Do In-Kind Grants Stick?

The Department of Defense 1033 program and local government spending*

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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.

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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 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 considered 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 dollars 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. Several aspects of this setting suggest that local fiscal responses to these grants may differ substantively from monetary grants of any type. First, transfers through the 1033 program are not intended to modify local spending or 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 (e.g., the department may not purchase additional uniform pants because they have received tactical pants through the 1033 program). 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 substantively less crowding out.

To empirically evaluate the relationship between 1033 program receipts and local public spending on police protection, we form a county-year panel data set comprised of county expenditure accounts from the Annual Survey of Governments matched to the value of 1033 equipment transfers from the Defense Logistics Agency received by county government Law Enforcement Agencies, usually Sheriff's offices. 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 a crowd-out effect of no larger than 0.026% for a 1% increase in 1033 transfers, and likewise, a crowd-in effect of no more than 1.1% per 1% increase in transfers. 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 endogenous 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, endogenous 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 implement an instrumental variables specification that exploits exogenous variation in transaction costs faced by departments when acquiring 1033 items, similar to 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. Estimated coefficients on 1033 receipts favor crowding in for both categories of goods, although our instruments do not perform as well when confined to non-vehicle acquisitions. Additionally, we might expect the effects of 1033 receipts on local police spending to vary by the fiscal or social conservatism of an area. To evaluate this possibility, we divide the county panel according to political leanings in the 2008 presidential election. We find that democratic counties exhibit greater evidence of crowding in, although that is likely partially because Democrat counties tend to be larger urban counties which likely have greater flexibility in budgets to accommodate relatively small ad hoc requests from law enforcement.

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 or one reason or another, policy makers might look to the structure of the 1033 program for guidance.

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. 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 an avenue for surplus defense items.

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 secondary transfers of items acquired through the 1033 program to other agencies.[2] Second, there is very little, if any, administrative costs 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, 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

[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.

cannot be engaged to produce public safety without additional inputs. Therefore, the receipt of 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 HIDTA designation

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 follow a a fixed effects instrumental variable (FE-IV) specification with Bartik-type instruments with an identification strategy similar to Harris et al. (2017). Although the empirical approach in this paper closely follows Harris et al. (2017), these two papers answer fundamentally different questions. Whereas the former examined the effects of acquiring tactical equipment on citizen complaints, deaths of police and civilians and drug arrests, this paper focuses solely on the impact of these grants in kind on local expenditures on law enforcements. An abbreviated discussion of the institutional details behind our identification strategy follows. For a more thorough discussion of institutional details, we refer the reader to Harris et al. (2017) for further discussion.

Our instruments are interactions of time-series and cross-sectional variables that rely on distinct sources of exogenous variation. On the time-series dimension, the amount of equipment available for distribution through the 1033 program varies exogenously year-by-year. Disbursements 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 steeply increased the availability of all types of equipment. More generally, the supply of equipment is determined by national military spending and need rather than any facet of local law enforcement. Regarding cross-sectional exogenous variation, law enforcement agencies face plausibly exogenous differences in the transportation costs associated with acquiring 1033 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

[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.

bureaucratic and voter preferences for public safety or other unobservable factors that determine police budgets.

The DLA also states that when there are competing claims by different jurisdictions for a given item, preference will be given to counties that have been designated "High Intensity Drug Trafficking Areas (HIDTAs)." There is a field for "HIDTA" on the 1033 vehicle request form. 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 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), being designated a HIDTA was a consideration in determining which claims were prioritized at a given point in time. The interaction of HIDTA with the 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. The location of these FAC's predates the 1033 program by at least 20 years, meaning that their placement is completely independent of whether nearby police departments are substantial users of surplus equipment. Additionally, being near a FAC does not equate to being near a military base. 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. Given that items are only made available for claim by local law enforcement in a 14-day window and that claimed items must be immediately taken into possession, the difference between traveling 100 miles or 200 miles to evaluate goods is non-trivial. We therefore also use inverse distance to the nearest FAC as a cross sectional component of these Bartik instruments.

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 surveys of the same government units. These data provide detailed information on the revenues and expenditures of different levels of government. We primarily focus on police expenditures of county governments because 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 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 always possible. Fortunately, only 1.3 percent of the sample reported zero expenditure for police protection. Assuming that data 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 Tables 9 and 10 that results are similar when using balanced panels.

Data on transfers of military equipment to local law enforcement agencies come from the DLA. These 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 2016 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

[6]The definition of police expenditure is available on the Annual Survey of State and Local Government Finances webpage. Source: https://www.census.gov/programs-surveys/gov-finances/about/glossary.html#par_textimage_613285724

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 rates 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 the 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 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.

The counties in our analytical sample constitute a substantial portion of county-level law enforcement in the United States. The Bureau of Justice Statistics reported that the U.S. as a whole spends \$100 billion on the policing aspect of law enforcement per year.[11] County Sheriff's Offices - the focus of our analysis - account for approximately \$30 billion of the \$100 billion spent nationally. In 2007, counties in our sample accounted for 73.5% of that \$30 billion (**?**). Additionally, county government entities in our sample absorbed a significant portion of the items distributed

[7] In particular, because Berkshire County Sheriffs Office (MA), Northern York County Police (PA), and York-Poquoson Sheriffs Office (VA) belong to multiple jurisdictions, we are unable to uniquely identify county information for those agencies.

[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 $diversity_{jt} = 1 - \sum_k race^2_{kjt}$, where $race_{kjt}$ represents the population share of a particular race k in county j in year t, 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.

[10] The data can be downloaded from the *The Guardian*'s webpage (Source: https://www.theguardian.com/news/datablog/2009/mar/02/us-elections-2008).

[11] The 'policing aspect' reflects that this estimate does not include funds spent on corrections. Source: http://www.justicepolicy.org/research/3906

Table 1: Summary statistics

	Unbalanced		Balanced	
	Mean	Std. Errors	Mean	Std. Errors
Lagged 1033 item value per capita ($)	0.24	(2.04)	0.19	(1.73)
Lagged 1033 value per capita if value>0	1.46	(4.82)	1.144	(4.17)
ln(Lagged 1033 item value per capita) ($)	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)
Land Area (sq. mi.)	1084.43	(1510.55)	1220.38	(1701.70)
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.

through the 1033 program. In 2012, county governments in our sample received 38 percent of the total value of goods transferred through the 1033 program.

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 shows 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 unbalanced panel is substantially smaller than the average population size of the balanced panel.

Because 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 $96.8 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.[12].

[12] Conditional on receipt, the average police spending is 99.8 in the unbalanced panel.

Table 2: National county average vs. counties in our estimation sample (in 2012)

	National average			Estimation sample		
	Obs.	Mean	Std. Errors	Obs.	Mean	Std. Errors
ln(Lagged 1033 item value per capita) ($)	3002	0.12	(0.43)	2656	0.13	(0.45)
ln(Lagged 1033 vehicle item value per capita) ($)	3002	0.11	(0.42)	2656	0.12	(0.45)
ln(Police expenditure per capita) ($)	2992	4.37	(0.74)	2656	4.37	(0.74)
Population (1,000)	3118	99.44	(319.95)	2656	103.18	(330.58)
Median income ($1,000)	3118	44.75	(11.32)	2656	44.94	(11.31)
Male (%)	3118	50.04	(2.24)	2656	50.00	(2.06)
Age 15 to 24 (%)	3118	13.03	(3.46)	2656	13.00	(3.26)
Diversity index (% point)	3118	20.47	(16.16)	2656	20.27	(15.76)
Poverty (%)	3118	17.22	(6.57)	2656	17.12	(6.49)
Unemployment rate (%)	3118	7.85	(2.77)	2656	7.88	(2.74)
Lagged murder per 100,000 population	3007	1.12	(5.76)	2656	1.16	(3.98)
Lagged rape per 100,000 population	3007	2.26	(8.59)	2656	2.39	(5.85)
Lagged robbery per 100,000 population	3007	4.22	(70.66)	2656	3.13	(7.86)
Lagged aggravated per 100,000 population	3007	32.66	(116.51)	2656	34.29	(55.85)
Lagged burglary per 100,000 population	3007	30.51	(59.73)	2656	33.31	(49.23)
Lagged assault per 100,000 population	3007	109.03	(197.08)	2656	118.84	(159.99)

Note: This table compares the average characteristics of the whole U.S. counties to the average characteristics of counties in our unbalanced panel as of 2012. We compute the county-level national average using data from various sources described in Section III. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars.

Counties in our unbalanced analytical sample are broadly representative of the U.S. as a whole. Table 2 compares summary statistics of our estimation sample in 2012 to the national average for each county. In no area are there significant differences.

IV Empirical Model

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

$$(1) \qquad protection_{j,t} = \beta_0 + \beta_1 value_{j,t-1} + \mathbf{\Gamma} \boldsymbol{X_{j,t}} + \mathbf{\Psi} \boldsymbol{C_{j,t-1}} + \theta_j + \delta_t + \varepsilon_{j,t}$$

where $protection_{j,t}$ represents police protection expenditure of county j in year t and $value_{j,t-1}$ 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 $protection_{j,t}$ and $values_{j,t-1}$ 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. $\boldsymbol{C_{j,t-1}}$ is a vector of lagged county j's arrest rates for murder, rape, robbery, aggravated assault, burglary, and other assaults. The parameters θ_j and δ_t are county fixed effects and year fixed effects, respectively. Robust standard errors are clustered at the county level. As a benchmark for interpretation, if $\beta_1 = 0$, that implies there is no crowding in or crowding out, but that the 1033 grants are perfectly sticky. If $\beta_1 < 0$, that implies there is some crowding out, that 1033 receipts displace some future county expenditures on law enforcement. If $\beta_1 > 0$, that result implies there is crowding-in, or that 1033 receipts attract additional resources to law enforcement. The recent economics literature on intergovernmental grants nearly always finds evidence of partial crowding out, the variation in the results nearly always boils down to the amount of crowding out on the intensive margin. The features of different grant programs determine the relative magnitude of those estimates.

The fixed effects model presented in Equation 1 relies on within-county variation in acquired 1033 items over time. Under 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. Reverse causality may also be a concern. If Sheriffs expect future reductions in expenditures on public safety, they may more proactively seek other resources such as 1033 transfers. 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.

To address concerns about endogeneity, we estimate the FE-IV model using the instrumental variables briefly described above, with more institutional detail available in Harris et al. (2017). We use the interaction between the total value of all items dispensed through the 1033 program in a given year with whether a county is at any point designated a HIDTA and the inverse distance of the county centroid to the nearest Field Activity Center.[13] The first stage of the FE-IV expression can be expressed as:

(2)

$$value_{j,t-1} = \alpha_0 + \alpha_1 (ln(V_{t-1}) \times \frac{1}{dist_{1j}}) + \alpha_2 (ln(V_{t-1}) \times HIDTA_j) + \mathbf{\Phi} \boldsymbol{X_{j,t}} + \mathbf{\Pi} \boldsymbol{C_{j,t-1}} + \theta_j^1 + \delta_t^1 + \varepsilon_{j,t-1}^1$$

where V_{t-1} represents the total value of all items disbursed through the 1033 program in year $t-1$. Relative to Harris et al. (2017), there are two main differences in the first stage. In our earlier work, modeling the effects of tactical items received through the 1033 program, we modeled stocks, i.e., cumulative numbers of items received through the program. In this paper, we model per-period flows of new 1033 receipts. As such, we expect the sign on the coefficients on the interaction terms, α_1 and α_2, to be negative. In Harris et al. (2017), we showed that counties closer to FAC's accrue greater numbers of goods over time. However, exogenous increases in items made available through the 1033 program are effectively supply shocks. Supply shocks induce agents with lower willingness to pay into the market. While there are not prices of these 1033 transfers per se, these exogenous increases in volume make it more likely that agencies farther away (or non-HIDTA counties) are likely to receive a larger share of the overall volume in that period. Second, we did

[13]HIDTA designation and all other time-invariant characteristics are implicitly captured by county-level fixed effects, as are time-invariant unobservable factors that are correlated with land area or proximity to FAC.

not include two instruments from the first paper, specifically those constructed from the distance to the sixth-nearest FAC and land area.[14]

While level-level (or dollar-for-dollar) specifications are the standard in the flypaper literature, we also use log-log specifications for our regressions due to the high degree of skewness in both the distribution of police spending and 1033 receipts. The 1033 receipts in particular are highly skewed not only between, but within counties over time. As the data on 1033 receipts are administrative data, we do not suspect that this skewness is due to reporting errors. Therefore, any strategy involving the trimming of outliers would be arbitrary and unacceptable. Generally, the results from dollar-for-dollar specifications and log-log specifications (in which the estimated coefficients are interpreted as an elasticity) are consistent with each other. However, the magnitudes of the outliers make it so that we do not strictly prefer one functional form to another.

Table 3: First-stage regression

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable		$value_{j,t-1}$			$ln(value_{j,t-1})$	
$ln(V_{t-1}) \times \frac{1}{dist_{1j}}$	-1.589**	-2.144***		-0.540***	-0.616***	
	(0.733)	(0.725)		(0.155	(0.154)	
$ln(V_{t-1}) \times HIDTA_j$	-0.150***		-0.154***	-0.021***		-0.022***
	(0.030)		(0.031)	(0.006)		(0.006)
Observations	17539	17539	17539	17539	17539	17539
County characteristics	✓	✓	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓

Note: Standard errors in parenthesis are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3 summarizes first-stage estimates for α_1 and α_2 of equation 2. Note that all estimates are negative, suggesting that agencies with lower willingness to participate, proxied by their distances to the closest FAC and HIDTA designation status, tend to receive higher-values 1033 grants in years experiencing larger supply shocks. Second, both instruments are highly significant, regardless of whether one or both are included. In our main analysis, we estimate both overidentified

[14]These instruments were omitted because they were not strong in this context. We do not view this as a problem from our earlier work, however. Differences in the data set, the sample construction, differences between stock and flow construction, differences between county governments and county totals can all affect the strength of empirical relationship between potential instrumental variables and the variable for which we are instrumenting.

and exactly-identified models with each instrument to show the main results are not driven by the choice of instruments.

V Results

Table 4 contains results of the baseline FE and FE-IV models. FE results are in column 1. Columns 2-4 contain IV results using three different instrumental variable sets: the interaction of annual total value of 1033 goods ($ln(V_t)$) with inverse distance to the nearest FAC ($1/dist_{1j}$), the interaction of $ln(V_t)$ with county HIDTA status ($HIDTA_j$), and their combination. Results from the baseline FE regression in Column (1), while imprecise, indicate that little, if any, of the value of 1033 receipts crowd out local police spending. With 95% certainty, police spending decreases by no more than 32 cents per $1 dollar in new 1033 transfers. Compared to previous studies in the flypaper literature, (see Table 13) this is an unusually sticky grant. The results from FE-IV specifications in columns 2-4 stand in sharper contrast to that literature. Results from our preferred Column (2) specification indicate that each $1 in additional 1033 receipts – promoted by national shocks in 1033 distributions interacted with HIDTA status and distance to a FAC – led to a weakly significant $8.57 increase in policy spending, or about 8% of a standard deviation. Results in columns (3) and (4) similarly favor crowding in, albeit with relatively wider confidence intervals that include zero. Positive point estimates across Table 4 specifications are notable and suggest that 1033 transfers elicit a much different response than other intergovernmental transfers, but the magnitude of FE-IV results in columns 2-4 warrant caution. The distribution of year-to-year changes in police expenditures is wide, with a mean of just $1.64 per capita alongside a standard deviation 33 times larger, at $54. While an $8.57 change in per-capita police spending is not exceptional (one in five year-to-year changes are larger), there is no precedent for a crowd-in effect of that magnitude in response to a $1 increase in external grants. We also note that FE-IV results are somewhat sensitive to instrument choice. Results in column 3 are far less precise and do include the possibility of complete crowd out at the lower end of the confidence interval.

Table 5 replicates results from Table 4 but uses a log-log specification, partially to address concerns about outliers in police spending. Similar to Table 4, the estimate of the elasticity of local police funding to 1033 receipts in Column (1) is a relatively precise zero. In the IV specifications in columns (2)-(4) - two of three specifications indicate statistically significant evidence of crowding in, while the third estimate is still positive, but less precise.

All together, these results indicate that the local public spending response to the 1033 program is unique compared to virtually all monetary grants. While results from FE specifications are a relatively precise zero, positive and significant results from FE-IV models seem to correct for a downward bias. One explanation for this bias could be reverse causality. If law enforcement officials expect reductions in budgets, they may be more likely to participate in the 1033 program to offset those losses. Although the magnitude of FE-IV point estimates are implausible, they nevertheless indicate that in-kind transfers in the form of surplus 1033 equipment elicit fundamentally different local funding responses than monetary grants.

Table 4: The effects of 1033 receipts on police spending - dollar for dollar

	(1) FE	(2) FE-IV	(3) FE-IV	(4) FE-IV
1033 recipts per capita (t-1)	0.090	8.568*	5.141	8.949
	(0.210)	(5.020)	(8.943)	(5.489)
Kleibergen-Paap F-statistic		16.029	8.739	25.318
Hansen J Statistic P-value		0.717	.	.
Observations	17539	17532	17532	17532
Instruments (each interacted with $ln(V_t)$)	.	$\frac{1}{dist_{1j}}$	$\frac{1}{dist_{1j}}$	
		$HIDTA_j$		$HIDTA_j$
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓

Column (2) is a panel IV specification using both instruments as specified in equation 2. Column (3) only uses the instrument which uses inverse distance to the nearest field activity center as the cross-sectional component, and Column (4) only uses whether a county is ever designated an HIDTA. All standard errors are in parenthesis and clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All specifications use lagged values of 1033 receipts to address concerns about simultaneity bias. However, receipts from the 1033 program may have effects on police spending beyond the first year. If on one hand, durable capital goods require continued inputs to be useful (e.g., vehicles) any crowding in may persist beyond the first year. Alternatively, if any crowding in from Tables 4 and 5 is just intertemporal displacement of flexible funds, crowding in the year of receipt may displace future expenditures on public safety. To explore this possibility, Tables 6 and 7 contain results from

Table 5: The effects of 1033 receipts on police spending - elasticity

	(1) FE	(2) FE-IV	(3) FE-IV	(4) FE-IV
ln(1033 receipts per capita (t-1))	0.001	0.562**	0.717*	0.478
	(0.009)	(0.268)	(0.367)	(0.348)
Observations	17539	17532	17532	17532
Kleibergen-Paap F-statistic		14.115	16.340	12.363
Hansen J Statistic P-value		0.629	.	.
Instruments	.	$\frac{1}{dist_{1j}}$ $HIDTA_j$	$\frac{1}{dist_{1j}}$	$HIDTA_j$
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓

Column (2) is a panel IV specification using both instruments as specified in equation 2. Column (3) only uses the instrument which uses inverse distance to the nearest field activity center as the cross-sectional component, and Column (4) only uses whether a county is ever designated an HIDTA. All standard errors are in parenthesis and clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

fixed effect regressions where we jointly estimate the effects of several lags of 1033 receipts and test the joint significance. The results are generally consistent between the level and log specifications: point estimates are almost always positive but insignificant. Note that all of the estimates of the first lag of receipts are consistent with the results from the FE specification in Table 4. Where we do see statistical significance is the estimated effect of 1033 transfers three years prior. When only including the first three lags, the effects of 1033 receipts on public spending is jointly significant, but largely driven by the three-year lag. Note also that our results are qualitatively similar when estimating dollar-for-dollar effects of elasticities as in Table 7.

Table 8 contains results from FE-IV specifications designed to capture effects of 1033 receipts beyond the first year. The instrumental variables approach requires a modification from the models in Tables 6-7, the variable of interest is the sum of the last K years receipts rather than including effects for each successive year.[15] Specifically, we use $ln(\sum_{i=1}^{k} V_{t-i}) \times \frac{1}{dist_{1j}}$ as well as $ln(\sum_{i=1}^{k} V_{t-i}) \times HIDTA_j$ to instrument for $\sum_{i=1}^{k} 1033 \;\; value_{t-i}$. While all estimates are positive, all are insignificant except for the estimated elasticities for the two-and-three year totals. However, these results should be interpreted with some caution, as the P-values from the overidentification

[15] Instruments were underpowered when attempting to estimate these effects separately.

Table 6: Long run effects of 1033 receipts: FE model, dollar-for-dollar

	(1)	(2)	(3)	(4)	(5)
1033 item value per capita (t-1)	0.079	0.054	0.113	0.024	0.002
	(0.207)	(0.175)	(0.175)	(0.172)	(0.001)
1033 item value per capita (t-2)	0.313	0.007	0.013	-0.092	0.000
	(0.236)	(0.166)	(0.165)	(0.238)	(0.001)
1033 item value per capita (t-3)		1.655*	1.888*	1.179**	
		(0.957)	(1.111)	(0.495)	
1033 item value per capita (t-4)			2.078	3.048	
			(1.703)	(2.291)	
1033 item value per capita (t-5)				0.352	
				(1.136)	
$\sum_{i=3}^{5} Itemvalue_{t-1}$					0.005*
					(0.003)
Observations	15875	13205	11548	9783	9783
P-value for Test of Joint Significance	0.241	0.090*	0.126	0.161	0.069*
County characteristics	✓	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Long run effects of 1033 receipts: FE model, elasticity

	(1)	(2)	(3)	(4)	(5)
ln(1033 item value per capita) (t-1)	0.000	0.006	0.008	0.003	0.002
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
ln(1033 item value per capita) (t-2)	0.013	0.010	0.011	0.012	0.014
	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)
ln(1033 item value per capita) (t-3)		0.037***	0.040***	0.037***	
		(0.013)	(0.014)	(0.013)	
ln(1033 item value per capita) (t-4)			-0.023	-0.008	
			(0.019)	(0.022)	
ln(1033 item value per capita) (t-5)				-0.000	
				(0.022)	
$ln(\sum_{i=3}^{5} Itemvalue_{t-1})$					0.020
					(0.014)
Observations	15875	13205	11548	9783	9783
P-value for Test of Joint Significance	0.446	0.025**	0.283	0.380	0.095
County characteristics	✓	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Long run effects of 1033 receipts: FE-IV Specification

Panel A: Estimated Dollar-for-dollar effects				
	(1)	(2)	(3)	(4)
$\sum_{i=1}^{2} 1033 \;\; value_{t-i}$	5.659			
	(3.531)			
$\sum_{i=1}^{3} 1033 \;\; value_{t-i}$		3.927		
		(2.686)		
$\sum_{i=1}^{4} 1033 \;\; value_{t-i}$			4.150	
			(2.941)	
$\sum_{i=1}^{5} 1033 \;\; value_{t-i}$				2.855
				(2.972)
Kleibergen-Paap F-statistic	14.892	14.143	11.273	11.125
Hansen J Statistic P-value	0.267	0.083	0.004	0.001

Panel B: Estimated elasticity of police funding to 1033 receipts				
	(1)	(2)	(3)	(4)
$\ln(\sum_{i=1}^{2} 1033 \;\; value_{t-i})$	0.226			
	(0.199)			
$\ln(\sum_{i=1}^{3} 1033 \;\; value_{t-i})$		0.236		
		(0.158)		
$\ln(\sum_{i=1}^{4} 1033 \;\; value_{t-i})$			0.133	
			(0.159)	
$\ln(\sum_{i=1}^{5} 1033 \;\; value_{t-i})$				0.037
				(0.150)
Kleibergen-Paap F-statistic	13.457	12.650	10.903	11.080
Hansen J Statistic P-value	0.917	0.665	0.071	0.038
Observations	15867	12872	11123	9359
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓

All specifications use both instruments as specified in equation 2. Standard errors in parentheses are clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

tests indicate the instruments are not performing as well as in the baseline specification. While the dollar-for-dollar estimates in particular are not sufficiently precise to eliminate crowding out, they do provide further evidence that at the expectation, local public spending responds differently to these grants-in-kind than to monetary intergovernmental transfers.

Tables 9 and 10 examine the sensitivity of results from the primary specification to sample restrictions and additional measures to address unobserved heterogeneity. In each table, the FE-IV specification that we evaluate is the one using both instruments.[16] In each table, column (1) contains results from a sample 'trimmed' on the basis of whether a particular count government is likely to receive items through the 1033 program or not. In a first stage, we estimate a propensity score for each county to participate in the program and then eliminate counties in the highest and lowest five percent of propensity scores. In so doing, we verify that our results are not driven by a few counties that are consistently engaged with the 1033 program. We find in both the dollar-for-dollar specifications and elasticity estimates (Tables 9 and 10 respectively) that imposing this restriction does not substantively change the magnitude or significance of the result. In Column (2), the sample is restricted to the balanced panel. While balanced panels have some advantages, in this context, it means restricting the sample towards larger county governments. However, restricting the sample to a balanced panel does not substantively affect the results. Columns (3) and (4) include county and year fixed effects but also state-time trends and state-year fixed effects respectively. While the estimates are no longer over the threshold for 10% significance (p-values of 0.11 and 0.16), the qualitative implications of the results do not change.

V.A Heterogeneous effects of the 1033 program

The previous section provides evidence that grants-in-kind through the 1033 program, at a minimum, result in far less crowding out than most other grants. However, these 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

[16]This corresponds to column (2) in Tables 4 and 5.

Table 9: Robustness checks - dollar-for-dollar effects

Panel A: Fixed Effects Model

	(1)	(2)	(3)	(4)
1033 item value per capita (t-1)	0.058	0.146	0.062	0.102
	(0.169)	(0.230)	(0.201)	(0.209)
Observations	16170	10998	17539	17539

Panel B: Fixed Effects Instrumental Variables Model

	(1)	(2)	(3)	(4)
1033 item value per capita (t-1)	8.398*	7.711*	8.524	7.262
	(4.762)	(4.304)	(5.370)	(5.228)
Kleibergen-Paap F-statistic	15.540	13.319	12.346	12.165
Hansen J Statistic P-value	0.397	0.631	0.132	0.113
Observations	16096	10998	17532	17532

Column (1) contains results from a 'trimmed sample,' excluding counties who are among the most or least likely to be takers of 1033 equipment. Column (2) contains results from restricting the sample to a balanced panel. The specification in Column (3) includes state-specific time trends in addition to year fixed effects, and Column (4) includes fixed effects for every state year. Standard errors in parentheses are clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Robustness Checks - log-log Specification

Panel A: Fixed Effects Model

	(1)	(2)	(3)	(4)
ln(1033 item value per capita) (t-1)	0.002	0.019	-0.002	0.003
	(0.009)	(0.014)	(0.008)	(0.009)
Observations	16170	10998	17539	17539

Panel B: Fixed Effects Instrumental Variables Model

	(1)	(2)	(3)	(4)
ln(1033 item value per capita) (t-1)	0.555**	0.628**	0.346	0.253
	(0.257)	(0.293)	(0.264)	(0.259)
Kleibergen-Paap F-statistic	14.787	12.546	10.207	10.050
Hansen J Statistic P-value	0.779	0.872	0.289	0.231
Observations	16096	10998	17532	17532

Column (1) contains results from a 'trimmed sample,' excluding counties who are among the most or least likely to be takers of 1033 equipment. Column (2) contains results from restricting the sample to a balanced panel. The specification in Column (3) includes state-specific time trends in addition to year fixed effects, and Column (4) includes fixed effects for every state year. Standard errors in parentheses are clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 11 summarizes estimation results from a specification identical to columns (1) and (2) in Tables 5, except that the key covariate in this analysis is either the lagged values of vehicle items or the lagged values of non-vehicle items. For vehicles, the descriptive estimate is a fairly precise zero, while the estimate for non-vehicles indicates some evidence of crowding out. For the causal estimates, however, results indicate that both lagged 1033 vehicle nor non-vehicles values lead to crowding in. However, the IV results for non-vehicle receipts must be interpreted with caution as our instruments are not as strong enough, per Stock and Yogo (2005).

In Table 12, 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 12 suggest that crowding in may be more concentrated in Democratic counties. While this may be attributable to difference in ideology, it is also plausible that Democratic counties tend to be larger, more urban counties. As such, they may have larger amounts of funding to move to accommodate requests for additional funding from law enforcement agencies with newly acquired vehicles.

[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 11: Heterogeneous effects of 1033 program on police spending: by item types

	(1)	(2)	(3)	(4)
	Vehicle		Non-vehicle	
	FE	FE-IV	FE	FE-IV
$ln(Vehicle\ item\ value\ per\ capita_{\ t-1})$	0.005	0.609**		
	(0.008)	(0.304)		
$ln(Non-vehicle\ item\ value\ per\ capita_{\ t-1})$			-0.039	2.291*
			(0.034)	(1.349)
Kleibergen-Paap F-statistic		14.176		4.263
Hansen J Statistic P-value		0.442		0.794
Observations	17539	17532	17539	17532
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓

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 1033 program. In column (3) and column (4), the dependent variable is the logged value of non-vehicle items. The FE-IV models use both instruments defined in equation 2. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

VI Discussion

We investigate the effect of aid-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 strong evidence that receipts of 1033 goods crowd out local spending on public safety. Virtually all of our estimated coefficients are positive, and most of instrumental variables results are significant or very close to significant. In other words, we find evidence of a perfect flypaper effect and perhaps some crowding in. In earlier work, Harris et al. (2017) find that 1033 receipts may enhance law enforcement capabilities in areas such as drug interdiction. While that paper does not offer a clear mechanism, it is unclear how tactical items in and of themselves will enhance drug interdiction capabilities. However, if 1033 receipts lead to substantive, crowding in, then 1033 receipts may indirectly lead to increases in overall capacity for the provision of public safety, similar to grant programs studied by Evans and Owens (2007) and Mello (2018).

However, these results are not always sufficiently precise to completely rule out any crowding out effects, and are somewhat sensitive in precision to instrument choice. Nevertheless, they indicate

Table 12: 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	0.265	-0.006	0.639*
	(0.012)	(0.432)	(0.012)	(0.347)
Kleibergen-Paap F-statistic		4.443		8.432
Hansen J Statistic P-value		0.292		0.772
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	8261	8255	9278	9277

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 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 **??** 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

that capital receipts through the 1033 program lead to much less crowding out than other sources of earmarked grants for transportation, law enforcement, and education. Estimated effects of intergovernmental transfers from recent economics papers are summarized in Table 13. While the first two estimates from this paper are elasticities rather than level-level effects, FE estimates are centered near zero, which is outside the 95 percent confidence interval for virtually all previous estimates. The FE-IV estimates are significant and bound the result away from zero. We also include the FE and FE-IV result from our level-level specification. This point estimate is also above zero, and the confidence interval implies that there is at most 30 percent crowding out, which is much less than most prior estimates in this literature. Additionally, the FE-IV results, while implausibly large in magnitude, are also statistically significant, favoring crowding in.

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

Table 13: Related research on intergovernmental transfers

Author(s)	Program/transfer type	Estimates
Knight (2002)	Federal Highway Aid	[-0.88(0.42), -1.33(0.53)]
Baicker and Jacobson (2007)	Police seizures	[-0.81(0.36), -1.3(0.39)]
Brooks and Phillips (2008)	Community Development Block Grant	[-0.66(0.31), -0.77(0.39)]
Lutz (2010)	New Hampshire's school finance reform	[-0.75(0.12), -0.98(0.19)]
Cascio, Gordon, and Reber (2013)	Introduction of Title I	-0.5(0.08)
Vegh and Vuletin (2015)	Fiscal transfers in Argentina	[-1.64(0.19), -1.7(0.22)]
Bruce et al (2018)	1033 Grants-in-Kind (FE)	[0.001 (0.009)]
Bruce et al (2018)	1033 Grants-in-Kind (FE-IV)	[0.562 (0.268)]
Bruce et al (2018)	1033 Grants-in-Kind (FE level-level)	[0.09 (0.21)]
Bruce et al (2018)	1033 Grants-in-Kind (FE-IV level-level)	[8.57 (5.02)]

Note: Ranges of estimates are given in the bracket. Standard errors of estimates are given in the parentheses.

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 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.

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