Appendix A
Identifying Drug and Opioid Deaths in the 1973-2015 MCOD Data

To construct cohort exposure measures for those aged 0-16 from 1990 to 2015 required data from the Multiple Cause of Death data files from 1973 through 2015. Over this period, these data sets used three different version of the International Classifications of Diseases (ICD): ICD 8 (through 1978), ICD 9 (1979 through 1998) and ICD 10 (1999 and on).

Identifying drug overdoses in all three versions of the ICD system is relatively straightforward. In each year, there are three sets of codes that identify unintentional poisoning deaths, intentional poisonings (e.g., suicides), and drug poisoning of unknown intent. These codes vary by the class of drug. ICD 8 has an additional code 304 that measure death due to drug dependence, which is a code under the mental health classifications. This code was dropped in subsequent versions. In the ICD 9 system, code E962 measures death from homicide due to drug poisonings. That code under the ICD 10 classification is X85. We list these codes in Table A1 below.

Table A1
Codes to Identify Drug Poisonings, ICD 8 through ICD 10

ICD Era	Unintentional Poisonings	Intentional Poisonings	Poisonings of Unknown Intent	Other codes
ICD-8	E850.0 – E858.9	E950.0 – E950.5	E980.0 – E980.3	304
ICD-9	E850.0 – E858.9	E950.0 – E950.5	E980.0 – E980.3	E962
ICD-10	X40 – X44	X60 – X64	Y10 – Y14	X85

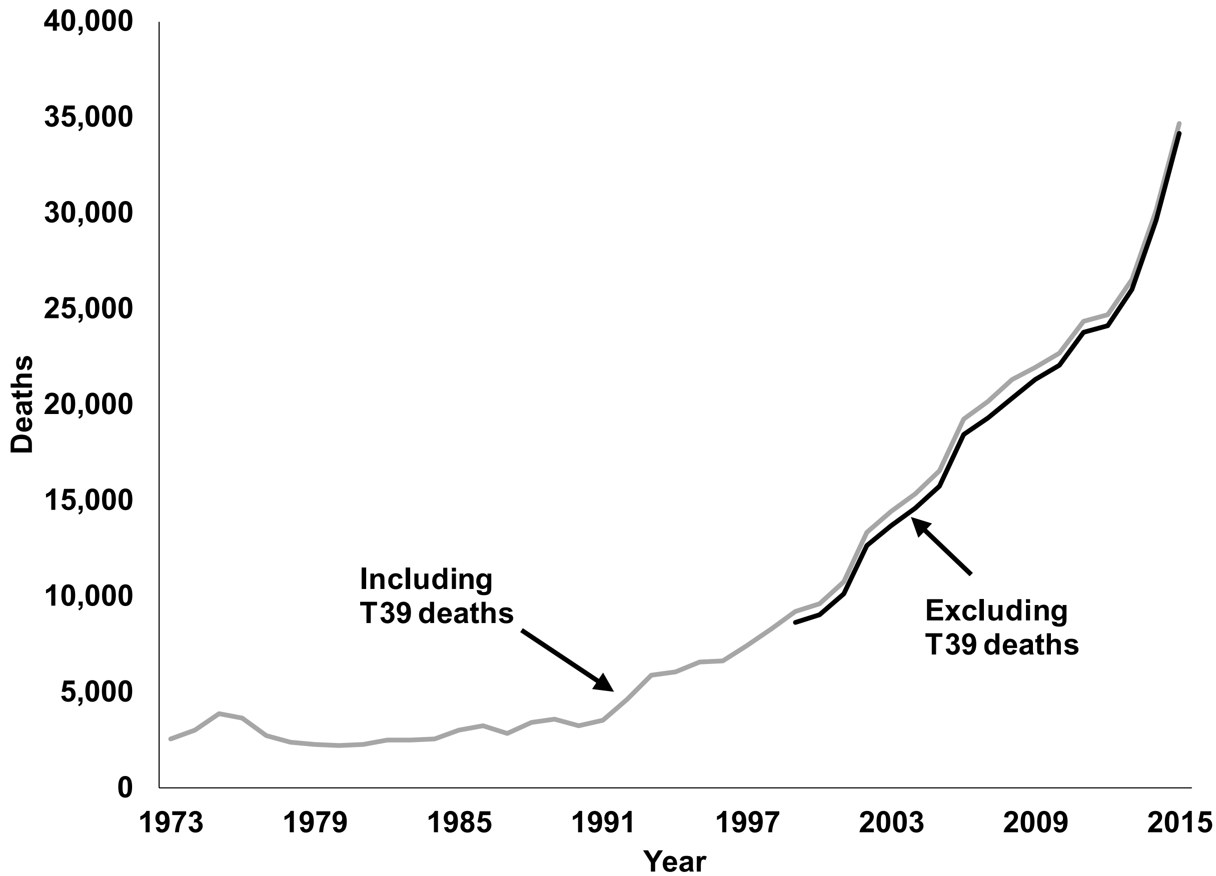
Identifying opioid deaths is relatively easy in ICD 10 as there are codes that identify conditions present at death to indicate specific drugs. These include T40.1 (heroin), T40.2 (other opioids) T40.3 (methadone), and T40.4 (synthetic opioids). Like Alpert et al. (2019), we also include T40.6 (other and unspecified narcotics) as well. There are similar codes in the ICD 9 classifications: 965.0 (opiates and related narcotics), 965.1 (heroin), 965.2 (methadone), 965.9 (other opiates and related narcotics). There is only one condition code that uniquely identifies opioids in the ICD-8 coding: 965.1 (opiates and synthetic analogues).

The problem we found is that in many cases during the ICD 8 and 9 era, the "965" condition codes are frequently not used when there was a drug death. In the ICD 9 era, we can identify opioids in some of the "E" codes – E850.0 (heroin), E850.1 (methadone), and E850.2 (opiates and related narcotics). Unfortunately, categories E950.0 and E980.0 (poisonings by analgesics, antipyretics, and antirheumatics for intentional and unknown intent, respectively) lump opiates in with other drugs (mostly non-opioid pain relievers).

In the ICD 10 era, the T39 condition code identifies non-opioid analgesics, antipyretics and antirheumatics and in 1999, there were only 759 deaths from these drugs, but 8,645 of the T40.x opioid/heroin deaths. As a result, to make a more consistent series without a noticeable jump in opioid deaths as we move from the ICD 10 back to the ICD 9 era, we use a broader opioid death rate category that includes the T39 cases. In the ICD 9 era, we consider the "965" conditions listed above, those that include non-opioid analgesics, and any E850.x code which contains opiates and the non-opioid analgesics, plus deaths with E950.0 and E980.0 codes. For ICD 8 years, we include in the broader opioid death category E5853.x codes which are opiates and other analgesics, E950.1 (suicides by salicylates and congeners), E980.1 (poisoning by salicylates and congeners of undetermined intent), and all 965.x condition codes.

In Figure A1 below, in the gray line, we report trends in the opioid death counts when we include this slightly broader set of drugs. The black line is the trend in opioid deaths that only uses the T40.x codes outlined above. The lines track each other well and the broader definition we use is greater by 501 to 952 deaths/year in the 1999-2015 period.

Figure A1
Opioid-Related Deaths, 1973 to 2015, Including and Excluding Non-opioid Analgesics, Antipyretics and Antirheumatics (T39) Causes



Data are from the Multiple Cause of Death Files, 1973-2015.

Appendix B
Sensitivity and Robustness

In this appendix, we probe the sensitivity of our primary estimates to a multitude of sample and specification choices. In addition to these robustness results, we also report clustered wild bootstrap confidence intervals for the first stage of our 2SLS procedure (Appendix Table B1) as well as all subsequent robustness analyses (Appendix Tables B2-B6). For ease of comparison, the first column in most tables in this section contains the basic 2SLS results from Table 1.

Appendix Table B2: Robustness to General Sample and Specification Choices

In our main analyses, our cumulative risk measure is based on all drug deaths. However, our instrument generates variation most directly in opioid deaths. The drug death measure is more general and more consistently coded across ICD classification systems, but an exposure measure based on opioid deaths is more closely tied to the variation generated by the instrument. In the second column of Appendix Table B2, we show results based on measuring a child's exposure with opioid deaths rather than drug deaths. The ratio of estimates based on opioids to the estimates based on all drug deaths ranges from 1.40 to 1.46; our exposure measure based on drug deaths increased by 124.6 while the measure based on opioids increased by 89.8—a ratio of 1.38. This suggests that approximately all of the changes in family structure we estimate are generated by changes in opioid death rates.

Alpert et al. (2018) and Evans et al. (2019) demonstrate that the reformulation of OxyContin in August of 2010, which made OxyContin more difficult to abuse, encouraged the shift in drug abuse away from prescription opioids towards heroin. Although the reformulation reduced mortality associated with prescription opioids, it increased heroin mortality to the point that the reformulation had no impact on drug mortality in the short run. The market for drugs was systematically changed in 2013 by another supply shock when fentanyl appeared in large scale in illegal drug markets. The rapid increase in mortality experienced in the US from 2013 to 2018 is primarily driven by increasing use of fentanyl and other synthetic opioids. One can argue that OxyContin abuse lead to its reformulation, which then expanded use of heroin, and the heroin market begat the fentanyl market. That said, one could also argue that our instrument can best explain the movement of drug use across triplicate and non-triplicate states prior to the end of 2010. In the third column of Appendix Table B2, we estimate our basic specifications with data only through 2010. The results are actually slightly larger in magnitude than our baseline estimates and do not suggest that changes in drug deaths in recent years are driving our results.

Exposure to the drug crisis might not only affect the living arrangements of children, but also the probability that a child is born at all. This in turn suggests that the composition of families in which children are living could be affected by the drug crisis. We test this hypothesis by limiting our sample to children born prior to the introduction of OxyContin, 1996. The benefit of this approach is that it precludes the possibility that OxyContin affected the birth of anyone in the sample; the drawback is that it cuts out approximately 50 percent of our sample and our standard errors increase considerably. The results from this exercise are shown in the fourth column of Appendix Table B2. In all but one case, missing at least one parent, the point estimates increase in size. This suggests that children born in non-triplicate states after the introduction of OxyContin tended to be positively selected, born to families less likely to have mothers or fathers absent. This is suggestive evidence that the primary mechanism through which the drug crisis affects children's living arrangements is through impacts on the family after the child is born.

As an additional check, we have included state-specific linear trends in the regressions. These trends will pick up any population, demographic, or other factors which are increasing or decreasing differentially across triplicate and non-triplicate states. Our estimated impacts of drug deaths are presented in the fifth column and again, we do not find results qualitatively different from our main estimates.

Appendix Table B3: Robustness to Additional Controls

It seems likely that economic wellbeing is an important determinant of both family structure as well as drug death rates. As such, we explore the degree to which various measures of economic conditions could be affecting our results. In the first column or results in Appendix Table B3, we report regressions in which each state's per-capita real GDP has been included as a control variable. The results are extremely similar to our baseline results.

In the second column, we include the state's unemployment rate. Hollingsworth et al. (2017) found a correlation between unemployment rates and drug overdose death rates. Again, our results are largely unaffected by this variable's inclusion. While unemployment rates might capture some of the variation in drug over dose death rates, it does not appear to be driving the portion which is related to family structure.

Using the granting of permanent normalized trade relations with China in 2000, Pierce and Schott (2020) showed that geographic regions most exposed to trade with China saw increases in opioid overdose death rates. To ensure that our results are not being driven by this same trade shock, we interact their measure of exposure with year dummies and include those variables in our regression. The results are very similar to our baseline results, suggesting that our instrument is not providing spurious results in which it happens to capture differences in exposure to trade shocks.

Triplicate programs were early versions of prescription drug monitoring programs (PDMP) systems designed to oversee and discipline the prescribing of controlled substances like opioids. In subsequent years nearly all states adopted some form of PDMP. Although evidence on the effectiveness of these subsequent PDMPs is mixed (e.g. Buchmueller and Carey, 2018), we create three different measures of PDMPs (based on Horwitz et al. (2019)) for each state and include them in the regression. Our measures are indicators for years including and after the state's PDMP 1) was legislated to be active, 2) actually became active (funding, and other, issues delayed many PDMPs), and 3) whether it was a modern, electronic system. As seen in the fourth column of Appendix Table B3, these variables have little impact on our point estimates and suggest that triplicate and non-triplicate states were not differentially enacting opioid-related legislation that was correlated with both cumulative drug mortality and family structure.

Welfare reforms took place at roughly the same time as the introduction of OxyContin, they varied across states, and they have been shown to have affected children's living arrangements (Bitler et al., 2006). Consequently, there is a possibility that our estimation strategy is partially capturing the effects of these policy reforms. Following Bitler et al. (2006), we create two variables which indicate whether the state had obtained a waiver for its Aid to Families with Dependent Children (AFDC) program and the first year in which Temporary Assistance to Needy Families (TANF) was implemented. As seen in the final column of Appendix Table B3, adding these measures has very little impact on our estimated effects.

Appendix Table B4: Robustness to Omitting Treatment States

In Appendix Table B4, we test whether any single triplicate state is driving our results. We do so by dropping each triplicate, one at a time, and rerunning the 2SLS regression. Although there are slight changes in the point estimate from one sample to the next, the evidence suggests that there was not a single triplicate state solely responsible for the estimated effects.

Appendix Table B5: Sensitivity to Population-Related Issues

In 1990, the triplicate states tended to have much greater population and some of the largest cities in the United States. California, New York, and Texas had the largest populations while Illinois had a larger population than all but two non-triplicate states. Clearly, the triplicate states differ from the non-triplicate states in terms of their population and tendency to have large metropolitan areas. If the changes in family structure tended to occur in less populous places (or those declining in population), our instrument might simply be picking up that difference between triplicate and non-triplicate states. In Appendix Table B5, the second column of results reports estimates from regressions in which we have included a fourth order polynomial in the states' populations of adults of child-bearing age. Three of the five estimates increase slightly in magnitude while the other two decrease very slightly. Overall, flexibly controlling for a state's population has little impact on the results.

Linked to the differences in population between triplicate and non-triplicate states, the triplicate states contain the largest cities in the United States. Instead of pure population, it might be that changes in family structure are actually caused by residing in an urban environment rather than by drug deaths. To explore this possibility, we have rerun our regressions separately for children living in metro areas and for children who are living in a non-metro area. These results are presented in columns three and four of Appendix Table B5. Although we lose a considerable amount of precision when splitting the sample in this way, our point estimates are quite similar to what we had found previously.[1] Moreover, the point estimates indicate that there are not large differences in our estimated impacts of the drug crisis across more and less urban areas, strongly

[1] It is worth noting that the results for metro and non-metro areas do not average up to the overall results. They do not have to do so because we are not restricting all of the other coefficients in the regression (e.g. year effects) to be the same across the two. In addition, there is a small number of individuals—approximately 1 percent—who could not be classified into either metro or non-metro areas. For these regressions, they were omitted from both groups.

suggesting our main results are not simply picking up differences in urban status across triplicate and non-triplicate states.

An alternative way to assess the importance of differences in population or urbanicity between triplicate and non-triplicate states is to restrict the sample of states used for the analysis to those with the largest populations. In the fifth column of Appendix Table B5, we present results in which the set of triplicate states has been restricted to California, Illinois, New York, and Texas and the set of non-triplicate states has been restricted to Florida, Pennsylvania, Ohio, and Michigan. These were the eight most populous states in 1990. The benefit of this restriction is that our non-triplicate states are much more similar to our triplicate states in terms of population and urbanicity; the cost is a considerable loss of statistical power. Even with this severe restriction, our point estimates tend to be quite similar to those we obtain when using all non-triplicate states in the regression.

Appendix Table B6: Heterogeneity by Race

Because the there are large baseline differences in the probability of living in different family structures for white and Black children, it is interesting to consider whether the effects of exposure to the crisis differ by race. To assess this possibility, we regress race-specific measures of family living arrangements on race-specific measures of exposure to the crisis. We report these results in Appendix Table B6. The first and third columns present means of the various living arrangement variables for white children and Black children respectively; these outcomes tend to be much more common among Black children. They are roughly twice as likely to live without a mother in the household, three times as likely to live without a father in the household, and 2.5 times as likely to live with a grandparent as the household head. The 2SLS results are presented in the second column for white children and the fourth column for Black children. We find that increased exposure to the drug crisis had substantial impacts on white children. These estimates are quite similar to our overall estimates and despite splitting the sample, retain reasonable precision. However, our estimates for Black children are far noisier, making it more difficult to reject a wide range of values. Though highly speculative, the implied total effect of the drug crisis implied by our point estimates is actually smaller in percentage terms for Black children than for white children for almost all of the outcomes.[2]

[2] For both white and Black children, we calculate the change in exposure between 1996 and 2015, multiply that by the estimated effect of exposure, and then divide by the sample mean for the outcome, using race-specific measures at each step. The smaller effect in percentage terms for Black children is due both to the much higher rates of these outcomes experienced by Black children, but also a much lower change in exposure to the drug crisis. Between 1996 and 2015, our measure of exposure rose by roughly 17 for Black children, but by 150 for white children. We have also run the analysis for white children and Black children using the overall measure of exposure to the crisis. In those analyses, we would expect to see larger percentage impacts of the rise in the drug crisis on white children because of their larger exposure, and this is indeed what we find.

Appendix Table B1
First-Stage Estimates of Cumulative Drug Death Equations, ASEC 1990-2015

Dependent variable	Sample mean	OLS Coefficient on $YearsExpNT_{ast}$	1st stage F-test
Cumulative drug deaths/100K of likely mothers	75.3	7.36 (0.80) [5.78, 9.37]	84.2
Cumulative drug death/100K of likely fathers	146.1	11.1 (1.24) [8.28, 13.63]	80.3
Cumulative drug deaths/100K of likely parents	110.9	9.22 (0.96) [7.25, 11.44]	92.5

All models include fixed effects for age, year and state, plus the fraction of observations that were female, Black (non-Hispanic), other race (non-Hispanic) and Hispanic. The model includes 17 ages x 51 states x 26 years = 22,542 observations. We calculate standard errors allowing for arbitrary correlation in errors at the state level (in parentheses); clustered wild bootstrap 95% confidence intervals presented in brackets.

Appendix Table B2
2SLS Estimates of the Impact of Cumulative Drug Deaths of Likely Parents on the Living Arrangements of Children: Alternative Sample and Specification Choices

Parameter Estimates (Standard Errors) [cluster wild bootstrap 95% CI] on $CEXPOSURE_{ast}$

Dependent variable	2SLS from Table 1	Use Opioid Death Rate	Restrict to Year < 2011	Restrict to Born < 1996	State-specific Time Trends
Mom not in household/100K	12.88 (3.36) [5.50, 20.33]	18.87 (4.99) [8.29, 29.35]	20.38 (4.80) [10.39, 31.14]	16.24 (9.18) [-3.05, 35.85]	10.30 (2.37) [5.61, 14.94]
Dad not in household/100K	9.68 (4.34) [-2.01,19.12]	13.59 (6.38) [-4.29, 27.21]	16.50 (7.40) [-3.20, 32.51]	9.48 (14.95) [-35.96, 48.00]	7.51 (3.82) [0.37, 14.78]
Missing at least one parent/100K	18.29 (5.63) [2.38, 30.13]	26.11 (8.44) [2.19, 43.25]	29.47 (9.99) [2.57, 51.75]	12.18 (19.77) [-45.78, 56.36]	16.82 (4.55) [7.77, 26.30]
Missing both Mom and Dad/100K	3.64 (1.43) [0.28, 6.62]	5.20 (2.12) [0.22, 9.66]	6.53 (1.82) [2.28, 10.33]	11.80 (3.39) [4.27, 20.71]	0.63 (1.40) [-2.00, 3.34]
Grandparent head of HH/100K	6.02 (2.17) [1.60, 11.12]	8.59 (3.19) [2.34, 15.74]	6.86 (2.91) [1.28, 13.87]	6.38 (6.34) [-8.38, 26.63]	8.39 (3.37) [0.74, 16.34]
Non-parent head of HH/100k	11.45 (2.62) [6.10, 17.53]	16.35 (3.67) [9.09, 24.87]	17.37 (4.07) [8.95, 26.37]	12.35 (7.82) [-5.70, 36.80]	14.81 (4.49) [4.45, 25.89]
Foster child / 100k	-0.07 (0.48) [-1.10, 0.95]	-0.10 (0.69) [-1.61, 1.37]	0.94 (0.83) [-0.69, 2.97]	1.96 (1.47) [-0.88, 6.56]	-0.43 (0.54) [-1.53, 0.62]

Data are from the Annual Social and Economic Supplement (ASEC) of the March Current Population Survey (CPS). All models include fixed effects for age, year, and state, plus the fraction of observations that were female, Black (non-Hispanic), other race (non-Hispanic) and Hispanic. The model includes 17 ages x 51 states x 26 years = 22,542 observations. Standard errors clustered by state are reported in parentheses; 95% confidence intervals estimated via a clustered (at state) wild bootstrap reported in brackets. Baseline results reproduced in the first column. The second column uses the (potentially noisily measured) opioid death rate to construct children's exposure. The third column restricts the sample to 2010 or earlier to avoid OxyContin's reformulation. The fourth column restricts the sample to those born before OxyContin's introduction. The final column includes state-specific linear time trends.

Appendix Table B3
2SLS Estimates of the Impact of Cumulative Drug Deaths of Likely Parents on the Living Arrangements of Children: Additional Controls

Parameter Estimates (Standard Errors) [cluster wild bootstrap 95% CI] on $CEXPOSURE_{ast}$

Dependent variable	2SLS from Table 1	State Per-capita Real GDP	Unemp. Rate	Trade Shock	Rx Drug Monitoring Programs	Welfare Reform Controls
Mom not in household/100K	12.88	13.23	13.16	12.78	12.07	12.49
	(3.36)	(3.40)	(3.24)	(3.37)	(3.02)	(3.29)
	[5.50, 20.33]	[6.03, 20.60]	[6.15, 20.26]	[5.69, 19.95]	[5.55, 18.72]	[5.30, 19.53]
Dad not in household/100K	9.68	9.03	9.64	9.44	8.62	8.88
	(4.34)	(4.66)	(4.31)	(4.44)	(4.02)	(3.70)
	[-2.01,19.12]	[-3.39, 19.03]	[-2.76, 18.80]	[-3.15, 18.71]	[-1.57, 17.23]	[-1.47, 17.00]
Missing at least one parent/100K	18.29	17.74	18.29	17.90	17.19	17.28
	(5.63)	(5.91)	(5.57)	(5.76)	(5.08)	(4.82)
	[2.38, 30.13]	[2.22, 31.30]	[2.53, 30.45]	[1.44, 30.61]	[3.53, 28.84]	[3.89, 27.64]
Missing both Mom and Dad/100K	3.64	3.71	3.82	3.67	2.85	3.36
	(1.43)	(1.50)	(1.36)	(1.45)	(1.35)	(1.40)
	[0.28, 6.62]	[0.47, 6.75]	[0.51, 6.50]	[0.20, 6.57]	[-0.24, 5.72]	[0.12, 6.22]
Grandparent head of HH/100K	6.02	5.34	5.99	6.08	5.72	5.63
	(2.17)	(2.64)	(2.19)	(2.22)	(1.97)	(2.04)
	[1.60, 11.12]	[-0.28, 11.59]	[1.87, 11.22]	[1.58, 11.26]	[1.91, 10.01]	[1.64, 10.24]
Non-parent head of HH/100k	11.45	11.40	11.50	11.61	11.38	11.05
	(2.62)	(3.12)	(2.66)	(2.70)	(2.64)	(2.58)
	[6.10, 17.53]	[4.92, 17.82]	[6.25, 17.47]	[6.46, 17.76]	[5.77, 16.84]	[5.77, 16.96]
Foster child / 100k	-0.07	-0.06	-0.06	-0.06	0.02	-0.10
	(0.48)	(0.52)	(0.49)	(0.48)	(0.49)	(0.49)
	[-1.10, 0.95]	[-1.20, 1.06]	[-1.15, 1.02]	[-1.04, 0.91]	[-0.96, 1.08]	[-1.12, 0.94]

Data are from the Annual Social and Economic Supplement (ASEC) of the March Current Population Survey (CPS). All models include fixed effects for age, year, and state, plus the fraction of observations that were female, Black (non-Hispanic), other race (non-Hispanic) and Hispanic. The model includes 17 ages x 51 states x 26 years = 22,542 observations. Standard errors clustered by state are reported in parentheses; 95% confidence intervals estimated via a clustered (at state) wild bootstrap reported in brackets. Baseline results reproduced in the first column. The second column controls for state-level per-capita real GDP. The third column controls for a state's unemployment rate. The fourth column includes a control for the 2001 trade shock interacted with year dummies. The fifith column includes controls for prescription drug monitoring programs. The final column includes controls for the welfare reforms that occurred in the 1990s.

Appendix Table B4
2SLS Estimates of the Impact of Cumulative Drug Deaths of Likely Parents on the Living Arrangements of Children: Omitting Each Treatment State

Parameter Estimates (Standard Errors) [cluster wild bootstrap 95% CI] on $CEXPOSURE_{ast}$

Dependent variable	2SLS from Table 1	Drop Texas	Drop New York	Drop Illinois	Drop Idaho	Drop California
Mom not in household/100K	12.88	12.99	13.65	13.09	12.54	9.76
	(3.36)	(3.49)	(3.58)	(3.50)	(3.36)	(2.20)
	[5.50, 20.33]	[4.99, 21.25]	[5.723, 21.72]	[5.81, 20.93]	[5.33, 20.01]	[5.42, 13.93]
Dad not in household/100K	9.68	12.71	10.15	7.28	10.07	7.67
	(4.34)	(3.47)	(4.82)	(3.85)	(4.41)	(5.19)
	[-2.01,19.12]	[3.84, 20.61]	[-5.34, 19.85]	[-3.68, 15.65]	[-2.84, 19.24]	[-4.55, 21.16]
Missing at least one parent/100K	18.29	21.87	19.53	15.98	18.62	14.46
	(5.63)	(4.55)	(6.11)	(5.50)	(5.67)	(5.69)
	[2.38, 30.13]	[8.85, 31.53]	[0.03, 32.16]	[1.48, 28.46]	[2.47, 31.16]	[1.64, 29.11]
Missing both Mom and Dad/100K	3.64	3.79	3.55	3.15	3.54	2.60
	(1.43)	(1.47)	(1.58)	(1.45)	(1.45)	(1.37)
	[0.28, 6.62]	[0.19, 6.68]	[-0.28, 6.87]	[-0.25, 6.19]	[-0.01, 6.52]	[-0.42, 5.28]
Grandparent head of HH/100K	6.02	4.46	5.71	5.87	6.06	7.22
	(2.17)	(1.54)	(2.34)	(2.21)	(2.18)	(2.18)
	[1.60, 11.12]	[1.61, 7.76]	[1.24, 11.71]	[1.35, 11.27]	[1.68, 11.26]	[2.65, 11.92]
Non-parent head of HH/100k	11.45	9.54	12.33	11.15	11.38	11.83
	(2.62)	(1.80)	(2.69)	(2.65)	(2.63)	(2.98)
	[6.10, 17.53]	[5.81, 13.21]	[6.93, 18.28]	[5.47, 17.77]	[5.91, 17.50]	[5.43, 18.68]
Foster child / 100k	-0.07	-0.26	-0.11	-0.38	0.05	-0.05
	(0.48)	(0.49)	(0.54)	(0.42)	(0.47)	(0.60)
	[-1.10, 0.95]	[-1.39, 0.73]	[-1.54, 0.93]	[-1.48, 0.50]	[-0.98, 1.02]	[-1.51, 1.25]

Data are from the Annual Social and Economic Supplement (ASEC) of the March Current Population Survey (CPS). All models include fixed effects for age, year, and state, plus the fraction of observations that were female, Black (non-Hispanic), other race (non-Hispanic) and Hispanic. The model includes 17 ages x 51 states x 26 years = 22,542 observations. Standard errors clustered by state are reported in parentheses; 95% confidence intervals estimated via a clustered (at state) wild bootstrap reported in brackets. Baseline results reproduced in the first column. Additional columns drop each triplicate state, one at a time.

Appendix Table B5
2SLS Estimates of the Impact of Cumulative Drug Deaths of Likely Parents on the Living Arrangements of Children: Sensitivity to Population-Related Issues

Parameter Estimates (Standard Errors) [cluster wild bootstrap 95% CI] on $CEXPOSURE_{ast}$

Dependent variable	2SLS from Table 1	Polynomial in Population	Restrict to Metro Areas	Restrict to Non-metro Areas	Largest Population States
Mom not in household/100K	12.88 (3.36) [5.50, 20.33]	11.18 (2.60) [5.52, 16.94]	11.48 (3.35) [3.89, 18.18]	10.33 (3.79) [2.73, 18.48]	10.82 (7.58) [-10.13, 28.43]
Dad not in household/100K	9.68 (4.34) [-2.01,19.12]	11.95 (4.43) [3.18, 22.10]	6.50 (4.09) [-2.91, 15.75]	8.89 (7.10) [-6.82, 23.99]	3.64 (4.99) [-9.76, 16.19]
Missing at least one parent/100K	18.29 (5.63) [2.38, 30.13]	20.78 (5.48) [9.03, 34.37]	14.66 (5.37) [1.73, 25.90]	16.12 (9.13) [-3.90, 36.73]	10.18 (8.32) [-13.74, 30.43]
Missing both Mom and Dad/100K	3.64 (1.43) [0.28, 6.62]	2.53 (1.62) [-1.34, 5.77]	2.19 (1.35) [-1.05, 4.88]	2.87 (1.65) [-0.59, 6.14]	2.14 (1.80) [-3.39, 7.06]
Grandparent head of HH/100K	6.02 (2.17) [1.60, 11.12]	8.71 (2.54) [3.08, 14.39]	3.46 (2.49) [-1.93, 9.39]	6.25 (3.12) [-0.66, 12.77]	6.09 (2.61) [-1.46, 13.66]
Non-parent head of HH/100k	11.45 (2.62) [6.10, 17.53]	14.78 (3.96) [5.76, 24.58]	7.63 (2.59) [2.35, 14.12]	11.83 (3.80) [3.85, 19.39]	8.91 (2.65) [1.47, 15.14]
Foster child / 100k	-0.07 (0.48) [-1.10, 0.95]	-0.20 (0.57) [-1.45, 0.97]	0.11 (0.53) [-0.95, 1.47]	-0.70 (0.78) [-2.40, 0.83]	-0.12 (0.70) [-1.73, 1.79]

Data are from the Annual Social and Economic Supplement (ASEC) of the March Current Population Survey (CPS). All models include fixed effects for age, year, and state, plus the fraction of observations that were female, Black (non-Hispanic), other race (non-Hispanic) and Hispanic. The model includes 17 ages x 51 states x 26 years = 22,542 observations. Standard errors clustered by state are reported in parentheses; 95% confidence intervals estimated via a clustered (at state) wild bootstrap reported in brackets. Baseline results reproduced in the first column. The second column includes a fourth order polynomial in the state's parent-aged population. The third column restricts the sample to children living in urban areas. The fourth column restricts the sample to children living in non-metro areas. The fifth column restricts the sample to the four triplicate states with large populations (California, Illinois, New York, and Texas) as well as the four non-triplicate states with the largest populations as of 1990 (Florida, Pennsylvania, Ohio, and Michigan).

Appendix Table B6
2SLS Estimates of the Impact of Cumulative Drug Deaths of Likely Parents on the Living Arrangements of Children by Race

Parameter Estimates (Standard Errors) [cluster wild bootstrap 95% CI] on $CEXPOSURE_{ast}$

Dependent variable	Mean for White Children	Restrict to White Children	Mean for Black Children	Restrict to Black Children
Mom not in household/100K	5,494	10.83 (3.07) [4.28, 17.58]	10,363	19.52 (19.92) [-14.48, 67.13]
Dad not in household/100K	18,318	7.24 (4.71) [-6.21, 18.31]	56,477	52.42 (50.33) [-56.23, 179.70]
Missing at least one parent/100K	21,603	15.49 (6.55) [-1.83, 31.18]	60,379	61.24 (47.19) [-14.05, 226.30]
Missing both Mom and Dad/100K	2,208	1.86 (1.15) [-0.87, 4.37]	6,461	13.62 (13.98) [-8.52, 49.79]
Grandparent head of HH/100K	4,214	5.91 (1.88) [0.84, 9.77]	10,470	16.44 (22.12) [-22.91, 73.85]
Non-parent head of HH/100k	7,935	10.30 (2.62) [3.73, 16.12]	16,739	45.21 (31.44) [-3.45, 144.40]
Foster child / 100k	241	-0.50 (0.32) [-1.15, 0.11]	631	7.53 (6.84) [-4.43, 30.36]

Data are from the Annual Social and Economic Supplement (ASEC) of the March Current Population Survey (CPS). All models include fixed effects for age, year, and state, plus the fraction of observations that were female and the fraction that were Hispanic. The model for Black children includes 18,013 observations; the models for white children include 22,529. This difference is due to some age-state-year combinations having zero children in that cell. Standard errors clustered by state are reported in parentheses; 95% confidence intervals estimated via a clustered (at state) wild bootstrap reported in brackets. For the estimate of the effect on "Dad not in household" for Black children, the conventional clustered wild boot strap would not find an upper limit to the confidence interval, and so we use a subcluster wild boostrap as suggested by MacKinnon and Webb (2018).

Appendix C
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