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 operated subsidized legal clinics in poor neighborhoods, tackled previously taboo family cases such as divorce, and challenged public welfare bureaucracies. Using the roll-out of the program across 251 counties from 1965-1975 and newly entered data, we first show that LSPs increased divorce rates and increased welfare participation. The combined effect was to increase nonmarital birth rates due to falling marriage rates, not increasing birth rates. Local-level efforts to expand poor communities' access to legal institutions thus contributed, directly and indirectly, to the unprecedented changes in family structure in the 1960s.

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American families changed dramatically in the 1960s. As married women's employment and unmarried women's welfare participation skyrocketed (Moffitt 1987, Goldin 2006), mothers' income as a share of family income reach one-third by 1980; double the share in 1960. At the same time, marriage rates fell while divorces and nonmarital births increased (Lundberg and Pollak 2007). This "second demographic transition" is one of the most dramatic but poorly understood demographic trends of the 20th century (Bailey, Guldi, and Hershbein 2014). Gary Becker reflected in 1991 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, pg. 1).

Understanding what caused these changes, however, has proven difficult. The wide range of controversial 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), or the growth of welfare programs (Murray 1984). Empirical evidence on the broad trends in family structure tends to be correlational, while causal evidence (e.g. Hannan, Tuma, and Groeneveld 1977) focuses on small interventions that cannot explain family change at 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 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 policing and economic empowerment, and sued local bureaucracies perceived as treating the poor unfairly (Johnson 1977). Its originators believed that legal services "would possibly be the single most important thing…in the poverty program" (Pollak quoting Sargent Shriver in Gillette 1996).

LSPs directly served thousands of families in divorce cases and in disputes with local welfare departments, and indirectly expanded welfare access through advocacy and local litigation. 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.[1] They worked with welfare advocacy groups, for example by writing plain language "welfare manuals" urging poor families to apply for benefits (Davis 1993), and suing local welfare departments over eligibility restrictions.

This 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 (Rosenzweig 1999, Becker 1991). Furthermore, as outcomes like divorce, single parenthood, and welfare receipt became more common, their social costs may have fallen within families (Solon et al. 1988) and social networks (Bertrand, Luttmer, and Mullainathan 2000). In short, by making poor people's theoretical legal access a practical reality, the LSP had the potential to move communities to a new equilibrium in terms of welfare take-up and family formation.

To identify LSPs' community-level effects, we use a difference-in-differences research design and a semi-parametric event-study specification (Jacobson, LaLonde, and Sullivan 1993) based on the program's rollout to 251 counties between 1965 and 1975 (Cunningham 2016). We

[1] Sar Levitan (1969, pg. 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, pg. 65).

digitized outcome data on divorces, marriages, nonmarital births, and welfare participation by county from 1960 to 1988. Because the LSP roll-out was concentrated within 5 years, we 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 in the same state (Bailey and Goodman-Bacon 2015, Cunningham 2016). The second specification weights untreated counties by inverse propensity scores to create a control group with similar pre-treatment characteristics (Abadie 2005). Both specifications yield the same qualitative results, which supports the validity roll-out design.[2]

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. LSP establishment is associated with short-run increases of up to 17,000-23,000 divorces per year (under half of the 56,000 LSP divorce cases in 1968) that revert to zero once pent-up demand is met. LSP is associated with between 228,000 to 400,000 additional AFDC cases by the 1980s, matching historical accounts of its contribution to skyrocketing applications and appeals. We also find that LSP had an indirect effect on family structure. Nonmarital birth rates rose by 16 percent after LSP establishment, and the probability that mothers lived with the father of their children fell by 2 percentage points. This comes not from an increase in births, but from a decline in marriage. We view increased access to welfare programs restricted to single parents as the most likely mechanism.[3]

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[2] Fixed effects estimates are generally smaller than reweighted estimates, though, consistent with state-level factors, for instance, that make nearby counties more comparable than observationally similar counties from disparate parts of the country.

[3] 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 other LSP counties 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 2018b)—a noted catalyst for urban decay and white flight (Collins and Margo 2007, 2004). We also estimate null placebo

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 family change that occurred in the 1960s. Our results show that policy mattered in the 1960s: the War on Poverty contributed to increasing welfare use and nonmarital births. But these changes came from expanded legal access, not statutory changes in "generosity", as is commonly claimed. 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." 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 imagined is how this large shift in access would alter family structure.

I. THE NEIGHBORHOOD LEGAL SERVICES PROGRAM

Jean and Edgar Cahn, two attorneys with the Ford Foundation's influential Gray Areas program, first proposed that university-affiliated, neighborhood law firms serve as intermediaries between the community and antipoverty bureaucracies and provide free civil legal representation and advice. The Cahn's felt that legal services would supply "impoverished communities the means with which to represent the felt needs of its members" (Cahn and Cahn 1964, pg. 1334). Sargent Shriver, head of the Office of Economic Opportunity (OEO), agreed predicting that the program would have "a far-reaching and continuing effect on the distribution of power in the society" (Pollak quoted in Gillette 1996, pg. 253). In 1965 the OEO issued over 155 LSP grants worth 20 million (nominal) dollars. By 1968 the LSP doubled in size and by 1975 it had rolled out to 273 counties in 48 states. Figure 1 maps the rollout of the LSP.

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effects for Community Health Centers, a program that likely shares confounding unobservables with LSPs, and our effects are similar when we compare to counties that received LSPs in different years.

LSPs clearly expanded the quantity of legal services available to the poor. Figure 2 plots the number of cases handled by traditional legal aid societies and LSPs from 1905-1971.[4] Aid societies grew to handle about 300,000 cases after 60 years. In contrast, the LSP handled 282,000 cases in 1968 alone and over a million by 1971. The average center employed 5 lawyers who each worked hundreds of cases per year (Cunningham 2016).

LSPs almost certainly provided *new* services, rather than crowding out existing legal aid.[5] The OEO-funded facilities served populations and handled topics that existing legal aid societies had been reluctant to take on such as divorce and bankruptcy. Grant recipients opened new law offices in poor neighborhoods with expanded hours of operation to increase accessibility.[6] LSPs served poorer clients, a high share of black women, and challenged public officials more than traditional legal aid societies (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 (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

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

[5] 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 roll-out by providing cheap labor in the form of newly trained lawyers, designing new curriculum in poverty law, and sometimes operating legal services offices directly (Johnson 2014, Cunningham 2016).

[6] 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).

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 rights to obtain a divorce as the rich and that these efforts protected poor women's economic interests (Foster and Freed 1967).

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 office and caseworkers exercised wide and often arbitrary discretion over benefit amounts, acceptances, 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*).[7]

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).[8] One welfare official testified in the Senate Appropriations committee in 1968 that "the OEO neighborhood legal attorneys are requesting more fair

[7] LSP leaders split over whether centers should provide day-to-day assistance to clients on simple issues (the Cahn's position) or vet clients for potential Supreme Court test cases (the position of Ed Sparer, head of Mobilization for Youth's legal unit). Sparer established the Center for Social Welfare Policy and Law at Columbia University's School of Social Work in 1965 to provide this kind of back up support for test cases (Davis 1993, pg. 34).
[8] 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.

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 pg. 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 appendix 2).

A. Expected Effects of LSP

This history suggests that LSPs should increase divorce rates, but only temporarily. For one, LSPs will 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" (pg. 53).[9] Second, the initial burst of divorces should occur among marriages most likely to break up. Therefore, the at-risk population shrinks and becomes more positively selected, both of which should reduce observed divorce rates.

We expect LSPs to increase AFDC participation (a stock) substantially. Local restrictions on eligibility and arbitrary caseworker decisions had become the norm in AFDC by the early 1960s. 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,

[9] 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). It also cited a similar experience soon after legal services began in the UK: "When England started its program in 1950, 80 per cent of the clients wanted divorces. Since then the rate of matrimonial disputes among English legal aid cases has declined to about 40 percent." Also see the discussion in Wolfers (2006) in the context of unilateral divorce.

had the same effect for a much larger class of people. LSPs simultaneously increased the 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 potentially eligible families, even those not served by LSPs (Ashenfelter 1983, Moffitt 1983).[10]

Finally, many theoretical models predict that by making public assistance a reliable and available source of income LSPs should have changed decisions about whether to form (or dissolve) marriages (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.[11] By expanding access to welfare benefits, LSPs increased expected public assistance income for poor women only if they raised children on their own. 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).[12] Growing prevalence of welfare participation, divorce, and single motherhood can also have a feedback effects by changing their social costs. In fact, Americans became much more accepting of these choices in the 1960s (Thornton 1989).

[10] 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).
[11] 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.
[12] Ethnographic studies support this. Stack (1974, pg. 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."

Figure 3 highlights how quickly these exact outcomes changed in the 1960s. Singleness among mothers remained constant from 1880-1960, but increased sharply after 1960, quadrupling from 10 percent to 40 percent by about 2010. At no other point in the 20th century has the relationship between fertility and marriage changed so much (Bailey, Guldi, and Hershbein 2014).

Of course this period coincides with many other trends that likely affected family structure. These include a lack of "marriageable" black men (Cox 1940, Wilson 1987),[13] the dissemination of oral contraception (Akerlof, Yellen, and Katz 1996),[14] second-wave feminism (Chafetz 1995), liberalized divorce laws (Wolfers 2006), and growing social welfare expenditures.[15] Despite the prevalence of hypotheses, we have little strong evidence about the root causes of the dramatic shift in American family structure—especially among poor women.[16] Many of the proposed hypotheses, however, relate closely to the law. Therefore, the LSP has a plausible role in sparking widespread change in family structure during this period.

[13] Research consistently shows a negative relationship between single-parenthood and men's economic circumstances (Autor, Dorn, and Hanson 2017, Kearney and Wilson 2017) or incarceration rates (Charles and Luoh 2010).

[14] Oral contraception increased women's human capital investments and earnings (Bailey 2006, Goldin and Katz 2002), and marriage rates tend to fall when women do better (Bertrand, Kamenica, and Pan 2015, Shenhav 2018).

[15] Studies that use area fixed effects find no relationship between short-run changes in family structure and changes in benefits (Hoynes 1997, Moffitt 1994). For non-benefit policies, Kearney (2004) finds no effect of family caps, and Bitler et al. (2004) find that welfare reform reduced divorce and marriage rates. Rosenzweig (1999) argues that it is not clear that "decisions about childbearing, given their long-term consequences, should be particularly sensitive to yearly variations in welfare program benefits." The LSP may affect families by creating a plausibly permanent change in access to benefits.

[16] Debates over family structure frequently focus on race. Daniel Patrick Moynihan's 1965 report, for example, 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 ratio of black to white poverty rates in 1960 was 3.8 for married mothers and 1.7 for unmarried mothers. 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 (see below). Therefore, an analysis of LSPs effect on race-specific family structure is beyond the scope of this paper.

II. DATA: 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 LSP. To address this, we entered new county-by-year data on family structure outcomes and welfare participation (see appendix 1 for 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 create a LSP treatment indicator equal to one starting in the year that counties and years received their *first* grant for "legal services." We choose not to use a continuous measure of LSP funding to avoid bias from subsequent grant decisions that either support successful centers or compensate for failing ones. Table 2 shows the distribution of LSP start dates across the 273 counties that received federal legal service grants up to 1975. 205 received funding in either 1966 or 1967.

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).[17] 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 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

[17] 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.

outcomes are age-adjusted (using the national female distribution in 1960) nonmarital births per 1,000 women ages 10-49.

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 1968-1988. Reports after 1980 only include counties in 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. They are 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. We calculate the probability that mothers are unmarried heads of household; live with the father of their children; and are poor.[18] We collapse these outcomes to county-year averages and then take the 1960-1970 difference so that all Census models have 81 observations.

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*). We then drop counties that fail to report marriages and divorces in every year from 1959-1988 (345 counties) or AFDC cases in every *available* year from 1960-1980 (10 counties). This yields a balanced panel of marriage and divorce from 1960-1988 and AFDC in every *available* year from 1960-1980 for 2,683 counties that contain 94 percent

[18] 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.

of women in the US. The second sample includes the 603 SMSA counties with AFDC rates through 1988 (71 percent of all women). The third sample includes 112 counties with nonmarital births in every year from 1960-1980 (23 percent of women). The fourth sample in our county-by-year estimates includes the 60 counties with nonmarital births from 1959-1988 (20 percent of women). The final sample includes the 81 counties identified in the 1960 and 1970 Censuses (36 percent of women).

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

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

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

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.[19] The first sum includes pre-LSP event-years so that the π_j estimate pre-treatment trends in outcome y_{ct}. The second sum includes post-LSP event-years. Because we observe data at the county level, the ϕ_j 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: over 98 percent of the identifying variation comes from LSP/non-LSP comparisons (see theorem 1 in Goodman-Bacon 2018b). Therefore, our identifying assumption is essentially

[19] We omit the event-study dummy for the year before LSP treatment ($j = -1$).

common trends between treated and untreated counties rather than common trends between all sets of counties that received LSP funding in different years.[20]

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 Legal Services Program from 1966-1968, "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, pg. 7).

Still funds mainly went to cities because of the OEO's focus on urban poverty (Bailey and Duquette 2014) and the need to hire trained lawyers. Columns 1 and 2 of table 3 show strong 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.

In response to this imbalance we take two complementary approaches to creating 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 for seven bins of the 1960 urban share, which restricts comparisons to similarly urban

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

counties. This controls for the common effects of state-level policy changes (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 (*p*-values are 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).[21]

The second model creates a control group that is balanced on pre-treatment characteristics. We estimate propensity scores, $\hat{p}(x_c)$, from a probit model for the probability that counties receive a LSP grant as a function of all the covariates listed in table 3, and 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 pre-treatment 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, except in the urban share which differs by only 3.6 percentage points. The final row shows after reweighting, pre-treatment characteristics do not jointly predict LSP treatment status (*p*-value = 0.58).

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[21] Section V also reports results from specifications that only rely 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 exactly the same process as LSPs.
[22] We recalculate propensity scores and weights for each estimation sample. Appendix Figure A3.4 shows the propensity score distributions, and appendix Figure A3.5 scatters the propensity scores from different samples against each other. The inclusion of pre-treatment levels and trends in outcomes is similar to the synthetic control estimator of Abadie, Diamond, and Hainmueller (2010). Goodman-Bacon (2018a) 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, though, so this is not a major limitation.

IV. INTENTION-TO-TREAT ESTIMATES

Figures 4 through 8 plot pre-treatment effects, π_j, and post-treatment effects, ϕ_j, from equation (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 effects for years 0-5 and 6-13 as well as *p*-values from 500 random permutations of the LSP treatment holding the distribution of treatment dates constant (see appendix figures A4.1-A4.3 for distributions).

A. Divorces

Figure 4 shows that divorce rates have a hump-shaped response to LSP establishment, rising initially and then falling after about eight years. Consistent with our 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 5.5-7.7 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 (s.e. = 0.16, permutation *p*-value = 0.114) in the reweighted specification and 0.34 (s.e. = 0.16, permutation *p*-value = 0.142) 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.

[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 being providing series. 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).

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 short-run results suggest increases of at most 3.3 to 4.6 divorces per 1,000 *poor* women. Rescaling the event-study coefficients this way and summing them implies that in their first seven years of operation LSPs could have led to between 1.7 and 2.5 percent of poor women to get divorced. By comparison, the share of poor women who were currently divorced rose from 5.1 percent to 8.5 percent between 1960 and 1970 (and this understates the probability of ever divorcing because some women remarry). These upper-bound calculations suggest a major role for LSPs in the short-run surge in divorces.

LSPs could have easily handled this number of divorces. The average treated county in our sample had 122,000 women between the age 10 to 49 when LSPs began operating, so the largest ITT estimates come from just 63 to 86 additional divorces per year. The average LSP lawyer, by comparison, handled 50-100 new cases each month. Summing across 273 treated counties implies that at its peak, LSP caused 17,000 to 23,000 divorces per year; 30-40 percent of the 56,000 divorces they handled in 1968 (one-fifth of 282,000 cases; Levitan 1969).

That these patterns match historical reports of large pent up demand as well as findings from other research on changes in divorce access (Wolfers 2006) supports a causal interpretation. These findings also reflect known LSP activities, so they act like a "first-stage" suggesting that our reduced form identification strategy successfully picks up 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 reweighted estimates on the set of counties observed through 1988) show evidence that these changes come from pre-existing trends. AFDC cases rise gradually after LSP establishment and stabilize after 9 years. Table 4 reports longer-run effects (years 6-13) of 6.55 cases per 1,000 women (s.e. = 1.53, permutation *p*-value = 0.000) in the fixed effects specification and 10.25 cases (s.e. = 1.90, permutation *p*-value = 0.000) in the reweighted specification.

Table 5 puts these magnitudes in context by using the estimated effects to calculate counterfactual outcomes in 1984. In treated counties, 56 women per 1,000 received AFDC in 1984, and the LSP treatment effects imply a counterfactual rate of between 44 and 49 women (column 1). This suggests that LSPs raised AFDC rates by between 7 and 12 women per 1,000, a 14-26 percent increase over the counterfactual. Scaling by the actual change in AFDC rates from 1964-1984 (37 women per 1,000) suggests that LSP explain 18-31 percent of the observed growth in treated counties. We repeat these calculations in column 2 for all 603 AFDC counties in SMSA which mechanically makes LSP's contribution smaller. Still, we find that LSP could explain up to 15 percent of the sample-wide growth in AFDC rates.[25]

As with the divorce results, rising AFDC rates match closely with high profile activities undertaken by LSP attorneys. Table 6 is comparable to Table 5 but scaled to reflect case counts instead of rates. It shows that LSPs generated 249,000 to 424,000 additional AFDC cases by 1984;

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[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 state courts or through threat effects on regulations. Some states immediately changed rules and regulations to avoid going to court (Champagne 1974).

900-1,550 in the average treated county. These are plausible effects over 10-20 years. Fair hearings, one 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 over 86,000 in the first six months of 1975 (National Center for Social Statistics 1976b). Applications, another choice influenced by LSPs via induced changes in bureaucratic behavior, increased information, or word-of-mouth effects, 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). Figure 5 therefore helps explain the sudden increase in welfare participation and supports our research design because of the correspondence between post-LSP growth in welfare and 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 six years before LSP establishment in either specification or sample. The pattern of ITT estimates is almost identical to the AFDC results in Figure 5: nonmarital births grow gradually at first but stabilize after about 8 or 9 years. Columns 5 and 6 of table 4 show short-run increases of 0.34 nonmarital births per 1,000 women (s.e.=0.15, permutation p-value = 0.018) in the fixed effects specification and 0.62 nonmarital births (s.e.=0.14, permutation p-value = 0.004) in the reweighted model; 6.5 and 12 percent over the baseline mean of 5.2.[26]

[26] Appendix Figure A4.7 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 eight 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.

Table 5 shows that these changes explain 30-44 percent of the growth in non-marital births in the 50 treated counties observed through 1988, and 19-31 percent of the change in all counties in the long sample. About 20 percent of 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 LSP 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 on nonmarital birth flows and compare to the effect on AFDC stocks. The fixed effects specification shows an increase in nonmarital birth rates of 0.34 in year 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 worked on behalf of current AFDC recipients and boosted take-up existing eligibles, only part of the LSP-induced growth in welfare comes from new nonmarital births.

D. Why did nonmarital births change?

For births to unmarried women to increase, either fertility must go up among women who would have not married, or marriages must fall among women who have already conceived (or both). 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 explains why we find increasing nonmarital births: after LSPs begin operating, pregnant women forego marriage. Lower marriage rates also help explain falling fertility rates overall. Since martial fertility exceeds nonmarital fertility, lower marriage rates should lead to fewer births.

Consistent with this, evidence from the Census sample (discussed below) in Appendix Figure A5.3 shows that lower-educated mothers are less likely to have more than one child.

Figures 6 and 7 are consistent with the predicted effects of increased welfare availability. Many models imply that LSPs should reduce the probability that unmarried women who become pregnant get married. In unitary models, welfare means that women do not need to marry for financial stability (Neal 2004). In bargaining models, some fathers refuse to marry knowing that single mothers can rely on welfare and will do so when children are public goods within a household (Willis 1999). Theoretically, by raising the expected value of single parenthood conceptions could increase, but we find no evidence of this.

E. Heterogeneity by State Divorce and Welfare Policy

If the short-run increase in divorces in Figure 4 came from LSP attorneys, we would expect the magnitude to be larger in states with legal environments, unilateral divorce specifically, that made it easier for poor families to request divorce cases and for LSPs to finish them. Panels A and B of table 7 report results that split the sample into late reform (after 1970) and early reform states (before 1971).[27] 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.

Similarly, if access to welfare conditioned on single parenthood caused family structure changes, we would expect larger welfare effects and smaller nonmarital birth effects in states where married mothers could get welfare. Women who could apply for the AFDC-UP program

[27] Twenty seven state switch to a no-fault regime between 1968 and 1977. Oklahoma change divorce laws in 1953. Divorce laws are from Wolfers (2006).

did not need to divorce or forego marriage altogether to receive aid. Therefore women who would always have married can get welfare (larger welfare effects) and women on the margin of being single parents in non-UP states could have remained married (smaller nonmarital birth effects). This is exactly the pattern we find in panels C and D of Table 7: 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 provide a unique ability to estimate pre-trends and dynamic ITT effects, but we cannot infer anything about living arrangements or other sources of household income. This section addresses these limitations using our Census sample.

Table 8 shows that LSP establishment is correlated with changes in living arrangements and stock measures of family 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.

Figure 8 shows the effect of the LSP on the distribution of mother's income by source. We find clear increases in low levels of unearned income, and the pattern almost perfectly matches the annualized distribution of AFDC benefits from 1967 administrative data (see appendix Figure A5.2). Earned income falls to some extent, although these findings are quite imprecise. Notably, other family income, which primarily consists of earnings of other family members, 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 8 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, though, 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 comparison, we reproduce the estimates from Table 4 in the first two rows.

A. Different Ways to Use LSP Timing

Three-quarters of treated counties received their first LSP grant in 1966 or 1967. Unmeasured determinants of family structure that changed sharply in 1966 or 1967, 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 would have experienced the upheaval of the 1960s differently under any circumstances. Row 4 presents estimates from a "stacked DD" strategy that matches 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 (Deshpande and Li 2017, Fadlon and Nielsen 2015).[28] This yields short-run DD estimates for early LSP counties that only compare treated counties to future-treated counties. Reassuringly, restricting comparisons to counties chosen by the OEO does not change our short-run estimates.[29]

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 2004), white flight, and 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.[30]

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 population data from the 1930-1990 Censuses (Haines and ICPSR 2010) to test whether treated counties experience changes in the relative supply of men. We find no change in sex ratios after the 1960s, when almost all LSPs rolled out, either in the decadal point estimates or in linear trends fit to the pre- and post-1960 data

[28] Our regression interacts county and year fixed effects with indicators for each "stack" defined by t^*.
[29] Appendix Table A4.1 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. Appendix Figures A4.4 – A4.6 also shows 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.
[30] 76 out of 112 counties in the short-run nonmarital birth sample experience a riot, so we include time-to-first-riot dummies in Figure 12 instead of dropping observations. Panel A of appendix Table A4.2 shows that dropping the counties in the highest quintile of growth in their black share, a consequence of riots, does not alter our estimates.

points. At least on an aggregate level changes in marriage markets due to the supply of men cannot bias our results.[31] 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.[32] 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 Programs (CAP), Head Start, Community Health Centers, 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 controls for time-to-first-CAP dummies and our main estimates do not change.[33]

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[31] In appendix 3 we test whether our aggregate analysis masks race-specific effects of premature mortality or incarceration (see Hinton 2016). We find no evidence of relative changes in race-specific sex ratios either.

[32] Appendix Figure A3.3 shows a gradual reduction in log employment that does not begin until six years after LSP establishment. Appendix Figure A4.4 uses the Census sample to estimate reweighted distributional effects on men's earnings as in Figure 8. Neither all men aged 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.

[33] 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 sharply changing unobservable determinants of family structure that are correlated with community decisions to apply for programs in general or OEO decisions to award them, this sample restriction should eliminate our effects. In fact they do not change.

E. The National Welfare Rights Organization

Another possible explanation is that our results confound the effect of LSPs with the independent effects of the welfare rights groups they helped; 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 held in the archives of NWRO's founder George Wiley. Row 7 shows that our results are robust to controlling for time-to-first-NWRO dummies.[34] LSPs work on behalf of WROs is a likely mechanism, but the welfare activism occurring more broadly cannot explain our results.[35]

F. Placebo Treatment: CHCs

The final row uses a similar War on Poverty program, Community Health Centers, 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

[34] Row 8 adds controls for riots, CAPs, and NWROs and again finds negligible changes in our main estimates.
[35] These are not admissible controls if LSPs causally affect the establishment of WROs. If, on the other hand, WROs spring up independently but LSPs make them more effective through their legal counsel, these estimates help net out the effect of a WRO alone. Appendix 2 provides archival evidence on how LSPs and WROs worked together that is consistent with the second explanation.

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 LSPs.

VI. Discussion: LSP Effects in Historical Context

We argue that our evidence supports 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 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. LSP as a Cause of Family Change in the 1960s

Debates over the causes of single-parenthood and its sudden explosion in the 1960s have continued for at least 50 years. In his controversial 1965 report, Assistant Secretary of Labor Daniel Patrick Moynihan argued that a self-perpetuating "tangle of pathology" generated 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 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).[36]

[36] Both explanations already had a long history by the 1980s. Skocpol (1992, pg. 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

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 results support the general conclusion that policy mattered, and highlight the previously overlooked legal services program. By conducting divorces and expanding access to welfare programs, LSPs appear to have changed take-up behavior and marriage decision of poor pregnant women.

These calculations still leave an important role for other factors, such as changing contraceptive technology, which almost certainly interact with LSPs. LSPs tended not to raise nonmarital birth rates in states where two-parent families could get welfare, for example. Male earnings and their desirability as partners may also interact with the package of treatments that LSPs represented. Stack (1974) recounts a father who partly moved out of his house after a job loss to allow his family back “on aid.” LSPs led to large increases in AFDC participation, but individual take-up, marriage, and cohabitation decisions almost certainly respond to relative benefits of welfare versus male earnings (Neal 2004).

B. Welfare as a Mechanism

We view the incentives created by AFDC and unlocked by LSPs as a key explanation for our nonmarital birth results. The two outcomes respond similarly to LSP establishment, while AFDC effects are larger but nonmarital birth effects are smaller in states where married women could receive welfare. Family structure changes were also largest for lower educated mothers for whom welfare was more common. Finally, we find that these changes came from marriage decisions not

“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).

fertility decisions, consistent with large changes in shotgun marriage for these cohorts but flat rates of premarital conceptions (England, Wu, and Shafer 2013).

These findings also help reconcile clear theoretical predictions that a benefit specifically for single parents should raise single parenthood, with a large body of empirical research that fails to find credible evidence of such an effect. Becker (1991, pg. 357), for example, concludes that welfare "raises the fertility of eligible women, including single women, and also encourage divorce and discourages marriage." Early cross-sectional empirical research compared family structure in higher benefit states to lower benefit states (e.g. Honig 1974) and appeared to support this prediction. But subsequent research used state fixed effects to address latent correlations between family structure and welfare benefits and found no effect of *changes* in generosity on family structure (Hoynes 1997, Moffitt 1994). Ellwood and Bane (1985) concluded that "welfare simply does not appear to be the underlying cause of the dramatic changes in family structure of the past few decades."

Our results suggest that changes in the ease with which poor women could actually receive welfare programs can help explain this discrepancy. Local restrictions on eligibility and arbitrary caseworker decisions had become the norm in AFDC by the early 1960s. Families did not respond to statutory changes in benefits because the probability that they could get and keep those benefits appeared low. Furthermore, 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.

We document, both historically and empirically, that the LSP 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. Setting new precedents for the way welfare bureaucracies treated recipients, 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 a uncertain benefits suggested.

It is important to note the potential interaction of LSPs effects on welfare with other changes in marriage markets. Hinton (2016) argues that many War on Poverty programs laid the groundwork for mass incarceration decades later but increasing surveillance in poor communities. Cunningham (2016) shows that LSP establishment led to higher arrest rates by improving community/police relations and increasing crime reporting. While our we find no evidence that these changes altered sex ratios in the way that prison policy did in the 1980s, these trends may have affected the quality of men in local marriage markets, possibly nationwide. As Neal (2004) emphasizes, the marriage market and safety net policy interact to determine family structure. 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.

[37] Our findings also help explain the "structural shift" in welfare participation measured in the late 1960s. Moffitt (1987) concludes that "the major nonquantifiable alternative explanations are two: (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.

C. What does an LSP effect mean for policy?

Public discourse and policy have rightly focused on the unprecedented changes to American families in the last 50 years, but different interpretations of these changes have led to 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, Daniel Patrick Moynihan advocated a federal job guarantee and then a basic income in order to address rising single motherhood (O'Connor 2001).

Decades later, an extreme interpretation of the 1960s attributes the growth in single parenthood to the growing generosity of the cash and in-kind safety net. Murray (1993, p. S225), summarized the perception by saying "that the two phenomena are linked seems too plausible to require more proof." He proposed "scrapping the entire federal welfare and income-support structure for working-aged persons" (Murray 1984, pg. 227). Ultimately, 1996's Personal Responsibility and Work Opportunity Reconciliation Act did dramatically shrink cash 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."[38]

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

[38] 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.

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 Service Program went through sweeping changes with new oversight from the newly created Legal Service Corporation (LSC). By 1980, tenant-landlord disputes dominated the caseloads of poverty lawyers and reform cases became essentially non-existent. But even more so, it is hard to imagine a plausible, legal, or ethical policy that re-imposes 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.

D. What did LSP mean for well-being?

We cannot draw conclusions about how the changes we document matter for well-being. There are reasons to believe these changes are make 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 is the "strongest correlate of upward income mobility" across neighborhoods (Chetty et al. 2014).[39]

On the other hand, LSP relaxed poor families' budget constraints and altered their *choices*. Evidence from unilateral divorce reforms, for example, show that increased access to divorce

[39] 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, pg. 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, pg. 4) argued that the "most important determinant of poverty is family structure".

raised divorces but reduced female suicide rates. All parties may be better off if marginal marriages break up. Moreover, easier access to welfare and divorce changes bargaining within marriages that did not break up, and we cannot measure well-being among these couples. One way to examine how LSP affected well-being among children would be to observe their adult outcomes. We leave this to future work.

VII. CONCLUSION

In summary, we provide the first evidence on the effect of federally subsidized legal assistance on welfare and family structure during the 1960s. Our results suggest that improved access to legal advice and representation increased divorce in the short run, led to a permanent shift up in welfare receipt, and 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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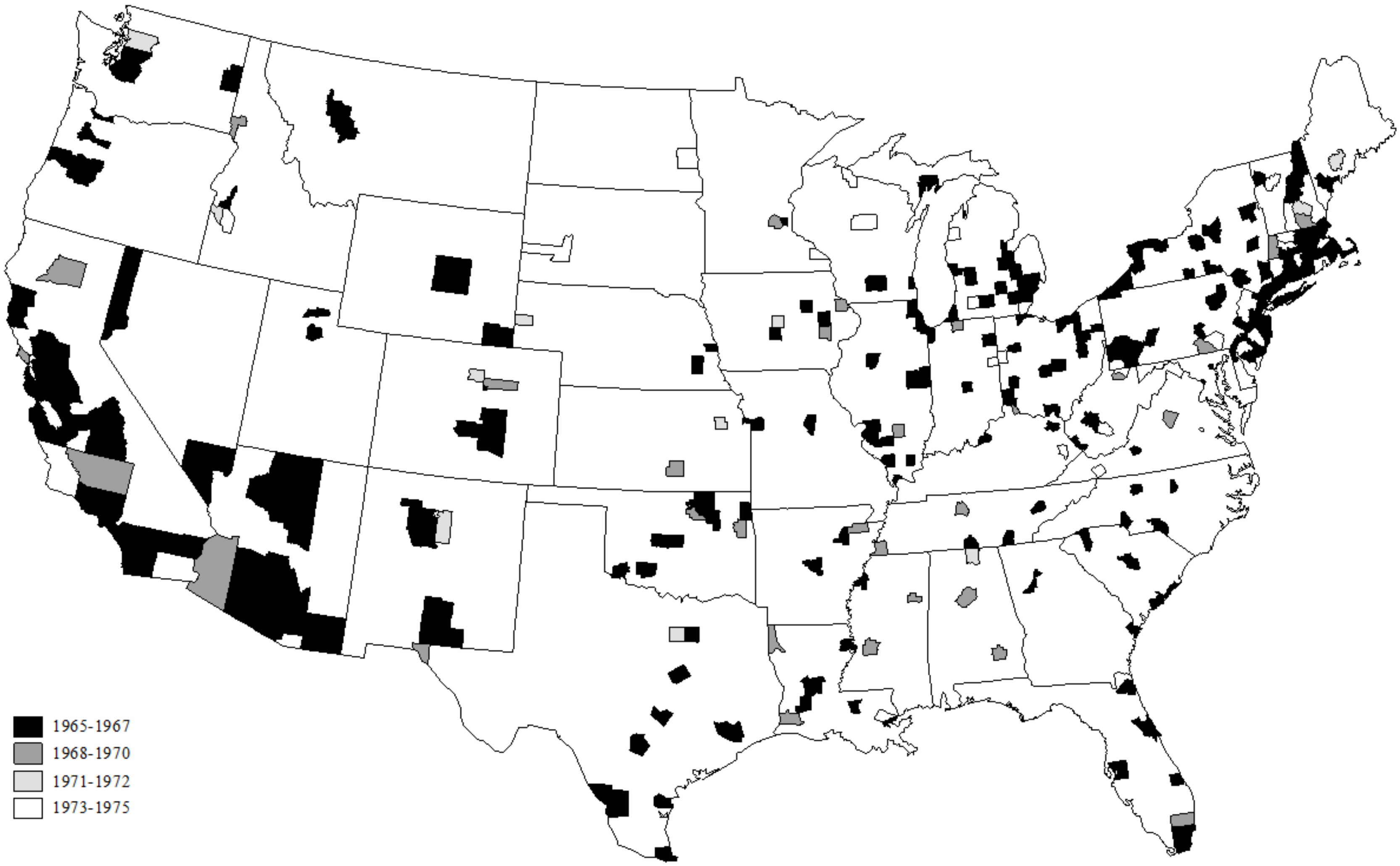
Figure 1. Establishment of Neighborhood Legal Services Programs by County, 1965-1975

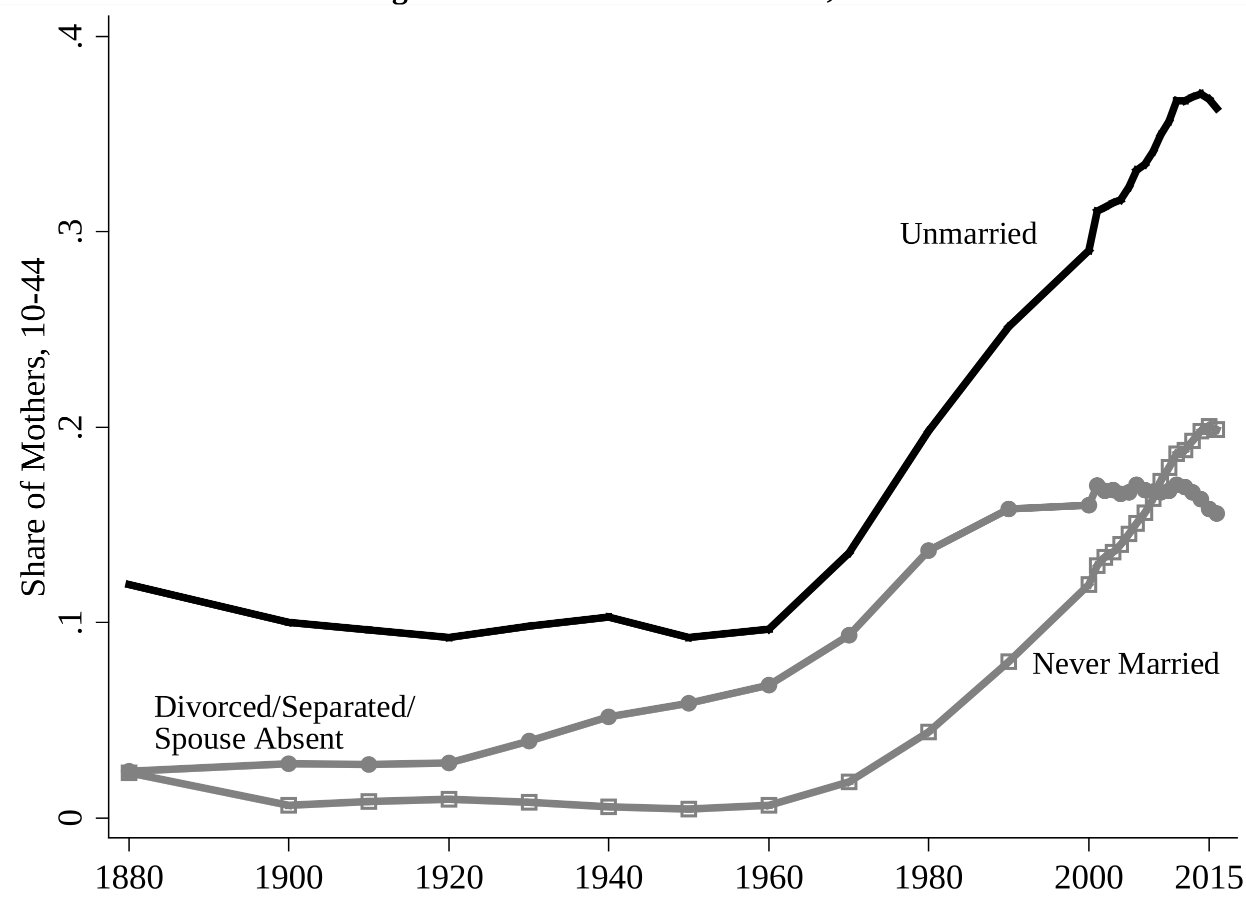


Source: National Archives Community Action Program files and Cunningham (2016).

Figure 2. Civil Cases Handled by Legal Aid Societies and OEO Legal Services Programs, 1905-1971

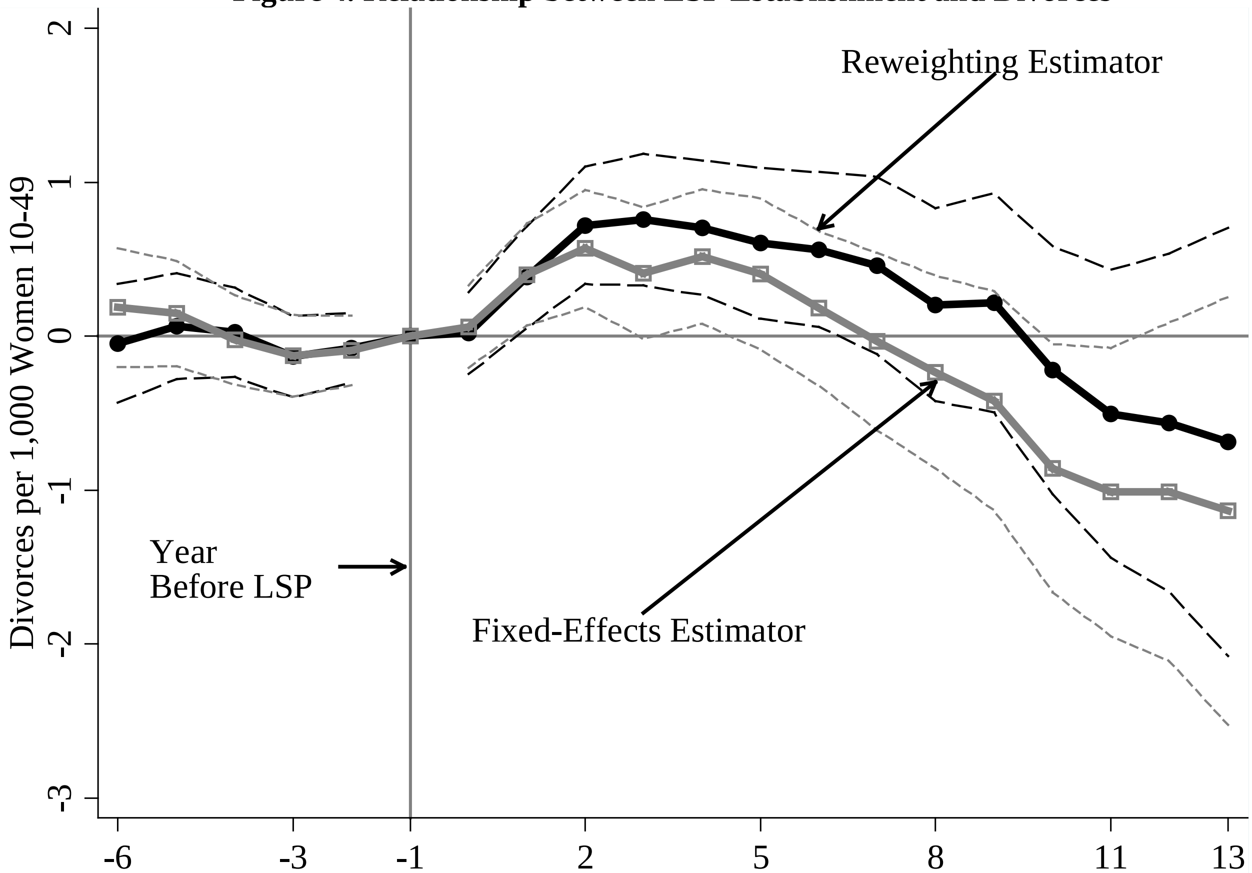
Notes: Data on legal aid cases were entered from Brownell (1951) and Brownell (1971) except for the estimated number of cases in 1965, which comes from a report by the John D. Ketelle Corporation (1971). Data on LSP cases for 1967-1971 were taken from Congressional testimony by Donald Rumsfeld.

Figure 3. Mothers' Marital Status, 1880-2016



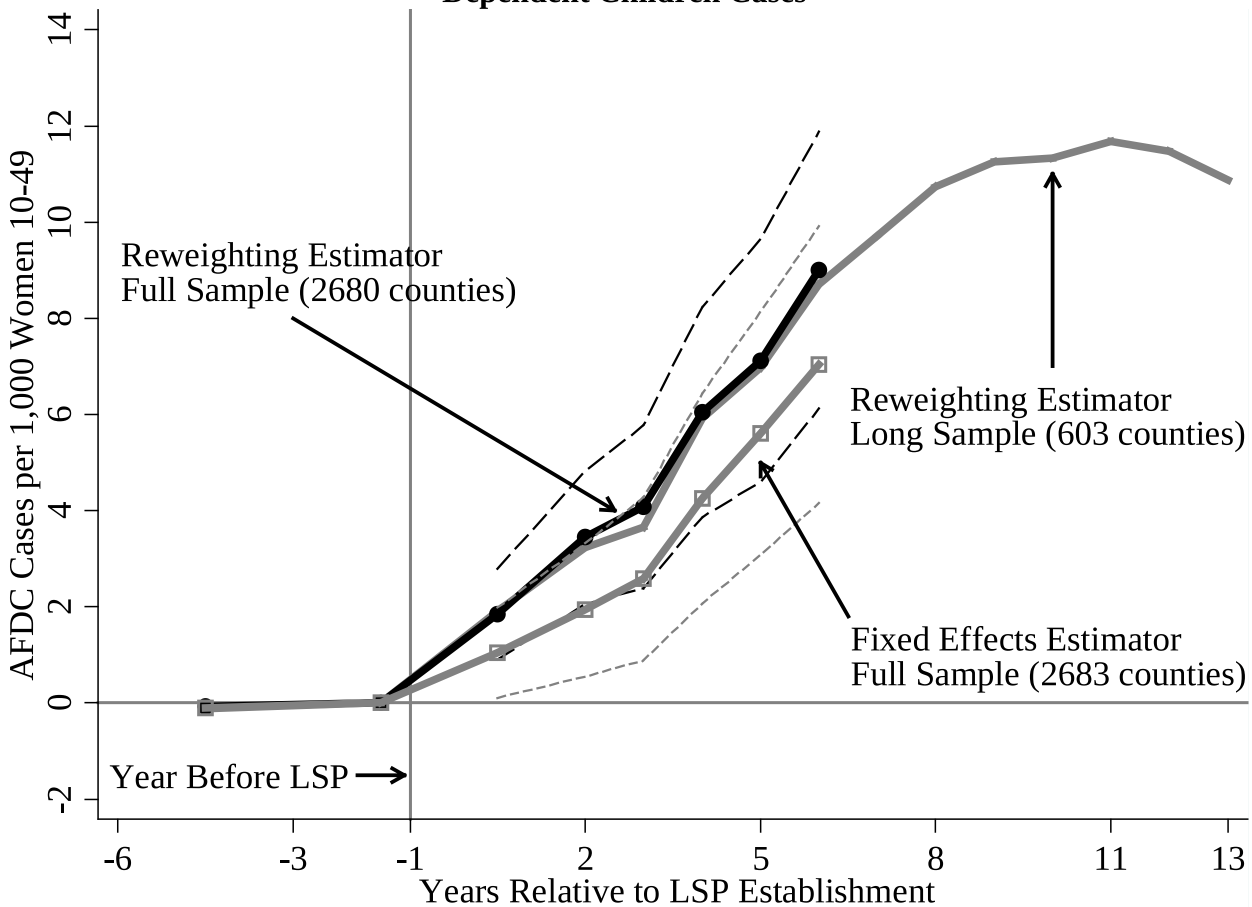
Notes: The figure plots the share of mothers in the Census and American Community Survey who are between ages 10 and 44 and who report different marital status. We group married women with an absent spouse with divorced and separated because "separated" was only added as a separate category in 1950. 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. Source: Ruggles et al. (2010)

Figure 4. Relationship between LSP Establishment and Divorces



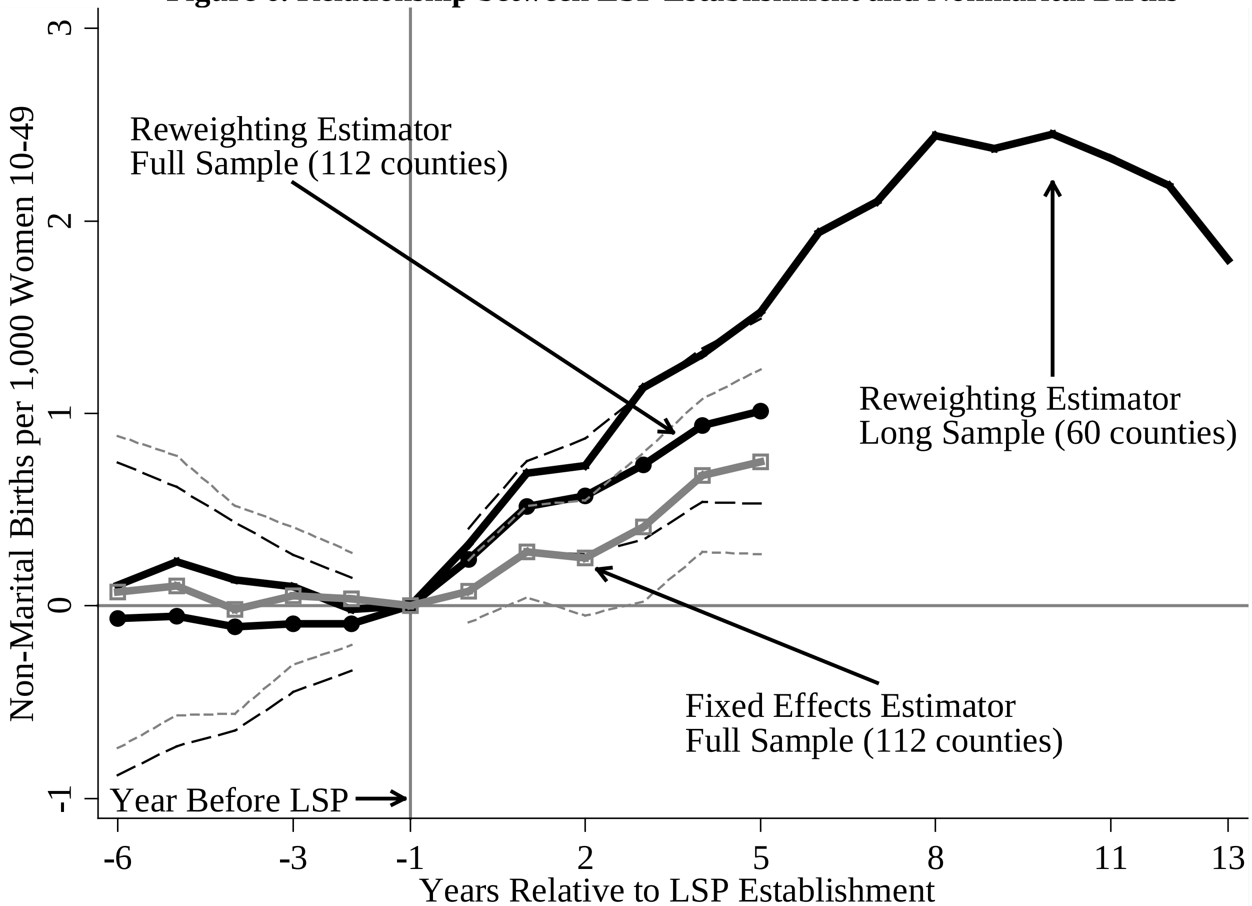
Notes: The dependent variable is the number of divorces that occur in county c and year t divided by the number of women ages 10-49 measured in thousands. The average dependent variable in treated counties in the year their LSP starts is 9 divorces per 1,000 women. The figure plots event-study estimates 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. The sample includes 2,683 counties.

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



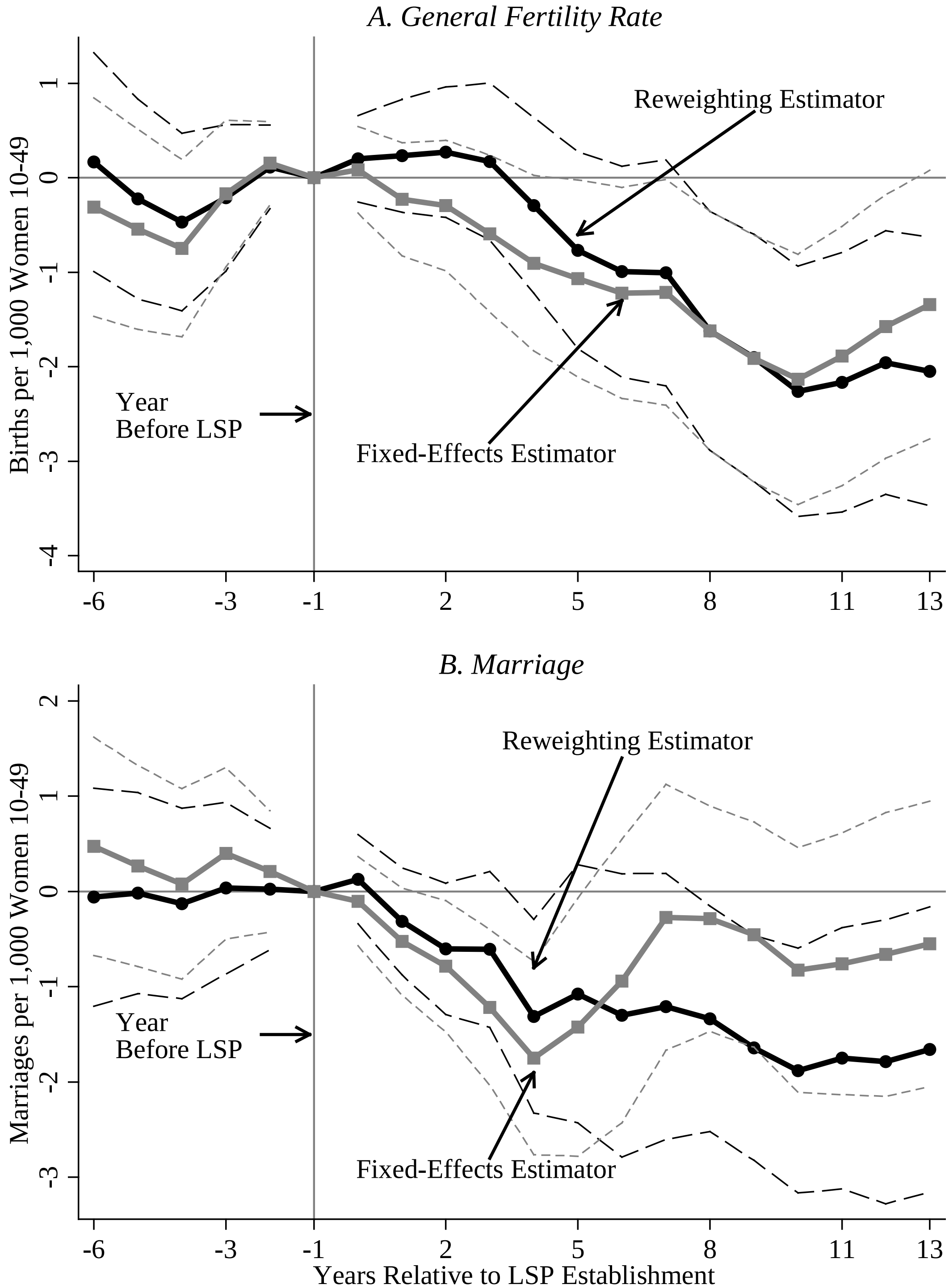
Notes: The dependent variable is the number of open AFDC cases in county c and (a given month of) year t divided by the number of women ages 10-49 measured in thousands. The average dependent variable in treated counties in the year their LSP starts is 23 cases per 1,000 women. See notes to Figure 4 for details on the specification. The full sample includes 2,683 counties and the long sample contains 603 counties.

Figure 6. Relationship between LSP Establishment and Nonmarital Births



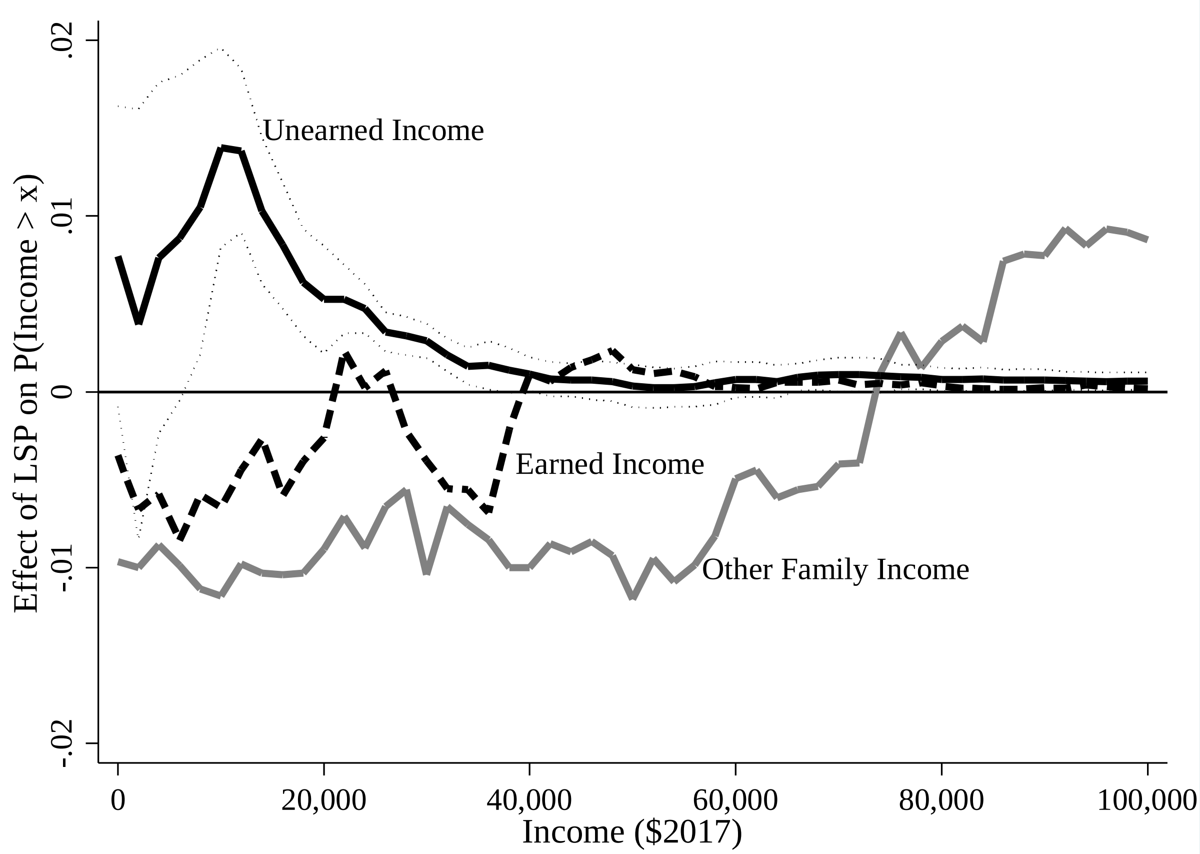
Notes: The dependent variable is the number of births to unmarried mothers in county c and year t divided by the number of women ages 10-49 measured in thousands. The average dependent variable in treated counties in the year their LSP starts is 5 births per 1,000 women. See notes to Figure 4 for details on the specification. The full sample includes 112 counties (65 treated) and the long sample contains 60 counties (28 treated).

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



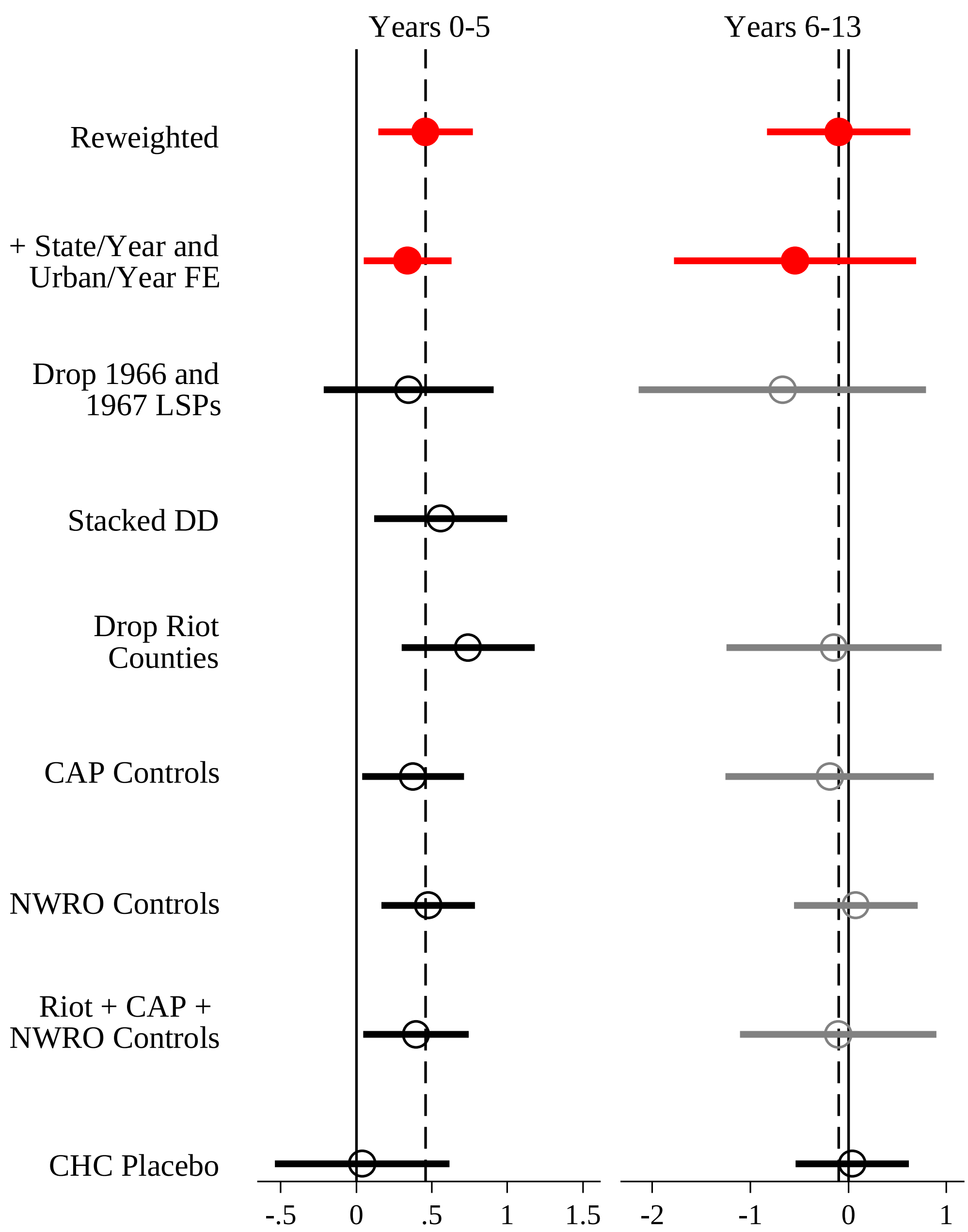
Notes: The dependent variable in panel A is the number of births in county c, year t divided by the number of women 10-49 measured in thousands. The average dependent variable in treated counties in the year their LSP starts is 62.7 births per 1,000 women. The dependent variable in panel B is the number of marriages that occur in county c, year t divided by the number of resident women 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. See notes to Figure 4 for details on the specification.

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



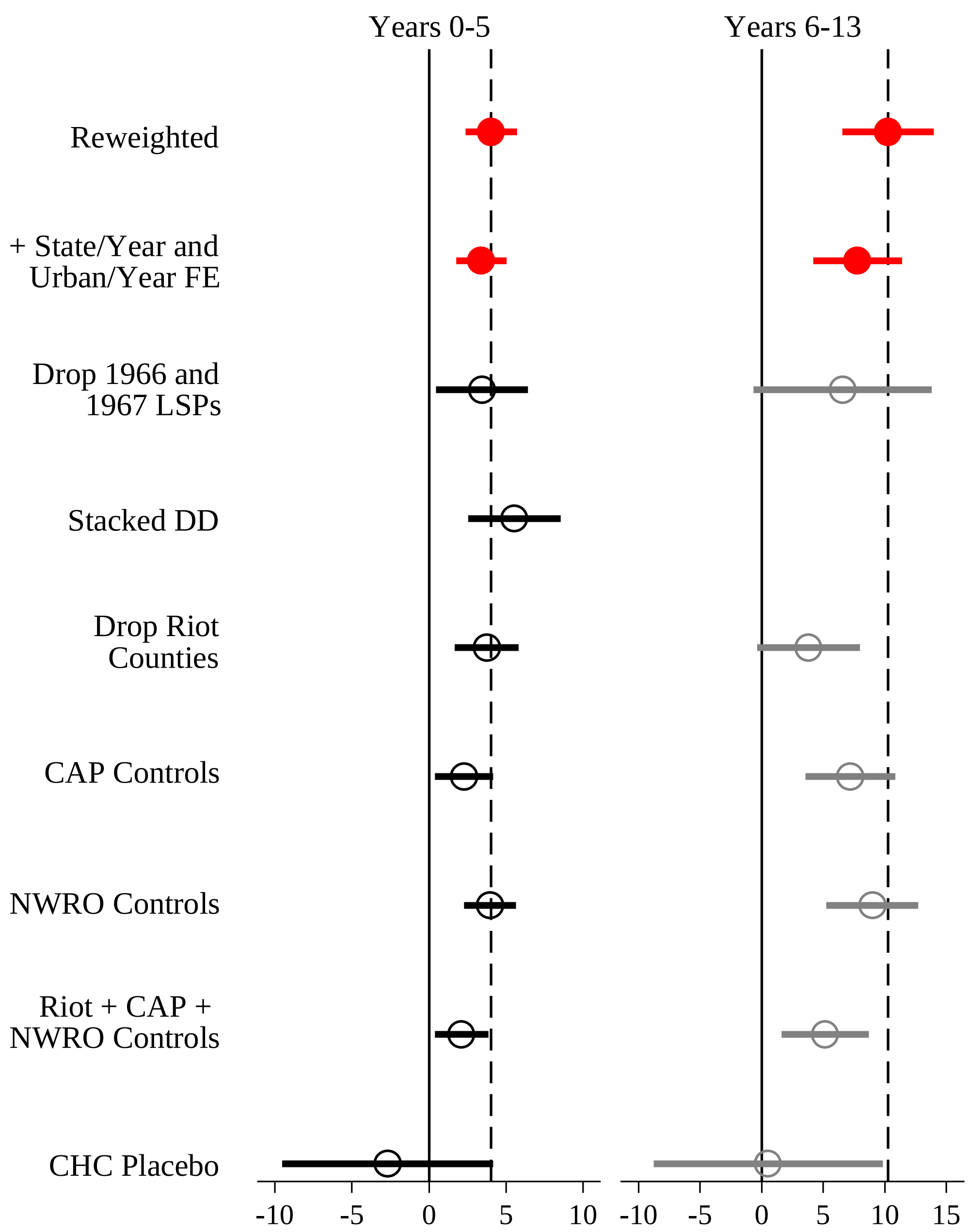
Notes: The figure plots difference-in-difference coefficients (from the reweighting estimator) with the outcome variable defined as the change from 1960-1970 in the county-level probability of having income greater than or equal to the amount on the *x*-axis (measured in $2,000 bins in 2017 dollars). For details on "distribution regression" see Chernozhukov, Fernández-Val, and Melly (2013). This reflects changes in the cumulative distribution of income by source. The sample includes mothers living with their children in the 1960 and 1970 Census in 81 counties identified in both years. 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



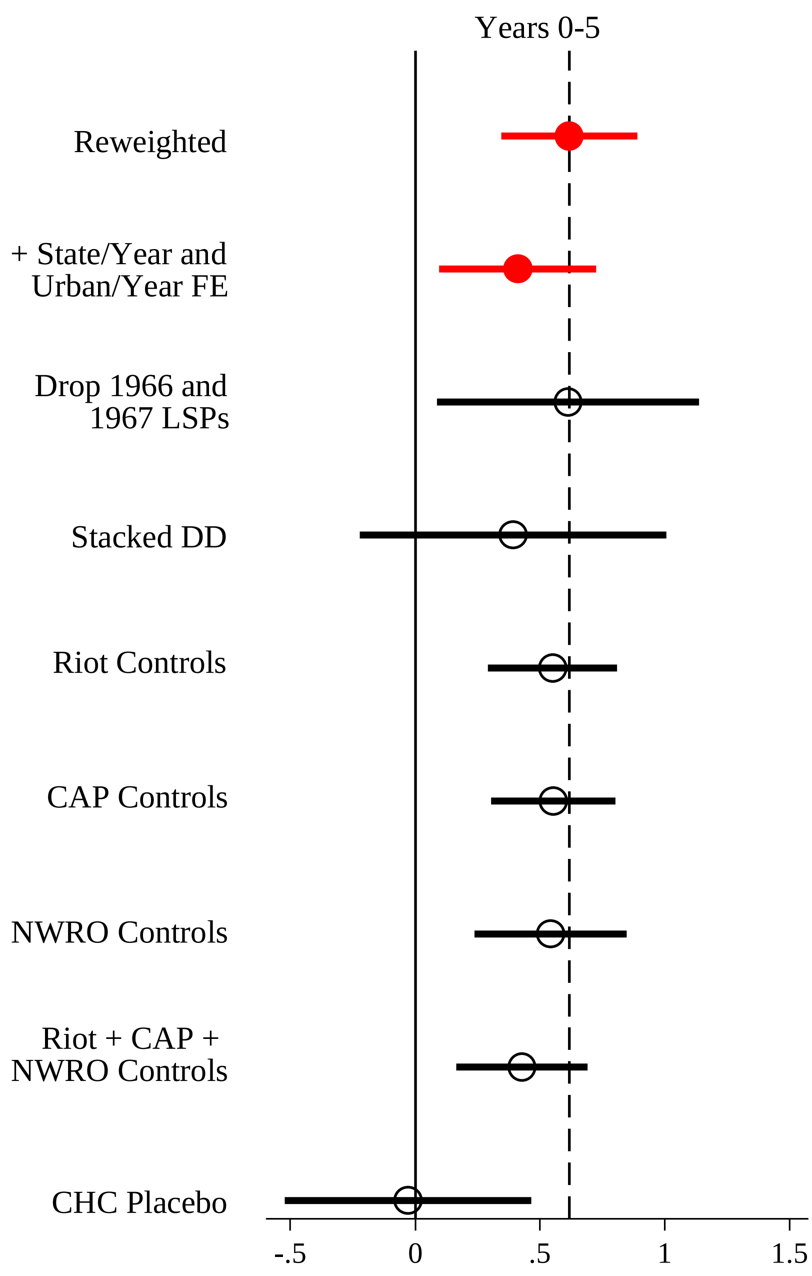
Notes: The figure plots shorter-run (years 0-5) and longer-run (years 6-13) estimates for alternative specifications discussed in section VI.

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



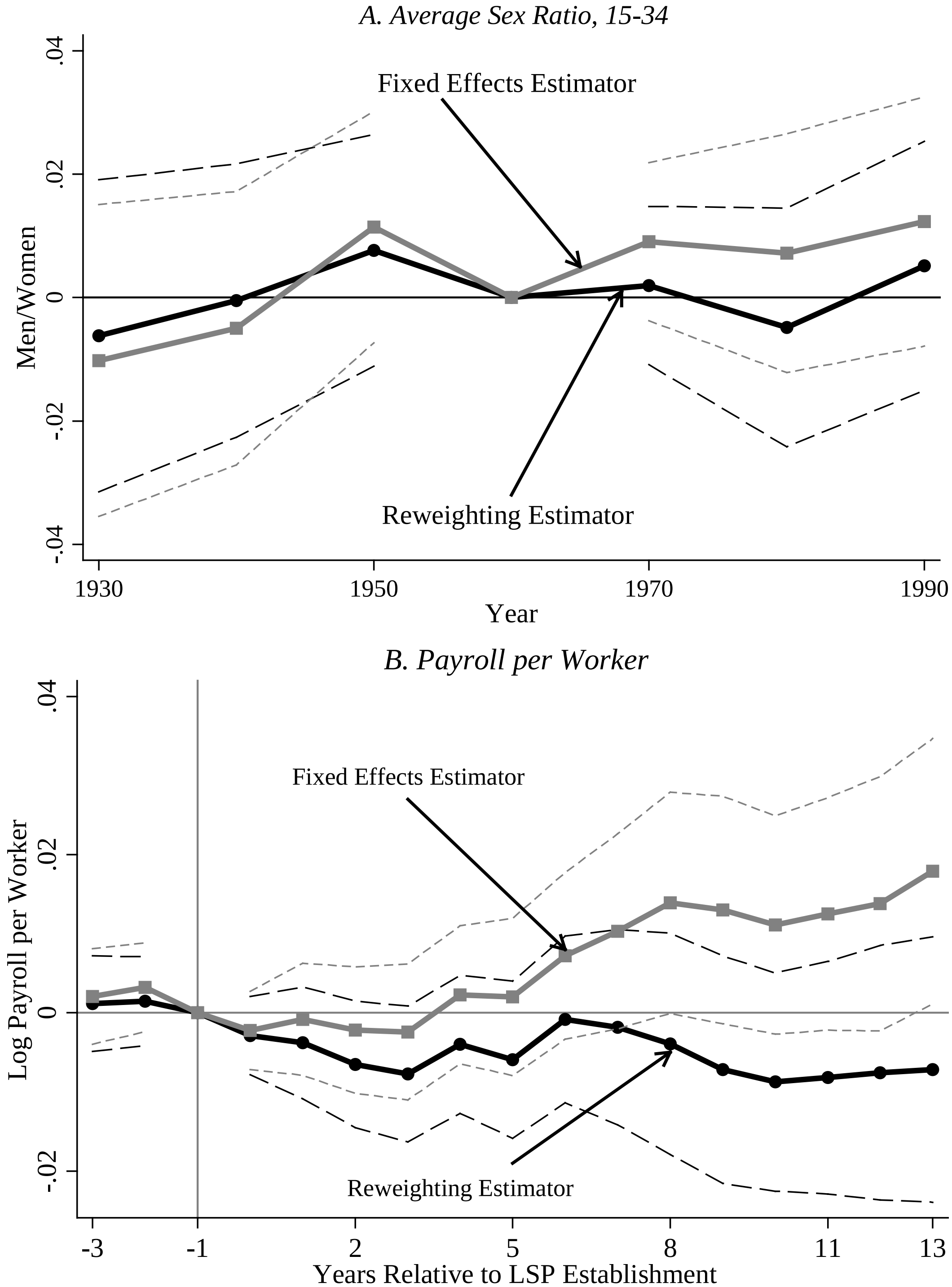
Notes: The figure plots shorter-run (years 0-5) and longer-run (years 6-13) estimates for alternative specifications discussed in section VI.

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



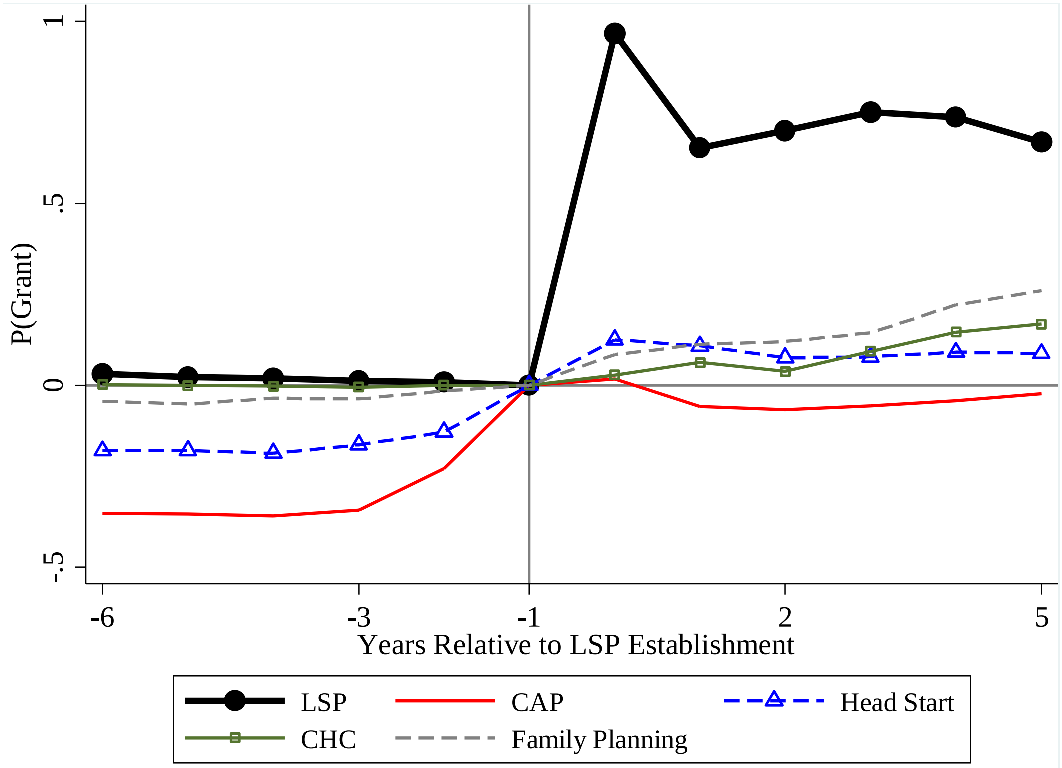
Notes: The figure plots shorter-run (years 0-5) and longer-run (years 6-13) estimates for alternative specifications discussed in section VI.

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. See notes to Figure 4 for details on the specification.

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).

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 III.D.

Table 2. Distribution of Legal Services Program Treatment Status

Treatment Status	Number of Counties	Percent of Counties	Percent 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 percent of counties, and the percent of our largest sample first treated by LSPs in different years. Nationwide 273 counties had a LSP, so we use this number to aggregate up our estimates. 251 counties received LSPs in our largest analysis sample (2,683 counties).

Table 3. Balance in Pre-Treatment 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*
Percent of 1960 county population:							
in urban area	71.7	-42.8	<0.01	-3.8	<0.01	-3.6	0.06
nonwhite	9.0	1.3	0.08	-3.1	<0.01	-0.8	0.43
under age 5	11.5	-0.5	<0.01	0.0	0.89	0.0	0.96
over age 65	9.2	1.7	<0.01	0.1	0.63	0.0	0.99
with less than 4 years of school	7.7	3.1	<0.01	-1.0	0.02	0.2	0.63
with more than 12 years of school	42.5	-5.6	0.02	19.7	0.29	0.0	0.99
with family income <$3k	19.7	16.7	<0.01	-0.4	0.54	1.2	0.26
with family income >$10k	14.6	-7.2	<0.01	-1.2	<0.01	-0.1	0.92
Median family income	$5,700	-$1,616	<0.01	-$57	0.32	-$84	0.55
Median years of schooling	10.7	-1.1	<0.01	0.1	0.11	-0.1	0.57
1960 levels of:							
AFDC cases/1,000 women 10-49	16.0	2.7	<0.01	-2.4	0.02	-0.7	0.58
marriages/1,000 women 10-49	28.4	11.7	<0.01	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	<0.01	-1.4	<0.01	-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**			***<0.01***		***<0.01***		***0.58***

Notes: The table gives summary statistics for our main sample from either our outcome data (see section III) or Haines and ICPSR (2010).

Table 4. Estimated Intention-to-Treat Effects of LSPs on Divorce, Nonmarital Births, and AFDC Cases per 1,000 Women

	(1)	(2)	(3)	(4)	(5)	(6)
	Divorce Rate		AFDC Rate		Nonmarital Birth Rate	
Pre-LSP						
-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						
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]
	(0.142)	*(0.114)*	*(0.002)*	*(0.000)*	*(0.018)*	*(0.004)*
Longer-Run Post LSP						
6 to 13	-0.59	-0.10	6.55	10.25		
	[0.49]	[0.37]	[1.53]	[1.9]		
	(0.836)	*(0.466)*	*(0.000)*	*(0.000)*		
Counties	2,683	2,680	603	603	112	112
Reweighted		x		x		x
Fixed Effects	x		x		x	

Notes: The table presents estimate that summarize the event-study figures by grouping event-times -6 to -2, 0 to 5, and 6 to 13. See notes to Figure 4 for details on the specification. Standard errrors clustered by state are in brackets, one-sided *p*-values for 500 random permutations of the LSP variable are in parentheses (see appendix 4 for permutation distributions).

Table 5. Quantifying LSPs Role in Nonmarital Births and AFDC Cases per 1,000 Women, 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 (3) contain averages for treated counties only, and columns (2) and (4) contain averages for all counties in our longest samples (600 counties for AFDC and 60 counties for non-marital births).

Table 6. Quantifying LSPs Role in Nonmarital Births and AFDC Cases, 1964-1984

	(1)	(2)	(3)	(4)
	AFDC Cases		Nonmarital Births	
	Treated Counties	Full Sample	Treated Counties	Full Sample
Observed Outcomes				
a. 1964	586,559	937,455	53,722	58,338
b. 1984	2,346,591	2,922,645	124,904	135,247
c. Change	1,760,032	1,985,190	71,182	76,909
Counterfactual Outcomes in 1984				
d. Reweighted Specification	1,922,274	2,498,328	102,624	112,967
e. Fixed Effects Specification	2,097,116	2,673,170	99,445	109,788
Treatment Effect Magnitudes				
	How much did LSPs raise outcomes compared to the counterfactual?			
f. Reweighted Specification: (b-d)/d	22%	17%	22%	20%
g. Fixed Effects Specification: (b-e)/e	12%	9%	26%	23%
	How much of the change in outcomes can LSPs explain?			
h. Reweighted Specification: (b-d)/c	24%	21%	31%	29%
i. Fixed Effects Specification(b-e)/c	14%	13%	36%	33%

Notes: To calculate counterfactuals we subtract the event-study estimates from observed outcomes by county and then multiply by county populations in each year. This yields total numbers of nonmarital births and AFDC cases. The percentages of counts that LSP can explain differ from the portion of rates that it can explain because population growth in treated versus untreated counties. Columns (1) and (3) contain averages for treated counties only, and columns (2) and (4) contain averages for all counties in our longest samples (600 counties for AFDC and 60 counties for non-marital births).

Table 7. Estimated Intention-to-Treat Effects of LSPs Stratified by State Divorce and Two-Parent Welfare Policy

	(1) Divorce Rate	(2) AFDC Rate	(3) Nonmarital Birth Rate	(4) Divorce Rate	(5) AFDC Rate	(6) Nonmarital Birth Rate
	A. Early No-Fault Divorce States			*C. AFDC-UP States*		
Shorter-Run Post-LSP						
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						
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						
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						
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 (UP) program before 1981 (31 states, mostly implementing 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.

Table 8. The Effect of LSP on Family Structure and Poverty, 1960-1970

	(1)	(2)	(3)	(4)
	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]
	(0.000)	*(0.002)*	*(0.008)*	*(0.086)*
Living with the Father of Any Children	-0.020	-0.029	-0.015	-0.012
	[0.007]	[0.010]	[0.008]	[0.012]
	(0.002)	*(0.002)*	*(0.086)*	*(0.010)*
Poor	0.004	0.014	0.007	0.004
	[0.008]	[0.014]	[0.006]	[0.010]
	(0.623)	*(0.399)*	*(0.423)*	*(0.124)*
	B. Reweighted Specification			
Unmarried Head of Household	0.014	0.029	0.015	0.020
	[0.005]	[0.005]	[0.004]	[0.006]
	(0.020)	*(0.002)*	*(0.012)*	*(0.060)*
Living with the Father of Any Children	-0.015	-0.032	-0.017	-0.019
	[0.007]	[0.005]	[0.009]	[0.006]
	(0.042)	*(0.000)*	*(0.016)*	*(0.094)*
Poor	0.001	0.004	0.001	-0.002
	[0.006]	[0.008]	[0.005]	[0.007]
	(0.305)	*(0.337)*	*(0.244)*	*(0.541)*

Notes: The table presents difference-in-differences estimates using a sample of mothers living 81 identified counties in 1960 (390,599 respondents) or 1970 (170,941 respondents). Columns 3 – 5 presents reweighting estimates but on separate samples 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 or mothers (in 5-year bins). Because there are so few counties, we use region fixed effects in column 2 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. Standard errors clustered by county are in brackets and *p*-values from 500 permutations of the treatment dummy are in parentheses (see appendix 4 for permutation distributions).