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The impact of preferential farmland taxation on local public finances

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Abstract

Preferential taxation of farmland, which typically takes the form of use-value assessment (UVA), is a staple of local property tax systems in the United States. With the exception of a handful of narrowly focused case studies, little is known about how the adoption of preferential farmland taxation shapes local government finances in subsequent post-policy periods. We leverage variation in the timing of UVA adoption across states to estimate how preferential tax treatment of farmland has affected local government revenue. Empirical results indicate that UVA adoption led to large and lasting net reductions in local tax revenue.

Keywords: Property taxation, use value assessment, public finance

JEL Codes: H71, Q14

1. Introduction

All 50 U.S. states provide property tax relief to farmland owners (Anderson and England, 2014). Farmland property tax relief programs were adopted at the state level between 1960 and 1995 with the goal of reducing the conversion of agricultural land to developed uses. Although the programs take a variety of forms, the most common type is use-value assessment (UVA). Under UVA, farmland is taxed according to its value in permanent agricultural use, rather than its full market value, which includes capitalized future development returns that have yet to be realized by the landowner (Plantinga et al., 2002). Proponents of UVA argue that by removing the capitalized future non-agricultural returns from assessed farmland values, landowners will face less pressure to convert their land to higher economic uses. Others, however, argue that lowering property taxes for agricultural landowners will require local governments to operate with lower expenditures or levy higher tax rates on the remaining property tax base (Anderson and Griffing, 2000). Despite its widespread adoption, there is little comprehensive empirical evidence pertaining to the influence of UVA on local government finance, which has implications for the provision of local public goods.

The paper examines the degree to which UVA shifts the property tax burden to other land uses or reduces the total tax base for local governments. Using state-level farmland tax expense data from DeBraal (1993) from 1940-1991, we first show that the adoption of farmland property tax relief through UVA coincided with a large reduction in state-level property tax expenses for farmland owners. Next, we exploit the variation in the timing of UVA adoption across states to estimate the net effects of UVA adoption on local tax revenue using data obtained from the Census of Governments. The Census of Government data spans 1957-2002, a period covering the adoption of UVA in all states (Anderson et al., 2015). Our empirical results suggest that UVA led to large and lasting net reductions in local tax revenue, which suggests that the revenue burden foregone through adoption of UVA was, at a minimum, not completely redistributed to other taxpayers.

The existing literature offers several case studies of the shift in tax burdens as a result of UVA. In a study of Coles County, Illinois, Chicoine and Hendricks (1985) find that UVA reduced taxes on agricultural properties by 45%, while taxes on non-farm properties increased by nearly 64%. In a more recent study of Wayne County, Ohio, Coogan et al. (2014) find a more pronounced reduction in the taxable property base for farmland of 69.7%. Further, through simulation analysis, the authors demonstrate that "every non-agricultural property type are better off when agricultural property is assessed at its market value" (Coogan et al., 2014, pp. 129). In a study of sub-county taxing authorities in Spokane County Washington, Dunford and Marousek (1981) also shows that increases in property taxes on other land use types are higher in areas with more farmland. The authors conclude that tax increases due to UVA may be relatively large in sub-county areas even when tax increases at the county aggregate level are quite small.

This study makes a number of important contributions to the existing literature. First, in contrast to the single-county case studies previously examined, our study relies on a national panel of county-level property tax information collected by the Census of Governments, which allows us to provide more comprehensive evidence on the effects that UVA adoption has had on local fiscal outcomes. Second, the empirical framework adopted in this paper yields a more robust identification strategy for estimating the impacts of UVA adoption. Lastly, we estimate both the static and dynamic effects of UVA policy, which provides a way to assess the persistence of any effects that UVA policy may have on local tax revenue.

2. Brief overview of preferential farmland taxation

Property tax is considered the "mainstay" of local government revenue (Bell, 2012). Real property tax is generally considered a "good" local tax because it produces stable revenues and, because of the match between local services received and payment for such services, it generally satisfies objectives of efficiency and equity (Bell, 2012).

As Bowman et al. (2009) outline, there are three policy levers available to jurisdictions that seek to extend preferential tax treatment to parcels based on use or other attributes, such as characteristics of the owner. The amount of taxes $T_{i,k}$ owed on parcel i with in the jurisdiction of the taxing body k can be expressed:

$$T_{i,k} = t_k a_k V_{i,k} \tag{1}$$

where t_k is the tax rate, a_k is the ratio of the assessed value to the market value, and $V_{i,k}$ is the market value. Thus, jurisdictions can extend preferential treatment through (i) preferential rates (t_k), (ii) preferential assessment (a_k), or (iii) partial or full exemptions.

Between 1960 and 1995, each state in the US adopted a policy that enabled use-value assessment of farmland by local tax assessors (Anderson et al., 2015). Although Wisconsin was technically the last state to adopt UVA in 1995, since 1974 they had been providing tax relief to farmland owners through a state income tax circuit breaker (Youngman, 2005). Michigan also employs a similar mechanism to provide farmland property tax relief. UVA policies vary along several dimensions, such as the income or acreage qualifications required for participation, whether or not qualifying farmland owners are automatically enrolled, and the presence of development penalties which require repayment of foregone tax expenses if the enrolled land is developed in the future. In addition to the small literature studying the effects of UVA on local public finance, other studies have examined how UVA affects farmland preservation (Morris, 1998; Bigelow and Kuethe, 2020), land values (Carman and Polson, 1971), farm investment (Conklin and Lesher, 1977; Bigelow and Kuethe, 2020), and the incidence of benefits derived from UVA (Dinterman and Katchova, 2019). In addition to farmland, preferential property tax policies in a number of states also apply to forestland, open space, and historical sites (Bell, 2012).

Many property tax jurisdictions employ "assessment limits" which, in various ways, constrain the rate of growth in the taxable value of property. The existing literature suggests that assessment limits lead to larger burdens on new homeowners because when transactions occur the taxable value is recomputed to reflect the current market value of the home (Haveman and Sexton, 2008). The impacts of assessment limits in the literature include Prop 13 in California (Sexton et al., 1999) and Prop 2 $\frac{1}{2}$ in Massachusetts (Wallin and Zabel, 2011). Bradley (2017) examines Michigan's assessment limit to show that house buyers overweight near-term tax savings and, consequently, overpay for houses by a nontrivial margin. The property tax is a tax on wealth, but it must be paid out of annual income (Bell, 2012). Chicoine and Hendricks (1985) note that assessment limits constrain taxing authorities ability increase taxes enough to fully offset base reductions spurred by farmland UVA. As a result, Chicoine and Hendricks (1985) demonstrate that UVA led to a substantial increase in state aid for rural school districts.

3. Preliminary analysis: Preferential farmland taxation and state-level farmland tax expenses

To show that preferential farmland tax policies coincided with meaningful changes to property tax expenses for farmland owners, we begin with a descriptive analysis of trends in several farmland tax variables. We emphasize that the purpose of this initial exercise is not to infer a causal effect of UVA policy. For example, we acknowledge that trends in farmland property tax expenses may have led states to adopt policies at different times. Rather, the following provides a descriptive backdrop for the primary objective of the paper, which is to estimate the causal effect of UVA policy on local government finances.

The following state-level analysis relies on a digitized version of the state-level farmland tax expense data from DeBraal (1993) covering 1940-1991.[1] Ideally, to conduct this baseline analysis we would use county-level data on farmland taxes paid by farmland owners. However, county-level data on farmland tax expenses was not collected in the Census of Agriculture prior to 1987. The Census data are thus of limited usefulness for our purposes since all but three states (Wyoming, Kansas, and Wisconsin) had adopted a preferential farmland tax policy by 1987.

In conducting our analysis, we remove observations from Alaska and Hawaii due to data limitations caused by their late (1959) admission to the US. We also remove observations from Wisconsin and Michigan because their preferential tax systems differ from those in other states. Wisconsin, in 1995, was officially the last state to adopt use-value assessment, though prior to that farmland owners in Wisconsin were given tax relief through an income tax circuit breaker. Similarly, Michigan provides allowances for income tax credits based on property

[1] Although the data are available back to 1890, we use a subset of the data beginning in 1940 to allow for at least 20 years of pre-policy data for each state, as Maryland, in 1960, was the first state to implement a preferential farmland tax policy.

taxes in excess of a certain percentage of household income. The preferential tax policies in Wisconsin and Michigan are similar in spirit to the UVA policies in other states, but their focus on income taxes makes them fundamentally different from a tax incidence and local public finance standpoint.

There are three farmland tax variables provided in DeBraal (1993): (1) farmland tax per-acre, (2) total farmland tax levy, and (3) average tax per $100 of full market value of agricultural property. The US inflation-adjusted total farmland tax levy rose dramatically in the aftermath of World War II until the early 1970s, as shown in Figure 1.[2] Total farmland tax levies plummeted throughout the 1970s, declining from a high of $6.1 billion in 1972 to $4.1 billion in 1986. As we next show, this striking decline in farmland tax levies coincided with the adoption of use-value assessment policies. Although all states currently have some form of UVA, 27 states adopted their policies over this 15-year period, a pattern that was partly driven by the rise in land values throughout the 1970s that foreshadowed the farm credit crisis of the 1980s.[3]

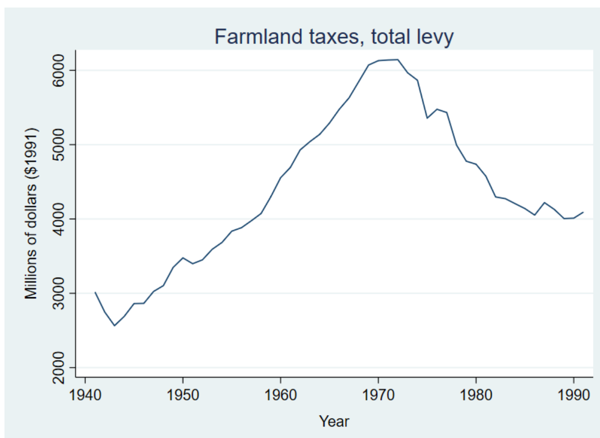


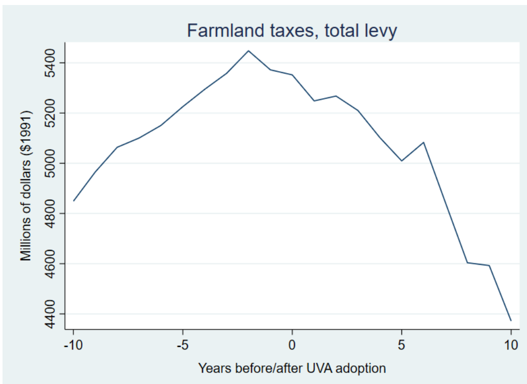
Figure 1: Total farmland tax levy, 1940-1991, in $1991

Figures 2a and 2b shows the same variable, total farmland taxes, plotted in "event time" with respect to each state's adoption of UVA. Negative values on the horizontal axis denote years prior to a state's policy going into effect and positive values represent post-UVA years. The figures are constructed such that there are an equal number of states included in each time period. Since the DeBraal (1993) data end in 1991, Figure 2a, which shows 10 pre- and post-UVA years, retains all states with a UVA adoption year of 1981 or sooner, while Figure 2b includes a more restrictive sample of all states with an adoption year of 1971 or sooner. Adoption of UVA coincides with a clear and consistent drop in total farmland tax levies across both event time samples. However, it appears that it takes several years for the full effect of the

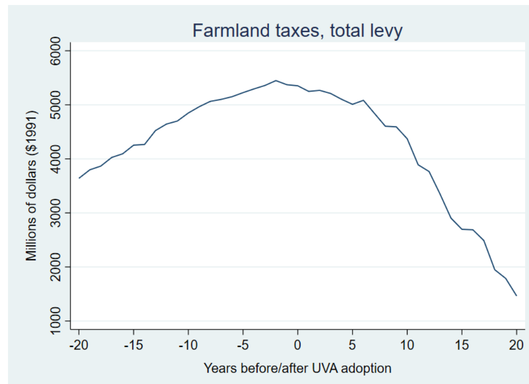
[2]All values in this section are adjusted for inflation to the year 1991 using the GDP implicit price deflator from the St. Louis Federal Reserve: `https://fred.stlouisfed.org/series/GDPDEF`.

[3]We use the UVA adoption years for different states given in Anderson et al. (2015), which were compiled from state records.

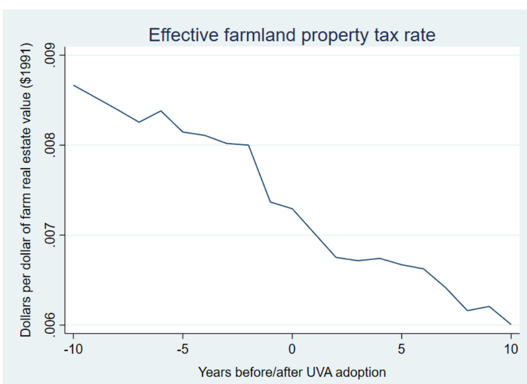
policies to be reflected in farm property tax expenses. These lag trends in tax expenses with respect to preferential tax policy timing are due to the fact that the adoption of UVA legislation is not always immediately followed by implementation of a concrete preferential assessment methodology.[4] In addition, the figures make clear how these policies were implemented, in part, to head off rising property tax payments for farmers, as UVA adoption is preceded by a period of rising tax payments.



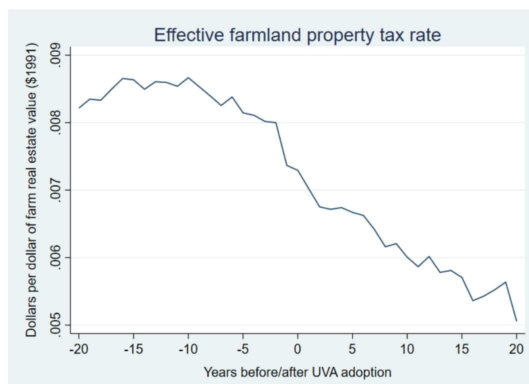
(a) Total farmland tax levy, 10-year UVA event time window



(b) Total farmland tax levy, 20-year UVA event time window



(c) Effective farmland tax rate, 10-year UVA event time window



(d) Effective farmland tax rate, 20-year UVA event time window

Figure 2: Trends in farm real estate value taxation, UVA adoption event time

The mechanism through which UVA operates is by reducing the effective farmland property tax rate, which is calculated as the ratio of taxes paid to total farmland market value. Effective farmland tax rates are plotted in Figures 2c and 2d, again with the same balanced event time samples. The average tax rates shown in the figures are weighted by total farmland acreage.Passage of UVA coincides with a meaningful reduction in the effective farm real estate tax rate. In addition, it appears that the average effective tax rate began declining several

[4]For example, Kansas passed legislation allowing for use-value assessment in 1985, but did not formally implement their policy until 1989.

years prior to UVA being adopted, which, again, we presume is due to discrepancies in the years of UVA implementation and legislation in each state. Although farmland taxes were low, averaging roughly 0.8-0.85% prior to UVA going into effect, Figures 2a and 2b show how small rate changes can translate to billions of dollars of reduced tax expenses, losses which are ultimately borne by local governments.

4. Data sources

Our primary analysis is conducted at the county level using data collected from a variety of sources covering the years 1957-2002. Local public finance variables, including total and per-capita measures of total tax revenue, property tax revenue, and non-property tax revenue, come the Census of Governments (CG).[5] The longest and most geographically comprehensive version of the county-level CG public finance data comes from the County Area Finance (CAF) database, which is available in five-year increments starting in 1957.[6] The local public finance variables included in the CAF database are inclusive of all local governments operating within a county. For example, the total property tax measure includes property taxes levied by the county government, but also any additional taxes levied by municipalities, towns, and any special districts operating within the county boundaries. The CAF database also contains more refined measures of the revenues collected by each distinct type of governmental entity operating within a county, but these are not as widely available and are missing for many years and counties.

For each outcome variable from the CAF data, the sample used in estimation comprises a balanced panel of counties that have non-missing, positive reported values in all 10 years of our study timeframe. We cannot account for zeroes because our dependent variables are log-transformed. Since different variables have different patterns of missingness, the estimation samples are different for each outcome variable. Due to data limitations, counties in Alaska and Hawaii are also excluded, as are those in Michigan and Wisconsin because of the aforementioned differences in the nature of UVA policies in those states. Since the data begin in 1957, an accurate accounting for pre-trends requires that we observe at least two pre-UVA adoption periods for each observation. The only two states with adoption years of 1962 or sooner are Maryland and Hawaii, and, accordingly, observations from Maryland are also excluded from the estimation samples.

Data on farm-related control variables come from historical Census of Agriculture records. These variables include the total area of the county, the share of land considered to be farmland, and the share of farmland used to grow crops. The share of land denoted as farmland is used as a weighting and screening variable, as we would expect the aggregate tax effect of preferential

[5]All values in this section are adjusted for inflation to the year 2017 using the GDP implicit price deflator from the St. Louis Federal Reserve: `https://fred.stlouisfed.org/series/GDPDEF`.

[6]Historical data from the Census of Governments may be found at: `https://www.census.gov/programs-surveys/gov-finances/data/historical-data.html`.

farmland taxation to be bigger in counties where a larger share of the property tax base comes from farmland property. Cropland share is important since cropland generally has a much higher value than land used to graze livestock, which suggests that the potential reduction in value should increase with the amount of farmland used to grow crops.[7] Demographic controls come from the decadal Census of Population. These include the share of housing units that are owner-occupied, share of residents that are non-white, median income, and median age. Both the Census of Agriculture and Census of Population data are not available at the same temporal frequency as the CAF data.[8] We use the most recent prior value of each variable to match the temporal availability of the CAF data. For example, for each 1962 CAF observation, we use the 1959 values of the Census of Agriculture controls and the 1960 values of the Census of Population controls.

5. Empirical framework

We estimate both static and dynamic effects of UVA on the tax revenue of local county governments. Our baseline static results come from the following model specification:

$$ln(y_{ct}) = \beta_0 + \beta_1 UVA\ Policy_{c(s)t} + \beta_{\mathbf{2}}^{'}\mathbf{X_{ct}} + \nu_t + \alpha_c + \epsilon_{ct}, \tag{2}$$

where *UVA Policy* is a staggered binary treatment variable which takes a value of 0 for each county prior to the year the state (s) in which the county is located adopted UVA and 1 thereafter. The UVA treatment is therefore "absorbing", in the sense that states' decisions regarding UVA policy are irreversible. Time-varying control variables are denoted by $\mathbf{X_{ct}}$. The terms ν_t and α_c denote year and county fixed effects, respectively. Outcome variables $ln(y_{ct})$ include per capita measures of various types of tax revenue, which are discussed in further detail below.[9]

For β_1 in equation (2) to identify the impact of preferential farmland taxation on local public revenues, it must be assumed that, conditional on agricultural and demographic controls ($\mathbf{X_{ct}}$), county fixed effects (α_c), and common time effects (ν_t), any additional changes in local revenue are due to the adoption of preferential farmland taxation. To relax this assumption, we enhance the model by including a set of state-specific linear trend variables:

$$\begin{aligned} ln(y_{ct}) = \beta_0 + \beta_1 UVA\ Policy_{c(s)t} + \beta_{\mathbf{2}}^{'}\mathbf{X_{ct}} + \\ \beta_3 1\{State_c = s\}t + \nu_t + \alpha_c + \epsilon_{ct}, \end{aligned} \tag{3}$$

[7] Cropland may also be more likely to be located in closer proximity to urban areas, which would also increase the property tax break stemming from use-value assessment.

[8] The Census of Population data we use range from 1950-2000 in ten-year increments. The Census of Agriculture is available for the following years: 1959, 1964, 1970, 1974, 1978, 1982, 1987, 1992, 1997, and 2002.

[9] The per capita tax measures are constructed using the population reported in the CAF data, rather than the Census of Population.

where $1\{State = s\}t$ is a linear time trend for state s. The state-specific time trends allow for the possibility that other changes in state policy may have affected local public finance outcomes during our period of study. We refer to equations 2 and 3 as static difference in differences (DD) models, as they estimate a time-constant, homogeneous average UVA effect, β_1, for all counties (Borusyak and Jaravel, 2017; Abraham and Sun, 2020). For the static DD models to identify the effect of UVA policy adoption on local public finance outcomes, the standard parallel trends assumption must apply. Specifically, the trend in the public finance outcome being considered must be similar between UVA adopters and non-adopters prior to the passage of each UVA policy. In the static DD models, we test for this by including a leading indicator for $UVA\ Policy_{c(s)t}$, which should be small and statistically insignificant if the parallel trends assumption is valid and no pre-trends exist.

To estimate a more flexible version of the model, which relaxes the time invariance of β_1 and generates dynamic effects of UVA policy (Borusyak and Jaravel, 2017; Abraham and Sun, 2020), we adopt an event study specification, which looks as follows:

$$\begin{aligned} ln(y_{ct}) = \beta_0 + \sum_{i \in PRE} \gamma_i 1\{t^*_{c(s)} - t = i\}_{c(s)t} + \sum_{i \in POST} \pi_i 1\{t - t^*_{c(s)} = i\}_{c(s)t} + \\ \beta_2' \mathbf{X_{ct}} + \beta_3 1\{State_c = s\}t + \nu_t + \alpha_c + \epsilon_{ct}, \end{aligned} \tag{4}$$

where $1\{t^*_{c(s)} - t = i\}_{c(s)t}$ and $1\{t - t^*_{c(s)} = i\}_{c(s)t}$ are indicator variables for the number of years prior to ("PRE") and since ("$POST$") preferential farmland taxation was adopted in a county's state ($c(s)$). Given the quinquennial frequency of the CAF data, each event time indicator is defined based on five-year increments. The first $POST$ period includes the year of adoption up through the first four post-adoption years, the second $POST$ period includes post-adoption years 5-9, and so on. All effects are interpreted relative to the omitted reference group, the first PRE period, which comprises the five years prior to each state's preferential tax policy going into effect.

The source of identification in the event study models is the same as that in the static DD models, namely state-level variation in the timing of when UVA policies were adopted. As noted above, a threat to this identification strategy stems from non-parallel pre-UVA trends in local public finance outcomes between adopters and non-adopters. An advantage of the event study specification is that it provides a convenient way to test for the presence of pre-trends by checking the sign and precision of the PRE event time dummy variables.[10] The event study specification also allows us to consider the dynamics of the UVA policy impact on local public finance outcomes, concerning, for example, how long it took for the effects to emerge and how

[10]We note, however, that several new, unpublished studies have pointed out the inadequacy of using this test to detect the presence of pre-trends in research applications involving staggered policy adoption due to the presence of negative, non-convex weights (Abraham and Sun, 2020; Goodman-Bacon, 2019; Callaway and Sant'Anna, 2019; Borusyak and Jaravel, 2017). For the present case, these concerns may be relevant, as a majority of our identifying variation stems from the use of previously treated observations as controls. In future versions of the paper, we plan to more fully explore the implications of this feature of our research design.

long they persisted.

We analyze the impact of UVA policies on three measures of per-capita tax revenue: total tax revenue from all sources, total property tax revenue, and total non-property tax revenue. Using total tax revenue from all sources provides the most comprehensive estimate of how the UVA policies affected local public finances. As some have suggested, the implementation of UVA may lead to a shift int he property tax burden the owners of non-agricultural land. If the foregone tax revenue collected from agricultural landowners was shifted to other property owners, we would fail to reject the null hypothesis that the estimates of β_1 in the static DD models and π_i in event-study models are different from zero. The effects of UVA should mainly operate through property taxes. However, it is also possible that, rather than shifting the tax burden to non-agricultural property owners, local governments raised local income or sales taxes to offset the UVA-induced revenue drop. The estimates based on using total taxes as the outcome variable are most comprehensive, and capture combined effects on both property and non-property tax revenue.

6. Main results: Effects of preferential farmland taxation on local public finances

In this section we present the parameter estimates for equations (2), (3), and (4). All models contain county and year fixed effects. Standard errors are clustered by state to reflect the scale at which the UVA policies were adopted. For each outcome variable, we present results for a sample of counties where at least 25%r of the county land area is classified as farmland by the USDA's Census of Agriculture in each of the ten study years. In addition, all results presented in this section are based on models estimated using farmland share weights, where farmland share is defined as the 1957 fraction of the county area identified as farmland in the Census of Agriculture. We test the sensitivity of these results to the use of weights and alternative farmland share restrictions in a set of robustness checks.

The static DD estimates indicate that adoption of UVA led to a decline in local public tax revenue. The first column of panel (a) in Table (1) shows that the effect of UVA on total tax revenue is negative but not statistically different from zero.[11] After including state-level trend variables, which relax the assumption of common temporal patterns in total tax revenue across states, the estimate in the second column indicates that adoption of UVA leads to a reduction in per-capita total tax revenue of 7.4%.[12] As shown in panel (b), this reduction in tax revenue came about due to a decline in property tax revenue amounting to roughly 9.4% (after accounting for state-level trends). In addition, it does not appear that the loss of property tax revenue was offset by increases in revenue from other taxes (panel (c)).

[11]Estimates for the control variables can be found in Appendix Table A1.

[12]Since the dependent variable is logged, we use the method in Kennedy (1981) to compute the marginal effect of the UVA treatment variable.

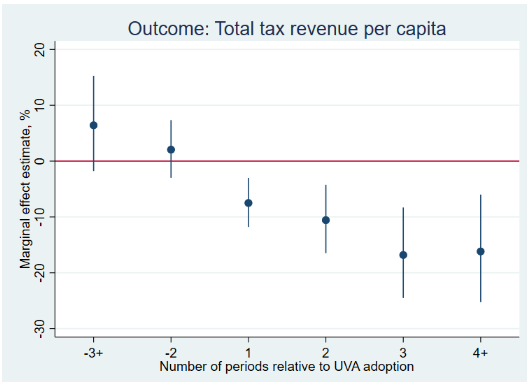
Table 1: Static difference-in-differences estimates, county tax revenue

	(1)	(2)	(3)
(a) Total tax revenue			
UVA policy	-0.087	-0.080	-0.079
	(0.058)	(0.034)**	(0.031)**
UVA policy, lead			-0.005
			(0.023)
Observations	20,110	20,110	20,110
(b) Property tax revenue			
UVA policy	-0.132	-0.104	-0.099
	(0.065)**	(0.038)***	(0.035)***
UVA policy, lead			-0.019
			(0.021)
Observations	20,110	20,110	20,110
(c) Non-property tax revenue			
UVA policy	0.027	0.037	-0.054
	(0.143)	(0.145)	(0.118)
UVA policy, lead			0.182
			(0.111)
Observations	19,940	19,940	19,940
County FE	Yes	Yes	Yes
State-specific linear trends	No	Yes	Yes

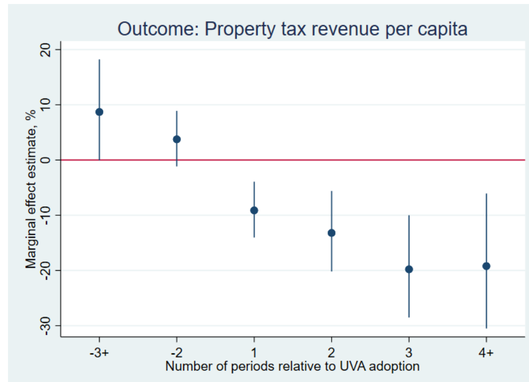
Notes: Panels (a), (b), and (c) in the above table represent models estimated with a different log-transformed, per-capita local public finance outcome variable. In addition to the UVA policy dummy variables, the following control variables are also included: % of farmland cropped, median age, % of population non-white, % of dwellings owner-occupied, and % of population school-age. Estimates for the control variables are reported in Table A1 of the supplementary appendix. Standard errors clustered by state are reported in parentheses. Asterisks denote significance at the 10% (*), 5% (**), and 1% (***) levels.

The validity of the static estimates hinges on the assumption of parallel trends in tax revenue between counties in different states prior to the adoption of UVA policies. To test whether or not this holds, we include a leading UVA policy dummy variable, which is measured in the same way as the actual UVA policy variable, but using an adoption year that is five years sooner than the actual adoption year for the state in which the county is located. In the case of total tax revenue and property tax revenue (cols. 3 and 6), we find that the UVA lead has a very small and statistically insignificant effect. For non-property tax revenue, the UVA lead has a large, though insignificant, positive effect.

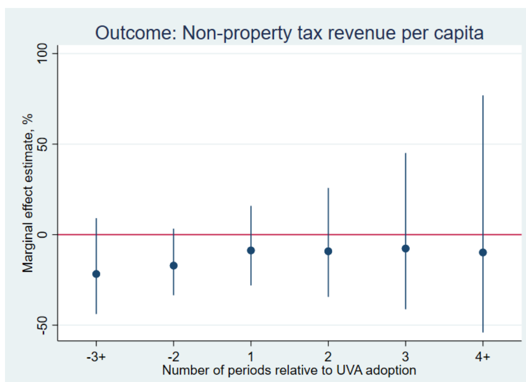
To explore the dynamic effects of UVA adoption on tax revenue, we now present results from the event study specifications, which illustrate how tax revenues evolved in the aftermath of the UVA policies being adopted. In the specifications shown here, the PRE event-time dummy variables are capped in the third period prior to UVA adoption and the $POST$ dummy variables are capped in fourth period after UVA adoption. The uncapped event-time dummy variables represent the periods over which the sample is balanced; i.e., we observe at least two pre-UVA outcomes and three post-UVA outcomes for all counties. The estimates, calculated as marginal effects in percentage terms, are displayed in Figure 3.



(a) Total tax revenue



(b) Property tax revenue



(c) Non-property tax revenue

Figure 3: Event study specification results, tax revenue

In general, the event study estimates corroborate the findings of the static DD models. Total tax revenue declines following implementation of the UVA policies (Figure 3a). The UVA policy effect increases in magnitude as one moves further from the time at which the policies were adopted, peaking at 15% in period 3 (10-14 years post-UVA). Event study estimates for property tax revenue show a nearly identical pattern, but with larger effect sizes that peak at 19% in period 3 (Figure 3b). In both cases, none of the pre-UVA event time dummy variables yield a significant effect on revenues, suggesting that the parallel trends assumption holds. This pattern of increasing effects can be explained by (1) the phased-in implementation of UVA by local governments after the initial enabling legislation was passed and (2) the fact that, in some states, such as California, UVA participation by landowners is voluntary. In both cases, the effect of UVA on local revenue would increase as participation becomes more widespread. Non-property tax revenues again do not show a significant relationship with the adoption of UVA policies (Figure 3c). Overall, these results highlight how the adoption of UVA, which provided preferential tax treatment of farmland throughout the US, led to economically significant reductions in county tax revenue that have persisted, on average, for at least 15 years after the policies went into place.

6.1. Robustness checks

In this section, we explore the robustness of our results to the use of weights and alternative estimation samples. Results for the robustness checks are presented in the supplementary appendix.

6.1.a. *Unweighted estimates.–* The models in the previous section were all estimated using weights based on the 1957 share of each county accounted for by farmland. Following Solon et al. (2015), as a first set of robustness checks, we reestimate each model without farmland share weights. The unweighted static county revenue estimates show minimal change relative to their weighted counterparts (Table A2), as is also the case for the dynamic tax revenue model estimates (Figure A1). One difference in the dynamic specifications, however, is that the capped pre-UVA dummy variable is positive and significant in the property tax model.

6.1.b. *Sample construction: Alternative farmland share thresholds.–* Counties were included in the baseline estimation samples if at least 25% of the county area farmland throughout the 1957-2002 study period. In this section we reestimate the models (1) without any farmland share restriction and (2) using a more stringent 50% farmland share threshold. Static specification results for the alternative farmland share sampling thresholds are given in Table A3. Results from these alternative samples are broadly consistent with those from the baseline 25% threshold sample. UVA policies have a negative effect of roughly 7% on total tax revenue and 9% on property tax revenue without the farmland share restriction, and 9% and 10% when using the 50% threshold. In both cases, moreover, the leading UVA indicator remains small and insignificant. Non-property tax revenue still does not show a statistically significant relationship with the UVA policy dummy variable. There is weakly significant evidence, however, that UVA policies were implemented in areas with higher pre-UVA non-property tax revenues. The dynamic model estimates show similar patterns to those found with the baseline sample (Figure A2 and Figure A3). One important difference, however, is that we find some evidence of pre-trends in property tax revenue with unrestricted sample (Figure A2b), suggesting that, among the full sample of counties, county-level property tax revenue was higher in adopting states prior to the policies going into effect. As it relates to studying the effects of UVA, this suggests that including counties with little farmland creates a poor balance of treatment and control counties over the study period and violates the parallel trends assumption.

7. Discussion and conclusions

Overall, the estimates provide robust evidence that the adoption of use-value assessment caused a persistent decline in county-level tax revenue. We are able to most definitively make this claim for counties that contain a nontrivial amount of farmland. In addition to being statistically precise, the estimates are also nontrivial from an economic standpoint. For example, using the dynamic specification and baseline sample, the initial five-year period following implementation of UVA caused a 7.5% decline in per-capita tax revenue. Using the average values of per-capita taxes ($726), population (36,890), and total tax revenue ($27.4 million) corresponding to the first pre-UVA event time period, the estimate corresponds to an average revenue loss of $2 million (7.3%; in 2002 $USD). The estimation results indicate that the effect of UVA on per-capita tax revenue grows over post-UVA event time. By the third post-treatment period, adoption of UVA resulted in a 16.8% decline in per-capita revenue, amounting to an average total tax revenue loss of $4.5 million (16.4%). For context, among the counties that reported expenditure measures over the same period, the implied post-UVA third period losses are equivalent to 13% of total public education expenditures, 343% of total fire protection expenditures, or 174% of total police protection expenditures. The UVA treatment effects are therefore quite substantial from a public expenditure standpoint.

In terms of tax burden redistribution, our estimates represent net effects and indicate that the tax loss stemming from preferential treatment of farmland was not perfectly reallocated to non-farmland property owners. We cannot rule out that some redistribution may have occurred, however. As Dunford and Marousek (1981) demonstrate, county aggregates may mask significant sub-county variations in the distribution of program costs to non-participating property owners. That is, tax increases due to UVA may be relatively large in sub-county areas even when tax increases at the county aggregate level are quite small. For example, if, in the first post-UVA event time period, tax savings accruing to farmland owners exceeded 7.5%, our results imply that the balance was shifted elsewhere, either through property tax increases to non-farmland owners or increases in other types of taxes. County-level tax revenue data for specific types of property owners is not available for the entirety of our study timeframe, which precludes a more precise decomposition of the tax effects to different groups of taxpayers.

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Appendix

Table A1: Static difference-in-differences effects, county tax revenue, full specifications

	Total tax revenue			Property tax revenue			Non-property tax revenue		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
UVA policy	-0.079	-0.076	-0.074	-0.129	-0.103	-0.099	0.082	-0.011	-0.052
	(0.058)	(0.034)**	(0.031)**	(0.064)*	(0.036)***	(0.034)***	(0.140)	(0.113)	(0.119)
% farmland in crop use	0.051	-0.103	-0.102	0.001	-0.105	-0.102	0.111	-0.461	-0.485
	(0.171)	(0.093)	(0.092)	(0.198)	(0.091)	(0.091)	(0.666)	(0.378)	(0.376)
Median age	-0.011	-0.002	-0.002	-0.004	-0.001	-0.001	-0.045	-0.007	-0.006
	(0.006)*	(0.004)	(0.004)	(0.007)	(0.004)	(0.004)	(0.022)*	(0.008)	(0.008)
% population non-white	-0.006	-0.005	-0.005	-0.006	-0.006	-0.006	-0.010	-0.003	-0.003
	(0.004)	(0.002)***	(0.002)***	(0.004)	(0.002)***	(0.002)***	(0.007)	(0.003)	(0.003)
% dwellings owner-occupied	0.012	-0.001	-0.001	0.010	-0.001	-0.001	0.003	-0.005	-0.005
	(0.003)***	(0.001)	(0.001)	(0.003)***	(0.001)	(0.001)	(0.012)	(0.004)	(0.004)
% population school-age	-0.041	-0.014	-0.014	-0.031	-0.017	-0.017	-0.076	-0.019	-0.019
	(0.006)***	(0.004)***	(0.004)***	(0.006)***	(0.004)***	(0.004)***	(0.021)***	(0.013)	(0.013)
UVA policy, lead			-0.005			-0.020			0.178
			-0.022			(0.020)			(0.107)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific linear trends	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	20,110	20,110	20,110	20,110	20,110	20,110	19,940	19,940	19,940
Counties	2,011	2,011	2,011	2,011	2,011	2,011	1,994	1,994	1,994

Notes: Dependent variables are included in logged, per-capita form. Standard errors clustered by state are reported in parentheses. Asterisks denote significance at the 10% (*), 5% (**), and 1% (***) levels.

Table A2: Static difference-in-differences estimates, county revenues, no weights

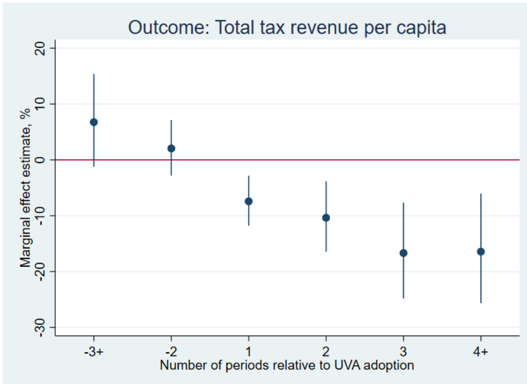
	(1)	(2)	(3)
(a) Total tax revenue			
UVA policy	-0.079	-0.076	-0.074
	(0.058)	(0.034)**	(0.031)**
UVA policy, lead			-0.005
			(0.022)
Observations	20,110	20,110	20,110
(b) Property tax revenue			
UVA policy	-0.129	-0.103	-0.099
	(0.064)*	(0.036)***	(0.034)***
UVA policy, lead			-0.020
			(0.020)
Observations	20,110	20,110	20,110
(c) Non-property tax revenue			
UVA policy	0.082	-0.011	-0.052
	(0.140)	(0.113)	(0.119)
UVA policy, lead			0.178
			(0.107)
Observations	19,940	19,940	19,940
County FE	Yes	Yes	Yes
State-specific linear trends	No	Yes	Yes

Notes: Panels (a), (b), and (c) in the above table represent models estimated with a different log-transformed, per-capita local public finance outcome variable. In addition to the UVA policy dummy variables, the following control variables are also included: % of farmland cropped, median age, % of population non-white, % of dwellings owner-occupied, and % of population school-age. Standard errors clustered by state are reported in parentheses. Asterisks denote significance at the 10% (*), 5% (**), and 1% (***) levels.

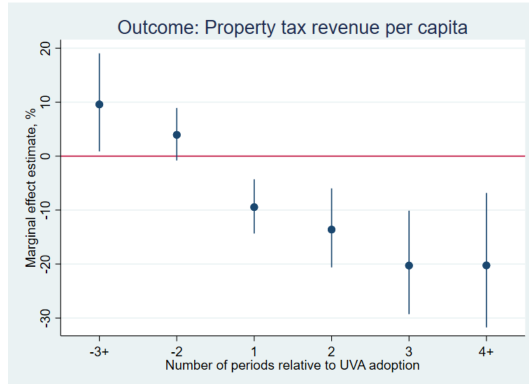
Table A3: Static difference-in-differences estimates, county revenues, alternative farmland share thresholds

	Minimum farmland share = 0			Minimum farmland share = 0.5		
	(1)	(2)	(3)	(4)	(5)	(6)
(a) Total tax revenue						
UVA policy	-0.069	-0.073	-0.071	-0.118	-0.094	-0.093
	(0.055)	(0.031)**	(0.029)**	(0.059)*	(0.037)**	(0.033)***
UVA policy, lead			-0.006			-0.006
			(0.021)			(0.029)
Observations	27,490	27,490	27,490	13,300	13,300	13,300
(b) Property tax revenue						
UVA policy	-0.117	-0.098	-0.093	-0.149	-0.105	-0.102
	(0.060)*	(0.034)***	(0.032)***	(0.070)**	(0.044)**	(0.040)**
UVA policy, lead			-0.023			-0.011
			(0.019)			(0.026)
Observations	27,490	27,490	27,490	13,300	13,300	13,300
(c) Non-property tax revenue						
UVA policy	0.069	-0.023	-0.063	-0.078	-0.175	-0.234
	(0.130)	(0.108)	(0.114)	(0.178)	(0.143)	(0.145)
UVA policy, lead			0.173			0.222
			(0.107)			(0.125)*
Observations	27,230	27,230	27,230	13,220	13,220	13,220
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific linear trends	No	Yes	Yes	No	Yes	Yes

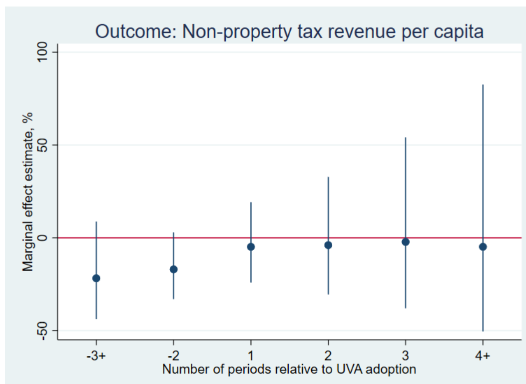
Notes: Panels (a), (b), and (c) in the above table represent models estimated with a different log-transformed, per-capita local public finance outcome variable. In addition to the UVA policy dummy variables, the following control variables are also included: % of farmland cropped, median age, % of population non-white, % of dwellings owner-occupied, and % of population school-age. Standard errors clustered by state are reported in parentheses. Asterisks denote significance at the 10% (*), 5% (**), and 1% (***) levels.



(a) Total tax revenue

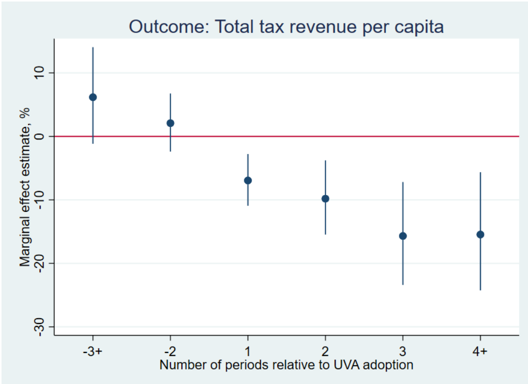


(b) Property tax revenue

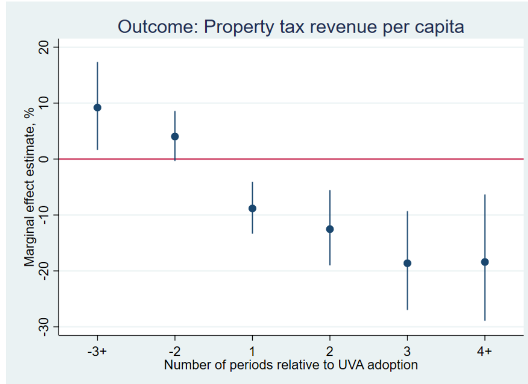


(c) Non-property tax revenue

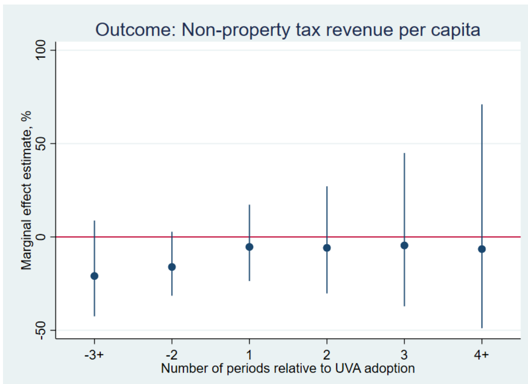
Figure A1: Event study specification results, county tax revenue, no weights



(a) Total tax revenue

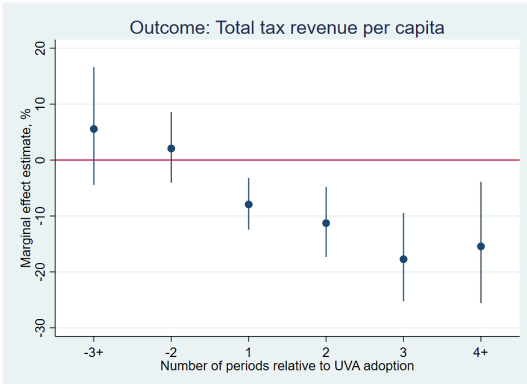


(b) Property tax revenue

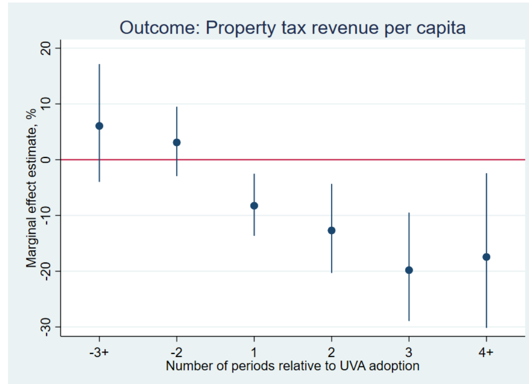


(c) Non-property tax revenue

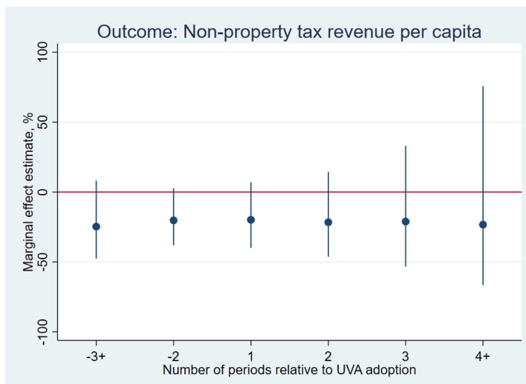
Figure A2: Event study specification results, county tax revenue, no farmland share sample restriction



(a) Total tax revenue



(b) Property tax revenue



(c) Non-property tax revenue

Figure A3: Event study specification results, county tax revenue, 50% farmland share sample restriction