Intergovernmental Cooperation and Tax Enforcement*

Ugo antonio Troiano

University of California Riverside

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Abstract

Improving the efficiency of tax collection is essential for both developmental and fairness purposes. I analyze the Audit Exchange Information Agreements, which are agreements established between the states and the U.S. federal government to share information on income tax audit plans and techniques. These agreements were signed between the 1950s and the 1970s. Using a doubly robust difference-in-differences approach, I demonstrate that the program increased state income tax revenues by approximately 20 percent. Furthermore, I find no evidence that the policy caused outmigration, suggesting that either the salience of the policy or the fixed costs of mobility deterred an extensive margin response.

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Keywords: fiscal federalism, tax compliance, audit.

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1 Introduction

The ability to collect taxes efficiently and fairly is one of the key determinants of state capacity and, consequently, economic growth (Besley and Persson, 2013). In this paper, I study the Audit Exchange Information Agreements, which are agreements between U.S. states and the federal government to exchange information about income tax audit plans and techniques. These agreements were signed in a staggered fashion between the 1950s and the 1970s, with both parties voluntarily participating.

The primary hypothesis I examine is whether, and under what circumstances, these agreements increased income tax revenues. There are three main reasons why such agreements may fail to achieve their intended outcomes. First, transaction costs may hinder their effectiveness. These agreements required coordination between the state and federal fiscal authorities, which operate under different bureaucratic structures. The agreements had implications for employees at the IRS as well as those at the revenue agencies of the involved states (often referred to as Departments of Revenue). Employees tasked with cooperation faced the challenge of navigating two distinct institutional settings. Second, increased enforcement might prompt individuals to relocate to avoid additional taxation, thereby nullifying or even worsening revenue outcomes. For example, Cassidy, Dincecco, and Troiano (2024) show that the introduction of state income tax in the U.S. led to outmigration to states with little or no income tax, such as Florida. Third, the agreements may not always be adhered to. For instance, while balanced budget rules are often respected within individual countries, they are less likely to be upheld between countries, as evidenced in the case of European nations (Alesina and Perotti, 1999).

A simple cross-sectional comparison of states that adopted these agreements before and after implementation may be tempting but is problematic. States that adopt such policies may differ systematically from those that do not. For example, states and the federal government may be more willing to cede jurisdictional control during periods of fiscal strain. Furthermore, omitted variables, such as the fiscal discipline of political leaders, could bias the results.

To address these concerns, I employ a difference-in-differences strategy. This approach compares income tax revenues in states that signed the agreements with those that did not, both before and

after implementation. Two key assumptions underpin this methodology: (1) the outcomes of treated and control states must follow parallel trends prior to treatment, diverging only afterward, and (2) no contemporaneous state-specific events unrelated to the agreements should differentially affect the treatment and control groups. The latter assumption is challenging to verify and requires a nuanced understanding of the institutional context during the relevant period.

I find that income tax revenues increased by approximately 20 percent following the agreements. There is no evidence of extensive margin responses to the policy. However, the main estimate might not capture a causal parameter due to the limitations of Ordinary Least Squares (Borusyak and Jaravel, 2017; de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2022). To address this, I apply the doubly robust difference-in-differences estimator proposed by Callaway and Sant'Anna (2020) and Sant'Anna and Zhao (2020), confirming the robustness of the results. Additionally, I show that pre-trends do not drive the findings.

Finally, I demonstrate that the results are robust to alternative explanations, by controlling for the introduction of state income tax withholding and state income tax laws, which were the major contemporaneous treatments during the same period. Together, these results suggest that the audit exchange agreements effectively increased state capacity.

The findings align with canonical models of tax evasion and enforcement, such as Allingham and Sandmo (1972), where the agreements can be interpreted as a reduction in enforcement costs. In this framework, the intervention raises tax revenues without increasing the total enforcement budget.

To my knowledge, this is the first paper to focus on intergovernmental audit exchange agreements within a single country. The study bridges the literature on tax enforcement, the historical evolution of U.S. tax policy, and fiscal federalism. It also contributes to the broader tax enforcement literature, which derives from Becker's (1968) economics of crime framework and Allingham and Sandmo's (1972) adaptations for tax evasion and enforcement. This paper shows that cooperation across layers of government can yield substantial increases in tax revenues without prompting outmigration or reduced economic activity.

Empirical tax enforcement literature focuses on information disclosure and auditing. While no

prior studies specifically address intergovernmental information sharing, a growing body of work explores related themes. Perez-Truglia and Troiano (2018) find that publicly exposing tax delinquents increases compliance by heightening the salience of delinquency to neighbors. Naritomi (2019) highlights how consumer receipts improve tax compliance through one-way information flows. Regarding auditing, Kleven et al. (2011) show that increased audit probabilities in Denmark significantly boost reported income, particularly among the self-employed, while Advani et al. (2023) find lasting compliance effects from random audit programs in the U.K. Pomeranz (2015) documents the role of information and enforcement in the context of Chilean firms.

In the historical literature, Cassidy, Dincecco, and Troiano (2024) find that the introduction of state income tax did not increase total government revenue due to outmigration. Bagchi and Dušek (2021) show that state income tax withholding increased revenues and altered budget compositions. This paper contributes by demonstrating that fiscal shocks, interpreted as reductions in enforcement costs, raise revenues without altering total budgets.

Within fiscal federalism, this paper is unique in examining two-way intergovernmental agreements. The closest related literature concerns fiscal rules, which impose budget constraints on local governments (Poterba, 1999; Wyplosz, 2012). Studies generally find that fiscal rules improve discipline, though their effectiveness can vary (Alesina and Perotti, 1999). Finally, this paper also connects to international treaty literature, such as Johannesen and Zucman's (2014) work on intercountry tax information agreements, which reduce deposits in tax havens.

The paper is organized as follows: Section 2 presents the institutional framework and data, Section 3 describes the empirical strategy, Section 4 reports the results, and Section 5 concludes.

2 Institutional framework and Data

In this section, I present the foundational concepts necessary to understand intergovernmental relations in state income tax administration in the United States. Following that, I discuss the dataset compiled for the analysis.

2.1 Audit Exchange Agreements

Cooperative federalism in tax administration requires actions by both the national and state legislative bodies, as well as joint efforts by the IRS and the states' departments of revenue. Since the introduction of the income tax, Penniman (1980) claims that cooperation between the states and the federal government for the enforcement of state income taxes has steadily increased. Interestingly, even in the early years of the 1920s, federal individual, joint, partnership, estate, and trust returns were freely available upon request by state governments, provided that "the request was for investigating issues arising from state income tax only." Penniman (1980) notes that by the 1930s, all but three income tax states had utilized this opportunity for tax enforcement purposes.

The first mention of formal agreements occurred during a joint federal-state conference in the 1940s, where a formal exchange of information on audit plans and techniques was recommended. The first states to adopt such a formal signed agreement were North Carolina and Wisconsin in 1950, followed shortly by Colorado, Kentucky, and Montana in 1951 and 1952.

The U.S. Treasury's 1952 Coordination Study (Washington D.C.: Government Printing Office) described the program as follows:

> Under the procedure adopted for the two initial projects, the examining officers in the offices of collectors and revenues agents-in-charge prepare abstracts of audit information for each changed return showing a deficiency in tax. The abstracts are prepared in longhand by the examining officer at the time his report of examination is made and are attached to the face of the return. After the deficiencies have been listed for assessment, the abstract is detached and forwarded to the State tax authorities. The states procedure with respect of furnishing the abstracts to the federal government is similar to the Federal practice.3 After the 1950s, the technological developments helped lowering the costs of cooperative agreement, and the audit agreements spread so fast that in less than 20 years the great majority of the income tax states had signed an audit information agreement with the federal government.

From the previous two paragraphs, it should be clear to the reader that these agreements are distinct from a hierarchical interaction. The agreements involve equal duties and responsibilities. Penniman

(1980) qualitatively shows that state audit tax collections resulting from IRS revenue agent reports totaled more than 50 million dollars, while the IRS has never provided statistics on the value of its recoveries based on state audit information, as it has never consistently maintained such data.

One of the main advantages of such agreements is the division of auditing work, which lowers enforcement costs. For instance, the Colorado and Minnesota agreements in the 1950s "explicitly provided that the state would assume audit responsibility for the lower-income returns, whereas the IRS would audit only the larger-income returns in the state" (Penniman, 1980). Therefore, it is safe to assume that in those agreements, both the state and the federal government had equal bargaining power. This assumption will be useful later in the econometric strategy. The dates of the agreements are summarized in Table 1.

2.2 Data

Census data about state government finances are available online every two years from 1942 to 1950 and at the yearly level onward (US Department of Commerce, 2015). The result is an unbalanced panel that covers all 50 states and Washington DC between 1942 and 2008. Table 2 displays the descriptive statistics for all of our main variables.

3 Econometric Strategy

In this section, I outline the approach used to estimate the policy responses to the audit exchange information agreements. I implement a difference-in-differences strategy, leveraging the staggered introduction of these agreements to analyze their impact on the ability to raise state revenues through mutual cooperation between the state and the federal government.

The first specification is, therefore:

$$Y_{it} = \beta_0 + \beta_1 Post_{is} + \phi_i + \phi_t + \epsilon_{ist} \tag{1}$$

The main dependent variable is the income tax revenue in state i and year t, in real terms. The other outcomes are the total revenues from any type of tax and the total revenues. All of

the dependent variables in the analysis are in logarithms. The dummy Post is equal to one after the introduction of the audit information exchange agreement in year s, which differs across states (see Table 1). The state and year fixed effects, ϕ_i and ϕ_t, control respectively for state and time invariant factors. The standard errors, ϵ_{ist}, are robust to heteroskedasticity and clustered at the state level. The coefficient of interest β_1 captures the effect of introducing cooperation between the federal and the state government under plausible assumptions. This methodology has two main assumptions. The first is that treated and control states have to be on parallel trends for the main outcomes of interest. If they were not on parallel trends, the coefficient β_1 may capture pre-existing differences in the evolution of the timeseries. I verify this assumption when discussing the results. The second assumption is more challenging to test: the absence of contemporaneous policy events that are systematically correlated with the treatment and affect differentially the treatment group and the control group. The rationale is clear: if there were such events, one would not be able to clearly ascribe the effect to the policy of interest.

Then, I look at the doubly robust difference-in-differences (DiD) estimator. This method is particularly suited for settings with staggered treatment adoption, as is the case here. I follow closely the approach of Callaway and Sant'Anna (2020), focusing on the not yet treated units as a proper comparison group. I consider a case with $\mathcal{T}$ periods, still denoting with t the individual period of interest.

Consider a random sample:

$$\{(Y_{i,1}, Y_{i,2}, ..., Y_{i,\mathcal{T}}, D_{i,1}, D_{i,2}, ..., D_{i,\mathcal{T}})\}_{i=1}^{n}$$

where Y_{it} is the outcome for unit i in period t, $D_{it} = 1$ if unit i is treated in period t, and 0 otherwise. In this setting the treatment is staggered and once signed an agreement the state typically retains it , therefore[1]

$D_{i,t} = 1 \Longrightarrow D_{i,t+1} = 1$, for $t = 1, 2, ..., \mathcal{T}$

[1]The only state that discontinued the agreement for a significant period of time is Montana in 1955. I keep Montana in the treated states in respect to the assumptions of the model.

Following Callaway and Sant'Anna (2020), I define the treatment start date for state i as $G_{i,g} = 1$ if unit i is the first treated at time g and zero otherwise, and $Y_{it}(0)$ as unit i's untreated potential outcome at time if they remain untreated through time period $\mathcal{T}$. For $g = 2, ..., \mathcal{T}$, let $Y_{it}(g)$ denote the potential outcome that unit i would experience at time t if the treatment begins at period g. The parameter of interest is the following:

$$ATT(g,t) = E[Y_t(g) - Y_t(0)|G_g = 1] \text{ for } t \geq g$$

which is the average effect for the group of units first treated at time period g, in calendar time t. I make an assumption which is clearly related to the parallel trends in traditional difference-in-differences. Callaway and Sant'Anna (2020) define the assumption a "conditional parallel trends assumption based on the "not-yet treated groups,"

For each $(s,t) \in \{2, ..., \mathcal{T}\} \times \{2, ..., \mathcal{T}\}$, $g \in \mathcal{G}$ such that $t \geq g$, $s \geq t$

:

$$E[Y_t(0) - Y_{t-1}(0)|G_g = 1] = E[Y_t(0) - Y_{t-1}|D_s = 0, G_g = 0] \; a.s.$$

With this assumption in mind, the estimand of interest becomes:

$$ATT^{ny}_{unc}(g,t) = E[Y_t - Y_{g-1}|G_g = 1] - E[Y_t - Y_{g-1}|D_t = 0, G_g = 0] \tag{2}$$

where *unc* means "unconditionally" from controls and *ny* stands for "not-yet treated" (the comparison group).[2]

[2]The robustness to controls will be tested in the last part of the empirical results.

4 Empirical Results

4.1 Main Results

In this section, I investigate the quasi-experimental consequences of signing the audit information exchange agreements on income tax revenues. I present two specifications. The first specification, shown in equation (1) and presented in the first panel of the main results table, is a traditional difference-in-differences model. The second specification, shown in equation (2) and presented in the second panel, is a doubly robust difference-in-differences model as described in Callaway and Sant'Anna (2020) and Sant'Anna and Zhao (2020). Please note that the number of observations drops with the doubly robust implementation. This is because the doubly robust implementation explicitly distinguishes between treated and never-treated groups, excluding the latter when calculating the Average Treatment Effect on the Treated (ATT).[3]

The first outcome I investigate is mobility. People often respond to changes in the tax environment by moving out of jurisdictions that increase taxes. For example, Kleven, Landais, and Saez (2013) and Cassidy, Dincecco, and Troiano (2024) document economically meaningful in-migration responses following reduction of taxes and significant outmigration following the introduction of income taxes. If people moved in response to the policy, this could raise concerns about selection and the composition of adjustments. To address this, I consider population as the first outcome analyzed in Table 3. The coefficients are consistently not statistically different from zero. There are two possible reasons why people do not move in response to the audit exchange information agreements. The first is related to salience (Chetty, Looney, and Kroft, 2009); the general public may not have been aware of the agreements. The second is that the response generated by the agreements may not have outweighed the adjustment costs associated with relocating (Chetty, Friedman, Olsen, and Pistaferri, 2011). The confidentiality of IRS auditing rules supports the relevance of these two channels.

The other outcomes examined are: revenues from income taxes, revenues from all types of taxes,

[3]Additionally, in staggered treatment designs, observations without comparable untreated groups in the same period or insufficient untreated units for valid comparisons may be excluded. Furthermore, the method's reliance on doubly robust estimation can result in dropping observations due to extreme propensity scores, lack of overlap, or failures in regression adjustments.

and total revenues. For income tax revenues, the coefficients are consistently statistically significant. In the doubly robust specification, income tax revenues increase by approximately 20 percent. For revenues from all types of taxes and total state revenues, statistical significance depends on the specification. These outcomes are positive and statistically significant only in the doubly robust specification. In the latter specification the agreements increase total tax revenues by 5.5 percent and total revenues by 3.5 percent.

4.2 Robustness

In Figure 1, I investigate whether the main effects are robust to dynamic considerations or whether pre-trends are driving the results. As one can see, there are no clear pre-trends in any of the outcomes. For the main outcome of interest, income from tax revenues, one can observe that shortly after the treatment, there is a trend-break, strongly suggesting that the treatment has affected the main outcome around the time period of interest. The pattern is broadly similar for revenues from taxes and total revenues. Population does not show either a pre-trend or a trend-break after the introduction of the income tax.

State income tax withholding was introduced in the United States throughout the second part of the twentieth century (Bagchi and Dušek, 2021), typically after the introduction of the state income tax (Cassidy, Dincecco, and Troiano, 2015). In Table 4, I investigate whether the effect on the main tax outcomes of interest is robust to the introduction of state income tax withholding and the introduction of the income tax. The specification I adopt is that of equation (1). It is remarkable that the coefficient for revenues from income taxes remains stable, even though, by itself, tax withholding increases income tax revenues by at least 20 percent.

5 Conclusion

Improving the efficiency of tax collection is a key issue for fostering economic development. In this short paper, I discussed the role of audit information exchange agreements, which are intergovernmental agreements between the state and the federal U.S. government to exchange information about audit plans and techniques. I find that, under plausible assumptions, signing the agreement

causally increases revenue from the income tax by about 20 percent.

The paper remained agnostic about which specific auditing practices worked and which did not, primarily due to the secrecy surrounding the details of the intergovernmental interactions I am studying. Future research should aim to unbundle the details of specific auditing practices to identify those that are effective and those that are not.

Tables and figures

Table 1: Years of the Information Exchange Agreement

State	Agreement Year	State	Agreement Year
Alabama	1970	Ohio	1961
Alaska	1967	Oklahoma	1963
Arizona	1966	Oregon	1961
California	1961	Pennsylvania	1965
Colorado	1952	Rhode Island	1970
Connecticut	1970	South Carolina	1964
Delaware	1965	Tennessee	1963
Florida	1963	Utah	1961
Georgia	1968	Vermont	1965
Hawaii	1965	Virginia	1963
Idaho	1964	West Virginia	1962
Illinois	1963	Wisconsin	1950
Indiana	1961		
Iowa	1962		
Kansas	1960		
Kentucky	1951		
Louisiana	1971		
Maine	1964		
Maryland	1963		
Massachusetts	1963		
Michigan	1965		
Minnesota	1957		
Mississippi	1966		
Missouri	1962		
Montana	1951		
Nebraska	1963		
New Hampshire	1964		
New Jersey	1966		
New York	1963		
North Carolina	1950		
North Dakota	1964		

Notes: Source: Penniman (1980). Montana discontinued the agreement in 1955. Alaska and Hawaii had individual and income taxes when they became states in 1959. Connecticut's individual income tax only taxes capital gains and dividends; Indiana's is a flat-rate income tax; Massachusetts' is a classified income tax; New Hampshire's only taxes interest and dividends. Michigan's 1967 corporate income tax was repealed in 1975. South Dakota's Depression-era income tax was repealed shortly thereafter; West Virginia's was repealed and reenacted soon after.

Table 2: Summary Statistics

	Mean	SD	Min	Max	N
Tot. Income Tax	25.19	54.77	0.00	675.95	3,187
Total Taxes	70.55	108.72	0.55	1196.48	3,187
Tot. Revenues	151.44	246.22	1.50	3122.65	3,187
Population (1000s)	4349.40	4872.88	137.00	36580.00	3,187
Post Information Agreement	0.66	0.48	0.00	1.00	3,187
Post Withholding	0.60	0.49	0.00	1.00	3,187
Post Income Tx	0.71	0.46	0.00	1.00	3,187

Notes: *Tot. Income Tax* stands for Total Income Tax Revenues in (100000s) of USD. *Total Taxes* stands for Total Tax Revenues in (100000s) of USD. *Tot. Revenues* stands for Total Revenues in (100000s) of USD. *Post Information Agreement* is a dummy which equals to 1 after the signing of the audit information exchange agreement in that given state-year combination. *Post Withholding* is a dummy which equals to 1 after the introduction of withholding in the collection of the state income tax. *Post Income Tx* is a dummy which equals to 1 after the introduction of the income tax in that given state-year combination.

Table 3: Population and Policy Responses to Audit Exchange Agreements

OLS Estimates				
	(1)	(2)	(3)	(4)
	Log pop	Inc. taxes	Taxes(Tot)	Revenues(Tot.)
Post Audit Agreement	-0.0591	0.200***	-0.0255	-0.0154
	(0.0989)	(0.0665)	(0.0963)	(0.0756)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	3187	2747	3187	3187
R^2	0.973	0.959	0.977	0.982
Doubly Robust				
	(1)	(2)	(3)	(4)
	Log pop	Inc. taxes	Taxes(Tot)	Revenues(Tot.)
ATT	0.0102	0.179*	0.0518**	0.0349*
	(0.0219)	(0.0973)	(0.0240)	(0.0204)
N	1055	870	1055	1055

Standard errors in parentheses

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

Notes: Standard errors are clustered at the state level. In the first panel the estimates are of equation (1). In the second panel the estimates are of equation (2). *Log pop* stands for number of inhabitands (in logarigthm). *Inc. Taxes* stands for Total Income Tax Revenues in logarithm. *Taxes(Tot)* stands for Total Tax Revenues in logarithm. *Revenues(Tot.)* stands for Total Revenues in logarithm. *Post Inf. Agreement* is a dummy which equals to 1 after the signing of the audit information exchange agreement in that given state-year combination.

Table 4: Population and Policy Responses to Audit Exchange Agreements

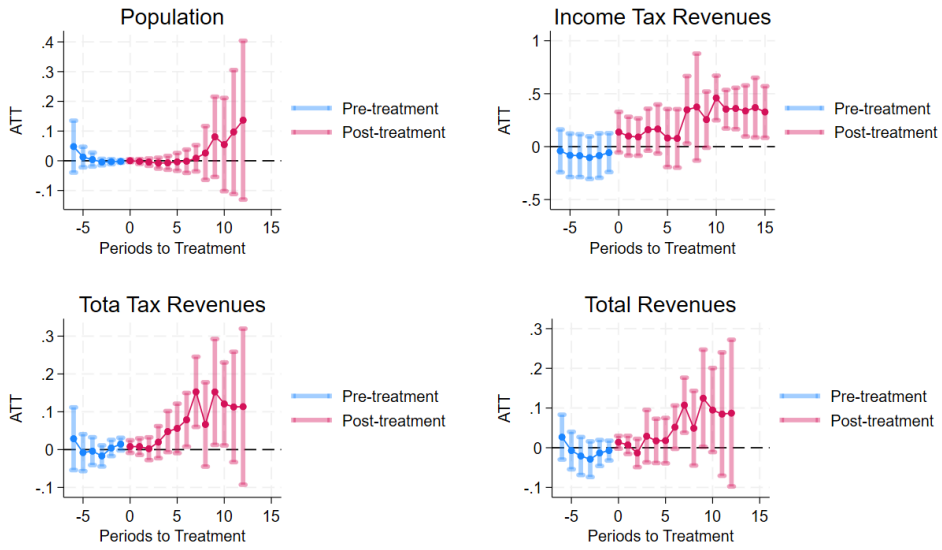
	(1) Inc. taxes	(2) Taxes(Tot)	(3) Revenues(Tot.)	(4) Inc. taxes	(5) Taxes(Tot)	(6) Revenues(Tot.)
Post Inf. Agreement	0.158**	-0.00893	0.00524	0.128**	-0.0193	0.00350
	(0.0760)	(0.0881)	(0.0717)	(0.0606)	(0.0956)	(0.0751)
Post Withholding	0.287**	-0.0567	-0.0705			
	(0.1128)	(0.0649)	(0.0561)			
Post Income Tx				0.625*	-0.0328	-0.0994
				(0.3110)	(0.0733)	(0.0635)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2747	3187	3187	2747	3187	3187
R^2	0.961	0.977	0.982	0.962	0.977	0.983

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the state level. The estimates use equation (1). *Inc. Taxes* stands for Total Income Tax Revenues in logarithm. *Taxes(Tot)* stands for Total Tax Revenues in logarithm. *Revenues(Tot.)* stands for Total Revenues in logarithm. *Post Inf. Agreement* is a dummy which equals to 1 after the signing of the audit information exchange agreement in that given state-year combination. *Post Withholding* is a dummy which equals to 1 after the introduction of withholding in the collection of the state income tax. *Post Income Tx* is a dummy which equals to 1 after the introduction of the income tax in that given state-year combination.

Figures

Figure 1: Pre-trends



Notes: *Population* stands for number of inhabitands (in logarigthm). *Income Tax Revenues* stands for Total Income Tax Revenues in logarithm. *Taxes(Tot)* stands for Total Tax Revenues in logarithm. *Total Revenues* stands for Total Revenues in logarithm. The estimation method is the doubly robust DiD estimator based on Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2020), as in equation (2).

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