CHANGES IN FAMILY STRUCTURE AND WELFARE PARTICIPATION SINCE THE 1960S: THE ROLE OF LEGAL SERVICES*

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Abstract

This paper evaluates the effects of the War on Poverty's Legal Services Program (LSP) on family structure and welfare participation. LSPs provided subsidized legal assistance to poor communities, focusing on divorce and welfare access. We use a difference-in-differences research design based on the rollout of the program to 251 counties from 1965 to 1975. We find temporary increases in divorce and persistent increases in welfare participation and nonmarital birth rates. Nonmarital births rose because marriage rates fell, not because birth rates rose. Expanded access to legal institutions thus contributed, directly and indirectly, to changes in family structure in the 1960s.

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American families changed suddenly and dramatically in the 1960s. Marriage rates fell while divorces and nonmarital births increased (Lundberg and Pollak 2007). The share of mothers who were not married quadrupled between 1960 and 2010 (Figure 1). At the same time, married women's employment and unmarried women's welfare participation skyrocketed (Moffitt 1987, Goldin 2006). By 1980, mothers brought in one-third of family income, double their share in 1960. In 1991, Gary Becker reflected that "the family in the Western world has been radically altered—some claim almost destroyed—by the events of the last three decades" (Becker 1991, p. 1).

Understanding what caused these changes, however, has proven difficult.[1] The range of explanations include a lack of marriageable men (Wilson 1987), intergenerational effects of a "matriarchal" family structure (Moynihan 1965), contraceptive technology (Akerlof, Yellen, and Katz 1996), second-wave feminism (Chafetz 1995), and the growth of welfare programs (Murray 1984). Evidence on broad trends in family structure tends to be correlational, while causal evidence focuses on small interventions (e.g. Hannan, Tuma, and Groeneveld 1977) or local changes (e.g. Black, McKinnish, and Sanders 2003) that cannot explain family change on such a large scale.

This paper quantifies the role of an overlooked catalyst of shifts in family structure: an expansion in poor communities' access to the legal system brought about by the Neighborhood Legal Services Program (LSP). This understudied piece of the War on Poverty began in 1965 and tripled the availability of free civil legal consultation in poor areas (Brownell 1971, Subcommittee on Employment Manpower and Poverty 1970). LSPs handled individual disputes on issues like divorce, housing, debt collection, welfare, and employment; engaged in community outreach on

[1] Ellwood and Jencks (2004, p. 1) argue that "is only a slight exaggeration to say that quantitative social scientists' main contribution to our understanding of this change has been to show that *nothing* caused single-parent families to become more common."

policing issues and economic empowerment; and sued local bureaucracies perceived as treating the poor unfairly (Johnson 1977). Its originators believed that by translating poor people's demands into effective legal action, the LSP "would possibly be the single most important thing…in the poverty program" (OEO Assistant General Counsel Stephen Pollak quoting Sargent Shriver in Gillette 1996).

Embodying the rights-based legal movement of the 1960s, LSPs worked to improve poor people's access to government services, protections, and institutions, many of which matter for decisions about family structure. LSPs directly served thousands of families in divorce cases and in disputes with local welfare departments. One-fifth of the 282,000 cases handled by LSP attorneys in 1968 were for divorce (Levitan 1969). LSP lawyers routinely consulted with families about welfare rules and represented them in administrative appeals over benefit reductions or terminations.[2] They also indirectly expanded welfare access by working with welfare advocacy groups, writing plain language "welfare manuals" urging poor families to apply for benefits (Davis 1993), and suing local welfare departments over eligibility restrictions. LSP advocacy and litigation created a plausibly permanent shift in expected public benefits even for those not directly served by LSPs, changing the financial incentive to form single-parent families. In short, by making poor people's theoretical legal access a practical reality, LSPs had the potential to move communities to a new equilibrium in terms of welfare take-up and family formation.

[2] Sar Levitan (1969, p. 187) argued that "the indirect impact of the Legal Services Program should not be minimized…Administrative agencies, too, may treat their clients in less arbitrary fashion and not view welfare recipients as passive wards of the state." Gilbert Steiner, then director of Governmental Studies at the Brookings Institution, noted that without the LSP "there would have been no expansion of public assistance in the 1960s, just as there had been none in the 1940s and 1950s" (Ginzberg and Solow 1974, p. 65).

To identify LSPs' community-level effects, we use a difference-in-differences (DD) research design and a semiparametric event-study specification (Jacobson, LaLonde, and Sullivan 1993) based on the program's rollout to 251 counties between 1965 and 1975 (Cunningham 2016). We digitized outcome data on divorces, marriages, nonmarital births, and welfare participation by county from 1960 to 1988. Because the LSP rollout was concentrated within 5 years, we primarily use two complementary methods to find valid control counties. One specification includes state-by-year and urban-group-by-year fixed effects to create a control group of similarly urbanized counties without LSPs in the same state (Bailey and Goodman-Bacon 2015, Cunningham 2016). The second specification weights non-LSP counties by inverse propensity scores to create a control group with similar pretreatment characteristics (Abadie 2005). The results from both approaches are quite close, which supports the validity rollout design.[3]

Our estimates suggest that the LSP played a central role in the rapidly changing family structure and welfare participation of the 1960s, 1970s, and 1980s (see Figure 1 and Table 1). We find no evidence that these outcomes changed differently in LSP versus non-LSP counties in the years before the program began. LSP establishment, however, is associated with short-run increases in divorce rates, and persistent increases in participation in the receipt of welfare payments for single parents (Aid to Families with Dependent Children; AFDC) and nonmarital births. Nonmarital births rise because women forego marriages, not because births rise, and we view increased access to welfare programs that target single parents as the most likely mechanism.[4]

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[3] Fixed effects estimates are generally smaller than reweighted estimates, consistent with factors like state-level policies or court decisions that make nearby counties more comparable than observationally similar but far away ones.
[4] We also take steps to rule out several alternative explanations. Neither male/female sex ratios, economic conditions, nor other local War on Poverty initiatives changed coincidentally with LSP introduction. We find similar effects when we restrict comparisons to counties treated at different times or to counties that received a broader measure of War on Poverty funding. Our effects are similar in counties that did and did not experience riots (Cunningham and Gillezeau

Evidence from Census data supports the county-level findings, verifies that living arrangements changed, and shows that, as expected, the effects are driven by mothers with less education.

Our results are both plausible and meaningful. They match historical accounts of LSPs' aggressiveness on divorce and welfare questions and their magnitudes are well within the reported size of LSP workloads. We find that the LSP accounts for 9 to 15 percent of the growth in AFDC participation between 1964 and 1984 and 30 to 34 percent of the growth in nonmarital birth rates (in a sample of 112 large cities).

These results provide a new window onto old questions about families and the safety net. We provide some of the first plausibly causal evidence on the massive changes in family structure that occurred in the 1960s. Our results show that policy mattered in the 1960s: the War on Poverty contributed to increases in welfare use and nonmarital births. But these changes came from expanded legal access, not statutory changes in "generosity." The LSP facilitated access to a package of benefits that had long been available in theory but in practice was restricted to the "deserving poor" (Katz 1986). As the program's architects envisioned, LSPs broke down financial and institutional barriers to the legal system and safety net. What policymakers might not have foreseen is that this large shift in access would alter family structure as well.

I. THE NEIGHBORHOOD LEGAL SERVICES PROGRAM

American legal aid societies traditionally provided limited services on noncontroversial areas that would not alienate their philanthropic base.[5] By the 1960s, new advocates sought to expand legal

2018b)—a noted catalyst for urban decay and white flight (Collins and Margo 2007, 2004). We also estimate null placebo effects for Community Health Centers, a program that likely shares confounding unobservables with LSPs.

[5] American legal aid societies date to the settlement house movement. Energized by Great Britain's federally funded legal aid society, American legal aid grew in the 1950s. There were about 49 legal aid societies in 1949, and 236 by 1961 (Brownell 1971). These organizations provided limited services because they could not afford lengthy appeals and turned away controversial cases (bankruptcy, divorce, or challenges to corporations or government agencies).

services in both size and scope. In a 1964 *Yale Law Review* article, Jean and Edgar Cahn, attorneys with the Ford Foundation's Gray Areas program, proposed that university-affiliated neighborhood law firms should provide free civil legal representation and advice in poor communities and incorporate the "civilian perspective" into policy by supplying these communities with "the means with which to represent the felt needs of its members" (Cahn and Cahn 1964, p. 1334). In contrast, Ed Sparer, head of the legal unit for the influential juvenile delinquency program Mobilization for Youth, argued for a test-case approach based on "legal action that would create new legal rights for the poor" (quoted in Davis 1993, p. 33). Almost immediately after passage of the 1964 Economic Opportunity Act, Sargent Shriver, head of the Office of Economic Opportunity (OEO), made legal services a National Emphasis Program that would do both.[6]

LSPs sharply expanded the quantity of legal services available to the poor. The OEO prioritized the program's funding early in the War on Poverty, issuing more than 20 million (nominal) dollars in grants in 1965. By 1968 LSP spending had doubled, and Figure 2 shows that by 1975 LSPs had been rolled out to 273 counties in 48 states. Figure 3 plots the number of cases handled by traditional legal aid societies from 1905 to 1965, and by LSPs from 1967 to 1971. After 60 years in existence, aid societies handled about 300,000 cases a year. In contrast, the LSP handled 282,000 cases in 1968 and over a million by 1971. The average LSP center had five lawyers, each working hundreds of cases per year (Cunningham 2016).

[6] Sparer set up the Center for Social Welfare Policy and Law at Columbia University's School of Social Work in 1965 to provide backup support for test cases brought by LSP attorneys.

Rather than simply crowding out existing legal aid, LSPs provided *new* services.[7] The OEO-funded facilities served populations and handled cases such as divorce and bankruptcy that existing legal aid societies had been reluctant to take on (Cantrell 2003). Grantees opened new law offices in poor neighborhoods with expanded hours of operation to increase accessibility.[8] LSPs served poorer clients and a higher share of black women than traditional legal aid societies did, and challenged public officials more (Fisher and Ivie 1971). Silverstein (1967) concludes that LSPs "unquestionably had a liberalizing effect on both financial and subject-matter rules of eligibility."

Nearly 40 percent of LSP cases involved family problems like divorce, nonsupport, or paternity, where free legal assistance and court fee waivers represented meaningful savings to those served (Stumpf 1975). The median divorce in 1968 cost between $200 and $299 (University of Michigan Survey Research Center 1984). For comparison, the poverty line for a family of four in the early 1960s was about $3,000. Critics quickly accused the LSP of dissolving families. One judge called them "divorce mills" (Stumpf and Janowitz 1969) and coverage in the *New York Times* carried the subtitle "How to Get a Free Divorce" (Graham 1966). Supporters countered that the poor had the same right to obtain a divorce as the rich and that these efforts protected poor women's economic interests (Foster and Freed 1967).

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[7] Thanks to early endorsements from the American Bar Association and the American Trial Lawyers' Association, legal aid societies received about 40 percent of the initial grants in order to expand services or open new facilities. Law schools facilitated the rollout by providing cheap labor in the form of newly trained lawyers, designing new curricula in poverty law, and sometimes operating LSP offices directly (Johnson 2014, Cunningham 2016).

[8] The OEO argued that "accessibility has long been recognized to be a prerequisite of effective legal assistance. The impoverished are the least capable of traveling long distances to reach a lawyer. Even carfare may be beyond the means of a slumdweller in legal trouble. Equally important, studies have demonstrated that a psychological barrier exists between the inhabitants of a ghetto and the alien world of a bustling downtown area" (OEO 1966).

In addition to family law cases, about 7 percent of LSP cases challenged welfare bureaucracies. Their primary target was Aid to Families with Dependent Children (AFDC), the means-tested cash welfare program for single-parent families. State and federal governments financed AFDC but localities controlled almost all aspects of it. From its inception in 1935, local caseworkers exercised wide and often arbitrary discretion over case acceptances, benefit amounts, and case terminations (Bell 1965). This behavior stemmed from traditional notions of deservingness (Skocpol 1992), local labor demand conditions (Alston and Ferrie 1985), and racial discrimination (Quadagno 1994). LSP lawyers brought dozens of Supreme Court cases, including notable victories that struck down residency requirements (*Shapiro v. Thompson 1969*) and restrictions on cohabitation (*King v. Smith 1968*), and guaranteed a right to administrative appeals (*Goldberg v. Kelly 1970* and *Wheeler v. Montgomery 1970*).

Importantly, poverty lawyers also changed *local* public officials' actions toward the poor. They helped individual clients to fill out applications and frequently represented them in appeals ("fair hearings"; Hollingsworth 1977).[9] One welfare official testified in the Senate Appropriations committee in 1968 that "the OEO neighborhood legal attorneys are requesting more fair hearings…[and] if we rule in their favor they immediately want to go back and review every similar case" (quoted in Piven and Cloward 1971). LSPs also provided crucial expertise for the growing welfare rights movement, which had "no access to lawyers, at least until federal legal services grants were made available in 1966" (Davis 1993 p. 41). Welfare rights groups organized recipients to protest administrative decisions, petitioned for additional benefits, and disseminated "welfare manuals" that described regulations in simple language (see Online Appendix A).

[9] Fair hearings are formal challenges to administrative decisions about eligibility and benefits. LSP lawyers represented recipients at these hearings and encouraged them to file the appeals.

A. Expected Effects of LSPs

This history suggests that establishment of a neighborhood LSP should increase divorce rates, but only temporarily. For one, LSPs would initially meet "pent-up demand." Handler, Hollingsworth, and Erlanger (1978) document a fall in the share of time devoted to family law between 1967 and 1972 and credit the "backlog of families with marital problems waiting for legal services."[10] Second, a higher divorce *hazard* shrinks the at-risk population and, ultimately divorce *rates*. A constant shift in the hazard can even lead to negative estimated effects on divorce probabilities (Wu and Wen 2019). Heterogeneous marriage quality implies a temporary effect as well. If divorces occur among those marriages most likely to break up the quality of remaining marriage will rise, reducing observed divorce rates. Finally, all else equal, if cheaper divorce lowers the cost of a bad marriage, marriage rates may rise, boosting divorce flows in the long run.

We also expect LSPs to increase AFDC participation (a stock) substantially. By the early 1960s, local restrictions on eligibility and arbitrary caseworker decisions had become the norm in AFDC. Families likely did not respond to statutory changes in benefits because the probability that they could get and keep those benefits appeared low (Hoynes 1997, Moffitt 1994). The simplest connection between LSPs and AFDC is that they boosted acceptances and reduced terminations for individuals they represented. LSP advocacy and legal action against welfare offices, however, had the same effect for a much larger class of people. LSPs simultaneously increased the

[10] Coverage of one LSP in rural Wisconsin in its sixth week described this phenomenon: "Statistics released by the Judicare office here revealed that 84 percent of its cases so far have involved divorces…Judicare officials predict that its high ratio of divorce cases will go down as soon as the first rush for long-delayed divorces is over" (Graham 1966). Graham also cites a similar experience soon after legal services began in the UK: "When England started its program in 1950, 80 percent of the clients wanted divorces. Since then the rate of matrimonial disputes among English legal aid cases has declined to about 40 percent" See also the discussion in Wolfers (2006) in the context of unilateral divorce.

probability of receiving welfare conditional on applying (higher expected benefit) and reduced the costs of applying or appealing decisions (lower expected costs). Both should increase *take-up* among eligible families, even those not served by LSPs (Ashenfelter 1983, Moffitt 1983).[11]

Finally, many theoretical models predict that by making public assistance a reliable and available source of income, LSPs should have reduced marriage and increased divorce (Rosenzweig 1999, Willis 1999, Neal 2004). The key reason is that AFDC was almost exclusively available to *single*-parent families, and public benefits for non-disabled two-parent families were rare.[12] By expanding access to welfare benefits, LSPs increased expected public assistance income for poor women only if they raised children on their own. (Reductions in marriage also contribute to a "hump-shaped" divorce effect because never-married mothers cannot get divorced.) Unitary models imply an increase in single motherhood stemming from women's choices (Rosenzweig 1999, Neal 2004, Lundberg and Plotnick 1990), and bargaining models show how men can use the availability of welfare as a pretext for desertion (Willis 1999, Lundberg and Pollak 1996).[13] Growing prevalence of welfare participation, divorce, and single motherhood can also have feedback effects by changing their social costs (Solon et al. 1988, Bertrand, Luttmer, and Mullainathan 2000). In fact, Americans became much more accepting of these choices in the 1960s (Thornton 1989).

[11] Divorces also raise AFDC eligibility, and AFDC applications raise divorces because many states required women to file for a divorce in order to receive welfare benefits after being deserted by their husband (Finman 1971).
[12] The two programs that two-parent families with no disabilities could have received, General Assistance and AFDC for Unemployed Parents (AFDC-UP), each had between 50,000 and 60,000 cases through most of the 1960s. AFDC for single-parent families by contrast had 1 million cases in 1965 and 3 million by 1973. See section IV.E.
[13] Ethnographic studies support this. Stack (1974, p. 113) concludes that "couples rarely chance marriage unless a man has a job…women come to realize that welfare benefits and ties within kin networks provide greater security for them and their children."

II. DATA ON THE LEGAL SERVICES PROGRAM, FAMILY STRUCTURE, AND WELFARE

Existing datasets like the Census (only decadal data) or Panel Study of Income Dynamics (which began in 1968) have serious limitations for studying LSPs. To address this, we digitized new county-by-year data on family structure outcomes and welfare participation using a number of sources (see Online Appendix B for further details).

A. Treatment Variable: Legal Service Grants

Data on federal legal service grants funded by the OEO come from the National Archives Community Action Program files originally compiled in Cunningham (2016). We created an LSP treatment indicator equal to one starting in the year that counties received their *first* grant for "legal services." We only consider new LSPs established through 1975, when the program was reorganized under the Legal Services Corporation (LSC). We chose not to use a continuous measure of LSP funding to avoid bias from subsequent grant decisions that either supported successful centers or compensated for failing ones. Table 2 shows the starting year for the 251 sample counties that received LSP grants from 1965 to 1975.

B. Family Structure Flows: Divorce, Marriage, and Nonmarital Births

Data on the number of divorces and marriages that occurred in each county come from the 1959–1988 volumes of the Vital Statistics of the United States (DHEW various years). Our marriage and divorce outcomes are rates per 1,000 women ages 10–49 (flows).[14] Population denominators come from the 1960 Census (Haines and ICPSR 2010) and the Surveillance, Epidemiology, and End Results (SEER 2013) annual data, which begin in 1968 (interpolated between 1960 and 1968). We

[14] We use population counts down to age 10 because nonmarital births are recorded for women "under 15." We use this as the denominator for all outcomes to facilitate comparisons across results.

also digitized information on the number of births to unmarried residents of a subset of large counties (1960 population over 50,000, or 1970 population over 100,000). Our nonmarital birth outcomes are age-adjusted (using the national female distribution in 1960) nonmarital births per 1,000 women ages 10–49.[15]

C. Welfare Stocks: AFDC Cases

County-level data on caseloads and spending on AFDC come from federal reports published in 1960, 1964, 1966, and annually from 1968 to 1988. Reports after 1980 only include counties in Standard Metropolitan Statistical Areas (SMSAs). Our welfare outcome is AFDC cases per 1,000 women ages 10–49. Our divorce, marriage, nonmarital birth, and AFDC data are all new and the only source of local-level, high-frequency information on these outcomes during this time period.

D. 1960 and 1970 Census Data

We also use a sample of 623,175 mothers living in 81 counties identified in the 1960 and 1970 Census to calculate probabilities that mothers are unmarried heads of household; live with the father of their children; or are poor.[16] We collapse these outcomes to county-year averages and then take the 1960–1970 difference so that all Census-based models have 81 observations.

[15] Debates over family structure frequently focus on race. For example, Daniel Patrick Moynihan's 1965 report, "The Negro Family: A Case for National Action," both implicated racism in the prevalence of single-parent black families and argued, controversially (Rainwater and Yancey 1967), that it represented a "tangle of pathology" (Moynihan 1965). While black mothers were more likely than white mothers to be unmarried (28.9 percent versus 7.7 percent in 1960), they were also significantly poorer even conditional on marital status. The proportional growth in unmarried motherhood between 1960 and 1980, however, was actually larger for white women (97 percent) than black women (75 percent). Race-specific explanations for changing family structure in the 1960s are necessarily incomplete. Furthermore, we cannot measure county-level outcomes by race. Therefore, an analysis of LSPs' effect on race-specific family structure is beyond the scope of this paper.

[16] We also construct dummy variables that describe the cumulative distribution of unearned income, earned income, and income from other family members. Each dummy equals one for mothers who report income (by source) greater than or equal to x. We estimate effects on a series of dummies that move x from $0 to $100,000 in $2,000 increments ("distribution regression" Chernozhukov, Fernández-Val, and Melly 2013). We describe this method further below.

E. Estimation Samples

Because of differences in reporting across data sources, we use five estimation samples. From all US counties, we first exclude Alaska, Hawaii, and Nevada (where a disproportionate number of non-resident marriages and divorces occur). For Sample 1, our main sample, we then drop counties that failed to report marriages and divorces in every year from 1959 to 1988 (345 counties) or AFDC cases in every year available in federal reports from 1960 to 1980 (10 counties). This yields a balanced panel of marriage and divorce from 1960 to 1988 and AFDC in every *available* year from 1960 to 1980, comprising 2,683 counties that contain 94 percent of women in the US. Sample 2 includes the 603 SMSA counties with AFDC rates through 1988 (71 percent of US women), allowing us to observe counties up to 13 years after LSP establishment. Sample 3 includes the 112 counties with nonmarital births recorded in every year from 1959 to 1979 (23 percent of US women). Sample 4 includes the 60 counties that record nonmarital births from 1959 to 1988 (20 percent of US women). Lastly, Sample 5 includes the 81 Census counties (36 percent of women).

III. EMPIRICAL STRATEGY: DIFFERENCE-IN-DIFFERENCES AND THE ROLLOUT OF THE LEGAL SERVICES PROGRAM

We use a two-way fixed effects event-study specification to trace out changes in outcomes in each year around LSP establishment:

$$y_{ct} = \alpha_c + \alpha_t + \boldsymbol{\beta}'\boldsymbol{X}_{ct} + \sum_{j \in PRE} \pi_j 1\{t - t_c^* = j\} + \sum_{j \in POST} \phi_j 1\{t - t_c^* = j\} + \nu_{ct}. \qquad (1)$$

α_c and α_t are county and year fixed effects, and the $1\{t - t_c^* = j\}$ are event-study dummies that equal one if an observation is exactly j years from county c's LSP treatment date, and zero otherwise.[17] The first sum includes pre-LSP event-years so that the π_j coefficients capture pretreatment trends in outcome y_{ct}. The second sum includes post-LSP event-years. Because we observe data at the county level, the ϕ_j coefficients are intention-to-treat (ITT) estimates.

Identification comes from comparing LSP to non-LSP counties and comparing counties that received LSPs in different years. Table 2, however, reveals little variation in the timing of LSP treatment: 205 counties are treated in 1966 or 1967 and over 98 percent of the identifying variation comes from LSP/non-LSP comparisons (see theorem 1 in Goodman-Bacon 2018). Therefore, our identifying assumption is essentially that there are common trends in untreated potential outcomes between treated and untreated counties (rather than common trends between counties that received LSP funding in different years).[18]

The history of the War on Poverty, especially the initial stages during which most LSPs received grants, supports this assumption. The early OEO was a "wild sort of operation" that fielded proposals from "various and sundry groups" (Davis 1993, Johnson 1974, Gillette 1996). According to Earl Johnson, Jr., the director of the LSP from 1966 to 1968, "we were committed to building a national institution overnight and could not afford to screen grantees through a fine mesh" (Johnson 2014, p. 102). Regional offices, in consultation and conflict with OEO officials (Clark 2002), made LSP grants based "neither on demographic nor geographic considerations…[but] quite simply, on the desire to give out as much money as quickly as possible" (Goodman and Walker 1975, p. 7).

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[17] We omit the event-study dummy for the year before LSP treatment ($j = -1$) and include but do not report on dummies for unbalanced event-times.

[18] As discussed in Goodman-Bacon (2018), the identifying assumption aggregates pairwise common trends by the variance of a treatment dummy across treatment-timing groups (i.e., rows of Table 1). In practice, the variance weights on each treatment-timing group are almost identical to sample shares.

Still, funds went mainly to cities because of the OEO's focus on urban poverty (Bailey and Duquette 2014) and LSPs' need to hire trained lawyers . Columns 1 and 2 of Table 3 show strong pretreatment imbalance between LSP and non-LSP counties. Large cities dominate the treatment group, which has 9 percent of counties but over half of the 1960 population. Non-LSP counties are less urban, poorer; have lower levels of education and divorce, but higher AFDC and marriage rates.[19]

In response to this imbalance we take two complementary approaches to creating a valid control group. First, we estimate models that form the implicit control group using state-by-year fixed effects, which restrict comparisons to counties in the same state, and separate year fixed effects interacted with dummies for seven bins of the 1960 urban share, which restricts comparisons to similarly urban counties. This controls for the common effects of state-level policy changes (in, e.g., Medicaid, birth control access, or divorce reform) and changes that affect urban areas (e.g., white flight or the 1973 recession). Column 4 shows that fixed effects achieve balance in many characteristics (see *p*-values in column 5), while the detectable differences tend to be much smaller than in the unadjusted comparison. Taken together, observable characteristics do predict LSP treatment status, but the *F*-statistic is half its size in the unadjusted comparison (and falls by half if we omit the urban share).[20]

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[19] The first panel of Appendix Figure D8 plots unweighted mean outcomes over time for LSP and non-LSP counties. All series diverge starting in the mid-1960s even without any kind of adjustment.

[20] Section V also reports results from specifications that rely only on the timing of LSP establishment, drop the 1966 and 1967 counties, trim to ensure propensity score overlap, compare only to contiguous counties, drop counties that ever experienced a riot (or the largest increase in black population shares), control for other War on Poverty policies, and control for local welfare activism. We also show null placebo effects for Community Health Center establishment, a policy that we do not expect to affect family structure but which was funded by the same process as that of LSPs.

The second model creates a control group that is balanced on pretreatment characteristics.[21] We estimate propensity scores, $\hat{p}(x_c)$, from a probit model for the probability that counties receive an LSP grant as a function of all the covariates listed in Table 3, and we weight untreated counties in our regressions by $\frac{\hat{p}(x_c)}{1-\hat{p}(x_c)}$ (Abadie 2005, DiNardo, Fortin, and Lemieux 1996).[22] This creates a control group that is balanced on pretreatment characteristics but may compare areas from very different parts of the country (possibly with different locally enacted policies). Column 6 of Table 3 verifies that reweighting achieves balance jointly and for each covariate individually, except in the urban share, which differs by only 3.6 percentage points.

IV. Intention-to-Treat Estimates

Figures 4 through 8 plot pretreatment coefficients, π_j, and post-treatment coefficients, ϕ_j, from (1) using both reweighted and fixed effects estimators. Dashed lines are 95-percent pointwise confidence intervals from standard errors clustered by county. Table 4 presents grouped estimates for years 0–5 and 6–13, and *p*-values from 500 random permutations of the LSP treatment that hold the distribution of treatment dates constant.

A. Divorces

Figure 4 shows that divorce rates have a hump-shaped response to LSP establishment, rising for about four years then falling and returning to zero after about eight years. Consistent with our

[21] The second panel of Appendix Figure D8 plots mean outcomes over time applying the inverse propensity score weights to the control group.

[22] We recalculate propensity scores and weights for each estimation sample. Online Appendix Figure C6 shows the propensity score distributions, and Online Appendix Figure C7 scatters the propensity scores from different samples against each other. The inclusion of pretreatment levels and trends in outcomes is similar to the synthetic control estimator of Abadie, Diamond, and Hainmueller (2010). Goodman-Bacon (2018) points out that this strategy only affects comparisons between treated and untreated counties (and not between counties treated at different times), and also does not impose balance between untreated counties and each timing group. Most counties receive LSPs in 1966 or 1967, however, so this is not a major limitation.

identifying assumption, neither specification provides any evidence of differential pre-LSP trends. Divorce rates in treated counties only change *after* LSP begins.[23] Five years after LSP establishment, divorce rates in treated counties rose by between 0.5 and 0.7 per 1,000 women relative to untreated counties, or about 5.5 to 7.7 percent over the baseline mean of 9 divorces per 1,000 women. Table 4 reports average short-run increases in years 0–5 of 0.46 (se = 0.16, permutation p = .11) in the reweighted specification and 0.34 (se = 0.16, permutation p = .142) in the fixed effects specification. The estimates for years 6–13 are negative but not statistically significant, consistent with both an increase in the divorce hazard shrinking the pool of at-risk marriages, and dynamic selection as remaining marriages are those least likely to divorce.

Relative to poor women's divorce rates, these magnitudes are large but plausible. If all divorces came from the 15 percent of urban women who were poor in the 1960s, our largest event-study estimates suggest increases of at most 3.3 to 4.6 divorces per 1,000 *poor* women per year. Rescaling the event-study coefficients this way and summing them implies that in their first seven years of operation, LSPs could have led to an increase of between 1.7 and 2.5 percent in the number of poor women getting divorced. Between 1960 and 1970, the share of poor women who were currently divorced rose from 5.1 to 8.5 percent (and this understates the probability of ever divorcing, because some women remarry). These upper-bound calculations suggest an important role for LSPs in the short-run surge in divorces.

[23] The LSP effects do not appear in the first year (event year 0), partly because we do not distinguish *when* in a year LSP grants were made and also because LSP grantees had to hire staff, find volunteers, and build community support in order to be able to begin providing services. Finally, a few LSPs restricted the number of divorce cases handled or only took clients for divorce on certain days of the week, partly due to their perception of the social ills associated with female-headed households (Silver 1969, Pious 1971, Katz 1978, Hannon 1969).

LSPs could have easily handled this number of divorces. The average treated county in our sample had 122,000 women aged 10 to 49 when LSPs began operating, so the largest ITT estimates come from just 63 to 86 additional divorces per year. For reference, the average LSP lawyer handled 50 to 100 new cases each month. Summing this maximum number of additional divorces across 273 treated counties implies that at its peak, LSP caused 17,000 to 23,000 divorces per year, 30 to 40 percent of the 56,000 divorces they handled in 1968 (one-fifth of 282,000 cases; Levitan 1969). The concordance of these divorce results with LSPs' documented activities, LSP capacity, and historical reports of pent-up demand provides "first-stage"-type evidence that our identification strategy successfully picks up the effects of changing legal access brought on suddenly by LSP establishment.

B. Welfare Participation

Figure 5 shows that AFDC participation increased sharply after LSP establishment.[24] Again, neither specification nor different samples show evidence that these changes come from pre-existing trends. After LSP establishment, AFDC cases rise steadily and stabilize after 9 years. Table 4 reports longer-run effects (years 6–13) of 6.55 cases per 1,000 women (se = 1.53, permutation p = .000) in the fixed effects specification and 10.25 cases (se = 1.90, permutation p = .000) in the reweighted specification.[25]

[24] Because the AFDC data are not available annually in the 1960s, we group event years into $[-6,-3]$, $[-2,-1]$, $[0,1]$ and individual event-years thereafter.

[25] Note that these results only reflect LSPs *local* effects. Supreme Court victories striking down residency and cohabitation restrictions and guaranteeing fair hearings affected welfare eligibility everywhere, but our design necessarily differences out these effects. State-by-year fixed effects also capture LSP victories in state courts or through threat effects on regulations. Some states immediately changed rules and regulations to avoid going to court (Champagne 1974).

Table 5 puts these magnitudes in context by using the estimated effects to calculate counterfactual outcomes in 1984. In 1984, 56 women per 1,000 in treated counties received AFDC (column 1 row b), and the LSP treatment effects imply a counterfactual rate of between 44 and 49 women (column 1 rows d and e). This suggests that LSPs raised AFDC rates by between 7 and 12 women per 1,000, a 14 to 26 percent increase over the counterfactual (rows h and i). Scaling by the actual change in AFDC rates in treated counties from 1964 to 1984 (37 women per 1,000) suggests that LSPs explain 18 to 31 percent of the observed growth in treated counties (rows j and k). We repeat these calculations in column 2 for all counties in Sample 2 and find that LSPs could explain up to 15 percent of the growth in AFDC rates across the 603 SMSA counties.

As with the divorce results, rising AFDC rates track high-profile activities undertaken by LSP attorneys. Scaling the calculations in Table 5 to reflect counts shows that LSPs generated from 249,000 to 424,000 additional AFDC cases by 1984; 900 to 1,550 in the average treated county. These are plausible effects over 10 to 20 years. Fair hearings, a common way that attorneys helped clients remain on aid, rose from about 29,300 in the last six months of 1970 (the earliest data available) to more than 86,000 in the first six months of 1975 (National Center for Social Statistics 1976b). Applications grew from 230,000 in the first quarter of 1965 (Bureau of Public Assistance 1948-1970) to 665,000 in the first quarter of 1975 (National Center for Social Statistics 1976a). The AFDC estimates further supports the rollout design because they correspond closely with historical reports of LSP efforts in this area.

C. Nonmarital Births

Figure 6 shows that LSP establishment is also associated with sharp increases in nonmarital births. We find no evidence of differential trends in the 6 years before LSP establishment in either specification or sample. The pattern of ITT estimates is almost identical to the AFDC results in

Figure 5: after LSP establishment, nonmarital births grow at first but stabilize after 8 or 9 years. Columns 5 and 6 of Table 4 show short-run increases of 0.34 nonmarital births per 1,000 women per year (se = 0.15, permutation p = .018) in the fixed effects specification and 0.62 nonmarital births (se = 0.14, permutation p = .004) in the reweighted model; 6.5 and 12 percent, respectively, over the baseline mean of 5.2.[26]

Table 5 shows that these changes explain 36 to 41 percent of the growth in non-marital birth rates in the 50 treated counties observed through 1988, and 30 to 34 percent of the change in all 60 counties in the long sample. About 20 percent of all US births in 1965 were to residents of these counties. Therefore, our effects apply to a large population, but still a relatively small fraction of the country. If we observed all counties we would (mechanically) conclude that LSPs account for a much smaller share of the nationwide changes in nonmarital births.

Another way to gauge these magnitudes is to sum up the effects of LSPs on nonmarital birth flows and compare them to the effect of LSPs on AFDC stocks. The fixed effects specification shows an increase in nonmarital birth rates of 0.34 in years 0–5, which implies an aggregate increase of 2.04 nonmarital births per 1,000 women (6*0.34). The AFDC effect in event-year 5 is 5.7 cases per 1,000 women, implying that over this time frame new nonmarital births could account for at most 36 percent of new AFDC cases (the share is 53 percent in the reweighted specification). As we would expect given that LSPs both worked on behalf of current AFDC recipients and boosted take-up, only part of the LSP-induced growth in welfare cases comes from new nonmarital births.

[26] Online Appendix Figure D4 shows that almost all of the overall increase comes from younger women most likely to conceive children before ever marrying. For both teens and women in their 20s, nonmarital births rise by about 3 per 1,000 women 8 years after LSP establishment. To a lesser extent we find increasing nonmarital births among women ages 30–39, whose marital status may also have been affected by subsidized divorce.

D. Why did nonmarital births change?

For births to unmarried women to increase, either births per unmarried woman must rise or the number of marriages must fall. Figure 8 shows that both marriage and fertility fell after LSP establishment, suggesting that marriage behavior and not conceptions drives the increase in nonmarital births. The reduction in marriages mechanically explains why we find increasing nonmarital births: after LSPs begin operating, pregnant women forego marriage. Economic theories of compensatory behaviors in the face of risk provide a behavioral explanation. Prior to LSP establishment some women likely entered into undesirable marriages to avoid poverty, but after LSP improved the outside option to marriage (cash welfare) they no longer needed to. Figure 8 also shows why divorce point estimates eventually become negative. Fewer marriages immediately after LSP establishment mean that fewer divorces can occur later.

This matches nationwide results showing flat premarital conceptions for these cohorts but falling shotgun marriage rates (England, Wu, and Shafer 2013). Lower marriage rates also help explain falling fertility rates overall. In the 1960 Census, 12.7 percent of married women and 0.8 percent of unmarried women ages 10-49 had an infant. Using the Census sample (discussed below), Online Appendix Table E3 finds a reduction in the share of women married of 2 percentage points. This implies a reduction in fertility rates of 2.4 births per 1,000 women; close to the event-study estimates in Panel B of Figure 7. Consistent with this, Online Appendix Figure E4 shows that mothers without a high school degree are less likely to have more than one child.

E. Heterogeneity by State Divorce and Welfare Policy

If the short-run increase in divorces in Figure 4 was due to LSP attorneys, we would expect to find larger effects in states with legal environments that made it easier for poor families to initiate divorce proceedings and for LSPs to finish them. Table 6 reports results that split the sample into states that implemented no-fault divorce after (Panel A) or before (Panel B) 1970. We find much larger divorce effects in the early reform states. Consistent with welfare incentives (rather than divorces per se) as the main mechanism for the AFDC and nonmarital birth effects, we find very similar short-run changes in both outcomes in the earlier and later divorce reform states.

We also estimate separate results by whether or not states operated an AFDC-UP (Unemployed Parent) program that allowed married women to get welfare. Here we expect larger effects on AFDC in states with more expansive eligibility criteria because of the UP program, but smaller nonmarital birth effects because women did not need to divorce or forego marriage altogether to receive aid. This is exactly the pattern we find in panels C and D of Table 6: in states with an UP program by 1975, post-LSP changes in AFDC are twice as big while changes in nonmarital births are half as big as in non-UP states. While these differences are generally not statistically distinguishable, the patterns of heterogeneity conform to expectations about how LSPs interacted with different legal and policy environments.

F. Effects on Stock Measures of Family Structure and Household Income

Our county-by-year data allow inference about pre-trends and dynamic ITT effects, but not about living arrangements or other sources of household income. This section addresses these limitations using our Census sample.

Table 7 shows that the increase in nonmarital births does translate to changes in household structure. We find significant increases of between 1.4 and 1.8 percentage points in the probability that mothers were unmarried heads of household, and a nearly identical reduction in the probability that they lived with the father of their children. Consistent with LSPs' mission to serve poor communities, columns 2–4 show the largest effects for mothers with less than a high school degree (although they are present to some extent for all mothers in the reweighting model).

Figure 8 uses distribution regression to estimate post-LSP changes in the distribution of mother's income by source. While the 1960 Census does not record welfare income, we find clear increases in low levels of unearned income. This likely represents welfare income, however, because the pattern almost perfectly matches the annualized distribution of AFDC benefits from 1967 administrative data (see Online Appendix Figure E2) and is only present for unmarried mothers (see Online Appendix Figure E3). Earned income falls to some extent, although these findings are quite imprecise. Notably, other family income, which primarily consists of husband's earnings, falls at the low end of the distribution. This follows from the finding that fathers are less likely to be in the household.

The last row in each panel of Table 7 shows that the combination of falling income and falling household size leaves poverty rates unchanged. Overall, the Census results suggest that family structure did change in meaningful ways, but that poverty did not rise because reductions in father's income were matched by changes in household size and offset by welfare income.

V. RULING OUT ALTERNATIVE EXPLANATIONS

The evidence presented so far uses two distinct control groups and shows changes in outcomes that only occur after LSP establishment. Given the profound social changes that occurred in the 1960s, however, a number of threats to a causal interpretation of our results therefore remain. Figures 9, 10, and 11 plot estimates from a range of specifications that test alternative explanations for changes in divorce rates, AFDC participation, and nonmarital birth rates, respectively. For comparison, we reproduce the estimates from Table 4 in the first two rows of each figure.

A. Different Ways to Use LSP Timing

Since so many counties received LSP funding in 1966 or 1967, determinants of family structure that changed sharply in these years, such as cultural shifts that affected cities, could bias our estimates. Row 3 shows reweighted estimates that drop the 1966 and 1967 LSP counties and are identified by the 68 counties that introduced the LSP in other years. Standard errors increase substantially but the point estimates do not meaningfully change.

Both of our specifications would be biased if the OEO allocated LSP funding to places that experienced the upheaval of the 1960s differently. Row 4 presents estimates that form a control group of LSP counties treated in the future so that, if selection by the OEO signals a particular pattern for outcomes during the 1960s, comparisons between treated counties should yield no effects. We use the estimator proposed by Callaway and Sant'Anna (2018) and match counties treated in year t^* with a control group of counties treated in $t^* + 4$ or later, dropping years after the controls receive their LSP (also see Deshpande and Li 2017, Fadlon and Nielsen 2015). We estimate short-run DD coefficients for each set of counties treated from 1965 to 1969 and average them together using the share of counties treated in each year.[27] Reassuringly, restricting comparisons to counties chosen by the OEO does not change our short-run estimates.[28]

B. Racial Uprisings

An obvious candidate explanation for our findings is that they stem from the shock and aftermath of racial uprisings that led to widespread violence and property damage, spikes in deaths due to law enforcement (Cunningham and Gillezeau 2018a), a permanent depression of property values (Collins and Margo 2007), worse labor market conditions for black Americans (Collins and Margo

[27] This yields a sample-weighted average ITT for treated counties as opposed to a variance-weighted average effect (Goodman-Bacon 2018).

[28] Online Appendix Table D1 shows that using a control group of "contiguous" untreated counties produces similar results. We also compare non-treated contiguous counties to non-treated counties further away from treated counties and find no statistical difference in family structure and AFDC take-up. This suggests limited spillovers. Online Appendix Figures D5–D7 also show that trimming the sample to include only counties with estimated propensity scores between 0.1 and 0.9 (as suggested by (Crump et al. 2009)) does not change the results.

2004), white flight, and a shrinking tax base (Boustan 2010). Row 5 re-estimates our models on a sample of counties that never experienced a riot. We find the same pattern of results in these areas as in the full sample.[29]

C. Urban Decay and Marriage Markets

Figure 12 provides more evidence on the possibility of bias from changing marriage markets or eroding economic conditions. Panel A uses local-level sex ratios calculated from the 1930–1990 Censuses (Haines and ICPSR 2010) as outcomes, and finds no change in sex ratios after the 1960s either in the decadal point estimates or in linear trends fit to the pre- and post-1960 data points. At least on the county level the supply of men to marriage markets cannot bias our results.[30] To test for differential changes in "marriageability," Panel B uses data on payroll per worker from the Bureau of Economic Analysis (available since 1962). We find no evidence that earnings diverged after LSPs began.[31] Falling male earnings therefore cannot explain the changing family structure and welfare participation we document.

D. Other War on Poverty Initiatives

The OEO set up many local programs besides the LSP. If LSP counties also systematically received grants for other programs that encouraged welfare take-up, for example, we would overstate the effect of LSP alone. Figure 13 uses data on annual grants for Community Action

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[29] Out of 112 counties in the short-run nonmarital birth sample, 76 experienced a riot, so we include time-to-first-riot dummies in Figure 11 instead of dropping observations. Panel A of Online Appendix Table D2 shows that dropping the counties in the highest quintile of growth in their black share, a consequence of riots, does not alter our estimates.
[30] Online Appendix Figure C2 shows no relative changes in race-specific sex ratios either.
[31] Online Appendix Figure C3 shows a gradual reduction in log employment that does not begin until six years after LSP establishment. Online Appendix Figure C4 shows no sharp changes in female population around LSP establishment. The female population aged 10-49 (the denominator in the Vital Statistics analyses) fall in LSP counties in the fixed effects specification but only after about 5 years. Online Appendix Figure C5 uses the Census sample to estimate reweighted distributional effects on men's earnings using the same method as in Figure 8. Neither all men ages 18–54 nor men without a high school diploma show evidence of differential changes in the distribution of earnings between 1960 and 1970, further suggesting that marriageability cannot explain our findings.

Programs (CAP), Head Start, Community Health Centers (CHC)s, and Family Planning clinics to test how often these new social programs rolled out together. Similar to Bailey and Goodman-Bacon (2015), we find little evidence of bundling. Compared to the (mechanically) large and sustained increase in LSP grants, no other program increases very much.

The largest change is in CAP grants, which precede LSP funding by a few years. CAPs had oversight over many experimental programs and development projects funded by the OEO, but they also served a community organizing function that could conceivably influence public assistance. Row 6 in Figures 9, 10, and 11 controls for time-to-first-CAP dummies and our main estimates do not change.[32]

E. The National Welfare Rights Organization

Our results may also confound the effect of LSPs with the independent effects of local chapters of the National Welfare Rights Organization (NWRO; West 1981). As we discussed, LSPs often served as the legal wing for welfare rights groups (Davis 1993) but the two did not always coincide. We gathered information on the spread of WROs from membership reports and national conference attendance sheets from the archives of NWRO founder George Wiley. Row 7 in Figures 9, 10, and 11 shows that our results are robust to controlling for time-to-NWRO dummies.[33] LSPs' work with WROs is a likely mechanism, but the welfare activism occurring more broadly cannot explain our results.[34]

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[32] We also estimated models on a sample of counties that ever received a CAP. This limits the controls to counties selected by the OEO for *some* bundle of programs. If our main estimates are biased by comparing counties that did or did not apply/receive funds, this sample restriction should eliminate our effects. In fact they do not change.

[33] Row 8 adds controls for riots, CAPs, and NWROs and again finds negligible changes in our main estimates.

[34] These are not admissible controls if LSPs causally affect WRO establishment. If, on the other hand, WROs spring up independently but LSPs make them more effective, these estimates net out the effect of a WRO alone. Online Appendix A provides archival evidence on how LSPs and WROs worked together that is consistent with the second explanation.

F. Placebo Treatment: Community Health Centers

Lastly, Row 9 uses a similar War on Poverty program, CHCs, as a placebo test. CHCs share important characteristics and probably unobservables with LSPs. They received local funding from the OEO in similar patterns over time and space. They required high-skilled labor (doctors instead of lawyers) and hired young, idealistic professional school graduates. We have no reason to expect that CHCs should affect family structure or welfare participation, however, as they focused almost exclusively on providing health services. We take CHC treatment dates from Bailey and Goodman-Bacon (2015), calculate propensity scores exactly as we do for our main results, and estimate short- and long-run DD coefficients for this placebo program. We find no evidence of changes in divorce, AFDC participation, or nonmarital birth rates after CHC establishment, even though the program arose from a nearly identical process to that of LSPs.

VI. DISCUSSION: LSP EFFECTS IN HISTORICAL CONTEXT

The evidence presented above is consistent with a causal interpretation of the relationship between LSP establishment and divorce, welfare participation, and nonmarital births. Event-study estimates show no evidence of pre-trends and match LSPs' reported activities both in sign and scale. Census results show larger changes among lower-educated mothers, those most likely to use LSPs. War on Poverty policies, welfare activism, urban riots, white flight, economic conditions, and sex ratios fail to account for our results. Lastly, other programs allocated in the same way bear no relationship to our main outcomes. We highlight three main implications of these findings for our historical understanding of the 1960s, evidence on economic models of family formation, and current policy.

A. LSPs as a Cause of Family Change in the 1960s

Public discourse and policy have rightly focused on the unprecedented changes to American families in the last 50 years. The controversial 1965 Moynihan report argued that a selfperpetuating "tangle of pathology" was generating skyrocketing black single motherhood rates (Moynihan 1965). Moynihan's analysis came under immediate attack for its assumptions about the causality between poverty and "family breakdown" and the fact that he ignored similar trends in white single parenthood (Rainwater and Yancey 1967). Single motherhood returned to the spotlight in the 1980s, its prevalence alternately ascribed to decades of safety net growth (Murray 1984) or the shrinking pool of "marriageable" (Black) men (Wilson 1987).[35] Recent evidence shows that in addition to their "marriageability," men's physical absence through incarceration can partly explain changing family structure (Charles and Luoh 2010).

Our reduced form results show that policy mattered and highlight the previously overlooked LSP. Some of the LSPs' effects lined up clearly with their goals. One lawyer observed that "if you can help a woman who was deserted ten years ago get a divorce now, I think that is breaking the cycle of poverty" (Finman 1971). LSP lawyers challenged welfare bureaucracies because they were especially unresponsive and arbitrary, and because they envisioned a constitutional right to guaranteed income (Krislov 1973). Increasing divorce and AFDC rates were therefore *intended* consequences of the LSP program. Importantly, these changes came from a policy of expanding access to legal services, institutions, and benefits, not a policy of expanding the "total welfare package," as is commonly claimed (Murray 1993, p. S225). This is a new historical interpretation based on the reduced-form effect of LSPs specifically.

[35] Both explanations already had a long history. Skocpol (1992, p. 467) quotes a 1914 argument against providing Mothers Pensions to groups other than widows from the *Report of the New York State Commission on Relief for Widowed Mothers:* "To pension desertion or illegitimacy would, undoubtedly, have the effect of putting a premium on these crimes against society." Du Bois and Eaton (1899, p. 53) note in their study of Philadelphia that "the first thing that strikes one is the unusual excess of females. This fact, which is true of all Negro urban populations, has not often been noticed, and has not been given its true weight as a social phenomenon." They credit sociologist Kelly Miller of Howard University with the observation (a fact brought to our attention by Gerald Jaynes).

Our estimates do not, however, speak to historical interpretations of Black family structure change. The government did not publish tables of divorce, marriage, and AFDC participation by race, and only broke out nonmarital births by race for a small for a handful of counties. We do not view this as a significant limitation, however, because both Black and White families changed dramatically during this period. In our Census sample of 81 urban counties, the share of mothers who did not live with the father of their children more than doubled between 1960 and 1980 for white women (7.5 to 16.5 percent) and increased by about 64 percent for nonwhite women (27.6 percent to 45.3 percent). An analysis of family structure change by race would require not only better outcome data but also better data on the factors that drive the large underlying differences in family structure, which mean that level changes (but not proportional changes) are larger for nonwhite families.

B. Welfare as a Mechanism

We do, however, view the incentives created by AFDC and unlocked by LSPs as a key explanation for our finding that marriage rates fell and nonmarital birth rates rose after LSP establishment. For one, LSPs' other activities are unlikely to have the same effects on families. For example, LSP lawyers also worked to represent tenants in housing disputes plausibly reducing evictions. However, it is not clear if this would reduce the likelihood of divorce and maintain two-parent families—the opposite of our findings--or bolster single parent households.[36] Second, theoretical predictions from unitary models (Becker 1991, Neal 2004) and bargaining models (Willis 1999) as well as ethnographic research (Stack 1974) support the interpretation that a benefit specifically

[36] Charles and Stephens (2004) highlights how financial stress due to layoff increases the likelihood of divorce. However, it is unclear if evictions themselves would lead to divorce as tenants typically show signs of financial distress well before an eviction filing (Humphries et al. 2019). Relatedly, once controlling on other observables, there is no evidence that relationship dissolution is correlated with future evictions (Desmond and Gershenson 2017).

for single parents should raise single parenthood. Moffitt (1992, p. 27) concludes that “virtually any model of marital status and childbearing behavior will have these implications.”

Empirical research on welfare and family structure, however, is mixed. Panel approaches find no effect of year-to-year *changes* in welfare generosity on family structure (Hoynes 1997, Moffitt 1994), although there is some evidence that living arrangements respond (Ellwood and Bane 1985). For non-benefit policies, Kearney (2004) finds no effect of family caps, Bitler et al. (2004) find that welfare reform reduced divorce and marriage rates, and Schoeni and Blank (2000) find that pre-welfare-reform waivers reduced female headship. Randomized evaluations of specific program parameters both from the Negative Income Tax experiments and AFDC waivers in the 1990s provide mixed evidence on marital status and fertility among women already receiving benefits (Grogger, Karoly, and Klerman 2002, Hannan, Tuma, and Groeneveld 1977).

Our results suggest that changes in the ease with which poor women could actually receive welfare benefits can help explain this discrepancy. Local restrictions on eligibility and arbitrary caseworker decisions had become the norm in AFDC by the 1960s. Families did not respond to benefit changes because the probability that they could get and keep those benefits appeared low. We document, both historically and empirically, that LSPs drastically changed this situation.[37] They ensured that families entitled to benefits could get them, which exposed poor families to welfare incentives in one sense for the first time. Moreover, these changes were plausibly permanent.[38] Setting new precedents for the way welfare bureaucracies treated recipients,

[37] Moffitt (1987) describes a “structural shift” in take-up in the 1960s, concluding that “(1) attitudes toward welfare changed over the period and the stigma of welfare receipt fell; and (2) a series of court and legislative decisions that liberalized eligibility during the period made participation easier.” The LSP is likely responsible to a degree for both.

[38] Rosenzweig (1999) argues that family structure, a long-term decision, should not be strongly affected by short-run changes in benefits, and McKinnish (2008) provides empirical support in the case of AFDC and fertility by comparing short and long differences.

improving information about rules, and encouraging applications and administrative challenges are essentially irreversible. Economic models of cash programs and family structure therefore have more empirical support than evidence from short-run changes in an uncertain benefit suggested. This is a behavioral interpretation consistent with economic theory and based on a collection of evidence pointing toward AFDC access as a mechanism for family structure change.

It is important to note the potential interaction of LSPs' effects on welfare with other changes in marriage markets. Neal's (2004) model of family structure emphasizes the interaction between the quality of men and safety net policy.[39] Therefore, while we interpret our findings as the causal effects of the LSP, they may not generalize to other contexts where background changes in men's circumstances are not the same. As we argued earlier, however, understanding family structure trends in the 1960s specifically is important for researchers and policymakers.

C. What does an LSP effect mean for policy?

Policymakers have relied on different interpretations of family structure changes to propose drastically different policies. In 1961, for example, President Kennedy expanded welfare to two-parent families because "too many fathers, unable to support their families, have resorted to real or pretended desertion to qualify their children for help" (Kennedy 1977). In 1965, on the other hand, Moynihan advocated a federal job guarantee and then a basic income in order to address rising single motherhood (O'Connor 2001). Murray (1984, p. 227) proposed "scrapping the entire federal welfare and income-support structure for working-aged persons." Ultimately, 1996's Personal Responsibility and Work Opportunity Reconciliation Act did dramatically shrink cash

[39] Cunningham (2016) shows that LSP establishment led to higher arrest rates by improving community/police relations and increasing crime reporting. We find no evidence that these changes altered sex ratios or men's earnings differentially in LSP counties in the way that prison policy did in the 1980s.

welfare for families. The legislation specifically sought to promote marriage, “reduce the incidence of out-of-wedlock pregnancies,” and “encourage the formation and maintenance of two-parent families.”[40]

Our findings support the claim that cash welfare restricted to single parents, when recipients can access it, affects family structure. But our findings are not consistent with a story where families changed because the safety net became more generous. The LSP brought the *de facto* welfare system closer to the *de jure* welfare system. We do not know how family structure would have responded to a less generous safety net without these arbitrary restrictions, or a more generous safety net not limited to single parents. (In fact, a motivation for AFDC was to keep children and mothers together.) Moreover, we do not find that these changes increased poverty.

Considering policy around legal services, it is hard to imagine policymakers “going the other way.” In 1975, the Neighborhood Legal Services Program went through sweeping changes with new oversight and restrictions from the newly created Legal Services Corporation (LSC). By 1980, tenant-landlord disputes dominated LSP caseloads and reform cases became essentially non-existent. But it is even harder to imagine a plausible, legal, or ethical policy that reimposes unconstitutional bureaucratic practices that were overturned by LSP lawyers. Facing loosened constraints presumably increased women’s utility, but the ultimate effects of the LSP on children, for example, remains an ambiguous and open question for future research.

[40] Charles and Luoh (2010), for example, conclude that increasing sentences for low-level drug crimes reduced marriage rates in the 1980s and 1990s by altering sex ratios and bargaining in marriage markets. An implication is that criminal justice policy affects families. Shenhav (2018) and Autor, Dorn, and Hanson (2017) show that strong labor markets for women relative to men reduce marriage. An implication is that labor market policy affects families.

D. What did the LSP mean for people's well-being?

We cannot draw conclusions about how the changes we document matter for well-being. There are reasons to believe such changes could have made parents and children worse off. Single parents cannot "gain from a division of labor between market and household activities" (Becker 1991, p. 3). Single parenthood also "deprives children of important economic, parental, and community resources" (McLanahan and Sandefur 1994, p. 3) and it is the "strongest correlate of upward income mobility" across neighborhoods (Chetty et al. 2014).[41]

On the other hand, LSPs relaxed poor families' budget constraints and altered their *choices*. Evidence from unilateral divorce reforms, for example, shows that increased access to divorce raised divorces but reduced female suicide rates (Stevenson and Wolfers 2006) All parties may be better off if marginal marriages break up. Moreover, easier access to welfare and divorce changed bargaining within marriages that did not break up, and we cannot measure well-being among these couples (Lundberg and Pollak 1996). Using the National Crime Victimization Survey from 1992 to 1998, Farmer and Tiefenthaler (2003) find that legal services for victims of domestic violence has a "significant negative effect on the likelihood that an individual woman is battered." Lastly, we do not find any increases in poverty; the cash safety net worked. One way to examine how LSPs affected well-being among children would be to observe their adult outcomes. We leave this to future work.

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[41] Policymakers clearly believe that family structure matters. A planning paper for the White House's 1966 Civil Rights conference argued "few would deny that a harmonious two-parent home offers the best prospect for a child to reach his full potential" (quoted in Rainwater and Yancey 1967, p. 322). Reflecting recently on the War on Poverty, Robert Rector (2014) blamed "the collapse of marriage in low-income communities" for persistent poverty, and former House Speaker Paul Ryan (2014, p. 4) argued that the "most important determinant of poverty is family structure."

VII. Conclusion

We provide the first evidence on the effect of federally subsidized legal assistance on welfare receipt and family structure during the 1960s. Our results suggest that improved access to legal advice and representation increased divorce in the short run and led to a permanent shift up in welfare receipt, as intended. We also find that the program raised nonmarital births. These changes in marital status at birth are reflected in a reduction in the probability that mothers lived with the father of their children between 1960 and 1970. Our findings contribute to debates about why single parenthood and welfare use increased so sharply in the 1960s, and help reconcile strong theoretical predictions about welfare and families with typically weak empirical evidence.

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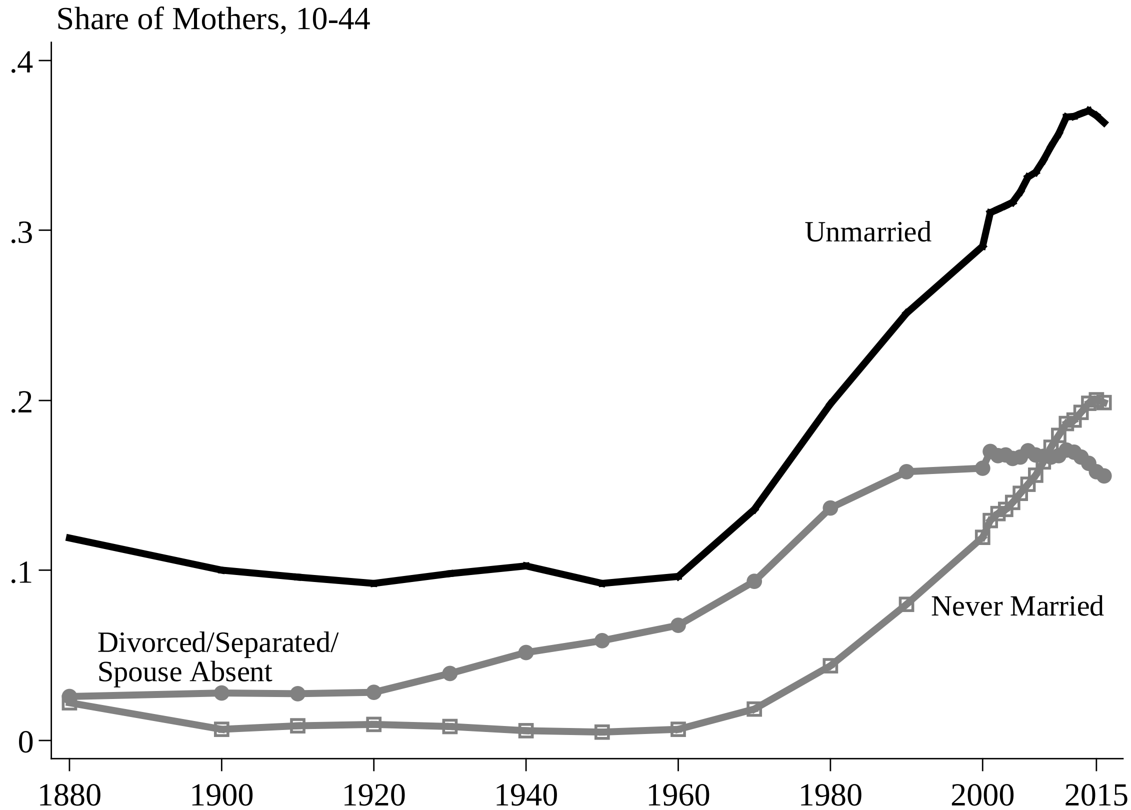
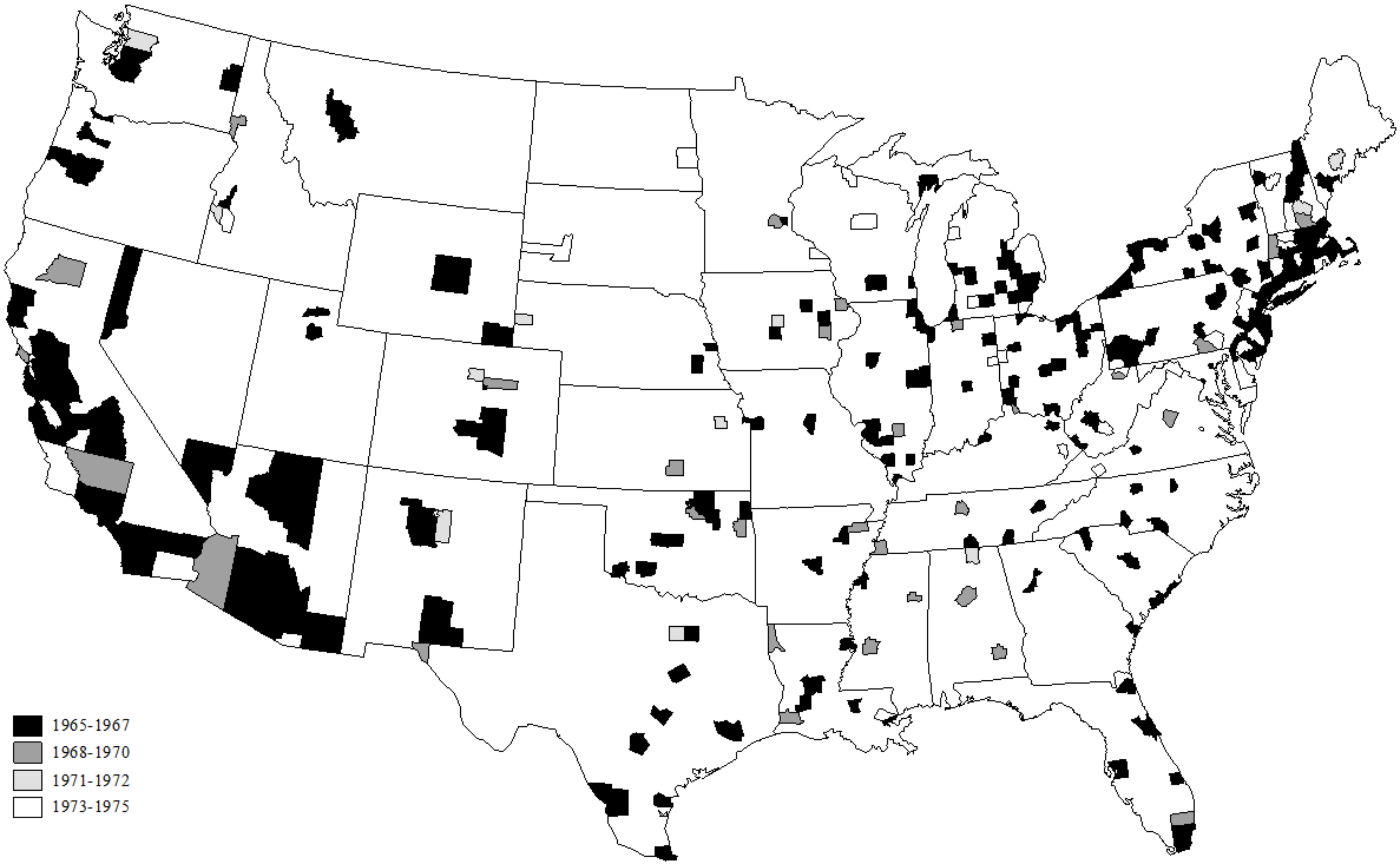
Figure 1. Mothers' Marital Status, 1880-2016



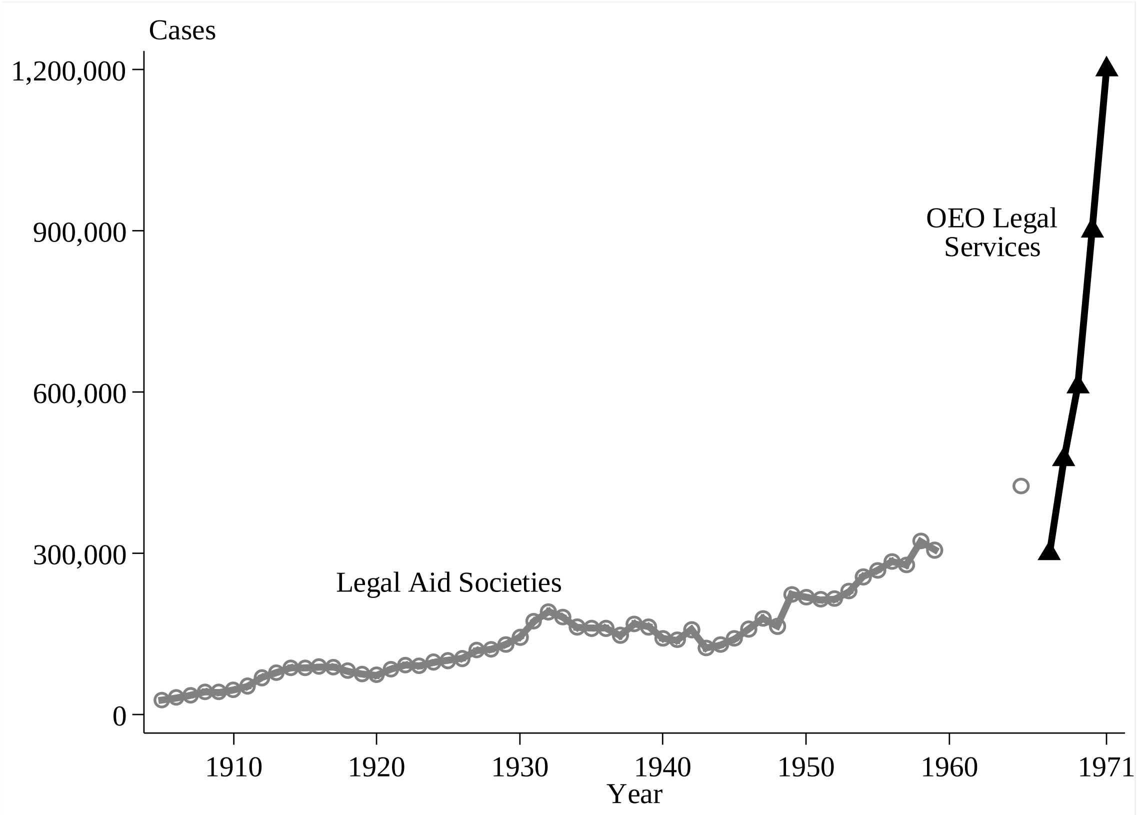
Notes: The figure plots the share of mothers in the Census and American Community Survey who are between ages 10 and 44 and who report a marital status other than married. We group married women with an absent spouse with those divorced and separated because "separated" was only added as a separate category in 1950. In 1930, the last pre-war Census before "separated" was added to the marital status question, about 2.6 percent of women reported that they were "married, spouse absent." In 1950, after "separated" was available, the share was 1.4 percent. The figure omits widowhood, which fell continuously throughout this period and was essentially the only reported reason for unmarried motherhood before 1920.
Sources: Ruggles et al. (2010).

Figure 2. Establishment of Neighborhood Legal Services Programs by County, 1965-1975



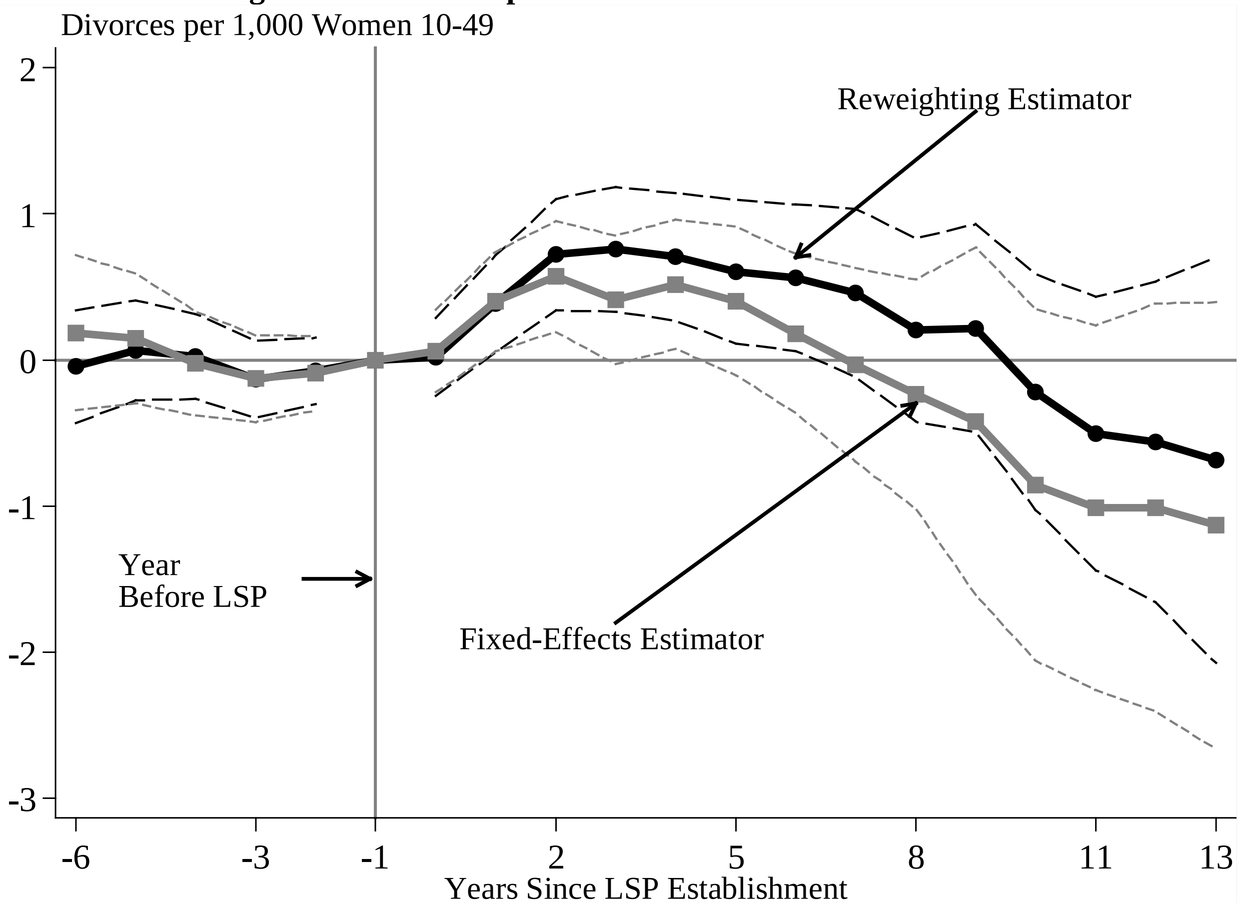
Notes: The figure maps the 251 counties in Sample 1 that received their first LSP grant between 1965 and 1975. Darker shading indicates earlier grant receipt.
Sources: National Archives Community Action Program files and Cunningham (2016).

Figure 3. Civil Cases Handled by Legal Aid Societies and OEO Legal Services Programs, 1905-1971



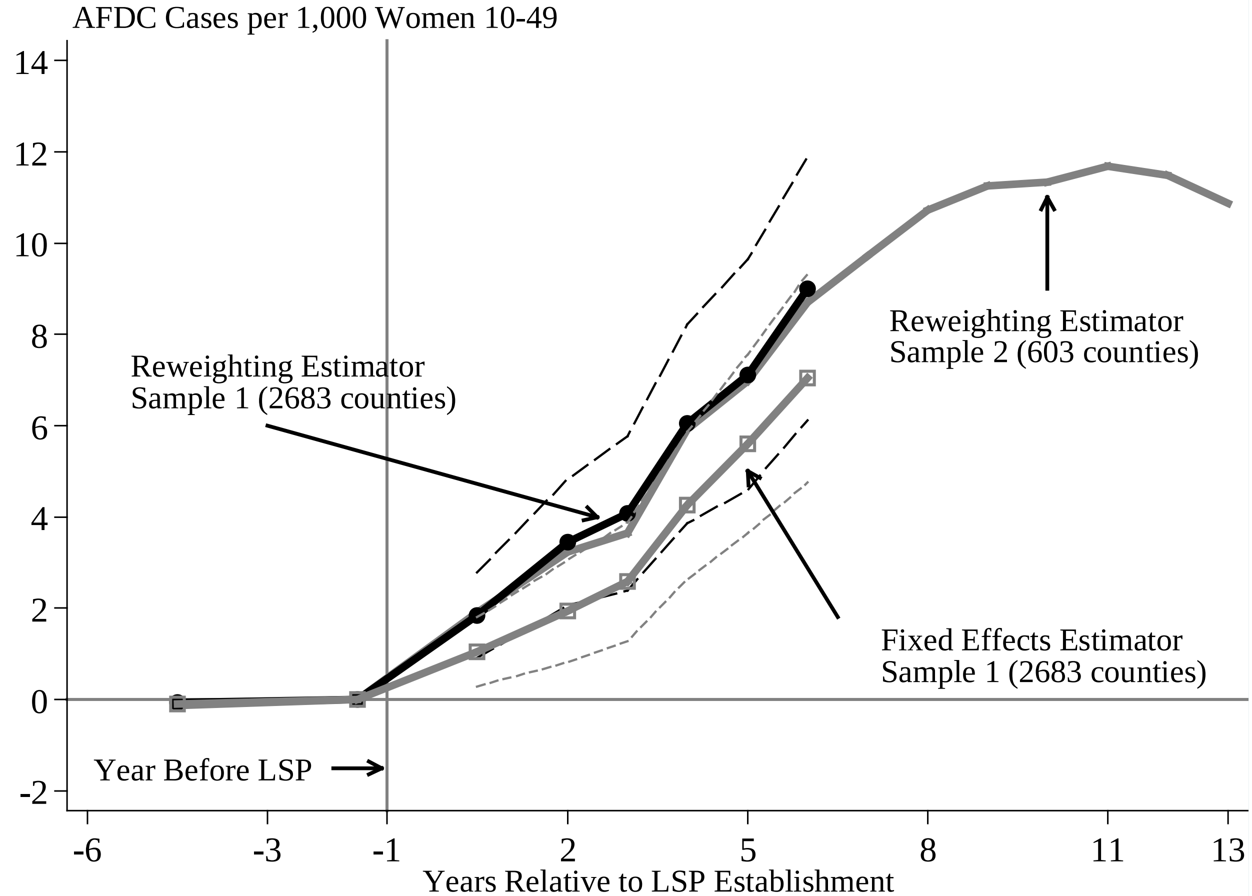
Sources: Data on legal aid cases were entered from Brownell (1951) and Brownell (1971) except for the estimated number of cases in 1965, which comes from a report by the John D. Ketelle Corporation (1971). Data on LSP cases for 1967–1971 were taken from Congressional testimony by Donald Rumsfeld.

Figure 4. Relationship between LSP Establishment and Divorces



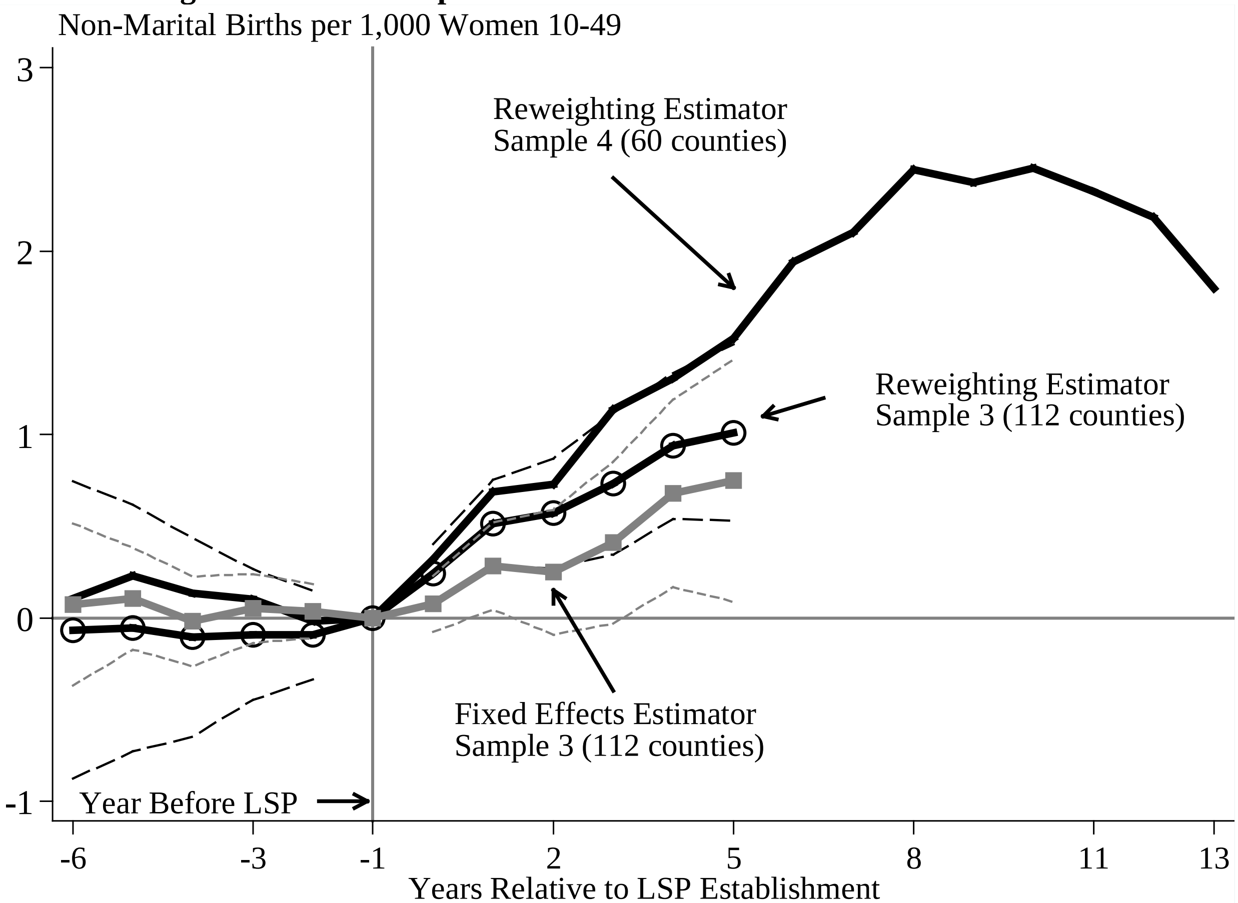
Notes: The dependent variable is the number of divorces that occur in county c and year t divided by the number of women ages 10–49 measured in thousands. The average dependent variable in treated counties in the year their LSP starts is 9 divorces per 1,000 women. The figure plots event-study estimates from equation (1) for the inverse propensity score reweighted specification (black lines with solid circles) and an unweighted specification that controls for state-by-year and urban-group-by-year fixed effects (gray lines with open squares). We do not observe event-times before -6 or after +13 for all treated counties. They are included in separate bins and their coefficients are not reported. Dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by county. Sample 1 includes 2,683 counties.

Figure 5. Relationship between LSP Establishment and Aid to Families with Dependent Children Cases



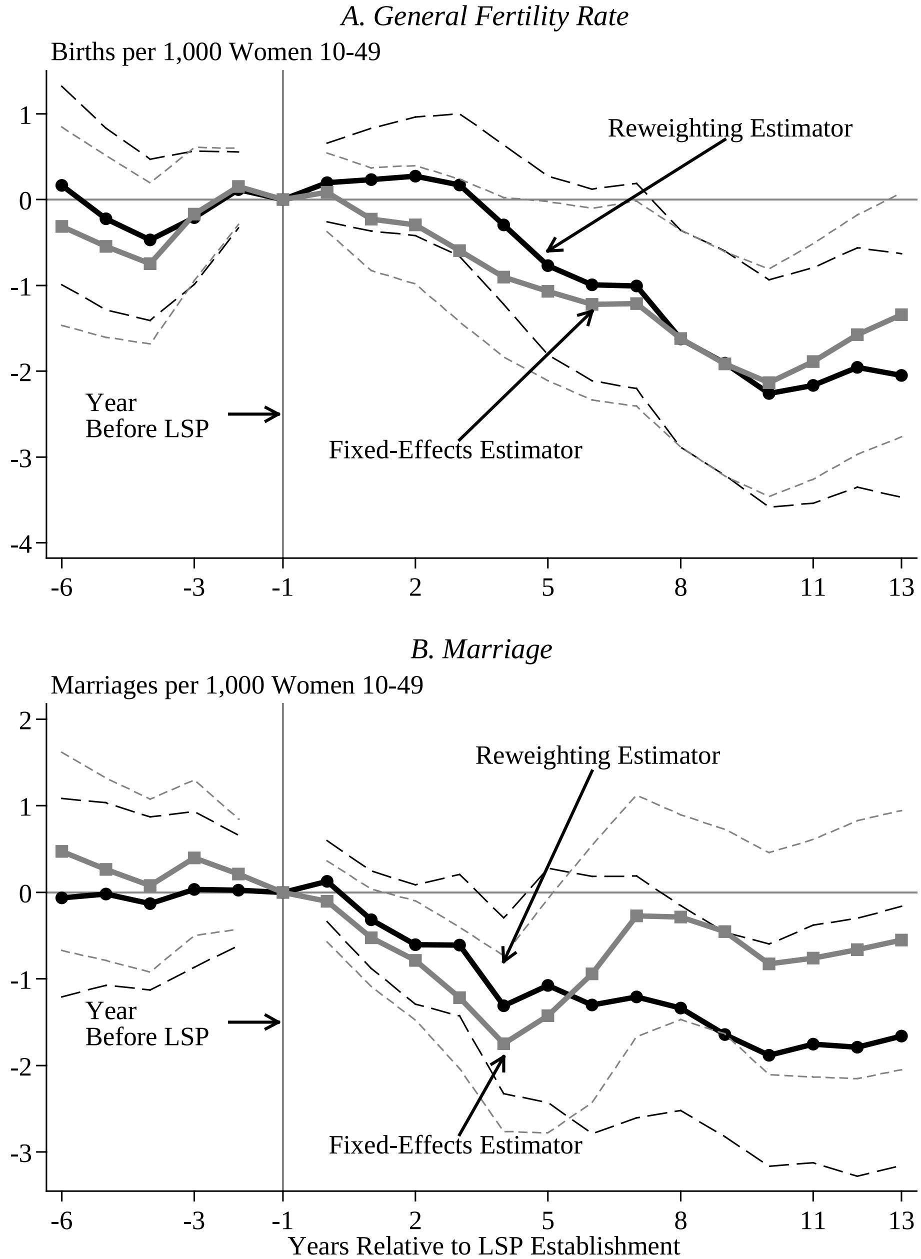
Notes: The dependent variable is the number of open AFDC cases in county c and (a given month of) year t divided by the number of women ages 10–49 measured in thousands. The average dependent variable in treated counties in the year their LSP starts is 23 cases per 1,000 women. The figure plots event-study estimates from equation (1) for the inverse propensity score reweighted specification and an unweighted specification that controls for state-by-year and urban-group-by-year fixed effects. Because data on AFDC cases are not available in every year before 1968 we group event-study dummies -6 through -3 into one dummy whose coefficient is plotted at -4.5. The omitted category combines event-times -2 and -1. We do not observe event-times before -6 or after +7 (Sample 2) or +13 (Sample 1) for all treated counties. They are included in separate bins and their coefficients are not reported. Dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by county. Sample 1 includes 2,683 counties and Sample 2 contains 603 counties in SMSAs.

Figure 6. Relationship between LSP Establishment and Nonmarital Births



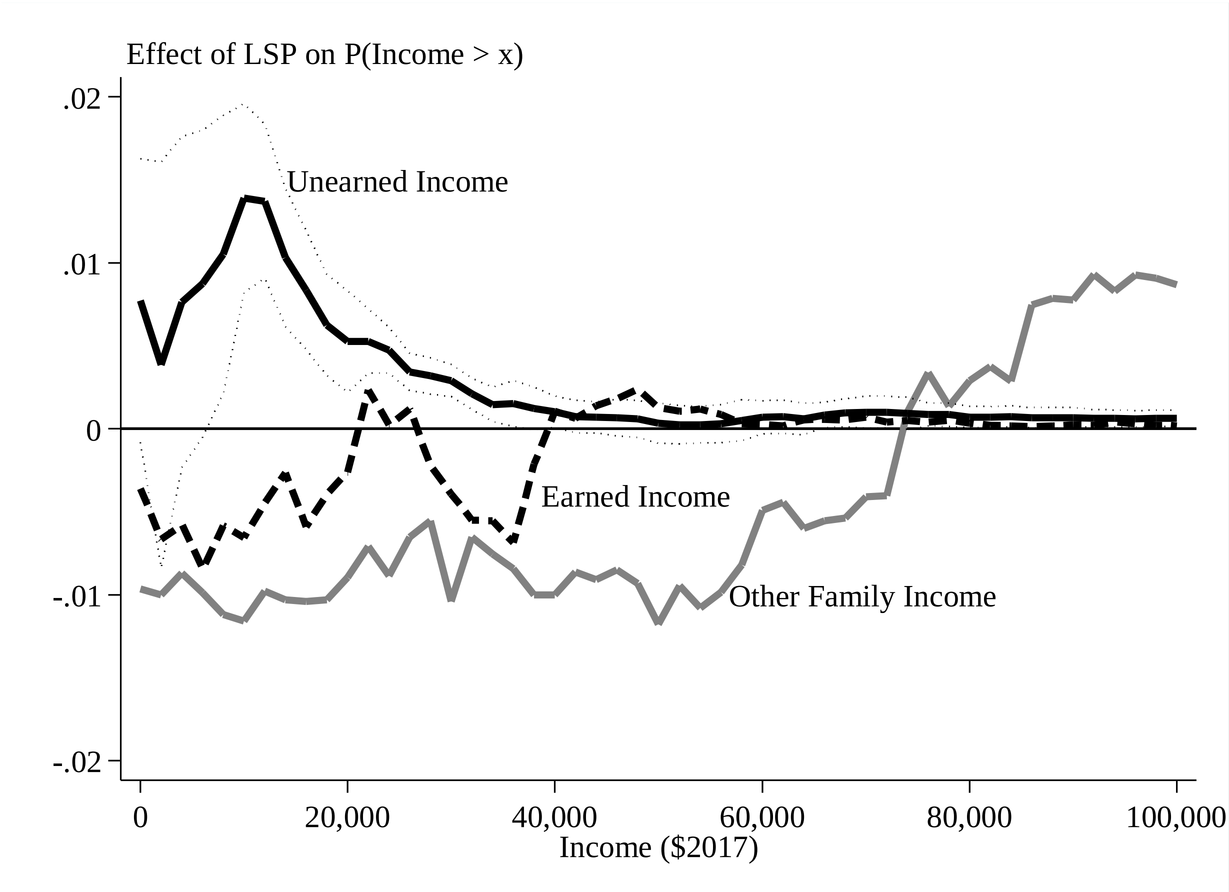
Notes: The dependent variable is the number of births to unmarried mothers in county c and year t divided by the number of women ages 10–49 measured in thousands. The average dependent variable in treated counties in the year their LSP starts is 5 births per 1,000 women. The figure plots event-study estimates from equation (1) for the inverse propensity score reweighted specification and an unweighted specification that controls for state-by-year and urban-group-by-year fixed effects. We do not observe event-times before -6 or after +5 (Sample 3) or +13 (Sample 4) for all treated counties. They are included in separate bins and their coefficients are not reported. Dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by county. Sample 3 includes 112 counties (65 treated) and Sample 4 contains 60 counties (28 treated).

Figure 7. Relationship between LSP Establishment, Fertility, and Marriage



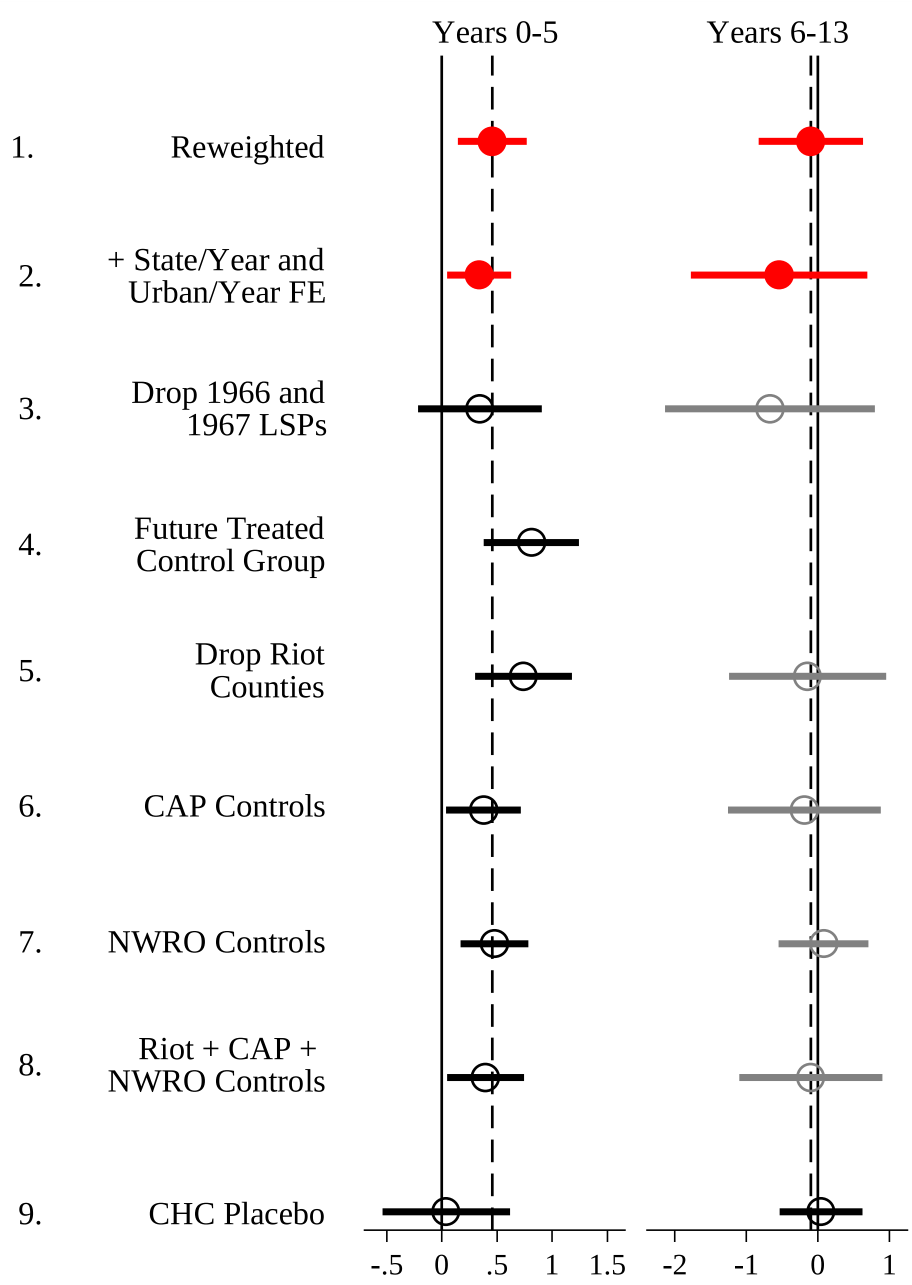
Notes: The dependent variable in Panel A is the number of births in county c, year t, divided by the number of women ages 10–49 measured in thousands. The average dependent variable in treated counties in the year their LSP starts is 62.7 births per 1,000 women. The dependent variable in Panel B is the number of marriages that occur in county c, year t, divided by the number of resident women ages 10–49 measured in thousands. The average dependent variable in treated counties in the year their LSP starts is 31.9 marriages per 1,000 women. The figure plots event-study estimates from equation (1) for the inverse propensity score reweighted specification and an unweighted specification that controls for state-by-year and urban-group-by-year fixed effects. We do not observe event-times before -6 or after +13 for all treated counties. They are included in separate bins and their coefficients are not reported. Dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by county. These results come from Sample 1 which includes 2,683 counties.

Figure 8. Relationship between LSP Establishment and the Distribution of Mother's Income by Source, 1960-1970



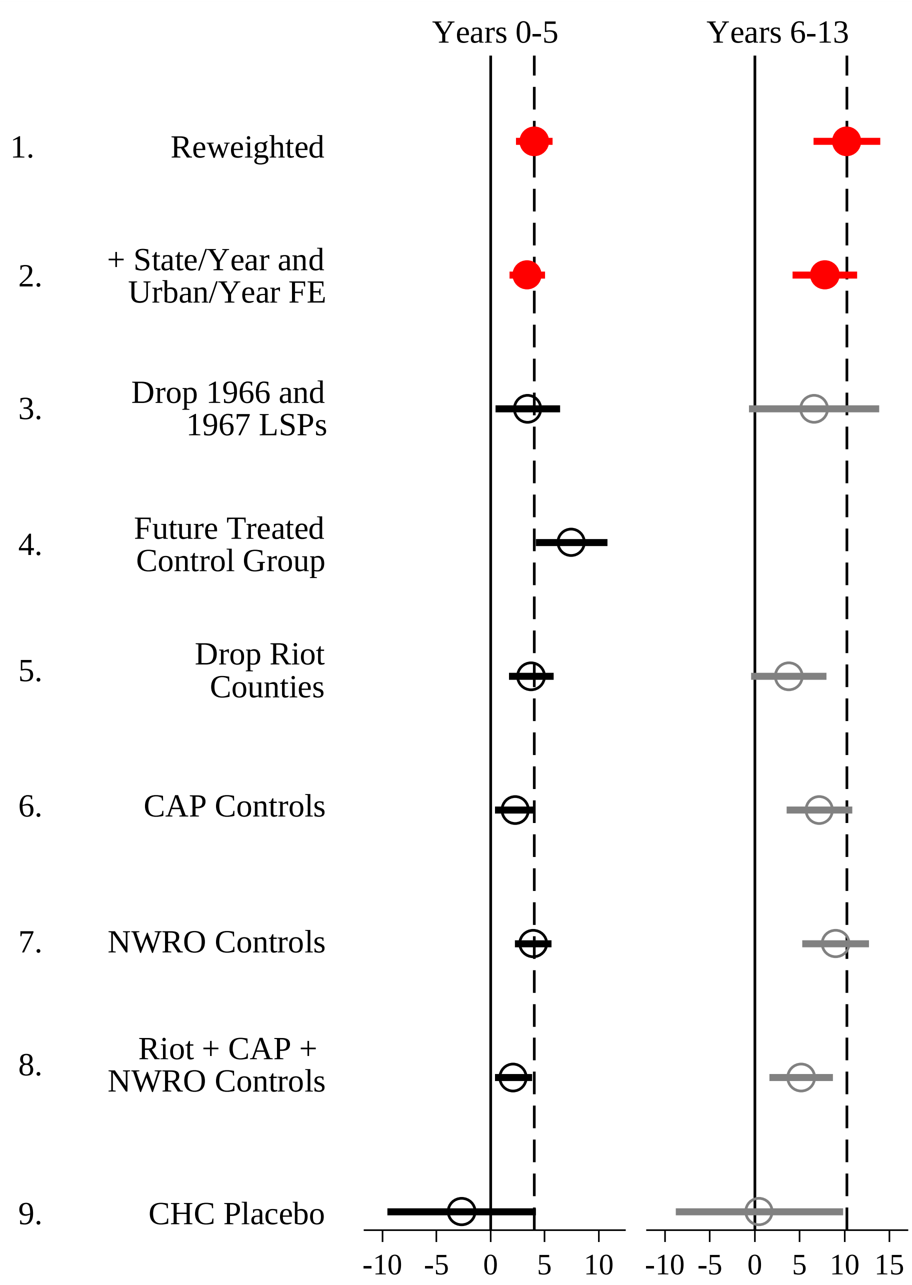
Notes: The figure plots DD coefficients (from the reweighting estimator) with the outcome variable defined as the change from 1960–1970 in the county-level probability of having income greater than or equal to the amount on the *x*-axis (measured in $2,000 bins in 2017 dollars). For details on "distribution regression" see Chernozhukov, Fernández-Val, and Melly (2013). This reflects changes in the cumulative distribution of income by source. Sample 5 includes mothers living with their children in the 1960 and 1970 Census in 81 counties identified in both years with underlying samples of 390,599 mothers in 1960 and 170,941 mothers in 1970. Unearned income equals total individual income minus earned income (wage, business, and farm income). Other family income equals total family income minus the mother's own income. The dotted lines are 95-percent pointwise confidence intervals for the unearned income results. None of the individual coefficients for other sources of income are statistically significant.

Figure 9. Robustness of Intention-to-Treat Effects for Divorce Rates



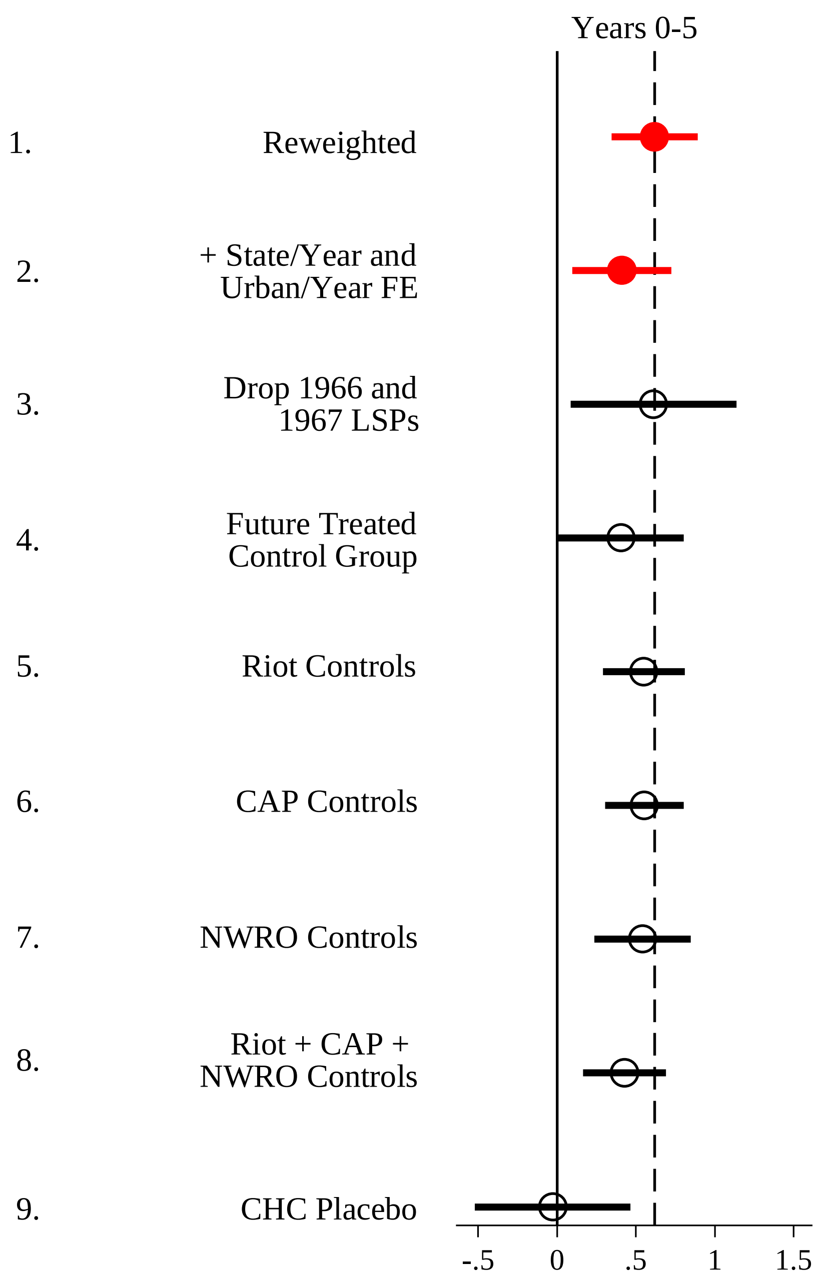
Notes: The figure plots shorter-run (years 0–5) and longer-run (years 6–13) estimates for alternative specifications discussed in section V. FE = fixed effects; DD = difference-in-differences; CAP = Community Action Program; NWRO = National Welfare Rights Organization; CHC = Community Health Center. Estimates with a future-treated control group follow equation 2.2 in Callaway and Sant'Anna (2018) by estimating a single post-treatment DD coefficient for each treatment year ("group") for event-years 0 through 4, and then averaging them together using the distribution of treatment years between 1965 and 1969. We use asymptotic standard errors.

Figure 10. Robustness of Intention-to-Treat Effects for AFDC Participation Rates



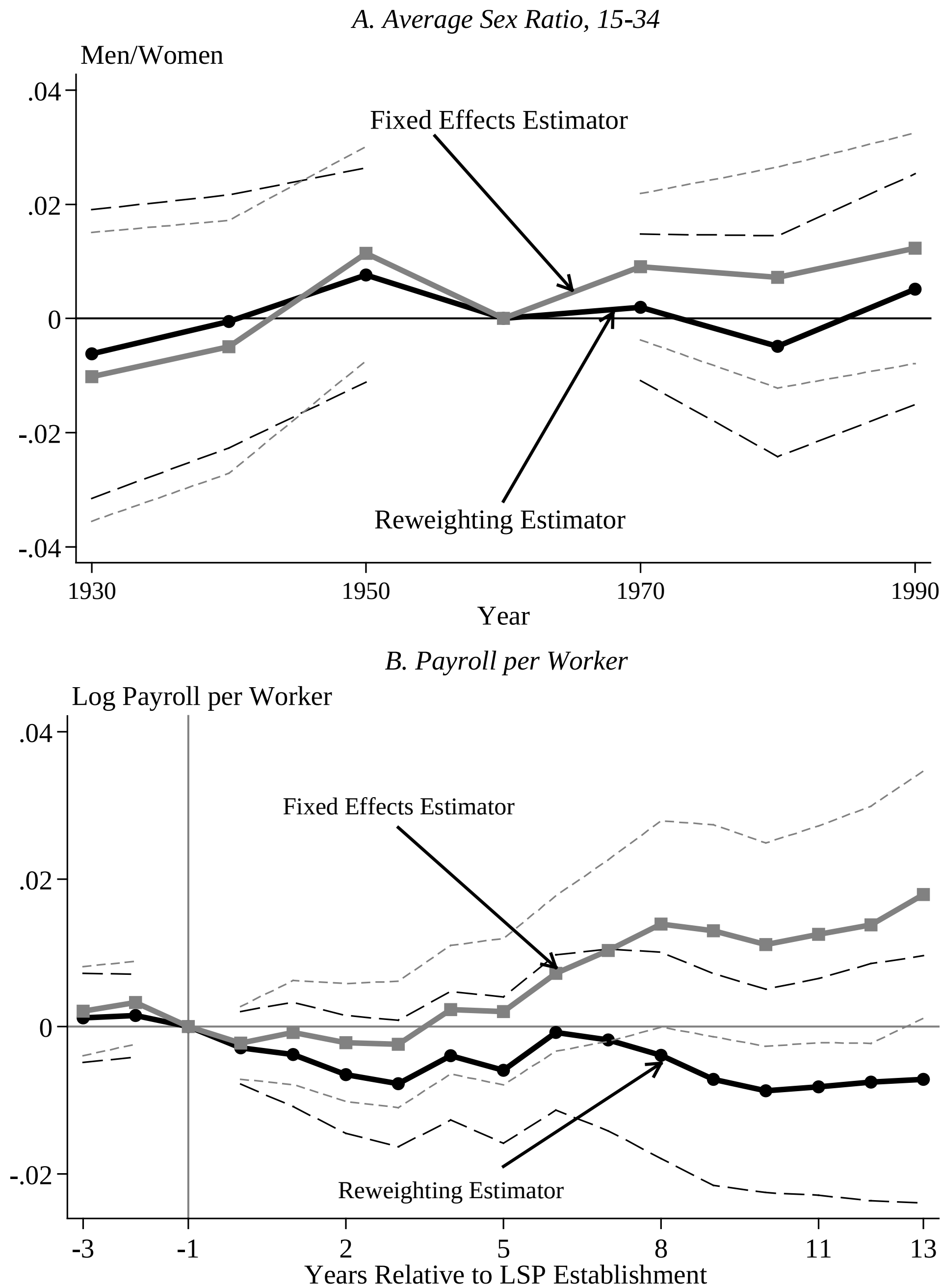
Notes: The figure plots shorter-run (years 0–5) and longer-run (years 6–13) estimates for alternative specifications discussed in section V. FE = fixed effects; DD = difference-in-differences; CAP = Community Action Program; NWRO = National Welfare Rights Organization; CHC = Community Health Center. Estimates with a future-treated control group follow equation 2.2 in Callaway and Sant'Anna (2018) by estimating a single post-treatment DD coefficient for each treatment year ("group") for event-years 0 through 4, and then averaging them together using the distribution of treatment years between 1965 and 1969. We use asymptotic standard errors.

Figure 11. Robustness of Intention-to-Treat Effects for Nonmarital Birth Rates



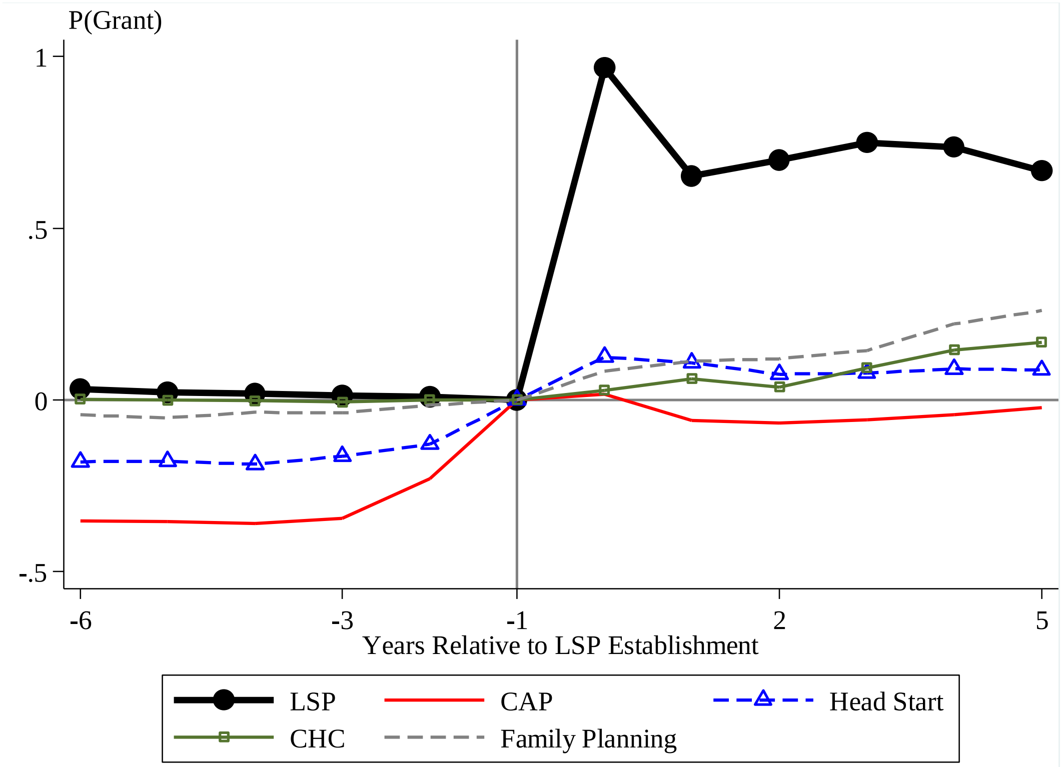
Notes: The figure plots shorter-run (years 0–5) estimates for alternative specifications discussed in section V. FE = fixed effects; DD = difference-in-differences; CAP = Community Action Program; NWRO = National Welfare Rights Organization; CHC = Community Health Center. Estimates with a future-treated control group follow equation 2.2 in Callaway and Sant'Anna (2018) by estimating a single post-treatment DD coefficient for each treatment year ("group") for event-years 0 through 4, and then averaging them together using the distribution of treatment years between 1965 and 1969. We use asymptotic standard errors.

Figure 12. Relationship between LSP Establishment, Payroll per Worker, and Sex Ratios



Notes: The dependent variable in Panel A is the ratio of men to women ages 15–34 in county c and year t from Census population tabulations (Haines and ICPSR 2010). The dependent variable in Panel B is the log of payroll per worker in county c and year t from County Business Patterns data. Panel A plots event-study estimates from a version of equation (1) that interacts a dummy for receiving any LSP grant with Census year dummies. Panel B plots event-study estimates from equation (1). Both panels plot estimates from the inverse propensity score reweighted specification and an unweighted specification that controls for state-by-year and urban-group-by-year fixed effects. We do not observe event-times before -3 or after +13 for all treated counties. They are included in separate bins and their coefficients are not reported. Dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by county. These results come from Sample 1 which includes 2,683 counties.

Figure 13. Relationship between LSP Establishment and Other War on Poverty Grants



Notes: The dependent variables are annual grant probabilities for the listed programs taken from Bailey and Goodman-Bacon (2015). The figure plots event-study estimates from equation (1) for the inverse propensity score reweighted specification and an unweighted specification that controls for state-by-year and urban-group-by-year fixed effects. We do not observe event-times before -6 or after +13 for all treated counties. They are included in separate bins and their coefficients are not reported. Dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by county. CAP = Community Action Program; CHC = Community Health Center. These results come from Sample 1 which includes 2,683 counties.

Table 1. Changes in Family Structure Outcomes, 1960–1980

	(1) 1960	(2) 1970	(3) 1980	(4) % Change, 1960–1970	(5) % Change, 1960–1980
Nonmarital Births per 1,000 Women	4.52	6.84	9.31	51%	106%
Teens	6.23	10.67	13.73	71%	121%
20s	9.16	10.61	15.02	16%	64%
30s	2.48	2.58	2.90	4%	17%
40+	0.23	0.24	0.20	4%	-12%
Divorces per 1,000 Women	8.00	12.00	18.03	50%	125%
Marriages per 1,000 Women	30.84	37.01	35.98	20%	17%
AFDC Cases per 1,000 Women	15.53	30.05	50.24	93%	223%
AFDC Children per Case	2.85	2.87	1.98	1%	-30%
AFDC Benefit per Recipient ($2012)	246.08	252.94	243.15	3%	-1%

Notes: The table gives population-weighted means based on the available counties described in Section II.E. Divorces, marriages, and AFDC outcomes are based on 2,683 counties in Sample 1. Nonmarital birth rates are based on 112 counties in Sample 3.

Table 2. Distribution of LSP Treatment Status

Treatment Status	Number of Counties	Percentage of Counties	Percentage of 1960 Population
Treated	**251**	**9.4**	**55.3**
Year Treated:			
1965	7	0.3	7.2
1966	96	3.6	29.2
1967	92	3.4	13.6
1968	11	0.4	1.6
1969	7	0.3	1.0
1970	11	0.4	1.0
1971	6	0.2	0.3
1972	4	0.1	0.5
1973	10	0.4	0.6
1974	4	0.1	0.1
1975	3	0.1	0.2
Untreated	**2,432**	**90.6**	**44.7**

Notes: The table shows the number of counties, the percentage of counties, and the percentage of our largest sample first treated by LSPs in different years. Nationwide 273 counties had an LSP by 1975, so we use this number to aggregate up our estimates. In our largest analysis sample (Sample 1: 2,683 counties), 251 counties received LSPs.

Table 3. Balance in Pretreatment Characteristics Between LSP and Non-LSP Counties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	LSP Counties	Difference in Non-LSP Counties					
		Unweighted	*p*-value	State + Urban-Group FE	*p*-value	Reweighted	*p*-value
Percentage of 1960 population:							
in urban area	71.7	-42.8	<.01	-3.8	<.01	-3.6	0.06
nonwhite	9.0	1.3	.08	-3.1	<.01	-0.8	0.43
under age 5	11.5	-0.5	<.01	0.0	0.89	0.0	0.96
over age 65	9.2	1.7	<.01	0.1	0.63	0.0	0.99
with <4 years of school	7.7	3.1	<.01	-1.0	0.02	0.2	0.63
with >12 years of school	42.5	-5.6	.02	19.7	0.29	0.0	0.99
with family income <$3k	19.7	16.7	<.01	-0.4	0.54	1.2	0.26
with family income >$10k	14.6	-7.2	<.01	-1.2	<.01	-0.1	0.92
1960 median family income	$5,700	-$1,616	<.01	-$57	0.32	-$84	0.55
1960 median years of schooling	10.7	-1.1	<.01	0.1	0.11	-0.1	0.57
1960 levels of:							
AFDC cases/1,000 women 10–49	16.0	2.7	<.01	-2.4	0.02	-0.7	0.58
marriages/1,000 women 10–49	28.4	11.7	<.01	2.0	0.25	0.3	0.76
divorces/1,000 women 10–49	8.2	-0.5	0.30	-0.1	0.75	-0.2	0.56
1960–1964 change in:							
AFDC cases/1,000 women 10–49	2.3	-3.3	<.01	-1.4	<.01	-0.8	0.30
marriages/1,000 women 10–49	0.8	2.6	0.02	0.3	0.75	0.0	0.96
divorces/1,000 women 10–49	0.5	-0.3	0.39	0.4	0.21	0.0	1.00
Joint *F*-stat			***29.21***		***13.62***		***0.89***
***p*-value**			***<.01***		***<.01***		***.58***

Notes: The table gives summary statistics for Sample 1 from either our outcome data (see section II) or Haines and ICPSR (2010).

Table 4. Estimated ITT Effects of LSPs on Divorce, Nonmarital Births, and AFDC Cases

	(1)	(2)	(3)	(4)	(5)	(6)
	Divorces per 1,000 Women		AFDC Cases per 1,000 Women		Nonmarital Births per 1,000 Women	
Pre-LSP						
Years -6 to -2	-0.01	-0.06	-0.07	-0.26	0.00	-0.12
	[0.16]	[0.13]	[0.87]	[0.92]	[0.11]	[0.24]
Shorter-Run Post-LSP						
Years 0 to 5	0.34	0.46	2.73	4.05	0.34	0.62
	[0.16]	[0.16]	[0.74]	[0.85]	[0.15]	[0.14]
	(.142)	*(.114)*	*(.002)*	*(.000)*	*(.018)*	*(.004)*
Longer-Run Post LSP						
Years 6 to 13	-0.59	-0.10	6.55	10.25		
	[0.49]	[0.37]	[1.53]	[1.9]		
	(.836)	*(.466)*	*(.000)*	*(.000)*		
Counties	2,683	2,683	603	603	112	112
Reweighted		x		x		x
Fixed Effects	x		x		x	

Notes: The table presents estimates from an inverse propensity score reweighted specification and an unweighted specification that controls for state-by-year and urban-group-by-year fixed effects that summarize the event-study figures by grouping event-times of years -6 to -2, 0 to 5, and 6 to 13. We do not observe event-times before -6 or after +13 for all treated counties. They are included in separate bins and their coefficients are not reported. Standard errors clustered by state are in brackets, one-sided *p*-values for 500 random permutations of the LSP variable are in parentheses (see Appendix D for permutation distributions). ITT = intention-to-treat.

Table 5. Quantifying LSPs' Role in Nonmarital Birth and AFDC Rates, 1964–1984

	(1)	(2)	(3)	(4)
	AFDC Cases per 1,000 Women		Nonmarital Births per 1,000 Women	
	Treated Counties	Full Sample	Treated Counties	Full Sample
Observed Outcomes				
a. 1964	19.07	18.41	5.30	5.25
b. 1984	55.74	40.72	11.32	10.98
c. Change	36.67	22.31	6.02	5.72
Counterfactual Outcomes in 1984				
d. Reweighted Specification	44.26	37.30	9.14	9.27
e. Fixed Effects Specification	48.99	38.71	8.83	9.03
Treatment Effect Magnitudes				
	How much did LSPs raise outcomes compared to the counterfactual?			
f. Reweighted Specification: (b-d)/d	26%	9%	24%	18%
g. Fixed Effects Specification: (b-e)/e	14%	5%	28%	22%
	How much of the change in outcomes can LSPs explain?			
h. Reweighted Specification: (b-d)/c	31%	15%	36%	30%
i. Fixed Effects Specification(b-e)/c	18%	9%	41%	34%

Notes: To calculate counterfactuals we subtract the event-study estimates from observed outcomes by county. Columns (1) and (2) contain averages for treated counties only and all counties in Sample 2, and columns (3) and (4) contain averages for treated counties only and all counties in Sample 4.

Table 6. Estimated ITT Effects of LSPs Stratified by State Divorce and Two-Parent Welfare Policy

	(1) Divorces per 1,000 Women	(2) AFDC Cases per 1,000 Women	(3) Nonmarital Births per 1,000 Women	(4) Divorces per 1,000 Women	(5) AFDC Cases per 1,000 Women	(6) Nonmarital Births per 1,000 Women
	A. Early No-Fault Divorce States			*C. AFDC-UP States*		
Shorter-Run Post-LSP						
Years 0 to 5	0.96	3.75	0.59	0.49	4.34	0.31
	[0.31]	[2.09]	[0.21]	[0.17]	[0.93]	[0.18]
Longer-Run Post LSP						
Years 6 to 13	0.45	4.99		-0.17	10.61	
	[0.39]	[3.85]		[0.51]	[2.15]	
	B. Late No-Fault Divorce States			*D. Non-AFDC-UP States*		
Shorter-Run Post-LSP						
Years 0 to 5	0.18	3.97	0.64	0.31	1.74	0.63
	[0.2]	[1.03]	[0.17]	[0.4]	[1.94]	[0.30]
Longer-Run Post LSP						
Years 6 to 13	-0.45	12.12		0.24	4.67	
	[0.48]	[2.27]		[0.65]	[3.72]	
H_0: Equal shorter-run coefficients	0.03	0.93	0.86	0.68	0.23	0.36
H_0: Equal longer-run coefficients	0.15	0.11		0.62	0.17	

Notes: The table presents estimated effects from the reweighting specification stratified by state policy characteristics. Panels A and B split the sample into states with no-fault divorces laws in 1970 or earlier (12 states) and those that introduced no-fault divorce after 1970 (37 states). No-fault divorce simplified the legal process for obtaining a divorce, making it easier for LSP lawyers, for example, to perform divorces (Wolfers 2006). Panels C and D split the sample into states that had an AFDC-Unemployed Parent (AFDC-UP) program before 1981 (31 states, mostly implemented in the 1960s) and states that introduced AFDC-UP after a federal mandate in 1981 (18 states). AFDC-UP allowed benefits to be paid to families with both parents present and therefore meant that women need not be unmarried or non-cohabiting to receive assistance. ITT = intention-to-treat.

Table 7. The Effect of LSPs on Family Structure and Poverty, 1960–1970

	(1)	(2)	(3)	(4)
		Mother's Education		
	All	< HS	HS	BA
		A. Fixed Effects Specification		
Unmarried Head of Household	0.018	0.026	0.014	0.006
	[0.005]	[0.008]	[0.005]	[0.011]
	(.000)	*(.002)*	*(.008)*	*(.086)*
Living with the Father of Any Children	-0.020	-0.029	-0.015	-0.012
	[0.007]	[0.010]	[0.008]	[0.012]
	(.002)	*(.002)*	*(.086)*	*(.010)*
Poor	0.004	0.014	0.007	0.004
	[0.008]	[0.014]	[0.006]	[0.010]
	(.623)	*(.399)*	*(.423)*	*(.124)*
		B. Reweighted Specification		
Unmarried Head of Household	0.014	0.029	0.015	0.020
	[0.005]	[0.005]	[0.004]	[0.006]
	(.020)	*(.002)*	*(.012)*	*(.060)*
Living with the Father of Any Children	-0.015	-0.032	-0.017	-0.019
	[0.007]	[0.005]	[0.009]	[0.006]
	(.042)	*(.000)*	*(.016)*	*(.094)*
Poor	0.001	0.004	0.001	-0.002
	[0.006]	[0.008]	[0.005]	[0.007]
	(.305)	*(.337)*	*(.244)*	*(.541)*

Notes: The table presents difference-in-differences estimates using a sample of mothers living in 81 identified counties in 1960 (390,599 respondents) or 1970 (170,941 respondents). Panel A presents estimates from the fixed effects specification and Panel B presents reweighted estimates. Because there are so few counties, we use region fixed effects in Panel A instead of state fixed effects. We collapse the data to county-level changes and estimate a cross-sectional regression with a dummy for having an LSP by 1970 as the right-hand-side variable of interest. The underlying sample in column 1 uses all mothers, columns 2–4 use means calculated separately by mother's education. We recalculate the propensity score weights using the 1960 Census characteristics from Table 3 and 1960 levels of poverty, single motherhood, and the age distribution of mothers (in 5-year bins). Standard errors clustered by county are in brackets and *p*-values from 500 permutations of the treatment dummy are in parentheses (see Online Appendix E for permutation distributions).