# MEMORANDUM

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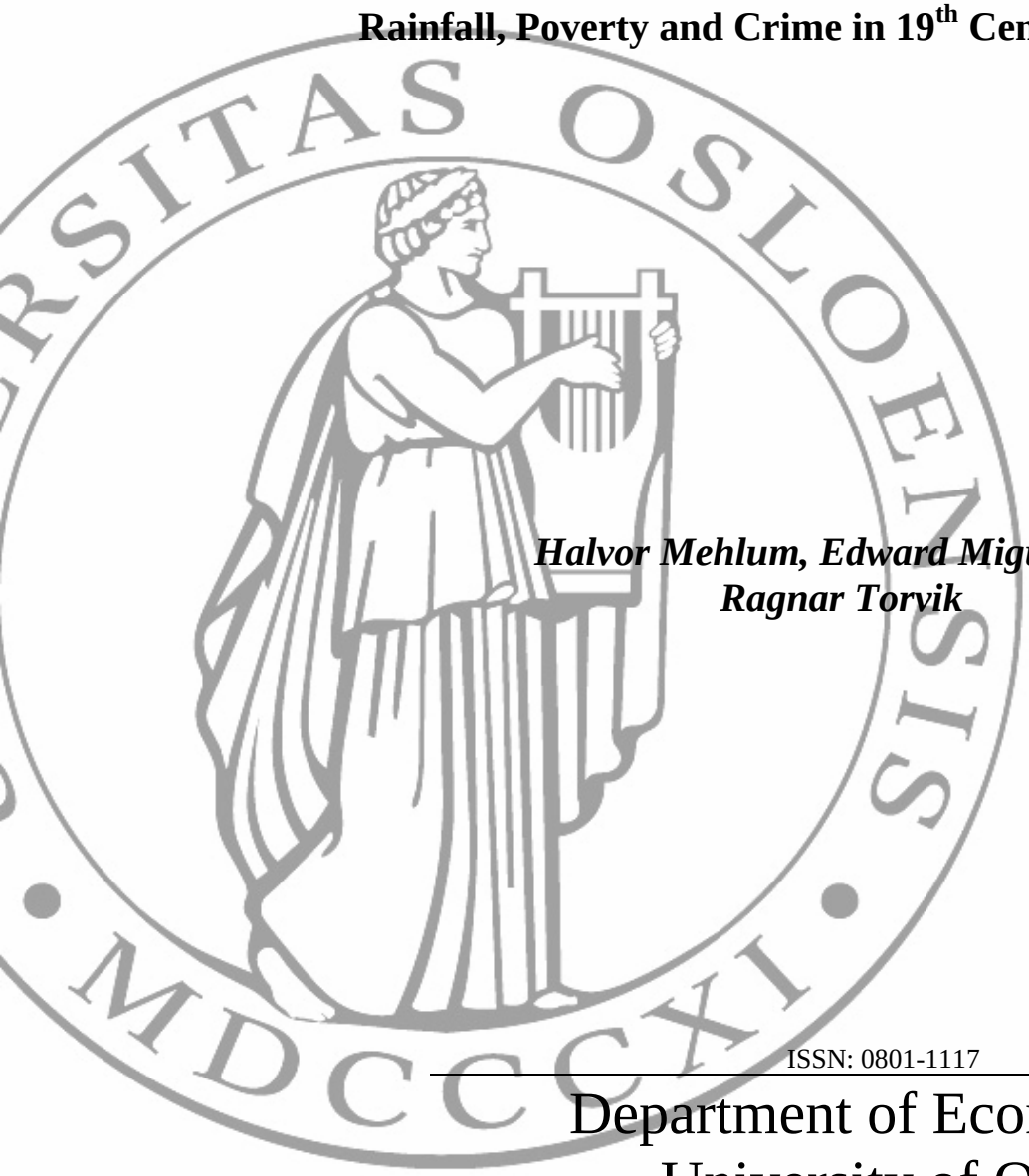
**Rainfall, Poverty and Crime in 19th Century Germany**



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# Rainfall, Poverty and Crime in 19th Century Germany

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**Abstract:** We estimate the impact of poverty on crime in 19th century Bavaria, Germany. Rainfall is used as an instrumental variable for the price of rye to address identification problems found in the existing literature. The rye price was a major determinant of the cost of living and poverty during this period. The rye price has a positive and statistically significant effect on property crime: a one standard deviation increase in the rye price increased property crime by a moderate 8 percent, a result similar to recent findings from the contemporary U.S. This result is robust to another poverty measure (the real wage), and when we restrict attention to lagged rainfall measures as instruments – ruling out some possible violations of the exclusion restriction. OLS estimates are twice as large as instrumental variable estimates. Higher rye prices lead to significantly *less* violent crime, though, and we argue that higher beer prices (caused by higher rye prices) are a likely explanation. We discuss implications for economic theories of crime, and for public policy in less developed countries today.

**JEL Classification:** I33, N93

**Keywords:** Poverty, Crime, Rainfall, Germany

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## 1. Introduction

We investigate the impact of poverty on crime in 19th century Bavaria, Germany. Figure 1 graphically presents the strong correlation between the price of rye – then the staple food of most Bavarians – and property crime during 1835-1861, and Figure 2 shows the link between real wages and crime.

FIGURE 1 ABOUT HERE.

FIGURE 2 ABOUT HERE.

Striking correlations of this sort have been interpreted as the causal effect of poverty on crime, but they are potentially subject to bias due to endogeneity and omitted variables. In other words, does poverty generate crime – or does crime lead to more poverty? Or does some third factor – say, government institutional performance or changes in public policy – affect both simultaneously? To credibly estimate the impact of poverty on crime, a source of exogenous variation in poverty is needed.

This paper's main innovation lies in using rainfall as a source of exogenous variation in rye prices – and by extension the cost of living – in 19th century Bavaria.[1] Using a U.N. Food and Agriculture Organization (FAO) historical rainfall database, described below, we find that rainfall alone explains 28 percent of variation in rye prices. Excessive rainfall sharply reduces rye yields, both by interfering with the sowing season for winter grains, as well as ruining the harvest (Baten 2002).

The analysis uses von Mayr's (1867) unique historical crime series for Bavaria. Using an instrumental variable method, we estimate an elasticity of property crime with respect to the rye price of 0.2 – which implies that a one standard deviation increase in the rye price increased property crime by a moderate 8 percent. The magnitude of this relationship is similar to recent findings on links between unemployment, wages, and property crime in the contemporary United States (Levitt 2004). These results are robust across specifications, and to the use of real wage data from a different historical source (Gømmel 1978) as a proxy for poverty. Ordinary least squares estimates are twice as large as instrumental variable estimates, and thus if interpreted causally would greatly exaggerate the

[1] Miguel (2003) and Miguel, Satyanath, and Sergenti (2003) employ related econometric strategies in contemporary African settings. It is worth noting that in semi-arid African regions, more rainfall is typically associated with higher crop yields and less poverty, unlike in 19th century Bavaria, where the opposite held.

effect of poverty on property crime, highlighting the value of our IV approach. The main limitation of the empirical analysis is the relatively small sample of only 25 years.[2]

A key remaining issue for econometric identification is the instrumental variable exclusion restriction: rainfall should not affect crime other than through its effect on the price of rye. One concern is that rainfall itself may have a direct effect on crime if heavy rains reduce criminals' likelihood of being apprehended by the police (due to flooded roads, for example), or make it more difficult to guard private property. Another possible channel is psychological: rainfall may affect people's moods in such a way that they are more likely to commit crimes. If such channels are present, the IV estimates could be misattributing direct effects of rainfall to poverty. However, these channels are unlikely to be important in this context. In particular, the main results are robust to using only lagged rainfall measures, from the previous two years, as instrumental variables (and not including current rainfall). Lagged rainfall affects previous harvests, which in turn affect current rye prices, but lagged rainfall should not directly affect people's current mood or current police capacity – thus helping us rule out these other channels.

In the second main empirical result, we find a strong negative relationship between poverty and violent crime (Figure 3).

FIGURE 3 ABOUT HERE.

The effect of the rye price on alcohol consumption provides a plausible explanation. Rye prices had a direct impact on the price of beer during this period, and greater alcohol consumption has been linked to increased violent crime in both historical and recent studies (Zehr 1976, Markowitz 2000a, 2000b). In interpreting these findings, we argue that it is not the case that poverty *per se* leads to less violent crime, but rather that in years of scarcity and high rye prices, beer is also expensive, and this is the channel that leads to less violent crime. The sharp contrast between the effect of rye prices on property crime versus violent crime highlights the importance of distinguishing between rational choice theories of crime (such as Becker 1968) versus psychological explanations for criminal

[2] Note that estimated serial correlation in annual rainfall is small (at -0.1) and not statistically significant.

behavior, in which the loss of self-control – here due to alcohol consumption – often play a central role, a point taken up again in the conclusion.

The remainder of the paper is organized as follows. Section 2 provides historical background on 19th century Bavaria, and Section 3 describes the data sources. The main empirical results are presented in Section 4, and the final section concludes.

## 2. Historical Background on 19th Century Bavaria, Germany

Germany was a predominantly agrarian society during the study period of 1835-1861 – lagging far behind the earliest European industrializers, such as Britain and Belgium – and the Kingdom of Bavaria was a laggard even by German standards (Baten and Murray 2000, p. 352). Poverty, malnutrition and poor health were widespread. Sheehan (1989, p. 639) notes that "even in the best of times the poorest families spent most of their income on food; when prices rose they bought less, ate less, and became susceptible to typhus, typhoid, and the other infections that raged throughout these years." More precisely, according to Blackbourn (1997, p. 139), "even in normal times, food accounted for 80 per cent of spending by poor families", a figure comparable to many contemporary less developed countries; for instance, Miguel (2003) finds that 70 percent of expenditures go to food in rural Tanzania. Ten percent of Bavarian household income went to beer purchases alone (Gømmel 1978). This extreme poverty often bred desperation and criminal activity in Bavaria: when food prices rose Sheehan notes that (p. 640) "on the fringes of society, among those without enough work, savings or marketable skills, the choice was often between stealing, begging or starving." Land reforms associated with the end of serfdom further increased rural poverty in the 19th century by stripping away feudalism's limited remaining safety nets (Baten 2003b).

Rye represented the major staple food for the poor in Bavaria, even more than the potato. Data for total rye, potato and wheat production in 1853 from Baten and Murray (2003) corroborate this view: protein intake from bread (derived from rye) was roughly four times that from potatoes and six times that derived from wheat.[3] The connection between rye prices and crime in Bavaria was first

[3] The diffusion of potato in Germany started in the late 18th century, but Baten (2003a, p. 6) notes that "contemporary sources frequently describe potatoes as being extremely unpopular in the southern part of the country." Rye remained the main German staple even into the 20th century: Gerschenkron (1989 [1943], p. 112)

noted by von Mayr (1867), based on the same data we use. Later, Blasius (1976, 1978) argued that the close connection between rye prices and crime also held for Prussia in 1836-1850. Johnson (1995, p. 138) similarly notes that:

> "During the 1840s, early 1850s … food prices were rising quickly, jobs were scarce – and property offences were rife. But from the mid-1850s on, the general trend in the economy was toward lower prices and improved economic outlook for the majority of Germans, and over most of these years there was a decrease in simple theft offences, which were far and away the most common types of economic crimes and the ones most affected by raw economic hardship."

Although most claims by historians rest on anecdotes or simple graphical evidence, several authors also report statistical correlations between food prices and crime. Woytinsky (1929) does so using von Mayr's Bavarian data, Blasius (1978) reports positive correlations between rye bread prices and theft in Prussia, and Zehr (1976) reports strong positive correlations in Germany and France for parts of the 19th century, although he correctly notes that (p. 151) this "in no way proves a cause-and-effect relationship between these variables; it only says that they more-or-less vary together". We are not aware of any existing historical study that uses more sophisticated econometric methods to address the identification problems highlighted in the introduction, as we do in this paper.

## 3. Data and Measurement

Despite being a laggard in terms of industrial development, Bavaria was a leader in legal reform: in 1813, Bavaria became the first German state to adopt a modern penal code (based on the 1810 French code) and this Bavarian code served as a model for the national German penal code of 1872 (Johnson 1995, p. 24). Bavaria also had the most comprehensive crime statistics in Germany prior to 1882 when the standard national crime reporting system was established (p. 117): the mid-19th century Bavarian reports have crime categories familiar to us today (p. 117). von Mayr (1867) contains information on annual property crime (e.g., theft, street robbery, armed robbery, embezzlement) rates per 100,000 inhabitants, on begging and vagrancy arrests, on violent crime (e.g., assault, gross indecency, kidnapping, excessive punishment, and rape), and on murder. These data are reported

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notes that "one of the most seriously considered proposals for the stabilization of the mark in the fall of 1923 was the plan … [that] a "rye-mark" should be created, that is to say, the value of the restored German currency was to be based on the value of rye."

cases from Bavarian police records, which von Mayr rightly argued was a better measure than the number of convicted criminals. The data are widely considered reliable and consistent over time.[4]

There is complete crime data for seven of eight Bavarian districts: Upper and Lower Bavaria, Upper, Middle, and Lower Franconia, Schwabia, and Upper Palatinate; the sum across all districts is referred to as All Bavaria. (The district with missing crime data is Palatinate, which is not contiguous with the rest of Bavaria, and we drop it from the analysis.) Bavaria had a total population of 4.5 million inhabitants in 1850 (Baten 2003a). The descriptive statistics (Table 1, Panel A), show that Bavarian property crime rates during the study period were roughly comparable to current levels in Spain and Greece, and the murder rate was similar to the United States in the 1990s (UN 1999).

TABLE 1 ABOUT HERE.

The rye price data are from von Mayr (1867) as well (Table 1, Panel B). An additional poverty measure is also employed, first, to capture costs of living other than the price of rye, and second to demonstrate the robustness of the results using a different data source. For that purpose, we use real wage data for *Bauhandwerker* (roughly translated as construction workers) in Nuremberg, Middle Franconia from Gømmel (1978). Note that although *Bauhandwerker* were often quite poor, they were not considered the poorest of the poor in 19$^{th}$ century Bavaria. Finally, there is annual rainfall in meters for all of Germany from the FAO (2001) historical database, which is derived from a dozen rainfall gauges throughout Germany – though there are none within Bavaria during the study period (Table 1, Panel C).[5] Thus while there is annual crime data at the district-level, due to data limitations there are just single annual measures of the rye price and of rainfall.

## 4. Main Empirical Results

### 4.1 First-Stage and Reduced-Form Specifications

[4] Zehr (1976, p. 42) argues that among French and German regions during this period "the best [crime] series available is the Bavarian". von Mayr had a distinguished career, producing a monumental work called *Statistics and Social Sciences*, which includes the volume *Moral Statistics with an element of Criminal Statistics*.

[5] Climate data derived from tree ring thickness (Briffa, Jones and Schweingruber 1988) did not strongly predict rye prices (regressions not shown). One reason may be that greater tree ring thickness reflects both higher rainfall and higher temperatures, and that, while excessive rainfall damages the rye harvest, warm weather typically works in the opposite direction.

Rainfall from the current year, and from the previous two years, are included as instrumental variables predicting the rye price. Previous harvests may influence the current rye price by affecting the supply of stored grain, as well as the amount available for future sowing. All three coefficient estimates on the rainfall terms are positive (Table 2, regression 1) and we cannot reject the hypothesis that they are equal at traditional confidence levels (not shown). One year lagged rainfall has the largest positive effect on prices (coefficient estimate 1.95, standard error 0.82) and is significant at the 95 percent confidence. Forward lags of rainfall – rainfall from future years – are not statistically significant as explanatory variables (regressions not shown), an important specification check. Rainfall alone explains 28 percent of rye price variation (regression 1). Adverse rainfall realizations hurt urban workers by generating higher food prices, as well as many peasants, by reducing crop yields. The positive first-stage relationship between lagged rainfall and the current rye price is presented graphically in Figure 4.

TABLE 2 ABOUT HERE.

FIGURE 4 ABOUT HERE.

Time controls are included to capture secular trends and policy changes made in response to the food crisis of the late 1840s and the failed German revolution of 1848. As expected, violent crime shows a sharp increase in 1848, the year of the failed revolution (Figure 3). Blackbourn (1997, p.138) notes that "revolution in Germany was a result of several superimposed crises. At the economic level, an old style crisis of harvest failure in 1845-7 … had important and cumulative effects throughout society. … Crop failure also doubled the price of basic foodstuffs like rye and potatoes, causing widespread distress among those living on the edge of subsistence". The FAO rainfall data corroborate this, indicating that there were consecutive years of heavy rain during 1843-1846, after which climatic conditions largely returned to normal.[6] The food crisis "eroded trust in 'complacent' governments, placing a question mark against their competence and their very legitimacy" (Blackbourn 1997, p. 140), and forcing them to respond aggressively. German states did so by a

[6] While historians have claimed that the 1845-47 food crisis generated popular support for the 1848 revolution, it is interesting to note that the return to normal rainfall in 1847 and 1848 might have also dampened enthusiasm for the revolution at a critical moment, leading to its ultimate failure.

variety of means after 1848 – including increased grain purchases and distribution, price controls, and limits on grain exports – and market intervention in Bavaria was particularly active.

To account for the effects of these policy shifts, an indicator variable for the post-1847 period, linear and quadratic time trend terms, and interactions between the post-1847 indicator and the time trend terms are included as controls; these time controls, together with the rainfall measures, capture 83 percent of variation in rye prices (Table 2, regression 2). Note the drop in rye prices after 1847, which in part reflects the government intervention described above. One-year lagged rainfall again has the strongest effect on current rye prices, and the three rainfall terms are jointly highly significant (F-statistic = 7.9). Not including current rainfall as an explanatory variable leaves the point estimates on the two lagged rainfall terms basically unchanged, with one-year lagged rainfall still significant at 99 percent confidence and two-year lagged rainfall at 95 percent confidence (regression not shown).

Using panel data from the seven Bavarian districts – in a framework that includes district fixed effects, and clusters disturbance terms by year – yields nearly identical point estimates and slightly smaller standard errors (regression 3). Rye prices are nearly identical across the seven Bavarian districts in the sample – for which there is a single rye price series – and the eighth district, Palatinate, for which there is a separate price series in von Mayr (1867) (results not shown). This finding implies that, even if rainfall data disaggregated by Bavarian district were to exist (and we have been unable to locate it), this data would not substantially improve the precision of IV estimates since there is minimal variation in the endogenous variable – the rye price – across districts in a given year.

Analogous regressions using the real wage of *Bauhandwerker* as the dependent variable broadly confirm the main result – that excessive rainfall reduces real income – although, perhaps not surprisingly, the first-stage relationship between rainfall and wages is somewhat weaker than its impact on rye prices (the F-statistic in Table 2, regression 4 is 1.8). This may understate the impact of rainfall on the real income of Bavarians even poorer than the *Bauhandwerker*, since rye purchases constituted a larger share of their income.

The link between rainfall and rye prices could also depend on transport costs and grain trade restrictions. The railroad spread rapidly throughout Germany during the 1840s and 1850s (Carr

1991)[7], and this development facilitated inter-regional and even international trade in grain, thus potentially weakening the link from German rainfall to rye prices over time. Looser trade regulations could also weaken this link. The German Customs Union (*Deutscher Zollverein*) came into being in January 1834, embracing eighteen states (including Bavaria) with a total population of 23 million, and during the mid-19th century restrictions across German states were gradually relaxed. By 1865 – just after the study period – "Germany had completely free trade in grains" (Gerschenkron 1943, p. 42).

To investigate if improved infrastructure and the expanding Customs Union weakened the link between rainfall and rye prices, interactions between rainfall and the time controls were included in the main specification (analogous to Table 2, regression 2), but there are no statistically significant effects on any of the interaction terms (regression not shown). One plausible explanation is that the climatic variables and rye harvests are quite highly correlated throughout Germany, limiting the scope for inter-state trade as a moderating force in grain prices in the absence of international trade.

In a reduced-form specification estimating the impact of rainfall on property crime, lagged rainfall is associated with higher crime at over 90 percent confidence in the All Bavaria specification including time controls (Table 2, regression 5), and results are robust to other specifications (not shown) – the first hint that poverty increases property crime, the focus of the next sub-section. This relationship is presented graphically is Figure 5.

FIGURE 5 ABOUT HERE.

The analogous reduced-form coefficient estimates on one-year lagged rainfall are -0.66 (standard error 0.28, statistically significant at 95 percent confidence – regression 6) when the log of violent crimes is the dependent variable, and -0.55 (standard error 0.32) for the log of murders (regression not shown), evidence that higher rainfall reduces violent crime, the focus of sub-section 4.3.

### 4.2 Rye Prices and Property Crime

The estimated elasticity of the rye price on property crime is 0.29, statistically significant at 99 percent confidence, in the OLS specification without time controls (Table 3, regression 1) and 0.41

[7] See Ritschl (2003) for an account of this chapter in German economic history.

when time controls are included (regression 2).[8] An OLS specification using crime data for the seven Bavarian districts in a panel framework yields similar point estimates (regression not shown, estimate 0.47, standard error 0.08).

TABLE 3 ABOUT HERE

An instrumental variable two-stage least-squares (IV-2SLS) approach is employed to address bias caused by endogeneity and omitted variable bias, where current rainfall, one-year lagged rainfall, and two-year lagged rainfall are the instrumental variables for the rye price. In a specification with time controls, this results in an estimated elasticity of 0.20 (Table 3, regression 3), which is significantly different than zero at 95 percent confidence, and using data for Bavarian districts in the panel framework yields nearly identical results (regression 4). This panel specification uses a considerably larger sample size (179 district-year observations), partially addressing sample size concerns. These estimates imply that a one standard deviation increase in the rye price increases property crime by 8 percent. Note that this is only half the magnitude of the analogous OLS estimate (0.41) – and the difference between the OLS and IV estimates is significantly different at over 90 percent confidence – suggesting that bias in the OLS regression is large and positive, leading to potentially misleading interpretations of simple correlations between rye prices and crime.[9]

The historical evidence provides one possible explanation for this significant divergence between OLS and instrumental variable estimates. Blackbourn (1997, p. 145) notes that social protest "triggered by particular local circumstances" often resulted in property crime: "Peasants, rural labourers, journeymen and workers demanded redress of their grievances and engaged in direct action", including "attacking grain stores" , in some cases. In other words, property crime sometimes directly affected the price of rye, when it was associated with the destruction of grain stores, leading

[8] When time controls are included, the Durbin-Watson statistic increases from 1.3 to 2.1, thus nearly eliminating all serial correlation in the residuals, and a similar result holds in all subsequent regressions. There is no significant autocorrelation in the residuals in a number of other regressions where lagged price and crime measures are also included as explanatory variables (not shown).

[9] Standard errors on the main regression coefficient estimate (on the rye price term) increase in IV specifications, but these increases are often relatively small – at least in part due to the strength of the first stage relationship presented in Table 2. The increase in standard errors is greater when the real wage is the endogenous variable, consistent with the weaker first stage for wages.

to endogeneity in simple OLS specifications attempting to estimate the impact of rye prices on crime. This is likely to account for at least part of the difference between the OLS and IV estimates.

To address potential violations of the exclusion restriction (discussed in Section 1 above), rainfall from the current year is not included as an instrumental variable in the first stage, and instead only the lagged rainfall measures are used. The resulting point estimate on the rye price increases slightly, to 0.26, in the All Bavaria specification and remains highly statistically significant (Appendix Table A1, regression 2), indicating that the IV-2SLS estimates are not driven by the direct effect of current rainfall on property crime.

The effect of real wages for *Bauhandwerker* (construction workers) on property crime is somewhat stronger than the rye price results: the IV-2SLS elasticity of -1.00 (Table 3, regression 7) implies that a one standard deviation decrease in the real wage increases property crime by 15 percent, and once again this result is highly statistically significant and robust to alternative specifications. The similarity between OLS and IV estimates when the real wage is the endogenous variable may be due in part to the fact that rainfall is a relatively weak instrument for wages, and this is likely to bias instrumental variable results toward the OLS estimate (Bound, Jaeger and Baker 1995).

The results with arrests for begging and vagrancy further suggest that rye prices were a major determinant of poverty during this period. In an IV-2SLS specification for All Bavaria including time controls, the elasticity of rye prices with begging and vagrancy is 0.60, again highly statistically significant (Appendix Table A2, regression 1). This implies that a one standard deviation increase in the rye price increases begging and vagrancy arrests by a very large 22 percent.

TABLE 4 ABOUT HERE

Some economic historians have claimed that the link between rye prices and crime in Germany broke down during the course of the 19$^{th}$ century due to rising incomes, nascent industrialization, and the diffusion of the potato – which is less sensitive to climatic fluctuations than rye; if extreme poverty fell, and if crime was to a large extent driven by poverty, one might expect the impact of the rye price on crime to decrease over time. For instance, Zehr (1976, p. 138) claims that "the relationship between basic subsistence costs and both violent and property crime loosened as the century progressed due, seemingly, to rising standards of living". However, this claim remains

contested, with other authors, including Gømmel (1978), finding that real wage levels were practically unchanged during our study period (although year-to-year fluctuations were often large). To test this claim, we include the interaction of the rye price and time trend terms – and instrument these with interactions between rainfall levels and the time trend terms – but do not find support for the hypothesis that the effect of the rye price on property crime weakened during the period: none of the coefficient estimates on the interaction terms are statistically significant (results not shown).

**4.3 Rye Prices and Violent Crime**

Violent crime displays a strong negative association with the price of rye. In an OLS specification, the elasticity of violent crime (excluding murders) is -0.48, significant at 99 percent confidence (Table 4, regression 1). The IV-2SLS specification yields a somewhat smaller elasticity of -0.42 (regression 2), which is also significant at 99 percent confidence, and utilizing the panel of districts yields a similar estimate (regression 3). The similarity of OLS and IV estimates may result from the absence of an endogeneity channel for violent crime comparable to the one described above for property crime (running from destroyed grain stores to higher rye prices).

The violent crime results are robust to using only lagged rainfall as instruments for the rye price (regressions not shown), and are similar, though somewhat weaker, when the real wage is the endogenous variable (not shown). Murder is also strongly negatively associated with the rye price in the IV-2SLS specification, with a point estimate similar to other violent crimes (elasticity -0.31, significant at 95 percent confidence – regression 5). The estimated IV-2SLS elasticity for rape – a crime not usually thought to be directly linked to economic motives, unlike the property crimes and perhaps some of the violent crimes (e.g., kidnapping) – is similar, at -0.3 (regression not shown).

The finding that violent crime is consistently negatively associated with rye prices is puzzling at first glance, given the strong positive impact of the rye price on property crime (Table 3). After all, violent crime is normally though to increase with poverty, as emphasized in Bonger's (1916) classic contribution on criminality and economic conditions as well as in more recent contributions (see Fajnzylber, Lederman and Loayza 2002, and Miguel 2003). However, several recent studies on crime and economic conditions in the contemporary United States find a similar pattern, namely, a strong

positive correlation between local economic distress – typically proxied by unemployment and low wages – and property crime, but a much weaker, and sometimes even negative, relationship with violent crime (Levitt 2004, Gould, Weinberg and Mustard 2002, Raphael and Winter-Ebmer 2001).

The most compelling explanation for this result in 19th century Bavaria is that higher rye prices led to higher beer prices, and that the resulting drop in alcohol consumption in turn reduced violent crime.[10] Using Nuremberg beer price data from Gømmel (1978), the correlation coefficient between the logs of rye and beer prices was 0.76 during the study period, and this is highly statistically significant. If the contemporaneous beer price is included as the endogenous variable, instead of the rye price, we find that the beer price also leads to significantly less violent crime (regression not shown); however, due to the fact that rainfall is the only source of exogenous variation, and that rye and beer prices are so highly correlated, we are unable to discriminate between the effect of beer prices versus the effect of rye prices – and, by extension, real wages – on violent crime. The impact of rye prices on violent crime is best understood as working through these two offsetting channels, and the results suggest that the negative effect of higher beer prices on violent crime far outweighed any positive impact of lower real wages.

Several other authors have found a strong correlation between alcohol consumption and violence. Markowitz (2000a, p. 1) notes that "the positive association between alcoholic beverage consumption and violence is well documented, as is the negative relationship between the quantity of alcohol consumed and its price", and, using U.S. National Crime Victimization Survey data, she finds that higher beer taxes across U.S. states lead to a lower incidence of assault during the 1990s (Markowitz 2000b). Similar violent crime patterns were found in France for the 19th century, where according to Zehr (1976, p. 98) "the correspondence between peaks and troughs in wine consumption and in assault before 1870 was excellent – almost perfect."

Zehr (1976) speculates that this was also the case for Germany, but notes that before 1870 (p. 102) "the lack of good alcohol consumption indexes for Prussia and Bavaria does not allow this

[10] Levitt (2004) makes a related point: "To the extent that activities that are associated with increased levels of either offending or victimization are normal goods – like alcohol consumption, frequenting night clubs, and owning a car – the link between economic activity and crime is theoretically ambiguous."

possibility to be tested." As a substitute for alcohol consumption data, we have attempted to obtain historical production records from present-day Bavarian brewers (such as Spaten-Löwenbrau, and others). However, large-scale industrial beer manufacturing began only in the 1850's in Bavaria with the adoption of more sophisticated production processes, hence existing production records do not cover most of the study period. It is also worth mentioning that we unsuccessfully searched for 19th century Bavarian data on alcohol-related crimes – for instance, public drunkenness, and disturbing the peace – in an effort to provide more direct evidence on the alcohol channel. As a result of these historical data limitations, the hypothesized alcohol consumption channel linking high rye prices to violent crime admittedly remains somewhat speculative.

Finally, there is a weak association between the rye price and total reported crime, including property crime, violent crime, and murder, as well as vandalism and rare violations such as perjury: the coefficient estimate is 0.08 (standard error 0.08) in the All Bavaria IV-2SLS specification with time controls (Appendix Table A2, regression 5). This finding highlights the importance of looking beyond aggregate crime measures in empirical studies, as these may obscure interesting underlying patterns across different crime categories.

## 5. Conclusion

We estimate the causal effect of high rye prices on crime in 19th century Bavaria, Germany using rainfall levels as instrumental variables for the rye price, and find that the effect of poverty on property crime is large and significant. Yet higher rye prices led to a sharp drop in violent crime, which we argue is due to a rise in the price of beer and drop in alcohol consumption.

The study lends support to the notion that economic conditions affect crime. Becker (1968) developed these ideas theoretically, and our property crime findings are probably best understood within his rational-choice framework: higher rye prices decrease real income from legal labor market activities, and this makes theft relatively more attractive for the poor.

However, while property crimes in 19th century Bavaria appear to be "crimes of poverty", violent "crimes of passion" are less easily understood in terms of economic conditions and Beckerian rational choice calculations. The link from real income and alcohol prices to alcohol consumption,

and from alcohol consumption to violence, could of course reflect a rational choice calculus: if people temporarily "lose control" when they drink, they might simply take this loss of control into account when deciding whether to drink in the first place. A more realistic (in our view) understanding of alcohol use and violence, however, is likely to be found in recent Psychology and Economics research. Risky behaviors like drinking may result in part because people systematically fail to consider the future consequences of their current actions – especially if there are immediate utility benefits to the activity, as is the case for drinking alcohol (see O'Donoghue and Rabin 2001). The violent crime findings are also consistent with "projection bias" (Loewenstein, O'Donoghue, Rabin 2003), the notion that people in a given "state" – for instance, when they are sober – systematically make faulty predictions about their behavior in another state (say, when drunk), and this may lead them to drink excessively and commit too many violent crimes.

The results also have implications for policymakers in present-day less developed countries, many of which are at income levels similar to 19th century Bavaria. The elimination of agricultural price controls and subsidies may yield the efficiency gains traditionally emphasized by economists, but if these reforms lead to higher and more variable food prices, and exacerbate poverty among workers, they may trigger outcomes not typically considered by economists – including crime waves and political unrest (Bates 1981).

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## 7. Tables and Figures

**Figure 1:** Rye prices and property crime in Bavaria


**Figure 2:** Real wage and property crime in Bavaria

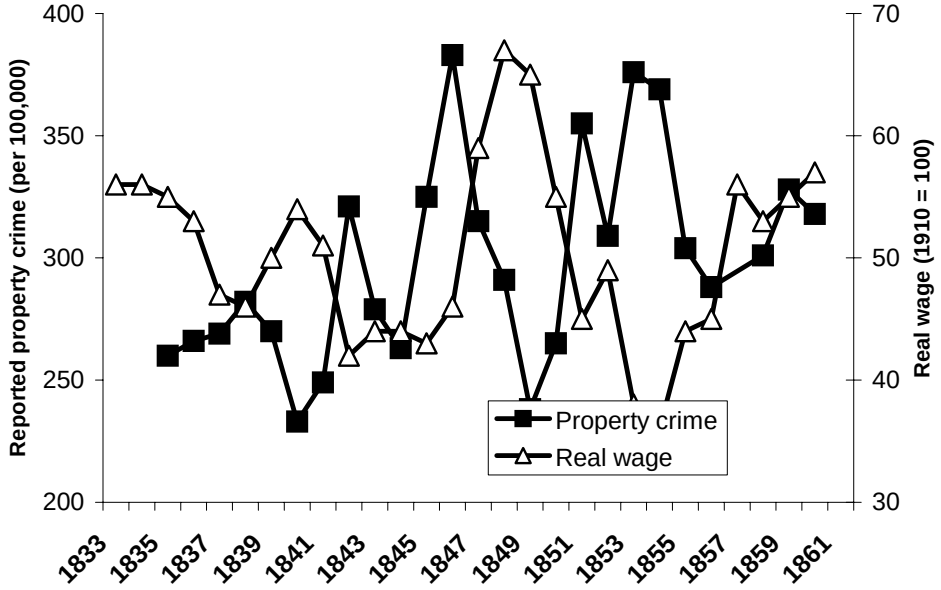

**Figure 3:** Rye prices and violent crime in Bavaria

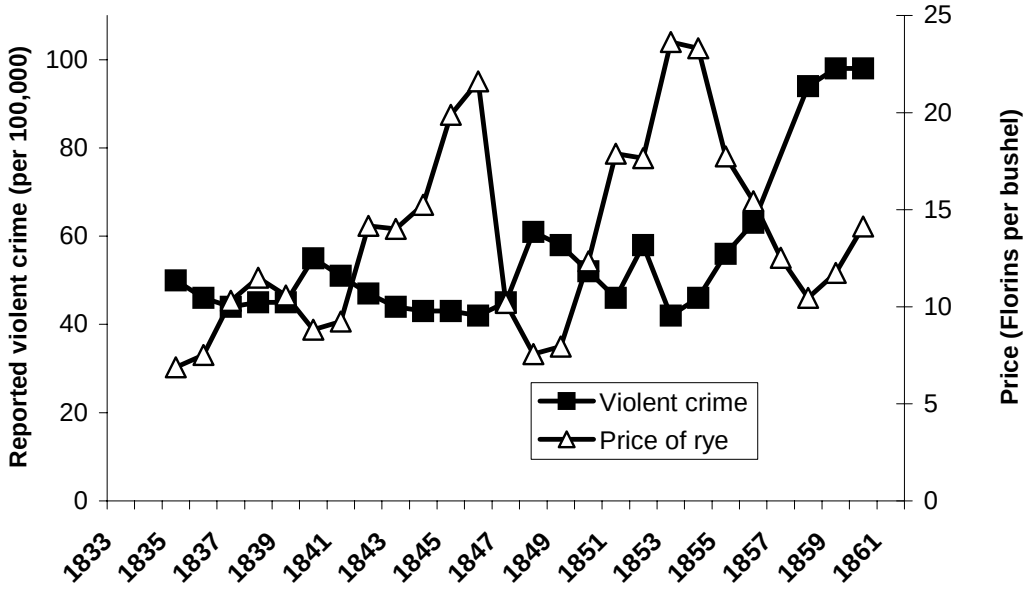


**Figure 4:** Rye prices and lagged rainfall in Bavaria

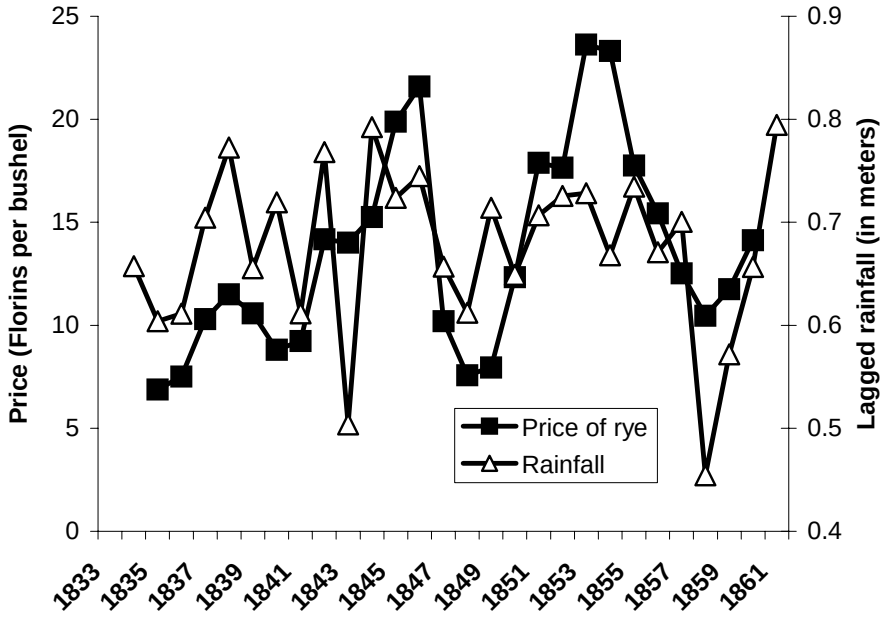

**Figure 5: Property crime and lagged rainfall in Bavaria**

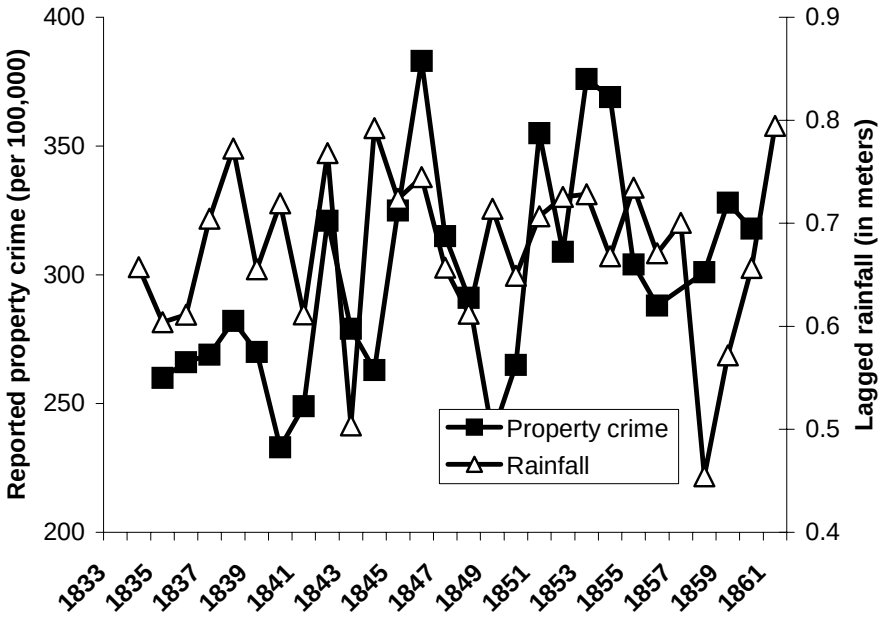

Table 1: Descriptive Statistics

| | All Bavaria | | | Bavarian States | | |
|---|---|---|---|---|---|---|
| | Mean | std dev. | Obs. | Mean | std dev. | Obs. |
| **Panel A: Crime measures** | | | | | | |
| Property crime (annual, per 100,000 population) | 298 | 41.8 | 25 | 290 | 78.8 | 179 |
| Ln (Property crime) | 5.69 | 0.14 | 25 | 5.64 | 0.26 | 179 |
| Violent crime (annual, per 100,000 population) | 54.9 | 16.9 | 25 | 54.4 | 22.6 | 179 |
| Ln (Violent crime) | 3.97 | 0.26 | 25 | 3.93 | 0.36 | 179 |
| Murders (annual, per 100,000 population) | 8.2 | 1.2 | 25 | 8.0 | 2.4 | 179 |
| Ln (Murders) | 2.09 | 0.13 | 25 | 2.04 | 0.28 | 179 |
| Total crime (annual, per 100,000 population) | 418 | 61.3 | 25 | 408 | 92.8 | 179 |
| Ln (Total crime) | 6.02 | 0.14 | 25 | 5.99 | 0.22 | 179 |
| Begging, vagrancy arrests (annual per 100,000 pop.) | 1934 | 528 | 25 | 1744 | 676 | 179 |
| Ln (Begging, vagrancy arrests) | 7.54 | 0.25 | 25 | 7.37 | 0.48 | 179 |
| **Panel B: Economic measures** | | | | | | |
| Rye price (Florins per bushel) | 13.6 | 5.0 | 25 | 13.6 | 4.9 | 179 |
| Ln (Rye price) | 2.55 | 0.37 | 25 | 2.54 | 0.36 | 179 |
| Real wage (1910 = 100) | 49.7 | 7.7 | 25 | 49.9 | 7.5 | 179 |
| Ln (Real wage) | 3.90 | 0.15 | 25 | 3.90 | 0.15 | 179 |
| **Panel C: Rainfall measures** | | | | | | |
| Rainfall (m), year t | 0.69 | 0.070 | 25 | 0.68 | 0.076 | 179 |
| Rainfall (m), year t-1 | 0.67 | 0.082 | 25 | 0.67 | 0.080 | 179 |
| Rainfall (m), year t-2 | 0.67 | 0.082 | 25 | 0.67 | 0.080 | 179 |

Table 1 Notes:
All crime and rye price data are from von Mayr (1867). The data are from seven of eight Bavarian districts: Upper and Lower Bavaria, Upper, Middle and Lower Franconia, Schwabia, and Upper Palatinate. The sum of these is referred to as All Bavaria. The eight district is Palatinate (which is not geographically contiguous with the rest of Bavaria, and has missing data). For price and crime variables, the year runs from 1 November (of the previous year) to 31 October. For the rainfall and wage variables, the year runs from 1 January to 31 December.

*Property crime*: Reported property crime per 100,000 inhabitants per year. Property crime includes petty larceny (e.g., theft of wood, charcoal, turf), grand larceny, street robbery, armed robbery, embezzlement, and other property crimes.

*Violent Crime*: Reported violent crime per 100,000 inhabitants per year. Violent crime includes assault, gross indecency, kidnapping, excessive punishment, and rape. (It does not include murder or robbery).

*Murder*: Reported murders per 100,000 inhabitants per year.

*Total crime*: Includes all property crimes, violent crimes, and murders, as well as vandalism, perjury and various other minor crimes. This measure does not include arrests of beggars and vagrants.

*Begging and Vagrancy:* Arrests of beggars and vagrants per 100,000 inhabitants per year.

*Price of Rye.* Measured in Bavarian Florin per Bavarian bushel.

The real wage data are from Gømmel (1978). This index is 100 in 1910. It is calculated using the officially regulated wage for builder craftsmen, corrected for a cost of living index.

The rainfall data are meters per year for Germany, FAO (2001).

Table 2: First-stage and Reduced-form Results

| | Dependent variable: | | | | | |
|---|---|---|---|---|---|---|
| | Ln (Rye price) | | | Ln (Real wage) | Ln (Property crime) | Ln (Violent crime) |
| | (1) OLS | (2) OLS | (3) OLS | (4) OLS | (5) OLS | (6) OLS |
| *Rainfall variables* | | | | | | |
| **Rainfall, year t** | 1.04 | 0.61 | 0.83** | -0.05 | -0.16 | -0.36 |
| | (0.83) | (0.51) | (0.40) | (0.29) | (0.33) | (0.27) |
| **Rainfall, year t-1** | 1.95** | 1.71*** | 1.66*** | -0.41* | 0.38* | -0.66** |
| | (0.82) | (0.47) | (0.34) | (0.22) | (0.20) | (0.28) |
| **Rainfall, year t-2** | 1.20 | 1.08** | 1.02** | -0.33 | 0.42 | -0.50 |
| | (0.82) | (0.49) | (0.40) | (0.25) | (0.36) | (0.30) |
| *Time controls* | | | | | | |
| Time trend | | -0.002 | -0.002 | -0.022 | -0.0590* | 0.039 |
| | | (0.050) | (0.043) | (0.030) | (0.033) | (0.028) |
| (Time trend)$^2$ | | 0.0056 | 0.0056 | -0.0007 | 0.0059** | -0.0031 |
| | | (0.0035) | (0.0030) | (0.0023) | (0.0024) | (0.0020) |
| Post-1847 | | -4.20** | -4.15*** | 3.21*** | -0.34 | 1.94 |
| | | (1.61) | (1.39) | (1.01) | (0.73) | (1.26) |
| Time trend*Post-1847 | | 0.46** | 0.45*** | -0.31** | 0.085 | -0.26* |
| | | (0.17) | (0.14) | (0.11) | (0.084) | (0.13) |
| (Time trend)$^2$*Post-1847 | | -0.016*** | -0.016*** | 0.0073 | -0.0062* | 0.0098** |
| | | (0.0051) | (0.0044) | (0.0034) | (0.0031) | (0.0037) |
| $R^2$ | 0.28 | 0.83 | 0.83 | 0.71 | 0.51 | 0.87 |
| Root MSE | 0.33 | 0.18 | 0.15 | 0.10 | 0.12 | 0.11 |
| Observations | 25 | 25 | 179 | 25 | 25 | 25 |
| Mean of dependent variable | 2.54 | 2.54 | 2.54 | 3.9 | 5.69 | 3.97 |
| F-stat., $H_0$: coefficient estimates on all rainfall measures = 0 | 2.4 | 7.9 | 14.1 | 1.8 | 1.3 | 4.3 |

Table 2 Notes: Robust standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. Regressions (1)-(4) are First stage results, and regressions (5)-(6) are the Reduced-form. The hypothesis of no first-order autocorrelation cannot be rejected (using the Durbin-Watson test statistic) in the specifications that include time controls. In the specification with district-level data (regression 3), disturbance terms are allowed to be correlated across states in a given year, and district fixed effects are included.

Table 3: Poverty and Property Crimes

| | Dependent variable: Ln(Property crime) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) OLS | (2) OLS | (3) IV-2SLS | (4) IV-2SLS | (5) OLS | (6) OLS | (7) IV-2SLS | (8) IV-2SLS |
| **Ln (Rye price)** | 0.29*** | 0.41*** | 0.20** | 0.22** | | | | |
| | (0.05) | (0.08) | (0.09) | (0.09) | | | | |
| **Ln (Real wage)** | | | | | -0.46*** | -0.88** | -1.00** | -0.93** |
| | | | | | (0.15) | (0.14) | (0.47) | (0.36) |
| Time controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
| District fixed effects | No | No | No | Yes | No | No | No | Yes |
| $R^2$ | 0.60 | 0.78 | - | - | 0.26 | 0.76 | - | - |
| Root MSE | 0.09 | 0.08 | 0.09 | 0.15 | 0.12 | 0.08 | 0.08 | 0.14 |
| Observations | 25 | 25 | 25 | 179 | 25 | 25 | 25 | 179 |
| Mean of dependent variable | 5.69 | 5.69 | 5.69 | 5.64 | 5.69 | 5.69 | 5.69 | 5.64 |

Table 3 Notes: Robust standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The hypothesis of no first-order autocorrelation cannot be rejected (using the Durbin-Watson test statistic) in the specifications that include time controls. In both specifications with district-level data (regression 4, 8), disturbance terms are allowed to be correlated across states in a given year. The instrumental variables are rainfall in the current year, in the lagged year (t-1), and two years earlier (t-2).

Table 4: Poverty and Violent Crimes and Murder

| | Dependent variable: | | | | | |
|---|---|---|---|---|---|---|
| | Ln(Violent crime) | | | Ln(Murder) | | |
| | (1) OLS | (2) IV-2SLS | (3) IV-2SLS | (4) OLS | (5) IV-2SLS | (6) IV-2SLS |
| **Ln (Rye price)** | -0.48*** | -0.42*** | -0.37*** | -0.25** | -0.31** | -0.25** |
| | (0.05) | (0.08) | (0.08) | (0.10) | (0.14) | (0.12) |
| Time controls | Yes | Yes | Yes | Yes | Yes | Yes |
| District fixed effects | No | No | Yes | No | No | Yes |
| $R^2$ | 0.95 | - | - | 0.63 | - | - |
| Root MSE | 0.07 | 0.07 | 0.19 | 0.09 | 0.09 | 0.20 |
| Observations | 25 | 25 | 179 | 25 | 25 | 179 |
| Mean of dependent variable | 3.97 | 3.97 | 3.93 | 2.09 | 2.09 | 2.04 |

Table 4 Notes: Robust standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The hypothesis of no first-order autocorrelation cannot be rejected (using the Durbin-Watson test statistic) in the specifications that include time controls. In both specifications with district-level data (regression 3, 6), disturbance terms are allowed to be correlated across states in a given year. The instrumental variables are rainfall in the current year, in the lagged year (t-1), and two years earlier (t-2).

## Appendix

Appendix Table A1: Poverty and Property Crimes – using only lagged rainfall as instruments

| | Dependent variable: Ln(Property crime) | | | | | |
|---|---|---|---|---|---|---|
| | (1) OLS | (2) IV-2SLS | (3) IV-2SLS | (4) OLS | (5) IV-2SLS | (6) IV-2SLS |
| **Ln (Rye price)** | 0.41*** | 0.26** | 0.24** | | | |
| | (0.08) | (0.09) | (0.11) | | | |
| **Ln (Real wage)** | | | | -0.88*** | -1.05* | -1.06* |
| | | | | (0.14) | (0.50) | (0.58) |
| Time controls | Yes | Yes | Yes | Yes | Yes | Yes |
| District fixed effects | No | No | Yes | No | No | Yes |
| $R^2$ | 0.78 | - | - | 0.76 | - | - |
| Root MSE | 0.08 | 0.08 | 0.15 | 0.08 | 0.12 | 0.14 |
| Observations | 25 | 25 | 179 | 25 | 25 | 179 |
| Mean of dependent variable | 5.69 | 5.69 | 5.64 | 5.69 | 5.69 | 5.64 |

Table A1 Notes: Robust standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The hypothesis of no first-order autocorrelation cannot be rejected (using the Durbin-Watson test statistic) in the specifications that include time controls. In both specifications with district-level data (regression 3, 6), disturbance terms are allowed to be correlated across states in a given year. The instrumental variables are rainfall in the lagged year (t-1), and two years earlier (t-2), not in the current year.

Appendix Table A2: Poverty versus begging and vagrancy and major crime categories

| | Dependent variable: | | | | |
|---|---|---|---|---|---|
| | Ln (Begging, vagrancy) (1) IV-2SLS | Ln (Property crime) (2) IV-2SLS | Ln (Violent crime) (3) IV-2SLS | Ln (Murder) (4) IV-2SLS | Ln (Total crime) (5) IV-2SLS |
| **Ln (Rye price)** | 0.60*** | 0.20** | -0.42*** | -0.31** | 0.08 |
| | (0.11) | (0.09) | (0.08) | (0.14) | (0.08) |
| Time controls | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | - | - | - | - | - |
| Root MSE | 0.07 | 0.09 | 0.07 | 0.09 | 0.08 |
| Observations | 25 | 25 | 25 | 25 | 25 |
| Mean of dependent variable | 7.54 | 5.69 | 3.97 | 2.09 | 6.02 |

Table A2 Notes: Robust standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The hypothesis of no first-order autocorrelation cannot be rejected (using the Durbin-Watson test statistic) in the specifications that include time controls. The instrumental variables are rainfall in the current year, in the lagged year (t-1), and two years earlier (t-2).