MEASURING THE FLYPAPER EFFECT: THE INTERACTION BETWEEN LUMP-SUM AID AND THE SUBSTITUTION EFFECT OF MATCHING AID

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ABSTRACT

According to one theory, consumer-voters mistake lump-sum aid for matching aid such that lump-sum aid reduces their tax price of the aided service. Consumer-voters misunderstand that the substitution effect of matching aid comes from lump-sum aid, resulting in higher expenditures for the aided service. This flypaper effect is more likely when both lump-sum aid and matching aid (or equivalent tax-price reducing mechanisms) coexist and there is a high likelihood of interaction between the lump-sum aid and the substitution effect. However, surprisingly few studies have developed a clear formula to evaluate the flypaper effect while the literature generally assumes the interaction as one of potential causes for the flypaper effect. This paper fills in the huge gap in the literature by providing a formula to show whether and how lump-sum aid causes the flypaper effect.

Keywords: flypaper effect, substitution effect, matching aid, lump-sum aid, fiscal illusion

1. INTRODUCTION

Numerous theoretical and empirical studies have reported that a one-dollar increase in lump-sum aid has much stronger fiscal impacts on recipient governments’ expenditures on the aided service than an equal amount of increase in consumer-voters’ income does. This phenomenon has been known as the flypaper effect. One dominant theory explaining the flypaper effect is the theory of fiscal illusion on the part of consumer-voters in the recipient governments. Consumer-voters mistake the lump-sum aid for matching aid and might perceive that the lump-sum aid reduces their marginal tax price of the governmental service aided by donor governments’ lump-sum aid. The lower tax price generates the substitution effect and consumer-voters tend to demand the aided service more. As a result, the expenditure for the service grows as well. This possibility is more likely whenever lump-sum aid and matching aid (or similar fiscal settings that render tax-price reducing impacts from matching aid) coexist and the two types of aid interact. However, surprisingly few studies provide a clear formula to measure the fiscal impact of the misperceived tax price on governmental expenditures on the aided service. This paper develops an explicit formula to measure the fiscal impact (i.e., the substitution effect), which interacts with lump-sum aid, by using Ohio’s local school dis-
trict data. The formula to measure the interacted substitution effect can be easily and clearly applied to similar aid structures in other states.

The next section introduces the literature on the flypaper effect. The following section explains a diagrammatic measurement of the substitution effect. The next section succinctly presents a model of expenditure for Ohio’s local school districts. After that, the section on data and methodology explains data sources and model estimation strategies. Empirical findings in this paper clarify that the substitution effect works through the lump-sum aid as the theory of fiscal illusion predicted. In addition, the findings clearly reveal the route, through which matching share, typically associated with matching aid, influences school district expenditures via lump-sum aid.

2. THE FLYPAPER EFFECT AND NEW MEASUREMENTS

2.1 THE FLYPAPER EFFECT IN THE LITERATURE

Fiscal impacts of lump-sum aid have been observed to be stronger than those from the increase in consumer-voters’ personal income. A one-dollar increase in the latter typically raises the expenditure on public goods delivered by governments receiving the lump-sum aid by about $0.05 to $0.10 (Cullis and Jones 2009, 388). In contrast, a one-dollar increase in the lump-sum aid tends to increase the public expenditure by about $0.25 to $0.50. This phenomenon has been known as the flypaper effect (Gramlich and Galper 1973; Fisher 2016, 229; Hines and Thaler 1995; Hyman 2011, 732-733; Rosen and Gayer 2010, 532-533). Some other studies report slightly stronger flypaper effect up to about one dollar (Case et al. 1993; Gamkhar and Oates 1996; Gennari and Messina 2013).

One of the most dominant theories explaining the flypaper effect is the theory of fiscal illusion either by consumer-voters or budget-maximizing bureaucrats. Consumer-voters might perceive that lump-sum aid reduces marginal cost of service deliveries by the governments receiving the aid. If the governments spend $100 per consumer-voter for a certain service and receive a $20 per consumer-voter lump-sum aid for the spending, the recipient governments shoulder only 80% of the cost incurred for the service. Consumer-voters are likely to mistake the reduction in the service delivery cost for a decrease in their tax price. The final fiscal impacts might be similar to the effect from matching grants that reduce tax prices through matching share (Courant, Gramlich, and Rubinfeld 1979; Oates 1979). If matching share is associated with local tax payment, as in the case of Ohio’s property tax rollback credit for local property taxpayers, this type of fiscal illusion would also be highly probable, as this paper shows.
In a similar vein, budget-maximizing bureaucrats also fall in fiscal illusion, but in this case over average costs of the service delivery. King (1984) applied the framework in Niskanen’s (1971) budget-maximizing bureaucrats to explain the flypaper effect. Budget-maximizing bureaucrats tend to request budgets larger than socially efficient ones where marginal benefits from the governmental service equal marginal costs of the service delivery. When their governments receive lump-sum aid, they misunderstand that the lump-sum aid lowers the entire costs of the service delivery and ultimately reduces average costs of the service delivery. This makes a downward shift of the cost curve in the Niskanen model. As a result, the lump-sum aid raises the governmental budgets since bureaucrats pursue budget-maximization on the newly perceived average costs. Some empirical findings support this possibility of fiscal illusion on the part of governmental bureaucrats. When bureaucrats in the governments receiving the lump-sum aid kept stronger budget-manipulating power, the flypaper effect was much higher (Bae and Feiock 2004; Schneider and Ji 1987). Budget-maximizing bureaucrats might also control the information on the lump-sum aid and induce consumer-voters to support higher spending while at the same time, they spend the lump-sum aid. This might also explain why the lump-sum aid can significantly increase the expenditures for the public services that receive grants from donor governments (Filimon, Romer, and Rosenthal 1982).

Other competing theories pay attention to the grant-decision process. When donor governments choose recipient governments for the lump-sum aid, they are likely to distribute their lump-sum aid to recipient governments that are willing and likely to spend larger amounts of local dollars. This process turns the lump-sum aid into a *de facto* matching aid with much higher fiscal impacts (Chernick 1979). Inman (2008) especially points at the political aspect inherent in the grant-decision process by citing Knight (2002). The legislators whose districts benefit most highly might be willing to spend more on the aided governmental service and as a result, tend to make the winning bids for the lump-sum aid. The end-result is a positive correlation between the lump-sum aid awarded and recipient governments’ spending.

Another provocative recent study indicated that the analytical methodology employed in the previous studies might have overestimated the flypaper effect. When logarithmic model specification was used, instead of linear specifications between the lump-sum aid and recipient governments’ spending, the flypaper effect was much weaker (Grizzle 2011). However, more recent and elaborate studies strongly confirm the flypaper effect noted above. For instance, Roemer and Silvestre (2002) indicate the single-policy-dimension and median voter framework in the previous studies. They allow for more complicated party competition under multidimensional issue frameworks but still support the previous studies. Sobel and Crowly (2014) investigate how an inter-temporal dimension of recipient governments’ spending and donor gov-
ernments’ grants affects the flypaper effect. Since governmental programs, which might have expanded by grants, are hard to cut, future recipient governments’ budgets, especially tax revenues, tend to grow. Their empirical findings show that grants tend to increase future state and local tax increases by about $0.40.

2.2 NEW MEASUREMENTS OF THE FLYPAPER EFFECT

Among the various theories explaining the flypaper effect, the theory of fiscal illusion by consumer-voters has recently garnered an interesting attraction. Rockoff (2010) developed an elaborate formula to measure consumer-voters’ fiscal illusion. However, his formula attempted to tap how much lump-sum aid dampens the price effect of matching aid rather than how much matching aid affects the fiscal impact of lump-sum aid. Based on his formula, his estimated flypaper effect in New York’s lump-sum aid to school districts is about $0.1, which is much lower than the range of the flypaper effect introduced above. One potential reason for this low estimation of the flypaper is because his interaction formula is developed primarily to tap how lump-sum aid affects matching aid, not the other way around. Duncombe and Yinger (1998, 2009; see also Eom et al. 2014) have developed one of the most elaborate measures of the interaction between lump-sum aid and matching aid, which is defined as DY Interaction in this paper. DY Interaction is a theoretically elaborate measure because unlike Rockoff’s interaction measure, it is derived from consumer-voters’ and local governments’ fiscal optimization equations. However, DY Interaction does not specifically test whether consumer-voters’ fiscal illusion explains the flypaper effect.

3. MATCHING AID, LUMP-SUM AID, AND SUBSTITUTION EFFECT

3.1 HOW TO MEASURE THE SUBSTITUTION EFFECT FROM LUMP-SUM AID

The main research question of this paper is whether a consumer-voter might perceive that lump-sum aid causes the substitution effect that typically comes from matching aid, and if so, how to measure the effect. For that purpose, one needs to first define the substitution effect.

Although the flypaper effect implies that lump-sum aid to recipient governments stimulates the recipient governments’ expenditure more than an equal amount of personal income does, matching aid tends to induce a much higher fiscal impact on the recipient governments than lump-sum aid does. While lump-sum aid generates only an income effect, matching aid has been theorized and observed to exert both income and substitution effects. The substitution effect from matching aid decreases the actual price of a public good delivered by a local government that receives matching aid and thereby induc-
es higher demand for the public good. A diagrammatic analysis can demonstrate how to measure the higher demand and the attendant increase in the expenditure for the public good.

Figure 1. Matching Aid, Lump-sum Aid, and Substitution Effect

Figure 1 depicts a budget constraint of a consumer-voter in a local jurisdiction where he resides. His original budget constraint is the line segment, $Y_0$. $OR_0$ is his personal income and shows the amount of the expenditure on private goods if he spends all of his income on private goods. Assume that he originally spends an amount of his personal income on private goods and an amount of the personal income on the public good. The slope of $Y_0$ measures the amount of the personal income he has to sacrifice to enjoy a unit of the public good. The slope is the tax price, defined as $P$ in this paper. Further assume that the consumer-voter receives per consumer-voter lump-sum aid, $A$, from a donor government, which is equal to $ac$ in Figure 1. His new budget constraint will be $ig$. He can spend more on the public good by the amount of the lump-sum aid, $A$ or $ac$. His purchasing power increases due to the increase in his overall income. This observation is generally known as the income effect of lump-sum aid. This income effect differs from the price ef-

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1. It is also likely that local governments might not behave in response to the preference of a representative consumer-voter, like a median voter. Instead, local governments might respond to the mix of entire jurisdictional tax and spending, which would represent the whole jurisdictional preference (Trimidas and Winer 2005; Turnbull and Mitias 1999). If that is the case, the average budget constraint of the entire jurisdiction, rather than the consumer-voter’s budget constraint, might be more relevant for Figure 1. In fact, Rockoff (2010) analyzed the average budget data, rather than an individual’s budget constraint. However, as long as the average constraint and a consumer-voter’s constraint show similar patterns, the analysis in this paper is still relevant.
ffect caused by matching aid, which is the sum of the income effect and the substitution effect as shown below. The flypaper effect implies that about 25 to 50% of $A$ or $ac$ will result in the increase in the expenditure on the public good. However, for the ease of analytic clarity, just assume that the increase in the expenditure on the public good will be as large as the full amount of the lump-sum aid, $A$ or $ac$. One further needs to be aware that the increase in the expenditure by the amount of the lump-sum aid, the income effect, happens where the consumer-voter spends an $OY_2$ amount of his personal income on private goods.

The question is how we can identify the matching aid that is as large as the lump-sum aid, $A$, in terms of the increase in the consumer-voter’s income but generates the substitution effect in addition to the income effect. The increase in his personal income provides a clue on how to measure the substitution effect typically resulting from matching aid. If the consumer-voter decides to spend the lump-sum aid for private goods, the aid will increase his personal income by $iY_0$ that is equal to $ab$. Since the slope of his new budget constraint, $ig$, is still $P$, $ab$ equals $P* A$. Matching aid only reduces his tax price of the locally delivered public good and therefore, his personal income is likely to be still $OY_0$. Since matching aid decreases the relative price of the public good, his budget constraint will swivel out from $Y_0f$ to $Y_0h$. An important point is that $Y_0h$ cuts through the point, $h$, such that the implicit matching aid is equal to the lump-sum aid in terms of how much his personal income might be augmented.\(^2\) The price effect, which denotes how much the price of the public good decreases by matching aid and as a result, the demand and expenditure for the public good increases, is measured by $ad$ in Figure 1. One important point is that $ad$ consists of two separate effects. $ac$ is the income effect that the now relatively cheaper price for the public good boosts the consumer-voter’s purchasing power. In contrast, $cd$ is the substitution effect that the relatively cheaper price for the public good induces him to demand the public good more because now the latter has a relatively cheaper commodity price. As noted earlier, the previous literature on the flypaper effect did not clearly distinguish the income effect caused by matching aid, which is assumed to be equal to that generated by lump-sum aid, from the substitution effect that matching aid brings about. More importantly, it did not develop formulas to measure the substitution effect and then tightly link them to the income effect caused by lump-sum aid.

The matching aid will lower the consumer-voter’s tax price by a matching share, $m$, inherent in the matching aid. Therefore, the new tax price will be $P*(1-m)$ that is the slope of the new budget constraint, $Y_0h$. The matching

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\(^2\) In standard public finance texts, this assumption, as well as the above ones, is generally accepted (Hyman 2011, 729-733).
aid will now generate the final fiscal impact on the recipient government,

\[ \frac{ab}{P(1-m)} - \frac{PA}{P(1-m)} = \frac{A}{1-m}. \]

which is as large as \( ad \) that can be measured as \( \frac{A}{1-m} \). Since the substitution effect \( cd \) is equal to \( ad - A \),

\[ cd = \frac{A}{1-m} - A = \frac{Am}{1-m}. \]

The main research question of this paper is whether the consumer-voter might perceive that the substitution effect was caused by the lump-sum aid, and if so, how to measure the effect. In other words, according to the fiscal illusion theory of the flypaper effect, the consumer-voter might be confused between the lump-sum aid and the matching aid to the extent which he perceives that the lump-sum aid reduces his tax price. A straightforward way to test this possibility is to interact the lump-sum aid with the substitution effect, \( \frac{Am}{1-m} \), in Figure 1.

Next section shows how to measure the matching share, \( m \). However, there is one caveat about the fiscal illusion. Niskanen’s (1971) budget-maximizing bureaucrat’s model or Romer and Rosenthal’s (1979) agenda-control model shows various ways that augment bureaucrats’ budget-setting power. Bureaucrats’ budget power will get even stronger when consumer-voters cannot clearly identify the links between tax revenues and the expenditures of public services. In the case of this paper, consumer-voters might not easily notice the links between matching share and the expenditures of aided services. This scenario is especially possible when local jurisdictions provide multiple services that are financed from tax or aid revenues. There is not high likelihood of fiscal illusion of consumer-voters for more general-purpose local jurisdictions in the first place. Therefore, the findings in this paper are more applicable to single-purpose local jurisdictions such as Ohio’s school districts.

3.2 OHIO’S LOCAL PROPERTY TAX ROLLBACK CREDIT AND MATCHING SHARE

Since 1971, a 10% property tax rollback credit has been applied to all real property in Ohio. In 2005, the Ohio General Assembly limited the credit to all real property that is not used in business. The state government reimburses local school districts and governments for the credit cost. This credit is now called non-business credit. Another 2.5% rollback credit, known as the owner occupancy credit, has been applied to owner-occupied homesteads that include

3. If the consumer-voter spends all of his personal income for the public good, the substitution effect will be theoretically \( gh \) that can be measured as \( Oh - Of - fg \). The latter is measured as

\[ \frac{OY \circ P}{P(1-m)} - \frac{A}{P} = \frac{OY \circ P}{P(1-m)} - \frac{A}{P} = \frac{OY \circ P}{P(1-m)} - \frac{A}{P(1-m)} = \frac{OY \circ P}{P(1-m)} + \frac{Ap + Amp}{P(1-m)} \]
dwellings plus one acre homeowners occupy. The state government also reimburses localities for this credit cost. Therefore, these rollback credits reduce the property tax liability of property taxpayers.

Ohio also allows a homestead exemption to all homeowners who are either: 65 years old or older, permanently or totally disabled, or surviving spouses who are at least 59 years old and whose deceased spouses had previously qualified for the exemption. Between 2007 and 2013, the homestead exemption had shielded $25,000 of the true or market value of a homestead property from property taxation. Although the homestead exemption is called a homestead “exemption,” taxpayers receive it in the form of property tax credits. For any eligible homeowners, the property tax credits directly apply to property tax bills and the state reimburses local governments and school districts for these rollback credits. For instance, the property tax assessment ratio in Ohio is 0.35 and the assessed or taxable property value, which qualifies for the homestead exemption, is $8,750 ($25,000 * 0.35). Property tax rates are applied to $8,750 and homeowners receive the final rollback “credit” over their final property tax bills. Beginning from 2014, this homestead exemption is means-tested. Only taxpayers with Ohio adjusted gross income of $30,500 or less qualify for the exemption (Ohio State Taxation Department 2014; Sullivan and Sobul 2010, 10).

Before developing a formula to measure matching share, we need to understand the difference between matching rate and matching share. Fisher (2016, 220-221) clarifies the difference. If a donor government distributes $R$ grant dollars for one local dollar, which a recipient government spends or raises as its revenue, the total available amount of local revenue or spending is now $1 + R$ dollars. Note that now the local government’s revenue base expands to $1 + R$. Of the new local revenue base, the share of the amount, which the donor government shoulders, is therefore $\frac{R}{1 + R}$. The latter is defined as matching “share,” $m$. We can apply the formulaic concept to, for example, Ohio’s 10% property tax rollback credit. If the Ohio state government has NOT offered the $0.1$ rollback credits to local governments, the potential revenue base of local governments would have been $1.1$: the total local revenue might have been tax revenues from taxable house value plus those from the house value exempted through credits. Of the local revenue base, the Ohio state government provides an implicit grant dollar $R$ of $0.1$. In sum, matching rate, $R$ is 0.1 but matching share in this case is about $0.09 \approx \frac{0.1}{1 + 0.1}$. When all three rollback credit amounts are summed up, matching share in Ohio, $m$, is defined as the following:

$$m = \frac{\text{Per Pupil Property Tax Rollback Credits}}{\text{Per Pupil Property Tax Rollback Credits} + \text{Per Pupil Property Tax}}$$ (1)
We can develop a similar measure of matching share for the amount of exemption that is directly applied to homeowners’ property value. For instance, New York’s School Tax Relief Program (STAR) has been providing state-funded property tax relief for homeowners. STAR exemption directly exempts a certain amount from their housing value (Eom et al. 2014; Rockoff 2010). Duncombe and Yinger (1998, 2009), Eom et al. (2014), and Eom and Rubenstein (2007) derived a formula to measure the matching rate from STAR. They define matching rate as \( \frac{X}{V} \), which reduces local governments’ tax price by \( V \) where \( X \) is the STAR exemption amount and \( V \) is a median voter’s housing value. Although they define \( \frac{X}{V} \) as matching “rate,” their measure actually denotes matching share as long as \( V \) is the sum of taxable house value and exempt value. Therefore, \( \frac{X}{V} \) measures exactly how much exempting a certain amount directly from house value reduces taxpayers’ tax price.

As Bradford and Oates (1971) pointed out, the impact of a matching grant is equivalent to that of a tax credit that reduces voters’ tax price by the rate equal to the matching share of the grant. This implies that a tax credit can duplicate the impact of a matching grant (Hyman 2011, 731-733), which the above formulas prove.

**4. EXPENDITURE MODEL**

Following previous studies (Feldstein 1975; Gramlich and Rubinfeld 1982; Duncombe and Yinger 2009, 2011; Rockoff 2010; Eom et al. 2014), the consumer-voter’s demand for Ohio local school district expenditure, \( E \) (Per Pupil Expenditure), in each school district is specified as follows:

\[
E = f(Tax\ Share, Median\ Income, Lump-sum\ Aid, Interaction, Other\ Factors)
\]

All variables are in natural log, except for two variables explained in the next section. \( Tax\ Share \) is defined as \( \frac{V}{V^*} (1 - m) \) where \( V \) is a median voter’s housing value and \( V^* \) is per pupil property valuation. \( m \) is defined in Equation (1). Thus, \( Tax\ Share \) measures the slope of \( Y_{ah} \) in Figure 1, or \( P^* (1 - m) \). \( Median\ Income \) is a median Ohio adjusted gross income, or \( OY_{a} \) in Figure 1. \( Lump-sum\ Aid \) is per pupil lump-sum aid, \( A \). The key research question of this paper is to test whether consumer-voters perceive as if the lump-sum aid reduces their marginal tax price and as a result absorbs the substitution effect of matching aid. Therefore, \( Interaction, A \cdot \frac{Am}{1 - m} \), directly measures the interac-
tion between the lump-sum aid, $A$, and the substitution effect, $1 - \frac{m}{Y}$. This paper also tests $DY$ Interaction as a comparison purpose. The variable, as applied to Ohio based on (Duncombe and Yinger 1998, 2009; Eom et al. 2014), is defined as $\left(\frac{A}{Y}\right) \cdot \left(\frac{Y}{1}\right) \cdot (1 - m)$, where $Y$ is Median Income. 4

Tax Share is expected to negatively correlate with Per Pupil Expenditure. The flypaper effect indicates that an increase in lump-sum aid, $A$, stimulates school expenditure more than an increase in consumer-voter’s income does. Both Lump-sum Aid and Median Income would be positively related with school expenditure while the fiscal impact of $A$ on school expenditure might be stronger than that of $Y$. As long as consumer voters mistake lump-sum aid for matching aid or similar tax-price-reducing mechanisms, Interaction will positively affect Per Pupil Expenditure. In line with the literature, $DY$ Interaction is expected to positively correlate with Per Pupil Expenditure.

Other Factors are included also based on the literature. Educational cost is usually defined as a function of teacher salaries, student enrollment, and pupil characteristics (Duncombe and Yinger 1999; Costrell, Hanushek, and Loeb 2008). Salaries for more experienced and professional teachers are likely to be higher, so Teacher Salary will be positively related with Per Pupil Expenditure. School expenditures tend to increase as there are more students. However, if school administration is more efficient, then there might be a scale economy. Therefore, the logged value of student enrollment, Formula ADM (Average Daily Attendance), might be negatively correlated with Per Pupil Expenditure. Harder-to-teach students might likely require more educational resources. Students in Poverty, Students with Disability, and Nonwhite Students would be positively correlated with Per Pupil Expenditure.

Property tax payers might be more vigilant about school district budgets. Therefore, Owner-occupied House Units might be negatively correlated with Per Pupil Expenditure. In contrast, parents with children, who are likely to attend local schools, generally demand better school service. As a result, the percentage of the population who are likely to attend local schools, Population 5-19 Years Old, would positively correlate with Per Pupil Expenditure.

5. DATA AND METHODOLOGY

Table 1 provides descriptive statistics of all variables in per pupil measures in non-logged values for 609 school districts in FY 2012, unless specified otherwise. It also gives data sources. Lump-sum aid without any matching provisions includes multiple components. Ohio’s state grants-in-aid to local school

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Table 1. Descriptive Statistics (N = 609)

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>Standard Deviation</th>
<th>Min</th>
<th>Max</th>
<th>Data Source</th>
</tr>
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<tbody>
<tr>
<td><strong>Dependent Variables</strong></td>
<td></td>
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<tr>
<td>Per Pupil Expenditure</td>
<td>9,918.10</td>
<td>1,789.12</td>
<td>0*</td>
<td>21,452.79</td>
<td>A</td>
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<td><strong>Independent Variables</strong></td>
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<tr>
<td>Teacher Salary</td>
<td>54,108.5</td>
<td>7,406.9</td>
<td>32,219.2</td>
<td>81,850.5</td>
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<tr>
<td>Formula ADM</td>
<td>2,830.73</td>
<td>4,822.08</td>
<td>189.89</td>
<td>65,975.04</td>
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<td>Students in Poverty</td>
<td>0.41</td>
<td>0.19</td>
<td>0</td>
<td>0.99</td>
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<tr>
<td>Students with Disability</td>
<td>0.13</td>
<td>0.03</td>
<td>0.06</td>
<td>0.25</td>
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<tr>
<td>Nonwhite Students</td>
<td>0.13</td>
<td>0.17</td>
<td>0.003</td>
<td>0.998</td>
<td>A</td>
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<td>Tax Share</td>
<td>0.27</td>
<td>0.07</td>
<td>0.07</td>
<td>0.62</td>
<td>C, D</td>
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<td>Lump-sum Aid (A)</td>
<td>3,594.53</td>
<td>1,719.13</td>
<td>-65.52</td>
<td>10,066.72</td>
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<td>Median Income</td>
<td>34,444.36</td>
<td>8,314.93</td>
<td>17,388</td>
<td>76,265</td>
<td>E, F</td>
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<td>Owner-occupied House Units</td>
<td>0.69</td>
<td>0.11</td>
<td>0.25</td>
<td>0.92</td>
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<tr>
<td>Population 5-19 Years Old</td>
<td>0.21</td>
<td>0.03</td>
<td>0.11</td>
<td>0.32</td>
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<tr>
<td>Interaction</td>
<td>2,675,282</td>
<td>2,579,794</td>
<td>649.19</td>
<td>1.67e+07</td>
<td>A, C</td>
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<td>DY Interaction</td>
<td>0.03</td>
<td>0.02</td>
<td>-0.00</td>
<td>0.17</td>
<td>A, C, D, E</td>
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<td><strong>Instrumental Variable</strong></td>
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<td>Per Pupil 2011 Property Tax Payment</td>
<td>4,133.1</td>
<td>2,471.8</td>
<td>1,036.1</td>
<td>19,896.2</td>
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<tr>
<td>2012 Administrator Number</td>
<td>17.69</td>
<td>27.81</td>
<td>1.5</td>
<td>443</td>
<td>A</td>
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<tr>
<td>Efficiency Index</td>
<td>0.49</td>
<td>0.10</td>
<td>0.22</td>
<td>0.996**</td>
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<td>2012 Average Annual Wage for the Manufacturing Industry</td>
<td>52,702.54</td>
<td>8,171.89</td>
<td>27,990</td>
<td>77,284</td>
<td>I</td>
</tr>
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</table>

Note: Some missing observations were replaced by mean values. All variables in 2011 values were converted into 2012 constant values, by using the state and local government price deflator.

Note*: There are two school districts with zero expenditure for FY 2012: Riverdale Local School District, Hancock and Switzerland of Ohio Local School District, Monroe. When the two cases were replaced with the mean value of per pupil expenditure, regression results were almost unchanged. The minimum value of Lump-sum Aid is less than zero due to the adjustment noted in the text.

Note **: The maximum value of Efficiency Index is set less than 1 to avoid losing some cases when taking logged values of the index.

Data Sources: A = Ohio State Education Department (2012a); B = Ohio State Education Department (2013a); C = Ohio State Education Department (2011); D = U. S. Census Bureau a; E = Ohio State Taxation Department (2012); F = Ohio State Education Department (2012b); G = U. S. Census Bureau b; H = Ohio State Education Department (2010); I = Ohio State Job and Family Services Department

5. The two zero values are recorded as zero in the original data file. They differed from system-missing values in the file, which were explicitly recorded as system-missing. These two observations were lost in the final regression due to converting all observations into natural log.
districts have been based on foundation formulas that inversely relate state funding to local wealth measured in terms of local property valuation and income levels. For FY 2012, about eleven state funding categories were based on the foundation formulas (Ohio State Education Department 2013b). There were some minor categories that were not based on the foundation formulas.\(^6\) Lump-sum aid is the sum of all these categories and the foundation-based aid items. As indicated above, Interaction and DY Interaction are added as non-logged values in final regression models. When logged Interaction was used, this variable did not pass instrumental variable tests explained below. DY Interaction is specified as non-logged value even in double-log estimation of Equation (2) (see, for details, Duncombe and Yinger 1998, 2009; Eom et al. 2014).

There are two methodological caveats in estimating Equation (2): heteroscedasticity and endogeneity. The Breusch-Pagan/Godfrey/Cook-Weisberg statistic (Breusch and Pagan 1979; Godfrey 1978; Cook and Weisberg 1983) tests the null of no heteroscedasticity under the assumption of the normality of error distribution. I used levels of all instrumental variables (i.e., included and excluded exogenous variables to be explained below) except the constant, and their squared values as indicator variables. The test statistics indicates the presence of heteroscedasticity (122.210 – Chi-sq (22): P = 0.000). The Koenker statistics (1981) drops the normality assumption. The White/Koenker statistics also indicates heteroscedasticity (94.485 – Chi-sq (22): P = 0.000) (White 1980; Koenker 1981). While the above tests assume that heteroscedasticity exists in the present equation only, Pagan and Hall (1983) relax this assumption and assume that heteroscedasticity might exist elsewhere in the entire system, including endogenous variables (Baum, Schaffer, and Stillman 2003). However, the Pagan-Hall statistic indicates no heteroscedasticity (18.335 – Chi-sq (22): P value = 0.686).\(^7\) As a way around the potential heteroscedasticity, heteroscedasticity-consistent estimation was used based on Newey and West (1987).

Another methodological caveat is endogeneity. School districts usually prepare budget proposals for teacher salary as a part of their regular district budget processes. This aspect implies that not only do teacher salaries affect school district budgets, but also that school expenditures might influence teacher salaries. For this reason, Teacher Salary in Equation (2) needs to be treated as endogenous. In addition, there is a possibility that future Lump-sum Aid might be adjusted based on current school expenditures, especially because most of lump-sum aid categories are the foundation formulas that in-

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7. The ivhettest option in STATA was used for heteroskedasticity tests.
versely relate local wealth with state grants-in-aid if we further account for a possibility that higher school spending might be capitalized into local property valuation. Similarly, Tax Share might suffer from endogeneity as well. Finally, since both Interaction and DY Interaction include Lump-sum Aid, they need to be treated as endogenous. However, an endogeneity test equivalent to the Hausman test (Baum, Schaffer, and Stillman 2007) indicates that Teacher Salary might be treated as exogenous.8

Hahn et al. (2004) show that the conventional two-stage least squares (2SLS) estimators perform poorly with weak instrumental variables. The Jackknife 2SLS estimator and Fuller’s k-class modified limited information maximum likelihood (LIML) method perform better than the conventional 2SLS method, especially when instrumental variables are weak with finite samples like those in this paper. Therefore, I used Fuller’s k-class modified LIML (Fuller 1977: Baum, Schaffer, and Stillman 2007).9

6. FINDINGS AND DISCUSSIONS

6.1 REGRESSION RESULTS

Table 2 reports the regression result for Equation (2) under the column, Test Model. For a comparison purpose, the regression results from two models are also reported: the base model and the DY model. As noted earlier, all variables except for Interaction and DY Interaction are in natural log. For Tax Share, Per Pupil 2011 Property Valuation in natural log was used as an instrumental variable. FTE Administrator Number in each school district was used as an instrument for Lump-sum Aid. Logged Efficiency Index for each school district developed based on Duncombe and Yinger (1998, 2009) and Eom et al. (2014) was used as an instrument for both Interaction and DY Interaction.10

Since the number of excluded instruments is equal to the number of endogenous regressors, the order condition is met for all three models. The rank condition must be met for identification as well. Kleibergen-Paap rk LM statistics reject the null of underidentification for all three models. As indicated above, the weak instrument problem happens when the correlations between excluded instruments and endogenous regressors are nonzero but small. Under potential heteroscedasticity, Kleibergen-Paap rk F statistic is recommended as

8. The ivreg2 in STATA was used for endogeneity tests as well as instrumental variable regressions.

9. \( \alpha \) is set to 4 because it yields estimators with smallest mean squared errors with modified limited information estimators as Fuller (1977) indicates.

10. Details are available upon request to the author.
a weak instrument test (Kleibergen and Paap 2006; Baum, Schaffer, and Stillman 2007). All three models report the statistic larger than 10, thus indicating the instruments are acceptable.\footnote{Details of first-stage regression results, including weak-identification-robust inference test results, are available upon request to the author.}

### Table 2. Regression Results (Dependent Variable = Per Pupil Expenditure)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Base Model</th>
<th>Test Model</th>
<th>DY Model</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>-7.61 (0.004)</td>
<td>-2.87 (0.23)</td>
<td>-6.87 (0.004)</td>
</tr>
<tr>
<td>Teacher Salary</td>
<td>0.74 (0.000)</td>
<td>0.57 (0.000)</td>
<td>0.57 (0.000)</td>
</tr>
<tr>
<td>Formula ADM</td>
<td>-0.07 (0.000)</td>
<td>-0.04 (0.009)</td>
<td>-0.05 (0.001)</td>
</tr>
<tr>
<td>Students in Poverty</td>
<td>-0.02 (0.292)</td>
<td>-0.02 (0.411)</td>
<td>-0.02 (0.157)</td>
</tr>
<tr>
<td>Students with Disability</td>
<td>0.02 (0.653)</td>
<td>-0.02 (0.612)</td>
<td>-0.01 (0.874)</td>
</tr>
<tr>
<td>Nonwhite Students</td>
<td>0.02 (0.051)</td>
<td>0.01 (0.321)</td>
<td>0.02 (0.013)</td>
</tr>
<tr>
<td>Tax Share (\frac{A}{V} \cdot (1 - m))</td>
<td>-0.65 (0.000)</td>
<td>-0.70 (0.000)</td>
<td>-0.87 (0.000)</td>
</tr>
<tr>
<td>Lump-sum Aid (A)</td>
<td>0.25 (0.001)</td>
<td>-0.00 (1.00)</td>
<td>0.11 (0.113)</td>
</tr>
<tr>
<td>Median Income</td>
<td>0.64 (0.005)</td>
<td>0.51 (0.000)</td>
<td>0.79 (0.001)</td>
</tr>
<tr>
<td>Owner-occupied House Units</td>
<td>-0.13 (0.058)</td>
<td>-0.07 (0.332)</td>
<td>-0.39 (0.001)</td>
</tr>
<tr>
<td>Population 15-19 Years Old</td>
<td>0.15 (0.016)</td>
<td>0.24 (0.001)</td>
<td>0.32 (0.000)</td>
</tr>
</tbody>
</table>

| Interaction \(\frac{A}{V} \cdot (1 - m)\)    | 6.28e-08 (0.000) |                    |                 |
| DY Interaction \(\frac{A}{V} \cdot (1 - m)\) | 6.65 (0.000)     |                    |                 |

<table>
<thead>
<tr>
<th>Underidentification Test (Kleibergen-Paap rk LM Statistic)</th>
<th>4.354</th>
<th>4.748</th>
<th>4.551</th>
</tr>
</thead>
<tbody>
<tr>
<td>Chi-sq (1) P-val = 0.0369</td>
<td>4.748</td>
<td>4.551</td>
<td>4.551</td>
</tr>
</tbody>
</table>

| Weak identification Test (Kleibergen-Paap rk F Statistic)    | 18.919| 11.138 | 12.016|

| R²                                                          | 0.9997 | 0.9998 | 0.9998 |

**Instrumental Variable**

Per Pupil 2011 Property Valuation, 2012 Administrator Number, Efficiency Index

Note: All values except for Lump-sum Aid * Substitution Effect and DY Interaction are in natural logarithm. There were two missing cases due to log transformation, so N = 607. Values of Pr > |z| are in parentheses.
All variables carry expected signs when statistically significant. The coefficients of Tax Share range between -0.65 and -0.87. The estimated values are slightly higher than those reported from previous studies (Fisher 2016, 57; Rockoff 2010; Duncombe and Yinger 1999, 2011). A possible reason for the discrepancy might be because the previous studies mostly used \( T \) as tax price while Tax Share further incorporates \( 1 - m \). The estimated coefficient values of Median Income are fairly consistent with those reported in previous studies.

Now the question is whether consumer-voters are likely to mistake lump-sum aid for matching aid, so the aid reduces their marginal tax price. As suggested earlier, a straightforward way is to test whether the coefficient of Interaction is statistically significant or not. Another interesting point is that the P value of Lump-sum Aid in the test model is completely insignificant (P = 1.00) while Interaction is statistically significant (P = 0.000). This observation statistically indicates that the two variables are highly redundant and Interaction ultimately explains how Lump-sum Aid and matching share together affect school expenditure. This is strong empirical evidence that consumer-voters do mistake lump-sum aid for matching aid, especially they perceive as if lump-sum aid has the substitution effect that reduces their marginal tax price. The next section shows more elaborate comparison among the base model, the test model, and the DY model.

6.2 THE FLYPAPER EFFECT AND MATCHING IMPACT VIA LUMP-SUM AID

Using estimated coefficients of the base model in Table 2, we can first obtain an approximate value of the fiscal impact of lump-sum aid, which will be close to that based on the conventional model that does not account for the interaction between lump-sum aid and the substitution effect. The dependent variable is Per Pupil Expenditure in natural log, \( \ln E \). Taking partial derivatives of \( \ln E \) and Lump-sum Aid in natural log, \( \ln A \), with respect to \( A \) leads to Equation (3) since \( E \) is a function of \( A \) and other factors:

\[
\frac{\partial}{\partial A} \ln E = \frac{1}{E} \frac{\partial E}{\partial A} = \frac{\partial}{\partial A} a \ln A = \alpha = \frac{1}{A} \quad (3)
\]

---

12. Let \( y = \ln E \), where \( E = f(A) \). By the chain rule, 
\[
\frac{d}{dA} y = \frac{d}{dA} \ln E = \frac{1}{E} \frac{dE}{dA} = \frac{1}{f(A)} \frac{df}{d(A)} = \frac{1}{E} E'.
\]

Since all other factors are held constant, by definition this derivation means a partial derivative for the dependent variable.
Therefore, the partial derivative of $E$ with respect to $A$, $\frac{\partial E}{\partial A}$, will be $\frac{1}{A}$. The latter value will be an approximation of the fiscal impact of lump-sum aid under the conventional model. Using sample mean values of $A$ and $E$, and the coefficient of $\alpha$ (e.g., 0.251214 of Lump-sum Aid for the base model in Table 2), the fiscal impact is about $0.693$. This value is larger than those reported in previous findings, $0.25$ to $0.5$.

As noted above, the fiscal impact of lump-sum aid is much stronger than the fiscal impact from an equal amount of personal income. Based on the same method above, we can compute the fiscal impact of Median Income by using the coefficient of Median Income (e.g., 0.642333 for the base model in Table 2). The estimated value is about $0.18$, somewhat higher than the previous findings that were about $0.05$ to $0.1$ as introduced earlier. The impacts of Median Income from the test model and the DY model were about $0.15$ and $0.23$. These values are much higher than the previous findings but one thing is consistent with the literature: the fiscal impact of lump-sum aid is much stronger than that of personal income.

Now, we can turn to our main research question: what is the estimated fiscal impact of lump-sum aid if we account for the interaction between lump-sum aid and the substitution effect that consumer-voters might erroneously perceive as coming from the lump-sum aid? In a similar method used above, taking partial derivatives of $ln E$ and $\left(\frac{A}{1-m}\right)$, with respect to $A$ leads to Equation (4), by treating $\left(\frac{m}{1-m}\right)$ as constant:

$$\frac{\partial}{\partial A} lnE = \frac{1}{E} \frac{\partial E}{\partial A} = \frac{\partial}{\partial A} \beta \left(\frac{A}{1-m}\right) = \beta \cdot 2A \cdot \left(\frac{m}{1-m}\right)$$

Using sample means of $A$, $E$, and $m$, and the coefficient of Interaction, $\left(\frac{A}{1-m}\right)$, (e.g., 6.28e-08 for the test model in Table 2), we can compute $\frac{\partial E}{\partial A}$ as $\beta \cdot 2A \cdot \left(\frac{m}{1-m}\right) \cdot E$. The estimated fiscal impact of lump-sum aid is about $0.691$, which is virtually the same as that estimated from the base model. This makes sense intuitively. In the base model, Lump-sum Aid absorbs the substitution effect that consumer-voters perceive coming from the lump-sum aid. Interaction in the test model clearly shows that the link between Lump-sum Aid and the substitution effect is real. The DY Model in Table 2 further enables us to compute the fiscal impact of the lump-sum aid on Per Pupil Expenditure via $DY Interaction$. The estimated impact, based on the method similar to those used above, is about $0.52$. $DY Interaction$ is developed based on the rationality assumption on the part of consumer voters. However, this elab-
orate measure does not seem to capture non-rational perceptions of consumer voters.

There is another interesting question: what is the fiscal impact of matching share, \( m \), on per pupil expenditure via the lump-sum aid? In the same method used above, taking partial derivatives of \( \ln E \) and \( \left( A \frac{Am}{1-m} \right) \) with respect to \( m \), in this case, leads to Equation (5), by now treating \( A \) as constant:

\[
\frac{\partial}{\partial m} \ln E = \frac{1}{E} \frac{\partial E}{\partial m} = \frac{\partial}{\partial m} \beta A^2 \left( \frac{m}{1-m} \right) = \beta \cdot A^2 \cdot \frac{1}{(1-m)^2} \quad (5)
\]

We can estimate the fiscal impact of a unit change in \( m \) on \( E \), \( \frac{\partial E}{\partial m} \), as \( \beta \cdot A^2 \cdot \frac{1}{(1-m)^2} \cdot E \). If we want to know the fiscal impact of a 0.01 increase in \( m \), we need to divide the estimated impact by 100 because the unit value of \( m \) is one. The estimated impact of a 0.01 increase in matching share on Per Pupil Expenditure via the lump-sum aid is about \$107.25, which is a strong fiscal impact on local school spending through the lump-sum aid.

In sum, matching share does impact local school spending through lump-sum aid, empirically supporting the assertion that consumer voters tend to mistake lump-sum aid for matching aid or similar tax-price-reducing mechanisms that generate the substitution effect. As noted earlier, however, one should be aware that we need to exert caution whenever we extend this research to multi-purpose general local jurisdictions.

7. CONCLUSION

Numerous studies have consistently reported that lump-sum aid has much stronger fiscal impacts on recipient governments’ spending on the aided service than an equal amount of increase in consumer-voters’ income does. This phenomenon has been known as the flypaper effect. According to one theory, consumer-voters mistake lump-sum aid for matching aid. While matching aid reduces their tax price of the aided service, they tend to perceive that lump-sum aid renders the decrease in their tax price. Consumer-voters misunderstand that the substitution effect of matching aid comes from lump-sum aid and as a result, they demand the aided service more and thus, its expenditures grow. This fiscal illusion effect is more likely when both lump-sum aid and matching aid (or equivalent tax-price reducing mechanisms) coexist and there is a high likelihood of interaction between the lump-sum aid and the substitution effect. While the literature generally assumes the interaction or the fiscal illusion as one of potential causes for the flypaper effect, surprisingly few studies have developed a clear formula to evaluate the fiscal illusion effect.
This paper fills this huge gap in the literature by developing a formula to measure the fiscal impact, which focuses on the interaction between lump-sum aid and the substitution effect that matching aid typically renders. The interaction effect clearly proves one of the theories why the fiscal impact of lump-sum aid is much stronger than that of personal income. Consumer voters are likely to perceive as if lump-sum aid has the substitution effect, thereby demanding higher levels of governmental services. In addition, the fiscal impact of matching share on per pupil spending of local school districts in Ohio is noticeably significant. The findings in this paper further suggest that policymakers can take advantage of the interaction between lump-sum aid and the substitution effect. When lump-sum aid and tax-price reducing mechanisms, which are potentially linked to lump-sum aid, coexist, lump-sum aid might show much stronger fiscal impacts on the aided services of recipient governments. The measure of the substitution effect in this paper can be easily applied to matching shares in matching aid and different types of local governments in other states.

REFERENCES


