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On the Causality Between Household and Government Spending on Education: A Panel Analysis Across Countries

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Abstract

This paper sheds light on an important causality which is of primary interest for policy makers, both at country level as well as broad institutional level, though it is largely ignored in the literature. Using panel data from a diversified group of countries and after controlling for various factors and endogeneities within the context of multivariate models, we present evidence that an increase in the intensity of government spending on education leads to an overall increase in the intensity of household spending on education of a roughly equal magnitude, within a span of two years. We further find that the reverse causality does not hold. Specifically, a 1% increase in the intensity of government spending on education induces a contemporaneous increase in the intensity of household spending on education of 3%, followed by a correction of 2% the subsequent year. Our mediation analysis within our set of variables suggests that the causality is only direct, and that there is no statistically significant distinction between low- and high-income countries.

Keywords: Household Spending on Education, Government Spending on Education, Causality, Credit Market

JEL classification: E2, G5, I22

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1. Introduction

Human capital accumulation via investment in education receives a lot of attention in the literature as it constitutes an engine of economic growth (e.g. Lucas, 1988; Barro, 2001; De La Fuente and Domenéch, 2006) and the foundation of understanding earnings and inequality (e.g. Castelló and Doménech, 2002; Guvenen and Kuruscu, 2010; Galor, 2011).[1] This paper does not constitute another study that relates education and growth but one that focuses on an aspect of investment in education that is largely ignored in the literature. Specifically, we shed light on the causality between the intensities of government and household spending on education. This causality constitutes a major instrument of optimal policy and thus is of significant interest for policymakers both at the country level and at broad institutional level. Do governments respond to changes in the intensity of household spending on education by revising their spending intensity or is it the other way around or both, and to what extent?

In examining the relationship between education and growth, the literature measures education either using expenditures or outcomes such as the number of years at school, literacy rates and school enrolment rates. Since our aim is to examine the causality between government and household actions for education, a natural approach for this paper is to use expenditures which are directly comparable between households and governments. Furthermore, estimates of causalities using panel data could be very sensitive to the measurement differences in outcomes as well as the possible biases in the sampling of outcomes across countries, which are not always easy to address.[2] For those reasons, we perceive measures of expenditures as relatively more standard measures of education within the context of a panel household-government comparison. Specifically, we use expenditures as a share of GDP for both households and governments, which we define as spending intensities on education.

Although knowledge can be transmitted without a formal schooling system, educating the masses effectively requires a certain infrastructure which takes the form of formal schooling. The

[1] Educational expenditure is implied to be one of the most substantial forms of human capital investments, because new learning, skills, and knowledge cannot be measured easily. A review of the literature that examines the effect of education on growth can be found in Benos and Zotou (2014).

[2] E.g. Benhabib and Spiegel (1994) raise the issue of the measurement of literacy rates. They note that apart from differences in the quality measurement across countries, data for literacy may suffer from bias due to the skewness of sampling towards urban areas, and the fact developed countries usually exhibit literacy rates which are close to unity. In general, educational systems differ substantially between countries and despite attempts to harmonize the levels of education in a standardized system (e.g. International Standard Classification of Education by UNESCO), significant cross-country discrepancies could still exist.

establishment of the latter requires significant funds which are provided by households and governments. As noted by Kelly (1997), the pertinent need for efficient government expenditure on education can transform the economic landscape of the country. Optimal government spending on education does not only lead to routes out of poverty and towards long run economic development but also constitutes a tool of overcoming and mitigating economic downturns.[3] Hence, the analysis of the relationship between government and household spending on education is critically important, primarily for policymakers. Understanding the causality, its magnitude and the timing of the aggregate bilateral effect, facilitates better design of assistance programs of institutions such as the IMF and the World Bank as well as better formulation of fiscal policies towards education at the country level.[4]

To estimate the causality between government and household intensities on education, we employ a diversified cross-country panel within the context of bivariate and multivariate models. In doing so, we allow for possible effects from a set of mediator variables. One of the mediator variables that we consider is a proxy for credit market tightness. While the earlier literature finds little evidence on the importance of credit constraints as a factor determining educational attainment (see Lochner and Monge-Naranjo, 2015), a growing recent literature finds stronger evidence (see Belley and Lochner, 2007, Bailey and Dynarski, 2011, Lochner and Monge-Naranjo, 2012, Johnson, 2013, and Hai and Heckman, 2017). To control for credit market risk while considering possible indirect effects of credit market constraints on the causality, we approximate credit risk with the share of non-performing loans in total loans. When banks are unable to collect interest payments from loans which are non-performing, they have less liquidity available to create new loans and thus new borrowers face fewer loan options. Credit restrictions induced by non-performing loans could be an important link connecting the intensities of household and government spending on education. For instance, intensified spending activity by households may increase credit risk which, in turn, may affect the accessibility of households to credit markers. Limited accessibility to credit markets may induce an increased government spending intensity.

[3] Sylwester (2002) uses a cross section of countries and finds that increasing expenditures on education reduces income inequality, as measured by the Gini coefficient. Similar results are reported by Chu et al. (1995), Gupta et al. (1999) and Jung and Thorbecke (2003).

[4] Our analysis is not concerned with the allocation of government spending on education across households or regions Although Judson (1998) finds that the allocation of investment in education matters for economic growth, the finding relies on certain assumptions due to data limitations. There is no sufficient cross-country data to measure accurately the allocation effects of government spending on education. After all, we consider allocation effects as being less relevant within the context of the current study.

Likewise, intensified spending activity by governments may reduce credit risk, increasing the accessibility of households to credit markers. Other possible mediator variables that we consider are consumer prices, as measured by the consumer price index, the state of the economy, as measured by the unemployment rate, and population density.

Our main findings are robust and indicate that the causal relationship between the intensities of government and household spending on education is dynamic and only direct. It runs from the former to the latter, exhibiting a significantly positive contemporaneous effect and a weaker negative lagged effect in the subsequent year. The *correction* in the household spending intensity following the change in the government spending intensity, is such that, overall, there is a one-to-one relationship between the two intensities. That is, when the spending intensity of the government increases by a percentage point, the spending intensity of households also increases by a percentage point within a span of two years. Although we find some evidence that the causality works the other way round for high income countries under a bivariate model, this feature disappears in the multivariate model which controls for other factors, contemporaneous relationships, cross-country dependence, homoskedasticity and within countries autocorrelation. Our findings further suggest that credit market restrictions as proxied by the share of non-performing loans do not impact the causality either directly or indirectly. Our mediation analysis shows that neither the unemployment rate nor CPI nor population density play a statistically significant role in driving the causality between the two intensities.

The rest of this paper is organized as follows. Section 2 presents bivariate causality tests, section 3 extends the analysis within the context of multivariate models and section 4 concludes.

2. Bivariate Causality Tests

We begin our analysis by presenting bivariate causality tests between household and government spending on education using cross-country panel data. To examine the possible link between government and household spending on education via credit constraints we also consider bivariate causality tests between education expenditures and a proxy of credit constraints.[5] The

[5] The idea is that if households are credit constrained and thus are unable to borrow to fund their education, the government may step in to subsidize education enabling more people to attain it. Therefore, there might be an indirect link embedded in the causality between household and government spending on education.

ability of households to borrow is considered because it may affect the causality both directly, as mediator, and indirectly, as control. In our cross-country analysis, we express both household and government spending on education as percentages of GDP, which we refer to as intensities. Our measure for household spending on education is *initial household funding of secondary education* which corresponds to total payments of households for educational institutions in secondary education, excluding any government transfers to households. The reason we use secondary education funding is twofold. On the one hand data availability for secondary education allows us to construct a sample with more countries and on the other hand, we consider the cross-country medium level of education as the most indicative measure of household spending on education. Our measure for government spending on education corresponds to total government expenditures which includes expenditures for all levels of education. The source for household and government spending on education are obtained from the data bank of the World Bank.[6] Specifically, HEX_{it} and GEX_{it}, for $i = 1, 2, \ldots N$ and $t = 1, 2, \ldots T$, denote the logarithms of the ratios of household and government spending on education to GDP, respectively.

Measuring the difficulty for households in accessing credit markets, which translates to credit constraints, especially within the context of panel data analysis is a bit tricky. The literature employed various methods to proxy credit constraints.[7] In this paper, we measure the cost of bank financing for household using the logarithm of the ratio of non-performing loans to total gross loans, denoted by NPL_{it}. As shown by Huljak et al. (2020), an increase in the change in *NPL* ratios tends to depress bank lending volumes and widens bank lending spreads. Thus, we assert that the higher the current share of non-performing loans the more credit constrained the households are as banks become less willing to lend. The data on non-performing loans and total loans are obtained from the IMF's International Financial Statistics database. The frequency of our data is annual, the sample period spans from 2004 to 2018 and our panel contains a mixture of 40 developed and developing countries.[8] Descriptive statistics of the variables are displayed in the appendix.

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[6] The data are collected by the UNESCO Institute for Statistics from official responses to its annual education survey.

[7] E.g. Dehejia and Gatti (2005) use the share of private credit issued by deposit-money banks to GDP.

[8] The countries in our panel are the following: Argentina, Australia, Austria, Burundi, Cambodia, Cameroon, Chile, Colombia, Cyprus, Czech Republic, Denmark, El Salvador, France, Ghana, Guatemala, Iceland, India, Indonesia, Israel, Italy, Kazakhstan, Kuwait, Latvia, Lebanon, Lithuania, Malawi, Malta, Mexico, Nepal, Pakistan, Paraguay, Peru, Poland, Portugal, Slovak Republic, Slovenia, Spain, Tajikistan, Uganda, Ukraine.

Table 1 displays results from the procedure proposed by Dumitrescu & Hurlin (2012) – referred hereafter as DH approach-, which is an extension of Granger's bivariate framework in testing stationarity in panel data. The null hypothesis is that x does not Granger cause y and the table presents p-values for the standardized statistics $\bar{Z}$ and $\tilde{Z}$, for the whole sample, a subsample that contains only the low-income countries and a subsample that contains only the high-income countries.[9] The p-values for the $\bar{Z}$ statistic suggest that there is bivariate causality between *HEX*, *GEX* and *NPL* that runs both ways in all samples apart from causality $NPL \rightarrow HEX$ which does not hold for the whole sample and the low-income sample. The $\bar{Z}$ statistic however is more appropriate when both T and N are relatively large, and T being large relative to N. The $\tilde{Z}$ statistic on the other hand, corresponds to the case where N is large relative to T and $T > 5 + 3K$, where K denotes the number of lags for x. Since $T = 18$, $N = 40$ and the maximum K is found to be 3 using BIC, the $\tilde{Z}$ statistic appears to be the most appropriate measure for our samples. The results based on the $\tilde{Z}$ statistic firstly indicate that for the low-income and whole sample the causality only runs from GEX to HEX at the 1% and 5% significance levels, respectively, and for the high-income sample it runs only from HEX to GEX at the 10% significance level. This leads us to conclude that the weakening of the statistical significance of $GEX \rightarrow HEX$ in the whole sample relative to that of the low-income sample is due to the reversal of the causality in the high-income sample. Secondly, the causalities are only direct as there are no secondary statistically significant causalities to support indirect effects.[10] It is also worth highlighting two more aspects of those results. First, the fact that causality $NPL \rightarrow GEX$ is statistically supported in the whole sample but in none of the other two subsamples, causes doubts about the validity of this causality. Since $NPL \rightarrow GEX$ holds for the whole sample, one would naturally expect it to also hold in at least one of the two subsamples. Second, while causality $HEX \rightarrow NPL$ is statistically insignificant in the whole sample, it appears to be highly statistically significant in the two subsamples. These results are consistent with one another, as long as the effects of HEX on NPL in the two subsamples go

[9] The second-generation panel unit root test of Im, Pesaran and Shin (1997) indicates that GEX_{it}, HEX_{it} and NPL_{it} are all level stationary at 5% significance levels, without including a trend.

[10] Even though causalities $NPL \rightarrow GEX$ for the whole sample and $HEX \rightarrow NPL$ for the two subsamples are statistically significant, neither of them supports indirect effects of GEX on HEX and HEX on GEX respectively. In the first case, the effect is not triggered by GEX while in the second case, although the effect is triggered by HEX, NPL has no effect on GEX.

in opposite directions in a way that they cancel one another. Our estimates suggest that this is indeed the case.

Table 1: Granger non-causality test – Dumitrescu & Hurlin (2012)

H_0	whole sample		high income		low income	
	p-values					
	$\bar{Z}$	$\tilde{Z}$	$\bar{Z}$	$\tilde{Z}$	$\bar{Z}$	$\tilde{Z}$
$HEX_{it} \nrightarrow GEX_{it}$	0.00***	0.15	0.00***	0.09*	0.01***	0.17
$NPL_{it} \nrightarrow GEX_{it}$	0.00***	0.03**	0.00***	0.15	0.02***	0.21
$GEX_{it} \nrightarrow HEX_{it}$	0.00***	0.02**	0.00***	0.26	0.00***	0.00***
$NPL_{it} \nrightarrow HEX_{it}$	0.33	0.92	0.00***	0.45	0.30	0.29
$GEX_{it} \nrightarrow NPL_{it}$	0.05**	0.80	0.00***	0.90	0.01***	0.96
$HEX_{it} \nrightarrow NPL_{it}$	0.00***	0.14	0.00***	0.01**	0.00***	0.00***

*** 1% significance level; ** 5% significance level; * 10% significance level.
Annual data for the period 2004-2018; whole sample, 40 countries; high income, 29 countries; low income 11 countries.
The lags for each case were chosen using BIC; the minimum number of lags is found to be 1 and the maximum 3.

3. Multivariate Models

Although the DH bivariate causality approach is useful as a preliminary diagnostic tool, it suffers of three shortcomings. First, the results for the effect of *x* on *y* are based solely on the impact of lagged values of *x*, ignoring any possible contemporaneous relationship between *x* and *y*. Second, the DH approach only considers the direct relationship between *y* and *x*, ignoring the presence of mediator variables and thus possible indirect relationship between *y* and *x* via, say, *z*. Third, the DH approach does not consider the impact of other control variables in estimating the causality. To address those issues in quantifying the causal relationships, we examine the following multivariate model:

$$\boldsymbol{A}_i \boldsymbol{Y}_{i,t} = \boldsymbol{A}_{i,0} + \sum_{j=1}^{K} \boldsymbol{A}_{i,j} \boldsymbol{Y}_{i,t-j} + \boldsymbol{\varepsilon}_{i,t}, \tag{1}$$

For country $i = 1, 2, \ldots N$ and year $t = 1, 2, \ldots T$, where $\boldsymbol{Y}_{i,t}$ is an *M*x1 vector of endogenous variables for country i, $\boldsymbol{A_i}$ is an *MxM* matrix which captures the contemporaneous relationships between the variables in $\boldsymbol{Y}_{i,t}$, $\boldsymbol{A}_{i,0}$ is an *Mx1* vector of intercepts and $\boldsymbol{\varepsilon}_{i,t}$ is an *Mx1* vector of *iid* error terms. Apart from $HEX_{i,t}$, $GEX_{i,t}$ and $NPL_{i,t}$, $\boldsymbol{Y}_{i,t}$ consists of three additional variables as controls. These variables are the logarithm of the consumer price index ($CPI_{i,t}$) which summarizes the effect of consumer prices, the unemployment rate ($UNP_{i,t}$) which summarizes the effect of market conditions, and the logarithm of population density ($POP_{i,t}$) which captures the concentration of population and the related effect.

Estimating (1) as a structural VAR however poses two additional problems. The first problem is the limitation that (1) would be estimated separately for each country with rather limited time series data and without exploiting the cross-country dimension of the data. In other words, there would be one set of estimates for each country obtained using a relatively short country sample. The second problem is that estimation of (1) requires the identification of the elements in $\boldsymbol{A}$ which entails identifying restrictions that originate from theory. The problem is that there is no theory to guide us through regarding specific restrictions on the covariance matrix which would allow us to identify with relative confidence the coefficients in $\boldsymbol{A}$. To address those problems, we adopt a rather heuristic approach by estimating directly and separately each equation included in equation (1) using cross-country data. Before proceeding with estimation, we ensure that all variables in our panel including the controls are stationary. While *CPI* is found to be stationary using the Im-Pesaran and Shin (1997) test, *POP* and *UNP* are stationary only in first differences and thus we include them as such. In the stationarity tests we excluded trends and statistical significance is concluded at the 5% level. The regressions are specified as follows:

$$y_{i,t} = \alpha^y + f_i^y + \boldsymbol{\beta}^y(L)\boldsymbol{Y}_{i,t} + \varepsilon_{i,t}^y, \tag{2}$$

where, $\boldsymbol{Y}_{i,t} \equiv [HEX_{i,t} \quad GEX_{i,t} \quad NPL_{i,t} \quad CPI_{i,t} \quad POP_{i,t} \quad UNP_{i,t}]'$ and $\boldsymbol{\beta}^y(L)$ is a 1 x 6 vector of polynomials in the lag operator, L, with elements $\beta_s^y(L) = \sum_{j=0}^{K} \beta_{s,j}^y L^j$ if $y \neq s$ and $\beta_s^y(L) = \sum_{j=0}^{K-1} \beta_{s,j+1}^y L^{j+1}$ if $y = s$, for $y, s \equiv HEX, GEX, NPL, CPI,\ \Delta POP, \Delta UNP$. Parameter α^y is the country invariant intercept and f_i^y is the time-invariant country specific intercept. Note that the use of logarithms allows us to interpret the coefficients in the regression as elasticities. Noticeably, there is an endogeneity problem as $y_{i,t}$ may also affect variables in $\boldsymbol{Y}_{i,t}$; e.g. $HEX_{i.t}$ may affect $GEX_{i,t}$ as $GEX_{i,t}$ may affect $HEX_{i,t}$. Therefore, the method of ordinary least squares is not an appropriate one to estimate the model. To deal with endogeneity, we estimate (2) by employing the two-step system Generalized Method of Moments (GMM), as suggested by Arellano and Bond (1991), Arellano and Bover (1995) and Blundell and Bond (1998).[11] This estimation procedure also deals with fixed country effects, heteroskedasticity and autocorrelation within countries and it is designed for dynamic panels with small *T* and large *N*. The endogeneity is dealt using lagged values of the regressors as instruments. Specifically, the sets of regressors, Y, and instruments, Z, are both $NT \times (5 + 6K)$. Y is partitioned to $Y = [Y_1\, Y_2]$, where Y_1 contains the five time *t* endogenous variables, excluding the dependent variable, and Y_2 contains the lagged values of those variables at $t-1, t-2, \ldots, t-K$, including the lags of the dependent variable. Z is partitioned to $Z = [Z_1\, Z_2]$, where Z_1 contains the six time $t-K-1$ variables and Z_2 contains the five time $t-1$ variables, excluding the dependent variable and the lags of all six variables at $t-2, \ldots, t-K+1, t-K$. Thus, Z_1 corresponds to the six excluded from the regression instruments while Z_2 contains the $5 + 6(K-1)$ included in the regression instruments. This implies that the number of excluded instruments equals the number of endogenous variables. Since the number of regressors equals the number of instruments, the equation is exactly identified.

There are two major issues that need to be tested: cross-sectional dependence (CD) and the validity of the instruments. As Sarafidis and Robertson (2006) highlight, cross-sectional dependence in the errors of a panel regression implies that all estimation procedures that rely on instrumental variables and the GMM are inconsistent for large *N* relative to *T*. It may not only cause efficiency loss but also yield invalid test statistics. The Pesaran (2004) CD test, displayed on table 2, shows that the p-values of the null hypothesis of cross-sectional independence are close to zero for all six regressions. Hence, clearly estimation of (2) suffers from strong cross-sectional dependence which causes doubts about the validity of the estimated coefficients. Notably, the estimated coefficients, which are not reported for sake of brevity, show a rather weak causality in terms of significance that runs only from *HEX* to *GEX*, contrary to what it is suggested by the DH bivariate test for the whole sample under $\tilde{Z}$.[12]

[11] The model is estimated using the xtabond2 command in Stata (Roodman, 2009).

[12] Specifically, *HEX* induces a positive contemporaneous effect on *GEX* and a significantly larger negative lagged effect. Both effects appear to be statistically significant at the 10% level. The full set of estimates are available upon request.

Table 2: Pesaran CD test on the errors of two-step system GMM

	dependent variables y					
	$HEX_{i,t}$	$GEX_{i,t}$	$NPL_{i,t}$	$CPI_{i,t}$	$POP_{i,t}$	$UNP_{i,t}$
$\alpha^y + \boldsymbol{\beta}^y(L)\boldsymbol{Y}_{i,t}$						
p-values	0.000	0.027	0.010	0.001	0.002	0.030

3 lags were used for each case according to BIC.

To control for cross-sectional dependence, we follow a common approach which replaces the dependent variable $y_{i,t}$ with $\breve{y}_{i,t} = y_{i,t} - \bar{y}_t$, where $\bar{y}_t \equiv \left(\sum_{j=1}^{N} y_{j,t}\right)/N$. In other words, we proxy the common country component using the cross-country average for each t and then subtracting it from each observation of the dependent variable. The aim of subtracting the common component from the dependent variable is to eliminate or, at least, reduce cross-sectional dependence inhibited by the disturbances. Our findings suggest (see table 4) that incorporating the dependent variables net of the common country component decreases substantially cross-sectional dependence. This is evident, as shown on table 4, by the significant increase of the Pesaran p-values relative to those of table 2. Cross-sectional dependence, if it exists, is now weaker, which implies that any bias originating from the dependence is also reduced. Thus, even though the presence of cross-sectional dependence cannot be ruled out completely, the outcome of the Pesaran CD test allows us to confidently claim that the estimates are indicative and meaningful. An additional factor that makes us confident, as discussed further below, is the fact that after controlling for the common component, the result becomes consistent with the whole sample finding from the DH bivariate causality test under $\tilde{Z}$.

Table 3 displays the estimated coefficients of the six panel regressions. In summary, table 3 confirms the main result of the DH bivariate test for the whole and low-income sample under $\tilde{Z}$, That is, only *GEX* causes *HEX*, while the causality is only direct. The latter is inferred from the fact that we do not find statistically significant mediators, among our set of variables, that would support an indirect relationship. Moreover, our estimates suggest that an increase of *GEX* by 1% affects *HEX* both in the current year as well as in the subsequent year, inducing an overall increase in *HEX* of a roughly equal percentage. Specifically, an increase in the intensity of government spending of education by 1% increases *HEX* on impact by about 3% and decreases it the following year by about 2%.

Table 3: Dynamic Panel Data Estimation: Two-step System GMM

	dependent variables					
regressors	$\widetilde{HEX}_{i,t}$	$\widetilde{GEX}_{i,t}$	$\widetilde{NPL}_{i,t}$	$\widetilde{CPI}_{i,t}$	$\widetilde{POP}_{i,t}$	$\widetilde{UNP}_{i,t}$
$HEX_{i,t}$		-0.223	-0.911	0.576	0.057*	9.319
		(0.49)	(1.72)	(0.68)	(0.03)	(7.35)
$GEX_{i,t}$	3.192**		1.941	0.351	0.188	-0.004
	(1.63)		(1.95)	(0.93)	(0.16)	(4.04)
$NPL_{i,t}$	0.121	-0.223		0.062	0.082	21.80
	(0.657)	(0.17)		(0.24)	(0.51)	(19.06)
$CPI_{i,t}$	3.435	1.485	-7.868*		-0.056**	-1.474
	(4.54)	(2.13)	(4.55)		(0.02)	(2.02)
$POP_{i,t}$	-1.333	0.040	0.606	33.38**		8.568
	(9.87)	(2.71)	(2.99)	(17.19)		(25.02)
$UNP_{i,t}$	0.073	0.015	.2103*	0.003	0.004	
	(0.18)	(0.05)	(0.12)	(0.09)	(0.01)	
$HEX_{i,t-1}$	0.733***	0.158	-1.313	-0.284	-0.045*	-6.128
	(0.13)	(0.33)	(1.10)	(0.63)	(0.02)	(2.81)
$GEX_{i,t-1}$	-1.98**	0.664***	0.428	-0.385	-0.095	0.281
	(1.09)	(0.13)	(1.091)	(0.52)	(0.10)	(2.38)
$NPL_{i,t-1}$	-0.163	0.176	1.078***	-0.062	0.051***	1.436
	(0.56)	(0.16)	(0.26)	(0.24)	(0.02)	(26.07)
$CPI_{i,t-1}$	-4.485	-2.434	10.266	1.809***	-0.052	-28.659
	(6.35)	(3.21)	(6.31)	(0.52)	(0.71)	(3.45)
$POP_{i,t-1}$	1.391	-23.18	19.043	-69.16**	1.639***	-3.876
	(0.89)	(2.23)	(1.89)	(34.88)	(0.64)	(0.20)
$UNP_{i,t-1}$	0.009	0.002	-0.144	0.042	0.004	0.402**
	(.06)	(0.024)	(0.10)	(0.04)	(0.01)	(2.12)
$HEX_{i,t-2}$	0.099	-0.017	-0.139	0.021	-0.014	-2.195
	(0.16)	(0.12)	(0.42)	(0.20)	(0.01)	(0.82)
$GEX_{i,t-2}$	-0.252	0.008	-0.047	-0.134	-0.032	0.487
	(0.25)	(0.10)	(0.44)	(0.12)	(0.02)	(1.08)
$NPL_{i,t-2}$	0.066	-0.005	-0.169	0.013	0.007	-0.601
	(0.14)	(0.06)	(0.31)	(0.13)	(0.01)	(5.18)
$CPI_{i,t-2}$	0.963	1.194	-1.691	-0.685	0.027	3.302
	(1.93)	(0.97)	(2.26)	(0.89)	(0.18)	(79.55)
$POP_{i,t-2}$	4.617	-1.191	-2.936	42.254*	-0.392	-26.794
	(27.7)	(7.21)	(8.75)	(23.98)	(1.28)	(0.11)
$UNP_{i,t-2}$	0.036	-0.008	0.058	0.005	0.002	0.038
	(0.05)	(0.01)	(0.05)	(0.04)	(0.01)	(1.17)
$HEX_{i,t-3}$	0.147	0.069	-0.424	-0.077	0.003	-0.915
	(0.11)	(0.11)	(0.42)	(0.25)	(0.01)	(0.92)
$GEX_{i,t-3}$	-0.767	0.265**	0.405	-0.019	-0.058	-0.120
	(0.51)	(0.11)	(0.46)	(0.17)	(0.03)	(0.36)
$NPL_{i,t-3}$	-0.000	-0.001	0.055	-0.001	-0.013	0.277
	(0.13)	(0.04)	(0.13)	(0.13)	(0.01)	(0.34)
$CPI_{i,t-3}$	0.341	-0.259	-1.047*	-0.640**	-0.204***	5.533
	(0.63)	(0.53)	(0.66)	(0.32)	(0.07)	(4.51)
$POP_{i,t-3}$	-3.283	1.156	2.344	-6.477	-0.246	18.148
	(17.98)	(4.58)	(6.64)	(8.66)	(0.65)	(54.78)
$UNP_{i,t-3}$	0.000	0.017	-0.046	0.002	0.000	-0.220
	(0.03)	(0.01)	(.05)	(0.02)	(0.003)	(0.14)
intercept	-1.267	-1.26**	-1.92	.067***	-2.602***	.067**
	(1.22)	(0.66)	(1.71)	(0.57)	(0.19)	(4.50)

*** 1% significance level; ** 5% significance level; * 10% significance level.
Numbers in parenthesis correspond to the standard deviations of the estimates.
According to the BIC criterion, we choose 3 lags for each regression.

Our findings indicate that an increase in the intensity of government spending on education encourages households to increase the intensity of their spending on education in the same year. Not only they do so but the percentage increase is three times higher than the increase in the intensity of government spending. For instance, an increase in government investment in infrastructure, say via an investment in school premises and computer labs, induces households to increase their spending significantly more, say by enrolling to private classes and purchasing new equipment. The overreaction of households to the increase in the intensity of government spending on education is followed by a "*correction*" the year after. The "*correction*" in relative spending that occurs in the subsequent year, brings the overall percentage increase of the intensity of household spending to the same level as the initial percentage increase of the intensity of government spending. Among others, we do not find any evidence that credit constraints, proxied by the share of non-performing loans, affect directly or indirectly the intensity of spending on education.

To confirm the validity of the instruments, we perform further tests which are displayed on table 4. Given that table 3 displays no evidence for mediator variables that channel indirect relationships between GEX and HEX, we report results only for the first two main regressions. First, we check for serial correlation in the residuals of the system GMM by employing the Arellano-Bond test. The results for the main regressions are displayed in the first two columns. While the first-order measure is found to be statistically significant for both regressions with p-values 5.2% and 6.5%, respectively, the second-order measure is clearly statistically insignificant as the p-values are as high as 82% and 66%, respectively. The presence of first-order serial correlation is not surprising since residuals in first differences correlate by construction. On the other hand, the absence of second-order serial correlation implies that residuals are uncorrelated in levels which suggests that the instruments are strictly exogenous. As shown on table 4, statistics from the Sargan and Hansen tests of overidentifying restrictions are in line with the Arellano and Bond test results. Specifically, the Sargan-Hansen result suggests that the instruments are jointly uncorrelated with the error term as the null hypothesis of overidentifying restrictions cannot be rejected.

Table 4: Two-step System GMM: test results

	dependent variables			
	$\overline{HEX}_{i,t}$	$\overline{GEX}_{i,t}$	$HEX_{i,t}$	$GEX_{i,t}$
regressors	$GEX_{i,j}$	$HEX_{i,j}$	$\overline{GEX}_j$	$\overline{HEX}_j$
$j = t, t-1, t-2, t-3$	$t^{**}, t-1^{**}$			
CD Pesaran test	0.105	0.143	0.341	0.464
Arellano-Bond, $AR(1)$	0.052	0.065	0.895	0.500
Arellano-Bond, $AR(2)$	0.824	0.658	0.870	0.812
Sargan test	0.992	0.273	0.110	0.413
Hansen test	0.940	0.243	0.206	0.384

The number of instruments is 33; $\overline{GEX}_t \equiv \left(\sum_{j=1}^{N} GEX_{j,t}\right)/N$ and; $\overline{HEX}_t \equiv \left(\sum_{j=1}^{N} HEX_{j,t}\right)/N$; ** 5% significance level.

To examine whether there are differences in the causality across -low and high-income countries, similarly to what the bivariate tests suggest, we extend (2) by introducing income-level dummies that enable us to capture possible differentiated effects. Nonetheless, the dummies are found to be statistically insignificant which indicates that, on average, income levels are irrelevant to the causality between the intensities of government and household spending on education.[13] This finding refutes the differentiated responses across the two subsamples implied by DH bivariate tests. The discrepancies between the results from multivariate and bivariate models could be attributed to the various aforementioned missing aspects of the bivariate test. Finally, to examine whether $GEX_{i,t}$ and $HEX_{i,t}$ respond to country invariant components of *HEX* and *GEX*, respectively, we replace regressors at time *t* with $\overline{HEX}_t$ and $\overline{GEX}_t$ and in all corresponding lags, and then re-estimate the models. We find that all regressors which involve the country invariant factors are highly statistically insignificant both contemporaneously and in lags. This result indicates that household spending intensities on education respond only to country specific changes in corresponding government intensities. To save on space, we do not report the full set of estimates which are available upon request. We report however the various tests for the two regressions in the last two columns of table 4 to demonstrate that the models are well-specified.

[13] To save on space, we do not report the estimates with dummies which are available upon request.

4. Conclusion

In this paper, we examine the causality between the intensities of government and household spending on education. Using data from a cross-country panel, we show that appropriate bivariate causality tests suggest that the intensity of government spending on education causes the intensity of household spending on education while the reverse does not hold. Although we find a rather weak reversal of the causality for high-income countries, we demonstrate that this reversal, and thus the differentiated responses among low- and high-income countries, disappear when we consider a multivariate model that also controls for contemporaneous relationships, cross-country dependence, homoskedasticity and autocorrelation within countries as well as country fixed effects. The result is not only particularly interesting, but also useful for policy makers as it further shows that not only the causality clearly runs from the intensity of government spending on education to the corresponding household intensity, but the effect is only direct. Our findings suggest that households tend to overreact in the year that government increases its spending intensity by increasing their own intensity three times more. The year that follows however, they *correct* their response by decreasing their spending intensity so that there is an overall one-to-one relationship between government and household spending intensities on education. Interestingly, when we approximate credit market tightness with the percentage of non-performing loans, we find no evidence that the latter affects either the intensity of household spending on education or the intensity of government spending on education.

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Appendix: Data description and Statistics

Table A: Descriptive Statistics

	Mean	Median	Maximum	Minimum	Std.Dev.
HEX_{it}	-1.182	-0.966	1.5247	-6.778	1.397
GEX_{it}	1.443	1.498	2.147	0.412	0.349
NPL_{it}	1.524	1.405	4.090	-0.581	0.8163
CPI_{it}	4.629	4.637	5.947	3.809	0.2412
POP_{it}	4.437	4.717	7.322	0.963	1.277
UNP_{it}	6.529	5.890	26.091	0.130	3.945
Observations	600	600	600	600	600

Note: Annual data for the period 2004-2018; whole sample, 40 countries. All variables, except from the unemployment rate are expressed in logarithmic form.