Dividend Taxes and the Allocation of Capital: Comment

By LAURENT BACH, ANTOINE BOZIO, ARTHUR GUILLOUZOUIEC, AND CLÉMENT MALGOUYRES

Boissel and Matray (2022) find that investment increased after 2013 in French firms facing higher dividend taxes. We identify an alteration in the code plotting the event study of the effect of this reform on investment. Using identical data and removing this alteration, we find differential pre-trends between treated and control firms. We also establish that the controls referred to as “size growth,” used in all the difference-in-difference specifications, effectively are controls for lagged investment, i.e., the main outcome variable. Removing such controls attenuates differential pre-trends but leaves no clear event study evidence of a positive effect of dividend taxation on investment. (JEL D22, G31, G35, H25, H32)

A recurring question in public finance is whether dividend taxes affect the level and quality of corporate investment. To this question, Boissel and Matray (2022a and b) (henceforth, BM) provide a novel empirical answer using a French dividend tax increase in 2013 as a source of identification. According to their results, dividend taxes boost corporate investment, particularly so among firms with profitable investment opportunities. The paper’s core finding goes against preexisting empirical evidence suggesting that dividend taxes either hinder (Becker, Jacob, and Jacob 2013) or have no effect (Yagan 2015) on investment. Yet, precisely because its main result goes against the prior in the literature, the contribution merits a strict reassessment (Harvey 2017). In this comment, we provide such scrutiny by replicating the main results in BM with identical data sources, undoing the alteration of some coefficients included in event study plots, and assessing the sensitivity of the main result to sensible alternative specifications.

Our findings are as follows. First, we identify a line in the code plotting the event study that significantly alters two pre-reform coefficients, as it divides these coefficients by a factor of 1.8. Removing this alteration leads to visually significant differential pre-trends in the two years affected by the altering line of code. Second, we find that a key source of differential pre-trends in the investment analysis is...
the inclusion, in all the regressions generating the results in the main text, of controls for the pre-reform average level of the outcome variable, referred to as “size growth” controls in BM’s text. Running regressions without controls for pre-reform investment attenuates pre-trends, which is evidence of mean reversion of investment but leaves no clear event study evidence of a positive effect of dividend taxation on investment. The current editor informed us that the baseline specification described by BM in the conditionally accepted manuscript included “size” rather than “size growth” controls. As a result, we also provide event study evidence using the level of capital as control, in which differential pre-trends are very strong and of the opposite sign.

Our results thus suggest that, contrary to BM’s claim, one cannot conclude that the dividend tax increase in France in early 2013 had any positive effect on companies’ investment. A replication package for this comment is available on the AEA data repository and includes details on the datasets we use and the code we run to generate our graphs and numbers.

I. The Impact of the Alteration of Pre-trend Coefficients

Using the released code, Figure 1 (blue curve) replicates BM’s Figure 4, an event study plot in which the identifying assumption in BM (i.e., parallel investment trends in the pre-period) and the main result (i.e., higher investment due to the reform) are assessed. Our results replicate BM’s; hence, the data and code we use are indeed the source of BM’s analysis.

Notes: This figure plots the yearly coefficients and 95 percent confidence intervals of the difference-in-difference estimator in equation (1) of BM of the 2013 dividend tax increase. The dependent variable is total investment scaled by capital in 2011. Event study 0) simply reproduces the coefficients, applying the code provided in the replication package. Event study 1) reproduces the coefficients shown in Figure 4 of BM after removing the alteration of two pre-treatment coefficients.

Figure 1. Effect of the 2013 Tax Reform on Investment, with and without the Alteration

<table>
<thead>
<tr>
<th>Years since reform</th>
<th>Coefficient</th>
<th>CI Lower</th>
<th>CI Upper</th>
</tr>
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<tbody>
<tr>
<td>-4</td>
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<td>-0.10</td>
<td>-0.00</td>
</tr>
<tr>
<td>-3</td>
<td>-0.02</td>
<td>-0.15</td>
<td>-0.00</td>
</tr>
<tr>
<td>-2</td>
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<td>-0.13</td>
<td>-0.00</td>
</tr>
<tr>
<td>-1</td>
<td>-0.04</td>
<td>-0.14</td>
<td>-0.00</td>
</tr>
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<td>-0.11</td>
<td>0.01</td>
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<tr>
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</tbody>
</table>

1 Bach et al. (2023); INSEE and DGFiP (2012, 2019a); INSEE (2014); INSEE and DGFiP (2019b).
We identify a line in the code that alters the shape of BM’s Figure 4. The released code follows a common procedure for each event study plot in BM: first, a regression with year-by-year interactions is run to produce coefficients and standard errors; second, this regression output is used to produce an event study plot as in BM’s Figure 4. In this last step, the authors include, specifically for BM’s Figure 4, a command to divide the coefficient for year-by-year event study estimates by 1.8 for two specific yearly observations: \( t = -2 \) and \( t = -1 \) relative to the onset of the dividend tax reform. Since the original standard errors for the two altered coefficients are untouched, this alteration largely attenuates the visual significance of the gap between treated and control units in two out of four years prior to the reform.

In our replication of BM’s Figure 4 without the alteration (orange curve in Figure 1), the confidence interval excludes the null effect for the \( t = -1 \) and \( t = -2 \) interactions and hence makes clear the presence of differential pre-trends. This conclusion is in contrast to BM’s original Figure 4 (blue curve in Figure 1), in which the null effect lies within, or very close to the edge of, the confidence interval prior to the reform.

II. The Impact of Controls for Lagged Outcomes

After removing the alteration from the code, there are differential pre-trends in BM’s analysis of investment. In order to tackle the issue of potential pre-trends, BM choose as baseline (including in their Figure 4, which is their central piece of evidence) the following difference-in-difference specification:

\[
Y_{i,j,t} = \beta_{Treated,i} \times Pos_t + \theta_i + SizeGrowthBin_{i,t} + \delta_{jt} + \gamma_{ct} + \varepsilon_{ij,t}.
\]

This includes a control variable \( SizeGrowthBin_{i,t} \), which BM define as “a vector of pre-reform annualized size growth quartile-by-year fixed effects” (p. 2896). Size growth refers to capital growth, which is identical to the investment rate, i.e., the main outcome in BM. In what follows, we assess the impact of lagged outcomes as controls.²

In Figure 2, we report the event study plot of the impact of the dividend tax reform on investment, using different sets of firm-level controls. The orange curve provides the plot as in BM’s Figure 4, where controls include “size growth,” except that the coefficients for \( t = -2 \) and \( t = -1 \) are not altered, and it exhibits very significant pre-trends. In the green curve, we remove controls for the pre-reform outcome variable. The difference in pre-trends is smaller, so controls for pre-treatment investment reinforce differential pre-trends. This happens because controlling for investment measured in the pre-period grounds the investment gap between treated and control units during this period, leading to mean reversion in the periods before and after (Daw and Hatfield 2018; Chabé-Ferret 2017). Usually, controlling for pre-treatment outcomes in a difference-in-difference setting is problematic instead because this forces pre-trends to be parallel and is akin to assuming conditional

²BM argue that the results do not rely on the inclusion of specific controls, yet the event study plots in its online Appendix Figure A14 show that the effect of the reform on investment is very sensitive to controlling for size growth.
independence rather than parallel trends for identification (Roth et al. 2022). Yet, under such reasoning, a specification without controls would display more significant differential pre-trends rather than less, as is the case here. Our analysis therefore shows the importance in difference-in-difference designs of displaying event study graphs with and without controls, with an eye to both preexisting and spurious violations of the parallel trends hypothesis.

The specification without controls suggests a much lower effect of the reform on investment, if any, but still displays significant pre-trends, so the corresponding difference-in-difference estimates would not carry a causal meaning either. Since treated firms are smaller, it may make sense to correct pre-trends for differences in the level of total capital, which we do in the brown curve. Indeed, the current editor informed us that this is the baseline specification as described by BM in the version of their paper that the reviewers saw and that the handling coeditor accepted for publication conditional on compliance with the AEA’s Data and Code Availability Policy. Yet, this third specification does not deliver clearer results, as differential pre-trends are very significant in the opposite direction, possibly because high levels of capital in rather young firms reflect recent investments and hence predict lower future investment due to mean reversion.

III. Conclusion

Using the same data and code as BM, we show that its estimation of the investment impact of the French dividend tax hike suffers from an alteration, is very
sensitive to the choice of controls, and, as a result, does not provide clear evidence that dividend taxes encourage investment. Providing a specification with convincing causal evidence is beyond the scope of this comment and should be the subject of other research.

REFERENCES


