris, Furstenberg and Marmer (1998) construct a "parental involvement index" and find that fathers' involvement with their adolescent children is higher with sons than with daughters, while mothers' involvement does not differ by child sex. This supports the claim that fathers bond more with sons, but does not lend evidence that mothers get better companionship from daughters. Thus the literature is more consistent with the theory that fathers bond more with sons, and that this has a stabilizing effect on the family unit.

Previous work (e.g. Angrist and Evans 1998) considers child sex as an exogenous variable when analyzing U.S. data and after conditioning on race. This seems particularly plausible when considering only births that took place prior to the 1980's, when ultrasound was limited to problematic pregnancies (Campbell 2000). Nevertheless, concern has been raised by some that child sex is endogenous to resource levels. Trivers and Willard (1973) argue that a male is likely to out-reproduce a female if both are in good condition, and vice versa if both are in bad condition. They argue that natural selection may therefore have favored parental ability to adjust sex ratios so that more girls are born in bad times, and they argue that data from mammals support their hypothesis. Our findings, however, are not consistent with the Trivers and Willard hypothesis. We find no relationship between economic circumstances at birth and child sex. Our findings are consistent with the hypothesis that the sex of the eldest child is exogenous.

Dahl and Moretti (2004), in simultaneous research, examine the relationship between child sex and divorce—that is, our first stage. They too find a significant relationship between having female children and divorce, although our estimates of the effect size are smaller because we address serious selection problems that otherwise overstate the relationship.

In related research, Bedard and Deschenes (2003) use the same instrumental variable for divorce: gender of the first child. When we use specifications similar to theirs, our results are generally consistent with their findings: they, like we, find that divorce has no significant negative causal effect on women's mean economic outcomes and that the cross-sectional relationship between divorce and household income at the mean is due to selection.

However, when we look beyond the mean to consider the way in which marital breakup affects the entire income distribution of women with children, we arrive at a different conclusion. Namely, we find that marital breakup has significant negative consequences for many women; in particular, it increases the probability that a woman lives in poverty.

We also find some evidence that women who remarry may actually become "better off" in terms of income after the end of their first marriages. This is consistent with an earlier literature that does not attempt to establish causality or deal with selection into remarriage: Mueller and Pope (1980) and Jacobs and Furstenberg (1986) find that women who remarry do better in terms of their husbands' education level and SES score. Duncan and Hoffman (1985) find that five years after their divorce, women who remarried gained in terms of family income. [12]

[12] Both Jacobs and Furstenberg and Duncan and Hoffman argue that this is largely due to life cycle effects, so that *if* the man these women divorced is similar to the average person in his age group, he would have made similar gains from the time of his divorce until the time we observe the woman's second husband. We suspect, however, that in our case the men who divorce are

Previous literature has not emphasized the relationship between divorce and inequality, although both have increased substantially over the past three decades. Much of the recent literature on the causes of inequality has focused on wage inequality and the forces that may be affecting it, such as: technology (Acemoglu 2002); the decline of labor market institutions (DiNardo, Fortin, and Lemieux 1996); and the rise of international trade. Our findings suggest that the decline of the traditional family unit as an institution may have contributed to the rise in income inequality.

Data

We use data on women living with minor children from the 5 percent 1980 Census file, which allow us sufficient power to identify the effect of sex of the first-born child on marital breakup.[13] We limit our sample to white women who are living with all of their children, whose eldest child is under 17, who had their first birth after marriage, after age 18 and before age 45, and had a single first birth. These limitations are necessary in order to create a sample for which measurement error in the sex of the observed first-born child has a classical structure. In particular it is important to consider only women who live with all their children, since by doing so we avoid any spurious correlation between child sex and household structure due to differential maternal custody rates of boys and girls. For further discussion of the sample construction, see the Appendix.

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negatively selected, so it is plausible that women who re-married did somewhat improve their economic situation.

[13] While earlier censuses have these measures, in previous decades the divorce rate was very low. Subsequent censuses, on the other hand, don't have all of the necessary measures. We did, however, try similar specifications using 1990 data and found similar patterns to those reported here; this finding lends evidence that cohort-specific effects are not driving the relationship between firstborn sex and divorce.

Table 1 shows summary statistics for our full sample and subsamples relative to the overall population of women with minor children. Our sample is quite similar to the overall population except in terms of age and marital status. Our sample is younger than average, consistent with the requirement that a woman's eldest child is under 17. The women in our sample are slightly less likely to be divorced, both because they are younger and because we require that they have custody of all children. And of course, unlike the overall population, women in our sample cannot be never-married. On other characteristics, however, the two groups differ little: women in our sample have slightly more education and household income than the overall population and work and earn slightly less.

In addition to estimating our model on the full sample, we look specifically at two subsamples that we use in specification checks: those who are at high risk of having ever divorced and those at low risk of having ever divorced. We create an index of exogenous risk for divorce, which is orthogonal to the sex of the eldest child (see Appendix). The high-risk subsample includes women whose predicted risk is in the top quartile; the low-risk subsample includes women whose predicted risk is in the bottom quartile.

The high-predicted divorce subsample is much older on average than the low-predicted divorce subsample, consistent with more years of exposure to risk of divorce. They are also less educated, but they have higher own and household income—probably due to lifecycle effects.

The two subsamples allow us to test the validity of our IV strategy. As Table 1 shows, women in the high-predicted divorce subsample are much more likely to have undergone divorce by the time we observe them, so if the responsiveness to the child-sex instrument is proportional to the overall level of divorce then we would expect the first stage to be bigger for this group. Moreover, the high predicted-divorce subsample has lower years of schooling and thus may be, on average, of lower SES; below, we conjecture that our instrument may play a larger role in marital breakup decisions for this group. Thus we expect the first stage to be stronger for the high-divorce subsample than for the low-divorce subsample. As a consequence, the two-stage effect of child sex on outcomes should also be more precise for the high-divorce subsample, if indeed our IV strategy is valid and child sex is affecting outcomes through divorce rather than through other channels.

Estimation of the Effect of Marital Breakup on Average Economic Outcomes

Estimation Framework

We begin our investigation of the causal effects of divorce by considering a simple econometric model. In this model, income is affected by the breakup of the first marriage, which is treated as a classic endogenous regressor:

(1) $$D = \alpha_1 Z + X\alpha_2 + U\alpha_3 + u$$

(2) $$Y = \beta_1 D + X\beta_2 + U\beta_3 + \varepsilon$$

The right-hand-side variable of interest in equation (2), D, is a dummy for the breakup of the first marriage. We define a woman as having her first marriage intact if she reported both that she was "currently married with spouse present" and that she had been married exactly once. We define as having her first marriage broken any woman who: has been married multiple times, is married but currently not living with her husband, is currently separated from her husband, is currently divorced, or is currently widowed.[14]

Our outcomes, Y, are total others' income, defined as total household income less total own income; household income; a measure of household poverty; and in some specifications hours worked last year. The change in others' income measures the direct effect on a woman of losing her husband as a source of income (to the extent that the husband is not replaced by other wage earners). The change in total household income captures this direct effect but also includes the indirect effects of divorce on income: transfers from the ex-husband in the form of alimony and child support[15]; transfers from the state in the form of cash assistance; and income generated by the woman's own labor supply response.[16]

Our controls, denoted by X, are a vector of pre-determined demographic variables including age, age squared, age at first birth and a dummy for high-school dropouts.[17] While our Ordinary Least Squares (OLS) estimates of the relationship between marital breakup and income may be sensitive to

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[14] We have run the analysis with ever-divorced, rather than first marriage broken, as the explanatory variable: in this case, widows and those separated from or not living with their first husband are coded as 0 rather than 1. Our results are not sensitive to this difference in categorization.

It is not possible to systematically remove widows from the sample, since the data do not allow us to identify those whose first marriage ended in death among those who have had multiple marriages. In any event, since widowhood is endogenous to both socioeconomic status and marital duration, it is probably not desirable to exclude widows.

[15] The 1980 Census question reads: "Unemployment compensation, veterans' payments, pensions, alimony or child support, or any other sources of income received regularly... Exclude lump-sum payments such as money from an inheritance or the sale of a home."

[16] We have also looked at various intermediate outcomes, such as the level of alimony and child support, the level of welfare, the woman's own earnings, and the woman's hours. All of the analyses gave results highly consistent with the results presented here, and are available from the authors upon request.

[17] Since we look only at women who gave birth to their first child after age 19, we can reasonably assume that the decision on whether to graduate from high school is made prior to the realization of the sex of the first-born child.

their inclusion, our IV specifications are robust to controls (results without controls are available upon request). We think of U as representing unobserved factors such as human capital, views on gender roles, and taste for non-market work relative to market work and leisure. Finally, our instrumental variable, Z, is an indicator for having a girl as one's firstborn child.

As discussed above, there are two major problems with estimating equation (2) with OLS. The first, correlation of U and D, can be seen as omitted variables bias or a selection problem, and it induces a bias of indeterminate direction. Women with worse earnings opportunities may be less attractive marriage partners: this would imply negative selection into marital breakup and upward bias in the estimated cost of marital breakup for women. On the other hand, women with worse earnings opportunities may be more interested in sustaining marriage. Or a stronger preference for household work relative to market work may cause a woman to work harder at staying married. In this case, the women who stay married would have supplied fewer hours of labor had their marriage broken up, and their earnings would have been lower.

The second major problem with OLS estimates of equation (2) is reverse causality. Conditional on a woman's observed and unobserved characteristics, a negative income shock to the household may increase marital tension and thereby raise the likelihood of divorce.

To address concerns both about omitted variable bias and about reverse causality, we estimate equation (2) using two-stage least squares, using the sex of the eldest child (Z) as an instrument for whether the first marriage is broken (D). Angrist and Imbens (1994) show that in the absence of covariates and given the standard two-stage least squares assumptions,[18] the IV approach identifies the local average treatment effect:

$$(3) \qquad \beta_{1,IV} = \frac{Cov(D,Y)}{Var(D)} = \frac{E[Y \mid Z=1] - E[Y \mid Z=0]}{E[D \mid Z=1] - E[D \mid Z=0]} = E[Y_1 - Y_0 \mid D_1 > D_0]$$

Here Y_1 and Y_0 denote the income (or other dependent variable) for women whose first marriage is broken and intact, respectively. D_1 is an indicator for whether a woman would divorce if her first child were a girl; D_0 is an indicator for whether she would divorce if her first child were a boy. D_1 and D_0 are, of course, just hypothetical constructs; in practice we can only observe the indicator for the child sex that is realized.

β_{1IV} therefore measures the change in income due to divorce for women whose first marriage breaks up if they have a girl and remains intact if they have a boy. That is, two-stage least squares estimates the average effect of divorce only for that part of the population for whom the treatment is equal to the instrument ($D_1 > D_0$), the group known as "compliers." As one might imagine, this group is typically not a random subsample of the population, and in fact we will show below that those who comply with sex of the first-born child tend to be of low socio-economic status (SES).[19]

[18] The assumptions are: conditional independence of Z, exclusion of the instrument, existence of a first stage and monotonicity – see Abadie (2002). The result can be generalized for the case with covariates.

[19] Note that β_{1IV} can only be interpreted as the causal effect of divorce for the average complier to the extent that the sample is not contaminated with "defiers". Defiers are women for whom $D_0 > D_1$, that is, women whose first marriage would dissolve only if their firstborn is a boy. We have run first-stage regressions for a variety of sub-populations and found no significant sign reversals in the coefficient of child sex on divorce, suggesting that defiers, if any, are a very small group.

First Stage Results: Child Sex and Marital Breakup

The first stage results for the entire population as well as for the full sample and various subsamples are presented in Table 2. When equation (1) is estimated for all women living with minor children, the coefficient on child sex is 1.13 percent. We believe that this figure overstates the actual effect of child sex on marital breakup, because endogeneity in the sex of the eldest child residing with a woman will create a spurious correlation between living with an eldest girl and being divorced.[20] That is, since women are more likely to end up with custody of girls than boys in the event of divorce, living with a girl is cross-sectionally correlated with being divorced above and beyond the causal effect of having a girl on marital breakup.

The second column gives the estimated relationship for our sample, which is constructed in order to close the custody channel, so that the relationship between child sex and marital breakup is causal. We find that the average effect of the first child being a girl on the breakup of the first marriage is about 0.63 percent, which is substantially smaller than the full-population estimate, but still highly significant. Furthermore, as discussed below, we replicate our result using data from the Current Population Survey, which identifies the sex of a woman's actual firstborn child (and hence requires no sample restrictions). The CPS results are highly similar to those from our Census sample.

The effect increases with the age of the eldest child, suggesting, sensibly, that the sex of the first child affects the hazard rate of divorce and separation.[21] This can also be seen in Figures 3 and 4, each presenting the results from 17 separate regressions by the age (0 to 16) of the eldest child. We also find

[20] In fact, a regression similar to those in Table 2 for a sample of women with all of our restrictions except that she must reside with all of her children gives an estimate of .010. This result is much closer to the estimate in column 1 than in column 2, suggesting that the factor driving the difference between the estimates in the full population and in our sample is the endogeneity of child residence.

[21] The base hazard rate of divorce may also change with the length of the relationship, which is correlated with the age of the eldest child.

that the effect size varies by the woman's education—high-school dropouts exhibit the strongest effect, and the effect size diminishes as education increases. This suggests that our instrument primarily provides us insight on the effects of marital breakup particularly for low-SES women.

Finally, we find a larger and more significant effect on those at higher exogenous risk of divorce: the quarter of our sample with the highest predicted risk of divorce has a first stage of 1.13 percent (highly significant), and the group that is least likely to divorce has a first stage of 0.36 percent (marginally significant). The differential effect by underlying probability of divorce lends evidence that the instrument is acting in the way we would anticipate.

One further implication of the figures should also be noted: a comparison between Figures 3 and 4 reveals that the effects of sex of the eldest child on the breakup of the first marriage are bigger than those on being currently divorced. Much of this difference comes from the fact that some of the women who divorced due to the instrument have since remarried. In addition, some of those who have not re-married have instead moved in with other adults (such as their parents). We find that having an eldest girl increases the probability that a woman is the head of household by only about half as much as the increase in probability that her first marriage is broken (result not shown).[22] It is therefore important to keep in mind that we are not estimating the effect of being currently divorced or of residing in a mother-only household on economic outcomes. Rather, we are estimating the effect of having *ever* been divorced on current outcomes.

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[22] Note that this will not capture all avenues of combining households, since a woman may share a household and be identified as its head.

Specification Checks

While the lack of over-identification implies that we cannot formally test the validity of our instrument, we do bring to bear some evidence that the instrument is not correlated with other omitted variables. First, we test the Trivers and Willard hypothesis that adverse conditions cause women to give birth to a higher proportion of girls. We have examined the income of mothers who were never married, and found that the income distribution in this group was very similar whether the eldest was a girl or a boy. We have also examined the income of women who were married when their first child was born and whose first marriage was still intact, and similarly found that the income distribution in this group was virtually the same whether the eldest was a girl or a boy. (Results not shown.)

This is still not a conclusive refutation of the endogenous sex hypothesis for our sample: the first group, those who were never married, are not included in our sample (we limit to those whose first birth occurred after their first marriage); the second group includes women who remain married after having a girl (i.e. those who do not respond to our instrument, and thus for whom we cannot measure the causal effect of divorce). But we have also found that the reduced-form effect of the instrument on household income and on others' income is small and statistically insignificant shortly after the child is born. Rather, the effect of the instrument increases with time.[23] This suggests that child sex is not the result of resources, but rather that later resources are the result of child sex, through its effect on marital breakup—note that the effect of sex on breakup similarly builds over time (see Figures 3 and 4).

If child sex is not the result of resources, we can interpret as causal the reduced-form relationship between having an eldest girl and economic outcomes. But child sex may affect outcomes through paths other than divorce, so that our IV estimate of the effect of divorce on

[23] We also regressed the sex of the eldest child on household income and controls for mothers whose children were aged 0-1 and found no significant effect of household income on child sex.

outcomes is misstated. There are various possible reasons why either or both parents might work more or less in response to the sex of the eldest child.[24] We again cannot refute these possibilities directly. However, when we looked at the sample of women whose marriages remain intact, the income distribution and labor supply of mothers did not substantially differ by the sex of the eldest child. Similarly, if child sex affects economic outcomes through channels other than marital breakup, we would expect differences in outcomes by firstborn sex for the group that is predicted to have a low divorce rate. But we do not find any significant effects for this subsample.

We might also overestimate the cost of divorce if fathers pay differential child support by the sex of the eldest child. It is possible that fathers of boys transfer more money to the mother of their child, even if they are no longer married to her. In this case, again, having a girl would be negatively correlated with women's outcomes, but not solely through marital breakup. To check this possibility, we examined the Census "other income" variable (which includes child support and alimony) for women who are currently divorced and for women who are ever divorced. We found that other income did not differ by the sex of the eldest child (results not shown).[25]

[24] For example, parents may work harder in the labor market when they have a boy. Lundberg and Rose (2002) find that men's labor supply and wage rates increase more in response to the births of sons than to the births of daughters. In addition, having a girl may, even in the absence of divorce, cause increased marital conflict, which could in turn negatively impact economic outcomes. In either case, having a girl would be negatively correlated with women's outcomes, but not solely through marital breakup, and using it as an instrument would cause an overstatement of the cost of divorce. Alternatively, women may increase their labor supply as a "defensive investment" in anticipation that their marriage may be less stable (Weiss 1997) if their first child is a girl.

[25] Dahl and Moretti (2003) find that divorced mothers of multiple children who are all girls are marginally significantly less likely to receive child support than those whose children are all boys. They, however, use a much less restricted sample, and use the gender of all children.

A final caveat remains for using the sex of the eldest child as an instrument for marital breakup: having a son as one's eldest child may affect the likelihood of re-marriage after the first marriage was broken. Note, again, that this would not invalidate the causal interpretation of the reduced-form estimates, but could cause us to overstate the direct effects of divorce. Again, we cannot rule out the existence of such a causal effect. However, when we regress a dummy for being currently married on having an eldest girl for ever-divorced women in our sample, the coefficient is indeed negative, but is not statistically significant (t-statistic=0.67).

As an additional check on the validity of the relationship between child sex and marital breakup, we replicate our Census estimates using CPS data. We do so to address potential concern about the reliability of our Census sample—that despite our best effort to restrict our sample to women for whom error in observed eldest child sex is uncorrelated with marital status, some error remains and is contaminating our estimate. We address this concern by using CPS fertility supplements. Because they record a woman's fertility history regardless of whether her children are still in the household, we do not have to worry about bias in the sex of the children still in the household.

Four June CPS supplements (1980, 1985, 1990, and 1995) record the sex of a woman's first-born child. In this sample, we need only restrict to women who are white, had their first birth after their first marriage, and had a single first birth—we need not make any of the Census restrictions that were geared towards assuring that we accurately observe the sex of the first child.[26] When we pool these supplements (N=107,519), we again find that having an eldest girl

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Even within their sample and methodology, when they examine all family sizes they do not find a significant effect.

[26] Compared to the census data, the CPS has drawbacks as well as advantages. On the one hand, the CPS allows us to look at older women, whose eldest children left their house after their full

significantly increases the probability of marital breakup. In fact, we find that having an eldest girl causes a 0.68 percent increase in the probability that the first marriage breaks up (t-statistic=2.31). We consider the similarity of the point estimates from both samples (the Census estimate is 0.63 percent) an encouraging sign that our results reflect a real and significant effect of the sex of the first child on the probability of marital breakup.

<u>The effect of marital breakup on mean outcomes</u>

Cross-sectional (OLS) regressions of income and labor supply on marital breakup (Table 3), which admit no causal interpretation, show that breakup of the first marriage is correlated with large losses in income and large increases in labor supply. Both of these relationships confirm the conventional view that women whose first marriages end are significantly worse off than women whose first marriages remain intact.

Two-stage least squares estimates, on the other hand, suggest that the mean effect of marital breakup on material well-being is quite different from the cross-sectional results. The two-stage estimate of the effect of divorce on others' income is negative but insignificant, though still within two standard deviations of the OLS estimates, as is the two-stage estimate for log household income.

The two-stage estimate of the effect of divorce on household income level, however, is significantly more positive than the OLS estimate—while the coefficient is not statistically different from zero, it is more than two standard deviations from the negative OLS estimate. The

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impact on marital stability had been realized, thus increasing the potential causal effect. On the other hand, older women are less likely to have experienced divorce (due to cohort effects) and widowhood may be higher, attenuating the estimated effect of child sex on marital breakup. Finally, in order to get a large enough sample, we needed to look at more recent data, since the CPS recorded women's fertility history only in the four samples we used.

same is true for the high predicted-divorce subsample. For the low predicted-divorce subsample our first stage is barely significant, so the two-stage least squares estimate cannot rule out zero effect or an effect equal to the cross-sectional effect. (The absence of significant estimates for the low predicted divorce subsample in all of our IV specifications is consistent with the fact that the first stage is only marginally significant for this group, supporting our hypothesis that child sex affects outcomes only through divorce).

Taken together, these results imply that on average there is negative selection into divorce—women who would have had low income anyway are more likely to divorce, creating a negative cross-sectional correlation between income and divorce—and that there is no significant causal effect of divorce on mean income. The lack of significant effect on others' income is probably due to the fact that most women compensate for the loss of their husbands' income by re-marriage or by moving in with other adults. Moreover, we find a very large increase in mean hours worked (about 1,000 hours a year), which is more than double the cross-sectional effect and clearly helps offset the loss of husband income in the measure of household income.[27]

As the labor supply results indicate, the stability of mean income should not be taken to imply that there is no mean welfare loss from divorce. The increase in work without a significant increase in income could imply a welfare loss for women, since they experience a mean decrease in leisure without an expansion of consumption possibilities. In addition, it may imply adverse consequences for children; for example, if parents are not perfectly altruistic they may divorce even if doing so disrupts joint household production, including childrearing. Our

[27] Our results thus far are broadly consistent with those of Bedard and Deschenes (2003), despite our use of different sample restrictions and specifications, lending further evidence of the robustness of the relationships we analyze.

results indicate that on average children receive no gain in consumption but likely experience a decrease in time with their mother (and probably with their father as well).

Estimation of the Effect of Marital Breakup on the Income Distribution

Estimation Framework

The difference in sign between the IV estimates of the effect of divorce on mean log income and on mean level of income leads us to investigate the possibility that there are important effects of marital breakup on the income distribution, particularly at the bottom, that aren't evident at the mean. In fact, it makes intuitive sense that the effect of divorce on the income distribution would be to fatten the lower tail, rather than to shift the entire distribution uniformly downward. After all, divorce—and the implied withdrawal of the husband's income—represents a discrete fall in income. To the extent that women remarry, move in with other relatives, or have high earning potential, many may end up as well off financially as before (or even better off)—resulting in little effect of divorce on the mean of the distribution. And yet a subset of women who cannot recover from the loss would experience a much greater than average effect of divorce, falling near the bottom of the distribution.

To address this possibility, we consider more flexible specifications in which we estimate the effect of marital breakup on the probability that a woman's income is below various thresholds. This enables us to identify the marginal effect of marital breakup on the cumulative distribution function (CDF). In addition, we obtain estimators of the CDF itself for those whose first marriage is broken and for those whose first marriage is intact. Doing so allows us to evaluate the magnitude of the effect of divorce on the income distribution.

Recall that the local average treatment effect formula in equation (3) gave the causal effect of treatment on the compliers. Similarly, Abadie (2002, 2003) demonstrates that in absence of covariates and with the same assumptions as the standard two-stage least squares model:[28]

$$(4) \qquad E[Y_0 \mid D_1 > D_0] = \frac{E[(1-D)Y \mid Z=1] - E[(1-D)Y \mid Z=0]}{E[(1-D) \mid Z=1] - E[(1-D) \mid Z=0]}$$

We can similarly estimate the following equation using two-stage least squares with controls, using the sex of the eldest child as an instrument:

$$(5) \qquad (1-D)Y = \gamma(1-D) + X\delta + \mu$$

This strategy gives a consistent estimator:

$$(6) \qquad \hat{\gamma}_{IV} \to \gamma = E[Y_0 \mid X; D_1 > D_0]$$

That is, γ gives the expected value of Y for compliers who have a boy (and whose marriage therefore remains intact). When we apply this method to the case where Y is the CDF of the income distribution we can effectively trace out the CDF for compliers who have boys. Similarly, we could estimate the CDF for compliers who have girls.[29] But a standard two-stage least squares estimate of the effect of marital breakup on an indicator for a given income level gives the difference between the two CDFs. Thus, by summing the standard coefficient and the estimate of equation (6), we can likewise

[28] Proof of equation (4): $Z = i \Rightarrow D = D_Z \Rightarrow Y = Y_Z$ for $i = 0,1$, so the numerator is:

$$E[(1-D)Y \mid Z=1] - E[(1-D)Y \mid Z=0] = E[(1-D_1)Y_1 - (1-D_0)Y_0]$$

By the monotonicity assumption this equals:

$$E[(1-D_1)Y_1 - (1-D_0)Y_0 \mid D_1 > D_0]P[D_1 > D_0] = E[Y_0 \mid D_1 > D_0]P[D_1 > D_0]$$

Dividing by the denominator yields: $E[Y_0 \mid D_1 > D_0]$.

Since X is discrete, this proof can be extended to the case where we add controls.

[29] By estimating the equation: $DY = \theta D + X\rho + \eta$ we can get $\hat{\theta}_{IV} \to \theta = E[Y_1 \mid X; D_1 > D_0]$. The proof is similar.

trace out the CDF for compliers who have girls. Finally, we also estimate equation (6) using OLS, and compare it to the instrumental variables estimate.

<u>The effect of marital breakup on the distribution of outcomes</u>

In each of Tables 4, 5, and 6, the columns of results labeled "First marriage intact" report the CDF for those who remain married, estimated using the method described above. (Recall that in the two-stage least squares regressions, the CDFs are estimated for the specific group of women—"compliers"—who divorce or stay married in response to the sex of the first-born child.) The columns labeled "Difference in CDFs: broken – intact" report the estimated difference in CDFs between the compliers who have divorced and those who stay married, which is the standard coefficient on breakup in the OLS or two-stage least squares estimation.

OLS estimates of the difference in distribution of income by marital breakup (first two columns in Tables 4 and 5) tell a familiar story. In cross-section, those with broken first marriages have income distributions —both household and others'— that are first-order stochastically dominated by those of women with intact first marriages. That is, divorce is correlated with a uniform shift downward in income at every point in the income distribution.[30]

The two-stage least squares estimates in Tables 4 and 5 give a more nuanced picture.[31] We do in fact find a large effect of marital breakup on the probability of being at the bottom of the income distribution: women whose first marriage is broken are 42 percentage points more

[30] Note also that the estimated income distribution for compliers whose first marriage remains intact is first-order stochastically dominated by the income distribution for the full sample. This result, which holds throughout Tables 4-6, is consistent with the view that the people who respond to the instrument are typically of relatively low SES.
[31] Note that the estimation of linear probability models results in some estimates that are slightly outside the [0,1] interval, though always well within two standard errors of this interval. Despite this drawback, we prefer to follow this methodology because of its transparency.

likely (about twelve times as likely) to have less than $5000 in others' income, and 23 percentage points more likely (about 80 percent more likely) to have less than $10,000. While legal transfers (which include child support and mean-tested transfers) reduce the number of compliers with no income, over a quarter of the divorced compliers have no unearned income even after accounting for transfers—compared to virtually none of the compliers who stay married (results not shown).

The analysis of household income similarly shows a large effect of marital breakup on the density at the bottom of the income distribution. Roughly one in six of those who experience marital breakup have less than $5000 in household income, compared to virtually none of those who remain married.[32] Results follow a similar pattern for the high predicted-divorce subsample, as expected. For the low predicted-divorce subsample, the effects are not significant and do not demonstrate any consistent pattern, also as expected.

Interestingly, the two-stage estimates also show that those who divorce due to the instrument (in both the full sample and the high predicted-divorce subsample) are somewhat *more* likely to have income near the top of the distribution, although the differences are not statistically significant. The reversal in the sign of the difference occurs at (in the case of others' income) or below (in the case of household income) the mean of the distribution, explaining why models for the mean find little effect of marital breakup on income. Previous literature (e.g. Mueller and Pope 1980, Jacobs and Furstenberg 1986) finds that when divorced women remarry, their second husband is typically more educated and has a higher occupational SES score

[32] Although they cannot earn enough to make up for the loss in others' income, divorced compliers do have a very large labor supply response to the loss. Since the distributional effect on hours does not differ markedly from the mean responsiveness, a separate analysis is not included here.

(although at least some of this is due to life-cycle effects). In addition, Bedard and Deschenes (2003) argue that many divorced mothers co-reside with their parents, who likely have higher joint incomes than their husbands. The reversal in sign is more substantial for total household income than for others' income, probably because that also includes top-end variation in women's earnings as well as sources of other income (such as alimony and child support).

The results discussed thus far ignore one important aspect of divorce—namely, it reduces family size. Thus even though income decreases at the bottom with divorce, women may not necessarily end up worse off; on the other hand, if income gains at the top come primarily through remarriage, the effect may be neutralized by increased family size. To estimate the effect of divorce on the ratio of income to needs, we divide each woman's total household income by the poverty line for a household of that size.

As shown in Table 6, we find that changes in family size do not mitigate our results. The OLS estimates still indicate that marital breakup decreases normalized household income at all levels. The two-stage least squares results indicate that virtually none of those still in their first marriage have household income below the poverty line, while nearly a quarter of those whose first marriage ended are below poverty. Compliers whose first marriage ended are, however, significantly more likely to be above 400 percent of poverty than are compliers whose first marriage remains intact. The results for the high-predicted divorce subsample are similar – divorce significantly increases the probability of poverty and the effect reverses at the top of the distribution, although the effect is not significant for the subsample. As always, the low-poverty subsample shows no significant effect and no discernable pattern, consistent with our hypothesis.

Discussion

Our results suggest that negative selection into divorce accounts for the observed relationship between marital breakup and lower mean income. Yet marital breakup does have a significant causal effect on the distribution of income: divorce increases the percent of women at the bottom—and perhaps at the top—tail of the income distribution. In net, divorce causally increases poverty, and perhaps inequality more generally, for women with children.

What would have been the poverty rate of women with children in 1995 had the fraction of ever-divorced mothers not changed since 1980? In 1995 (the most recent year for which we have data on marital history, from the CPS), the poverty rate for women with our sample characteristics who are still in their first marriage was 8.7 percent, while the rate for women who have ever divorced was 21.7 percent. Thus if divorce had stayed at its 1980 prevalence (17.2 percent ever divorced), the overall poverty rate in this sample would be:

$$\begin{aligned}\text{Counterfactual poverty rate} &= (\%\text{ ever divorced 1980})*(\text{poverty rate} \mid \text{ever divorced 1995})\\ &\quad + (\%\text{ never divorced 1980})*(\text{poverty rate} \mid \text{never divorced 1995})\\ &= 0.172*0.217 + (1-0.172)*0.087 = 0.109,\end{aligned}$$

or 10.9 percent. Because the prevalence of divorce rose to 28.6 percent, the poverty rate became:

$$\begin{aligned}\text{Actual poverty rate} &= (\%\text{ ever divorced 1995})*(\text{poverty rate} \mid \text{ever divorced 1995})\\ &\quad + (\%\text{ never divorced 1995})*(\text{poverty rate} \mid \text{never divorced 1995})\\ &= 0.286*0.217 + (1-0.286)*0.087 = 0.124,\end{aligned}$$

or 12.4 percent. Thus the increase in divorce may potentially have caused an increase of 1.5 points, or 13.8 percent, in the poverty rate for women with our sample characteristics. If we assume that the effect holds outside of those with our sample characteristics, we can conclude that nearly 1.4 million more women and children were in poverty in 1995 than would have been if the divorce rate had remained at its 1980 level.

We conclude by noting that our findings do not have clear-cut policy implications. While some might interpret them as evidence for the importance of traditional family structures, others may conclude that they emphasize the need for more generous welfare policies.

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Data Appendix

The 5 percent 1980 Census data contain several measures that allow us to analyze a woman's fertility history. These include the number of children ever born to a woman, the number of marriages, the quarter as well as year of first birth, and the quarter and year of first marriage. This information permits us to identify the sex of the first-born child for most women, although not for women whose eldest child has left the household.

A substantial drawback of using cross-sectional data is the fact that we can only observe the sex of the eldest child residing with a woman, whereas our true characteristic of interest is the sex of the firstborn child. It is imperative that we create a sample of women for whom measurement error in the sex of the observed first-born child has a classical structure.

To that end, we attempt to restrict the sample to those women observed with all their biological children. We do so in order to limit the risk that our results will be affected by differential attrition of boys and girls. In particular, we are concerned that boys may be differentially likely to end up in the custody of their fathers in the event of marital breakup. In fact, as Table A, panel 1, shows, girls are significantly less likely than boys to be living with their father and not their mother. This pattern could lead to endogeneity of our instrument if the sample were left uncorrected. If, in the event of divorce, fathers keep the sons and mothers keep the daughters, there will be a spurious positive correlation in the overall sample between marital breakup and the eldest *observed* child being a girl.

To address this issue, we exclude any woman for whom the number of children ever born does not equal the number of children living with her. If a mother lives with stepchildren or adopted children in a number that exactly offsets the number of her own children that are not living with her, this rule will fail to exclude her. We therefore further minimize the possibility of including women who have non-biological children "standing in" for biological children by including only women whose age at first birth is measured as between 19 and 44.

Limiting our sample to women who are living with all of their children reduces but does not eliminate the threat that differential custody rates could bias our result—because we could still be more likely to include divorced women with two girls than those with one boy and one girl or those with two boys. We find, however, that mothers living with all their children and mothers in the overall population are equally likely to be observed with a girl as the eldest child, which suggests that sex of the eldest child is not a major determinant of living with all of one's children (See Table A, panel 2).

Second, we limit our sample to women whose first child was born after their first marriage, since breakup of the first marriage is our focus. If instead we included out-of-wedlock births, we would be concerned that the sex of the first child affected selection into the first marriage. To the extent that people could learn the sex of the child before it was born and thereby select into "shotgun marriages," we may still have selection into first marriage. But ultrasound technology was not yet widely used in 1980 (Campbell 2000), so this threat is not of particular concern. In addition, because we can only identify the beginning of the first marriage and not the end, we may include some women whose first child was born after the breakup of the first marriage. But this should create only classical measurement error in our first stage estimation.

Third, we look only at mothers whose eldest child is a minor, since those who still live with their adult children may be a select group. Further, since girls are differentially likely to leave home early (at ages 17 and 18), we restrict to mothers whose eldest child is under age 17. Fourth, we limit our sample to white women because black women's childbearing and marital decisions may be quite different, and fully modeling the differences would greatly complicate the analysis. Finally, we leave out women whose first child was a twin, both because different-sex twins would complicate our instrument and because twins increase the number of children a woman has.

Selection into our restricted sample might be cause for concern, just as selection into the labor market is cause for concern when measuring labor outcomes. To test for such a problem, we generated the predicted probability that a woman was included in our sample based on age and birthplace dummies. Then we re-ran our two-stage estimates, treating a woman's predicted probability of inclusion as an endogenous regressor. Our first- and second-stage estimates were quite stable with and without this variable.

To create our high- and low-predicted divorce subsamples, we create an exogenous risk index (Table A, panel 3) by predicting "ever divorced" using age, age squared, age at first birth, and dummies for place of birth and for high school dropout (recall that our sample includes only women whose first birth occurs after age 19, so the decision on whether to graduate from high school can be treated as pre-determined).

In summary, the limitations we place on the sample are designed to create a group of women for whom we can measure the sex of the firstborn child with only classical measurement error. These restrictions weaken the power of our first stage, but we believe this compromise is necessary in order to minimize concerns about endogeneity of our instrument.

Table 1. Descriptive Statistics—Mothers with Minor Children Living at Home

		Samples		
	All women with children		Predicted-divorce index	
Variable		Full sample	High	Low
Demographics				
Age	35.1	31.6	36.0	25.2
Years of schooling	12.0	12.8	11.5	13.0
Household income	22,747	23,114	23,598	19,549
Total own income	4,905	4,458	4,625	3,805
Weeks worked last year	23.9	22.5	23.1	21.4
Usual hours worked	21.1	19.9	19.8	21.2
Marital status				
Currently married, spouse present	0.802	0.891	0.869	0.913
Currently separated	0.040	0.024	0.028	0.025
Currently divorced	0.085	0.072	0.084	0.052
Ever divorced	0.215	0.172	0.202	0.118
Never married	0.043	0.000	0.000	0.000
Number of observations	1,610,516	619,499	154,041	155,355

NOTE: The full sample includes white women who are living with all of their children, whose eldest child is under 17, who had their first birth after marriage, after age 18 and before age 45, and had a single first birth. The high predicted-divorce subsample includes only women in the top quartile of the predicted divorce distribution; the low predicted-divorce subsample includes only those in the bottom quartile. Income is in 1980 dollars.

Table 2. The Effect of Eldest Child Sex on Marital Breakup

	All women with children	Samples					
			Age of oldest child			Predicted-divorce index	
		Full sample	0-5	6-11	12-16	High	Low
All education levels	0.0113	0.0063	0.0038	0.0057	0.0106	0.0113	0.0036
	(0.0007)	(0.0010)	(0.0015)	(0.0018)	(0.0021)	(0.0022)	(0.0018)
Observations	1,605,339	619,499	236,522	221,937	161,040	154,041	155,355
High school dropouts		0.0149	0.0106	0.0090	0.0264		
		(0.0033)	(0.0054)	(0.0057)	(0.0059)		
Observations		73,426	25,111	24,849	23,466		
High school graduates		0.0057	0.0041	0.0053	0.0084		
		(0.0015)	(0.0024)	(0.0027)	(0.0030)		
Observations		277,155	100,601	99,816	76,738		
At least some college		0.0045	0.0023	0.0052	0.0075		
		(0.0015)	(0.0020)	(0.0026)	(0.0034)		
Observations		268,918	110,810	97,272	60,836		

NOTE: The sample includes white women who are living with all of their children, whose oldest child is under 17, who had their first birth after marriage, after age 18 and before age 45, and had a single first birth. The table reports estimates of the coefficient on (eldest child is a girl) in equation (1) in the text. Other covariates are: Age, Age squared, Age at first birth and High school dropouts. Robust standard errors in parentheses.

Table 3. The Effect of Marital Breakup on Mean Economic Outcomes

	Full Sample		Predicted divorce index (Specification check)			
			High		Low	
Dependent Variable	OLS	2SLS	OLS	2SLS	OLS	2SLS (Falsification check)
Ln(Household income)	-0.501	-0.230	-0.547	-0.867	-0.439	1.514
	(0.005)	(0.437)	(0.009)	(0.542)	(0.011)	(1.693)
Others' income	-9241	-1041	-10561	-8450	-5014	8605
	(42.57)	(5178)	(78.68)	(5939)	(87.82)	(15369)
Household income	-5577	6548	-6721	-981	-2649	9147
	(43.41)	(5570)	(80.57)	(6224)	(89.76)	(15817)
Hours worked last year	420	1053	347	937	369	951
	(2.85)	(360)	(5.39)	(408)	(6.31)	(1180)
Number of observations	619,499		154,041		155,355	

NOTE: The full sample includes white women who are living with all of their children, whose eldest child is under 17, who had their first birth after marriage, after age 18 and before age 45, and had a single first birth. High predicted-divorce subsample includes only women in the top quartile of the predicted divorce distribution; the low predicted-divorce subsample includes only those in the bottom quartile. All the regressions include the following controls: age, age squared, age at first birth and a dummy for high school dropouts. There were 4,766 observations, or about 0.77% of our observations, for which household income was zero or negative. For those observations we set Ln(Household income) to zero. Income is in 1980 dollars. Robust standard errors are in parentheses.

Table 4. Cumulative Distribution of Income of Women's Other Household Members, by Marital Status

	OLS		2SLS					
					Predicted divorce index (Specification check)			
	Full Sample		Full Sample		High		Low (Falsification check)	
Dependent Variable	CDF when first marriage is intact	Difference in CDFs: broken-intact	CDF when first marriage is intact	Difference in CDFs: broken-intact	CDF when first marriage is intact	Difference in CDFs: broken-intact	CDF when first marriage is intact	Difference in CDFs: broken-intact
Others' income								
≤$5000	0.0507	0.3928	0.0378	0.4177	0.0800	0.5249	0.1587	0.2041
	(0.0003)	(0.0014)	(0.0785)	(0.1202)	(0.0929)	(0.1457)	(0.3061)	(0.4067)
≤$10000	0.1458	0.3850	0.2770	0.2284	0.2932	0.2727	0.8545	-0.4356
	(0.0005)	(0.0015)	(0.1270)	(0.1567)	(0.1413)	(0.1785)	(0.6081)	(0.6870)
≤$20000	0.5459	0.2226	0.8751	-0.0416	0.5735	0.1968	1.0290	-0.0898
	(0.0008)	(0.0014)	(0.1814)	(0.1937)	(0.1866)	(0.2054)	(0.5957)	(0.6209)
≤$30000	0.8412	0.0754	1.0369	-0.0821	0.9996	-0.0195	0.8886	0.1992
	(0.0006)	(0.0010)	(0.1323)	(0.1399)	(0.1554)	(0.1629)	(0.3166)	(0.3620)
Number of observations	619,499		619,499		154,041		155,355	

NOTE: The sample includes white women who are living with all of their children, whose eldest child is under 17, who had their first birth after marriage, after age 18 and before age 45, and had a single first birth. All the regressions include the following controls: age, age squared, age at first birth and a dummy for high school dropouts. Income is in 1980 dollars. Robust standard errors in parentheses.

Table 5. Cumulative Distribution of Household Income, by Marital Status

	OLS		2SLS					
					Predicted divorce index (Specification check)			
	Full Sample		Full Sample		High		Low (Falsification check)	
Dependent Variable	CDF when first marriage is intact	Difference in CDFs: broken-intact	CDF when first marriage is intact	Difference in CDFs: broken-intact	CDF when first marriage is intact	Difference in CDFs: broken-intact	CDF when first marriage is intact	Difference in CDFs: broken-intact
Household income								
≤$5000	0.0294	0.0982	-0.0155	0.1900	0.0082	0.3086	0.2163	-0.0367
	(0.0005)	(0.0007)	(0.0603)	(0.0865)	(0.0739)	(0.1137)	(0.2437)	(0.3082)
≤$10000	0.0921	0.1999	0.1502	0.2875	0.1941	0.3994	0.6388	-0.2840
	(0.0008)	(0.0010)	(0.1024)	(0.1312)	(0.1193)	(0.1591)	(0.4912)	(0.5475)
≤$20000	0.3994	0.2153	0.7760	-0.2547	0.5994	-0.0094	1.7693	-0.8853
	(0.0013)	(0.0015)	(0.1806)	(0.2048)	(0.1821)	(0.2086)	(0.8697)	(0.8331)
≤$30000	0.7496	0.0788	1.1079	-0.2360	0.8813	-0.0610	0.8815	0.2333
	(0.0012)	(0.0013)	(0.1621)	(0.1729)	(0.1740)	(0.1897)	(0.4202)	(0.4758)
Number of observations	619,499		619,499		154,041		155,355	

NOTE: The sample includes white women who are living with all of their children, whose eldest child is under 17, who had their first birth after marriage, after age 18 and before age 45, and had a single first birth. All the regressions include the following controls: age, age squared, age at first birth and a dummy for high school dropouts. Income is in 1980 dollars. Robust standard errors in parentheses.

Table 6. Cumulative Distribution of Poverty, by Marital Status

	OLS		2SLS					
					Predicted divorce index (Specification check)			
	Full Sample		Full Sample		High		Low (Falsification check)	
Dependent Variable	CDF when first marriage is intact	Difference in CDFs: broken-intact	CDF when first marriage is intact	Difference in CDFs: broken-intact	CDF when first marriage is intact	Difference in CDFs: broken-intact	CDF when first marriage is intact	Difference in CDFs: broken-intact
Percentage of the poverty threshold								
≤ 100	0.0565	0.1322	0.0041	0.2407	0.0714	0.2982	0.2015	-0.0188
	(0.0004)	(0.0011)	(0.0806)	(0.1081)	(0.1038)	(0.1407)	(0.2983)	(0.3736)
≤ 200	0.2224	0.2069	0.2958	0.2487	0.3695	0.2160	1.0154	-0.2754
	(0.0006)	(0.0015)	(0.1449)	(0.1684)	(0.1659)	(0.1941)	(0.6610)	(0.6492)
≤ 300	0.5051	0.1550	0.8077	-0.1800	0.5079	0.1726	2.2120	-1.2861
	(0.0008)	(0.0015)	(0.1824)	(0.2011)	(0.1869)	(0.2078)	(1.0217)	(0.9594)
≤ 400	0.7349	0.0780	1.2086	-0.4442	0.8366	-0.0519	1.1202	-0.1784
	(0.0007)	(0.0013)	(0.1751)	(0.1909)	(0.1673)	(0.1842)	(0.5366)	(0.5563)
Number of observations	619,499		619,499		154,041		155,355	

NOTE: The sample includes white women who are living with all of their children, whose eldest child is under 17, who had their first birth after marriage, after age 18 and before age 45, and had a single first birth. All the regressions include the following controls: age, age squared, age at first birth and a dummy for high school dropouts. Poverty is calculated using 1980 measures. Robust standard errors in parentheses.

Table A. Specification Checks and Sample Construction

Panel 1	Dependent variable: Child is a girl
Child lives with father and not with mother	-0.0501
	(0.0022)
Constant	0.4888
	(0.0003)
Observations	2,810,256
Panel 2	Dependent variable: Eldest child is a girl
Mother lives with all her children	0.0000
	(0.0013)
Constant	0.4871
	(0.0011)
Observations	790,746
Panel 3	Dependent variable: Ever divorced
Age	0.0264
	(0.0003)
Age squared ÷ 100	-0.0305
	(0.0005)
Age at first birth	-0.0025
	(0.0001)
Dummy for high school dropout	0.0947
	(0.0012)
Observations	1,092,433

NOTES: The sample in panel 1 includes white children under age 19 (the unit of observation is a child) The sample in panel 2 includes women meeting the set of restrictions imposed throughout the paper, except that we do not restrict to mothers living with all their children. That is, it includes white mothers who had their first birth after marriage after age 18 and before age 45, had a single first birth, and whose eldest child is under 17.

The sample in panel 3 includes white women who had their first birth after marriage, after age 18, and before age 45, and had a single first birth. Controls also include birthplace dummies (147 categories) Robust standard errors in brackets

Figure 1. Mothers' Poverty Rates, by Marital Status

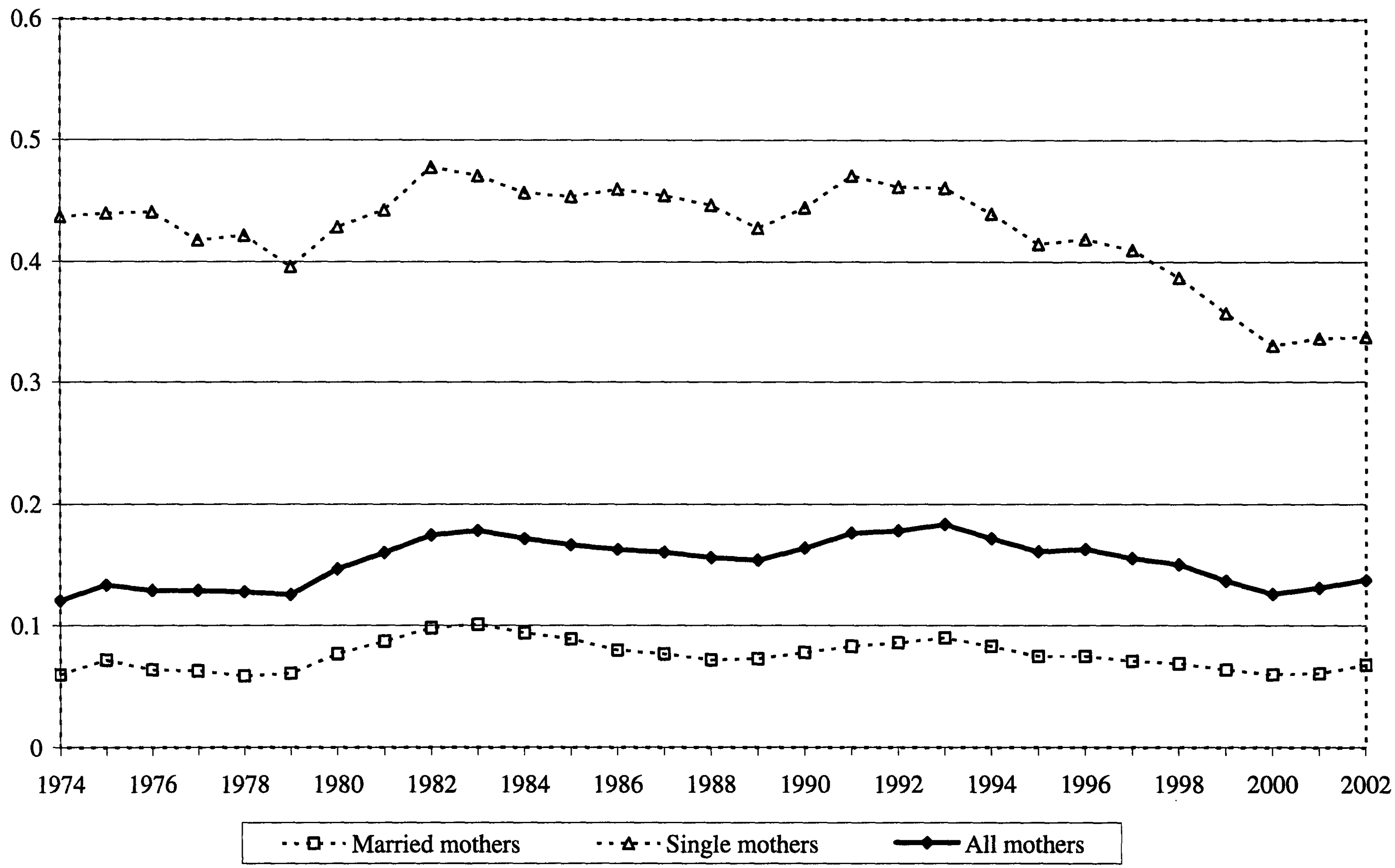
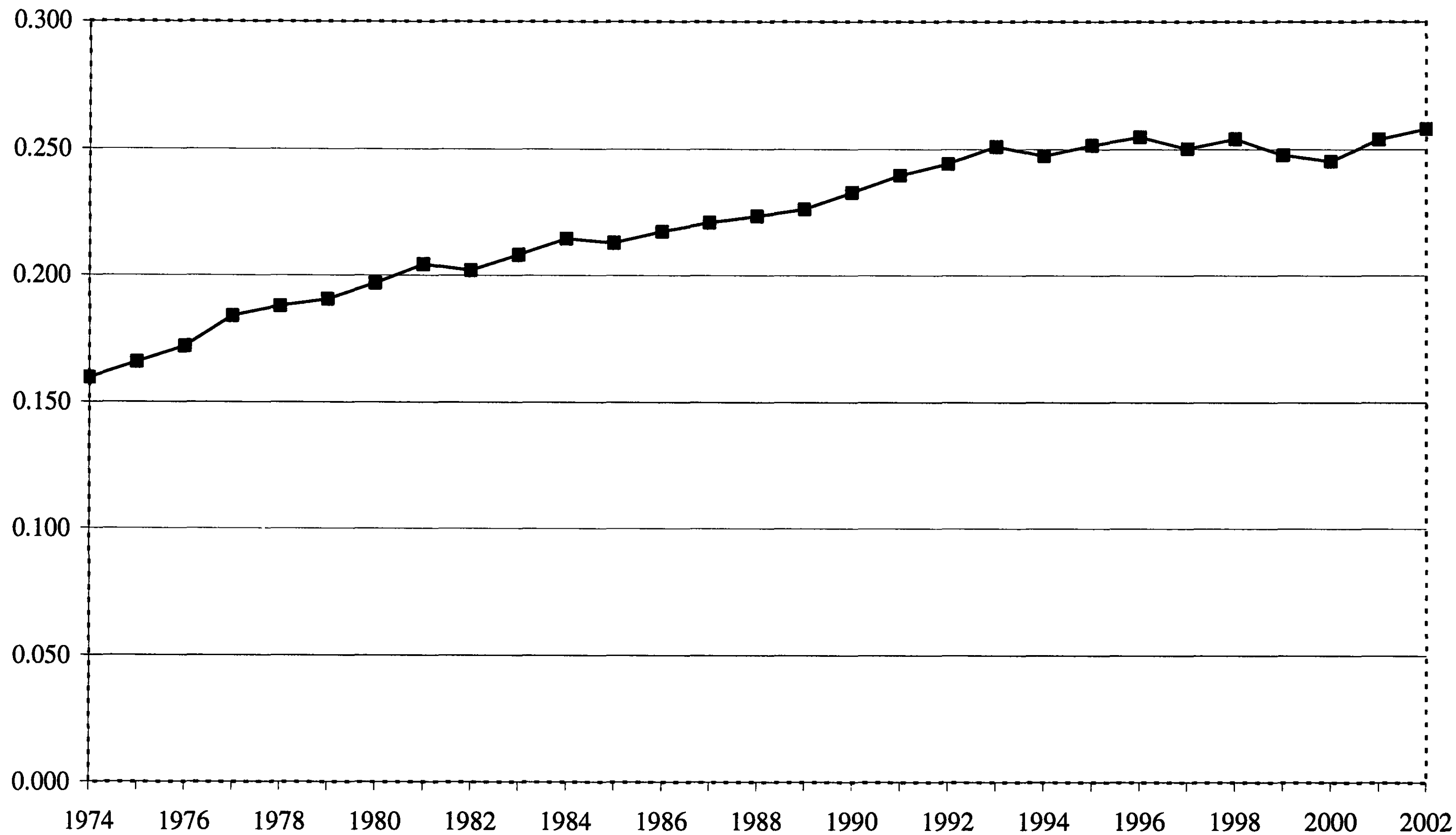


Figure 2. Percent of Mothers Who are Single



Abortion Legalization and Lifecycle Fertility

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Abstract

Previous research has convincingly shown that abortion legalization in the early 1970s led to a significant drop in fertility at that time. But this decline may have either represented a delay in births from a point where they were "unintended" to a point where they were "intended," or they may have represented a permanent reduction in fertility. We combine data from the 1970 U.S. Census and microdata from 1968 to 1999 Vital Statistics records to calculate lifetime fertility of women in the 1930s through 1960s birth cohorts. We examine whether those women who were born in early legalizing states and who passed through the early 1970s in their peak childbearing years had differential lifetime fertility patterns compared to women born in other states and in different birth cohorts. We consider the impact of abortion legalization on both the number of children ever born as well as the distribution of number of children ever born. Our results indicate that much of the reduction in fertility at the time abortion was legalized was permanent in that women did not have more subsequent births as a result. We also find that this result is largely attributable to an increase in the number of women who remained childless throughout their fertile years.

I. Introduction

The ability to control one's fertility through legal abortion is one of the most profound changes in family planning of the last fifty years. The significant change in the cost of abortion brought about by its legalization enabled women to abort pregnancies that would otherwise have resulted in an unwanted birth. Past research has shown that women responded very strongly to this incentive when abortion was legalized in the early 1970s, reducing their fertility by at least 4 percent (Levine et al., 1999).

The immediate drop in the birth rate in response to legal abortion does not, however, necessarily indicate that women have fewer children over the rest of their childbearing years. Many of the women who chose to abort rather than have an unwanted birth still had a number of fertile years remaining. If the births that did not occur were "replaced" by additional births subsequently, then women would not experience a net reduction in lifetime fertility. Therefore, the short-run impacts on birth rates estimated in earlier work may not represent long-run reductions in birth rates and completed fertility.[33]

It is important to understand the extent to which the short-run fertility is permanent because of the different implications for subsequent child outcomes. For instance, in response to the legalization of abortion, Gruber et al. (1999) find improvements in child well-being and Donohue and Levitt (2001) report a reduction in crime when those children mature. One explanation for these findings is that those children who were born are differentially selected among those who could have been born if abortion had not been legal. But if births are just

[33] Angrist and Evans (1999) raise this issue as well in their analysis of the longer term outcomes for teens, whose fertility fell dramatically when abortion was legalized in the early 1970s. Our approach is partially aided by the passage of more time since abortion was legalized, so that completed fertility (or "virtually" completed fertility) can be addressed for more birth cohorts. In addition, our use of Vital Statistics Microdata in addition to Census data substantially improves the precision of our estimates.

timed differently, then any selection effect would strongly depend upon whether children's outcomes are determined by the fixed characteristics of mothers or the age at which those mothers give birth. If it is the mother's fixed characteristics that matter, then the effects reported in Gruber et al. (1999) and Donahue and Levitt (2001) should diminish over time since the children those mothers would have had will eventually be born. If it is the age of the mother that matters, then even the delay will lead to lasting improvements in child well-being.

The purpose of this paper is to estimate the impact of abortion legalization on lifecycle fertility, beginning from the point at which abortion was legalized through the remainder of women's childbearing years. We combine data from the 1970 U.S. Census and microdata from 1968 to 1999 Vital Statistics records to calculate lifetime fertility of women in cohorts born in the 1930s through 1960s. We examine whether those women who were born in early legalizing states and who passed through the early 1970s in their peak abortion and childbearing years had differential lifetime fertility patterns compared to women born in other states and in different birth cohorts. We consider the impact of abortion legalization on both the number of children ever born as well as the distribution of number of children ever born.

We find that much of the reduction in fertility at the time abortion was legalized was permanent; women did not have many more subsequent births as a result. We also find that this result is attributable to an increase in the number of women who remained childless throughout their fertile years.

II. Background

The process by which abortion was legalized in the United States in the early 1970s is an integral component of the identification strategy we employ. A detailed description of the events leading up to the legalization of abortion in the United States is provided in Garrow (1994). Briefly, abortion became widely available in five states in 1970. In four of these states (New York, Washington, Alaska, and Hawaii), there was a repeal of anti-abortion laws. In the fifth, California, there was a "de facto" legalization, since in late 1969 the California State Supreme Court ruled that the pre-1967 law outlawing abortion was unconstitutional. Following the 1973 Supreme Court decision in Roe v. Wade, abortion became legal in all states. These events contributed to a dramatic increase in the frequency with which women chose to end a pregnancy through abortion; the abortion rate almost doubled from 16.3 abortions per 1,000 women aged 15-44 in 1973 to 29.3 in 1980 (Finer and Henshaw, 2003).

Levine et al. (1999) use the natural experiment provided by the staggered introduction of legalized abortion across states to estimate its impact on contemporaneous birth rates. They categorize states by abortion legality in different years to jointly exploit two legislative changes. They first compare changes in fertility rates in the “early legalizing” (or “repeal”) states from before 1970 to after 1970, relative to other (“non-repeal”) states where abortion was still illegal. Then they reverse the treatment in 1973, comparing births in those states where the Roe decision legalized abortion to the repeal states where it had already been legal. The results indicate that abortion legalization in the early 1970s reduced the fertility rate by over 4 percentage points. Teens and women age 35 and over experienced much larger reductions in fertility, as did nonwhite and unmarried women.

As an extension to their analysis, Levine et al. provide estimates separately comparing repeal states to non-repeal states that vary by their distance to repeal states, using the distance categories of within 250 miles (from closest population centroids), 250 to 750 miles, and greater than 750 miles. If we assume that women in closer non-repeal states were more likely to cross state lines to obtain an abortion (e.g. those in New Jersey crossing into New York), then the effects of abortion legalization should be strongest for those living farthest from the early repeal states. They also find that this effect was much larger when early repeal states are compared to more distant comparison states, suggesting that travel between states dampened the estimated impact.

III. Methodology

To examine the impact of abortion legalization on life-cycle fertility, we will again exploit the quasi-experiment provided by the variation across states in the exact timing of abortion legalization in the early 1970s. The main methodological extension in the present analysis is to convert the focus on period effects that Levine et al. estimated to a focus on cohort effects. The period effect analysis operated by comparing women at different ages at the time of the law changes (either early repeal or Roe v. Wade), and measuring their current fertility rates. The cohort analysis instead compares women who passed through these different legal states at the time when they were most likely to be affected by abortion availability, and examines measures of completed fertility.

A. Defining "Treatment Cohorts"

We focus on those cohorts who were most likely to be affected by changes in abortion law in the early 1970s based on their age at that time. Women in these cohorts from repeal states experienced greater exposure to legal abortion and may have had different fertility patterns over the remainder of their lives as a result. Once these "treatment cohorts" are defined, the methodological approach is analogous to that based on period effects. Women in the different sets of states in cohorts before the treatment cohorts and after the treatment cohorts should have been equally affected by changes in abortion policy; abortion would have either been uniformly illegal (for the earlier cohorts) or uniformly legal (for the later cohorts) during the years in which they were most at-risk of unwanted pregnancies.

To define our "treatment cohorts," we extend the analysis in Levine et al. to examine the impact of abortion legalization by exact age, rather than by broad age groups, using Vital Statistics microdata between 1968 and 1980 (described in the next section). Comparable to Levine et al.'s findings (albeit with greater age-specificity), we find that legalized abortion had the biggest relative impact on births to teens and to older women. The impact is largest at age 15, declines to zero by age 28, and then rises again.[34]

For the purposes of the present analysis, however, these results are somewhat misleading because they reflect the impact on *relative* birth rates. In 1970, birth rates are highest between the late teens and late 20s, peaking at age 23, and are low in the early teens and into the 30s and 40s. Therefore, the large relative impact of abortion legalization on births at older ages needs to be tempered by the fact that the base is relatively low. The absolute impact on the number of births is small at older ages.

To address the absolute impact on fertility, we repeat Levine et al.'s estimating strategy, but replace the natural logarithm of the birth rate as the dependent variable with the birth rate in levels. As such, parameter estimates represent the absolute effect on the birth rate rather than the relative effect based on the percentage change. When we make this change, we see that ages 16 through 26 represent the peak ages at which the *number* of births was affected.[35] At these ages, births fell by roughly 6 per 1,000 women in response to abortion legalization. Therefore, we

[34] Detailed results are provided in Ananat et al. (2004).
[35] Detailed results of this analysis are provided in Ananat et al. (2004) as well.

would expect the largest impact on lifecycle fertility to hit those cohorts who passed through the early 1970s between the ages of 16 and 26.

The next step requires transitioning from these age-specific period effects to a definition of the treatment cohorts. We know that abortion laws differed between repeal and other states over the 1970 to 1972 period, which, given the nine month delay of pregnancy, largely applies to births in 1971 to 1973. We can combine this information with the fact that the peak ages of abortion legalization's impact was 16 to 26. This means that the birth cohorts of the later 1940s and earlier 1950s were the ones whose fertility was most directly affected by abortion legalization. As an approximation, in our analysis we define the 1946 to 1955 birth cohorts to represent the treatment cohorts.[36] If abortion legalization in the early 1970s reduced fertility at those ages for these women, any permanent impact on lifetime fertility would show up as a gap in children ever born between those who lived in early repeal states and others. Women born after 1955 would have entered childbearing ages following the Roe decision, so they would have faced a lifetime of exposure to legal abortion regardless of their state of residence. Similarly, women born in the 1930s and earlier 1940s were in their later stages of childbearing by the early 1970s. These women passed through their peak childbearing years with no legal abortion access, regardless of where they lived.

B. Empirical Model

These results bring us to an empirical specification of our model of completed fertility. We estimate models of the form:

[36] Our results are not sensitive to small changes in the start and end date of the treatment cohort. We also experimented with alternative specifications within the 1946-1955 cohort window (like breaking it up into two windows – one from 1946-1950 and one from 1951-1955), but found no pattern strong enough to statistically distinguish.

$$CF_{cs} = \beta_1{*}REPEAL_s{*}D4655 + \beta_2{*}REPEAL_s{*}D(post55) \qquad (1)$$

$$+ \delta_s + \delta_c + \delta_s{*}TREND + \varepsilon_{cs}$$

where CF_{cs} represents alternative measures of completed fertility in cohort c in state s, D4655 is an indicator of the 1946-1955 birth cohorts and D(post55) is an indicator of post-1955 birth cohorts. We include state-specific (δ_s) and cohort-specific (δ_c) fixed effects to capture longstanding differences in fertility patterns across states over time as well as aggregate patterns of changing fertility preferences over time. We also allow the state-specific differences to trend over time. The omitted term here is the difference before 1946 between repeal and non-repeal states, which is captured by the set of state fixed effects. The coefficient β_1 measures the difference in completed fertility between repeal and non-repeal states for those women born in the 1946-1955 period (the treatment cohorts), relative to those born before 1946 (who were beyond prime childbearing age when abortion was legalized). The coefficient β_2 measures the difference in completed fertility between repeal and non-repeal states for the set of women born after 1955 (for whom there should be no effect since they were all subject to legal abortion throughout their childbearing years), relative to those born before 1946. As with the period tests of Levine et al., this framework provides two tests for the effect of abortion legalization: that β_1 is negative, and that β_2 is zero.

It is important to recognize the additional power that we obtain by including the interaction for the post-1955 cohort. Without it we would be unable to differentiate an early repeal effect from a differential trend by repeal versus non-repeal states. That is, the early repeal of abortion, followed by Roe v. Wade, allows researchers to go beyond the typical "difference-in-difference" setup, in which it is difficult to distinguish policy-induced changes from underlying trends. By finding *both* an effect on fertility for the treatment cohort and that there is no effect for later cohorts, we are able to confirm that there is a causal effect of abortion availability and not an underlying trend that differs between these sets of states.

There are two other important differences in this model relative to standard difference-in-difference models, like Levine et al. (1999), which rely on contemporaneous data. First, states no longer represent the state where the birth occurred since states are designed to identify the mother's state of residence in the early 1970s. As described below, however, it is impossible to determine states of residence in the early 1970s from the available data, so we use mothers' states of birth instead. Using state of birth has two advantages for our purposes: it is exogenous to any mobility decisions related to abortion availability[37]; and it is a fixed characteristic that allows us to merge the two data sources we use below. Second, we no longer are able to control for other time varying, state-specific covariates. In the cohort context, individuals are potentially influenced by some complex function of social conditions since they were born and it is unclear how to specifically control for them in a regression framework.[38] Instead, we rely on state of birth fixed effects along with linear trends within states of birth to control for these other factors.[39]

[37] Implicitly, we use repeal status of the state of birth to instrument for repeal status of the state a woman lived in during her childbearing years. If migration between repeal and non-repeal states increased over time, then state of birth would be a weaker instrument for later cohorts, causing our estimate of β_2 to be biased toward zero and overstating the evidence for our hypothesis. However, migration rates between early-legalizing and other states have not changed significantly over the period in question: in 1980, 90.02% of women of childbearing age were living in a state whose repeal status was the same as that of their state of birth; in 2000, the analogous proportion was 90.21%. Therefore legalization timing in a woman's state of birth appears to be an equally strong instrument for exposure to legalization in older and younger cohorts.

[38] One type of time varying, state-specific covariate that is more easily incorporated into this framework is exposure to other relevant social policies during women's childbearing years. We experimented with two such variables: number of years of exposure to legal access to contraception between the ages of 18 and 20 (derived from the data in Bailey, 2006) and the number of years of exposure to Medicaid funding of abortion (derived from the data in Levine, 2004). These policies both varied over states and time, so women in different state/year of birth cohorts would have different exposure to these policies over their childbearing years. Since these policies are also related to abortion access, they have the potential to bias our results. Nevertheless, when we included these additional variables neither were statistically significant and including them had very little impact on the estimated repeal/year interactions. As a result, we chose to report the results omitting these variables.

[39] The findings reported later in the paper are similar when we control for state-specific quadratic trends.

In addition to estimating models of this form, we also follow Levine et al. and estimate models comparing completed fertility to women in repeal states to women in non-repeal states who differ in their distance to repeal states. If travel between states occurred in response to the early legalization of abortion in repeal states, then the estimated impacts should be greater when comparing repeal states to more distant non-repeal states.

IV. Description of the Data

Based on this discussion, we need data providing measures of women's completed fertility for different birth cohorts of women along with geographical identifiers that can isolate those women exposed to the early legalization of abortion in repeal states. No single dataset provides all of this information. We rely on the Public Use Micro Sample from the 1970 Census and data from the Vital Statistics Natality Detail Files between 1968 and 1999 to construct measures of completed fertility for birth cohorts beginning in the 1930s and going through the 1960s. This combination provides a unique opportunity to examine the life-cycle fertility of the relevant treatment and control cohorts.

The Vital Statistics data represent individual records on births that took place in the United States between 1968 and 1999.[40] Microdata prior to 1968 are not publicly available. These data are invaluable because they specify the parity of the birth, the mother's age (from which we can estimate the year she was born), and the mother's state of birth.[41] With these data,

[40] From 1985 onward, these data represent a complete count of births. Prior to 1972, births were sampled at a 50 percent rate nationwide. In the intervening period, some states sampled at a 50 percent rate and others included all births. In our analysis, we applied appropriate weights to provide estimates of all births.

[41] For a very small number of births, this information is missing. These births are not included in the analysis. These data also contain information regarding whether the birth was a singleton or part of a multiple birth. The issue of multiple births is an interesting one in the present context, because fertility treatments over time have increased the likelihood of multiple births and may have made it more difficult to achieve "optimal" family size if multiple births result from a pregnancy for which only one child was intended. To pursue this issue, we estimated rates of singleton births for 1971 and 1999 separately for repeal and non-repeal states. Our results indicate that

we can attribute every birth that takes place in the United States to the mothers' state/year of birth cohort.[42] Moreover, we can use the information available on the parity of each birth to help formulate the distribution of the number of births to each woman over her childbearing years.[43]

For those women born in the mid 1950s and later, these Vital Statistics microdata contain a record of every birth they have had. Since fertility trails off considerably after age 35 and is very low after age 40, birth records for the 1968 through 1999 period will capture most, if not all all births to women born as recently as the mid 1960s. Therefore, these data on their own are sufficient to measure completed (or almost completed) fertility for roughly a decade's worth of birth cohorts. But these data are inadequate for capturing completed fertility for women born before the mid 1950s since they will already have given birth to at least some of their children prior to 1968. If they give birth to additional children after 1968, we will see them in the Vital Statistics data from later years, but we would miss any women whose fertility is completed before 1968.

To count births prior to 1968 we use data from the 1970 Census, which provides information on both women's state of birth and their children ever born.[44] Based on these data, we can count children ever born who are more than two years old, and thus were born before 1968, by mothers' birth cohort and state. This becomes a baseline to which we can add information on later births from the Vital Statistics data. For example, consider the cohort of women born in 1940. We use the 1970 census data to measure how many children those women

singleton births have declined slightly over time (from 98 to 97 percent) and that there is no difference in these values between repeal and non-repeal states.

[42] Another minor limitation of these data is that births to women who were born in the United States but gave birth in another country would not be captured in these data. It is our impression that this is a very infrequent event and we ignore it here.

[43] Each birth at a given level of parity contributes to the counts in a cumulative birth distribution, which can then be converted to a probability distribution.

[44] The Census data we use come from the standardized Minnesota Integrated Public Use Microdata Series (Ruggles and Sobek 2003).

had through 1967. We then use the Vital Statistics data to track the additional births to this cohort that occurred in 1968 or later. Since both sources of data are sorted by state of birth, we can be sure that we are tracking the same women.

We use these combined Census and Vital Statistics microdata to create measures of lifetime fertility for women born in the 1930s through the 1960s.[45] For the earliest of these cohorts, the Census data captures virtually all of their fertility.[46] For those born after the mid 1950s their fertility history is virtually entirely captured by the Vital Statistics microdata. For the intervening cohorts, these two data sources are combined to create complete fertility histories. Using the available data, we are able to construct measures of the number of children ever born by age 25, by age 30, by age 35, and then total completed fertility.[47] We convert counts of births by these ages to measures of children ever born per woman by incorporating estimates from the 1970 Census on the number of women in each state/year of birth cohort. In addition, we have constructed distributions of the number of children born to each woman in each state/year of birth cohort and focus, in particular, on the percentage of women who never have children.[48]

[45] Although the approach we use to construct these data provides what we believe is the best method available, it is not without limitations. First, the Vital Statistics data do not contain information on mother's state of birth in 1968, 1969, and 1972. To overcome this, we estimate the number of births in those years by mother's state of birth by using rates of migration between birth states and state of residence computed from the 1970, 1971, and 1973 data. A second limitation of the Census data is that it only provides information on an individual's age and quarter of birth, without specific birth dates. We employ an algorithm that assumes all responses were filed on April 1st to determine the number of children born since the beginning of 1968 and then take the difference between this and children ever born to determine the number born prior to 1968. These issues are described in greater detail in Ananat et al. (2004); we do not believe that they have any systematic effect on our results.

[46] For example, the 1933 birth cohort would have been 35 years old in 1968. Their completed fertility included an average of 3.17 children; 3.04 of them were born by age 35.

[47] Where sufficient data are available, we define total completed fertility to include births up to age 44. In some instances, however, we use births up to age 39 since so little fertility takes place after that. We have tested the sensitivity of our results to including births up to age 39 for all cohorts and found little difference to that reported here.

[48] All of these data can be constructed separately for white women and non-white women (blacks or other race) separately. We have done so and estimated all the models reported subsequently separately by race. Not surprisingly, results for white women look very similar to those for the whole population because whites represent a large majority of the population. Point estimates for non-whites were also similar, but they lacked the precision to be able to draw strong conclusions.

V. Results

Initial Evidence

Before presenting estimation results from equation (1) using our constructed data on completed fertility, we first present an analysis that relies exclusively on contemporaneous age-specific birth rates beginning in 1968 from the Vital Statistics microdata. We attribute births at each age in each year to the relevant birth cohort of the mother, defined as the year of birth minus mother's age. For example, we can construct a birth history for the 1959 birth cohort by using birth rates at age 15 from 1974, age 16 from 1975, age 17, from 1976, and so on. We then plot the percentage difference in age-specific birth rates between women in repeal states and other states for three selected cohorts. For the 1959 birth cohort, the differential timing across states in abortion legalization would have had no impact on the difference in birth rates since these women would not have reached their childbearing years until after Roe v. Wade. Therefore, this cohort can serve as a control group.

The results of this analysis are plotted in Figure 1 for the 1949, 1954, and 1959 birth cohorts. Women in repeal states are more likely to have their births at later ages, as indicated by the general upward slope of these lines. The treated cohorts are those born in 1954 and 1949, who would have been 17 and 22 years old in 1971, respectively, when abortion was legalized in the early repeal states. The trends in age-specific fertility for these cohorts clearly show the period effects that past research has found. Age-specific birth rates for women in repeal states in the 1954 birth cohort show a clear drop exactly at age 17 relative to women in non-repeal states and to other birth cohorts. This drop lasts for the few years in which abortion laws differed across states and then reverts back to the pattern for the control cohorts. Similarly, births to women in repeal states in the 1949 birth cohort fall exactly at age 22, which corresponds to their births in 1971, and remain lower just for the three years in which abortion policies differed across states.[49]

The advantage of this figure is that it enables us to compare births at later ages for these cohorts that were clearly contemporaneously affected by differential abortion access. The impression one would take away from this figure is that the contemporaneous reduction in births did not alter childbearing patterns at later ages. The age-profile in the difference in births for the 1954 cohort is no different after the early 20s from that of the control cohort. If anything, women in the 1949 birth cohorts from repeal states had fewer births over the remainder of their main childbearing years compared to women in other states and the control cohort. These results provide the first evidence that the reduction in period fertility attributable to abortion legalization may have been permanent.

Main Results

Figure 2 and the top panel of Table 1 present the main results of our cohort-based analysis examining the impact of abortion legalization on children ever born. Figure 2 focuses on children ever born throughout the childbearing years and Table 1 separately considers children ever born by age 25, 30, 35, and completed fertility (by age 44, or age 39 for cohorts who have not yet reached 44 in 1999).[50] As we described earlier, we are looking for a gap to

[49] Note that births for the 1949 cohort are only reported beginning at age 19, which corresponds to their fertility in 1968, the first year in which Vital Statistics microdata are available.

[50] All fertility information before 1968 comes from the 1970 Census, which only includes data on children ever born when the Census was completed. It is impossible to date when births occurred prior to that year in those data. This explains why data on births by age 25 and age 30 are only available as far back as the 1943 and 1938 birth cohorts, respectively. To maintain at least some balance to our sample, we do not use birth cohorts prior to 1933 since we would not be able to date births by age 35 prior to that year. At the other extreme, births by age 35 are only available through the 1964 birth cohort since our micro Vital Statistics data ends in 1999. In addition, children ever born represents births to age 44, except for the 1956 through 1960 birth cohorts in which births to age 39 are used since these birth cohorts had not yet reached age 44 by 1999. We have estimated all of our models using data for

emerge in the difference in children ever born between women in repeal and other states beginning around the 1946 birth cohorts that would then disappear for the post-1955 birth cohorts.

Figure 2 displays the difference in children ever born between women born in repeal states and other states over time. The results indicate a clear upward trend in this difference, as illustrated by the trend line added to the figure. Apparently, it used to be the case that women in non-repeal states had more children than women in repeal states, but this difference has been slowly going away. This highlights the need to consider the deviation from state-specific trends. Such a deviation from trend is clearly apparent for the relevant birth cohorts, those born between 1946 and 1955. For that set of cohorts, there is a sharp reduction in the differential of children ever born between repeal and non-repeal states.

This conclusion is confirmed in the top panel of Table 1. By the end of childbearing (the final column), we see that abortion legalization reduced children ever born by 0.054.[51] Moreover, the difference in children ever born that emerged for the 1946 to 1955 birth cohorts in repeal states was statistically eliminated in the post-1955 cohorts. The coefficient for that cohort is statistically and substantively insignificant. This further confirms the causal interpretation of our finding. The effects we estimate here are present only for the 1946-1955 cohorts, relative to both cohorts born before 1946 (the omitted group), and cohorts born after 1955; these are not just trends in differential state fertility behavior.

Evidence in the top panel of Table 1 also suggests that the similar reductions in fertility are observed by ages 25, 30, and 35. By each of those ages, we see that children ever born to

just the 1943 through 1960 birth cohorts, which provide relevant information for children ever born by each age (and assuming that "completed fertility" ends at age 39) and obtained results comparable to those reported below.

[51] To provide some perspective on the magnitude of this number, roughly 3 million women were born between 1946 and 1955 in repeal states. If the each had 0.054 fewer children, this would amount to about 150,000 fewer children.

women in repeal states had fallen by .07 relative to women in non-repeal states in the 1946 to 1955 birth cohorts. Importantly, this gap is statistically and substantively insignificant in the post 1955 cohorts (except, perhaps, by age 25). Although these point estimates for the 1946 to 1955 birth cohorts by age 35 are slightly larger than that by the end of childbearing, they are not statistically different. These results suggest that the reduction in births associated with abortion legalization in the early 1970s was essentially permanent. It does not appear that much, if any, of it was made up in the form of additional births at later ages.

As a rough assessment of the validity of these findings, we estimated models comparable to that in Levine et al. (1999) using age-specific birth rates estimated from 1968 to 1980 Vital Statististics microdata. We then applied the estimates of the impact of abortion legalization on these birth rates (a 6.7% reduction) to the 1970 age-specific birth rates to determine the steady state impact on children ever born. The results suggest that children ever born would have fallen by 0.043, which is comparable to (and not significantly different from) our estimate in Table 1 of 0.054 by the end of childbearing.

Distance Estimates

A means of confirming this striking finding is to take advantage of the fact that the non-repeal states are better "controls" the farther they were from the repeal states (due to the possibility of travel to the repeal states from nearby states). Following Levine et al., we can divide the non-repeal states into those within 250 miles, between 250 and 750 miles, and more than 750 miles from repeal states.[52] By doing so, we can assess whether our results are stronger for the farther away set of control states, as we would expect.

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[52] States in each category include:
Repeal: AK, CA, HI, NY, and WA
Distance < 250: AZ, CT, DE, DC, ME, MD, MA, MI, NV, NH, NJ, OH, OR, PA, RI, VT
Distance > 250 and < 750: CO, GA, ID, IL, IN, IA, KY, MN, MS, MT, NM, NC, SC, TN, UT, VA, WV, WI, WY

Figure 3 and the remainder of Table 1 report the results of our analysis, separately for control states of different distances to the repeal states. Figure 3 displays the difference in children ever born by the end of childbearing to women in repeal states relative to women in each of these distance categories by mother's birth cohort. Again, this figure shows that women in repeal states historically had fewer births, but it also shows that the difference is greater when they are compared to further away states. In each case the general trend is toward a narrowing of the gap. But for far away non-repeal states, another clear pattern is the break from trend that occurs in the difference in children ever born among cohorts born in the late 1940s and early 1950s. No such trend is apparent when comparing children ever born between repeal states and the nearest group of comparison states.

These assessments are confirmed in the bottom three panels of Table 1. By age 25, children ever born to women in repeal states born between 1946 and 1955 fell by an insignificant amount relative to women in the closest group of comparison states, by 0.085 children relative to the middle distance group, and by 0.099 children relative to the most distant group of comparison states. Moreover, the estimates are roughly stable throughout the remainder of the childbearing years. And, for all distances, the results are largely insignificant for the cohorts born after 1955.[53] These results provide further evidence of a causal and largely permanent impact of abortion legalization on children ever born.

Distribution of Children Ever Born

We turn our attention now to the impact of abortion legalization on the distribution of children ever born rather than just the count. We pay particular attention to the percentage of

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Distance > 750: AL, AR, FL, KS, LA, MS, NE, ND, OK, SD, TX.

[53] The coefficient on the post-1955 interaction for the middle distance states is statistically significant, contrary to the model's predictions. In a table estimating this many coefficients, one might expect random variation to provide an occasional odd result and this would be our interpretation of this finding.

childless women, which is of primary importance since it is the only point in this distribution for which we have an unambiguous prediction; more women should remain childless. Since the remainder of the distribution should be shifting to the left, it is difficult to tell whether the flow out of a particular level should be greater than the flow in from a larger value (e.g. the share of women who only had one child will be falling as those who otherwise would have one child move to zero children, but it will also be rising as those who otherwise would have two children move to one child).

The main results of this analysis are reported in Figure 4 and in the top panel of Table 2. Figure 4 displays the difference in the percentage of childless women between repeal and non-repeal states by birth cohort. For all birth cohorts before the mid 1940s, this difference is reasonably stable. But by the mid 1940s we begin to see a sizeable jump in the number of childless women in repeal states relative to non-repeal states. The gap widens through the early 1950s and then recedes, just as we would predict if the cause of the gap was differential abortion access. By 1956, the difference is at the same level as before the "treatment" cohort began in 1946.

Table 2 presents a regression-based version of these results that also includes the percentage of women with different numbers of children. The results presented here confirm our observations from Figure 2 in that abortion legalization is estimated to have increased the number of childless women in the treatment cohort by 3.47 percent. Once again, there is no significant impact on the post-1955 cohort, consistent with the causal interpretation of this finding.[54]

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[54] Another interesting simulation estimates the number of children that those who became childless would have had if abortion were not legalized. If the number of women having children fell by 3.47 percentage points and this reduction led to a reduction in children ever born by 0.054, then it must have been the case that these women who remain childless otherwise would have had .054/.0347 = 1.57 children if abortion remained illegal.

The evidence regarding the impact at other points in this distribution is not that strong. Although we see a reduction in the number of women with one child and two children in the 1946 to 1955 repeal cohorts, a similar reduction is also observed for the later birth cohorts in those states. Therefore, it is difficult to attribute this change to abortion legalization itself as compared to some other ongoing trend that differed across states in the later birth cohorts.

Figure 5 and the remainder of Table 2 conduct the analogous exercises differentiating comparison states by their distance to repeal states. In Figure 5 we see that the difference in the percentage of childless women between repeal states and the closest set of non-repeal states was stable across birth cohorts. But as the distance increases across comparison states, we see the gap in the percentage of childless women for the late 1940s and early 1960s birth cohorts grow.

The bottom three panels of Table 2 confirm these impressions. Relative to the closest group of comparison states, there was no statistically significant increase in the percentage of childless women among women born in repeal states between 1946 and 1955. Relative to the middle distance and farthest distance groups, however, these women are estimated to be 3.89 percent and 5.45 percent more likely to be childless. The pattern by repeal status, birth cohort, and distance to repeal states provides strong evidence that this finding is causally related to abortion legalization.

Discussion and Implications of Lifecycle Fertility Results

The results from this analysis tell a very consistent story: the early availability of abortion in the repeal states led to a decline not only in contemporaneous but in permanent fertility. This reduction in lifetime fertility is largely attributable to an increase in the number of women who complete their fertile years without having any children. This finding is clear both graphically and statistically. And it is confirmed both by the absence of any differential effect for cohorts born after 1955, and by the fact that the effect is much stronger the farther away the control states were from the early repeal states.

These results are also substantively large. The 1945 birth cohort, which had already passed through its peak childbearing years by the time abortion was legalized, averaged about 2.4 births per women; about 11.3 percent had no children at all.[55] By the 1960 birth cohort, which was fully exposed to legalized abortion in all states at the time they hit childbearing age, each woman had 1.9 children on average; 18.2 percent had no children. Our overall estimates imply that abortion availability lowered the number of children ever born by 0.054, and the odds of being childless by 3.6 percent. These figures account for 11 and 52 percent of the time series changes in these variables.

Moreover, this calculation likely provides a lower bound of the effect of legalized abortion because it does not account for the fact that some women in non-repeal states traveled to repeal states prior to the Roe decision. Using our estimates based on the control states farthest from the early repeal states, we would be able to explain 17 and 79 percent of the time series change in these variables.

Other Outcomes for Women

The fact that abortion availability had such a fundamental impact on the lifecycle fertility patterns of women raises the issue of whether legalization had impacts on other aspects of these women's lives as well. Women who reduced the total number of children they had or chose never to have any at all may have invested in more education and worked more, which would have led to higher wages. Their marital decisions may have been altered as well. Changes in

[55] This estimate is based on the data employed in this project. Estimates based on data from Current Population Fertility Supplements are more like 14 percent.

both earnings and family composition effects also may have affected their poverty rates and welfare participation.

We attempted to investigate this by looking at measures of these outcomes available in the decennial Census. In order to focus on the full effects of reduced fertility, we looked at outcomes for women aged 35 to 44, for whom most childbearing is complete.[56] We observed the outcomes of the various cohorts of interest at these ages by pooling multiple Censuses. Observations for women born between 1930 and 1935 come from the 1970 Census, so that the outcomes of those born in 1930, for example, are measured at age 40, while the outcomes of the 1935 cohort are measured at age 35. For the 1936 to 1945 cohorts, observations come from the 1980 Census; for the 1946 to 1955 cohorts, from the 1990 Census; and for the 1956 to 1960 cohorts, from the 2000 Census.

We estimated models comparable in format to those reported earlier regarding children ever born to assess the impact of abortion legalization on women's outcomes, categorizing women according to state and year of birth and then examining the patterns in outcomes over time between women born in repeal and non-repeal states. The outcomes examined include: the probability of having completed high school or having completed college; current labor force participation; poverty status, and household income as a percent of the poverty line. Using these samples, we were unable to identify effects of increased fertility control on any of these outcomes, but our results were not sufficiently precise to rule out such effects. Apparently, the availability of virtually universal Vital Statistics data on fertility provided statistical power in our earlier analysis that we were unable to attain relying exclusively on the 5 percent samples (or, in the 1970 Census, a 4 percent sample) available in the Census data.

[56] Ideally, we would focus on a somewhat older age range, but the youngest of treated women, who were 15 in 1970, had only reached age 45 by 2000.

VI. Conclusions

It is clear from past research that the legalization of abortion in the U.S. induced a major change in fertility. Estimates from past research, replicated here, suggest that total fertility fell by roughly 5 percent when abortion was legalized. Previous research on fertility has emphasized the importance of decomposing fertility changes into temporary fertility (timing) effects and permanent (completed fertility) effects. Yet this has not been done for this critical change in the nature of birth planning.

In this paper, we directly investigate the impact of abortion legalization on the lifecycle pattern of fertility of cohorts affected by legalization. The power of our analysis derives from the observation that abortion legalization had its primary effects on those aged 16 to 26; for younger and older ages, there was either a sufficiently small effect of abortion access, or sufficiently low baseline fertility. Those ages 16 to 26 in the early 1970s, when abortion laws differed across states, were mainly in the cohorts born between 1946 and 1955. Thus, by comparing differences in children ever born between women in early repeal states to women in other states during the 1946 to 1955 window relative to the analogous difference for women born before and after those years, we can provide a causal estimate of the impact of abortion legalization on lifetime fertility.

Our results are striking. It appears that the response of fertility to legalization was largely composed of reductions in completed fertility, not changes in timing. Moreover, abortion legalization is found to have led to a very large rise in childless women in these treatment cohorts. The causal nature of our findings is confirmed by the comparison to other cohorts surrounding the treatment cohort, and by the fact that the relationship is strongest relative to the most distant states whose residents could not benefit from early legalization.

There are several important implications of these findings. First, they suggest that improved fertility control does not simply lead to changes in fertility timing, as suggested by some other papers. Indeed, the majority of the effect of abortion legalization was permanent and not attributable to timing effects. Second, they suggest that the availability of abortion can explain a sizeable share of the time series trend in reduced completed fertility and in childless women over these birth cohorts. Finally, there are potentially important implications for the long-term well-being of these cohorts of mothers from the improved fertility control. We were unable to draw strong conclusions about these implications from the available data, but this is a worthy goal for future research.

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Table 1: Estimated Impact of Abortion Legalization on Children Ever Born, by Age

	Children Ever Born by: Age 25	Age 30	Age 35	End of Childbearing
Mean of Dependent Variable (all states)	1.10	1.73	2.16	2.48
Repeal States Relative to All Non-Repeal States				
Repeal*1946-1955 birth cohorts	-0.068	-0.070	-0.070	-0.054
	(0.012)	(0.018)	(0.023)	(0.012)
Repeal*post-1955 birth cohorts	-0.030	0.006	0.002	-0.002
	(0.018)	(0.030)	(0.029)	(0.018)
Sample Size (birth cohorts*states)	1,377	1,632	1,632	1,377
Repeal States Relative to Non-Repeal States within 250 Miles of a Repeal State				
Repeal*1946-1955 birth cohorts	-0.030	-0.010	-0.007	-0.007
	(0.019)	(0.022)	(0.030)	(0.022)
Repeal*post-1955 birth cohorts	-0.015	0.017	0.006	-0.009
	(0.022)	(0.032)	(0.038)	(0.026)
Sample Size (birth cohorts*states)	567	672	672	567
Repeal States Relative to Non-Repeal States between 250 and 750 Miles from a Repeal State				
Repeal*1946-1955 birth cohorts	-0.085	-0.098	-0.095	-0.073
	(0.016)	(0.021)	(0.025)	(0.013)
Repeal*post-1955 birth cohorts	-0.042	-0.005	-0.006	-0.008
	(0.021)	(0.033)	(0.035)	(0.027)
Sample Size (birth cohorts*states)	648	768	768	648
Repeal States Relative to Non-Repeal States greater than 750 Miles from a Repeal State				
Repeal*1945-1955 birth cohorts	-0.093	-0.108	-0.114	-0.087
	(0.023)	(0.030)	(0.026)	(0.019)
Repeal*post-1955 birth cohorts	-0.028	0.009	0.016	0.028
	(0.032)	(0.047)	(0.039)	(0.033)
Sample Size (birth cohorts*states)	432	512	512	432

Notes: The dependent variable in each regression represents the average children ever born to women by each age. The "end of childbearing" is defined to be the oldest age for which births occur. For younger cohorts additional births are likely to result after the period for which data is available here. To address this, I only include the 1960 and earlier birth cohorts whose fertility at least through age 39 is recorded. Since so few births occur after that, I define lifetime births to equal the births that have occurred through the last age observed. All specifications include state and cohort-specific fixed effects along with state-specific trends. Standard errors are corrected for heteroskedasticity and an arbitrary covariance structure within birth states (i.e. clustered on state of birth).

Table 2: Estimated Impact of Abortion Legalization on the Distribution of Children Ever Born (standard errors in parentheses)

	Number of Children Ever Born:				
	No Children	1 Child	2 Children	3 Children	4+ Children
Mean of Dependent Variable (all states)	12.7%	15.1%	32.0%	21.1%	19.2%
Repeal States Relative to All Non-Repeal States					
Repeal*1946-1955 birth cohorts	3.47	-1.67	-1.81	-0.71	0.72
	(1.25)	(1.00)	(0.43)	(0.49)	(0.36)
Repeal*post-1955 birth cohorts	-0.25	-1.08	-1.42	1.43	1.32
	(0.55)	(0.32)	(0.38)	(0.50)	(0.79)
Sample Size (birth cohorts*states)	1,581	1,581	1,581	1,581	1,581
Repeal States Relative to Non-Repeal States within 250 Miles of a Repeal State					
Repeal*1946-1955 birth cohorts	1.55	-1.21	-1.65	0.34	0.97
	(1.51)	(1.09)	(0.48)	(0.57)	(0.44)
Repeal*post-1955 birth cohorts	-0.07	-1.09	-1.35	1.89	0.56
	(0.78)	(0.44)	(0.55)	(0.55)	(0.90)
Sample Size (birth cohorts*states)	651	651	651	651	651
Repeal States Relative to Non-Repeal States between 250 and 750 Miles from a Repeal State					
Repeal*1946-1955 birth cohorts	3.89	-1.75	-1.64	-1.00	0.50
	(1.33)	(1.07)	(0.55)	(0.50)	(0.46)
Repeal*post-1955 birth cohorts	-0.45	-1.03	-1.15	1.43	1.20
	(0.75)	(0.56)	(0.58)	(0.50)	(1.06)
Sample Size (birth cohorts*states)	744	744	744	744	744
Repeal States Relative to Non-Repeal States greater than 750 Miles from a Repeal State					
Repeal*1946-1955 birth cohorts	5.45	-2.23	-2.44	-1.57	0.79
	(1.42)	(1.07)	(0.58)	(0.52)	(0.43)
Repeal*post-1955 birth cohorts	-0.53	-1.22	-2.14	1.05	2.83
	(0.99)	(0.54)	(0.49)	(0.58)	(0.92)
Sample Size (birth cohorts*states)	496	496	496	496	496

Notes: The dependent variable in each regression represents the percentage of women who have no children, 1 child, ..., or 4 or more children by the end of childbearing. Data represents 1930 to 1960 birth cohorts. All specifications include state and cohort-specific fixed effects along with state-specific trends. Standard errors are corrected for heteroskedasticity and an arbitrary covariance structure within birth states (i.e. clustered on state of birth).

TABLE 3: REGRESSION RESULTS FOR OTHER OUTCOMES
(standard errors in parentheses)

	Employed	High School Graduate	College Graduate	Poverty Status	Income-to-Needs Ratio
Repeal States Relative to All Non-Repeal States					
Mean of Dependent Variable	66.3%	38.9%	18.8%	10.3%	311%
Repeal*1946-1955 birth cohorts	-2.391	-1.584	-0.109	-0.943	7.526
	(0.851)	(1.202)	(0.576)	(0.785)	(9.430)
Repeal*post-1955 birth cohorts	-3.997	1.130	-1.263	-0.360	0.545
	(1.363)	(1.714)	(1.250)	(1.334)	(11.189)

Notes: Data represents 1930 to 1960 birth cohorts. All specifications include state and cohort-specific fixed effects along with state-specific trends. Standard errors are corrected for heteroskedasticity and an arbitrary covariance structure within birth states (i.e. clustered on state of birth).

Figure 1: Percentage Difference in Birth Rates between Repeal and Non-Repeal States

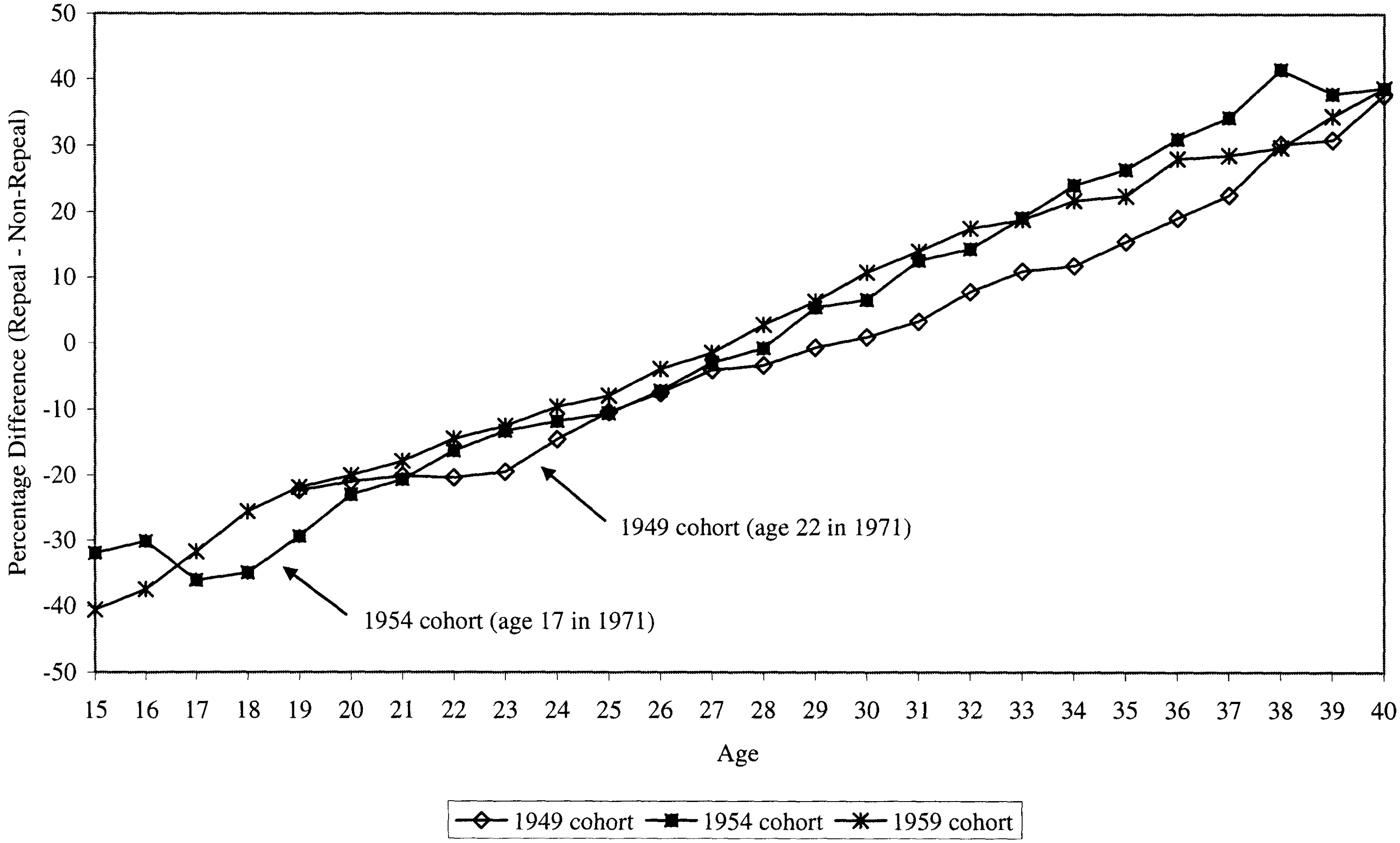

Figure 2: Effect of Abortion Legalization on Children Ever Born

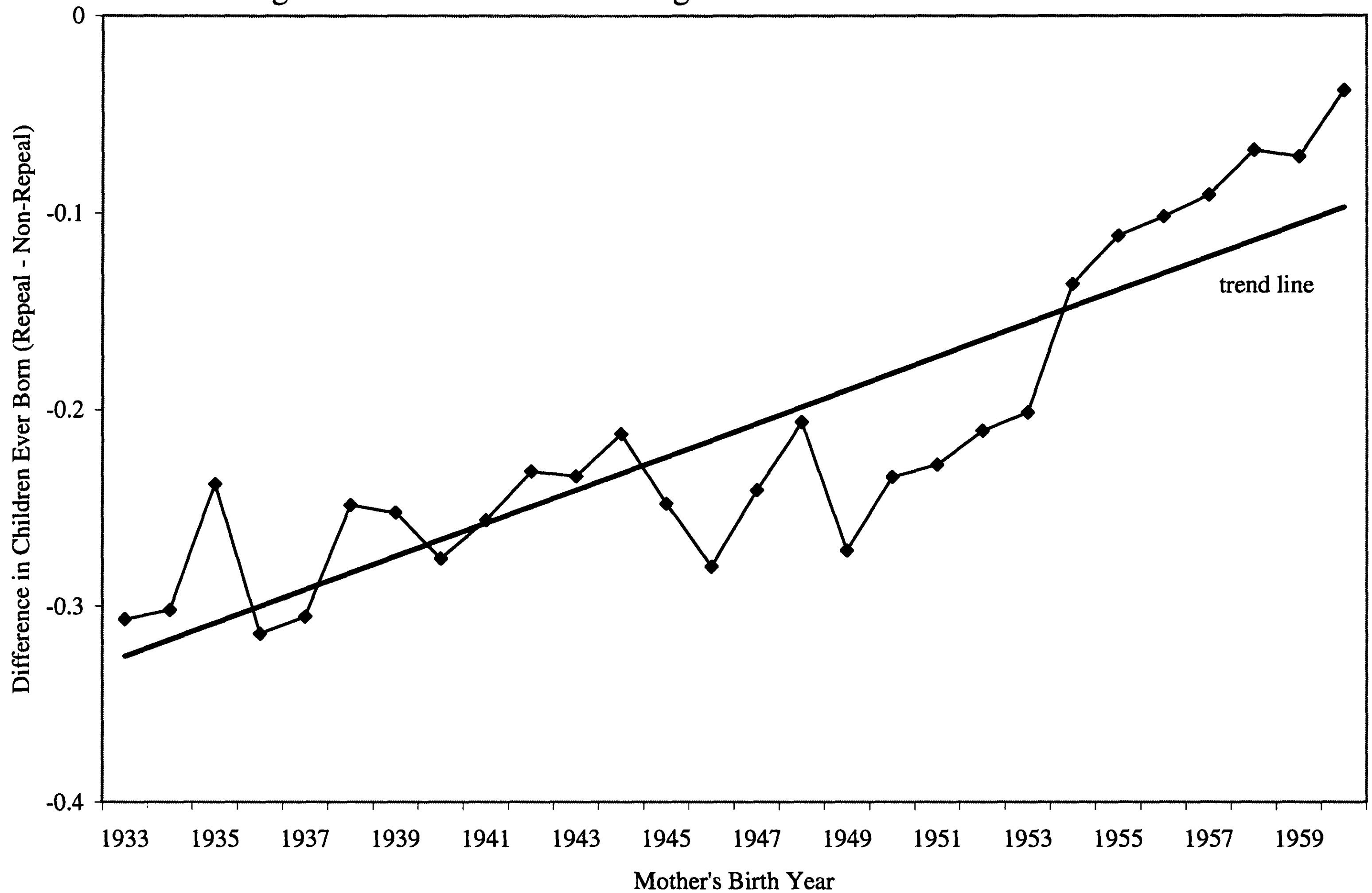

Figure 3: Impact of Abortion Legalization on Children Ever Born, by Distance from Repeal State

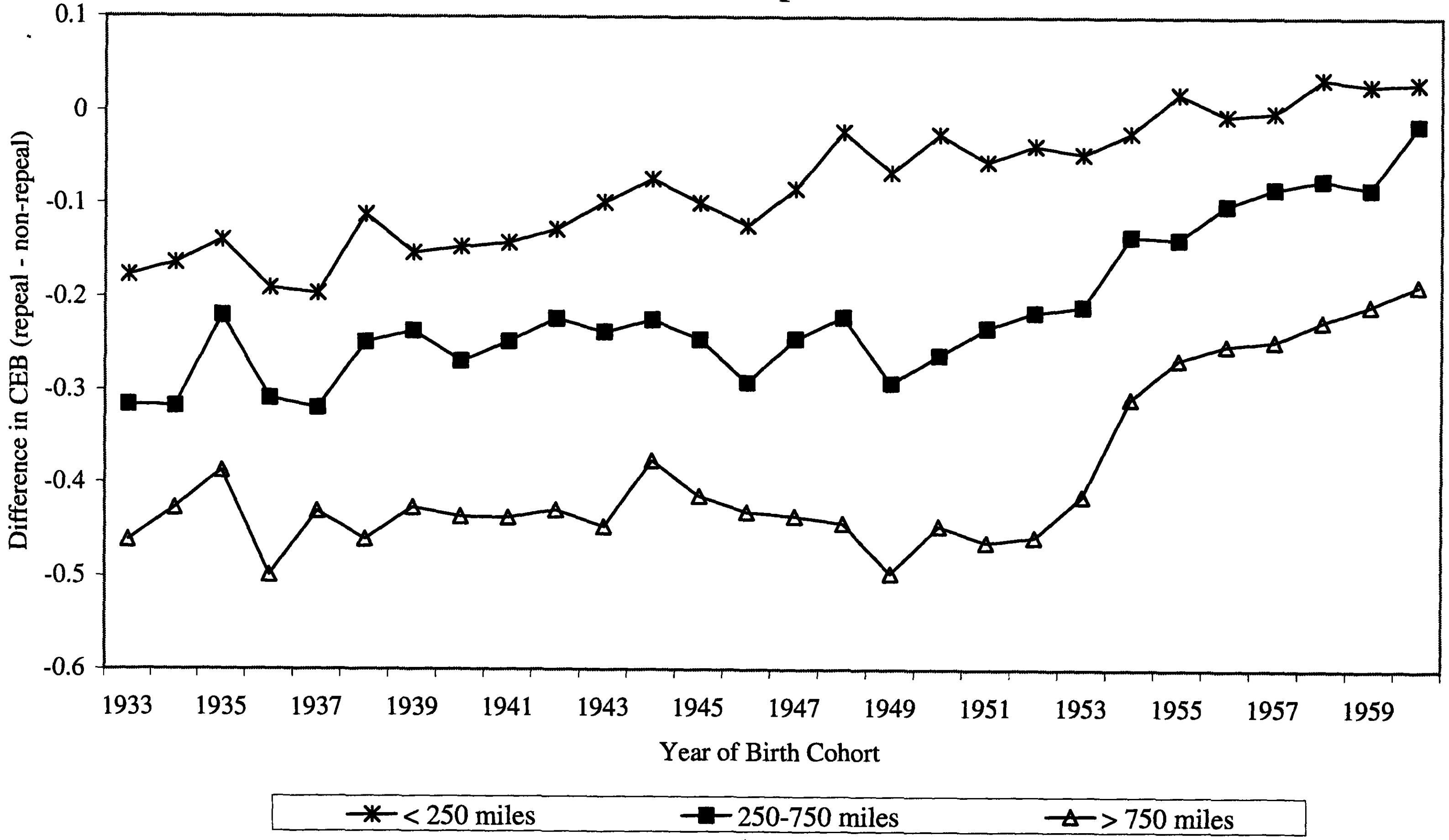

Figure 4: Difference in Percentage of Childless Women, by Birth Cohort

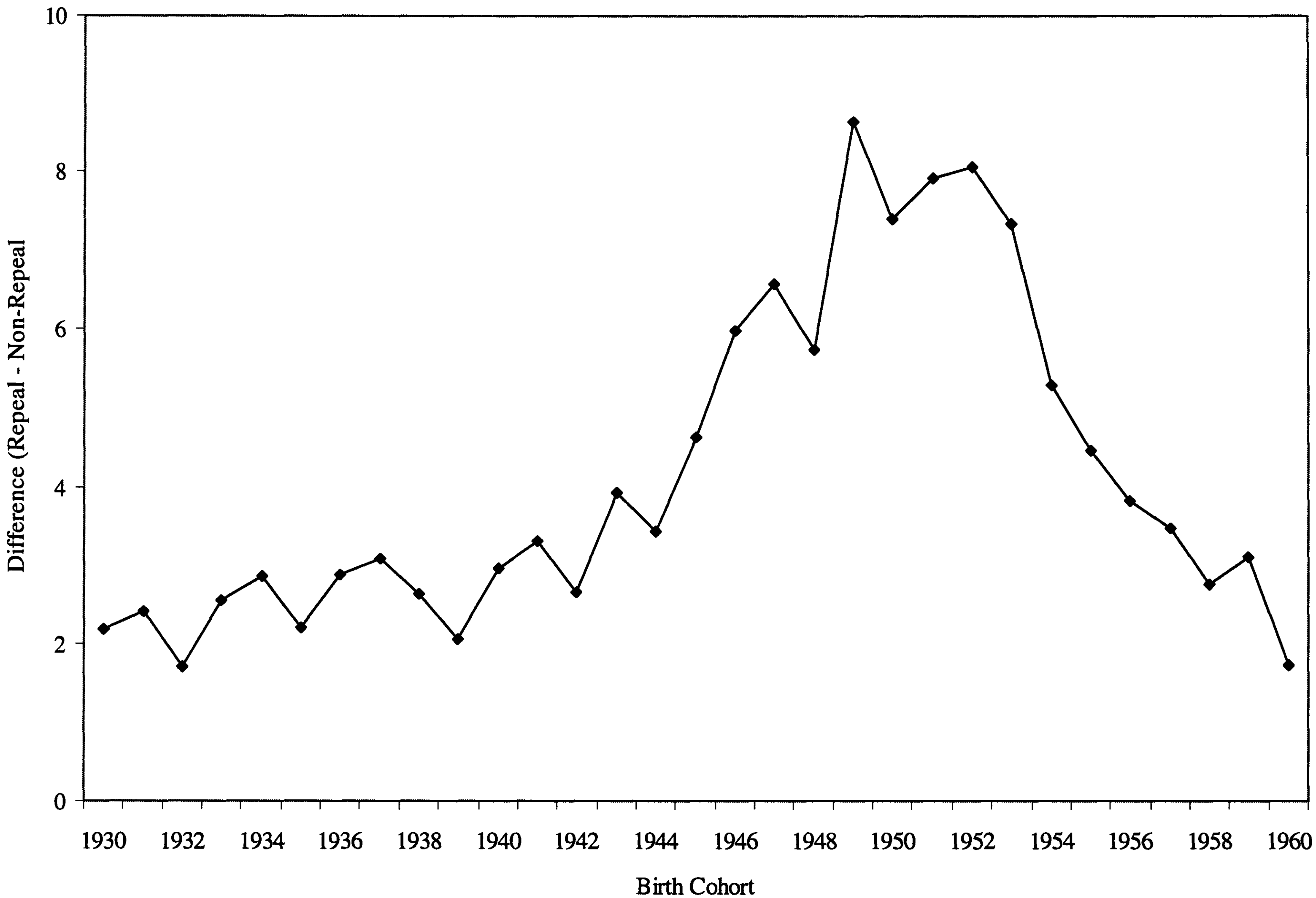

Figure 5: Impact of Abortion Legalization on the
Percentage of Women with No Children, by Distance from Repeal State