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Arturo Galindo
Marcela Meléndez

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Inter-American Development Bank
Department of Research and Chief Economist

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Arturo Galindo*
Marcela Meléndez**

* Inter-American Development Bank
** ECON ESTUDIO

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Abstract[1]

This paper uses a yearly dataset of plant-level investment in Colombian firms during the period 1997 to 2007 to assess the impact of a tax incentive for firms that invest in fixed assets implemented in 2004. A positive and statistically significant correlation is found between the boom observed in investment and the adoption of the tax policy. However, the correlation vanishes when year-specific effects are controlled for. This result is robust to changes in the empirical specification, changes in estimation techniques, the inclusion of additional controls, and changes in the data set, among other tests. Overall, it is concluded that the tax stimulus analyzed was ineffective in promoting investment in Colombia.

Keywords: Taxes, Investment, Colombia
JEL Codes: E22, H2, O54

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Contact information: Galindo: arturog@iadb.org; Meléndez: marcela.melendez@econestudio.com.

1. Introduction

Many public finance economists have studied the relationship between corporate income taxes, investment and growth, and have found evidence suggesting that through their impact on the user cost of capital, taxes may have adverse effects on economic activity.[2] While, this result is robust to different estimation techniques, there is much less consensus on its magnitude.[3]

Taxing the income of firms may be particularly harmful in economies where firms face financial constraints. By definition, for a constrained firm the return to its marginal investment is higher than the after-tax real interest rate. By reducing the corporate tax rate, the incentives to invest are increased by raising the return to highly potential productive investment and allowing the firm to use more of its internal funds to invest. The greater the reliance on internal funds, the greater should be the expected impact on investment of reducing corporate tax rates.

With this in mind, several countries in Latin America that have usually faced low investment rates, have implemented tax stimuli to promote investment and long-term economic growth. Chile, Colombia and Mexico among others, are countries that have explicitly adopted policies in this direction. The purpose of this paper is to explore the effectiveness of a policy of that type adopted recently in Colombia. In 2003 a 30 percent deduction of investment expenditure from taxable income was introduced. After this policy was introduced, investment boomed. Its real growth rate, which averaged 8 percent during the first half of the decade, rose to an average of 16 percent between 2004 and 2007. While to the best of our knowledge there has been no formal evaluation of the policy in question, many analysts have attributed to it an important share in the explanation of the investment boom.

It is worth noting that during the recent expansive phase of investment, many significant events were simultaneously taking place in Colombia and more broadly in Latin America. After many years of political unrest, violence in Colombia was notably reduced, most likely due to government efforts oriented to strengthening the military. Overall the country experienced an increased perception of security. In addition, the region as a whole was receiving unprecedented

[2] See Barro (1991), DeLong and Summers (1991), Graham (2003), and Baumol, Litan, and Schramm (2007), for discussions.

[3] Some examples of this are Summers (1981), Feldstein, Dicks-Mireaux and Poterba (1983), Cummins, Hassett and Hubbard (1996), Hassett and Hubbard (2002), Gordon and Hines (2002) and Djankov et al. (2008).

flows of capital. As noted elsewhere, pushed by favorable global financial conditions, Latin America enjoyed a period of exceptional economic and financial performance.[4]

A proper evaluation of the role of tax incentives on investment needs to account for these crucial developments. Both the increase in capital flows and the rise in security may have fuelled investment growth. The main purpose of this paper is to provide such an evaluation.

We find that while it is true that investment increased significantly after the tax stimulus was adopted, the stimulus fails to explain the rise of investment once overall economic conditions are controlled for. In other words, the behavior of investment seems to be explained much more by macroeconomic developments, probably the boom in capital flows, than by domestic tax policy. Our results are very strong and are robust to changes in the specification of the empirical model, to different measures of the tax stimulus, the control set, econometric methods, and even the dataset used to compute firm-level investment.

There are several possible reasons for these results. One is that the reduction in the effective tax rate brought about by the tax exemption may not have been large enough to affect investment significantly.[5] Another is that entrepreneurs interpreted the reduction in the current tax rate as not representing a long-term reduction in the user cost of capital, since increasing investment contemporaneously would lead to higher future tax payments once their benefits were realized. Finally, a third possible explanation is that our estimations focus solely on investment by established firms and do not incorporate the possible impact of the tax stimulus on firm entry. If so, we could be underestimating the potential impact of the tax exemption.

The main purpose of this paper is to document impact of the policy and not to estimate the determinants of investment in Colombia. Following this guideline, we do not explore in detail the causes of the failure of the policy, and instead concentrate on estimating its impact.

The rest of the paper is organized as follows. Section 2 describes tax policies in Colombia and explains in detail the tax stimulus evaluated. Section 3 presents the data used in our study. In Section 4 we describe our empirical approach and present our baseline results. A variety of robustness tests are reported in Section 5. Section 6 concludes.

[4] See Izquierdo and Talvi (2008 and 2009) for a detailed discussion.

[5] It is likely that the cross-country empirical works mentioned earlier that point to a negative relation between tax rates and investment fail to incorporate some type of relevant non-linearity.

2. Tax Policy in Colombia

The Colombian tax structure includes a significant number of exemptions for specific plants and industries that not only make the tax system complex and administratively burdensome, but also lead to the erosion of important sources of fiscal income.[6] As a consequence, despite efforts towards expanding it in the recent decades, the Colombian taxable base remains a small proportion of GDP compared to that of other Latin American countries.

Exemptions to the tax code have been justified as a corrective mechanism to reduce market imperfections. A tax exemption may be beneficial, for example, if it renders the tax structure more equal or if it promotes (discourages) the production of goods with positive (negative) externalities. Similarly, in industries that rely heavily on learning-by-doing, an infant-industry argument would suggest that tax shelters or protectionist policies, more generally, have positive long-run implications for productivity. The Colombian government claims to use tax instruments as development tools for specific sectors or to promote social and regional equality.

The potential benefits of tax exemptions have to be weighed against efficiency losses that may result from their use. Exemptions to the tax code may, for example, distort investments that would have taken place in their absence or induce firms to attempt to qualify for them at all cost, even if they are not among the original benefactors. Evidence shows that the fiscal cost of tax exemptions is often greater than the direct benefits they generate. Empirically it is difficult, however, to quantify the level of rent transfers, because lobbying pressures play a significant role in instigating tax exemptions in addition to efficiency and welfare concerns.

We are in particular interested in exploring the effect of a tax stimulus put in place in December 2003 (Law 863, 2003) aimed at promoting investment, by which firms were allowed to deduct 30 percent of investment in fixed assets from taxable income during the period 2004-07. Law 1111 of December 2006 made this policy permanent starting in 2008 and increased the share of investment in fixed assets that can be deducted to 40 percent. Most likely the long-term fiscal cost of this policy is not insignificant, and assessing its impact on investment seems crucial.

[6] According to the National Council for Economic and Social Policy (CONPES) calculations, tax benefits to specific sectors or activities amounted to USD 1,520 million (1.41 percent of GDP) in 2004. Regulations resulting in this value are still in place.

In the remainder of this section we present the tax data made available to us by the Ministry of Finance of Colombia for the period 2000-2007, which we use in the empirical exercises that follow to test if the policy explains the investment boom, and some statistics to further motivate the relevance of the questions this paper addresses.

Table 1 provides a general overview of the magnitude of the tax stimulus and its distribution across sectors. The amounts in millions of US dollars (USD) are the tax savings resulting from the fixed assets investment tax deduction. A first thing to note is that total tax benefits from this measure amounted to USD 1,694 million in 2007 and were increasing over time since 2004, both in value and as a share of total income tax revenue, perhaps due to a learning process of the private sector about the tax policy in place. A second aspect to note is that despite being transversal in character, tax benefits from investment in fixed assets have not been evenly distributed across sectors of activity and have been concentrated (as could have been expected) in the most capital-intensive sectors.

Table 1. Tax Benefits from Reduction for Investment, by Sector

ISIC 1-digit Sector	2004	2005	2006	2007	Total 2004-07	Average share (%)
Agriculture, hunting and forestry	5.8	8.5	11.4	27.0	52.7	1.4
Fishing	0.1	0.4	0.3	0.6	1.5	0.0
Mining and quarrying	121.6	186.6	248.3	408.0	964.5	26.1
Manufacturing	108.4	138.5	162.8	359.9	769.6	20.9
Electricity, gas and water supply	49.3	60.1	102.7	149.1	361.2	9.8
Construction	6.1	7.8	28.1	79.1	121.1	3.3
Wholesale and retail trade; repair of motor vehicles, and household goods	53.3	62.7	89.8	163.0	368.8	10.0
Hotels and restaurants	1.4	2.4	4.5	6.2	14.5	0.4
Transport, storage and communications	60.6	66.6	201.7	339.1	668.0	18.1
Financial intermediation	15.4	21.5	30.3	54.4	121.6	3.3
Real estate, renting and business activities	13.3	20.9	29.5	64.8	128.4	3.5
Public administration and defence; compulsory social security	0.4	0.4	1.2	1.4	3.4	0.1
Education	0.9	1.2	1.4	1.4	4.9	0.1
Health and social work	4.2	10.2	12.5	14.0	40.8	1.1
Other community, social and personal service activities	14.3	14.0	14.1	26.3	68.7	1.9
Total tax benefits (USD million)	**455.1**	**601.8**	**938.7**	**1,694.2**	**3,689.8**	**100**
Income tax revenue (USD million)	**9,626.3**	**10,490.9**	**12,190.6**	**13,148.4**	**45,456.1**	
Tax benefits as share of income tax revenue (%)	**4.7**	**5.7**	**7.7**	**12.9**	**8.1**	

Source: Ministry of Finance, DIAN and calculations from the authors. Monetary values are 2008 pesos converted to USD at that year's average exchange rate.

Table 2 presents the tax stimulus further disaggregated for the manufacturing sector, on which this paper focuses for data availability reasons.[7] Savings from the fixed assets investment tax deduction follow a similar pattern over time, showing substantial increase since 2004, when the measure was put in place. In 2007 they reached an amount equivalent to 31 percent of the income tax payable by the manufacturing sector. Also as before, tax benefits from this measure were not evenly distributed across subsectors but rather concentrated in a small number of them. For instance, *Food, Beverages and Tobacco* and *Chemical, Petroleum, Coal, Rubber and Plastic products* captured 38 percent and 22 percent of the tax savings between 2004 and 2007, respectively.

Table 2. Tax Benefits from Reduction for Investment, by Manufacturing Sector

ISIC 2-digit sectors	2004	2005	2006	2007	Total 2004-07	Average share (%)
Food, Beverages and Tobacco	30,875	52,076	52,441	155,866	291,258	37.9
Textile, Wearing Apparel and Leather Industries	10,711	8,913	9,471	17,240	46,335	6.0
Wood and Wood Products, Including Furniture	3,127	3,035	4,520	7,714	18,395	2.4
Paper and Paper Products, Printing and Publishing	7,909	16,127	14,480	33,853	72,368	9.4
Chemical, Petroleum, Coal, Rubber and Plastic	28,297	30,379	41,339	66,458	166,472	21.7
Non-Metallic Mineral Products	14,677	9,656	18,445	29,487	72,264	9.4
Basic Metal Industries	3,969	6,673	9,891	21,657	42,190	5.5
Fabricated Metal Products, Machinery and Equipment	8,756	11,531	12,186	26,744	59,218	7.7
Total tax benefits (USD thousand)	**108,320**	**138,390**	**162,773**	**359,018**	**768,501**	**100.0**
Income tax revenue (USD thousand)	**899,110**	**913,719**	**1,150,641**	**1,157,652**	**4,121,122**	
Tax benefits as share of income tax (%)	**12.0**	**15.1**	**14.1**	**31.0**	**18.6**	

Source: Ministry of Finance, DIAN and calculations from the authors. Monetary values are 2008 pesos converted to USD at that year's average exchange rate.

To assess the extent of exemptions at the sector-level and the importance of the investment in fixed assets tax reduction, we construct a tax benefit measure in the form of an effectively paid tax rate based on the data available for the 2000-2007 period. A firm's income tax burden can decrease either due to exemptions that reduce the firm's taxable income directly—as in the case of the tax stimulus under examination—or due to explicit reductions in the income tax payable.

[7] We were able to obtain firm-level investment data for the manufacturing sector from DANE's Annual Manufacturing Survey. These data will be described in more detail in the following section.

The tax data provided by the Colombian Ministry of Finance allow us to calculate the effective income tax rate paid by each sector of the economy. We compute it as the product of the nominal tax rate and the ratio between the effective tax payment and the tax payment that would have resulted in the absence of exemptions. These variables were calculated from data made disaggregated at the four-digit ISIC level.[8]

While the nominal tax rate is set uniformly across all sectors at an average of 34.9 percent over the period, the average effective income tax rate paid by the manufacturing sector after applying the fixed assets investment tax reduction amounts to only 29.5 percent (without accounting for this tax reduction it equals 31 percent). As shown in Table 3, the variance in exemption rates is large.

Table 3. Income Tax Exemption Rates, before Tax Reduction

		2000	2001	2002	2003	2004	2005	2006	2007	Average
Food, Beverages and Tobacco	**Mean**	**0.80**	**0.81**	**0.81**	**0.87**	**0.88**	**0.91**	**0.80**	**0.90**	**0.85**
	St. Dev.	0.19	0.15	0.16	0.13	0.14	0.13	0.29	0.20	0.17
Textile, Wearing Apparel and Leather Industries	**Mean**	**0.92**	**0.90**	**0.93**	**0.96**	**0.95**	**0.96**	**0.94**	**1.00**	**0.95**
	St. Dev.	0.07	0.08	0.06	0.06	0.07	0.05	0.11	0.01	0.06
Wood and Wood Products, Including Furniture	**Mean**	**0.89**	**0.91**	**0.96**	**1.00**	**1.00**	**1.00**	**0.98**	**0.99**	**0.97**
	St. Dev.	0.19	0.12	0.03	0.00	0.00	0.00	0.03	0.03	0.05
Paper and Paper Products, Printing and Publishing	**Mean**	**0.67**	**0.71**	**0.66**	**0.67**	**0.70**	**0.61**	**0.59**	**0.65**	**0.66**
	St. Dev.	0.37	0.37	0.40	0.39	0.41	0.40	0.43	0.32	0.39
Chemical, Petroleum, Coal, Rubber and Plastic	**Mean**	**0.90**	**0.90**	**0.86**	**0.87**	**0.92**	**0.93**	**0.88**	**0.96**	**0.90**
	St. Dev.	0.12	0.12	0.23	0.24	0.13	0.09	0.14	0.06	0.14
Non-Metallic Mineral Products	**Mean**	**0.80**	**0.86**	**0.88**	**0.98**	**0.98**	**0.98**	**0.95**	**0.96**	**0.92**
	St. Dev.	0.21	0.12	0.11	0.04	0.03	0.02	0.11	0.06	0.09
Basic Metal Industries	**Mean**	**0.95**	**0.91**	**0.92**	**0.98**	**0.82**	**0.84**	**0.83**	**0.97**	**0.90**
	St. Dev.	0.04	0.09	0.09	0.04	0.23	0.23	0.27	0.04	0.13
Fabricated Metal Products, Machinery and Equipment	**Mean**	**0.92**	**0.93**	**0.94**	**0.97**	**0.99**	**0.95**	**0.95**	**0.99**	**0.96**
	St. Dev.	0.15	0.17	0.14	0.08	0.03	0.18	0.15	0.03	0.12
Other Manufacturing Industries	**Mean**	**0.87**	**0.87**	**0.84**	**0.89**	**0.94**	**0.91**	**0.94**	**1.00**	**0.91**
	St. Dev.	0.13	0.18	0.28	0.19	0.07	0.17	0.11	0.00	0.14
Average exemption rate		0.86	0.87	0.87	0.91	0.91	0.90	0.87	0.93	0.89
Nominal tax rate		0.35	0.35	0.35	0.35	0.35	0.35	0.35	0.34	0.35

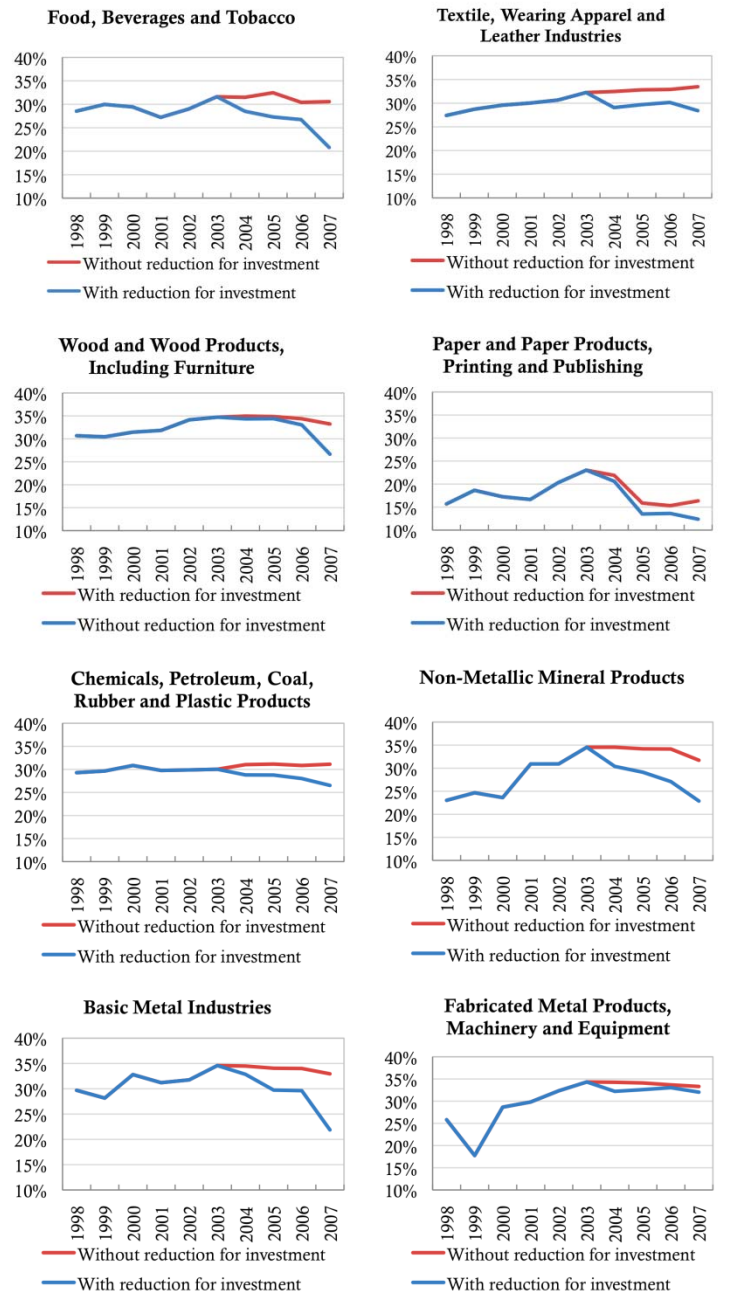
Source: Ministry of Finance, DIAN and calculations from the authors.

Figure 1 illustrates the effect of the fixed assets investment tax reduction on effective income tax rates computed as described above. The difference between the 4-digit sector-level effectively paid tax rates with and without the tax reduction, that measures the incremental tax benefit from this policy, is one of the variables we use in estimation.

[8] The Ministry of Finance provided detailed tax data for 89 (out of 127) ISIC 4-digit Revision 3 manufacturing sectors.

Finally, Table 4 presents total manufacturing corporate tax benefits as a share of both total government income and total manufacturing corporate taxes. These numbers evidence once again the magnitude of the tax stimulus granted by Law 863 of 2003 and the potential fiscal cost of the tax reduction extension granted by Law 1111 of 2006.

Figure 1. Effectively Paid Corporate Tax Rates



Source: Ministry of Finance, DIAN and authors' calculations.

Table 4. Tax Revenue Loss from incentives to Manufacturing (USD million)

Year	Central government income	Corporate tax revenue from manufacturing	Corporate tax reductions and exemptions to manufacturing		
			Total	% of government income	% of corporate tax revenue
2000	19,031.4	681.2	139.8	0.7%	20.5%
2001	22,058.2	704.7	145.4	0.7%	20.6%
2002	22,409.7	762.9	139.0	0.6%	18.2%
2003	23,946.4	821.4	120.0	0.5%	14.6%
2004	25,761.7	899.1	212.7	0.8%	23.7%
2005	27,971.2	913.7	235.7	0.8%	25.8%
2006	33,068.9	1,150.6	293.6	0.9%	25.5%
2007	36,062.1	1,157.7	447.6	1.2%	38.7%
Total	**210,309.7**	**7,091.4**	**1,733.8**	**0.8%**	**24.4%**

Source: Central Bank of Colombia, DIAN and calculations from the authors. Monetary values are 2008 pesos converted to USD at that year's average exchange rate.

3. Data

In addition to the sector-level tax data described in the previous section, obtained from the Ministry of Finance, we use two additional datasets. Our primary data source is the Annual Manufacturing Survey of Colombia ("Encuesta Anual Manufacturera," henceforth EAM) collected by the Colombian Statistical Office, DANE. The survey is a complete census of the manufacturing sector that accounts for approximately 15 percent of Colombian GDP and is available from 1977 to 2007.[9] For the purpose of this study we have constructed a panel dataset for the period 1997-2007 that allows us to follow plant-level performance over time. We drop all observations of plants exiting before 2004 (before the fixed assets investment tax reduction was put in place), and we also drop observations of plants appearing only one year in the data or belonging to firms that own multiple plants (only 3 percent of plants belong to multi-plant firms, so the data loss is very small).

Table 5 provides a flavor of manufacturing performance during the period under analysis using our dataset. We find sustained positive output growth at impressive rates since 2003. These numbers do not yet indicate the slowdown of the years that follow.

[9] Data for 2007 are still under revision at DANE. Access to them was granted for this study on the condition that we acknowledge they still are preliminary.

Table 5. Output by Manufacturing Subsector

ISIC 2-digit sector	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	Average Share (%)
Food, Beverages and Tobacco	9,866	10,528	9,917	10,121	10,833	11,240	11,494	11,901	12,257	13,070	13,477	27.2
Textile, Wearing Apparel and Leather Industries	3,168	3,168	2,792	3,438	3,606	3,423	3,682	4,058	4,206	4,567	4,659	8.9
Wood and Wood Products, Including Furniture	565	529	408	466	529	524	600	702	798	900	1,088	1.6
Paper and Paper Products, Printing and Publishing	2,070	2,080	2,151	2,314	2,299	2,345	2,645	2,701	2,914	3,006	3,087	6.0
Chemical, Petroleum, Coal, Rubber and Plastic	10,019	10,121	10,121	12,104	12,155	12,155	14,444	16,275	17,190	19,173	20,750	33.7
Non-Metallic Mineral Products	1,902	1,734	1,368	1,643	1,729	1,536	1,811	1,831	2,024	2,222	2,660	4.5
Basic Metal Industries	783	737	687	1,277	1,236	1,460	2,100	2,599	2,868	3,667	4,033	4.7
Fabricated Metal Products, Machinery and Equipment	4,964	4,359	3,026	3,626	4,353	4,359	4,445	5,696	6,561	7,781	9,104	12.7
Other Manufacturing Industries	287	286	240	264	277	269	305	336	354	369	391	0.7
Total output (USD thousand)	33,625	33,542	30,710	35,253	37,016	37,309	41,526	46,098	49,173	54,756	59,249	100.0
Annual growth (%)		-0.2	-8.4	14.8	5.0	0.8	11.3	11.0	6.7	11.4	8.2	

Source: DANE's Annual Manufacturing Survey and calculations from the authors.
Note: Monetary values are 2008 pesos converted to USD at that year's average exchange rate.

Our other data source is the financial statements firm-level dataset from Superintendencia de Sociedades (henceforth Supersociedades) that we use in combination with the EAM dataset to produce measures of firm-level financial constraints. We also use it to perform a robustness check on our econometric results. This dataset is a firm-level panel that covers all economic sectors but is only representative for medium and large firms.[10] For this reason, estimations using variables from this dataset are done on subsamples of the EAM data. It is also because firm-level accounting data cannot be broken out by plant (cases of multi-plant firms) that we chose to keep in the EAM dataset only single-plant firms. As stated, the majority of plants fall in this category.

Table 7 reports the number of firms in the EAM dataset that appear in the Supersociedades dataset and comply with the mandatory reporting size thresholds each year. The number of medium and large manufacturing firms that comply with the thresholds falls over time.

[10] Firms with assets exceeding 5,000 legal minimum wages each year must report their financial statements to Superintendencia de Sociedades. Smaller firms appear in the data, but not necessarily on a regular basis and are not legally obliged to report. We drop them from the dataset to avoid biases from selection and data errors.

Table 6. Manufacturing as a Share of Total Economic Activity

Year	GDP	Investment	Employment
1998	14.1	13.8	2.9
1999	13.8	19.6	2.6
2000	14.5	22.1	2.5
2001	15.0	16.9	2.3
2002	14.7	15.5	2.4
2003	15.1	12.3	2.3
2004	15.6	8.5	2.5
2005	15.6	8.8	2.5
2006	15.9	10.8	2.8
2007	16.2	12.0	2.8
Average	15.0	14.0	2.6
Std. Deviation	0.8	4.5	0.2

Note: Investment is gross investment.
Source: DANE Annual Manufacturing Survey, National Household Survey, Continuous Household Survey and authors' calculations.

Table 7. Manufacturing Firms in Financial Statements Dataset

Year	EAM	Supersociedades	(%)
1997	6635	1351	20.4
1998	6513	1292	19.8
1999	6281	1243	19.8
2000	6173	1196	19.4
2001	5948	1138	19.1
2002	5916	1176	19.9
2003	6048	1135	18.8
2004	6101	1080	17.7
2005	6370	1039	16.3
2006	6241	1000	16.0
2007	6127	955	15.6

Source: DANE Annual Manufacturing Survey, Supersociedades dataset and authors' calculations.

4. Empirical Approach and Baseline Results

To assess the effect of the fixed assets investment tax reduction empirically, we use a variety of estimation approaches and model specifications. As described above, we can compute ISIC 4-digit sector level effective tax rates with and without the fixed assets investment tax reduction. While we have to deal in estimation both with the fact that our policy variables are available at a

lower disaggregation level than our measures of performance, and with the potential simultaneity bias arising from the fact that by construction firms investing in fixed assets see their effective corporate tax rates reduced, the availability of tax data permits a richer empirical exploration that we carry out as follows.

We use the ISIC 4-digit sector level tax rates calculated to build three policy variables that we use alternatively: (1) the difference between the effective corporate tax rate that would have prevailed without the fixed assets investment tax reduction and the effective corporate tax rate paid ("tax rate reduction"); (2) the ratio of tax savings from the fixed assets investment tax reduction to output ("tax benefit as % of output"); and (3) an dummy variable equal to one if the sector claimed the fixed assets investment tax reduction, and equal to zero otherwise. If indeed the tax reduction helped to boost investment, these variables should obtain a positive coefficient in estimation.

All policy variables enter estimation lagged to control for potential biases from simultaneity. This concern is also minimized by the fact that these variables are sector level as opposed to firm level. To control for the fact that we use data at different aggregation levels, standard errors are robust and clustered at the sector level in all regressions using firm-level data.

Our baseline model is the following investment equation:

$$\left(\frac{I}{K}\right)_{i,t} = \alpha_i + \lambda_t + PolicyVariable_{n,t-1}\beta_{PV} + \varepsilon_{i,t} \quad (1)$$

where α_i is a firm-specific fixed effect; λ_t is a year-specific fixed effect; $PolicyVariable_{n,t\text{-}1}$ denotes the alternative policy variables described above; $\varepsilon_{i,t}$ is the error term; and β_{PV} is the coefficient of interest. For this baseline model, as well as for all other regressions estimated to check the robustness of our results, we present estimation results both with and without controlling for time-specific fixed effects.

The dependent variable is investment in year t divided by capital at the end of the previous year:

$$\left(\frac{I}{K}\right)_{i,t} = \frac{\sum_j I_{j,t}}{\sum_j K_{j,t-1}} \quad (2)$$

where j indexes three types of capital: machinery and equipment, office equipment and transportation equipment. Table 8 presents summary statistics of the variables we use in estimation. To avoid biases from measurement error we drop from our dataset firms with investment to capital ratios smaller than -1 and greater than 3 in any year.

Table 9 reports our baseline results. Columns (1) – (3) report estimations of equation (1) without including the year fixed effects (λ_t) and columns (4) – (6) replicate them with the year effects. Columns (1) and (4) use the tax rate reduction as the policy proxy, (2) and (5) use the tax ratio savings, and (3) and (6) use the dummy variable indicating if in the firm's sector, the reductions were claimed.

Columns (1) – (3) suggest that tax policies could have played a role in explaining the investment boom in Colombia. In all three estimations the tax policy is statistically significant. Estimated coefficients suggest a sizeable impact of the policy. A one standard deviation increase in the tax policy measures of columns (1) and (2) is associated with 0.019 and 0.014 increases in the ratio of investment to capital of firms in sectors using the tax benefit, respectively. Column (3) suggests that a firm in a sector that claimed the benefit had an investment rate 3.5 percentage points higher than a firm in a sector that did not claim the benefit. These numbers are significant, when taking into account that on average I/K in our full sample is 0.155, including years in which the tax incentive under evaluation was not available.

Table 8. Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
(a) Full Sample					
I/K	36119	0.155	0.302	-0.999	2.999
Tax rate reduction (t-1)	34276	0.307	0.017	0	0.260
Tax benefit as % of output (t-1)	34276	0.001	0.005	0	0.316
Dummy =1 if tax benefit used in (t-1)	34276	0.283	0.450	0	1
Output (in logs)	36119	14.485	1.819	7.021	22.117
(a) Firms in sectors that used the tax benefit in (t-1)					
I/K	9693	0.194	0.285	-0.975	2.897
Tax rate reduction (t-1)	9693	0.026	0.023	0	0.260
Tax benefit as % of output (t-1)	9693	0.003	0.010	0.000	0.316
Dummy =1 if tax benefit used in (t-1)	9693	1	0	1	1
Output (in logs)	9693	14.721	1.785	7.021	22.117

When controlling for year fixed effects in columns (4) – (6) all of the results reported in columns (1) – (3) vanish. Including for year effects allows us to control for every possible countrywide variable that may be affecting investment. This may include developments in national security, nationwide regulatory changes, local macroeconomic developments, or regional shocks such as an increase in capital flows to Latin America and other emerging markets. Our exercises do not allow us to specifically address what has driven investment in Colombia. What they do show, however, is that once we control for possible relevant determinants, the impact of the tax policy is eliminated. A possible explanation for finding significant results in columns (1) – (3) and insignificant elsewhere is that the policies were adopted at the same time when other relevant country-specific and worldwide events were occurring and the policy variables were serving as a proxy for them. Once these are controlled for, the policy variables are reflecting the true impact of the policy, which in this case is null.

Table 9. Baseline Results: Fixed Effects Panel Regressions

Dependent variable: I/K	(1)	(2)	(3)	With time dummies (4)	With time dummies (5)	With time dummies (6)
Tax rate reduction (t-1)	0.843**			0.25		
	[0.201]			[0.193]		
Tax benefit as % of output (t-1)		1.364*			0.214	
		[0.649]			[0.160]	
Dummy =1 if tax benefit used in (t-1)			0.035**			0
			[0.007]			[0.018]
Constant	0.141**	0.146**	0.137**	0.212**	0.212**	0.212**
	[0.001]	[0.001]	[0.002]	[0.009]	[0.009]	[0.009]
Observations	34276	34276	34276	34276	34276	34276
R-squared	0.21	0.21	0.21	0.24	0.24	0.24

Note: All regressions include firm-level fixed effects. Robust standard errors in brackets. * significant at 5%; ** significant at 1%.

In the following section, we present a wide variety of robustness exercises that in the end carry the same message of our baseline scenario. Without controlling for year fixed effects, there seems to be a positive correlation between the tax reduction policy and investment. Once we control for them we find no impact of the analyzed policy on investment.

5. Robustness Exercises

We present a wide variety of exercises to check the robustness of the baseline results. We develop seven alternative checks using firm level data, and three using data aggregated at the sector level.

The first two robustness exercises consist of adding extra controls and are reported in Tables 10 and 11. In Table 10, in addition to the other regressors, we control for firm size, measured as the firm-level lagged real output (in log form). Size is expected to be a potential driver of investment. Presumably, especially in economies where financial constraints prevail, larger firms are more likely to invest more than smaller ones. In fact as shown in Table 10, size is positive and statistically significant. When controlling for size the estimated coefficients for the tax policy variables remain significant, as long as we do not control for year effects. Once we control for year effects, as above, the impact of the tax policy vanishes. In addition, we also ran some non-reported regressions in which we interacted the policy variables with the size variable. The results were insignificant across all specifications, suggesting that firms of different size did not react differently to the tax policy.

Table 10. Controlling for Firm Size
(Firm level: robustness check 1)

Dependent variable: I/K	(1)	(2)	(3)	With time dummies (4)	With time dummies (5)	With time dummies (6)
Tax rate reduction (t-1)	0.773**			0.246		
	[0.193]			[0.193]		
Tax benefit as % of output (t-1)		1.268*			0.227	
		[0.585]			[0.159]	
Dummy =1 if tax benefit used in (t-1)			0.032**			-0.001
			[0.007]			[0.018]
Log output (t-1)	0.019**	0.022**	0.017**	0.010+	0.010+	0.010+
	[0.006]	[0.006]	[0.006]	[0.005]	[0.005]	[0.005]
Constant	-0.135	-0.176+	-0.113	0.069	0.068	0.068
	[0.089]	[0.093]	[0.084]	[0.079]	[0.079]	[0.079]
Observations	34276	34276	34276	34276	34276	34276
R-squared	0.21	0.21	0.21	0.24	0.24	0.24

Note: All regressions include firm-level fixed effects. Robust standard errors in brackets. + significant at 10%; * significant at 5%; ** significant at 1%.

In Table 11 we control for the number of kidnappings (in log form) in the region where the firm is located, a measure of the violent conflict that varies both across years and firm locations. As noted above, at the same time that the tax policy in question was implemented, Colombia went through an important transformation in its security institutions that provoked an important reduction in several dimensions of the prevailing armed conflict. It is likely that the tax variable could be proxying for this, and if violence in fact is a significant determinant of investment, it is the reduction in violence rather than the tax policy that is driving the results. In columns (1) – (3) of Table 11 we report our results using this measure of violence. In fact, and as expected, the coefficient on the kidnappings variable is negative and significant, suggesting that when violence is reduced, investment increases. A notable finding in this set of results is that once we control for violence the tax policies lose significance. This indicates that the tax policy variables are indeed proxying for something else. Interestingly, when we control for time effects the kidnappings variable also loses its significance. This may be reflecting the fact that during the period under examination violence varied more over time than across regions, with all

regions experiencing a similar drop in violence, or that there are more relevant nationwide factors explaining the dynamics of investment.

Table 11. Controlling for Armed Conflict
(Firm level: robustness check 2)

Dependent variable: I/K	(1)	(2)	(3)	With time dummies (4)	With time dummies (5)	With time dummies (6)
Tax rate reduction (t-1)	-0.058			0.218		
	[0.177]			[0.260]		
Tax benefit as % of output (t-1)		-0.684			0.05	
		[0.507]			[0.452]	
Dummy =1 if tax benefit used in (t-1)			-0.011			0.03
			[0.007]			[0.024]
Kidnappings in region	-0.011**	-0.011**	-0.013**	0.003	0.003	0.003
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Constant	0.186**	0.187**	0.199**	0.086**	0.087**	0.086**
	[0.013]	[0.014]	[0.013]	[0.018]	[0.018]	[0.018]
Observations	30835	30835	30835	30835	30835	30835
R-squared	0.21	0.21	0.21	0.24	0.24	0.24

Note: All regressions include firm-level fixed effects. Robust standard errors in brackets. ** significant at 1%.

In Table 12 we try a different approach inspired by Hsieh and Parker (2007).[11] According to Hsieh and Parker, tax benefits have different impacts on firms with different degrees of financial constraints. Firms with greater financial constraints can invest only if they have enough free cash flow to do so, since by definition they cannot access financial markets. If a tax policy increases a constrained firm's cash flow, then it will be able to invest more. If a firm is not financially constrained, its investment decision should be neutral to its cash flow, and hence, the tax policy should have no impact on its investment plan.

Like Hsieh and Parker, we construct a simple measure of financial constraints by computing the correlation coefficient between each firm's investment and its cash flow[12] for the years prior to the implementation of the policy (the years prior to 2004). We divide firms into

[11] Hsieh and Parker (2007) analyze the role of the 1984 a corporate tax reform in Chile that cut the tax rate on retained profits from around 50 percent to 10 percent.
[12] The cash flow is proxied using operating profits.

three groups according to their relative position in the distribution of these correlation coefficients. Firms in the lower third of the distribution are assumed to be unconstrained. Firms in the intermediate third are labeled as possibly constrained, and those in the top third are assumed to be financially constrained. The correlation between investment and cash flow is computed using the balance sheet data included in the Supersociedades database. We then match these data with the EAM data. A drawback of this strategy is that we lose a relevant set of information since the dataset used to construct the financial constraint index (the Supersociedades database) does not cover small firms.

With this in mind we carried out several exercises. In Table 12 we report the same regression as that reported in Table 9 for the sample of firms that we identify as financially constrained. The interpretation of the result is identical to that above. Once we control for time effects, the result of the policy vanishes. The fact that it vanishes for this set of firms that presumably should benefit more from the tax policy, suggests that in fact the tax policy had very little or no effect on firms.[13]

Table 12. Subsample of Firms Facing Financial Constraints
(Firm level: robustness check 3)

Dependent variable: I/K	(1)	(2)	(3)	With time dummies (4)	With time dummies (5)	With time dummies (6)
Tax rate reduction (t-1)	0.592**			-0.124		
	[0.220]			[0.270]		
Tax benefit as % of output (t-1)		5.808*			-0.448	
		[2.683]			[2.214]	
Dummy =1 if tax benefit used in (t-1)			0.036**			-0.104
			[0.011]			[0.065]
Constant	0.145**	0.144**	0.140**	0.032**	0.032**	0.032**
	[0.002]	[0.002]	[0.003]	[0.011]	[0.011]	[0.011]
Observations	2361	2361	2361	2361	2361	2361
R-squared	0.20	0.20	0.20	0.27	0.27	0.27

Note: All regressions include firm-level fixed effects. Robust standard errors in brackets. * significant at 5%; ** significant at 1%.

[13] We also tried these estimations restricting the sample to the subset of financially constraints only (top third of the distribution) with identical result. We tried interacting the constraint measure with the policy variables rather than splitting the sample as in the table, getting similar results.

In Tables 13 and 14, we replicate the baseline regression and the exercise on the set of financially constrained firms using a different methodology. We allow for a dynamic specification of investment by including lags of the dependent variable among the explanatory variables and use a system GMM estimator to deal for potential simultaneity biases. Our results are also robust to changes in the specification and the estimation method.

Table 13. GMM estimation (Firm level: robustness check 4)

Dependent variable: I/K	(1)	(2)	(3)	With time dummies (4)	With time dummies (5)	With time dummies (6)
I/K (t-1)	0.262	0.346	0.176	0.196	0.196	0.196
	[0.071]**	[0.073]**	[0.067]**	[0.132]**	[0.129]**	[0.128]**
I/K (t-2)	0.24	0.17	0.322	0.252	0.253	0.253
	[0.057]**	[0.059]**	[0.055]**	[0.097]**	[0.094]**	[0.094]**
Tax rate reduction	1.14			0.027		
	[0.092]**			[0.111]		
Tax benefit as % of output		2.331			-0.241	
		[0.974]*			[0.216]	
Dummy =1 if tax benefit used			0.072			0.027
			[0.006]**			[0.021]
Constant	0.046	0.056	0.032	0.157	0.16	0.132
	[0.006]**	[0.006]**	[0.006]**	[0.022]**	[0.021]**	[0.031]**
Observations	32311	32311	32311	32311	32311	32311
Number of firms	4838	4838	4838	4838	4838	4838
P-Value Hansen Stat	0.000	0.000	0.000	0.289	0.288	0.287
P-Value AR1	0.000	0.000	0.000	0.002	0.002	0.001
P_Value AR2	0.105	0.655	0.002	0.219	0.2	0.199

Note: * significant at 5%; ** significant at 1%.

Table 14. GMM Estimation, Subsample of Firms Facing Financial Constraints (Firm level: robustness check 5)

Dependent variable: I/K	(1)	(2)	(3)	With time dummies (4)	With time dummies (5)	With time dummies (6)
I/K (t-1)	0.363	0.373	0.305	0.001	-0.001	0.003
	[0.112]**	[0.119]**	[0.114]**	[0.112]	[0.112]	[0.112]
I/K (t-2)	0.126	0.111	0.163	0.22	0.22	0.221
	[0.087]	[0.093]	[0.090]+	[0.081]**	[0.082]**	[0.083]**
Tax rate reduction	1.424			0.18		
	[0.209]**			[0.248]		
Tax benefit as % of output		9.505			-0.38	
		[1.823]**			[1.737]	
Dummy =1 if tax benefit used			0.07			0.003
			[0.011]**			[0.028]
Constant	0.041	0.047	0.038	0.173	0.185	0.18
	[0.014]**	[0.014]**	[0.014]**	[0.030]**	[0.027]**	[0.034]**
Observations	2288	2288	2288	2288	2288	2288
Number of firms	313	313	313	313	313	313
P-Value Hansen Stat	0.000	0.000	0.000	0.66	0.664	0.66
P-Value AR1	0.000	0.000	0.001	0.004	0.004	0.004
P_Value AR2	0.800	0.712	0.854	0.093	0.097	0.102

Note: + significant at 10%; ** significant at 1%.

The sample used throughout the previous estimations comprises exclusively manufacturing firms. The reason for this, as noted above, is that our source of information (the EAM) is restricted to firms in the manufacturing sector. As a further robustness check, in table 14 we repeat the baseline exercise using another dataset: that of Supersociedades. While this dataset includes firms in all sectors, it covers only medium-sized and large firms. The investment data may also be less reliable than the EAM measurements of investment since the Supersociedades data are extracted from balance sheets and incorporate data that have been "accommodated" by firms for tax purposes.

Table 15 is divided into two panels. Panel (a) reports results for the full panel of firms, and panel (b) reports results for the subsample of manufacturing firms. Once again, regardless of the firm sample chosen, or the definition of investment used, the results mimic those described above.

Table 15. All Sectors, Medium and Large Firms, Supersociedades Data
(Firm level: robustness check 6)

Dependent variable: I/K	(1)	(2)	(3)	With time dummies (4)	With time dummies (5)	With time dummies (6)
			(a) Full Sample			
Tax rate reduction (t-1)	0.759**			-0.071		
	[0.138]			[0.192]		
Tax benefit as % of output (t-1)		6.705**			0.115	
		[1.262]			[1.499]	
Dummy=1 if tax benefit used (t-1)			0.068**			0.008
			[0.009]			[0.063]
Constant	0.147**	0.148**	0.143**	0.194**	0.194**	0.168**
	[0.001]	[0.000]	[0.001]	[0.012]	[0.012]	[0.010]
Observations	40124	40109	40124	40124	40109	40124
R-squared	0.001	0.001	0.002	0.128	0.128	0.128
Number of firms	9968	9965	9968	9968	9965	9968
			(b) Restricted Sample			
Tax rate reduction (t-1)	0.793**			-0.282		
	[0.145]			[0.217]		
Tax benefit as % of output (t-1)		6.636**			-1.332	
		[1.276]			[1.540]	
Dummy=1 if tax benefit used (t-1)			0.075**			-0.011
			[0.009]			[0.052]
Constant	0.163**	0.165**	0.152**	0.175**	0.176**	0.230**
	[0.001]	[0.001]	[0.002]	[0.011]	[0.011]	[0.053]
Observations	15296	15296	15296	15296	15296	15296
R-squared	0.002	0.001	0.006	0.132	0.132	0.132
Number of firms	2652	2652	2652	2652	2652	2652

Note: All regressions include firm-level fixed-effects. Robust standard errors in brackets. ** significant at 1%.

A final robustness exercise using firm level data follows Hsieh and Parker (2007) more strictly. A drawback of the approach followed above is that the relevant policy variable used varies at the sector level while investment is at the firm level. Using Hsieh and Parker's identification strategy we can compute a firm-level indicator of the tax policy to explain firm-level investment. They estimate plant-level panel regressions using indicator variables for the years after the tax reform begins, combined with measures of credit constraints, as well as time dummies and plant-level fixed effects, to explain investment to capital ratios.

We replicate their basic exercises using indicator variables for the years 2004 to 2007, interacted with the measures of financial constraints (in firm-level regressions[14]) described above. Based on Hsieh and Parker's basic plant-level investment equation, we estimate the following:

$$\left(\frac{I}{K}\right)_{i,t} = \alpha_i + \lambda_t + C_i D_t \beta_C + PC_i D_t \beta_{PC} + \varepsilon_{i,t} \qquad (3)$$

where α_i is a firm-specific fixed effect; λ_t is a year-specific fixed effect; C_i is an indicator variable of whether the firm is financially constrained; PC_i is an indicator variable of whether the firm is possible financially constrained; D_t is a vector of indicator variables for years after 2003; and $\varepsilon_{i,t}$ is the error term. Table 16 reports these results. Column (1) reports the results excluding the year fixed effects and column (2) includes them.

Table 16. Hsieh and Parker Identification (Firm level: robustness check 7)

Dependent variable	I/K (1)	I/K (2)
Dummy Constrained		
Dummy Posibly Constrained		
Dummy Constrained*2004	0.0461**	-0.0105
	[0.0175]	[0.0270]
Dummy Constrained*2005	0.0336+	0.0245
	[0.0201]	[0.0279]
Dummy Constrained*2006	-0.0126	-0.00696
	[0.0182]	[0.0283]
Dummy Constrained*2007	0.0839**	-0.033
	[0.0189]	[0.0319]
Dummy Posibly Constrained*2004	0.0391*	-0.0145
	[0.0161]	[0.0261]
Dummy Posibly Constrained*2005	0.0331+	0.0269
	[0.0201]	[0.0279]
Dummy Posibly Constrained*2006	-0.0114	-0.00293
	[0.0235]	[0.0320]
Dummy Posibly Constrained*2007	0.131**	0.0168
	[0.0256]	[0.0363]
Constant	0.130**	0.225**
	[0.00192]	[0.00577]
Observations	25087	25087
Year dummies	No	Yes
R-squared	0.248	0.279

Note: All regressions include firm-level fixed effects. Robust standard errors in brackets. * significant at 5%; ** significant at 1%.

[14] Recall that we are restricting the database to only single-plant firms.

The results once again follow the same pattern. When not controlling for year effects, the policy indicator appears to have a significant explanatory power for investment, and moreover appear to go in line with Hsieh and Parker's interpretation. The policy seems to have a significant impact on investment in constrained and possibly constrained firms. Again the results vanish when we control for time fixed effects.

The last set of robustness exercises substitutes firm-level data for sector-level data. We aggregate the data at the ISIC 4-digit sector level. By doing this we obtain sector-level measures of investment that are compatible with the tax level data used to construct the policy indicators. The drawback here is that the potential endogeneity problem is deepened, and results need to be interpreted as correlations rather than as providing evidence of causality. These regressions are interesting, however, inasmuch as they address the concern that in the previous exercises we are pairing firm-level investment with sector-level average policy measures, when it is possible that not all firms in a sector actually used the tax incentive. In the sector-level regressions we avoid this mismatch.

Table 17 reports the sector-level equivalent of our baseline regression with the same column ordering as in Table 9. Once again the results mimic the baseline scenario. Without controlling for year effects we find a positive and significant association between the tax policy variables and investment in most specifications (columns (1) and (3)). Once we control for year effects, the correlation vanishes.

Table 17. Baseline Regression
(Sector level: robustness check 8)

Dependent variable: I/K	(1)	(2)	(3)	With time dummies (4)	With time dummies (5)	With time dummies (6)
Tax rate reduction (t-1)	1.214**			0.216		
	[0.352]			[0.372]		
Tax benefit as % of output (t-1)		0.607			-0.189	
		[0.373]			[0.334]	
Dummy =1 if tax benefit used in (t-1)			0.078**			0.022
			[0.013]			[0.035]
Constant	0.115**	0.123**	0.104**	0.095**	0.270**	0.081*
	[0.007]	[0.006]	[0.007]	[0.022]	[0.022]	[0.036]
Observations	821	820	821	821	820	821
R-squared	0.10	0.09	0.12	0.24	0.24	0.24

Note: All regressions include sector-level fixed effects. Robust standard errors in brackets.
* significant at 5%; ** significant at 1%.

In Table 18, we reduce the sectors to focus on those that are more likely financially constrained. Once again we follow Hsieh and Parker on their Chile paper. Hsieh and Parker also run sector-level regressions in which they test whether investment increases at the time of the tax reform were concentrated in industries for which external finance is important, using Rajan and Zingales (1998) measures of industry dependence on external finance. By definition, financially dependent sectors are more likely to face financial constraints than others. The sample for which regressions are estimated includes sectors with a financial dependence measure greater than the sample median, computed as in Rajan and Zingales. Once again, the results are identical to those reported above.

Table 18. Financially Dependent Sectors
(Sector level: robustness check 9)

Dependent variable: I/K	(1)	(2)	(3)	With time dummies (4)	With time dummies (5)	With time dummies (6)
Tax rate reduction (t-1)	1.030*			0.13		
	[0.437]			[0.443]		
Tax benefit as % of output (t-1)		0.423			-0.278	
		[0.347]			[0.359]	
Dummy =1 if tax benefit used in (t-1)			0.079**			0.003
			[0.021]			[0.042]
Constant	0.108**	0.116**	0.095**	0.02	0.02	0.02
	[0.009]	[0.009]	[0.010]	[0.022]	[0.022]	[0.022]
Observations	402	402	402	402	402	402
R-squared	0.06	0.04	0.08	0.21	0.21	0.21

Note: All regressions include sector-level fixed effects. Robust standard errors in brackets.
* significant at 5%; ** significant at 1%.

As a final robustness exercise we replicate Hsieh and Parker's sector-level regression specification:

$$\left(\frac{I}{K}\right)_{n,t} = \alpha_n + \lambda_t + RZ_n D_t \beta_{RZ} + \varepsilon_{n,t} \quad (4)$$

where α_n is a sector-level fixed effect; λ_t is a year-specific fixed effect; RZ_n is the dependence on external finance for industry n as measured by Rajan and Zingales; and Dt is a vector of indicator variables for years after 2003. Again, we present a model specification excluding the year-

specific fixed effects that control for overall economic conditions. As reported in Table 19, the results remain the same.

Table 19. Hsieh and Parker Identification
(Sector level: robustness check 10)

Dependent variable	I/K (1)	I/K (2)
Dummy Financial Dependence *2004	0.107**	0.0278
	[0.0306]	[0.0349]
Dummy Financial Dependence *2005	0.0535*	-0.0198
	[0.021.0]	[0.0315]
Dummy Financial Dependence *2006	-0.00383	-9.79e-05
	[0.0235]	[0.0356]
Dummy Financial Dependence *2007	0.172**	0.0162
	[0.0379]	[0.0434]
Constant	0.117**	0.204**
	[0.00673]	[0.0186]
Observations	907	907
Year dummies	No	Yes
R-squared	0.12	0.23

Note: All regressions include 4-digit sector-level fixed effects. Robust standard errors in brackets. * significant at 5%; ** significant at 1%.

6. Conclusions

This paper explores the impact of a tax benefit policy adopted in Colombia in 2003 on investment. Analysts have linked the policy with the investment boom witnessed by the country in the latter half of the first decade of the twenty-first century, but to our knowledge no formal evaluation had been made to support such claim.

We find that in fact there is a strong correlation between the adoption of the policy and the investment boom, but once aggregate factors are controlled for, the correlation vanishes, suggesting that the tax policy analyzed had little or no role in explaining the boom. This result is robust to a variety of experiments. We include additional firm and sector level controls, restrict the sample to focus on the subset of firms that are most likely affected by the policy, allow for

different specifications and estimation techniques, use different measures of investment and datasets, and explore sector aggregated data, and in all cases the same result prevails.

Overall our analysis suggests that the tax reduction policy implemented in Colombia since 2003 has not promoted investment. The investment boom is instead explained by countrywide or regional factors. In this regard, the tax losses assumed by the fiscal authorities as a consequence of the tax reduction are not compensated by the generation of future income produced by the new investments it was supposed to promote.

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