The Impact of Living Wage Ordinances on Urban Crime

Jose Fernandez
Department of Economics
University of Louisville
jose.fernandez@louisville.edu

Thomas Holman
San Francisco, California
thomas.t.holman@gmail.com

John V. Pepper
Department of Economics
University of Virginia
jvpepper@virginia.edu

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ABSTRACT: We examine the impact of living wages on crime. Past research has found that living wages appear to increase unemployment while at the same time providing greater returns to market work. The impact on crime, therefore, is unclear. Using data on annual crime rates for large cities in the United States, we find that living wage ordinances are associated with substantial reductions in property related crime and little impact on non-property crimes.

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I. Introduction

Over the past 15 years, a number of city governments have adopted living wage ordinances mandating wage floors exceeding the federal minimum wage for certain classes of workers. These wage floors have been found to impact the labor market for low skilled workers, leading to a fairly substantial increase in expected wages and a small but statistically significant reduction in employment [Neumark and Adams (2003b), Campolieti, Fang, and Gunderson (2005)]. To the extent that these wage floors impact the labor market of the low-skilled, they might also affect non-labor market behaviors such as crime. In this paper, we examine the unintended impact of living wage ordinances on crime.

In the standard neo-classical model where the propensity to commit crime responds to the expected benefits of the illegal activity, the opportunity cost of working in the legal sector should play a role in crime (Becker, 1968; Levitt, 1997). These ideas, in fact, are supported in the existing empirical literature which reveals a modest effect of unemployment and a somewhat more pronounced and lasting effect of wages on crime (see, for examples, Freeman, 1999; Gould etl al., 2002; Grogger, 1998; Lin, 2008). This model, however, does not lead to a qualitative prediction about the impact of wage floors on crime. Rather, by simultaneously increasing the returns to employment and the likelihood of unemployment, theoretical predictions about the impact of living wage ordinances on crime are ambiguous. An increase in wages among the working low skilled might be expected to decrease crime, yet the associated decrease in employment might lead to an increase in crime. Given that the living wages have been found to have only a small negative impact on employment but a much more pronounced positive effect on wages, we might reasonably speculate these ordinances lead to a reduction in crime.

Ultimately, however, this is an empirical question which, to our knowledge, has not yet been addressed in the literature. [1]

To resolve this ambiguity, we use panel data on annual city level crime rates and living wage ordinances from 1990-2002. For each city-year, we observe the rates of homicide, robbery, burglary and motor vehicle theft (MVT), as well as the minimum and the living wage. Given these data, the basic empirical approach compares crime rates in cities before and after the adoption of living wage laws, as well as crime rates between cities that did and did not adopt living wage ordinances.

As with all such studies, a primary concern is that living wage ordinances are not exogenous; living wages may be adopted or changed in response to factors that are unobserved to the econometrician but are arguably associated with crime. For example, local labor market conditions, the fiscal stability of local governments, and changes to social services provided by local governments may all be associated with living wage provisions and crime. To account for the nonrandom adoption of living wage laws, we exploit the panel nature of the data. In particular, we include both city and time fixed effects as well as city specific linear and quadratic time trends. This is similar to the strategy used by Adams and Neumark in their work examining the impact of living wages on employment and earnings.

After describing the data in Section II, we evaluate the effects of living wage laws on crime rates in Section III. Cities with living wages have higher crime rates than those

[1] In fact, there is almost no research on the impact of minimum wage laws on crime. Two notable exceptions are Hashimoto (1987) who finds that minimum wage laws increase property crimes (or arrests) committed by teenagers , and Corman and Mocan (2005) who find that minimum wages are associated with a reduction in murders, robberies and grand larcenies in New York City.

that do not adopt, but this association appears to be spurious: after accounting for city specific fixed effects and city specific time trends, we find that living wages have a substantial negative effect on robbery, burglary and MVT. The estimated impact of living wages on homicide is negligible and statistically insignificant.

To further assess how robust our initial findings of the relationship between living wage laws and crime are, we consider three alternative specifications in Section IV. First, we evaluate how the estimated effects of the living wage vary with the types of workers who are covered by the living wage ordinances. In particular, following Neumark and Adams (2003a), we classify living wage laws into those that cover municipal employees, those that cover companies under contract with the city, and those that cover businesses receiving assistance from the city. The faction of low-wage workers potentially covered by living wage ordinances varies substantially across these three types of ordinances, from around 3-6% for laws that cover municipal employees, to 15-20% for laws that cover city contractors, to over 80% for laws that cover business receiving assistance (Neumark and Adams, 2003a).[3] Using this classification, Neumark and Adams (2003a) find evidence that coverage plays a central role in understanding the impact of living wages on the labor market. In particular, they find that ordinances applying to businesses receiving assistance have a pronounced impact whereas those only covering contract workers have no impact at all. Likewise, when we estimate models using these classifications, the results from crime are quite similar. Living wage laws covering

[3] These coverage probabilities reflect the potential number of workers who might be covered, not the atual fractions. Thus, these figures provide an upper bound on the fraction that might be covered by living wage ordinances.

business receiving government assistance have a notable and negative impact on crime. Laws covering contract workers have almost no impact.

Second, we explore whether the estimates are sensitive to the demographic composition of the city. The existing literature has found the effects of the living wage on the labor market are primarily concentrated among low-skilled persons. While we cannot explicitly condition on the skill distribution of the potential offenders -- our city level crime data do not reveal these details –demographic factors associated with skills and crime can be interacted with the living wage. In particular, we find that the impact of the living wage increases with the fraction of persons age 15-19.[4]

Finally, we restrict the analysis to cities that had a formal living wage campaign brought before the city government, some of which passed and others of which did not. Arguably, this sub-sample provides a more natural although smaller comparison group for the analysis (Adams and Neumark, 2005a). When we replicate the analysis on this subsample, we find consistent results. That is, living wages, especially when applied to businesses receiving assistance from the city government, reduce the incidence of burglary and robbery, but have no discernable impact on homicides.

II. Data Description

The data are a panel of annual crime rates and living wage ordinances for the largest U. S. cities (approximately all cities with greater than 100,000 persons) over the period 1990 to 2002. We began with a sample of 240 cities or "census places" in the

[4] In contrasts, much of the research evaluating the impact of living wages on labor market outcomes does attempt to explicitly control for skills. See, for example, Neumark and Adams (2003b).

United States with populations of at least 100,000 in 2000. Out of these 240 cities, 194 are included final sample, with 46 being dropped either due to a lack of information regarding living wage ordinances or due to missing crime data.

Of the 194 cities included in the sample, 24 had successful living wage campaigns. Table 1 lists these cities, along with the 2002 living and minimum wages, and the types of workers or businesses covered by the ordinance. Several important features of living wage ordinances are revealed. First, the living wage can be substantially higher than the minimum wage. Eleven of the twenty four cities have living wages that exceed $9 per hour, and four cities – Boston, New Haven, San Francisco, and San Jose – have living wages of $10 per hour or more. Second, many cities – one third of our sample -- with living wage ordinances are located in states that have minimum wages in excess of the federal floor. Thus, it will be important for us to control for the state minimum wage. Finally, in contrast to the minimum wage, living wage ordinances only cover limited groups of workers; those that are municipal employees, contract workers, or workers in businesses receiving assistance from the state.

For each city-year, we observe the living wage, if it exits, and the rates of homicide, robbery, burglary, and motor vehicle theft as measured by the Federal Bureau of Investigations Uniform Crime Reports (UCR).[5] Table 2 displays basic descriptive statistics for the variables used in the analysis. The first column of sample statistics

[5] Data on the living wage ordinances come from the Employment Policies Institute (www.epionline.org/lw_proposal_map.cfm) and were provided by Scott Adams. In years where living wages were adopted or changed, the annual living wage is computed as a weighted average of the corresponding minimum wage rates or living wage rates that applied in a given city in a given year, where the weights were based on the amount of time that year that each wage rate applied. This was done because while the living wage rate information was available by month, the crime rate information by city was only available on an annual basis.

provides means and standard deviations for all cities across all years, while the next two columns display the sample means for cities that adopted living wage ordinances and those that did not, respectively. Cities that adopted living wage ordinances have notably higher average crime rates than other cities. For example, the average homicide rate in cities adopting a living wage ordinance is 17.57 (per 100,000) whereas the analogous rate for other cities is 10.87.

These differences are further highlighted in Figure 1, which depicts the trends in crime rates for cities that did and did not adopt a living wage law. Crime rates per capita are greater on average in cities that adopted living wage laws, and this pattern holds across all years. However, the drop in crime beginning in the early 1990s was notably more pronounced in cities that adopted living wage laws, especially for homicide and robbery rates. Thus, crime rates in cities adopting living wage ordinances differ in both levels and in trends from the corresponding rates from other cities.

Whether these differences in the levels and trends are due, in part, to the living wage ordinances or simply unobserved factors that confound the observed associations remain unclear. To account for factors that are thought to be associated with both crime and living wage rates, we include a number of covariates in the regressions. Most notably, since many cities with living wage ordinances are located in states with minimum wage rates in excess of the federal floor (see Table 1), we include city-year specific minimum wages in our analysis. Likewise, we include variables measuring the demographic characteristics of the city, the number of police per capita, and the state incarceration rate.[6]

Before turning to the regression analysis, insight into the question of whether living wage ordinances impact crime can be found by comparing the before and after time series patterns in crime rates for states that adopted living wage ordinances. Figure 2 traces out the crime rate time series for cities that adopted living wage ordinance, where horizontal axis measures the number of years before or after the adoption of the living wage ordinance. For robbery, burglary, and MVT, there appears to be a sharp drop in crime around the date the wage floors are adopted. No such drop is observed for homicide rates. Figure 3 reveals that there is no discernable change in the number of police or the incarceration rate at the point living wage laws are adopted. Consistent with Adams and Neumark (2005a) there is a slight increase in unemployment just after living wage ordinances are past.

III. The Effect of Living Wage Ordinances on Crime

To evaluate whether the relationships between living wages and crime depicted in Figures 1 and 2 reflect the effects of living wage laws, we estimate a series of linear mean regression models that account for observed and unobserved city specific characteristics. In Section III.A, we outline the basic fixed effect model that explicitly accounts for unobserved city specific factors that may be related to both crime rates and living wage policies. In Section III.B, we present and discuss the estimates from a series of models

[6] Minimum wage rate data come from the U.S. Department of Labor, the city level number of police per 100,000 from the UCR Law Enforcement Officers Killed or Assaulted data files, the state level year end incarceration rates from the Bureau of Justice Statistics. Population data come from the Census Bureau.

that evaluate the effects of living wages on crime rates. We begin by pooling the data across cities and then extend the analysis to exploit the panel.

A. Model

Formally, consider the linear model

$$Y_{it} = \alpha_t + \beta_1 \cdot \ln(w_{it}^{\min}) + \beta_2 \cdot \max[\ln(w_{it}^{liv}) - \ln(w_{it}^{\min}), 0] + X_{it}\theta + \varepsilon_{it} \qquad (1)$$

where Y_{it} is the log-crime rate for city *i* in year *t*, $w_{it}^{\min}$ is the higher of the applicable state or federal minimum wage rates, w_{it}^{liv} is the applicable living wage rate or zero if there is no living wage ordinance, and X_{it} is the observed vector of other city *i* characteristics in year *t* that are thought to influence the number of crimes. The random variable associated with the living wage, $\max[\ln(w_{it}^{liv}) - \ln(w_{it}^{\min}), 0]$, equals the percentage increase of the living wage relative to the effective minimum wage in cities with non-trivial living wage ordinances and equals zero for all non-living wage cities.

The parameters α_t, β_1, β_2, and θ are unobserved, with α_t being a year fixed effect. Our primary parameter of interest is in learning β_2, the effect of a living wage on log-crime. In particular, holding the minimum wage constant, β_2 measures the elasticity of crime rates with respect to the living wage for non-trivial living wages that exceed the minimum wage.

Finally, the random variable ε_{it} measures unobserved factors influencing crime rates. The conventional assumption is that this unobserved random variable is mean zero independent of all the covariates, $E[\varepsilon_{it} \mid w^{\min}, w^{liv}, X] = 0$, in which case living wage laws are exogenous. Arguably, however, the unobserved factors, ε_{it}, influencing crime are not independent of unobserved factors associated with the city specific living and minimum wages. For example, unobserved local labor market conditions and social programs may influence the passage of living wage ordinances and crime, in which case the observed correlations between living wage laws and crimes rates will be spurious.

To account for this identification problem, we allow for the expectation of the unobserved factors to vary across city and time as follows:

$$E[\varepsilon_{it} \mid w^{\min}, w^{liv}, X] = C_i + T_{1i}t + T_{2i}t^2. \qquad (2)$$

Equations (1) and (2) imply a mean regression that includes a time fixed effect, α_t, a city fixed effect, C_i, and city specific linear and quadratic time trends, $T_{1i}t + T_{2i}t^2$. Thus, the model explicitly accounts for unobserved time and city specific factors that might jointly influence living wage laws and crime rates. The effect of the living wage is identified using within-city variation in living wage after netting out city-specific time trends. This flexible panel data specification should minimize the influence of many unobserved confounders.

We assess the sensitivity of the parameter estimates to different assumptions on city specific parameters, C_i, T_1, and T_2. In each case, we use a least-squares estimator, weighted to account for differences in city populations, to consistently estimate the parameters.

B. Results

Table 3 presents the estimated effect of living wages on crime along with the associated standard errors. The table presents estimates from four specifications: the first, with time fixed effects alone, the second with city fixed effects, the third with a city specific linear time trend, and the fourth with city specific linear and quadratic trends. No other covariates are included in these models.

The estimated effect of living wages is highly sensitive to whether we account for unobserved city specific effects. Model 1 confirms our findings from Figure 1 that cities with living wages have higher crime rates. Once we account for city fixed effects in Models 2-4, however, this observed association appears spurious. Most notably, rather than having a positive impact on crime, the estimates from these fixed effects models imply that living wages decrease the three property related crimes – robbery, burglary and MVT -- analyzed in this study.[7] For example, the elasticity of the robbery rate with respect to the living wage is estimated to be -0.26 in Model 2 and -0.17 in Model 4, and analogous estimates for burglary are -0.14 and -0.26. In models with city specific effects, all three property related crimes have estimated living wage elasticities that indicate a substantial and generally statistically significant drop in crime. In contrasts, there appears to be no impact of the living wage on homicides. The Model 4 estimates on homicide, for example, suggest the living wage has a negligible (-0.02) and statistically insignificant impact.

In Table 4, we present analogous coefficient estimates on the minimum and living wage after including regressors on the city population, the logarithm of police per capita, and the logarithm of the state incarceration rate.[8] Perhaps the most striking results are found in the Model 1 specification, which does not include city specific fixed effects.

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[7] Even though robbery involves taking of property, it is generally classified as a violent crime.

[8] Tables A1 and A2 found in the appendix report the estimated coefficients for the additional covariates with respect to Models 3 and 4.

Without the controls, the Model 1 estimates on the living wage parameters for homicide, robbery, and MVT are positive and statistically significant. The estimate for burglary is small and insignificant (see Table 3). With the control variables, however, the point estimates associated with the three property related crimes are all negative, substantial, and statistically significant while the point estimate for the homicide rate is negligible. In models with city fixed effects and time trends (Models 2-4), the estimates notably increase yet the living wage is still estimated to decrease the incidences of property related crimes, but have almost no impact on homicides.

Below we provide insight into how the magnitudes of these qualitative results appear to be consistent with the literature that finds living wages have a substantial positive effect on wages and a small negative effect on employment.

The coefficient estimates associated with these covariates are generally sensible and consistent with results found in the previously literature.[9] For example, the elasticity estimates associated with incarceration rates found in Models 3 and 4 lie between -0.3 and -0.8, which are comparable to those found in the literature. However, the estimates for homicide exceed those found in other studies within the literature (see Levitt, 1996; 2004).[10] The estimates associated with the policing variable are large and positive in the Model 1 specification, but then become much smaller and statistically insignificant in

[9] For brevity, we do not present the estimated coefficients associated with these control variables. These estimates are available from the authors.

[10] The literature evaluating the impact of prison population on crime finds elasticity estimates -.14 for homicide, -.7 for robbery, -.4 for burglary, and -.26 for motor vehicle theft.

Models 2-4. The sensitivity of the crime on policing regression to city/state level fixed effects is well documented (Levitt, 1997).[11]

Finally, the minimum wage, which covers a much larger fraction of the workforce, also appears to have a negative impact on crime but the results are less precisely estimated than those for the living wage. For example, in Model 4 the estimated elasticity of crime with respect to the minimum wage is substantially negative but statistically insignificant for all four crimes. The lack of within state variation in the minimum wage leads to imprecise estimates of these coefficients. These elasticity estimates are consistent with the results reported in Corman and Mocan (2005), who find that minimum wages are associated with large (and statistically significant) reductions in murders, robberies and grand larcenies in New York City, but are qualitatively different than those found by Hashimoto (1997).

To summarize our primary findings, we observe that living wages have a large negative effect on property related crimes. The estimated elasticities on property crimes lie between -0.10 and -0.25, suggesting that a 1 percentage point increase in living wage results in 0.10 to 0.25 percent drop in property related crime. At the same time, we find that the living wage has no discernable effect on homicide, a crime with weak pecuniary motives.

[11] The literature assessing the impact of police on crime tends to find elasticity estimates around -0.50, whereas the Model 3 and 4 estimates are positive but statistically insignificant. Given the inherent difficulty in identifying the impact of policing on crime, the best studies in this area have used instrumental variables in addition to fixed effects. See, for example, Levitt (1997, 2002) and Evans and Owens (2007). In this paper, we do not attempting to separately identify the impact of policing on crime but rather simply use the force size as a control variable.

These findings are generally consistent with both the literature evaluating the impact of the living wage on the low skilled labor market and the literature evaluating the impact of the labor market on crime. The former literature finds that the living wage has a large positive effect on average wages and only a small negative effect on employment. Thus, to the extent the living wage serves to increase the expected benefits of participating the labor market, we would predict an associated drop in crime, especially crimes with pecuniary motives. The latter literature finds, in fact, that labor market for low skilled persons have a notable impact on crimes with pecuniary motives but little effect on non-pecuniary crimes such as rape and murder (Gould et al., 2002). Since we expect that living wage to impact crime via the low skilled labor market, the lack of relationship between the living wage and homicide suggests that our conclusions are not due to a spurious correlation between these ordinances and general levels of crime.

These estimated elasticities are substantial but notably smaller than estimates found for more direct policy measures aimed at reducing crime. The literature evaluating the impact of incarceration on property crime, for example, mostly finds elasticity estimates of at least-0.25, and in many cases exceed -0.50 (Levitt, 1996; Johnson and Raphael, 2010). Likewise, the literature examining the impact of policing on property related crimes tends to find somewhat higher point estimates of at least -0.50 (Levitt, 1997, 2002 and 2004; and Evans and Owens, 2007).

Finally, further perspective on these estimated elasticities can be found by simulating the impact of the observed living wages in particular cities. Focusing on the fifteen cities that adopted a living wage policy by 1999, we use the estimates from Model 4 with control variables included (see Table 4) to assess the impact of the living wage on the changes in crime from 1999 to 2002.[12] Over this four year period, the living wage increased an average of just over ten percent, although three cities made no change at all and several cities increased their living wage by over twenty percent. Our Model 4 estimates imply that increases in the living wage over this period resulted in an average drop of 1.8% in the robbery rate, 2.8% in the burglary rate, and 1.1% in the MVT rate. This translates into an average annual city level decrease of about 80 robberies, 223 burglaries, and 103 MVTs.

IV. Robustness

To further assess the robustness of our findings, we consider three alternative specifications. First, we evaluate how the estimated effects of the living wage vary with the types of workers who are covered. Second, we explore whether the estimates are sensitive to the demographic composition, namely age and race, of the city. Finally, we restrict the analysis to cities that had a formal living wage campaign brought before the city government, some of which passed and others of which did not.

A. Disaggregated Living Wage Measures

A richer picture of how living wage laws affect crime can be found by exploiting the variation in the types of workers covered by the living wage policies. While some living wage laws effectively cover large fractions of the lower wage labor market, others may only cover a small (e.g., a few percent) fraction. Presumably, wide ranging ordinances should have a larger impact on crime.

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[12] Three cities that adopted by 1999, Baltimore, Hartford, and Tucson, are not included in this analysis because they either are missing some data for the key years (Baltimore) or because the living wage policy was adopted after September 1999.

To assess this possibility, we re-estimate the model in Equations 1 and 2 allowing the impact of living wages to differ by the coverage of the ordinance. In particular, we compare the impact of ordinances that cover all businesses receiving assistance – e.g., tax abatements, grants, low interest loans, or other forms of financial assistance -- from the city government, to those that cover municipal employees or contract workers. To do this, we introduce a variable that interacts the living wage measure, $\max[\ln(w_{it}^{liv}) - \ln(w_{it}^{\min}),0]$, with an indicator variable for whether the living wage provision applies to businesses receiving government assistance. Thus, the associated coefficient reveals the added impact of these *business assistance living wage ordinances* relative to contract worker (and municipal employee) ordinances. We focus on the business assistance ordinance because they cover much larger fractions of low wage workers and have been found to have a much more substantial impact on low wage labor markets than contract-only laws (Adams and Neumark, 2005c; and Neumark and Adams, 2003a).[13]

Table 5 presents the coefficient estimates and standard errors from the Model 4 regressions with this added interaction term. The results from Model 3 are similar. Consistent with the findings of Neumark and Adams (2003a), the effects of living wage laws depend critically on the type of ordinance. In particular, living wages applied to businesses receiving government assistance have large and statistically significant impact on robbery and motor vehicle theft, a large but insignificant impact on burglary, and almost no impact on homicides. In contrasts, with the exception of burglary, living

[13] Neumark and Adams (2003a) argue that business assistance ordinances are likely to have more pronounced affects because of extended coverage and because there are fewer avenues for employers to effectively avoid the wage floor. In contrasts, contractor-only laws often only require the living wage be paid for work done as part of the contract.

wages applied to the relatively small population of contract workers and municipal employees appear to have almost no impact on crime.

B. Age Interactions

The literature evaluating the impact of the living wage finds that the labor market effects are concentrated among low-skilled persons (Neumark and Adams, 2003b). Since UCR crime data do not reveal information on the characteristics of victims or offenders, we cannot explicitly condition on the skill distribution of the potential offenders. We do, however, observe city level demographic factors associated with skills and crime. In particular, we interact the living wage variable with the fraction of the city population between the ages of 15-19. Based on limited schooling and experience, this cohort will have inherently lower skills and is known to have a higher propensity to commit crime than older cohorts (Gould et. al, 2002). Moreover, research on the minimum wage suggests that younger less skilled workers are most likely to suffer adverse disemployment effects of wage floors (Neumark and Wascher, 2008).

Table 5 presents the coefficient estimates and standard errors from the Model 4 regressions with this added interaction term. We find that the impact of living wages increases with the fraction of persons age 15-19. Table 5 also shows that marginal effects of the living wage evaluated at the mean (0.06) and interquartile range [0.05, 0.07] of the fraction of persons aged 15-19. For the three crimes with pecuniary motives, the estimated marginal effects are all negative, although for robbery and MVT the estimates are statistically insignificant when evaluated at the 0.25 quantile. Most notably, the estimated effects converge to zero as the fraction of persons aged 15-19 increase. This suggests that for the youngest least skilled cohorts, the living wage increases property related crimes whereas for the older cohorts the living wage decreases crime. This result is consistent with the notion that younger persons are more likely to be adversely impacted by the disemployment effects of the living wage, whereas older adult workers are more likely to benefit from the wage gains.

C. Failed Campaigns

Living wage campaigns have been unsuccessful in numerous cities. In our sample, for example, 20 of the 194 cities had unsuccessful campaigns.[14] Arguably, cities that have undergone unsuccessful campaigns provide a better control group for estimating the effects of living wage laws than the broader set of all cities. After all, living wage movements may be accompanied by increased attention, organization and public debate on workers at the bottom of the wage distribution. Narrowing the control group to those cities with living wage campaigns, may avoid confounding the effects of living wage laws and living wage campaigns.

Thus, in this section, we estimate the model using the subset of cities that have had living wage campaigns.[15] Table 7 displays the Model 4 coefficients and standard errors for both the aggregated and disaggregated living wage measures using the restricted sample of 44 cities with living wage campaigns. These results are similar to those reported in Tables 3- 5; the evidence again suggests a substantial negative effect of the living wage on property related crimes. In particular, the elasticity estimates range

[14] These cities are Baton Rouge, LA, New Orleans, LA, Provo, UT, St. Louis, MO, Salt Lake City, UT, Albuquerque, NM, Austin, TX, Buffalo, NY, Charlotte, NC, Dallas, TX, Greensboro, NC, Houston, TX, Knoxville, TN, Nashville, TN, Omaha, NE, Pittsburgh, PA, Santa Rosa, CA, South Bend, IN, Syracuse, NY, and Ventura, CA.

[15] The use of failed living campaign cities used as a control group is first suggested by Adams and Neumark (2005a).

from -0.12 for MVT to -0.27 for burglary. Likewise, all three property related crimes are especially affected when businesses assistance ordinances are used.

V. Conclusion

In this paper, we evaluate the unintended consequences of living wage policies on crime. Using a panel of city level annual crime rates from 1990 to 2002, two contributions are made to the existing literature. First, while previous studies have focused on the impact of living wages on the labor market, we are the first to study the impact of living wages on related deviant behaviors. Second, using the panel data set of cities, we are able to explicitly account for the endogeneity of living wage ordinances.

We find robust evidence that living wages can substantially reduce robbery, burglary and MVT but have no impact on homicide. These findings are supported in the descriptive figures, as well as numerous different regression models. Depending on the specification and the crime being examined, our elasticity estimates for the three property related crimes lie between -0.10 and -0.30. Finally, we find that the impact of living wage laws is much more pronounced for business assistance provisions that cover larger fractions of the low-wage workforce.

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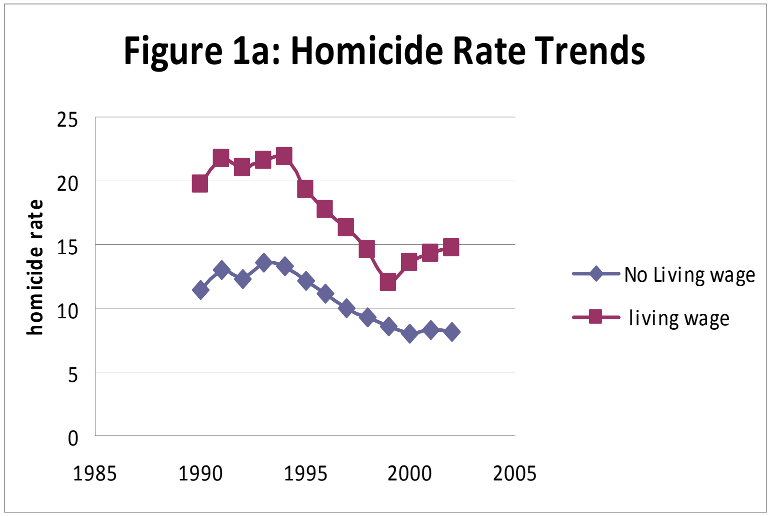
Figure 1a: Homicide Rate Trends
homicide rate
25
20
15
10
5
0
1985
1990
1995
2000
2005
No Living wage
living wage

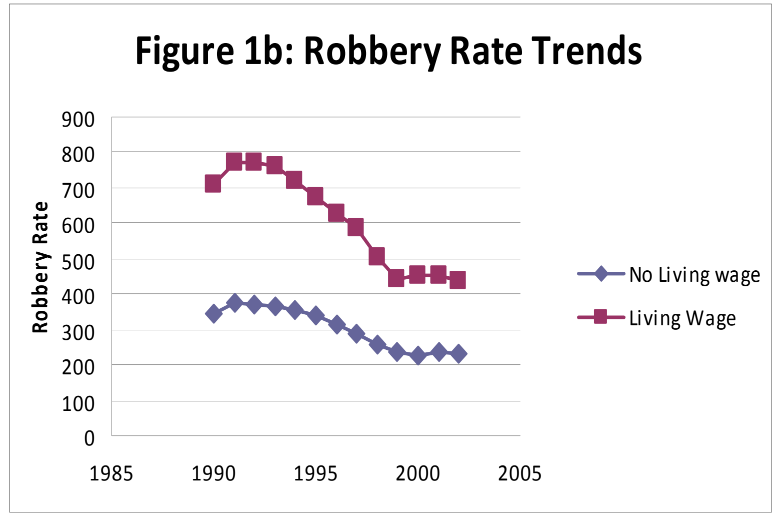
Figure 1b: Robbery Rate Trends
Robbery Rate
900
800
700
600
500
400
300
200
100
0
1985
1990
1995
2000
2005
No Living wage
Living Wage

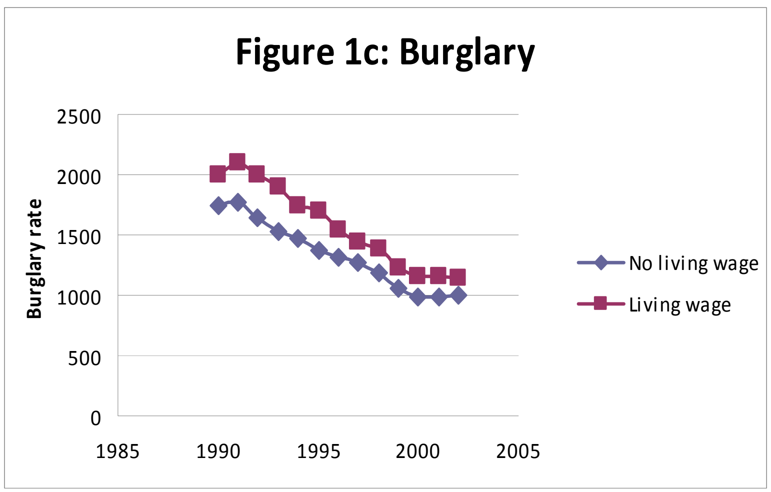
Figure 1c: Burglary
Burglary rate
2500
2000
1500
1000
500
0
1985
1990
1995
2000
2005
No living wage
Living wage

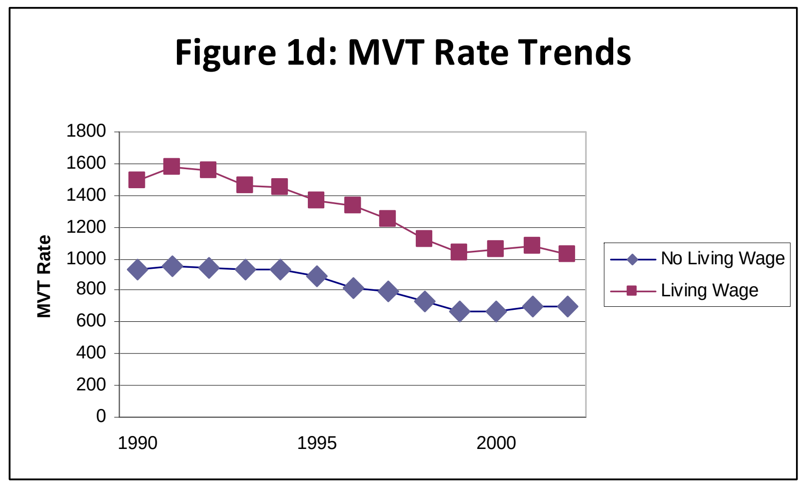
Figure 1d: MVT Rate Trends
MVT Rate
1800
1600
1400
1200
1000
800
600
400
200
0
1990
1995
2000
No Living Wage
Living Wage

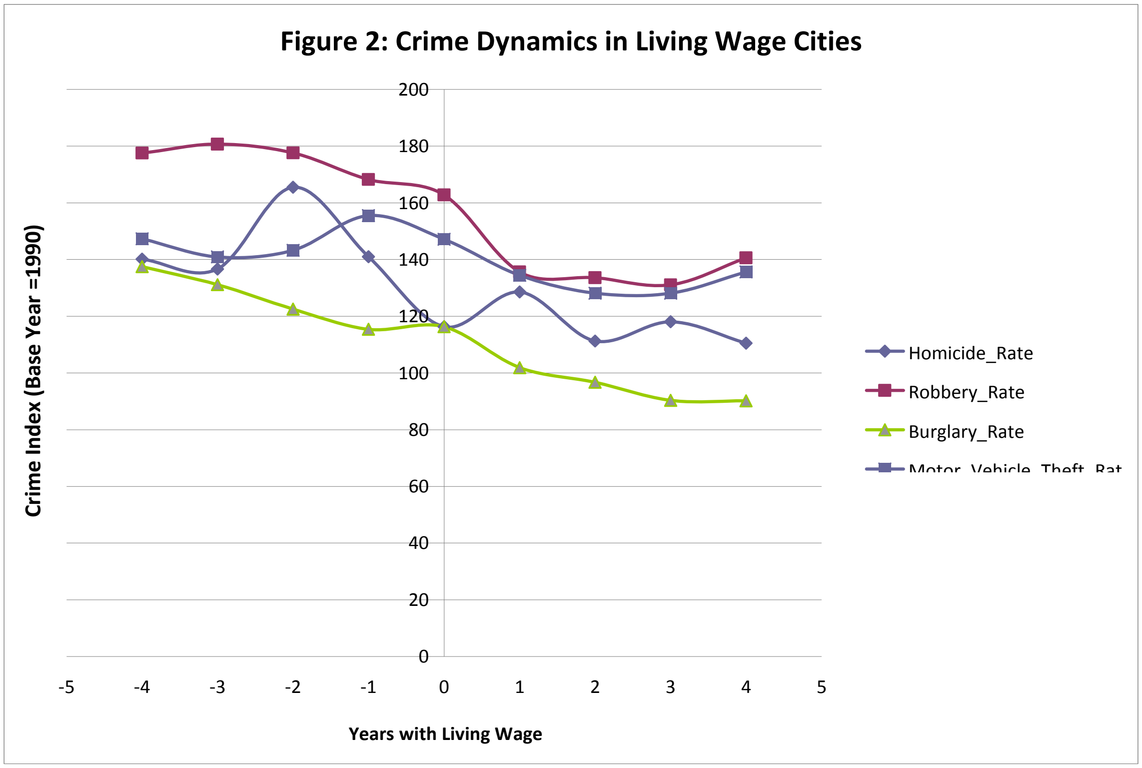
Figure 2: Crime Dynamics in Living Wage Cities
200
180
160
140
120
100
80
60
40
20
0
-5
-4
-3
-2
-1
0
1
2
3
4
5
Crime Index (Base Year =1990)
Years with Living Wage
Homicide_Rate
Robbery_Rate
Burglary_Rate

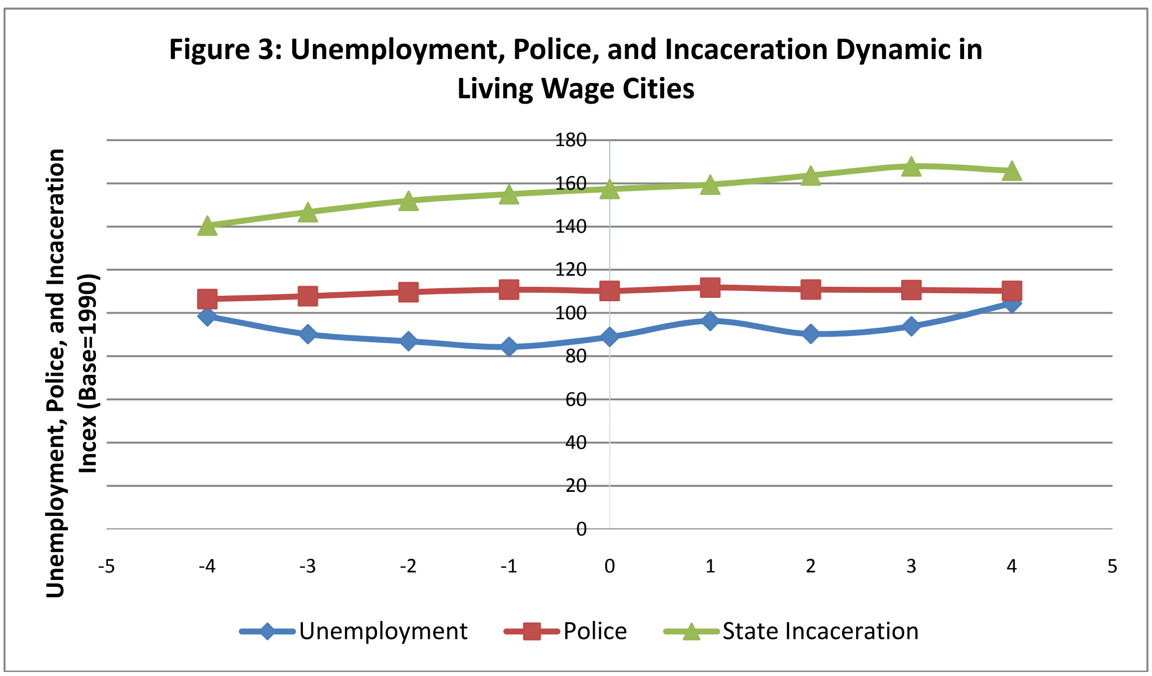
Figure 3: Unemployment, Police, and Incaceration Dynamic in Living Wage Cities
180
160
140
120
100
80
60
40
20
0
-5
-4
-3
-2
-1
0
1
2
3
4
5
Unemployment, Police, and Incaceration Incex (Base=1990)
Unemployment
Police
State Incaceration

Table 1: Cities with Living Wage Laws

City	Year Enacted	2002 Minimum Wage	2002 Living Wage	Coverage[1]
Ann Arbor, MI	2001	5.15	8.70	B,C
Baltimore, MD	1995	5.15	8.20	C
Boston, MA	1998	6.75	10.25	C
Chicago, IL	1998	5.15	9.05	C
Cleveland, OH	2001	5.15	883	B,C,M
Dayton, OH	1998	5.15	7	M
Denver, CO	2000	5.15	8.67	C
Detroit	1998	5.15	9.01	B,C
Durham, NC	1998	5.15	8.45	C,M
Hartford, CT	1999	6.70	9.92	B,C
Jersey City, NJ	1996	5.15	7.5	C
Los Angeles	1997	6.75	8.08	B,C
Madison	1999	5.15	9.01	B,M
Milwaukee	1995	5.15	7.19	C
Minneapolis	1997	5.15	9.01	B
New Haven	1997	6.70	10.82	C
Oakland	1998	6.75	8.69	B,C
Portland	1996	6.50	8.00	C
Rochester	2001	5.15	8.76	B,C,M
San Antonio	1998	5.15	9.27	B
San Francisco	2000	6.75	10.00	C
San Jose	1998	6.75	10.34	B,C,M
Toledo	2000	5.15	9.92	B,C,M
Tucson	1999	5.15	8.57	C

Notes:
[1]B indicates business assistance recipients are covered, C indicates that contractors are covered and M indicates that municipal employees are covered.

Table 2: Descriptive Statistics – Means and Standard Deviations

Outcome variables (Y)	All	Cities that Adopted Living Wage	Cities without Living Wage
Homicide rate per 100,000 population	11.72	17.57	10.87
	(11.83)	(12.20)	(11.53)
Robbery rate per 100,000 population	345.95	609.46	307.39
	(287.13)	(355.23)	(253.86)
Burglary rate per 100,000 population	1377.04	1574.88	1348.09
	(653.60)	(676.71)	(645.24)
Motor Vehicle Theft rate per 100,000 population	886.68	1293.49	827.14
	(565.80)	(632.76)	(529.89)
Living Wage	8.07		
	(1.20)		
Living Wage –Minimum Wage	2.63		
	(1.057)		
Covariates (X)			
Minimum Wage	4.75	4.76	4.75
	(0.68)	(0.69)	(0.67)
Number of Police per 100,000 population	197.06	276.85	185.42
	(74.23)	(91.96)	(63.51)
State Incarceration Rate per 100,000 population	404.24	356.97	411.22
	(138.9)	(118.16)	(140.32)
Population (100,000)	2.76	6.83	2.16
	(3.85)	(8.22)	(2.08)
Percent Female	0.51	0.51	0.51
	(0.01)	(0.01)	(0.01)
Percent African-American	0.17	0.27	0.17
	(0.18)	(0.21)	(0.17)
Percent White	0.66	0.54	0.65
	(0.18)	(0.21)	(0.17)
% Aged 0-14	0.18	0.21	0.18
	(0.04)	(0.04)	(0.05)
% Aged 15-19	0.06	0.07	0.06
	(0.02)	(0.01)	(0.02)
% Aged 20-24	0.07	0.09	0.07
	(0.03)	(0.03)	(0.03)
% Aged 25-34	0.31	0.18	0.32
	(0.15)	(0.05)	(0.15)
% Aged 35-44	0.13	0.15	0.13
	(0.03)	(0.01)	(0.03)
% Aged 45-54	0.09	0.12	0.09
	(0.03)	(0.02)	(0.03)
% Aged 55-64	0.06	0.07	0.06
	(0.02)	(0.01)	(0.02)
N (number of city-year observations)	2428	310	2118

Notes: Minimum wage rate data come from U.S. Department of Labor, the city level number of police per 100,000 from the UCR Law Enforcement Officers Killed or Assaulted data files, the state level year end incarceration rates from Bureau of Justice Statistics, and population data come from the Census Bureau.

Table 3:
The Estimated Effect of Living Wage Laws on the Logarithm of Crime Rates
N = 2428

	Model 1	**Model 2**	**Model 3**	**Model 4**
	Homicide	**Homicide**	**Homicide**	**Homicide**
Minimum Wage (β)	-3.09	-1.47	-0.49	-0.32
	(0.71)	(0.44)	(0.51)	(1.11)
Living Wage (γ)	2.00	0.18	0.09	-0.02
	(0.27)	(0.12)	(0.17)	(0.19)
R^2	0.06	0.62	0.67	0.71
	Robbery	**Robbery**	**Robbery**	**Robbery**
Minimum Wage (β)	-1.08	-1.12	-0.23	-0.18
	(0.42)	(0.13)	(0.12)	(0.17)
Living Wage (γ)	1.32	-0.26	-0.13	-0.17
	(0.24)	(0.07)	(0.08)	(0.08)
R^2	0.11	0.95	0.97	0.98
	Burglary	**Burglary**	**Burglary**	**Burglary**
Minimum Wage (β)	-2.32	-1.02	-0.31	-0.22
	(0.21)	(0.10)	(0.12)	(0.17)
Living Wage (γ)	-0.07	-0.14	-0.23	-0.26
	(0.11)	(0.06)	(0.08)	(0.07)
R^2	0.32	0.88	0.93	0.95
	MVT	**MVT**	**MVT**	**MVT**
Minimum Wage (β)	-0.41	-0.84	0.07	-0.24
	(0.26)	(0.15)	(0.15)	(0.23)
Living Wage (γ)	0.63	-0.25	-0.13	-0.13
	(0.17)	(0.09)	(0.09)	(0.11)
R^2	0.08	0.86	0.93	0.96
Year Fixed Effects	YES	YES	YES	YES
City Fixed Effects	NO	YES	YES	YES
City Time Trend	NO	NO	YES	YES
City Quadratic Time	NO	NO	NO	YES

Standard errors are in parentheses. The standard errors are robust to arbitrary heteroskedasticity.

Table 4
The Estimated Effect of Living Wage Laws on the Logarithm of Crime Rates
N = 2428

	Model 1	Model 2	Model 3	Model 4
	Homicide	**Homicide**	**Homicide**	**Homocide**
Minimum Wage (β)	-0.42	-1.34	-0.37	-0.33
	(0.49)	(0.50)	(0.53)	(1.16)
Living Wage (γ)	-0.33	0.24	0.11	-0.05
	(0.20)	(0.14)	(0.20)	(0.25)
R^2	0.37	0.63	0.67	0.71
	Robbery	**Robbery**	**Robbery**	**Robbery**
Minimum Wage (β)	0.70	-0.71	-0.10	-0.13
	(0.25)	(0.13)	(0.15)	(0.17)
Living Wage (γ)	-0.44	-0.26	-0.15	-0.17
	(0.13)	(0.07)	(0.08)	(0.08)
R^2	0.68	0.95	0.97	0.98
	Burglary	**Burglary**	**Burglary**	**Burglary**
Minimum Wage (β)	-0.72	-0.58	-0.13	-0.18
	(0.17)	(0.10)	(0.11)	(0.17)
Living Wage (γ)	-0.47	-0.22	-0.22	-0.26
	(0.09)	(0.06)	(0.07)	(0.07)
R^2	0.55	0.90	0.94	0.96
	MVT	**MVT**	**MVT**	**MVT**
Minimum Wage (β)	0.49	-0.46	0.08	-0.23
	(0.25)	(0.14)	(0.16)	(0.22)
Living Wage (γ)	-0.39	-0.25	-0.12	-0.11
	(0.14)	(0.07)	(0.09)	(0.11)
R^2	0.47	0.89	0.94	0.96
Year Fixed Effects	YES	YES	YES	YES
City Fixed Effects	NO	YES	YES	YES
City Time Trend	NO	NO	YES	YES
City Quadratic Time	NO	NO	NO	YES

Standard errors are in parentheses. The standard errors are robust to arbitrary heteroskedasticity. The covariates included in the regression are the city population, the fraction female, African American, white, the fraction in different age cohorts, the logarithm of the state incarceration rate per capita, and the logarithm of the number of police per capita.

Table 5

The Estimated Effect of Living Wage Laws on the Logarithm of Crime Rates by Type of Coverage

N = 2402

	Model 4	Model 4	Model 4	Model 4
	Homicide	**Robbery**	**Burglary**	**MVT**
Minimum Wage (β)	-0.33	-0.10	-0.16	-0.18
	(1.16)	(0.16)	(0.16)	(0.22)
Living Wage	-0.06	0.01	-0.13	0.22
	(0.22)	(0.09)	(0.07)	(0.18)
Living Wage x	0.03	-0.32	-0.24	-0.62
Business Coverage	(0.37)	(0.14)	(0.13)	(0.21)
R^2	0.71	0.98	0.96	0.96

Standard errors are in parentheses. The standard errors are robust to arbitrary heteroskedasticity. Model 4 includes year and city fixed effects as well as city specific quadratic time trends. The covariates included in the regression are the city population, the fraction female, African American, white, the fraction in different age cohorts, the logarithm of the state incarceration rate per capita, and the logarithm of the number of police per capita. Municipal and contractor provisions are merged into one grouping, contractors, for this analysis.

Table 6
The Estimated Effect of Living Wage Laws on the Logarithm of Crime Rates by Age Distribution

N = 2402

	Model 4	Model 4	Model 4	Model 4
	Homicide	**Robbery**	**Burglary**	**MVT**
Minimum Wage (β)	-0.33	-0.13	-0.18	-0.24
	(1.16)	(0.17)	(0.16)	(0.22)
Living Wage	-4.32	-1.19	-1.37	-0.93
	(2.43)	(0.37)	(0.29)	(0.41)
Living Wage x % 15-19	64.08	15.32	16.70	12.32
	(37.46)	(5.31)	(4.14)	(5.64)
R^2	0.71	0.97	0.96	0.96
Marginal Effects Evaluated At % 15-19				
% 15-19 @ Mean	-0.43	-0.25	-0.35	-0.18
	(0.29)	(0.09)	(0.07)	(0.12)
% 15-19 @ 0.75 Quartile	0.23	-0.10	-0.18	-0.05
	(0.33)	(0.07)	(0.07	(0.11)
% 15-19 @ 0.25 Quartile	-1.25	-0.45	-0.57	-0.34
	(0.67)	(0.13)	(0.10)	(0.16)

Standard errors are in parentheses. The standard errors are robust to arbitrary heteroskedasticity. Model 4 includes year and city fixed effects as well as city specific quadratic time trends. The covariates included in the regression are the city population, the fraction female, African American, white, the fraction in different age cohorts, the logarithm of the state incarceration rate per capita, and the logarithm of the number of police per capita.

Table 7

The Estimated Effect of Living Wage Laws on the Logarithm of Crime Rates for Cities with Living Wage Campaigns

N = 556

	Model 4	**Model 4**	**Model 4**	**Model 4**
	Homicide	**Robbery**	**Burglary**	**MVT**
Minimum Wage (β)	-0.85	-0.48	-0.64	-0.73
	(0.88)	(0.40)	(0.38)	(0.41)
Living Wage	-0.05	-0.19	-0.27	-0.12
	(0.21)	(0.08)	(0.07)	(0.10)
R^2	0.87	0.98	0.96	0.96
Minimum Wage (β)	-0.87	-0.42	-0.59	-0.62
	(0.89)	(0.36)	(0.37)	(0.42)
Living Wage	-0.13	-0.004	-0.12	0.23
	(0.20)	(0.11)	(0.09)	(0.17)
Living Wage x	0.15	-0.35	-0.29	-0.66
Business Coverage	(0.36)	(0.16)	(0.14)	(0.23)
R^2	0.87	0.98	0.96	0.96

Standard errors are in parentheses. The standard errors are robust to arbitrary heteroskedasticity. Model 4 includes year and city fixed effects as well as city specific quadratic time trends. The covariates included in the regression are the city population, the fraction female, African American, white, the fraction in different age cohorts, the logarithm of the state incarceration rate per capita, and the logarithm of the number of police per capita. Municipal and contractor provisions are merged into one grouping, contractors, for this analysis.

Appendix: Additional regression tables containing covariates estimates

Table A1: **The Estimated Effect of Living Wage Laws on the Logarithm of Crime Rates with Covariates**

VARIABLES	Homicide	Robbery	Burglary	MVT
Minimum Wage*	-0.367	-0.103	-0.125	0.0807
	(0.533)	(0.148)	(0.141)	(0.158)
Living Wage*,†	0.113	-0.148*	-0.217***	-0.115
	(0.195)	(0.0775)	(0.0721)	(0.0912)
Police Per Capita*	0.280	0.264**	0.226***	0.137
	(0.526)	(0.103)	(0.0803)	(0.110)
Incarceration Rate*	-0.910***	-0.575***	-0.651***	-0.635***
	(0.175)	(0.0575)	(0.0496)	(0.0624)
Population*	-0.385	-0.281***	-0.399***	-0.386***
	(0.277)	(0.0369)	(0.0316)	(0.0354)
% Female	-412.6	-338.1	-62.92	-600.1*
	(808.6)	(337.3)	(297.4)	(347.3)
% African-American	122.9*	-7.631	-18.81*	-6.470
	(65.67)	(12.24)	(10.14)	(10.20)
% White	-19.14	-8.650**	-4.410	-5.639
	(18.71)	(4.146)	(3.592)	(4.208)
% Age: 1-14	68.18*	4.685	-2.702	18.98***
	(41.21)	(5.625)	(4.375)	(6.273)
% Age: 15-19	-1.712	-11.39	-20.01	-8.472
	(46.67)	(19.48)	(17.22)	(18.04)
% Age: 20-24	52.59	11.65	1.778	11.40
	(50.79)	(7.820)	(6.980)	(9.104)
% Age: 25-34	38.62	0.312	-2.380	12.78***
	(36.01)	(4.310)	(3.370)	(4.923)
% Age: 35-44	28.27	9.245	-0.568	13.74
	(48.95)	(7.309)	(6.493)	(8.667)
% Age: 45-54	-3.021	-5.843	3.385	0.782
	(30.77)	(5.935)	(5.088)	(6.114)
% Age: 55-64	75.57	1.825	-12.15	44.19***
	(124.7)	(11.97)	(9.297)	(13.10)
Year and City FE	Y	Y	Y	Y
City Time Trend	Y	Y	Y	Y
City Quadratic Time	N	N	N	N
Observations	2,402	2,402	2,402	2,402
R-squared	0.673	0.973	0.937	0.940

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
* natural log of the variable; † log change between the living wage and minimum wage

Table A2: **The Estimated Effect of Living Wage Laws on the Logarithm of Crime Rates with Covariates**

VARIABLES	Homicide	Robbery	Burglary	MVT
Minimum Wage*	-0.327	-0.130	-0.178	-0.235
	(1.162)	(0.174)	(0.168)	(0.221)
Living Wage*,†	-0.0480	-0.168**	-0.256***	-0.108
	(0.246)	(0.0809)	(0.0716)	(0.108)
Police Per Capita*	0.355	0.148	0.147*	0.111
	(0.563)	(0.0994)	(0.0777)	(0.122)
Incarceration Rate*	-0.502**	-0.242***	-0.355***	-0.539***
	(0.241)	(0.0755)	(0.0731)	(0.0879)
Population*	-0.0312	-0.237***	-0.410***	-0.374***
	(0.249)	(0.0248)	(0.0337)	(0.0279)
% Female	0	0	0	0
	(0)	(0)	(0)	(0)
% African-American	-720.6	141.6	-37.84	-49.91
	(1,099)	(94.48)	(72.77)	(81.85)
% White	110.9	35.48	-108.2*	34.00
	(352.3)	(69.81)	(58.53)	(67.38)
% Age: 1-14	-96.26	-44.13	82.43**	40.74
	(241.9)	(37.91)	(32.55)	(39.45)
% Age: 15-19	-504.5	-196.5***	-248.6***	-380.3***
	(386.9)	(70.81)	(58.26)	(80.71)
% Age: 20-24	296.9	72.35	205.6***	257.0***
	(317.0)	(63.36)	(58.15)	(66.82)
% Age: 25-34	-112.6	-50.57	28.09	24.34
	(212.7)	(42.20)	(37.56)	(43.27)
% Age: 35-44	543.7	-48.99	-26.45	56.19
	(350.8)	(70.68)	(65.66)	(77.47)
% Age: 45-54	-443.6	85.44	313.5***	187.3**
	(535.7)	(87.35)	(74.30)	(86.54)
% Age: 55-64	-407.5	-127.0*	-48.41	-66.27
	(638.1)	(70.62)	(60.27)	(77.09)
Year and City FE	Y	Y	Y	Y
City Time Trend	Y	Y	Y	Y
City Quadratic Time	N	N	N	N
Observations	2,402	2,402	2,402	2,402
R-squared	0.713	0.981	0.956	0.960

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
* natural log of the variable; † log change between the living wage and minimum wage