Do In-Kind Grants Stick?

The Department of Defense 1033 program and local government spending*

Donald J. Bruce, Celeste K. Carruthers, Matthew C. Harris †, Matthew N. Murray and Jinseong Park

University of Tennessee‡

July 31, 2017

Abstract

The U.S. Department of Defense 1033 program transfers decommissioned military goods to local police departments. This is one of the largest grant-in-kind initiatives in the country's history, accounting for over $5.2 billion in transferred goods and vehicles since 1997. Two features of this program are unique among intergovernmental grants, each working against the tendency to let grants supplant local resources: goods from the 1033 program are less directly fungible than monetary grants, and their acquisition entails little to no oversight by officials outside of law enforcement. While previous research shows that intergovernmental grants crowd out a large or equivalent degree of local spending, we find no evidence of crowd-out in the wake of 1033 acquisitions. The features of this program may therefore be useful when designing grants to increase local spending in a targeted category, but welfare is likely tempered by the absence of local oversight.

JEL classification: TBD

Keywords: Grant in-kind, Flypaper, Crowding Out

---

*We sincerely thank the Defense Logistics Agency, including Carlos Torres, Ken MacNevin, Michelle McCaskill and Susan Lowe, for their generosity with their time and for educating us on the institutional details and context of the 1033 program necessary for identification. We also thank Alan Barreca, Charles Stoecker, Robert Schwab, and seminar participants at Tulane, the National Tax Association, and Northern Illinois for their careful comments. All errors are our own.

†Corresponding Author: 722A Stokely Management Center, 916 Volunteer Boulevard, Knoxville, TN 37996-0570; mharris@utk.edu, (865) 974-5591

‡Haslam College of Business, Department of Economics and Boyd Center for Business and Economic Research.

I Introduction

Intergovernmental transfers are a key source of funds to local governments. The National League of Cities reports that approximately 5 percent of municipal budgets comes through the federal government, while an additional 30-40 percent of local government revenues comes from states.[1] The motivation for and structure of grant programs are both important because this external funding will affect the allocation of local public resources, overall local spending, and subsequent welfare. In some cases grants may be used to redistribute resources from high-income to low-income areas, in turn addressing differentials in local tax capacity. In other instances, local government provision of a public good may be preferable to state or federal provision, either due to diseconomies of scale or heterogeneity in preferences between local jurisdictions. In these circumstances, providing grants to local governments for the provision of a particular service may generate more welfare than the federal government providing the service directly. Grants are also widely used to address interjurisdictional spillovers (as with transportation infrastructure), and they may serve as a lever for paternalistic oversight from higher levels of government in a federalist system. If local preferences lead to too little spending on a public good, state or national agencies may choose to encourage more provision through grants.

The underlying motivation for a grant matters because it determines how the grant should be structured to maximize welfare. At the heart of this issue is the extent to which grants from higher levels of government replace, or crowd out, lower-level government spending. For example, if the goal is simply to redistribute income, a lump sum grant would yield the largest welfare gain to local citizens. Such a grant, however, could crowd out a substantial degree of funds that local budget-setters would have committed in the absence of external support. Thus, if the impetus for a grant is to correct the under-provision of a public good or to elicit spillovers, the grant should be designed to minimize crowd-out and promote stickiness. In this case, crowding out is undesirable and may defeat the objectives of the grant program.

Intergovernmental grants typically crowd out some degree of recipient spending no matter how grant funds are earmarked, which is consistent with theoretical expectations outlined by Bradford and Oates (1971). For example, Lutz (2010) examines the fiscal consequences of New Hampshire's

[1] http://www.nlc.org/revenue-from-intergovernmental-transfers

1999 school finance reform and shows that the tax burden of local residents falls by 90 cents per grant dollar. Lutz attributes this finding to the context surrounding the reform, including the state's direct democracy that allows for perfect reflection of median voter preferences, perfect information on the part of the voters regarding the reform, fiscal autonomy of the state, and fungibility of the grants. Crowding out is rarely perfect in settings that have been studied: most of the literature on intergovernmental grants shows that some share of grant proceeds translates into higher spending, a result dubbed the "flypaper effect" (Hines and Thaler, 1995). The extent of crowd out has been studied for a number of funding sources and funding intentions, including matched Title I education grants (Gordon, 2004; Cascio et al., 2013), health care (Baicker and Staiger, 2005), highway funding (Knight, 2002), and law enforcement (Baicker and Jacobson, 2007; Evans and Owens, 2007). A related literature describes agency or grant features that make income equivalence unlikely, usually highlighting institutional features that differ from the conditions considered by Bradford and Oates (1971). See, for example, Filimon et al. (1982), Strumpf (1998), Payne (2009), Brooks et al. (2011), Glaeser (2012).

While much is known about the effect of intergovernmental funding transfers on recipient spending, the literature is much quieter, to date, on the effect of intergovernmental grants in-kind. There are numerous examples of in-kind transfers from the public sector directly to individuals, such as food assistance, health care, and housing vouchers (Tabor, 2002; Currie and Gahvari, 2008). Such transfers have been shown to raise targeted consumption (i.e., supplement would-be spending in target areas) but perhaps not to the same extent as cash (Cunha, 2014) and not without some substitution between private purchases and government provision (Gruber and Simon, 2008). In a related vein of research, Carruthers and Wanamaker (2013) detect an incomplete flypaper effect arising from a pre-War school building campaign in the segregated South, which amounted to a series of large capital transfers from private philanthropies to public school districts. The question of whether intergovernmental in-kind transfers crowd out local government spending, however, is open and unstudied. Examples of in-kind transfers from one level of government to another include the Morrill Act establishing land-grant universities, emergency response equipment provided to local governments following natural disasters, and the Department of Defense 1033 program under examination here.

We contribute to our understanding of the public finance implications of external grants by exploring local effects of the federal 1033 program, which provides surplus military gear and vehicles to local governments. This application is the first to our knowledge that investigates the fiscal impacts of intergovernmental grants taking the form of goods rather than income. Since 1997, Section 1033 of the National Defense Authorization Act has allowed for the transfer of surplus or decommissioned U.S. military equipment to local law enforcement agencies at a nominal price of zero. Decommissioned capital initially used to provide national defense is re-purposed for the provision of public safety. From 1997 through 2014, the department of defense transferred over \$5.2 billion in equipment to local law enforcement agencies, making it the largest federal-to-local grant of capital goods of which we are aware.

The 1033 program offers a unique context in which to examine the effects of intergovernmental grants; in short, this setting is one where we would expect little to no crowding out. Indeed, transfers through the 1033 program are not intended to modify local spending, adjust for local preferences, or to transfer resources from rich to poor. Items received through the 1033 program are physical goods and are thus less fungible than cash. They cannot be sold or transferred to other local governments. Crowding out is still possible, of course, and could manifest as intra-agency substitution within or across functions related to 1033 equipment. This is most likely to arise for in-kind goods that have close substitutes within the local government's public safety budget. Additionally, decisions to acquire items through the 1033 program are made solely by police chiefs, typically without local institutional oversight, public input, or even a signature from a city or county government official. The preferences of law enforcement executives are thus pivotal and may or may not reflect those of other bureaucrats or the voting populace. As a result, the median voter is less relevant to the acquisition of goods. The opacity of the process implies that city or county officials with presumptive budgetary authority may have incomplete information about the amount of equipment that police chiefs acquire through the 1033 program. These features of the 1033 program are the inverse of the New Hampshire school finance reform that resulted in almost complete crowd out (Lutz, 2010). Accordingly, we expect that 1033 transfers will result in little to no crowding out.

To empirically evaluate the relationship between 1033 program receipts and local public spending on police protection, we use panel data on county expenditure accounts from the Annual Survey of Governments matched to the value of 1033 equipment transfers from the Defense Logistics

Agency. Exploiting within-county intertemporal variation in 1033 acquisitions over time, we find that the value of 1033 receipts has no significant effect on local spending (i.e., a complete flypaper effect with no crowding out and no crowding in). In our baseline specification, the effect of 1033 acquisitions on local police spending is a relatively precise zero. Confidence intervals imply that a one percent increase in 1033 transfers affects police spending in the following year by an absolute value of no more than 0.02 percent. In no specification do we find statistically significant evidence that receiving goods through the 1033 program reduces spending. The robust lack of crowding out of intergovernmental transfers stands in sharp contrast to nearly all of the related grant literature.

While grants through the 1033 program are much less likely to reflect local voter preferences than other intergovernmental transfers (Knight, 2002), local police spending might be correlated with other time-varying unobserved factors (e.g., preferences of police chiefs, severity of departmental budget constraints, voter preferences, perhaps, or time-varying heterogeneity in law enforcement leadership). To address concerns about omitted variables driving both police budgets and 1033 receipts, we achieve causal identification by exploiting exogenous variation in transaction costs faced by departments when acquiring 1033 items as in Harris et al. (2017). Instrumental variables estimates are less precise, but estimated coefficients for the effect of 1033 receipts on local police expenditures are positive, nearly significant, and more consistent with crowding in rather than crowding out.

Within the limitations of our data, we examine the possibility that null results mask heterogeneous effects by county type or equipment. Acquiring items through the 1033 program may raise spending (i.e., crowd in additional resources) if the items in question are not regular purchases and require complementary inputs. Of the various types of equipment considered, vehicles are the largest and most likely to fit this description. A tactical truck, for example, may require a new storage facility, specialized training, and unforeseen maintenance. To examine this possibility, we divide 1033 receipts into vehicle and non-vehicle items. Although estimated coefficients are positive, favoring crowding in, we do not find statistically significant evidence for either category of goods. Additionally, we might expect the effects of 1033 receipts on local police spending to vary by areas' fiscal or social conservatism. To evaluate this possibility, we divide the county panel according to political leanings in the 2008 presidential election. We find no evidence of heterogeneous effects by political leaning. We also split the sample into counties with above or below median population to examine whether crowding in is more or less prominent in smaller counties. We find statistically significant crowding in among smaller counties, but the precision of this result is not robust to changes in the sample or means of addressing time-varying heterogeneity.

The absence of crowding out is likely attributable to the design of the 1033 program rather than special features of law enforcement. Evans and Owens (2007) and Baicker and Jacobson (2007) examine intergovernmental financial transfers to law enforcement and find significant but partial crowding out, in accord with the body of research on intergovernmental grants. The 1033 program transfers capital goods, often large and rarely purchased by agencies on their own, with decision makers positioned at the end-user department rather than higher levels or inter-departmental levels of local authority. On the surface, the absence of a significant crowd-out (or crowd-in) effect of 1033 transfers on local spending can be viewed as a positive outcome, given that the program is not designed to replace or amplify local spending. More importantly, the program-specific circumstances offer lessons for how intergovernmental transfers could be designed to minimize crowd out. In cases where higher levels of government wish to increase overall local spending for one reason or another, policy makers might look to the structure of the 1033 program for guidance. However, we caution that the welfare properties of such a scheme are unclear.

II Relevant Background on the 1033 program

The 1033 program was created as a part of the National Defense Authorization Act of 1997. The stated purpose of section 1033 was to enable the Department of Defense to transfer military equipment no longer in use to local Law Enforcement Agencies (LEAs) to assist in drug interdiction. While the 1033 program has garnered considerable attention for transferring tactical equipment (e.g., assault rifles and armored personnel carriers) to local law enforcement, it has also facilitated transfers of clothing, ice chests, first aid kits, flashlights, etc. In actuality, over 70 percent of the items transferred were of a non-tactical nature. The 1033 program was not designed to be a grant-in-kind program, but rather a well-intentioned recycling program that takes items that are no longer useful for national defense and reallocates them to the production of public safety.

Several features of the 1033 program stand in sharp contrast to most intergovernmental transfers. First, the items transferred are relatively non-fungible goods of a very specific nature.

The Defense Logistics Agency (DLA) forbids transferring items acquired through the 1033 program to other agencies.[2] Second, there is very little, if any, administrative cost to participating in this program. There is a simple two-page form to register as a receiving agency. There is then a one page form to request non-tactical items, a one page form to request an armored vehicle, a one-page form to request an aircraft, and so forth. The highest-ranking signature on these forms is that for the chief of police or a law enforcement officer of similar rank. Unlike Title 1 grants, for example, these transfers are not overseen or administered by the state.[3] Similarly, there is no oversight at the local level. Therefore the acquisition process does not operate under the auspices of county or municipal governments who make funding decisions, nor is there any other form of public oversight. Rather, 1033 acquisitions may occur with or without the knowledge of the local budgetary authority and likely without the knowledge of voters. While federal monetary grants to local governments must be accounted for and therefore included in the budgetary process, that is not the case with the 1033 program.[4]

These features create conditions under which transfers can have an ambiguous effect on local budgets for law enforcement. If local voters and elected officials are aware of equipment transfers through the 1033 program, they may choose to reduce police budgets in subsequent years as is the case with receipts from civil asset forfeitures (Baicker and Jacobson, 2007). This would be especially likely for transfers that are a close substitute for items that could be acquired from private sector vendors. Alternatively, if voters and elected officials are (relatively) unaware of 1033 transfers, these transfers may have no effect on police budgets. However, there are at least two mechanisms by which transfers through the 1033 program may actually increase police budgets, or lead to crowding in. First, many state programs contain clauses that equipment must be used in some form, or returned to the DLA. While these clauses are essentially toothless (no burden of proof is required), they would give law enforcement officials leverage to request budget increases. Second, for certain sorts of equipment there may be a complementary inputs effect, where the items cannot be engaged to produce public safety without additional inputs. Therefore, the receipt of

[2] For example, a police department cannot request utility trucks and gift them to the county ambulance service.

[3] While each state does have a coordinator, the function of this role is to facilitate communication rather than control or coordinate the use of funds or gather data.

[4] The field on the ASG questionnaire explicitly asks respondents to exclude depreciation and other capital asset accounting from their reported expenditure figures. The survey is designed to capture operating costs rather than the depreciation of buildings or in this case, military surplus.

items through the 1033 program increases the marginal returns to discretionary resources allocated to police departments, thus justifying higher spending until net marginal benefits equalize across funding categories.[5]

II.A Program Logistics and the Role of Land Area and Proximity

Due to the unique structure of the 1033 program, receipts through this program are less vulnerable to the sorts of endogeneity concerns first addressed by Knight (2002). Nevertheless, we adopt a fixed effects instrumental variable (FE-IV) specification relying on the same identification strategy as Harris et al. (2017).

We rely on two sources of exogenous variation. First, the amount of equipment available for distribution through the 1033 program varies exogenously over time. Harris et al. (2017) show that distribution of tactical items increased sharply in 2006 when the M-16 was replaced by the M-4 carbine as the standard issue weapon for the Army and Marine Corps. After 2009, the draw down from Iraq and Afghanistan exogenously increased the amount of equipment of all types available by an order of magnitude. More generally, the supply of equipment is determined by national military spending and need rather than any facet of local law enforcement. Second, law enforcement agencies face time-invariant, county-specific exogenous differences in the logistical costs of acquiring said items. The interaction of these two factors (variation in availability and transaction costs) yields variables that affect the cost of acquiring goods through the 1033 program but are uncorrelated with bureaucratic and voter preferences for public safety or other unobservable factors that determine police budgets. While we refer the reader to Harris et al. (2017) for an in-depth discussion of these instruments, we present an abridged version below to frame this application.

The DLA explicitly states that, particularly for vehicles, preference will be given to jurisdictions with larger land areas. There is a field for "land area" on the 1033 vehicle request form. Counties whose law enforcement officers have more ground to cover will likely gain greater benefits from the use of vehicles. This is an important source of variation, given that vehicles account for approximately half of the total value of goods distributed through the program. While DLA

[5] For example, if a police department receives a large military vehicle, they may need a storage facility and a diesel mechanic. With these inputs in place, the vehicle may contribute substantially toward public safety; without the complementary inputs, the vehicle will simply sit idle. The same analogy can be drawn for guns requiring ammunition and training time, or non-tactical items requiring storage space.

relayed that they were able to meet the needs of local enforcement agencies over time (meaning there was no long-term two-way selection problem), land area was a consideration in determining which claims were prioritized at a given point in time. The interaction of land area with total value of equipment distributed through the 1033 program in a given year serves as our main instrument in the empirical specification. Additionally, local LEAs were responsible for their own costs in identifying, evaluating, and acquiring equipment through the 1033 program. There are 18 Field Activity Centers (FAC) that are in charge of distributing 1033 items. All decomissioned items, particularly those of a sensitive nature, are sent to the FAC nearest the military unit from which the decomissioned item originated for processing. LEAs that acquire 1033 items must pay for all transaction costs, including evaluation and shipping costs from the FAC where the decommissioned items are processed. All else equal, LEAs that are farther from a FAC will face higher evaluation and acquisition costs than LEAs in close proximity to a FAC. We therefore also use inverse distance to the nearest FAC as an instrument in some specifications and sensitivity analyses.

III Data

The main source of local finance data is the Census of Governments and the Annual Survey of Government Finances (ASG) from 2006 to 2014, collected by the U.S. Census Bureau. While the Census Bureau canvassed the universe of government units in 2007 and 2012, data for other years are based on voluntary-response surveys of the same government units. The data provide detailed information on the revenues and expenditures of different levels of government. We primarily focus on police expenditures of county governments since this is the finest unit of government for which all necessary data are available. Police protection expenditures comprise spending on "police patrols and communications, crime prevention activities, detention and custody of persons awaiting trial, traffic safety, and vehicular inspection."[6]

Although the ASG data are widely used in the public finance literature, there are some known issues that must be acknowledged. First, any zero values in ASG data can either represent a true zero or a missing figure; differentiating between the two is not possible. Fortunately, only 1.3 percent of the sample reported zero expenditure for police protection. Assuming that data

[6] https://www.census.gov/govs/local/definitions.html.

from counties that consistently reported zeros for police protection are valid, those dubious zeros account for less than one percent of the observations. Second, the county-year panel in the ASG data is not balanced. If survey participation decisions of governments are not random conditional on the control variables, our estimates would be biased. While we use the unbalanced panel as our primary analytical sample, we show in Appendices A and C that results are similar when using balanced panels.

Data on transfers of military equipment to local law enforcement agencies come from the DLA. The data contain information on agencies to which transfers were made, item name and corresponding National Stock Number (NSN), shipping date, quantity, and the acquisition value of the item. The data that we use in the analysis were last updated in September 2015 and coded at the individual agency level. Thus, we infer county information from the agency name and the state to which the agency belongs by combining DLA data with the Law Enforcement Agency Identifiers Crosswalk file created by the Bureau of Justice Statistics and the National Archive of Criminal Justice Data (NACJD). We first combine the two datasets using character-merge and then manually match observations that have low matching scores or cannot be matched automatically. Observations from agencies whose county information cannot be identified are dropped from the sample.[7]

We also collect information on county characteristics from other sources. Data on crime rates are obtained from the county-level Uniform Crime Reports, published by the Federal Bureau of Investigation and reproduced by the NACJD. Using agency-level data provided by the FBI, NACJD imputes missing data and aggregates the data by county.[8] We use aggregate counts of arrests for murder, rape, robbery, aggravated assault, burglary, and other assaults. The Census provides intercensal population estimates, which includes demographic information for counties. Using these estimates, we calculate the male population share, the share of population aged 15 to 24, and an index of racial diversity similar to one used by Alesina et al. (1999).[9] Data on household median income and unemployment rates are obtained from the Small Area Income and Poverty

[7]These counties include Berkshire County (MA), York County (PA), York County (VA).

[8]For more details regarding the imputation, refer to the NACJD's Uniform Crime Reporting Program Resource Guide (https://www.icpsr.umich.edu/icpsrweb/content/NACJD/guides/ucr.html).

[9]The racial diversity measure is defined as $diversity_{jt} = 1 - \sum_k race_{kjt}$, where $race_{kjt}$ represents the population share of a particular race k, where k consists of white, black or African American, American Indian and Alaska Native, Asian and Native Hawaiian/Other Pacific Islander, two or more races, in county j in year t.

Table 1: Summary statistics

	Unbalanced		Balanced	
	Mean	Std. Errors	Mean	Std. Errors
Lagged 1033 item value per capita ($)	0.24	(2.04)	0.19	(1.73)
ln(Lagged 1033 item value per capita) ($)	0.08	(0.34)	0.06	(0.30)
ln(Lagged 1033 vehicle item value per capita) ($)	0.06	(0.32)	0.05	(0.28)
Police expenditure per capita ($)	96.75	(103.49)	91.34	(76.33)
ln(Police expenditure per capita) ($)	4.29	(0.79)	4.24	(0.79)
Population (1,000)	130.42	(374.86)	189.13	(462.28)
Median income	45.45	(11.91)	47.73	(12.90)
Male (%)	49.80	(1.84)	49.57	(1.48)
Age 15 to 24 (%)	13.38	(3.40)	13.77	(3.58)
Diversity index (% point)	20.80	(15.82)	22.81	(15.76)
Poverty (%)	15.32	(6.39)	15.89	(6.38)
Unemployment rate (%)	7.00	(2.94)	6.90	(2.95)
Lagged murder per 100,000 population	1.17	(4.31)	1.03	(3.07)
Lagged rape per 100,000 population	2.34	(5.92)	2.09	(4.96)
Lagged robbery per 100,000 population	3.46	(8.77)	3.80	(9.17)
Lagged aggravated per 100,000 population	34.98	(60.83)	31.50	(51.80)
Lagged burglary per 100,000 population	30.18	(46.24)	27.22	(42.07)
Lagged assault per 100,000 population	116.76	(162.73)	112.16	(162.47)
Observations	17,539		10,998	

Note: The unbalanced panel consists of all county governments that at least once participated in the Annual Survey of State and Local Government Finances from 2006 to 2014 (including census years 2007 and 2012) and the balanced panel consists of county governments that fully participated in the survey. Both samples consist of county-level law enforcement agencies that are matched to county governments. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars.

Estimates of the Census Bureau and Bureau of Labor Statistics, respectively. Finally, we collect data on the 2008 U.S. presidential election from the Guardian.[10] The data contain the number of votes that each presidential candidate received by county.

Table 1 lists summary statistics. The first two columns show the mean and standard deviation of each variable in the unbalanced sample and the second set of columns show the same statistics for a balanced subset of our main panel. Note that lagged 1033 item values and police expenditures are expressed in per capita terms and crime rates are expressed per 100,000 population. All monetary values are adjusted for inflation using CPI-U-RS and given in thousands of logged 2012 U.S. dollars. The unbalanced panel contains about 30 percent more observations than the balanced panel. While statistics of most variables look quite similar across the panels, the average population size of the

[10]The data can be obtained from the following webpage (https://www.theguardian.com/news/datablog/2009/mar/02/us-elections-2008).

unbalanced panel is substantially smaller than the average population size of the balanced panel. Since small county governments have greater participation rates in the census years (2007 and 2012), the difference is not surprising. In the unbalanced panel, the average county spends about $96.80 per capita for police protection and receives 1033 items that amount to 24 cents per capita. While that number appears small, it is an artifact of irregular participation in the 1033 program. In the balanced panel, 61.3 percent of counties participated in the 1033 program in at least one year, whereas only 16.7 percent of counties received any 1033 item in a given year. Conditional on receipt, the average value of receipts in the unbalanced panel increases to 1.47 dollars per capita, or 1.5 percent of total police spending.[11]

IV Empirical Model

We estimate the following equation to evaluate the effect of 1033 item acquisition on police protection expenditures of county-level LEAs:

$$ln(protection_{j,s,t}) = \beta_0 + \beta_1 ln(values_{j,s,t-1}) + \mathbf{\Gamma} \boldsymbol{X_{j,t}} + \theta_j + \delta_s t + \varepsilon_{j,t} \quad (1)$$

where $ln(protection_{j,s,t})$ represents logged police protection expenditure of county j in year t and $ln(values_{j,s,t-1})$ represents a monetary value of items that the law enforcement agency of the government of county j received through the 1033 program in year $t-1$. Note that both $ln(protection_{j,s,t})$ and $ln(values_{j,t-1})$ are normalized to the county population and expressed in per capita terms. $\boldsymbol{X_{j,t}}$ is a vector of time-varying county-level characteristics including median household income, male population share, the share of the population aged 15 to 24, racial diversity index, poverty rate, and unemployment rate. The parameters θ_j and $\delta_s t$ are county fixed effects and state-specific time trends, respectively. We show that our results are robust to inclusion of year fixed effects in lieu of state-specific time trends for both the unbalanced and balanced panel samples (see Appendices B and C). Robust standard errors are clustered at the county level.

The fixed effects model presented in Equation 1 relies on within-county variation in the 1033 item values over time. We are mainly interested in the estimate for β_1, which captures how police protection expenditures of counties typically responded to the receipt of 1033 items. Under

[11] Conditional on receipt, the average police spending is $99.80 in the unbalanced panel.

the assumption that the error term $\varepsilon_{j,t}$ is not systematically correlated with 1033 item values after controlling for other variables, we can obtain an unbiased estimate for β_1. However, this assumption might not hold for several reasons. For example, Knight (2002) shows that endogeneity arises due to a correlation between grant receipts and unobserved tastes for public goods. In general, any correlation between time-varying unobserved factors that affect both public expenditures on public safety and the value of receipts through the 1033 program (e.g., motivation by law enforcement leadership to court additional resources) can lead to biased estimates. Recent work in the public finance literature has addressed this endogeneity very carefully.

To address concerns about endogeneity, we estimate the FE-IV model using a subset of instrumental variables that are also employed by Harris et al. (2017): most critically the interaction between total item value and the land area of a county (value-land interaction).[12] While we rely on institutional details from Section II to claim that these instruments are exogenous, we empirically evaluate their relevance.

V Results

We first report results from the FE models to establish a baseline descriptive relationship. We then present results from our preferred IV specification. Almost all specifications return a null result for 1033 item value, implying that these particular in-kind grants do not crowd out or crowd in local spending on law enforcement. Finally, we investigate whether null results mask heterogeneous treatment effects by county or equipment characteristics.

Table 2 contains the results of the baseline FE model. Estimated coefficients are generally close to zero with economically small confidence intervals, implying a perfect flypaper effect. Column (1) lists results from the baseline Equation 1 specification, and column (2) includes additional control variables for crime rates.[13] In both cases, the 95 percent confidence interval implies that a one percent increase in the value of receipts from the 1033 program leads to a change in local

[12] For specifications with year fixed effects in Appendices B and C, we use the inverse distance to the nearest field activity center as the cross-sectional component of our instrument.

[13] While adding crime variables to the baseline specification improves the precision of the estimates, it is unclear whether the best specification includes or excludes crime rates. While arguments for including crime rates as control variables are obvious, studies have also shown that police protection expenditures are endogenous to crime (Levitt, 1997; Di Tella and Schargrodsky, 2004). Therefore, results in (1) and (2) should not be viewed as a horse-race, but simply specification checks that our results are not sensitive to the inclusion/exclusion of crime rates as controls.

police expenditures between -0.012 and 0.004 percent. Because many of the items received through the 1033 program are durable capital goods, they may have budgetary effects beyond the next year, particularly if crowding in occurs due to the need for complementary inputs.[14] Column (3) evaluates this possibility by examining whether items received in period $t-2$ or $t-3$ affect contemporaneous spending on public safety. While only the third lag is statistically significant, all estimates are positive, reinforcing that we find no evidence of crowding out. Column (4) represents a specification that includes item values for year $t+1$ as a falsification test of the timing assumptions in our model. The estimate for the lead term is insignificant, indicating that pre-existing trends are not of primary concern in our setting. Columns (5) and (6) demonstrate that results are not sensitive to alternative ways of controlling for time-varying heterogeneity, such as year fixed effects and the inclusion of both state-specific time trends and year fixed effects. While we find no evidence of crowding out, confidence intervals indicate that if 1033 receipts do crowd out local police spending, they do so with an elasticity less than 0.02.

While the institutional features of the 1033 program make sources of endogeneity discussed by Knight (2002) less likely, they do not eliminate all concerns about correlated unobservables that affect local spending on police protection and acquisition of items through the 1033 program. For example, if chiefs of police who participate in the 1033 program aggressively court resources from all sources (including the local government), our results will suffer from omitted variable bias. We therefore implement a fixed effects instrumental variables approach as described in Harris et al. (2017), the results of which are found in Table 3. Panel A of Table 3 contains the results of the first-stage estimation, and Panel B contains FE-IV estimates of the effect of 1033 receipts on local police spending. For all specifications in Table 3, we use land area as a time invariant characteristic, interacted with the nationwide value of goods released through the 1033 program as an instrument for the value of 1033 receipts.[15]

Specifications in columns (1) and (2) are the same as columns (1) and (2) in Table 2, but with instrumental variables predicting 1033 item value. First stage F-statistics on the exclusion restrictions are greater than 20, which far exceeds the benchmark value of 10 (Staiger and Stock,

[14] For example, if a department acquires assault rifles, it will need additional funding for ammunition to train/qualify officers beyond the next calendar year.

[15] We again refer readers to Harris et al. (2017) for additional supporting evidence on the validity of these instruments.

Table 2: The effects of 1033 program on police spending: FE model

	(1)	(2)	(3)	(4)	(5)	(6)
$ln(Item\ value\ per\ capita\ _{t-1})$	-0.004	-0.004	0.009		0.001	-0.002
	(0.008)	(0.008)	(0.008)		(0.009)	(0.008)
$ln(Item\ value\ per\ capita\ _{t-2})$			0.005			
			(0.013)			
$ln(Item\ value\ per\ capita\ _{t-3})$			0.031^{**}			
			(0.014)			
$ln(Item\ value\ per\ capita\ _{t+1})$				0.002		
				(0.010)		
County characteristics	✓	✓	✓	✓	✓	✓
Crime controls		✓	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓		✓
Year fixed effects					✓	✓
Observations	17539	17539	9069	13101	17539	17539
R^2	0.098	0.098	0.053	0.110	0.046	0.102

Note: This analysis uses the unbalanced panel described in Table 1. The dependent variable is logged police protection expenditure of county governments per capita. We control for county-level time-varying characteristics such as median household incomes, male shares, shares of the population aged from 15 to 24, racial diversity, poverty rates, and unemployment rates in all specifications. In column (2) to column (6), we add arrest rates for the following crime types: murder, rape, robbery, aggravated assault, burglary, and assault. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

1997). While coefficients are larger in magnitude and less precise than those from reduced form fixed effects, they are positive (favoring crowding in) and nearly significant (p-value ≈ 0.11). Columns (3) and (4) address concerns about whether results are driven by counties that are highly likely or unlikely to participate at the extensive margin. To do so, we estimated a propensity score for a county's participation in the 1033 program.[16] Column (3) contains results from a sample restricted to counties with a greater than 10 percent estimated propensity to participate. Column (4) also excludes counties with a less than 15 percent estimated propensity to participate. While these sample restrictions decrease the magnitude of estimates, they further support the inference that receipts from the 1033 program do not crowd out county expenditures on law enforcement.

[16]To obtain propensity scores, we estimate a logit model where the dependent variable is a binary indicator that takes 1 if a county received any 1033 item in a given year and 0 otherwise. The model controls for county population as well as all covariates included in the column (2) of Table 2. The average propensity score in the unbalanced sample is 0.188.

Table 3: The effects of 1033 program on police spending: FE-IV model

	(1)	(2)	(3)	(4)
Panel A: First-stage				
$ln(Value_{t-1} \times Land_j)$	0.023***	0.023***	0.022***	0.021***
	(0.005)	(0.005)	(0.006)	(0.007)
Panel B: Second-stage				
$ln(Item\ value\ per\ capita\ _{t-1})$	0.346	0.346	0.085	0.088
	(0.217)	(0.223)	(0.240)	(0.301)
Kleibergen-Paap LM statistic	25.584	24.613	14.921	9.329
Kleibergen-Paap Wald F statistic	25.774	24.773	14.986	9.360
Endogeneity Test	0.090	0.097	0.728	0.791
Sample	Full	Full	$0.1 \leq p$	$0.15 \leq p$
County characteristics	✓	✓	✓	✓
Crime controls		✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	17532	17532	13627	10618

Note: Column (1) and column (2) use the full unbalanced sample. Column (3) and column (4) restrict the sample based on the likelihood of receiving any items through the 1033 program, p. Panel A summarizes the first-stage estimates where the dependent variable is the value of items that a county-level agency has received through the 1033 program. $Value_{t-1}$ is the total value of 1033 items released by the Department of Defense in a year $t-1$. $Land_j$ is the land area of county j. Panel B summarizes the second-stage results where the dependent variable is logged police protection expenditure of county governments per capita. The Kleibergen-Paap Wald F statistic shows the relevance of the instruments. The Kleibergen-Paap LM statistic is used to test whether a model is under-identified. We also test the null hypothesis that the item value can actually be treated as exogenous using a GMM distance test proposed by Baum et al. (2007) and present the results. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

V.A Heterogeneous effects of the 1033 program

We show in the previous section that grants-in-kind through the 1033 program do not crowd out subsequent local police spending. However, our null results may mask significant heterogeneous effects either by equipment type or subsample. For example, it is possible that certain ordinary items may result in substantial crowding out, while vehicles may require additional funds for maintenance and operation resulting in crowding in. Alternatively, if acquiring resources through the 1033 program gives chiefs additional political capital in the budget process, we may expect crowding-in effects to be more or less pronounced in counties with greater fiscal conservatism, for which we use the county's vote in the 2008 presidential election as a proxy. Finally, the size of the county may also lead to heterogeneity in the effects of 1033 receipts. On one hand, larger counties may find it easier to shift resources to provide complementary inputs. Alternatively, the moving of resources between budget areas may simply be more visible in smaller counties.

To investigate heterogeneous effects by equipment type, we split the value of receipts into two categories: vehicle and non-vehicle. While it is not always clear which items will require complementary inputs, vehicles are the most likely candidates. In the absence of additional funds for storage, fuel, maintenance, and possibly personnel who can operate and maintain military vehicles, these items will not be productive inputs for public safety.[17] Table 4 summarizes estimation results from a specification identical to column (1) in Tables 2 and 3, except that the key covariate in this analysis is either the lagged values of vehicle items or the lagged values of non-vehicle items. Results imply that neither lagged 1033 vehicle nor non-vehicles values lead to crowding out in local police spending. The FE-IV estimates for both vehicles and non-vehicles, shown in columns (2) and (4) are both positive for vehicles and non-vehicles, but much larger and nearly significant for vehicles. While results are somewhat consistent with a complementary inputs story for vehicles, they are ultimately too weak to be conclusive.

In Table 5, we question whether the fiscal impact of the 1033 program on local police expenditures depends on the political preferences of counties. We divide the sample into two groups, Democrat and Republican, based on the 2008 U.S. presidential election votes. If the number of votes for the candidate from the Democratic party is above the median in our balanced panel sample, the county is considered as Democrat. Otherwise, the county is considered as Republican. We repeat both FE and FE-IV specifications on each subsample. Results in Table 5 do not suggest heterogeneous effects by party affiliation. For both subsamples, baseline results are a relatively precise zero, and IV results are weakly positive but imprecise.

Finally, we investigate heterogeneous effects of 1033 receipts on local police spending by the size of the county. We split the sample into large counties and small counties based on the population size and estimate FE and FE-IV specifications. Results are summarized in Table 6. Similar to prior

[17] We define an item as a vehicle if its FSG is 15 (aircraft and airframe structural components), 16 (aircraft components and accessories), 17 (aircraft launching, landing, and ground handling equipment), 19 (ships, small craft, pontoons, and floating docks), 20 (ship and marine equipment), 23 (ground effect vehicles, motor vehicles, trailers, and cycles), or 24 (tractors). Non-vehicle item values are obtained by simply subtracting the vehicle item values from the total item values at the agency level.

Table 4: The heterogeneous effects of 1033 program on police spending: by item types

	(1)	(2)	(3)	(4)
	Vehicle		Non-vehicle	
	FE	FE-IV	FE	FE-IV
$ln(Vehicle\ item\ value\ per\ capita\ _{t-1})$	0.001	0.590		
	(0.007)	(0.391)		
$ln(Non-vehicle\ item\ value\ per\ capita\ _{t-1})$			-0.049	0.174
			(0.033)	(0.256)
Kleibergen-Paap LM statistic		8.553		242.324
Kleibergen-Paap Wald F statistic		8.535		270.915
Endogeneity test		0.077		0.385
Observations	17539	17532	17539	17532

Note: We use the unbalanced panel and estimate the baseline equation (1) using both FE and FE-IV models. In column (1) and column (2), the dependent variable is the logged value of vehicle items that a county has received through the 1033 program. In column (3) and column (4), the dependent variable is the logged value of non-vehicle items. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

tables, FE estimates in column (1) and column (3) are a relatively precise zero, reinforcing the null result. FE-IV results actually imply that 1033 receipts lead to crowding in' among small counties, but are insignificant for larger counties. Larger counties may find it easier to move money between line items because of the breadth and size of the overall budget. Small counties are more likely to face sharper and potentially more visible tradeoffs in reallocating resources, and are therefore more likely to seek additional money from the county government. However, conclusions that 1033 acquisitions crowd in more spending in small counties are not robust to sample or specification changes in Appendicies B and C.

VI Discussion

We investigate the effect of grants-in-kind from the 1033 program on local expenditures for police protection. In sharp contrast to virtually all recent empirical literature on intergovernmental transfers, we find no evidence that receipts of 1033 goods crowd out local spending on public safety. In other words, we find evidence of a perfect flypaper effect. This lack of crowd out is robust to using unbalanced or balanced panel samples, year fixed effects, or a level-level, rather than a log-log specification. These specification checks are available in Appendices A-D.

Table 5: The heterogeneous effects of 1033 program on police spending: by political preferences

	(1)	(2)	(3)	(4)
	Republican		Democrat	
	FE	FE-IV	FE	FE-IV
$ln(Item\ value\ per\ capita\ _{t-1})$	-0.004	0.308	-0.010	0.522
	(0.009)	(0.242)	(0.012)	(0.494)
Kleibergen-Paap LM statistic		18.585		6.637
Kleibergen-Paap Wald F statistic		18.709		6.625
Endogeneity test		0.177		0.240
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	10741	10734	6798	6798

Note: We split the unbalanced panel into two groups based on political preferences of counties and estimate the baseline equation (1) using both FE and FE-IV models. The sample is restricted to counties whose Democrat vote shares for the 2008 U.S. presidential election are less than or equal to the median of the balanced panel ("Republican") in the first two columns and to counties whose Democrat vote shares are greater than the median ("Democrat") in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The stickiness of the 1033 grants may be attributable to unique features of the program. Unlike other intergovernmental aid programs, 1033 grants are provided in the form of less fungible, non-transferable capital goods and the take-up decision is made by a chief of police rather than local voters or budgetary personnel. The opacity of the process means that there is little if any public oversight associated with gear acquisition. While the stickiness of the grant program may be deemed positive when the goal of the grant is to increase the total amount of resources allocated for a public good in a specific category, the lack of local oversight raises questions regarding welfare consequences of the 1033 program.

Due to data limitations, we leave for future work the question of which specific features of the program are necessary to achieve perfect stickiness. Understanding which features (lack of transparency, weak fungibility, or application below the level of budget authority) leads to stickiness or crowd in would be of great value in designing future grants in contexts where crowd out is highly undesirable. Each such feature, however, leads to some inefficiency and reduces total welfare compared to lump-sum cash transfers. However, when either paternalistic motivations or

Table 6: The heterogeneous effects of 1033 program on police spending: by population size

	(1)	(2)	(3)	(4)
	Small		Large	
	FE	FE-IV	FE	FE-IV
$ln(Item\ value\ per\ capita\ _{t-1})$	-0.010	0.655**	0.006	-0.351
	(0.009)	(0.262)	(0.010)	(0.468)
Kleibergen-Paap LM statistic		19.807		7.703
Kleibergen-Paap Wald F statistic		19.945		7.700
Endogeneity test		0.002		0.432
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	10289	10282	7250	7250

Note: We split the unbalanced panel into two groups based on the population size of counties and estimate the baseline equation (1) using both FE and FE-IV models. The sample is restricted to counties whose population sizes are less than or equal to the mean of the balanced panel ("Small") in the first two columns and to counties whose population sizes are greater than the median ("Large") in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the desire to capitalize on positive externalities motivates the grant, programs that share some structural or administrative features of the 1033 program may be more effective in preventing the crowd out of intergovernmental transfers.

References

Alesina, A., R. Baqir, and W. Easterly (1999). Public goods and ethnic divisions. *Quarterly Journal of Economics 114:4*, 1243–1284.

Baicker, K. and M. Jacobson (2007). Finders keepers: Forfeiture laws, policing incentives and local budgets. *Journal of Public Economics 91*, 2113–2136.

Baicker, K. and D. Staiger (2005). Fiscal shenanigans, targeted federal health care funds, and patient mortality. *Quarterly Journal of Economics 120(1)*, 345–386.

Baum, C. F., M. E. Schaffer, and S. Stillman (2007). Enhanced routines for instrumental variables/generalized method of moments estimation and testing. *Stata Journal 7(4)*, 465–506.

Bradford, D. and W. E. Oates (1971). The analysis of revenue sharing in a new approach to collective fiscal decisions. *Quarterly Journal of Economics 85(3)*, 416–439.

Brooks, L. and J. H. Phillips (2008). An institutional explanation for the stickiness of federal grants. *Journal of Law, Economics, and Organization 26(1)*, 263–284.

Brooks, L., J. H. Phillips, and M. Sinitsyn (2011). The cabals of a few or the confusion of a multitude: The institutional trade-off between representation and governance. *American Economic Journal: Economic Policy 3(1)*, 1–24.

Carruthers, C. K. and M. H. Wanamaker (2013). Closing the gap? The effect of private philanthropy on the provision of African-American schooling in the US south. *Journal of Public Economics 101*, 53–67.

Cascio, E. U., N. Gordon, and S. Reber (2013). Local Responses to Federal Grants: Evidence from the Introduction of Title I in the South. *American Economic Journal: Economic Policy 5(3)*, 126–159.

Cunha, J. M. (2014). Testing paternalism: Cash versus in-kind transfers. *American Economic Journal: Applied Economics 6*(2), 195.

Currie, J. and F. Gahvari (2008). Transfers in cash and in-kind: Theory meets the data. *Journal of Economic Literature 46(2)*, 333–383.

Di Tella, R. and E. Schargrodsky (2004). Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack. *American Economic Review 94(1)*, 115–133.

Evans, W. N. and E. G. Owens (2007). C.O.P.S. and crime. *Journal of Public Economics 91*, 181–201.

Filimon, R., T. Romer, and H. Rosenthal (1982). Asymmetric information and agenda control: The bases of monopoly power in public spending. *Journal of Public Economics 17(1)*, 51–70.

Glaeser, E. L. (2012). Urban public finance. *NBER Working Paper Series w18244*.

Gordon, N. (2004). Do federal grants boost school spending? Evidence from Title I. *Journal of Public Economics 88*, 1771–1792.

Gruber, J. and K. Simon (2008). Crowd-out 10 years later: Have recent public insurance expansions crowded out private health insurance? *Journal of health economics 27*(2), 201–217.

Harris, M. C., J. Park, D. J. Bruce, and M. N. Murray (2017). Peacekeeping force: Effects of providing tactical equipment to local law enforcement. *American Economic Journal: Economic Policy Forthcoming*.

Hines, J. R. and R. H. Thaler (1995). The flypaper effect. *Journal of Economic Perspectives 9(4)*, 217–226.

Knight, B. (2002). Endogenous federal grants and crowd-out government spending: Theory and evidence from the federal highway aid program. *American Economic Review 92(1)*, 71–92.

Levitt, S. D. (1997). Using electoral cycles in police hiring to estimate the effect of police on crime. *American Economic Review 87(3)*, 270–290.

Lundqvist, H. (2015). Granting public or private consumption? Effects of grants on local public spending and income taxes. *International Journal of Tax and Public Finance 22(1)*, 41–72.

Lutz, B. (2010). Taxation with representation: Intergovernmental grants in a plebiscite democracy. *The Review of Economics and Statistics 92(2)*, 316–332.

Nunn, N. and N. Qian (2011). The potato's contribution to population and urbanization: Evidence from a historical experiment. *Quarterly Journal of Economics 126(2)*, 593–650.

Payne, A. A. (2009). Does government funding change behavior? An empirical analysis of crowd out. In J. R. Brown and J. M. Poterba (Eds.), *Tax Policy and the Economy, Volume 23*. University of Chicago Press.

Singhal, M. (2008). Special interest groups and the allocation of public funds. *Journal of Public Economics 92*, 548–564.

Staiger, D. and J. H. Stock (1997). Instrumental variables regression with weak instruments. *Econometrica 65(3)*, 557–586.

Strumpf, K. S. (1998). A predictive index for the flypaper effect. *Journal of Public Economics 69*, 389–412.

Tabor, S. R. (2002). Assisting the poor with cash: Design and implementation of social transfer programs. *World Bank Social Protection Discussion Paper 223*.

United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data: Arrests by Age, Sex, and Race, Summarized Yearly, 2005-2014. ICPSR04716-v2. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2009-09-28. https://doi.org/10.3886/ICPSR04716.v2.

A Appendix: Log-log specifications, balanced panel, state-specific time trends

Table A.1: The effects of 1033 program on police spending: FE model, balanced panel, state-specific time trends

	(1)	(2)	(3)	(4)	(5)	(6)
ln(Item value per capita $_{t-1}$*)*	0.010	0.009	0.018*		0.019	0.013
	(0.012)	(0.012)	(0.010)		(0.014)	(0.013)
ln(Item value per capita $_{t-2}$*)*			-0.001			
			(0.016)			
ln(Item value per capita $_{t-3}$*)*			0.037**			
			(0.017)			
ln(Item value per capita $_{t+1}$*)*				-0.003		
				(0.012)		
County characteristics	✓	✓	✓	✓	✓	✓
Crime controls		✓	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓		✓
Year fixed effects					✓	✓
Observations	10998	10998	7332	9776	10998	10998
R^2	0.124	0.125	0.061	0.132	0.055	0.130

Note: This analysis uses the balanced panel described in Table 1. The dependent variable is logged police protection expenditure of county governments per capita. We control for county-level time-varying characteristics such as median household incomes, male shares, shares of the population aged from 15 to 24, racial diversity, poverty rates, and unemployment rates in all specifications. In column (2) to column (6), we add arrest rates for the following crime types: murder, rape, robbery, aggravated assault, burglary, and assault. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: The effects of 1033 program on police spending: FE-IV model, balanced panel, state-specific time trends

	(1)	(2)	(3)	(4)
Panel A: First-stage				
$ln(Value_{t-1} \times Land_j)$	0.021***	0.020***	0.019***	0.018***
	(0.005)	(0.005)	(0.006)	(0.007)
Panel B: Second-stage				
$ln(Item\ value\ per\ capita\ _{t-1})$	-0.069	-0.070	-0.320	-0.161
	(0.265)	(0.274)	(0.336)	(0.380)
Kleibergen-Paap LM statistic	18.843	17.769	9.363	6.955
Kleibergen-Paap Wald F statistic	19.047	17.934	9.421	6.986
Endogeneity Test	0.766	0.773	0.289	0.612
Sample	Full	Full	$0.1 \leq p$	$0.15 \leq p$
County characteristics	✓	✓	✓	✓
Crime controls		✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	10998	10998	8385	6878
R^2	-0.005	-0.005	-0.137	-0.047

Note: Column (1) and column (2) use the full balanced sample. Column (3) and column (4) restrict the sample based on the likelihood of receiving any items through the 1033 program, p. Panel A summarizes the first-stage estimates where the dependent variable is the value of items that a county-level agency has received through the 1033 program. $Value_{t-1}$ is the total value of 1033 items released by the Department of Defense in a year $t-1$. $Land_j$ is the land area of county j. Panel B summarizes the second-stage results where the dependent variable is logged police protection expenditure of county governments per capita. The Kleibergen-Paap Wald F statistic shows the relevance of the instruments. The Kleibergen-Paap LM statistic is used to test whether a model is under-identified. We also test the null hypothesis that the item value can actually be treated as exogenous using a GMM distance test proposed by Baum et al. (2007) and present the results. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: The heterogeneous effects of 1033 program on police spending: by item types, balanced panel, state-specific time trends

	(1)	(2)	(3)	(4)
	Vehicle		Non-vehicle	
	FE	FE-IV	FE	FE-IV
$ln(Vehicle\ item\ value\ per\ capita\ _{t-1})$	0.018*	-0.019		
	(0.011)	(0.781)		
$ln(Non-vehicle\ item\ value\ per\ capita\ _{t-1})$			-0.072	0.111
			(0.052)	(0.365)
Kleibergen-Paap LM statistic		2.063		149.778
Kleibergen-Paap Wald F statistic		2.054		173.569
Endogeneity test		0.080		0.385
Observations	10998	10998	10998	10998
R^2	0.125	-0.001	0.125	-0.003

Note: We use the balanced panel and estimate the baseline equation (1) using both FE and FE-IV models. In column (1) and column (2), the dependent variable is the logged value of vehicle items that a county has received through the 1033 program. In column (3) and column (4), the dependent variable is the logged value of non-vehicle items. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: The heterogeneous effects of 1033 program on police spending: by political preferences, balanced panel, state-specific time trends

	(1)	(2)	(3)	(4)
	Republican		Democrat	
	FE	FE-IV	FE	FE-IV
$ln(Item\ value\ per\ capita_{\ t-1})$	0.018	-0.083	-0.007	-0.060
	(0.017)	(0.270)	(0.014)	(0.758)
Kleibergen-Paap LM statistic		13.609		3.590
Kleibergen-Paap Wald F statistic		13.778		3.570
Endogeneity test		0.710		0.944
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	5499	5499	5499	5499
R^2	0.123	-0.009	0.153	-0.002

Note: We split the balanced panel into two groups based on political preferences of counties and estimate the baseline equation (1) using both FE and FE-IV models. The sample is restricted to counties whose Democrat vote shares for the 2008 U.S. presidential election are less than or equal to the median of the balanced panel ("Republican") in the first two columns and to counties whose Democrat vote shares are greater than the median ("Democrat") in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: The heterogeneous effects of 1033 program on police spending: by population size, balanced panel, state-specific time trends

	(1)	(2)	(3)	(4)
	Small		Large	
	FE	FE-IV	FE	FE-IV
$ln(Item\ value\ per\ capita\ _{t-1})$	0.004	0.363	0.013	-0.959
	(0.016)	(0.334)	(0.012)	(0.618)
Kleibergen-Paap LM statistic		11.954		5.878
Kleibergen-Paap Wald F statistic		12.060		5.867
Endogeneity test		0.261		0.049
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	5445	5445	5553	5553
R^2	0.145	-0.108	0.127	-0.785

Note: We split the balanced panel into two groups based on the population size of counties and estimate the baseline equation (1) using both FE and FE-IV models. The sample is restricted to counties whose population sizes are less than or equal to the mean of the balanced panel ("Small") in the first two columns and to counties whose population sizes are greater than the median ("Large") in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Appendix: Log-log specifications, unbalanced panel, year fixed effects

Table B.1: The effects of 1033 program on police spending: FE model, unbalanced panel, year fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
$ln(Item\ value\ per\ capita_{\ t-1})$	0.001	0.001	0.010		0.001	0.001
	(0.009)	(0.009)	(0.009)		(0.009)	(0.009)
$ln(Item\ value\ per\ capita_{\ t-2})$			0.012			
			(0.014)			
$ln(Item\ value\ per\ capita_{\ t-3})$			0.044***			
			(0.014)			
$ln(Item\ value\ per\ capita_{\ t+1})$				0.003		
				(0.010)		
County characteristics	✓	✓	✓	✓	✓	✓
Crime controls		✓	✓	✓	✓	✓
State-specific linear time trends					✓	✓
Year fixed effects	✓	✓	✓	✓		✓
Observations	17539	17539	9069	13101	17539	17539
R^2	0.045	0.046	0.006	0.048	0.046	0.046

Note: This analysis uses the unbalanced panel described in Table 1. The dependent variable is logged police protection expenditure of county governments per capita. We control for county-level time-varying characteristics such as median household incomes, male shares, shares of the population aged from 15 to 24, racial diversity, poverty rates, and unemployment rates in all specifications. In column (2) to column (6), we add arrest rates for the following crime types: murder, rape, robbery, aggravated assault, burglary, and assault. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: The effects of 1033 program on police spending: FE-IV model, unbalanced panel, year fixed effects

	(1)	(2)	(3)	(4)
Panel A: First-stage				
$ln(Value_{t-1} \times \frac{1}{Dist_j})$	-0.077***	-0.076***	-0.087***	-0.095***
	(0.020)	(0.019)	(0.021)	(0.023)
Panel B: Second-stage				
$ln(Item\ value\ per\ capita\ _{t-1})$	0.475	0.450	0.232	0.365
	(0.332)	(0.339)	(0.295)	(0.291)
Kleibergen-Paap LM statistic	14.801	14.422	18.596	20.563
Kleibergen-Paap Wald F statistic	15.700	15.432	17.454	17.021
Endogeneity Test	0.147	0.181	0.440	0.227
Sample	Full	Full	$0.1 \leq p$	$0.15 \leq p$
County characteristics	✓	✓	✓	✓
Crime controls		✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	17532	17532	13041	9615
R^2	(0.332)	(0.339)	(0.295)	(0.291)

Note: Column (1) and column (2) use the full unbalanced sample. Column (3) and column (4) restrict the sample based on the likelihood of receiving any items through the 1033 program, p. Panel A summarizes the first-stage estimates where the dependent variable is the value of items that a county-level agency has received through the 1033 program. $Value_{t-1}$ is the total value of 1033 items released by the Department of Defense in a year $t-1$. $Dist_j$ measures distance between the centroid of county j and the closest Field Activity Center from the county. Panel B summarizes the second-stage results where the dependent variable is logged police protection expenditure of county governments per capita. The Kleibergen-Paap Wald F statistic shows the relevance of the instruments. The Kleibergen-Paap LM statistic is used to test whether a model is under-identified. We also test the null hypothesis that the item value can actually be treated as exogenous using a GMM distance test proposed by Baum et al. (2007) and present the results. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: The heterogeneous effects of 1033 program on police spending: by item types, unbalanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Vehicle		Non-vehicle	
	FE	FE-IV	FE	FE-IV
$Vehicle\ item\ value\ per\ capita_{\ t-1}$	0.005	0.626*		
	(0.008)	(0.380)		
$Non-vehicle\ item\ value\ per\ capita_{\ t-1}$			-0.039	0.641
			(0.034)	(5.768)
Kleibergen-Paap LM statistic		16.250		0.230
Kleibergen-Paap Wald F statistic		17.126		0.243
Endogeneity test		0.100		0.904
Observations	17539	17532	17539	17532
R^2	0.046	-0.320	0.046	-0.048

Note: We use the unbalanced panel and estimate the baseline equation (1) using both FE and FE-IV models. In column (1) and column (2), the dependent variable is the logged value of vehicle items that a county has received through the 1033 program. In column (3) and column (4), the dependent variable is the logged value of non-vehicle items. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: The heterogeneous effects of 1033 program on police spending: by political preferences, unbalanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Republican		Democrat	
	FE	FE-IV	FE	FE-IV
ln(Item value per capita $_{t-1}$*)*	-0.000	0.308	0.001	0.520
	(0.011)	(0.638)	(0.014)	(0.357)
Kleibergen-Paap LM statistic		3.744		16.507
Kleibergen-Paap Wald F statistic		4.735		9.752
Endogeneity test		0.625		0.157
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	10741	10734	6798	6798
R^2	0.046	-0.093	0.051	-0.189

Note: We split the unbalanced panel into two groups based on political preferences of counties and estimate the baseline equation (1) using both FE and FE-IV models. The sample is restricted to counties whose Democrat vote shares for the 2008 U.S. presidential election are less than or equal to the median of the balanced panel ("Republican") in the first two columns and to counties whose Democrat vote shares are greater than the median ("Democrat") in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: The heterogeneous effects of 1033 program on police spending: by population size, unbalanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Small		Large	
	FE	FE-IV	FE	FE-IV
$ln(Item\ value\ per\ capita\ _{t-1})$	-0.002	-0.001	-0.005	0.148
	(0.011)	(1.538)	(0.012)	(0.390)
Kleibergen-Paap LM statistic		0.422		13.994
Kleibergen-Paap Wald F statistic		0.472		11.399
Endogeneity test		0.999		0.695
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	10289	10282	7250	7250
R^2	0.060	0.000	0.032	-0.015

Note: We split the unbalanced panel into two groups based on the population size of counties and estimate the baseline equation (1) using both FE and FE-IV models. The sample is restricted to counties whose population sizes are less than or equal to the mean of the balanced panel ("Small") in the first two columns and to counties whose population sizes are greater than the median ("Large") in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Appendix: Log-log specifications, balanced panel, year fixed effects

Table C.1: The effects of 1033 program on police spending: FE model, balanced panel, year fixed effects

	(1)	(2)	(3)	(4)
ln(Item value per capita $_{t-1}$*)*	0.019	0.019	0.016	
	(0.014)	(0.014)	(0.011)	
ln(Item value per capita $_{t-2}$*)*			0.004	
			(0.018)	
ln(Item value per capita $_{t-3}$*)*			0.049***	
			(0.018)	
ln(Item value per capita $_{t+1}$*)*				-0.000
				(0.013)
County characteristics	✓	✓	✓	✓
Crime controls		✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	10998	10998	7332	9776
R^2	0.053	0.055	0.007	0.059

Note: This analysis uses the balanced panel described in Table 1. The dependent variable is logged police protection expenditure of county governments per capita. We control for county-level time-varying characteristics such as median household incomes, male shares, shares of the population aged from 15 to 24, racial diversity, poverty rates, and unemployment rates in all specifications. In column (2) to column (4), we add arrest rates for the following crime types: murder, rape, robbery, aggravated assault, burglary, and assault. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: The effects of 1033 program on police spending: FE-IV model, balanced panel, year fixed effects

	(1)	(2)	(3)	(4)
Panel A: First-stage				
$ln(Value_{t-1} \times \frac{1}{Dist_j})$	-0.082***	-0.081***	-0.084***	-0.089***
	(0.019)	(0.019)	(0.021)	(0.022)
Panel B: Second-stage				
$ln(Item\ value\ per\ capita\ _{t-1})$	0.396	0.370	0.168	0.232
	(0.331)	(0.338)	(0.329)	(0.358)
Kleibergen-Paap LM statistic	21.517	21.375	21.513	20.900
Kleibergen-Paap Wald F statistic	17.661	17.546	16.097	15.838
Endogeneity Test	0.262	0.307	0.644	0.549
Sample	Full	Full	$0.1 \leq p$	$0.15 \leq p$
County characteristics	✓	✓	✓	✓
Crime controls		✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	10998	10998	8477	6449
R^2	-0.111	-0.096	-0.022	-0.051

Note: Column (1) and column (2) use the full balanced sample. Column (3) and column (4) restrict the sample based on the likelihood of receiving any items through the 1033 program, p. Panel A summarizes the first-stage estimates where the dependent variable is the value of items that a county-level agency has received through the 1033 program. $Value_{t-1}$ is the total value of 1033 items released by the Department of Defense in a year $t-1$. $Dist_j$ measures distance between the centroid of county j and the closest Field Activity Center from the county. Panel B summarizes the second-stage results where the dependent variable is logged police protection expenditure of county governments per capita. The Kleibergen-Paap Wald F statistic shows the relevance of the instruments. The Kleibergen-Paap LM statistic is used to test whether a model is under-identified. We also test the null hypothesis that the item value can actually be treated as exogenous using a GMM distance test proposed by Baum et al. (2007) and present the results. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: The heterogeneous effects of 1033 program on police spending: by item types, balanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Vehicle		Non-vehicle	
	FE	FE-IV	FE	FE-IV
$Vehicle\ item\ value\ per\ capita\ _{t-1}$	0.027**	0.504		
	(0.013)	(0.386)		
$Non-vehicle\ item\ value\ per\ capita\ _{t-1}$			-0.063	0.681
			(0.057)	(2.176)
Kleibergen-Paap LM statistic		24.505		2.108
Kleibergen-Paap Wald F statistic		17.700		2.357
Endogeneity test		0.229		0.729
Observations	10998	10998	10998	10998
R^2	0.055	-0.162	0.055	-0.055

Note: We use the balanced panel and estimate the baseline equation (1) using both FE and FE-IV models. In column (1) and column (2), the dependent variable is the logged value of vehicle items that a county has received through the 1033 program. In column (3) and column (4), the dependent variable is the logged value of non-vehicle items. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: The heterogeneous effects of 1033 program on police spending: by political preferences, balanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Republican		Democrat	
	FE	FE-IV	FE	FE-IV
$ln(Item\ value\ per\ capita_{\ t-1})$	0.020	0.183	0.012	0.489
	(0.020)	(0.821)	(0.017)	(0.343)
Kleibergen-Paap LM statistic		4.154		21.030
Kleibergen-Paap Wald F statistic		5.058		9.089
Endogeneity test		0.843		0.175
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	5499	5499	5499	5499
R^2	0.065	-0.023	0.047	-0.142

Note: We split the balanced panel into two groups based on political preferences of counties and estimate the baseline equation (1) using both FE and FE-IV models. The sample is restricted to counties whose Democrat vote shares for the 2008 U.S. presidential election are less than or equal to the median of the balanced panel ("Republican") in the first two columns and to counties whose Democrat vote shares are greater than the median ("Democrat") in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

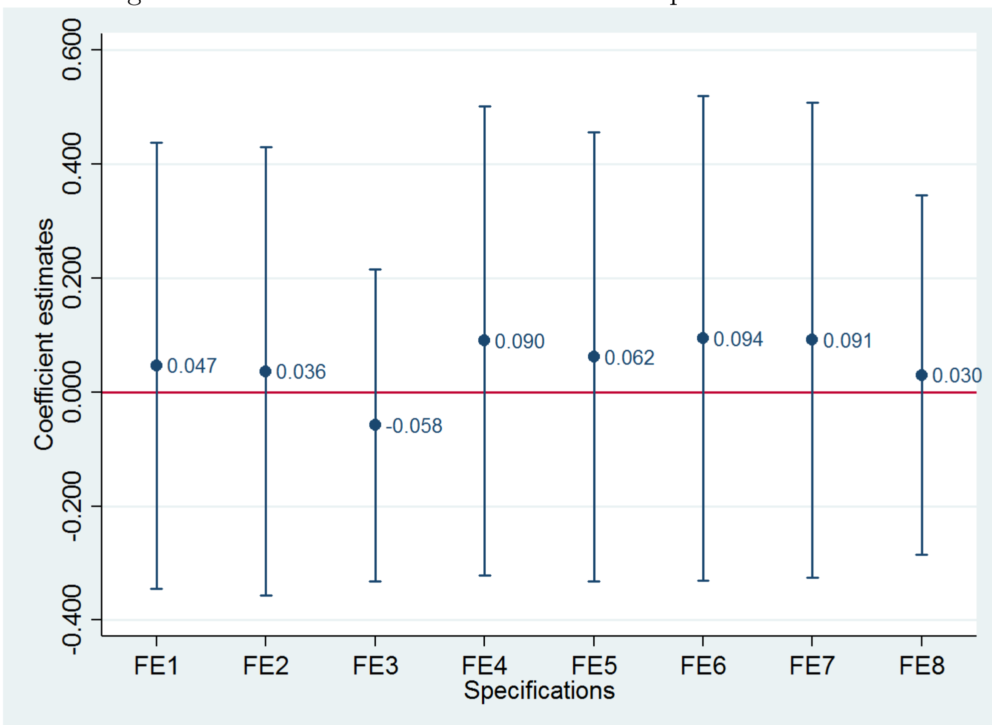
Table C.5: The heterogeneous effects of 1033 program on police spending: by population size, balanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Small		Large	
	FE	FE-IV	FE	FE-IV
$ln(Item\ value\ per\ capita_{\ t-1})$	0.022	0.004	0.000	0.016
	(0.020)	(1.497)	(0.014)	(0.383)
Kleibergen-Paap LM statistic		0.635		22.760
Kleibergen-Paap Wald F statistic		0.661		13.404
Endogeneity test		0.991		0.968
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	5445	5445	5553	5553
R^2	0.072	0.000	0.041	-0.000

Note: We split the balanced panel into two groups based on the population size of counties and estimate the baseline equation (1) using both FE and FE-IV models. The sample is restricted to counties whose population sizes are less than or equal to the mean of the balanced panel ("Small") in the first two columns and to counties whose population sizes are greater than the median ("Large") in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 3 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

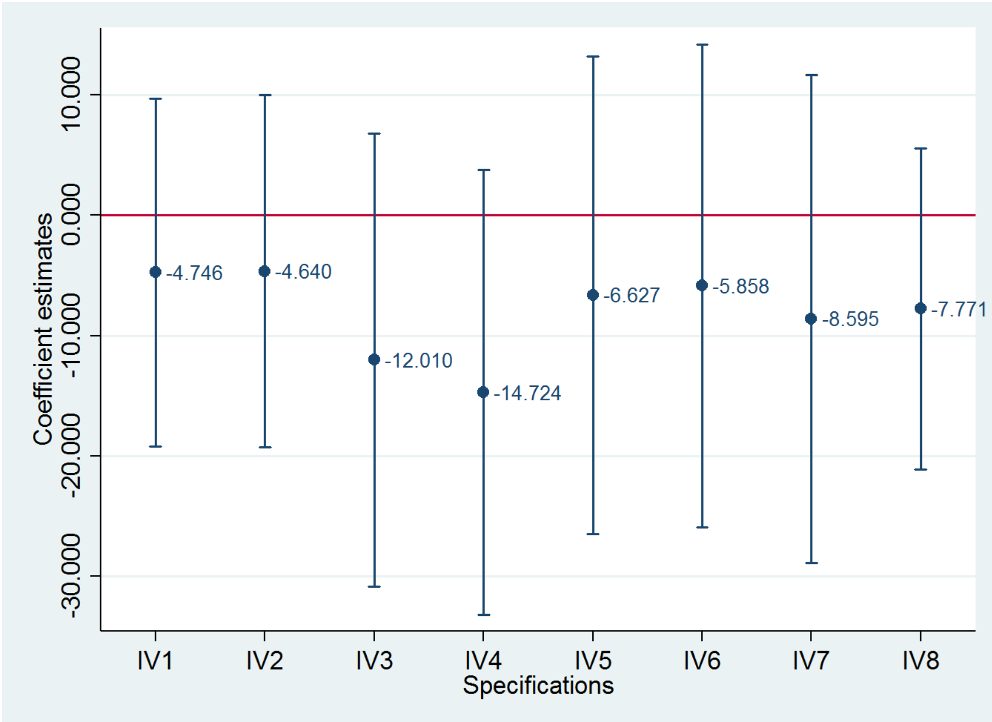
D Appendix: Level-level specifications estimates

Figure D.1: FE model estimates with state-specific time trends



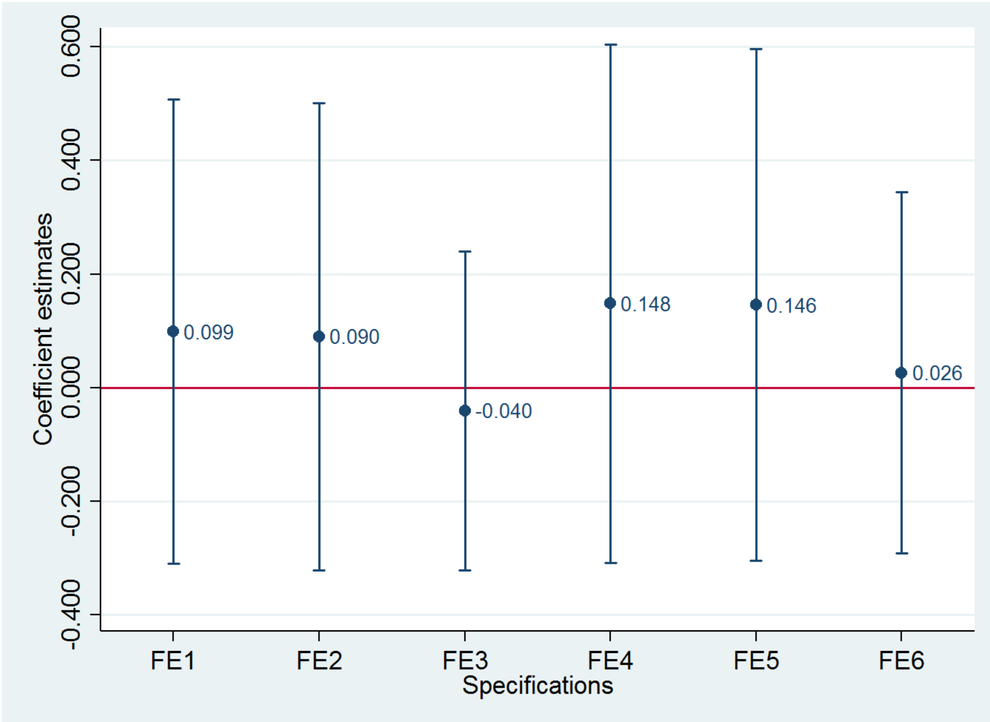
Note: FE1 to FE5 correspond to column (1) to column (5) in Table 2, except that the police expenditure and the item value are given in level terms. From FE6 to FE8, we re-estimate FE1 to FE3 using the balanced panel. Each dot represents the coefficient estimate for the item values and the line that passes through it represents the 95% confidence interval for the estimate.

Figure D.2: FE-IV model estimates with state-specific time trends



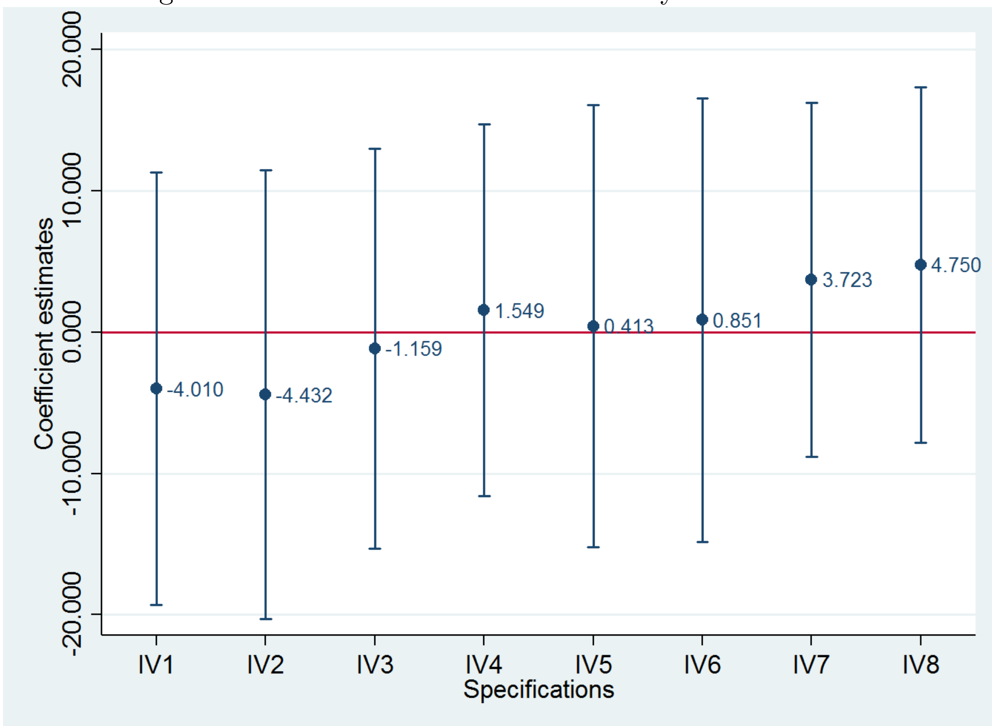
Note: IV1 to IV4 correspond to column (1) to column (4) in Table 3, except that the police expenditure and the item value are given in level terms. From IV5 to IV8, we re-estimate IV1 to IV4 using the balanced panel. Our instrument in this analysis is the interaction between $Value_{t-1}$ and $ln(land_j)$. The corresponding F-statistics for all specifications are less than 10. Each dot represents the second-stage coefficient estimate for the item values and the line that passes through it represents the 95% confidence interval for the estimate.

Figure D.3: FE model with year fixed effects



Note: FE1 to FE3 correspond to the column (1) to column (3) in Table B.1, except that the police expenditure and the item value are given in level terms. From FE4 to FE6, we re-estimate FE1 to FE3 using the balanced panel. Each dot represents the coefficient estimate for the item values and the line that passes through it represents the 95% confidence interval for the estimate.

Figure D.4: FE-IV model estimates with year fixed effects



Note: IV1 to IV4 correspond to column (1) to column (4) in Table B.2, except that the police expenditure and the item value are given in level terms. From IV5 to IV8, we re-estimate IV1 to IV4 using the balanced panel. Our instrument in this analysis is the interaction between $Value_{t-1}$ and $\frac{1}{Dist_j}$. The corresponding F-statistic is less than 10 in IV3, IV5, IV6, IV7, and IV8. Each dot represents the second-stage coefficient estimate for the item values and the line that passes through it represents the 95% confidence interval for the estimate.