The Effect of SNAP Work Mandates on Crime: Evidence from Ohio

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Work mandates are a contentious component of most welfare programs in the U.S., meant to decrease dependence on the programs. Yet there is insufficient literature on their effectiveness, particularly on salient social outcomes like crime. This paper uses variation from SNAP work mandate waivers in Ohio to evaluate their effect on crime with a fixed effects model. Results indicate that work mandates have strong crime-reducing effects, robust to model specification. The broad nature of the estimates also mask heterogeneity in their distribution among poverty level. Separating effects by poverty level and creating a "crime elasticity of SNAP cases" metric shows that offenses are less sensitive to work mandates at higher levels of poverty. Finally, I create an interval with available statistics to estimate that TOT effects could be anywhere from 4.5-7.7x larger than the observed ITT.

I. INTRODUCTION

Welfare policy in the U.S. has a contentious history, and it consistently lags behind other developed countries in metrics such as poverty reduction and generosity to the poorest (Alesina et al., 2001). Some argue that in the U.S., welfare is meant only to provide temporary assistance, funneling users toward employment. To this, many of the largest programs like Unemployment Insurance (UI) and Temporary Assistance for Needy Families (TANF) have time limits and/or work mandates. The Supplementary Nutrition Assistance Program (SNAP) is not an exception. It provides redeemable food vouchers to qualifying low-income individuals and families, serving over 40 million people in 2018 (Center on Budget and Policy Priorities, 2018). Its work requirement affects just Able-Bodied Adults Without Dependents (ABAWD) who represent 13% of SNAP beneficiaries nationwide (Urban Institute, 2019). The work requirements limit ABAWDs to just 3 months of benefits in 3 years, if they do not work 80 hours a month.

In 2009, the American Relief and Recovery Act was passed to address the deep economic downturn. One of its measures was to suspend welfare work mandates retroactively and into the future, resulting in a blanket suspension from 2007-2013. After 2013, states were given the power to accept or deny waivers for SNAP work mandates at any level—county, city, or town. A later 2016 policy change subjected states to certain requirements for waived areas (USDA, 2016). The variation in the acceptance and denial of these waivers provides a natural experiment that this paper exploits. When states gained control of waivers in 2014, Ohio Governor Kasich denied them for 72 out of 88 counties in the state. His administration exempted counties based on 12-month unemployment numbers for 2013—those with unemployment 120% the national average received waivers (Policy Matters Ohio, 2015).

II. LITERATURE

Much literature is interested in how the effects of welfare manifest; Moffitt (1992) explores labor market responses and the reinforcement of conditions necessitating welfare. Crime receives similar treatment in economic literature, with comprehensive investigations into its mechanisms and motives. In seminal papers on the subject, Becker (1968) establishes a cost-benefit framework of criminal behavior, and Ehrlich (1973) finds links between income inequality and crime, while formalizing crime as an occupational choice under uncertain conditions.

Both heavily inform more modern forays into criminal behavior and its relationship to welfare spending. Machin and Magir (2004) build directly off of Becker and Ehrlich, studying the effects of labor markets on crime. They find that low-skill workers experience the highest crime rates in tightening labor markets, which is a reflection of their relatively higher returns to crime. Similarly, Raphael and Winter-Ebmer (2001) find positive effects of unemployment on crime, further suggesting a channel of increasing relative returns to crime with decreasing income. In opposition to these theoretical mechanisms, Burek (2005) indicates that social welfare may increase crime, via a number of channels including increasing disincentives to work, resulting in negative community effects. Liebertz and Bunch (2018) discover varied effects of TANF and work mandates on crime. Welfare restrictiveness at high levels of poverty may increase property crime, and at low levels decrease it, while restrictiveness increases violent crime in general. Ultimately, previous work has come to no particular consensus.

When investigating SNAP work mandates, we may expect to see similar conditions to those found by Machina and Magir (2004): the work mandates create an artificially tight labor market as the shock in demand for work leads to shortages, and at the lowest end of the wage distribution, the returns to crime likewise experience a positive shock. Corman et al.

(2014) and Corman et al. (2018) propose a different channel, finding that work mandates reduce crime by increasing employment and thus lowering returns to crime. Schoeni and Blank (2000) and Meyer and Sullivan (2004) similarly conclude that welfare employment reform has negative effects on crime, looking at subgroups like women. They theorize that the employment effect is a competition between the increased employment rate of those who are able to find it, and the continued unemployment of those unable to. When the employment-on-crime effects outweigh the higher-returns-to-crime effects, then crime rates will decrease and vice versa. The direction of the effect may differ based on the size of the affected population, differential effects between populations, and the intensity of treatment among populations.

Previous research on work requirements and welfare displays a variety of identification strategies. Gupta et al. (2003) look at a cross-section of countries, focusing on the relationship between welfare health spending and health outcomes. Their findings show positive relationships between the two. Trans-national cross-sections are dubious at best when looking for causal interpretations, and the inability to extricate in a more granular manner the affected populations remains a weakness of many macro-level attempts like these. Moreover, cross-national regressions have distinct flaws, being unable to account for the myriad factors that vary from country to country by either omitting or lacking measurements for these factors. McCartney (2008) highlights both concerns, asserting that cross-country regressions are less useful for answering specific, microeconomic, and causal—rather than associational—questions.

Accordingly, the results from other cross-sectional approaches to welfare and crime have generally yielded effects in the same, expected direction yielding an inverse relationship between social welfare and crime (Defronzo, 1983, 1997; Hannon, 1998; Pratt and Godsey, 2002; Zhang, 1997). This could suggest the presence of pervasive endogeneity, in the form of non-random selection to treatment, wherein units with less crime tend to receive more welfare. Some have used panels to observe unit changes over the time dimension, with similar results (Grant and Martinez, 1997; Burek, 2005). Worrall (2005) employs a fixed effects model to examine California welfare and crime at the county level, finding no effects for five of eight Part 1 offenses. However, he uses less than 525 observations with no exogenous variation to make claims for causality, and ultimately presents a flawed case.

In recognition of these shortcomings, Foley (2011) and Carr and Packham (2019) attempt to evaluate a more focused question, using a natural experiment involving changing benefit schedules. Both find negative effects of more staggered benefit schedules on crime, with large magnitudes, suggesting high degrees of salience in increasing effective availability of benefits. The random nature of the policy application and the robustness of their findings suggests their strategy was a good one, and that finding accurate effects for need-based transfers relies on plausibly random variation.

This study follows in the footsteps of more recent work that seek to use natural experiments and random variation to investigate questions of welfare and crime. In the case of Ohio in 2014, this variation is the allotment of work mandate waivers to states. Though not entirely random—mandate counties were the poorest, and most rural—the policy-change variation in SNAP benefits across similar units adds to the literature of stronger identification strategies.

III. DATA

III. A. Data Sources

Data was primarily drawn from the Uniform Crime Report (UCR), sorted into county-level data on crime statistics database ICPSR. The data from ICPSR cover 2010-2016 but do not include 2015, since it was not in the database. For that, the Ohio Office of Criminal Justice Services Crime By County Statistics provided the same information on crimes by county, drawn from the UCR's master file on Ohio. Due perhaps to the timing of data collection or differing standards, there are discrepancies between the two sources. Estimates from the UCR reports were consistently larger, by around 1%. However, the differences were largely consistent between the two sources every year. To account for the differences, I use OCJS data for 2010-2016 for Ohio. Incorporating time-fixed effects will account for the consistent differences between OCJS data and UCR data for the other states without skewing estimates. Unfortunately, the lack of data from 2015 could not be rectified for the other states in the sample.

Figures for SNAP disbursement by county were retrieved from the Department of Agriculture, in the Food and Nutrition Service's (FNS) Bi-Annual State Project Area/County-level Participation and Issuance Data. Unemployment numbers at the county level came from the Local Area Unemployment Statistics (LAUS) at the Bureau of Labor Statistics. Population statistics came from the US Census Bureau's County Population Totals: 2010-2019.

III. B. Sample Construction

The sample was constructed using Part 1 offenses reported in both crime sources. These are separated into violent and property offenses: Murder, rape, robbery, and aggravated assault are violent crimes, while burglary, larceny, motor vehicle theft, and arson fall under property. Each of these, as well as the aggregates of violent, property, and total offenses reported are reported per 100,000, by state and by county. The sample includes 3 states: Indiana, Michigan, and Ohio. Indiana has 92 counties, Michigan 83, and Ohio 86. From Ohio, two counties were removed from the sample—Noble County and Seneca County—because the OCJS did not contain all 7 years of observations for either.

The explanatory variable of interest is Mandate. Mandate is a variable indicating that a county is treated with the work mandate in that year, where 1 is a county under the mandate and 0 is county with a waiver. To measure SNAP use in a county, the variable used is a measure of the number of cases. Cases are broken up by household and personal cases. Because they have no dependents, ABAWD are not typically part of a household, so the personal case numbers will more accurately capture changes for them. Where stated, SNAP cases are population adjusted to the number of cases per 100,000.

III. C. Sample Description

Table 1 gives the selected means from Ohio counties in the pre-period, 2010-2013. Mandate and non-mandate counties here are selected based on waiver status in 2014, the first treatment year in Ohio. The table shows that even before the treatment, the two groups of counties differed greatly. In both, property offenses far outnumber violent, and mandate counties had more of both. Similarly, mandate counties had lower unemployment rates and higher SNAP caseloads per 100,000. The clear difference in baselines would confound any simple correlation, associating high benefits with higher population-adjusted rates of crime.

Likewise, comparisons across states show disparities. In Table 1 under States, means are given by state. Indiana and Michigan are both adjacent to Ohio, and together may provide comparisons. Total offense rates suggest that Michigan and Ohio have similar profiles, but Michigan violent crime is nearly double that of Ohio while Indiana is around the same. In 2010, unemployment and SNAP cases per 100,000 differ significantly between all three, but are closer between Indiana and Ohio. The difference in population adjusted crime, unemployment, and SNAP cases do not imply that Michigan and Indiana are imperfect comparisons, though. Their proximity to Ohio is the most important factor, which may inform how similarly they respond to treatment and what treatments they experience. In other words, they should exhibit similar pre-trends.

Table 1: Sample Means Across Groups

	Ohio Counties 2014			States		
	Mandate	No Mandate	Difference	Indiana	Michigan	Ohio
Part 1 Total	2465	1638.7	819.51 *** (179.8667)	1909.3	2129.7	2330.1
Part 1 Violent	146.9	62.87	735.94 *** (167.8036)	140.7	246.8	133.2
Part 1 Property	2318.1	1575.9	83.57 *** (18.80624)	1768.6	1882.9	2196.9
Murder	2.139	1.903	0.2285 (0.4144549)	2.173	2.544	2.1
Rape	22.45	11.01	11.44 *** (1.912297)	15.96	59.99	20.58
Robbery	48.24	15.44	32.66 *** (9.088071)	27.95	24.35	42.89
Aggravated Assault	74.08	34.52	39.32 *** (10.00485)	94.66	159.7	67.62
Burglary	642.5	433.1	207.56 *** (56.03528)	400.2	501.4	608.3
Larceny	1593.2	1091.6	497.46 *** (114.7872)	1275	1301.1	1511.3
Motor Vehicle Theft	83.25	51.2	30.92 *** (10.39918)	93.4	80.34	77.27
Arson	15.39	8.257	7.08 *** (2.601118)	8.475	13.33	14.22
Unemployment	8.777	11.94	-3.16 *** (0.2802048)	9.053	11.17	9.294
SNAP Cases (per 100,000)	13675.3	19539.5	-5880.3 *** (768.9201)	12395.9	17770.8	14632.7
Poverty (%)	14.81	18.56	-3.75 *** (.63817)	13.88	16.53	14.95

*significant at 10% **significant at 5% ***significant at 1%

IV. METHODOLOGY

IV. A. Fixed Effects Model

At the beginning of 2014 in Ohio, SNAP benefits were made conditional on employment status, with the exception of 17 counties. The identification strategy relies on the differences in outcomes between the two groups. However, the counties untreated by the work mandate change as there is movement to and from the treatment groups. A simple difference-in-differences design is unable to account for this. If an observation is in the post-period, and will be treated at some point, it must be coded with a 1 for the post variable and 1 for the treatment variable; however, in the case of this model, some observations are not treated while still in the overall post-period. Others still will go from treatment to non-treatment, while it is still the overall post-period. Attempting to code these counties with post=0 until they are treated creates perfect collinearity. Table 2 breaks down the non-mandate counties, showing the movement to and from the group. Italicized counties move to the treatment in the following year, and newly untreated are underlined.

A two-way fixed effects model does not have these issues. It is capable of accounting for this movement as well as effectively analyzing panels, since the treatment variable is an indicator for treatment in just one unit time, rather than at any point. The primary fixed-effects model is given by:

$$y_{ist} = Mandate_i\beta + X_i\delta + T_t\lambda + S_s\theta + \varepsilon_{ist}$$

Here, there are 4 pre-periods, 2010-2013, and a number of post periods within 2014-2016 for counties Ohio. *Mandate* is an indicator for treatment status, X_i are county-fixed effects while T_t is vector of time fixed effects in the form of year dummies. $S_s\theta$ is a vector of controls including county-level unemployment rate. Additionally, a second model is used to estimate crime effects by quartile of poverty to uncover heterogeneity. β remains the coefficient of interest, but the treatment variable *Mandate* is interacted with each quartile of poverty.

$$y_{ist} = \sum_{n=1}^{4}(Mandate_i \times Quartile\ of\ Poverty_i)_n\beta + X_i\delta + T_t\lambda + S_s\theta + \varepsilon_{ist}$$

IV. B. Assumptions

Though not a DiD design, the same assumption of parallel trends must hold in this case. Figure 1a, 1b, and 1c shows Ohio counties that receive the work mandate versus those that do not in 2014. 1a shows trends in total Part 1 offenses in Ohio. The trend going back to 2010 is very similar, only diverging after treatment. Figure 1b depicts the same: property offenses for both groups appear to trend in tandem until 2014. In Figure 1c, is there potential for concern. Though neither trend is strong, they appear opposite. In order to test for a difference, I perform a t-test on the pre-trend fitted lines for each. With a p-value of .6569, the test finds no significant difference between the two coefficients, meaning the trends are sufficiently parallel.

Table 2: Non-Mandate Counties in Ohio

2014		2015		2016	
FIPS Code	Name	FIPS Code	Name	FIPS Code	Name
001	Adams	001	Adams	001	Adams
015	*Brown*	007	Ashtabula	007	Ashtabula
027	Clinton	027	Clinton	015	Brown
031	Coshocton	031	Coshocton	027	Clinton
071	Highland	071	Highland	031	Coshocton
077	Huron	077	Huron	053	Gallia
081	Jefferson	079	Jackson	071	Highland
105	Meigs	081	Jefferson	077	Huron
111	Monroe	105	Meigs	079	Jackson
119	Morgan	111	Monroe	081	Jefferson
123	Muskingum	119	Morgan	105	Meigs
127	Perry	123	*Muskingum*	111	Monroe
131	Pike	127	Perry	123	Morgan
145	Scioto	131	Pike	127	Perry
		145	Scioto	131	Pike
				145	Scioto
N	15		16		17

Parallel trends must also hold across states. For every year in 2010-2016, Michigan uses work mandate waivers across the state, so it is in essence a control state. Indiana also accepts state-wide waivers until 2016. Thus, pre- trends in both can only ever be non-mandate trends, since in both states, all counties are always in one group or the other. Figure 2a shows total Part 1 offense trends for all 3 states, and both mandate and non-mandate counties in Ohio. Indiana passes a simple eye test, mimicking paths of both Ohio groups. Michigan trends generally down, but not in the same manner. To test the parallel assumption here, a t-test equating the trends of all

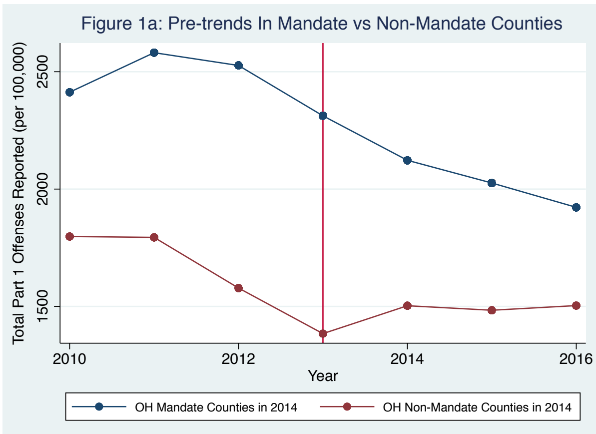

Figure 1a: Pre-trends In Mandate vs Non-Mandate Counties

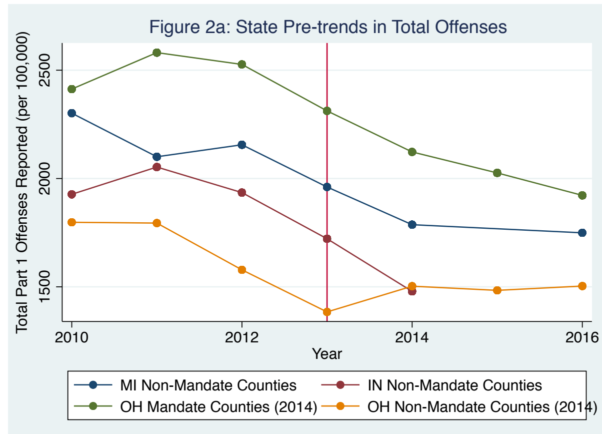

Figure 2a: State Pre-trends in Total Offenses

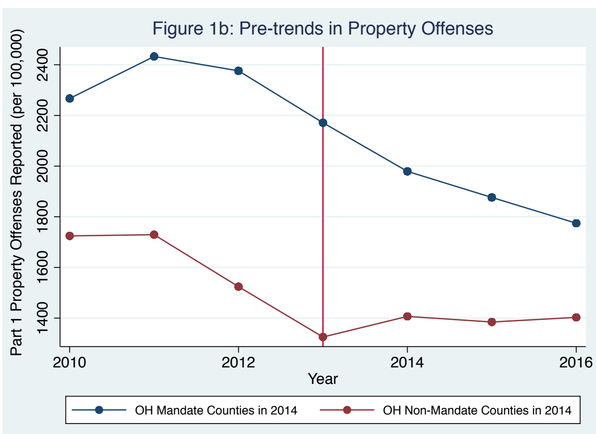

Figure 1b: Pre-trends in Property Offenses

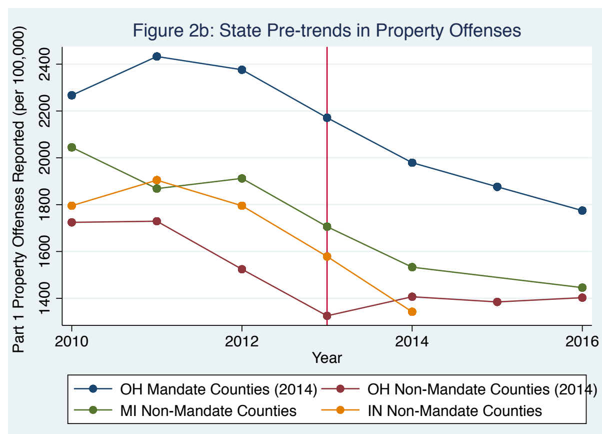

Figure 2b: State Pre-trends in Property Offenses

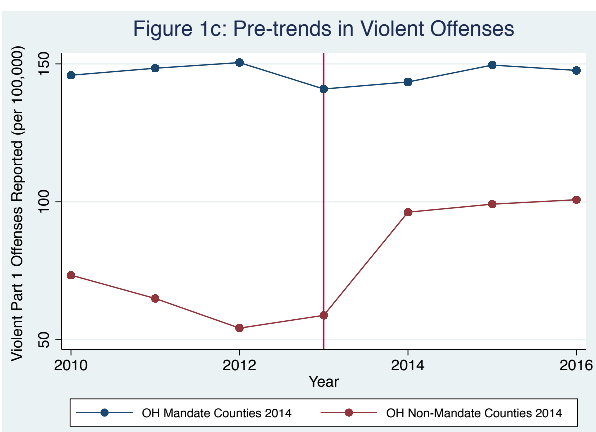

Figure 1c: Pre-trends in Violent Offenses

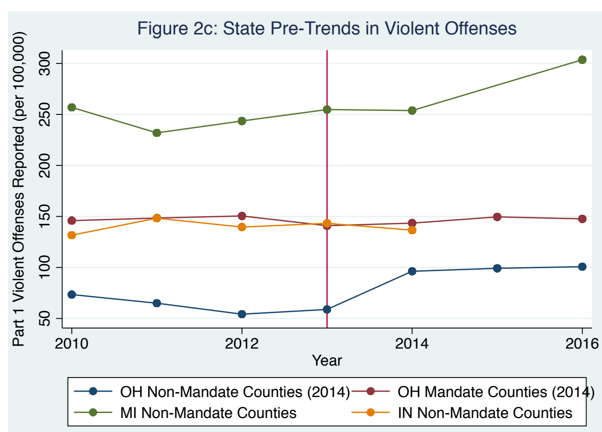

Figure 2c: State Pre-Trends in Violent Offenses

groups yields a p-value of .8106, meaning there is no evidence that they are different. Tests on Michigan against each of the other trends individually reveals the same. Figure 2b, Part

1 property offenses, is nearly identical to 2a, and t-tests reveal that all of its trends are sufficiently parallel. Violent crime trends (Figure 2c) again resist eye-test. While Michigan and both Ohio groups seem to decrease slightly over the period, Indiana increases slightly. Tests finds no evidence that these trends are significantly different. Yet both graphs of violent crime present some evidence that violent offenses may not be as tightly intertwined with conditions linking property offense trends between states. Overall, analysis indicates that a fixed effects model will not be confounded by inconsistent non-parallel paths.

V. RESULTS

V. A. Full-Sample

Beginning with the full sample of all states, I estimate the effects on 3 different types of crime: Part 1 Total, violent, and property. The first model is the most basic specification, and gives negative estimates—around a 200 per 100,000 decrease in total offenses—all of which are significant. With county-level fixed effects, there is a large jump in effect size, and a close to halving of standard errors. The exception is violent crime, which is now positive. Adding in year dummies bumps up standard errors and drops the magnitude of estimates, while the further addition of an unemployment control increases the magnitude of property effects enough to be significant. The last model (4) is preferred, since the two-way fixed effects remove much undesired bias and the unemployment control is a demonstrably constructive addition. This specification shows decreases across all crime outcomes, which are significant at varying levels. From a 4-year (2010-2013) mean, that is a 5.4 percent reduction in Part 1 offenses, 4.3 percent for property offenses, and 17 percent for violent offenses. The modest drop in property crime is unsurprising (Corman et al., 2014), while the estimate for violent crime is particularly surprising and largely uncorroborated in the literature.

Table 3: Full-Sample Specifications, Part 1 Offenses

	(1)	(2)	(3)	(4)
Part 1 Total Offenses	-200.57 ***	-450.76 ***	-99.51 **	-113.81 **
	(74.92)	(39.45)	(45.74)	(47.92)
Part 1 Property Offenses	-166.16 **	-451.91 ***	-69.53	-84.29 *
	(68.74)	(37.33)	(42.9)	(45.19)
Part 1 Violent Offenses	-34.42 ***	1.16	-29.98 ***	-29.51 ***
	(10.28)	(5.17)	(7.04)	(7.08)
County-Level Fixed Effects	No	Yes	Yes	Yes
Year Dummies	No	No	Yes	Yes
Unemployment Control	No	No	No	Yes
N	1652	1652	1652	1652

*significant at 10%   **significant at 5%   ***significant at 1%

V. B. Restricted versus Full Sample

The primary model chosen in Table 3, while preferred among the others, is imperfect. There are concerns about its inability to control for the state-level heterogeneity of response to various shocks in the same period. State policing policy, the incentives created by the state criminal justice systems, and the level of welfare spending in each state could be potential sources of bias in the full-sample results. To counter this, I restrict the sample to just Ohio and combinations of Ohio and the other two states, with the full sample for comparison. By itself, Ohio shows consistently significant results and larger point estimates. The steep reduction in sample size prevents them from being more precise. Models 2 and 3 are not particularly useful, with effects of varying significance and magnitude.

Table 4: State Specifications

	OH	OH & MI	OH & IN	OH & IN & MI
	(1)	(2)	(3)	(4)
Part 1 Total	-284.98 **	-123.52 *	-72.24	-113.81 **
	(123.89)	(65.56)	(64.52)	(47.92)
Part 1 Property	-251.35 **	-91.48	-59.94	-84.29 *
	(115.05)	(62.38)	(61.07)	(44.91)
Part 1 Violent	-33.63 **	-32.04 ***	-12.3	-29.41 ***
	(13.9)	(7.64)	(8.29)	(7.08)
N	602	1100	1154	1652

*significant at 10%   **significant at 5%   ***significant at 1%

The results from the restricted Ohio sample reveal that the full sample may not be yielding the best estimates. Some sources of bias between counties from each state could be bypassing the fixed effects and positively biasing the results. On the other hand, the full sample might have effectively accounted for a region-wide shock that affected Ohio mandate counties differently than it did non-mandate counties. Ultimately, the tradeoff between potential sources of bias means that both are useful to continue investigating.

V. C. Effects by Level of Poverty

Previous work on this topic suggests that work mandates may have different effects conditional on the level of poverty in an area (Liebertz & Bunch, 2018). If the poverty level is high, it follows that more recipients of welfare may either be unqualified for a job, or that there are less jobs in the area to have. To see if a singular treatment variable is masking heterogeneity across poverty levels, I use the secondary model proposed earlier, where the treatment *Mandate* is interacted with quartiles of poverty. Quartiles are calculated according to the distribution of poverty in both the relevant sample.

Table 5A gives the results of the model, where the Bottom quartile represents the lowest rates of poverty. Between both specifications, the restricted Ohio sample shows the most consistently significant results, while only five of twelve estimates in the full sample are significant. In both, estimates tend to lose precision as poverty level increases, which may be expected given that the distribution of poverty is right-skewed, so there are less observations at higher levels of poverty. The effects also appear to be strongest in the Lower Middle and Top quartiles, each experiencing decreases of over 300 total Part 1 offenses per 100,000. This trend is driven largely by property crime. For violent crime, though the Lower Middle also has large comparative effects for violent crime, the Top quartile is more similar to the others.

Table 5A: Crime Effects by Level of Poverty

	Quartile of Poverty			
	Bottom	Lower Middle	Upper Middle	Top
	(1)	(2)	(3)	(4)
OH				
Poverty Rate Mean	9.3	13.18	16.34	20.9
Part 1 Total	-253.02 *	-354.49 ***	-229.29	-321.08 **
	(135.09)	(132.09)	(149.52)	(161.96)
Part 1 Property	-224.61 *	-298.8 **	-207.65	-294.89 *
	(126.69)	(123.17)	(140.19)	(151.86)
Part 1 Violent	-28.41 **	-55.69 ***	-21.64	-26.18
	(13.93)	(16.55)	(15.86)	(16.01)
N	602	602	602	602
OH & IN & MI				
Poverty Rate Mean	9.39	13.36	16.43	20.74
Part 1 Total	-96.99	-147.44 **	-105.17	-95.83
	(64.48)	(62.98)	(92.47)	(101.36)
Part 1 Property	-64.26	-109.25 *	-82.34	-83.6
	(60.08)	(60.11)	(87.66)	(95.28)
Part 1 Violent	-32.73 ***	-38.18 ***	-22.78 **	-12.23
	(8.89)	(11.20)	(10.64)	(11.68)
N	1652	1652	1652	1652

*significant at 10%   **significant at 5%   ***significant at 1%

The raw point estimates are not the best metric, though, considering that each level of poverty inherently has different initial levels of crime. Table 5B presents the estimates as percentage decreases from their 4-year means for Ohio, since the full sample results are imprecise. The results show that there is a sharp drop-off in relative effectiveness of the mandate from the Bottom and Lower Middle quartiles to the Upper Middle and Top, consistent with the proposed mechanism of heterogeneous effects. Similar to the non-stratified treatment results from both the full and restricted samples, violent offenses decrease the most. However, the revised % change estimates show enormous effects in the bottom two quartiles, that taper off sharply to suggest little or no different effect from property crime in the Top quartile. This further nuances our findings, indicating that the violent crime-reducing effects of work mandates are considerably lessened for the poorest areas.

Table 5B: Crime Effects Relative to 4-Year Mean in Ohio

	Quartile of Poverty			
	Bottom	Lower Middle	Upper Middle	Top
	(1)	(2)	(3)	(4)
Part 1 Total	-14.93%	-15.71%	-9.32%	-9.33%
Part 1 Property	-13.87%	-13.96%	-8.97%	-9.26%
Part 1 Violent	-37.68%	-48.29%	-15.19%	-10.18%

V. D. Mechanism of Crime-Reducing Effects

The general consensus of the literature is that employment effects are responsible for work mandates' influence on crime (Corman et al. 2014; Schoeni & Blank, 2000). After the work mandate is implemented, unemployed ABAWD in the pre-period can be separated into two groups: those who get jobs and those who do not. Employment effects are the sum of the reduction of crime for those who are employed versus the increase in crime for the unemployed. When enough are employed, crime is reduced, and vice versa. At a more fundamental level, SNAP caseloads can also give insights into the mechanisms for crime effects, since changes in cases counts those who could not find jobs. To check the validity of these mechanisms, I look at the raw changes in unemployment and SNAP cases next to the percent changes, by quartiles as well as total.

Table 6: Mechanism of Effect in Ohio

	OH Total	Quartile of Poverty			
		Bottom	Lower Middle	Upper Middle	Top
Unemployment Rate	.72 * (.34)	0.79 * (.41)	0.79 ** (.39)	0.57 (.39)	0.71 * (.41)
% Change	8.2%	10.5%	9.2%	6.1%	7.4%
SNAP Cases (per 100,000)	-1350.49 ** (601.83)	-1154.56 ** (545.59)	-1455.32 *** (501.53)	-1195.69 * (718.7)	-1823.88 (1889.42)
% Change	-9.9%	-13.6%	-12.3%	-8.1%	-9.2%
N	602	602	602	602	602

Surprisingly, unemployment is up significantly not only in total but in all but one quartile. Meanwhile, SNAP cases are down significantly in most quartiles and in total, showing that work mandates had a definite impact. The 9.9% total decrease is consistent with Harris' (2018) nationwide estimation of SNAP participation decrease due to work mandates. Contrary to proposed theory of poverty levels, work mandates may be more effective at higher levels of poverty. Compared to the lower quartiles, both upper quartiles experience a smaller decrease in the proportion of populations receiving SNAP relative to the total proportion of SNAP recipients. Assuming that the ratio of ABAWD to SNAP recipients is the same across all levels of poverty, then the percent change relative to the total should reflect how easily ABAWD could find employment to stay on SNAP. The assumption that ABAWD to SNAP recipient ratios are constant is unreliable, but the data constrains this paper to make the assumption since there is no way to find how the true ratios vary.

SNAP cases and unemployment seem to vary in tandem across quartiles: the percent increases in unemployment are larger where the percent decreases in SNAP cases are larger. This may reflect aforementioned larger increases in employment for the upper quartiles where work mandates are more effective at increasing employment. However, unemployment rising at all while work mandates are clearly in effect is largely inconsistent with priors. Theoretically, there should only be decreases in unemployment as the affected population of ABAWD are already either unemployed, or employed and experience no change in status due to the mandate. In other words, SNAP dropouts would not have a positive effect on the unemployment rate since they were already counted as unemployed.

The only other population affected, those who become employed, should contribute negatively to unemployment rates.

Anecdotal evidence from Ohio may propose an alternate answer. In multiple counties in Ohio, local grocery stores shut down due to loss of revenue from food stamps. The denied waivers amounted to $464 million dollars of SNAP aid in 2014 alone (Policy Matters Ohio, 2019). Based on this information, we can infer that the increases in unemployment are not due to decreases in ABAWD employment, but in total employment. This implies large spillover effects. Subtracting the employment effects of the work mandates, the magnitude of the spillover is even greater than the .72 pp increase in unemployment from the results.

But that still does not account for observed crime effects. Since even in the face of consistently higher unemployment, crime went down, it is necessary to assume that the crime-preventing returns to a small increase in employment outweighed the crime-incentivizing returns to a larger increase in unemployment. This, of course, carries its own assumptions that places the SNAP-less ABAWD into the same group of unemployment as those unemployed by the spillover effects, who may well be eligible for SNAP and have different incentives for crime. Unfortunately, there is no way to separate the unemployment changes for ABAWD versus non-ABAWD, and no way to calculate the returns to employment via crime-reduction.

V. E. Crime Elasticity of SNAP Cases for Work Mandates

In Table 5B, the relative decreases in crime show that low levels of poverty experience larger total negative crime effects than high levels. But areas with high levels of poverty also tend to have larger proportions of SNAP-using populations, meaning that the Table 5A and B estimates are not adjusted to reflect varying amounts of SNAP users. To create a more universal comparison, I create the following metric:

$$\frac{\%\Delta\ in\ Part\ 1\ Offenses}{\%\Delta\ in\ Proportion\ of\ Population\ that\ are\ SNAP\ Recipients}$$

The crime elasticity of SNAP cases is useful. The denominator essentially measures how well the work mandates increased employment by looking at the negative—how many stopped receiving SNAP as a result of work mandates. Since the only counterfactual is employment, if more people lost benefits as a proportion of SNAP recipients at a level of poverty, then less found employment (once again assuming the ratio of ABAWD to total SNAP recipients is constant across levels of poverty). In other words, if the employment effects were the same across all levels, then the results would show the same percent decrease across all levels since the same proportion of ABAWD would lose benefits. As an elasticity, the metric essentially gives the sensitivity of crime to the effect of the work mandate. This will enable comparisons of how well work mandates decrease crime at each level of poverty.

Table 7: Crime Elasticity of SNAP Cases for Ohio, by Level of Poverty

OH Total	Quartile of Poverty			
	Bottom	Lower Middle	Upper Middle	Top
Part 1 Total	1.098	1.277	1.151	1.014
Part 1 Property	1.019	1.135	1.107	1.007
Part 1 Violent	2.771	3.926	1.864	1.107

Table 7 displays the elasticities of each type of crime at each level of poverty. By poverty level, the trends are consistent across offense types: the mandate's efficacy plateaus in the middle quartiles, and is consistently the lowest in the Top quartile. The difference between the Top quartile and the Lower Middle is 26.3 percent for total offenses, and 12.6 percent between the Lower and Upper Middle. Looking at violent offenses, the results confirm those from tables 5A and B. They are highly sensitive to the work mandate, decreasing in the Lower Middle quartile by almost 4% for a 1% decrease in the amount of SNAP recipients per 100,000 people. On average, violent offenses are 2.24x more sensitive to the mandate than property crimes. Curiously, this sensitivity drops off enormously from the Lower Middle to the Top, to the point where the Top quartile's elasticity for violent offenses is almost the same as other offense types at that level.

The results show broadly that work mandates are more effective in the middle of the poverty distribution. This is likely due to competing effects of low and high poverty on the mechanisms of the work mandate. At the low-poverty end, Table 6 shows that work mandates employ less people but there are still large reductions in offenses, meaning that jobs in low-poverty areas are better at preventing offenses. Practically, this may happen because of higher pay, which spills over more to affected communities. Work mandates at the high end of poverty employ more people, but reduce crime much less. The opposite story from low-poverty areas may be true—high-poverty area jobs pay less, and consequently community effects are reduced. In the middle quartiles, the two effects become weaker and work mandates are more sensitive.

V. F. TOT Estimates

Only a fraction of SNAP recipients qualifies as ABAWD. Nationally, they represent 13% of SNAP users (Urban Institute, 2019). In Franklin County, Ohio, in 2014, that fraction is 22% (Shaw & Hooker, 2016). This paper does not have the advantage of county-level data on ABAWD, and as such cannot produce true TOT estimates. However, it is important to note that such a small relative population as the makeshift first stage of this these estimations implies very large true effects. If the national and Franklin County numbers are used to create an interval (13-22%), then TOT effects could be somewhere from 4.5 to 7.7x the size of the observed ITT.

VI. DISCUSSION

In times of political and economic uncertainty where work mandates are at the forefront of policy debate, it is crucial to understand their effects, particularly on social outcomes like crime. In order to do this, this paper uses variation in work mandate waivers for counties in

Ohio. I compare Part 1 offenses in counties with and without waivers using a fixed effects model. The full-sample specification estimates a 5.4 percent decrease in total offenses in mandate counties, with a 17 percent drop for violent offenses in particular. The sample was then restricted to Ohio to see if results are robust to potential biases at the state level. Results show that work mandates are associated with a 12.2 percent decrease in total offenses rather than 5.4, driven by a large jump in the magnitude of property offense estimates. Violent offense estimates likewise decrease by 5.6 percentage points. Looking at effects by poverty level, only the restricted sample is useful. Point estimates from it suggest that the Lower Middle and Top quartiles experience the largest decreases in total offenses. However, as percent decreases from pre-period levels, the lower half of the distribution declines an average of 15.3 percent and the upper half, 9.33 percent. To investigate the mechanism of these effects, I estimate the mandate's impact on unemployment and SNAP cases. Unemployment increased significantly by .72 pp, which I determine is an externality of the work mandate. SNAP cases decrease by 9.9% overall, and like offenses, decrease more at low poverty levels than high. To understand the true effects of the mandate of each poverty level, I create a metric the "crime elasticity of SNAP cases" that is equal to the percent change in offenses divided by the percent change in SNAP cases. This elasticity shows that total offenses in the bottom and top quartiles are the least sensitive to work mandates while the middle two are the most sensitive, which I speculate is due to opposing wage effects and their weakening in the middle of the poverty distribution. Total offenses for the most elastic quartile (Lower Middle) were 26.3% more responsive to work mandates than the most inelastic quartile (Top). Additionally, violent offenses were an average of 2.24x more sensitive to work mandates than property offenses. Finally, I use findings from other sources to create an interval of ABAWD to SNAP recipient ratios, and estimate that TOT effects are 4.5-7.7x the size of the observed ITT effects.

These findings should be interpreted carefully, with some cautions. First, the differences in the full versus restricted sample suggested a large amount of bias in one or both of the samples. Regional shocks or trends could have heterogeneous effects on crime due to State-specific sources of bias correlated with crime (state policing policy, criminal justice policy, state welfare provision). Conversely, the full sample may have correctly accounted for a region-wide shock that affected Ohio mandate counties differently than it did non-mandate counties. Second, the paper was limited by a lack of power. The full sample contained just 1652 observations over a 7-year period. The restricted sample had just 602 observations, and data availability would not allow either the pre- or post-period windows to be extended. As a result, many estimates were likely imprecise, rather than systematically insignificant, but should still be taken with wariness. Finally, the major impediment to this paper was a lack of particular types of data at the county-level. Though the push to quantify things is continuing, the fact that county level data does not exist for measures as simple as GDP from 2010 seriously harmed efforts to control for many salient covariates. This extends to county-level data on ABAWD, which would have improved the quality of the analysis tremendously.

The results have many implications for work-mandate policy. If work mandates appear more effective in low-income areas in terms of increasing employment, then increasing the employment-inducing and crime-reducing efficacy of work mandates is as simple as subsidizing wages in those areas rather than reducing benefits. Higher wages create better, positive incentives to find jobs and per the community spillover effects, will reduce more crime. Importantly, my results imply that work mandates may cause more people to be unemployed than ABAWD they employ, by a large margin. The spillover effects observed in mandate counties increased unemployment by more than .72 pp, due to the large amounts of money leaving—around $1.4 billion in three years (Policy Matters Ohio, 2019). This should be a warning to policymakers about the externalities of large reductions in welfare programs. However, a crucial dimension along which this paper and many others have not evaluated work mandates is the welfare of those receiving benefits. Our results show that an average of 1350 people per 100,000 lost SNAP benefits, and had no job. The personal welfare implications of that are staggering, and need to be explored beyond the quantification of their need to commit crime to survive. Future research should address the several pitfalls of this paper as better, more granular data becomes available, and start to evaluate the welfare effects of work mandates.

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