Economic Inquiry

LOSING BY LESS? IMPORT COMPETITION, UNEMPLOYMENT INSURANCE GENEROSITY, AND CRIME

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Increased import competition from China has brought about a host of negative consequences for the most exposed industries and labor markets. Do social programs attenuate these harmful effects? We examine changes in import competition between 1990 and 2007, taking crime as our outcome of interest and unemployment insurance as our mitigating program. We find strong evidence that counties with access to more generous unemployment insurance experienced relatively smaller increases in trade-induced property crime. This highlights a new and important positive externality of unemployment insurance. (*JEL* H00, R10)

I. INTRODUCTION

It is now widely accepted that the distributional costs and benefits of international trade are not evenly distributed. This conclusion is largely informed by a growing literature documenting that, among the most exposed industries and labor markets, increased import competition from China brought a sharp and persistent reduction in both wages and employment.[1] It has also been shown that these costs were not borne solely by those employed in disadvantaged industries. As Chinese import competition intensified, the resulting decline in labor market conditions led to a host of negative ancillary effects, including rising crime, decreased public good provision, increased political polarization, and declining health outcomes.[2]

The mounting evidence that trade imposes substantial negative effects on a non-negligible portion of the population begs the question as to whether government programs might help mitigate these effects. While this question has been the subject of considerable political and theoretical interest (Davidson and Matusz 2006; Feenstra and Lewis 1994), it has received surprisingly little empirical attention. One potential reason for this is the difficulty in finding plausibly exogenous variation in both import competition and the social safety net.

We overcome these issues by analyzing the extent to which unemployment insurance (UI) mitigates the negative consequences of increased import competition. We consider crime as our outcome variable because of its unique ability to capture both the direct and indirect effects

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1. See Arkolakis, Costinot, and Rodríguez-Clare (2012), Melitz and Redding (2015), and Ossa (2015) on measuring the gains from trade. The literature on distributional consequences is rapidly growing. See, for instance, Topalova (2007), Autor, Dorn, and Hanson (2013, 2016), Hummels et al. (2014), Caliendo and Parro (2015), Acemoglu et al. (2016), Pierce and Schott (2016a), and Hakobyan and McLaren (2016).

2. See Feler and Senses (2017) for evidence related to public goods provision and crime. Pierce and Schott (2016b), McManus and Schaur (2016) and Lang, McManus, and Schaur (Forthcoming) examine the effect of import competition on health. Autor et al. (2016) examine import competition and political polarization.

ABBREVIATIONS

2SLS: Two-Stage Least Squares
AFDC: Aid to Families with Dependent Children
CBP: County Business Pattern
CZ: Commuting Zone
GDP: Gross Domestic Product
OLS: Ordinary Least Squares
SIC: Standard Industrial Classification
TAA: Trade Adjustment Assistance
TANF: Temporary Assistance for Need Families
UI: Unemployment Insurance
WTO: World Trade Organization

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of import competition.[3] Our primary empirical approach follows the work of Autor, Dorn, and Hanson (2013) and exploits variation across U.S. labor markets in the extent of increased import competition from China between 1990 and 2007. However, because UI generosity is determined at the state level, there is also substantial variation in the generosity of benefits across local labor markets. As a result, we observe labor markets that experienced similar changes in import competition but had access to varying levels of UI generosity, which in turn allows us to assess whether UI generosity serves to mediate the consequences of increased import competition.

Our motivation for considering UI as a mitigating factor is twofold. First, UI is among the most important forms of assistance available for displaced workers. The federal government's Trade Adjustment Assistance (TAA) program is tasked with offsetting the costs of trade-induced displacement, but many of its core benefits, including wage subsidies, worker retraining, and income support, are only available for workers that have already exhausted their UI benefits.[4] Consistent with this, Autor, Dorn, and Hanson (2013) estimate that the amount of dollars per capita dollars paid by TAA was largely unresponsive to increased import competition between 1990 and 2007. Some workers may respond to trade-induced shocks by filing for retirement or disability, in which case the Social Security Administration's disability and retirement benefits may be another important program. Indeed, Autor, Dorn, and Hanson (2013) report results suggesting these programs did respond to rising import competition. However, these options are typically only feasible for a subset of workers.

This brings us to our second motivation: that UI not only affects a substantial portion of the working-age population but that, since the program is administered at the state level, it also has plausibly exogenous spatial variation in the extent of the safety net. Social Security's disability and retirement benefits, for instance, are determined at the federal level, and again, are predominantly available to older workers. Similarly, Temporary Assistance for Need Families (TANF) is only available to workers with children in the home. TAA is also a federal program, but more importantly estimates of its effect on labor market outcomes have been mixed, suggesting that its ability to mitigate nonlabor market effects may be limited.[5] In contrast, two recent papers suggest that UI may play an important role in mitigating local labor market shocks. Hsu, Matsa, and Melzer (2018) show that UI played an important role in helping individuals avoid foreclosure during the Great Recession.[6] Di Maggio and Kermani (2016) use Bartik shocks as a source of labor market fluctuations. They find that employment, earnings, and consumption in counties with access to more generous UI benefits are all less responsive to labor market shocks.

While Hsu, Matsa, and Melzer (2018) and Di Maggio and Kermani (2016) suggest that UI is effective at helping individuals buffer the income shocks associated with labor market fluctuations, whether UI mitigates the rise in crime that is otherwise associated with increased import competition from China is less clear. First, unlike the shocks considered in Hsu, Matsa, and Melzer (2018) and Di Maggio and Kermani (2016), which were temporary in nature, the increased import competition from China was much more permanent. Further, the counties affected by the rise in imports from China were different from those affected by the Great Recession, and so it is not clear that the results from Hsu et al. generalize to our setting. Finally, while foreclosures and consumption are clearly affected by UI's ability to buffer short-run income shocks, income loss is only one of the many drivers of crime. As Chalfin and McCrary (2017) note, the link between employment and crime may be a result of behavioral changes (e.g., displaced workers

3. The direct effect comes from the well-established relationship between labor market conditions and crime. See Chalfin and McCrary (2017) for an overview. The indirect effect operates through a range of channels, including deteriorating housing markets and decreases in public good provision that accompany increases in import competition.

4. Temporary wage subsidies are only available for older workers, worker retraining is only available for workers that cannot find "adequate work," and income support is only available upon the exhaustion of unemployment benefits.

5. Schochet et al. (2012) find that TAA participation increased receipt of retraining services, but failed to lead to improved labor market outcomes 4 years after job loss. Park (2012) finds that individuals successfully matched to the occupation in which they are trained through TAA enjoy slightly higher wage replacement rates than individuals who were not successfully matched. More recently, Hyman (2018) exploits quasi-random variation in TAA investigator strictness to identify causal effects of the program on worker outcomes and finds substantial initial wage and labor force participation effects, but finds that annual effects fully decay after 10 years.

6. Specifically, they use household data from the Survey of Income and Program Participation to compare trends in mortgage delinquency among employed and unemployed workers over time in the face of changes to UI generosity. They find a substantial mitigating effect of increased UI generosity between 1991 and 2010, as well as of UI extended benefits during the Great Recession.

developing feelings of anger and loss). Thus, it is ex ante unclear whether UI will effectively mitigate the rise in crime.

Our results indicate that UI indeed played a substantial role in softening the blow of import competition. Examining differences between 1990 and 2000, as well as between 2000 and 2007, we find that a $1,000 increase in imports per worker increased property crime rates by approximately 2.7% in U.S. labor markets with the mean level of UI generosity. In labor markets where UI generosity was roughly 1.4 standard deviations above the mean, however, this effect was completely mitigated. A back of the envelope calculation suggests that about 11% to 28% of the costs associated with increasing UI generosity were recovered in the form of reduced crime. Our results are robust to the inclusion of a range of potentially confounding factors, including state and local government policies that are potentially correlated with UI generosity, local demographic characteristics, and measures of social capital at the local level.

These results contribute to the growing literature on the distributional consequences of trade. The existing literature has consistently shown that the consequences of increased import competition are widespread, affecting the entire local labor market Redundant. With respect to the effect on crime, several recent papers have documented a causal link between increases in import competition and increased crime: See Che, Xu, and Zhang (2018), Deiana (2016), and Feler and Senses (2017) for evidence in the United States; Iyer and Topalova (2014) for evidence from India; and Dix-Carneiro, Soares, and Ulyssea (2018) on the experience of Brazil. Relative to these papers, as well as the broader literature on the consequences of trade, our paper is distinct in testing the ability of social insurance to act as a buffer against the negative consequences of trade.[7] Our results suggest that fiscal policy, and UI in particular, can be an effective way to ameliorate declining economic conditions resulting from such shocks.

II. METHODOLOGY

Our primary question of interest is as follows: to what extent does access to higher levels of UI generosity serve as a buffer against the negative effects of increased import competition? To answer this question, we must first define the relevant labor market. The existing literature tends to use commuting zones (CZs) as the primary unit of analysis. CZs are nonoverlapping collections of counties that are constructed such that each CZ defines a specific labor market, meaning that individuals are highly likely to live and work in the same CZ (Tolbert and Sizer 1996). A complication with this approach in our context is the fact that many CZs span state lines. Because UI generosity is determined at the state level, any measure of UI generosity will be imprecisely measured for residents of CZs that cross state lines. To remedy this, we conduct our primary analysis at the county level, where our measure of UI generosity will only be mismeasured for the subset of individuals that work and live in different states.[8]

Our primary approach to identifying changes in import competition builds on that of Autor, Dorn, and Hanson (2013). Autor et al. note that while U.S. imports from China rose rapidly between 1991 and 2007 (rising from $25 billion in 1991 to $300 billion in 2007), the extent of this increase was not uniform across industries. The manufacturing industry at the 95th percentile of the distribution experienced a 710% increase in imports while the industry at the 5th percentile saw a much more modest, though still substantial, increase of 88%. As industrial composition varies across labor markets within the United States, this variation in import changes across

7. Among existing work on the trade-crime relationship, Che, Xu, and Zhang (2018) also raise the possibility that transfers may mitigate the rise in crime. In the final table of their paper they consider heterogeneity of the trade-crime relationship based on aggregate transfers from all levels of government, nonprofit institutions, and businesses. As the authors note "Ideally, one would rely on arguably exogenous policies that lead to variation in government transfers across counties for this analysis, which can alleviate the concern on the endogeneity of the interaction term. We consider this as a limitation of our study and future works which attempt to address this issue in a standard way are extremely helpful." Indeed, focusing on UI generosity is crucial for our identification strategy, as UI generosity is a preexisting policy that is plausibly exogenous at the local level. We also dedicate considerable effort to ensuring our results are not driven by correlations between UI generosity and other variables.

8. Our county-level analysis trades off precision in the measurement of UI generosity for imprecision in how we measure import competition. This is because—to the extent that neighboring counties do not experience similar changes in import competition—a displaced worker may seek employment in a neighboring county. This type of story would undermine the link between UI generosity and should attenuate results. If so, then our results can be thought of as conservative. We present results at the CZ level in the Appendix and the qualitative takeaways are identical: import competition increases crime and UI generosity mitigates this effect.

industries implies large differences in increased import competition across U.S. labor markets.

Relying on this variation, we examine county-level changes in imports per worker as follows:

$$(1) \qquad \Delta IPW_{i,t} = \sum_j \frac{L_{ijt}}{L_{it}} \frac{\Delta Imports_{USjt}}{L_{USjt}},$$

where t represents the change between either 1990 and 2000 or 2000 and 2007.[9] That is, for each industry j, we weight national changes in imports per worker, $\frac{\Delta Imports_{USjt}}{L_{USjt}}$, by the share of a county's employment accounted for by industry j, $\frac{L_{ijt}}{L_{it}}$. All employment counts are measured at the start of the period (i.e., 1990 or 2000). Aggregating over all industries yields a county-year-specific measure of changes in import competition. As described above, this equation illustrates that variation in this measure across counties comes from two sources: differences in the extent of increases in import competition across industries and differences across counties in the importance of industries to the local labor market.

One concern with using Equation (1) as our measure of import competition is that changes in imports and labor market outcomes might be jointly determined by demand shocks, which would bias estimates. For instance, an increase in demand for an industry's products may lead to a simultaneous increase in both employment and imports in the industry. This would bias estimates of the relationship between import competition and labor market outcomes, which would, in turn, introduce bias into estimates of the relationship between import competition and crime.[10] Thus, following Autor, Dorn, and Hanson (2013), we isolate the supply-side channel by instrumenting for import competition using changes in Chinese exports to other high-income countries over the same years. As discussed at length in Autor, Dorn, and Hanson (2013), this relies on the notion that Chinese export growth was largely determined by supply-side factors, including Chinese market reforms and urbanization, as well as China's entry into the World Trade Organization (WTO) in 2001. Additionally, in the instrument we use employment counts lagged 10 years to reduce the effect of anticipated increases in import competition on contemporaneous employment levels. Together, these changes yield the following measure of changes in import competition per worker.

$$(2) \qquad \Delta IPW_{IV\ i,t} = \sum_j \frac{L_{ijt-10}}{L_{it-10}} \frac{\Delta Imports_{Othjt}}{L_{USjt-10}}.$$

The intuition is broadly similar to Equation (1). Here, $\frac{\Delta Imports_{Othjt}}{L_{USjt-10}}$ represents the change in imports per worker, where $\Delta Imports_{Othjt}$ is aggregate imports from China in other high-income countries—specifically, Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland—and U.S. employment in industry j is measured 10 years prior to alleviate concerns about endogeneity in labor market shares. We weight each industry by its labor share within the county as measured 10 years earlier, $\frac{L_{ijt-10}}{L_{it-10}}$.

While Equation (2) provides a clean labor market shock, we also must construct a measure of UI generosity in order to assess whether UI generosity provides a buffer against the negative effects of trade. As defined by the U.S. Department of Labor, UI provides temporary income support for eligible workers who become unemployed through no fault of their own. Eligibility is largely determined by both the number of hours worked and wages earned over some base period. Eligible claimants receive weekly income support for a specified number of weeks, or until they become reemployed, whichever occurs first. Weekly income support is typically calculated as the lesser of (1) the claimant's weekly wage during the base period multiplied by a replacement rate—approximately 50% on average—or (2) a specified maximum weekly benefit. Because UI is state-administered, eligibility criteria, duration length, and benefit levels vary both across states and over time. In practice, however, most variation in benefit generosity comes from variation in the maximum benefit level, rather than duration or eligibility criteria. Thus, we define overall unemployment generosity as the product of the maximum benefit level and maximum

9. Autor, Dorn, and Hanson (2013), as well as the broader literature that follows, examine changes between 1990 and 2000—Census years—as well as 2000–2007. The rationale for stopping in 2007 is that it avoids confounding effects of the financial crisis. As a robustness check we extend our analysis to 2010 and find similar results.

10. Note that the direction of the bias is ambiguous. Shifts in demand that increase employment in an industry could also reduce imports—through changing demand for quality, for instance.

duration.[11] This is the same measure used in Agrawal and Matsa (2013) and Hsu, Matsa, and Melzer (2018). To explore the effect of UI on the trade-crime relationship, we interact UI generosity at the start of each period with the instrument for imports per worker, as described above:

(3)
$$\Delta IPW_{IV\ i,t} \times UI_{it} = \sum_j \frac{L_{ijt-10}}{L_{it-10}} \frac{\Delta Imports_{Othjt}}{L_{USjt-10}} \times UI_{it}.$$

With these measures in hand, we turn to the estimating equation of interest. As described above, we are interested both in the effect of import competition on local crime rates and the ability of government assistance to mitigate this effect. We thus estimate variations of the following regression equation:

(4)
$$\Delta \ln\left(Crime_{it}\right) = \beta_0 + \beta_1 \Delta IPW_{IVi,t} + \beta_2 UI_{it} + \beta_3 \Delta IPW_{IVi,t} \times UI_{it} + \beta_4 \boldsymbol{X}_i + \delta_r + \gamma_t + \epsilon_{it},$$

where our dependent variable is the change in ln crime rates at the county level between 1990–2000 and 2000–2007.[12] On the right-hand side we include $\Delta IPW_{IVi,\,t}$—the instrumented change in county import competition defined in Equation (2). We also include UI_{it}—the county's start-of-period UI generosity, defined as the maximum weekly benefit multiplied by the maximum duration of benefits. We fix our measure of UI generosity to avoid the concern that states may adjust the generosity of their benefits in response to changes in labor market conditions. We are primarily interested in the interaction of import competition and UI generosity, which allows us to identify the extent to which UI generosity mitigated the effect of import competition on changes in crime. To ease interpretation, we standardize UI_{it}, so that β_1 can be interpreted as the effect of import competition on crime rates for the county with the mean level of UI generosity, and $\beta_1 + \beta_3$ represents the crime effect for a county one standard deviation above the mean in terms of UI generosity. In addition to the main covariates of interest, we control for Census region fixed effects, δ_r, and time fixed effects, γ_t, as well as a host of county and state-specific controls $\boldsymbol{X}_i$, which we discuss below.

III. DATA

Our main analysis draws on three broad pieces of data. The first is our measure of import competition, which relies on both aggregate import statistics and local employment shares. The second is our primary outcome variable: annual county-level crime. Finally, we include a rich set of county-specific control variables.

To construct our import competition measure, we obtain data on Chinese exports to the United States and eight other high-income countries from the U.S. Census Bureau. These data are reported at the four-digit standard industrial classification (SIC) industry level, and are available in aggregate form on David Dorn's website.[13] We then obtain county-level industry employment data, also at the four-digit SIC level, from the Census County Business Patterns (CBP) database. Together, these allow us to construct county-specific industry employment weights and the measures of imports per worker defined in Equations (1) and (2).

Our primary outcome variables, annual county-level crime counts by type, come from the county-level Federal Bureau of Investigation Uniform Crime Reports, as provided by Justin McCrary.[14] We construct 3-year averages (centered on each period, 1990, 2000, and 2007) for each of the following crimes: aggravated assaults, burglaries, forcible rapes, larcenies, motor vehicle thefts, murders, and robberies.[15] These counts reflect crimes that were either reported to a law enforcement agency or discovered by that agency. Because crime reporting is voluntary, coverage is far from universal. We restrict our sample to the set of counties that consistently report data in each of our 3 years.[16] This removes a number of

11. These data were retrieved from the Department of Labor's "Significant provisions of state unemployment insurance laws" publications, which can be retrieved from: http://www.unemploymentinsurance.doleta.gov/unemploy/statelaws.asp

12. Changes between 2000 and 2007 and scaled to by 10/7 to make the two changes comparable.

13. Dorn also makes available the template for cleaning and aggregating the County Business Pattern employment data. Code and data can be found at http://www.ddorn.net/data.htm

14. http://eml.berkeley.edu/~jmccrary/UCR/index.html

15. We use 3-year averages to reduce noise due to random year-to-year fluctuations, but results are nearly identical when we simply use observations from 1990, 2000, and 2007.

16. Specifically, we keep county-year pairs in which all agencies report. This alleviates concerns that agencies select into the sample as a function of crime rates. Agencies report monthly. We keep all agencies that report in at least 1 month, scaling by 12 divided by the number of reported months.

TABLE 1
Summary Statistics

	Regression Sample			Counties with Incomplete Data		
	Observations	**Mean**	***SD***	**Observations**	**Mean**	***SD***
Property crimes per 1,000 persons	3,062	26.462	17.405	—	—	—
Δper-worker import competition ($1,000s)	3,062	1.831	3.380	3,044	2.563	5.560
Unemployment generosity (U.S. dollar)	3,062	8282	1549	3,044	7989	1910
Income per capita in 1990 ($1,000s)	3,062	15.636	3.549	3,038	14.831	3.256
Manufacturing share in 1990	3,062	0.225	0.163	3,038	0.279	0.171
Bachelor of Arts share in 1990	3,062	0.141	0.067	3,038	0.126	0.059
Female labor force participation in 1990	3,062	0.439	0.027	3,038	0.441	0.027
Under 25 share in 1990	3,062	0.360	0.046	3,038	0.364	0.041
Foreign share in 1990	3,062	0.027	0.041	3,038	0.016	0.026
Black share in 1990	3,062	0.069	0.121	3,038	0.099	0.159
Hispanic share in 1990	3,062	0.062	0.131	3,038	0.027	0.085
Per capita expenditures in 1987	3,062	0.496	0.394	2,996	0.401	0.362
Per capita revenues in 1987	3,062	0.508	0.399	2,996	0.413	0.379
Per capita intergovernmental revenues in 1987	3,062	0.176	0.189	2,996	0.140	0.144
Per capita police spending in 1987	3,062	0.028	0.031	2,996	0.020	0.021
Per capita cash assistance expenditures in 1987	3,062	0.013	0.039	2,996	0.005	0.017

Notes: Per-worker import competition is defined in Equation (1). Unemployment generosity equals the maximum weekly benefit multiplied by the maximum number of weeks that unemployment can be collected.

counties from our regression sample. However, as we show below, the counties included in our analysis are quite similar to those with missing crime data.

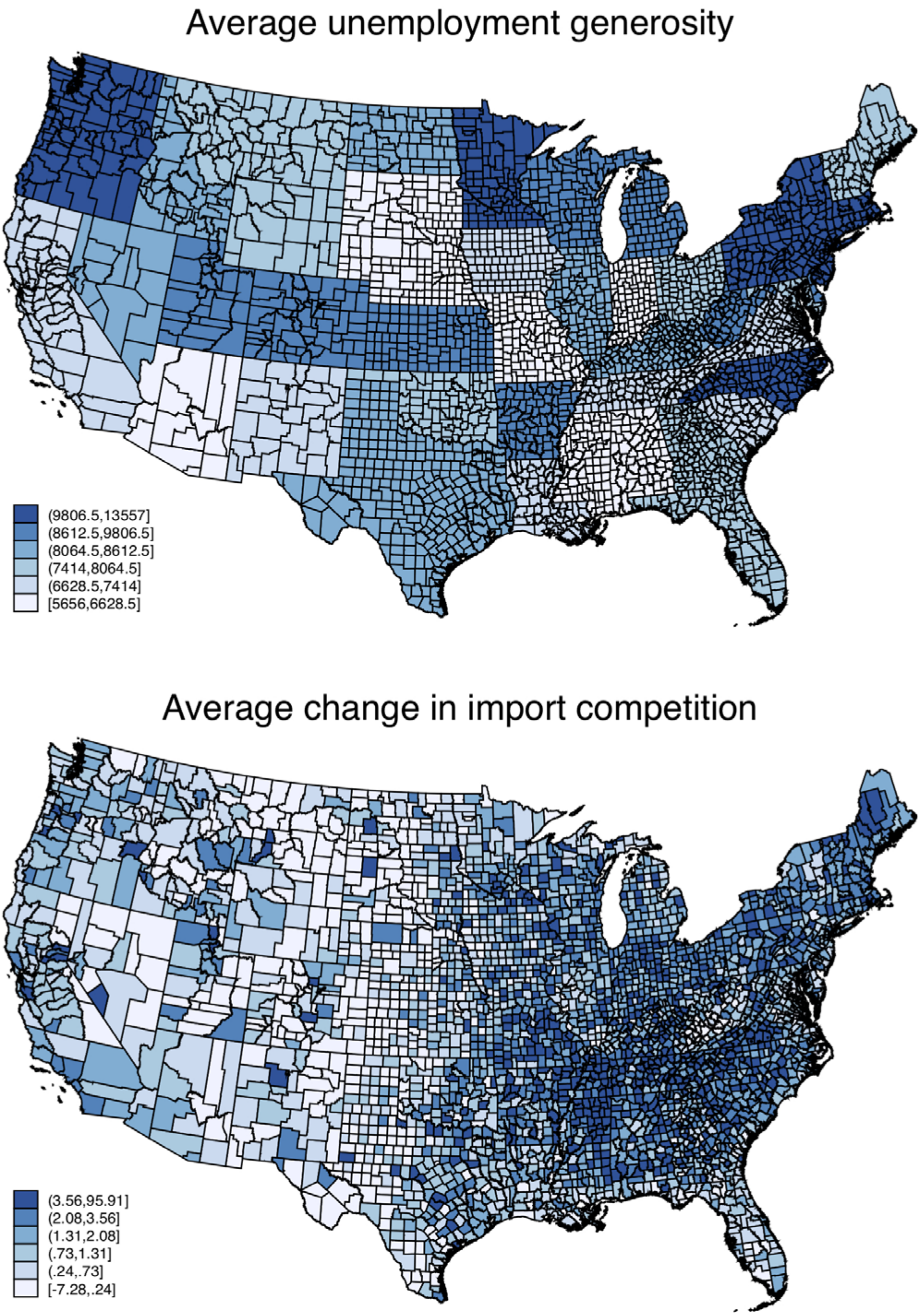
Our analysis also draws on a number of fiscal and demographic controls. Our county-level fiscal controls (police expenditures, revenue transfers from other governments, and welfare expenditures) come from the Census counties database, which reports county-level fiscal characteristics every 5 years from 1972 to 2007. We draw on the 1987 report as it is the closest report to 1990 when the rest of our controls are measured. The demographic controls—the share of population with a college degree, share of female population in the labor force, share of population under the age of 25, black and Hispanic population shares, and foreign-born share—come from the 1990 census. We use Census population estimates from 1990, 2000, and 2007 to construct annual crime rates. We also calculate the share of 1990 county employment accounted for by the manufacturing sector using CBP employment data.

Our final dataset includes 3,062 county-year pairs. Table 1 presents summary statistics for our regression sample, as well as the remaining 3,044 county-year pairs with incomplete crime data. Our regression sample has a slightly higher average per capita income in 1990 ($15,636 instead of $14,831) and experienced a slightly smaller change in import competition ($1,831 per worker instead of $2,653 per worker). Aside from these two characteristics, the two samples are quite similar.

Figure 1 illustrates the spatial variation in both county-level UI generosity and import competition. The first panel depicts average UI generosity across 1990 and 2000 while the second panel presents average county level changes in import competition. That is, for each county, we take the average change in import competition from 1990 to 2000 and 2000 to 2007.[17] Although the spatial variation in UI generosity is less pronounced, there is substantial within-state variation in import competition, which ensures that we observe counties with identical access to UI generosity but substantially different changes in import competition. Illustrative of this is the fact that the raw correlation between UI generosity and our measure of import competition is −0.061. In Supporting Figure S1, we plot a two-way density of UI generosity and imports per worker and see that there is substantial variation in UI generosity across the entire distribution of imports per worker. This variation allows us to separately identify the two effects across counties.

17. As mentioned previously, the change between 2000 and 2007 is weighted by 10/7 to make the two changes comparable.

FIGURE 1
Spatial Variation in UI Benefits and Import Competition



Notes: UI generosity equals the maximum weekly benefit multiplied by the maximum number of weeks that unemployment can be collected. We report the average UI generosity as measured in either 1990 or 2000. Import competition is measured in 1,000s of dollars per worker.

TABLE 2
Change in ln(Property Crime Rate) as a Response to Import Competition

	(1)	(2)	(3)	(4)
OLS				
Δper-worker import competition	0.024**	0.031***	0.010**	0.010**
	(0.010)	(0.006)	(0.004)	(0.004)
(Δper-worker import competition) × UI generosity		−0.014***	−0.010***	−0.010***
		(0.005)	(0.004)	(0.004)
UI generosity (main effect)	N	Y	Y	Y
Demographic controls	N	N	Y	Y
Fiscal controls	N	N	N	Y
Observations	3,052	3,052	3,052	3,052
R-Squared	0.199	0.207	0.277	0.288
2SLS				
Δper-worker import competition	0.025***	0.027***	0.029***	0.027***
	(0.007)	(0.008)	(0.007)	(0.007)
(Δper-worker import competition) × UI generosity		−0.019***	−0.020***	−0.020***
		(0.005)	(0.008)	(0.007)
UI generosity (main effect)	N	Y	Y	Y
Demographic controls	N	N	Y	Y
Fiscal controls	N	N	N	Y
Kleibergen–Paap joint *F*-statistic	100.755	333.230	144.287	141.909
First stage *F*-statistic (imports per worker)		28.504	37.450	36.034
First stage *F*-statistic (imports per worker × UI gen.)		5.570	5.320	5.694
Observations	3,052	3,052	3,052	3,052
R-Squared	0.185	0.198	0.266	0.278

Note: Robust standard errors (clustered at the state level) reported in parentheses. All specifications include year and region fixed effects. In the 2SLS panel, per-worker import competition is instrumented following Equation (2). UI generosity equals the maximum weekly benefit multiplied by the maximum number of weeks that UI can be collected. Both UI variables are measured at the start of period (1990 or 2000). A one-unit change in per-worker import competition represents a 1,000 dollar per-worker increase. Demographic controls include manufacturing share, income per capita, share of population with a college degree, share of female population in the labor force, share of population under the age of 25, foreign-born share, black share, and Hispanic share (all measured in 1990). Fiscal controls include per capita police expenditures, per capita revenue transfers from other governments, per capita welfare expenditures, total expenditures per capita, and total revenue per capita (all measured in 1987). Regressions are weighted by county-population in 1990.

*$p < .10$. **$p < .05$. ***$p < .01$.

IV. RESULTS

A. *Examining whether UI Generosity Acts as a Mediating Force*

As a starting point for our analysis, Table 2 presents baseline results of the relationship between increased import competition and crime. The dependent variable in all columns is the change in ln(property crime rates) between 1990–2000 and 2000–2007. The top panel reports ordinary least squares (OLS) results while the bottom panel instruments for changes in import competition as described in Equation (2). In column 1 we simply consider the relationship between changes in imports per worker and changes in property crime rates (with year and region fixed effects). There we see that a $1,000 increase in imports per worker is associated with a roughly 2.5% increase in property crime rates. This is true in the OLS specification and when we instrument for imports per worker. In column 2 we fully interact our rise in import competition with UI generosity and find evidence that the rise in crime was much lower in places with access to more generous UI. In columns 3 and 4 we add our broad sets of controls. Column 3 includes our demographic controls (manufacturing share, income per capita, share of population with a college degree, share of female population in the labor force, share of population under the age of 25, foreign-born share, black share, and Hispanic share, all measured in 1990). Column 4 adds a host of fiscal controls (including per capita police expenditures, per capita revenue transfers from other governments, per capita welfare expenditures, total expenditures per capita, and total revenue per capita, all measured in 1987). Results are largely unaffected by the inclusion of these controls: we continue to see strong evidence that access to more generous UI benefits attenuates the rise in crime that would otherwise accompany an increase in import competition.

TABLE 3

Change in ln(Crime Rates) as a Response to Import Competition

	Property (1)	Violent (2)	Decomposed Property Crimes: Burglary (3)	Larceny (4)	Motor Vehicle Theft (5)	Robbery (6)
Panel A: Documenting the trade-crime relationship						
Δper-worker import competition	0.025**	0.018**	0.018	0.024***	0.044	0.030*
	(0.010)	(0.008)	(0.015)	(0.009)	(0.029)	(0.016)
Kleibergen−Paap F-statistic	50.079	50.089	50.063	49.995	49.777	47.128
Observations	3,052	3,027	3,035	3,038	2,973	2,298
R-Squared	0.255	0.248	0.457	0.151	0.180	0.276
Panel B: Interacting changes in trade with UI generosity						
Δper-worker import competition	0.027***	0.015*	0.021**	0.026***	0.045**	0.029***
	(0.007)	(0.009)	(0.010)	(0.010)	(0.020)	(0.010)
(Δper-worker import competition) $\times$ UI generosity	−0.020***	0.002	−0.025***	−0.015**	−0.038*	−0.024**
	(0.007)	(0.010)	(0.008)	(0.006)	(0.020)	(0.012)
First stage F-statistic (imports per worker)	141.909	141.898	141.855	141.595	141.034	126.892
First stage F-statistic (imports per worker $\times$ UI gen.)	36.034	36.046	36.019	35.836	35.902	32.675
Kleibergen−Paap joint F-statistic	5.694	5.707	5.692	5.679	5.654	5.063
Observations	3,052	3,027	3,035	3,038	2,973	2,298
R-Squared	0.278	0.254	0.463	0.166	0.219	0.300

Notes: Robust standard errors (clustered at the state level) reported in parentheses. Change in per-worker import competition is instrumented following Equation (2). UI generosity equals the maximum weekly benefit multiplied by the maximum number of weeks that UI can be collected. Both UI variables are measured at the start of period (1990 or 2000). A one unit change in per-worker import competition represents a 1,000 dollar per-worker increase. All regressions include year fixed effects and Census region fixed effects and manufacturing share in 1990. Regressions also include the following demographic variables (measured in 1990): income per capita, share of population with a college degree, share of female population in the labor force, share of population under the age of 25, foreign-born share, black share, and Hispanic share. Finally, we also include the following county-level fiscal controls (measured in 1987): per capita police expenditures, per capita revenue transfers from other governments, per capita welfare expenditures, total expenditures per capita, and total revenue per capita. In Panel B all regressions also include the noninteracted start-of-period UI generosity. All regressions are weighted by county-population in 1990. Because our outcome is the change ln(crime rates) note that observations may vary from crime to crime if a county experiences no crimes in one of the reported years.

$^{*}p<.10.$ $^{**}p<.05.$ $^{***}p<.01.$

Table 3 presents our main IV results for overall property crime rates and overall violent crime rates as well as each component of our property crime measure (burglaries, larcenies, motor vehicle thefts, and robberies).[18] Columns 1 and 2 present results for aggregate property and violent crimes, while columns 3–6 present results for each component of aggregate property crimes (burglary, larceny, motor vehicle theft, and robbery). The estimated effects on violent crime (aggregate or decomposed) tend to be either unstable or insignificant in most specifications, thus we omit the decomposed violent crime results. This is consistent with the rise in crime being more economically motivated rather than being driven by behavioral changes.[19]

18. OLS results are available in the Appendix, as are reduced form results. The main result (the interaction between import competition and UI generosity) is broadly similar though often less precisely estimated.

19. See, for instance, Lindo, Schaller, and Hansen (2018) on the relationship between economic conditions and child maltreatment.

Panel A presents estimates of the average relationship between changes in import competition. Results indicate that a $1,000 increase in imports per worker raised property crime rates by roughly 2.5% and violent crimes by nearly 2%. By decomposing property crimes, we find that burglary and robbery rates increased by roughly 1.8% and 3%, respectively, larceny rates increased by 2.5%, and motor vehicle thefts increased by nearly 4.5%, although only the effect on larceny is significant at the 5% level. These results are in line with the existing literature. Feler and Senses (2017), for instance, estimate that a $1,000 increase in import competition raised CZ property crime rates by approximately 3.5%.

In Panel B we interact our instrumented change in import competition with UI generosity. Here we consistently find that UI generosity served as a buffer against the rise in crime that would have otherwise accompanied the increase in import competition. Specifically, we find that increasing UI generosity by roughly 1.4 standard deviations completely mitigates the rise in crime. This suggests that UI generosity may play an important role in stabilizing the local economy against local labor market shocks.

The Kleibergen–Paap F-statistic strongly rejects weak instruments in Panel A. Once we move to the interacted model, we see that the first stage Angrist–Pischke F-statistic for our primary covariate of interest (the interaction between UI generosity and changes in import competitions) strongly rejects weak instruments. The Kleibergen–Paap joint F-statistic, evaluated against the critical values in Stock and Yogo (2005) for 5% significance, is also reasonable—the hypothesis that the maximum size distortion is 15% is clearly rejected.

Before proceeding to our main robustness checks, it is worth noting that a relatively recent literature has emerged on the econometrics of shift-share instruments that draw on regional employment patterns as the basis of their instrument. For instance, Goldsmith-Pinkham, Sorkin, and Swift (2018), Borusyak, Hull, and Jaravel (2018), and Adão, Kolesár, and Morales (2018) all have applications to the Autor, Dorn, and Hanson (2013) instrument used above. One of the central takeaways from these papers is that clustering standard errors at the state level may not be sufficient. The literature has, unfortunately, not yet settled on a solution to this issue. It is worth noting, however, that unless our standard errors increase by more than 50% we would still be able to reject the hypothesis that our interaction is equal to zero at the 5% significance level. It would take a 74% increase in our standard errors to fail to reject at the 10% significance level. These numbers are at or above the upper end of what Adão, Kolesár, and Morales (2018) illustrate is likely to happen when they explore alternative standard error corrections.

B. Additional Tests to Validate Our Empirical Approach

We now conduct several tests to help validate our empirical design. The first test applies our instrumental variables approach while taking as outcomes *previous* changes in crime. Specifically, we relate changes in crime between 1990 and 2000 to (instrumented) changes in imports per worker between 2000 and 2007, and changes in crime between 1980 and 1990 to changes in imports per worker between 1990 and 2000. The results of this placebo test are presented in Table 4. One of the 18 coefficients of interest is statistically significant, which is roughly what we would expect by chance. Notably, with the exception of robbery, the magnitudes of all coefficients are effectively zero. This suggests that preexisting changes in crime are not predictive of future changes in economic conditions, which could otherwise be an important source of bias for our analysis.

Our second test asks whether it is appropriate to model the interaction between UI generosity and import competition as linear. We take two approaches to assess this question. First, we consider how our baseline results change when we model the interaction between UI generosity and import competition as a quadratic function. These results are presented in the first panel of Table 5. There we see that the coefficient for the quadratic term of the interaction is not only statistically insignificant but it is also very close to zero. We interpret this as suggestive that our linear specification is correct. In the bottom panel of Table 5, we take an alternative approach in that we explore the extent to which increases in import competition affected crime rates for each quartile of the UI generosity distribution. There we see that, as expected, counties at the bottom of the distribution (0–25th percentile) saw their property crime rate increase by roughly 5.4% for each \$1,000 increase in imports per worker. For counties with UI generosity falling in the 25–50th percentiles the effect was roughly 4%. Both of these effects are statistically significant at the 5% level. For counties with generosity between 50 and 75th percentiles we see an effect on the order of 2.4% but it is no longer statistically significant. For counties falling between the 75th and 100th percentiles the coefficient is effectively zero and statistically insignificant. The transitions between each of these bins are also roughly linear, which further increases our confidence in our modeling assumptions.

Next, we implement a series of tests to illustrate that the above results are not being driven by other policies that are correlated with UI generosity. As a first step toward examining this issue, we regress several state and local characteristics and policies on UI generosity. We separately consider each of our county-specific controls: per capita police expenditures, per capita transfers from the state and federal governments, per capita public welfare expenditures, total expenditures per capita, total revenues per capita, manufacturing employment share, the share of population with a college degree, share of female population in the labor force, share of population under

TABLE 4

Placebo Test Assessing whether Future Changes in Import Competition Predict Past Changes in Crime

			Decomposed Property Crimes			
	Property (1)	Violent (2)	Burglary (3)	Larceny (4)	Motor Vehicle Theft (5)	Robbery (6)
Panel A: Documenting the trade–crime relationship						
Δper-worker import competition	−0.003	0.004	0.000	−0.006	0.000	0.015
	(0.007)	(0.013)	(0.009)	(0.008)	(0.015)	(0.012)
Kleibergen–Paap F-statistic	47.791	47.771	47.782	47.611	47.585	44.628
Observations	2,989	2,939	2,973	2,979	2,918	2,271
R-Squared	0.487	0.400	0.417	0.426	0.354	0.190
Panel B: Interacting changes in trade with UI generosity						
Δper-worker import competition	−0.002	0.002	0.001	−0.006	0.002	0.012
	(0.007)	(0.013)	(0.009)	(0.008)	(0.014)	(0.012)
(Δper-worker import competition) × UI generosity	0.002	0.002	0.005	0.002	0.004	0.023**
	(0.004)	(0.011)	(0.009)	(0.004)	(0.012)	(0.011)
First stage F-statistic (imports per worker)	138.587	138.506	138.557	137.960	136.916	124.775
First stage F-statistic (imports per worker × UI gen.)	34.530	34.473	34.523	34.183	34.231	31.097
Kleibergen–Paap joint F-statistic	5.337	5.329	5.336	5.325	5.318	4.828
Observations	2,989	2,939	2,973	2,979	2,918	2,271
R-Squared	0.492	0.402	0.423	0.429	0.360	0.188

Notes: Robust standard errors (clustered at the state level) reported in parentheses. Change in per-worker import competition is instrumented following Equation (2). UI generosity equals the maximum weekly benefit multiplied by the maximum number of weeks that UI can be collected. Both UI variables are measured at the start of period (1990 or 2000). A one unit change in per-worker import competition represents a 1,000 dollar per-worker increase. All regressions include year fixed effects and Census region fixed effects and manufacturing share in 1990. Regressions also include the following demographic variables (measured in 1990): income per capita, share of population with a college degree, share of female population in the labor force, share of population under the age of 25, foreign-born share, black share, and Hispanic share. Finally, we also include the following county-level fiscal controls (measured in 1987): per capita police expenditures, per capita revenue transfers from other governments, per capita welfare expenditures, total expenditures per capita, and total revenue per capita. All regressions are weighted by county-population in 1990. Because our outcome is the change ln(crime rates) note that observations may vary if a county experiences no crimes in one of the reported years.

* $p < .10$. ** $p < .05$. *** $p < .01$.

the age of 25, foreign population share, black population share, Hispanic population share, and ln(per capita income). As discussed above, each of these are measured as of 1990 except for the local finance measures, which are from 1987. We also consider the county's unemployment rate in 1990, changes in imports per worker over the 1990–2000 period, property and violent crime rates in 1990 as well as two state-level variables: an indicator for whether the state has a "Right to Work" law and the share of workers belonging to a union.

The estimated UI generosity coefficient from each of these separate regressions is reported in Figure 2. Of all the controls we consider, we only observe one statistically significant difference: counties with access to more generous UI happen to be in states with slightly more union coverage (and relatedly, these states are less likely to have adopted "Right to Work" legislation). What is reassuring for our analysis, however, is that UI generosity does not appear to be systematically related to local government spending, manufacturing shares, or overall changes in import competition. As to how UI generosity might relate to other state-level characteristics, Hsu, Matsa, and Melzer (2018) construct a panel of UI generosity spanning 1991–2010 and regress that generosity on several state-level characteristics (unemployment rates, gross domestic product (GDP) per capita, house price growth, average wages, and UI trust fund reserves). Hsu et al. find that none of those variables are a significant predictor of UI generosity, further establishing the exogeneity of UI generosity with respect to local economic conditions. This is perhaps not surprising, as changes to UI generosity typically result from lawmakers submitting and voting on specific bills. Thus, political considerations and bureaucratic delays make it difficult for state-specific UI generosity to respond quickly to changes in local economic conditions.

Moving beyond these correlations, we conduct a series of robustness checks to more explicitly rule out the possibility that

TABLE 5
Exploring Nonlinearity in the UI Generosity Interaction

	Property (1)	Violent (2)	Decomposed Property Crimes: Burglary (3)	Larceny (4)	Motor Vehicle Theft (5)	Robbery (6)
Panel A: Interacting the square of UI generosity with trade						
Δper-worker import competition	0.027***	0.009	0.025**	0.025**	0.045**	0.031***
	(0.008)	(0.010)	(0.010)	(0.010)	(0.021)	(0.012)
(Δper-worker import competition) × UI generosity	−0.021***	−0.005	−0.023**	−0.017***	−0.042*	−0.024*
	(0.008)	(0.009)	(0.010)	(0.006)	(0.023)	(0.012)
(Δper-worker import competition) × (UI gen. sq)	0.000	0.008	−0.004	0.001	0.001	−0.002
	(0.003)	(0.006)	(0.004)	(0.004)	(0.013)	(0.007)
Panel B: Assessing the average trade effect by UI generosity quartile						
Δper-worker import competition × 0–25th pct.	0.054***	0.015	0.055***	0.043***	0.108***	0.065***
	(0.008)	(0.014)	(0.011)	(0.011)	(0.024)	(0.013)
Δper-worker import competition × 25–50th pct.	0.040**	0.014	0.014	0.045*	0.056***	0.052*
	(0.018)	(0.028)	(0.017)	(0.025)	(0.019)	(0.027)
Δper-worker import competition × 50–75th pct.	0.024	0.031	0.023	0.014	0.034	0.083**
	(0.028)	(0.033)	(0.023)	(0.031)	(0.039)	(0.037)
Δper-worker import competition × 75–100th pct.	0.002	0.016	−0.008	0.008	−0.006	−0.002
	(0.007)	(0.010)	(0.007)	(0.009)	(0.012)	(0.011)

Notes: Robust standard errors (clustered at the state level) reported in parentheses. Change in per-worker import competition is instrumented following Equation (2). UI generosity equals the maximum weekly benefit multiplied by the maximum number of weeks that UI can be collected. Both UI variables are measured at the start of period (1990 or 2000). A one-unit change in per-worker import competition represents a 1,000 dollar per-worker increase. All regressions include year fixed effects and Census region fixed effects and manufacturing share in 1990. Regressions also include the following demographic variables (measured in 1990): income per capita, share of population with a college degree, share of female population in the labor force, share of population under the age of 25, foreign-born share, black share, and Hispanic share. Finally, we also include the following county-level fiscal controls (measured in 1987): per capita police expenditures, per capita revenue transfers from other governments, per capita welfare expenditures, total expenditures per capita, and total revenue per capita. In Panel A all regressions also include the noninteracted start-of-period UI generosity as well as its square. In Panel B all regressions include a series of indicators for each quartile of the UI generosity distribution. All regressions are weighted by county-population in 1990.

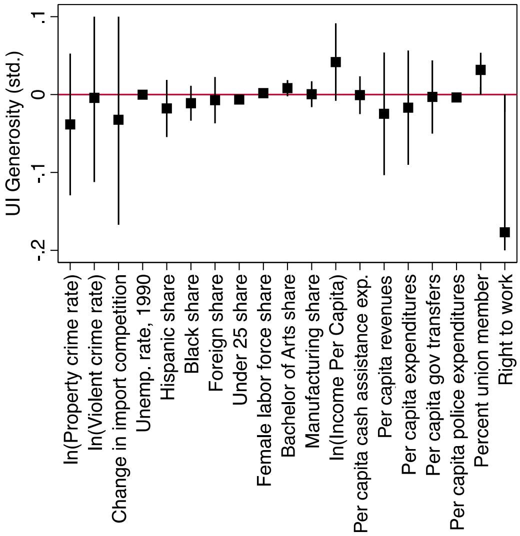
$^{*}p < .10$. $^{**}p < .05$. $^{***}p < .01$.

confounding programs are driving our results. We display these results graphically in Figure 3. There we plot the 95% confidence interval for the coefficient of interest (the interaction between UI generosity and changes in import competition) across a number of tests. The first specification corresponds to the baseline results presented in Table 3. Each panel corresponds to a different type of crime (aggregate property, aggregate violent, burglary, larceny, motor vehicle theft, and robbery). While we only present results for the interaction between UI generosity and changes in import competition, it is worth noting that the baseline effect of import competition on crime is also stable across these robustness checks. Full regression results are presented in Online Appendix Tables S3 through S10.

The first set of robustness checks (specifications 2, 3, and 4) are aimed at alleviating concerns that confounding state or local policies could also have helped buffer the increase in crime in a way that drives our results. In specification 2 we add to the baseline specification the full interaction between a number of state-level policies and changes in import competition, in addition to the interaction between UI generosity and import competition. Specifically, we consider the state's minimum wage, Aid to Families with Dependent Children (AFDC) generosity, state public welfare spending per capita, state police spending per capita, state per capita revenue transfers to local governments, union membership share, and an indicator for whether the state has a right to work policy.[20] Each policy is measured as of 1990. While the aggregate property crime confidence interval falls just outside the 5% significance level, the point estimate is stable. Furthermore, in many ways this specification is overly conservative in that it includes a large number of additional interactions. In Table S3 we report results in which we consider each interaction one at a time and find that the UI generosity interaction

20. AFDC was replaced by TANF in 1996. Here, 1990 AFDC generosity is defined as the maximum monthly benefit payable to a single parent caring for two dependents.

FIGURE 2
Is UI Generosity a Meaningful Predictor of Other Characteristics and Policies?



Notes: This graph plots the 95% confidence interval obtained by regressing each outcome variable on 1990 UI generosity. Standard errors are clustered at the state level and regressions are weighted by county-level population in 1990. County-level fiscal outcomes come from a 1987 Census survey. All demographic outcomes are from the 1990 census. County-specific unemployment rate is from 1990.

remains negative and statistically significant at the 5% level or higher in each specification.

Along the same lines, specification 3 considers the full interaction between several county-level policies and changes in import competition. Specifically, we add county-level police expenditures per capita, per capita revenue transfers from state and federal governments, cash assistance expenditures per capita, county-level crime rates in 1990, the county's 1990 unemployment rate, and the Rupasingha and Goetz (2008) measure of social capital.[21] In specification 4, we add to our baseline specification state-by-year fixed effects. The inclusion of these fixed effects flexibly control for state-level changes occurring in each time period. Results from these three specifications increase our confidence that UI generosity is not simply serving as a proxy for other state or local-level policies.

Specifications 5 through 8 present some additional robustness checks. In specification 5 we alter our sample to include changes between 2000 and 2010 as our second period rather than changes between 2000 and 2007.[22] In specification 6 we fix UI generosity based on 1990 levels to avoid concerns that states may have changed their UI generosity in response to local-level changes in import competition. Finally, in specifications 7 and 8 we separately consider changes between 1990 and 2000 and 2000 and 2007. Results are largely unaffected by any of these changes.

Two additional robustness checks are presented in the Appendix. First, in Table S10 we re-run our main specification at the CZ level. As noted earlier, we prefer the county to the CZ for this analysis because many CZs cross state lines and any measure of UI generosity will be imprecisely measured for such CZs by construction. Our qualitative story, however, is largely unaffected when we re-run our analysis at the CZ level. Specifically, we find that—in a CZ with average UI generosity—property crime rates rose by about 3.4% for every $1,000 increase in imports per worker but that a 1.26 standard deviation increase in UI generosity was sufficient to fully mitigate this effect.

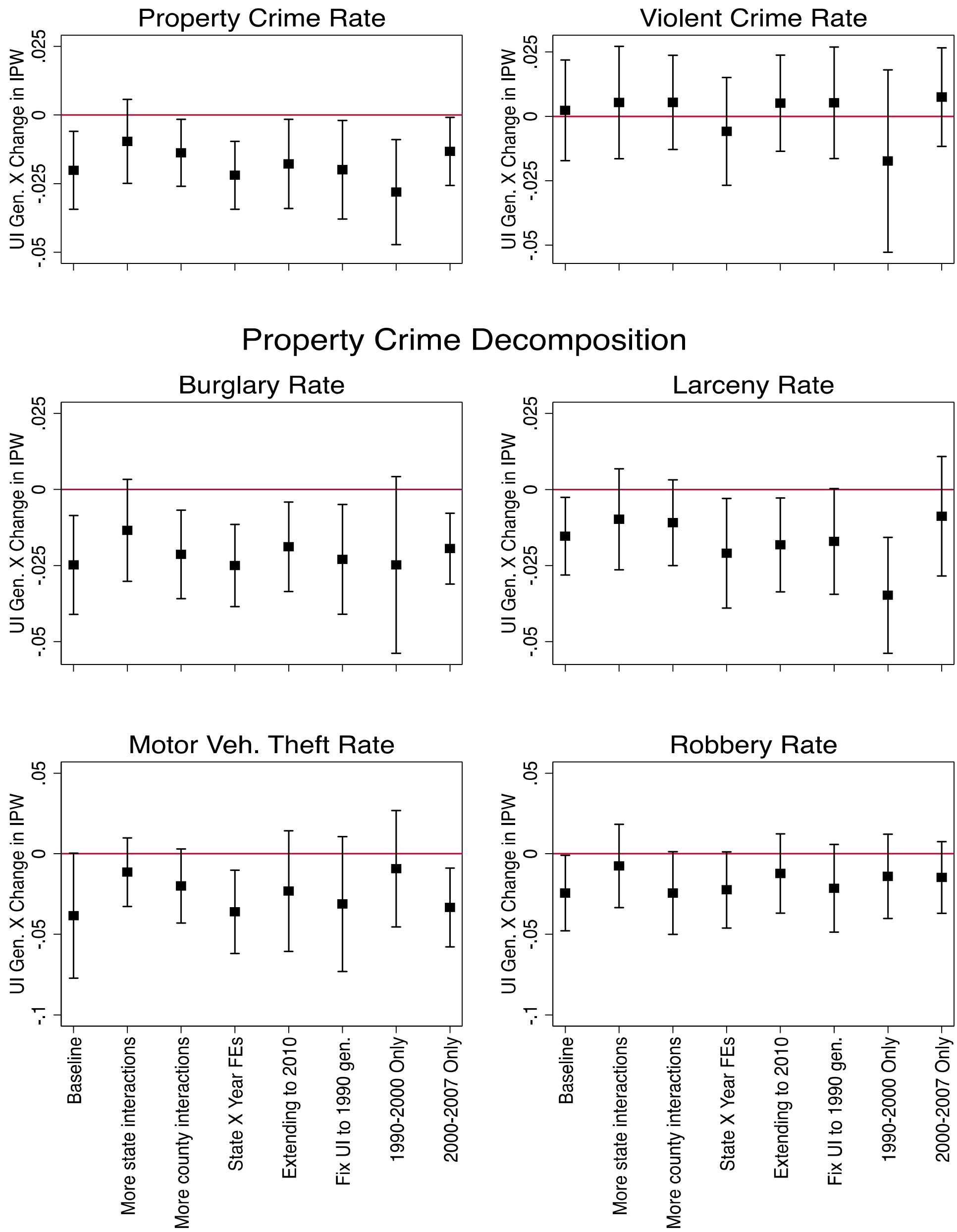
Our second test exploits an entirely distinct source of variation in import competition across labor markets. Specifically, we follow Pierce and Schott (2016a), who explore cross-industry variation in reductions of potential tariff increases that followed China's accession to the WTO in 2001. Pierce and Schott (2016a) show that U.S. industries facing the largest potential tariff increases experienced a reduction in employment in the years following China's entry to the WTO and subsequent elimination of tariff risk. Variation in this approach is policy-driven, and thus mitigates endogeneity concerns about relying on a measure of import competition based on observed trade flows. We use this variation to create a labor-market-level measure of exposure to import competition following China's WTO entry, and find qualitatively identical results: local property crime rate rose in response to increased import competition, but the extent of this increase was

21. Social capital is typically thought of as the networks, norms, and social trust that facilitate coordination and cooperation for mutual benefit (Putnam 1995). Consistent with this, a large literature has found social capital to be associated with a host of benefits, including lower mortality (Kawachi et al. 1997), lower crime (Buonanno, Montolio, and Vanin 2009), and increased economic mobility (Chetty and Hendren 2018).

22. Here it is worth noting that our source for crime data ends in 2010, and so our 2010 crime rate is simply the average of 2009 and 2010 rather than the average of 2009 through 2011.

FIGURE 3

Examining the Robustness of the Interaction between UI Generosity and Changes in Import Competition



Notes: Each panel corresponds to a specific change in ln(crime rates). The coefficient of interest is the interaction between changes in import competition and UI generosity. Specification 1 corresponds to the baseline specification, as in Table 2. In specification 2 we interact changes in import competition with each state's minimum wage, AFDC generosity, public welfare spending per capita, police spending per capita, and per capita revenue transfers to local governments, each measured in 1990. In the third specification we fully interact changes in import competition with each county's police expenditures per capita, per capita revenue transfers from state and federal governments, cash assistance expenditures per capita, preexisting crime rates, unemployment rate in 1990, and social capital index. Fiscal controls are as of 1987. In specification 4 we add state-by-year fixed effects to the baseline specification. In specification 5 we consider changes between 2000 and 2010. In specification 6 we fix UI generosity at 1990 levels.

TABLE 6

How Do UI Programs Respond to Changes in Import Competition?

	Δ(Per Capita Benefits Paid) (1)	Δln(Total Benefits Paid) (2)	Δln(Weeks Compensated) (3)	Δln(Weeks Claimed) (4)	Δln(Average Weekly Benefit) (5)
Δper-worker import competition	5.570	0.065	0.013	0.008	0.050
	(6.621)	(0.051)	(0.051)	(0.046)	(0.038)
(Δper-worker import competition) × UI generosity	12.503**	0.068**	0.071***	0.059***	−0.003
	(5.042)	(0.033)	(0.022)	(0.021)	(0.027)
First stage F-statistic (imports per worker)	56.126	56.126	56.126	56.126	56.126
First stage F-statistic (imports per worker × UI gen.)	111.037	111.037	111.037	111.037	111.037
Kleibergen−Paap joint F-statistic	52.323	52.323	52.323	52.323	52.323
Observations	86	86	86	86	86
R-Squared	0.478	0.514	0.545	0.579	0.298

Notes: Robust standard errors (clustered at the state level) reported in parentheses. Change in per-worker import competition is instrumented following Equation (2), except that we are using state-level labor shares. UI generosity equals the maximum weekly benefit multiplied by the maximum number of weeks that UI can be collected. Both UI variables are measured at the start of period (1990 or 2000). A one unit change in per-worker import competition represents a 1,000 dollar per-worker increase. All regressions include year fixed effects and Census region fixed effects. Each regression also includes the noninteracted UI generosity variable.

* $p < .10$. ** $p < .05$. *** $p < .01$.

much smaller in counties with access to more generous UI benefits. We discuss this approach in detail in the Appendix.

While the previous robustness checks have focused primarily on ruling out alternative interactions, another approach to help determine the mechanisms at work is to assess whether our measure of UI generosity actually predicts variation in observed UI payments. We do this by drawing on state-level data from the Department of Labor, which provides annual data on UI claims and payments for each state.[23] In Table 6 we use these data to assess how changes in state-level import competition affected changes in total benefits paid, total weeks of compensation provided, the number of weeks claimed, and average weekly benefits. Our measures of import competition are calculated as in Equations (1) and (2), with employment counts calculated at the state level. As before, we consider changes between 1990 and 2000, and between 2000 and 2007.

In columns 1 through 4 we see strong evidence that, for a given $1,000 increase in imports per worker, states with more generous UI ended up: paying more benefits to workers (columns 1 and 2); compensating workers for more weeks (column 3); and receiving more claims (column 4). These results suggest that states with more generous benefits saw a disproportionate increase in workers using those benefits as exposure to Chinese import competition increased. Results from columns 1 and 2 are encouraging because they suggest that our measure of UI generosity did translate into higher payments to workers. Results from columns 3 and 4 are consistent with individuals in more generous states facing a stronger incentive to file claims and/or delay the acquisition of a new job.[24] Finally, in column 5 we consider average weekly benefits as our outcome variable and find no evidence of a differential change in average benefit level for states with more generous UI benefits. This suggests that high UI states did not respond to trade shocks by further increasing the generosity of their UI program, which in turn, lends further support for our identification strategy.

Next, we construct two alternative measures of generosity to further assess whether these effects are driven by an increase in cash assistance. First, we consider a measure of leniency in granting UI benefits. As noted above, the data report, by state,

23. The data are available at https://oui.doleta.gov/unemploy/claimssum.asp

24. Recent work by Nekoei and Weber (2017) suggests that the moral hazard effects of UI will be largest when increasing one's search intensity will not improve their subsequent job match. Because of the nature of our import competition measure, it was likely not possible for displaced workers to find a comparable job in the manufacturing sector. Accordingly, and consistent with the predictions of Nekoei and Weber, we might expect displaced workers with access to generous UI benefits to disproportionately decrease their job search intensity. This would explain why the increase in compensation payments is concentrated among labor markets with more generous UI benefits.

TABLE 7
Alternative Measures of UI Generosity

			Decomposed Property Crimes			
	Property (1)	Violent (2)	Burglary (3)	Larceny (4)	Motor Vehicle Theft (5)	Robbery (6)
Panel A: Expected UI generosity						
Δper-worker import competition	0.024***	0.014	0.012	0.025***	0.042*	0.015
	(0.008)	(0.011)	(0.012)	(0.009)	(0.023)	(0.016)
(Δper-worker import competition) × Expected UI	−0.019**	0.001	−0.023**	−0.013	−0.041*	−0.040***
	(0.009)	(0.013)	(0.009)	(0.009)	(0.022)	(0.015)
First stage F-statistic (imports per worker)	89.144	88.685	88.779	89.094	87.124	77.488
First stage F-statistic (imports per worker × Exp. UI)	33.560	33.223	33.494	33.429	33.247	28.578
Kleibergen−Paap joint F-statistic	9.841	9.746	9.774	9.727	9.606	6.853
Observations	3,025	2,966	2,969	2,998	2,824	1,846
R-Squared	0.269	0.221	0.426	0.154	0.199	0.269
Panel B: Average weekly benefits						
Δper-worker import competition	0.029***	0.016	0.019	0.029***	0.055**	0.031**
	(0.008)	(0.014)	(0.012)	(0.010)	(0.022)	(0.014)
(Δper-worker import competition) × UI generosity	−0.017***	−0.007	−0.020***	−0.011*	−0.043***	−0.049***
	(0.006)	(0.012)	(0.006)	(0.006)	(0.015)	(0.011)
First stage F-statistic (imports per worker)	117.848	116.797	117.663	117.681	116.087	114.339
First stage F-statistic (imports per worker × UI gen.)	110.232	108.948	109.988	109.436	110.712	84.830
Kleibergen−Paap joint F-statistic	37.614	37.624	37.391	37.127	36.831	24.434
Observations	3,025	2,966	2,969	2,998	2,824	1,846
R-Squared	0.266	0.232	0.425	0.148	0.204	0.273

Notes: Robust standard errors (clustered at the state level) reported in parentheses. Change in per-worker import competition is instrumented following Equation (2). Expected UI is calculated by multiplying start-of-period UI generosity by the share of weeks claimed that were actually compensated. Average weekly benefits comes directly from the Department of Labor reports. All underlying UI data come from the start of period (either 1990 or 2000 A one unit change in per-worker import competition represents a 1,000 dollar per-worker increase. All regressions include year fixed effects and Census region fixed effects and manufacturing share in 1990. Regressions also include the following demographic variables (measured in 1990): income per capita, share of population with a college degree, share of female population in the labor force, share of population under the age of 25, foreign-born share, black share, and Hispanic share. Finally, we also include the following county-level fiscal controls (measured in 1987): per capita police expenditures, per capita revenue transfers from other governments, per capita welfare expenditures, total expenditures per capita, and total revenue per capita. All regressions are weighted by county-population in 1990. Because our outcome is the change ln(crime rates) note that observations may vary from crime to crime if a county experiences no crimes in one of the reported years.

$^{*}p<.10$. $^{**}p<.05$. $^{***}p<.01$.

the number of weeks claimed and the number of weeks compensated. We are thus able to construct a measure of expected benefits that multiplies our measure of UI generosity by the probability of receiving benefits, which we measure as the ratio of weeks compensated to weeks claimed. These results are presented in the top panel of Table 7. These results are nearly identical to our main results. This suggests that our results are not being driven by variation in the leniency, which is perhaps not surprising since the workers displaced by increased in import competition were likely able to point toward a verifiable reason for their job loss.

In the bottom panel of Table 7, we use the average unemployed worker's weekly benefit as our measure of generosity. This is similar in spirit to our primary specifications, as well as the above measure of expected benefits, but employs observed payments as a measure of UI generosity, rather than policy itself. This alleviates concerns that our primary measure of UI generosity may over or understate actual UI payments in way that is correlated with the measure itself. This would be the case, for instance, if workers were systematically less likely to exhaust benefits in high or low UI generosity states. Reassuringly, using this alternative measure we find results that are qualitatively identical to our main results. This suggests that UI generosity mitigated the rise in crime by providing additional resources to unemployed workers.

V. VALUING THE POSITIVE EXTERNALITY

The previous section illustrates that UI generosity offers an important buffer against the rise in crime that would have otherwise accompanied the increase in Chinese import competition. Of course, unemployment programs are not designed for the specific purpose of reducing crime; thus the previous results highlight a positive externality of the UI program. This begs the question: to what extent did this additional UI generosity "pay" for itself by reducing crime during this period?

As a starting point for our cost–benefit analysis, we convert the changes in crime rates into actual crimes. Table 3 indicates that, for every $1,000 increase in imports per worker, a standard deviation increase in UI generosity would lower the trade-induced effect on crime by 2.5% for burglaries, 1.5% for larcenies, 3.8% for motor vehicle thefts, and 2.4% for robberies. This translates into 25.4 fewer burglaries, 40.4 fewer larcenies, 9.6 fewer motor vehicle thefts, and 1.4 fewer robberies for the average county.[25]

Next, we turn to existing literature to assign a monetary benefit to crime prevention. Our estimates of the social cost of crime come from Cohen and Piquero (2009). Cohen and Piquero provide two distinct measures of the cost of crime: total cost and willingness to pay. Total cost includes costs to the victim, criminal justice costs, and lost productivity of offenders who are incarcerated. This, of course, ignores many important costs of crime, such as fear, decreased social cohesion, and individual actions taken to avoid crime. Thus, the authors also provide a measure of willingness to pay in order to avoid crime, which is based on contingent valuation survey methodology. Both measures are consistent with the larger criminology literature (see McCollister, French, and Fang 2010 and citations therein). Cohen and Piquero (2009) estimate the average total cost of a burglary as $5,000, while the public would be willing to pay $35,000 to prevent an additional burglary. For larceny, the total cost is $2,800 and the willingness to pay estimate is $4,000. The total cost estimate for motor vehicle theft is $9,000 and the willingness to pay estimate is $17,000. Lastly, the total cost of a robbery is estimated at $23,000 while the willingness to pay estimate is $39,000. All estimates are in 2007 U.S. dollars. By applying these values, a standard deviation increase in UI generosity generates a social benefit ranging from $1.07 to $2.56 million per year for the average county.

To put these benefits into perspective, it is important to understand the costs associated with increasing UI generosity. We measure UI generosity as the product of the maximum weekly benefit and maximum duration, which can be interpreted as additional dollars over a full unemployment spell. In our sample, a one standard deviation increase in generosity corresponds to $1,549, also in 2007 U.S. dollars. Assuming an unemployment rate of 5%, increasing UI generosity by one standard deviation for an average-sized county would cost just over $6.5 million. We next scale this figure by 40% to reflect Gruber's (2010) estimate of the efficiency loss associated with the necessary increased taxation to fund the increase in UI generosity. This leaves us with a total cost of $9.1 million. This figure may be an upper bound for two important reasons. First, not all recipients will fully exhaust their unemployment benefits. Second, for the subset of unemployed workers whose base period earnings are too low to qualify for the current maximum weekly benefit, any increase in unemployment generosity will not generate additional expenses for the government. Nevertheless, setting these two caveats aside, our results suggest that 11% to 28% of the costs associated with increasing unemployment generosity are recovered in the form of reducing criminal activity.

VI. CONCLUSION

A growing literature has shown that the consequences of increased import competition are substantial. Increased import competition not only brings about substantial reductions in wages and employment in the most affected labor markets, but is also accompanied by a host of secondary effects, including rising crime, deteriorating housing markets, lower quality public good provision, worsening health outcomes, and increased political polarization.

Exploiting variation in UI generosity across states and time, we ask whether labor markets with access to more generous UI benefits fared better than labor markets with less robust assistance when exposed to increased import competition from China. Taking property crime as our outcome variable, we find that for the average county, a $1,000 per-worker increase in import competition raised the property crime rate by approximately 2.7%. However, in counties with

25. To generate these values we multiply the above percent changes by both the average change in import competition ($1,831) and the average county-level crimes. We obtain average crime counts by multiplying the average crime rates by the average population. Average crime rates are as follows: 6.61 burglaries per 1,000 residents, 17.53 larcenies per 1,000 residents, 1.65 motor vehicle thefts per 1,000 residents, and 0.38 robberies per 1,000 residents. The average county has 83,932 residents.

UI generosity 1.4 standard deviations above the mean, the net effect on crime was completely mitigated. Drawing on economic valuations of the cost of crime, we find that roughly 11%–28% of the costs of increasing UI generosity were recovered as a social benefit to society. This highlights a previously undocumented positive externality of UI, and suggests that government programs can in fact serve as a buffer against the consequences of trade-induced job loss.

While these estimates indicate that the associated reduction in crime went a long way toward "paying" for the increase in UI generosity, there is an important caveat worth addressing. We are unable to say with certainty whether UI generosity mitigated the rise in crime through direct channels (e.g., extending income support to workers displaced by trade) or through indirect channels (e.g., stabilizing the broader local economy). Disentangling these two channels would require data on individual-level UI payments as well as crime data tabulated by "trade displaced worker" status. We are unaware of any datasets that include such rich information. For this reason, we hesitate to claim that direct transfers for displaced workers are an effective policy tool. Research that can better disentangle these two mechanisms would be useful.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Figure S1. Two-way density of change in imports per worker and unemployment insurance (UI) generosity.

Table S1. Ordinary least squares estimates of the relationship between changes in ln(Crime rates) and changes in import competition.

Table S2. Reduced form estimates of changes in ln(Crime rates) as a response to our instrument for changes in import competition.

Table S3. Interacting other state-level policies with import competition.

Table S4. Interacting other county-level characteristics with import competition.

Table S5. Adding state-by-year fixed effects to baseline.

Table S6. Analyzing changes between 1990 and 2010.

Table S7. Results fixing UI generosity in 1990.

Table S8. Results considering 1990–2000 changes only.

Table S9. Results considering 2000–2007 changes only.

Table S10. Results aggregating to the commuting zone.

Table S11. Results analyzing the impact of trade on crime.