

Replication of Bronzini & Iachini (AEJ:Policy, 2014)

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Raffaello Bronzini and Eleonora Iachini empirically study the effectiveness of government grants for research and development, using data from the balance sheets of firms that applied for funding from the provincial government of Emilia-Romagna in Italy in 2003.

While there are several reasons governments might decide to support research and development, whether the programs are effective depends on the extent to which they finance marginal projects. In other words, do grants lead firms to pursue research projects that they would not be able or willing to take on without government funding? Several factors determine if grants have a marginal effect, including the program design, operating environment, and firm characteristics. Instead of funding marginal projects, grants may simply crowd out privately financed investment in research. Many firms may be able to make the research investments using internal funds, but apply for the grants anyway. Moreover, firms may act strategically, deciding only to pursue research projects when they become subsidized, and will write grant proposals for the same project across multiple years.

Bronzini and Iachini seek to answer this empirical question – are grants effective? – using a sharp regression discontinuity design (“Sharp RDD”). The authors argue this allows them to place fewer restrictions than similar studies, which in the past have relied on strong ignorability assumptions and/or instrumental variables to estimate an effect of research subsidies on investment.

Although the evidence whether government incentives increase investment in research is mixed, the authors hypothesize the program funded marginal projects – that firms receiving grants from the Emilia-Romagna government “substituted public for privately financed research and development” (pg. 102). They fail to reject this hypothesis, using a global polynomial regression discontinuity design, and two local regression discontinuity designs. They also suggest firm size affects the extent to which private investment is crowded out by grant money – that large firms receiving grants made inframarginal investments (swapping private funds with public funds), while small firms receiving grants made marginal investments.

My replication is structured as follows: I first present the regression discontinuity design and estimation strategy deployed by Bronzini and Iachini. I then present their model in the potential outcomes framework. Importantly, their paper was written and published before advancements in regression discontinuity designs, making some of their estimation choices and inferences dated or wrong. I’ve replicated their results, but instead, present estimation results using methods outlined in the slides and forthcoming book chapters. (Cattaneo et. al. 2017) I conclude by highlighting limitations and what I perceive are errors by Bronzini and Iachini.

Identification and Estimation

- Y_i is the outcome variable, indexed by firm i
- T_i is the treatment variable, where $T_i = 1$ if firm i is subsidized, and $T_i = 0$ otherwise.
- The score (denoted $Score_i$) ranged from 29 to 97 (out of a 100). Firms with $Score > 75$ were awarded grants of up to €250,000.
- $S_i = Score_i - 75$ is the distance from the cutoff.

Bronzini and Iachini consider a range of measures for investment; for simplicity and conciseness, I focus on their main outcome, post-treatment net investment normalized by pre-treatment sales.

They estimate a third-order global polynomial model using unweighted OLS:

$$\text{Equation (1): } Y_i = \alpha + \beta T_i + (1 - T_i) \sum_{p=1}^3 \gamma_p(S_i)^p + (1 - T_i) \sum_{p=1}^3 \gamma'_p(S_i)^p + \epsilon$$

The authors include parameters of the distance from the cutoff, S_i , to allow the population regression functions to differ to the left and to the right of the threshold. They also estimate equation (1) with local regressions around the cutoff, using two different sample windows (balanced quantile windows above and below the cutoff covering 50% and 35% of observations).

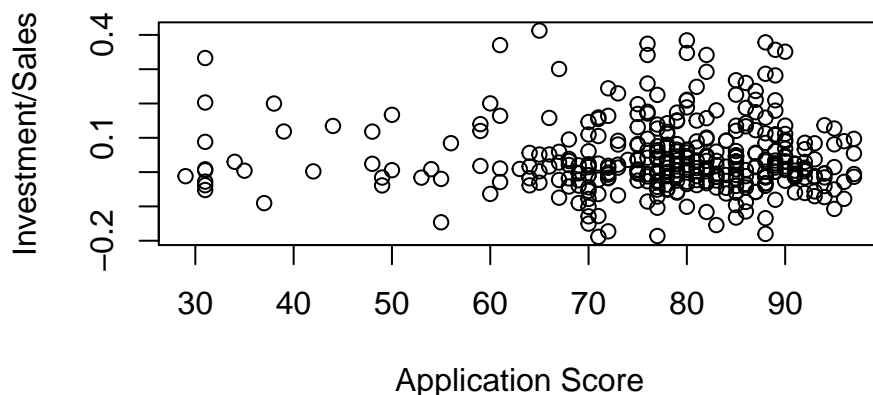
In the equation (1) model, $\hat{\beta} = 0$ is evidence of complete “crowd out,” that the firms who receive grants substitute private investment with public funds at a dollar for dollar rate. Graphically, we’d see no discontinuity or jump at $Score_i = 75$.

Lastly, as a “robustness check” they re-run equation (1) using weighted OLS, where the weights are determined by an Epanechnikov kernel.

Evaluating their design and estimation strategy

The program has all the characteristics of an RDD: a treatment, score, and cutoff.

Figure 1. Scatter of all firms



While a scatter (above) doesn’t show a difference in the outcome to the left and to the right of the cutoff, RD plots that bin the data suggest there may be a “jump” in normalized investments for firms that scored just above the cutoff and received funding. Figure 2 (below) is replicated from Page 115 of the paper, using the mimicking variance evenly-spaced method to select bins. Figure 2 looks slightly different than what is presented in their paper, due to different binning – the authors never say what they’ve done to plot the data.

Figure 2. RD Plot from Pg. 115

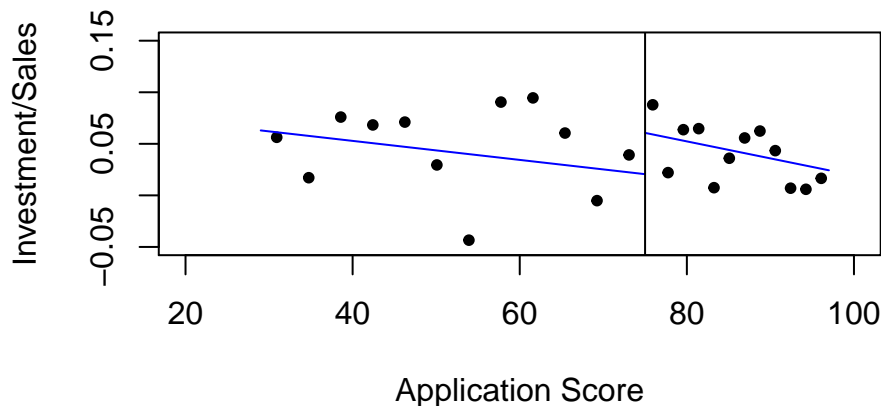
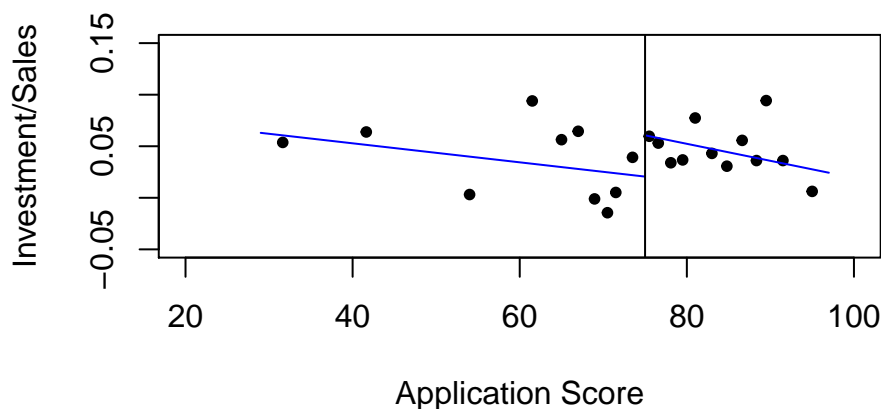


Figure 3 (below) presents the same data using the mimicking variance quantile-spaced method using spacings

estimators to select bins. While the bin choice affects the visual depiction of the discontinuity to the left and right of the program cutoff, the outcome variable varies a lot and is similar to the left and to the right of the cutoff. (*Investment/Sales* was between $[-0.189, 0.413]$ for untreated firms, and between $[-0.186, 0.384]$ for treated firms.)

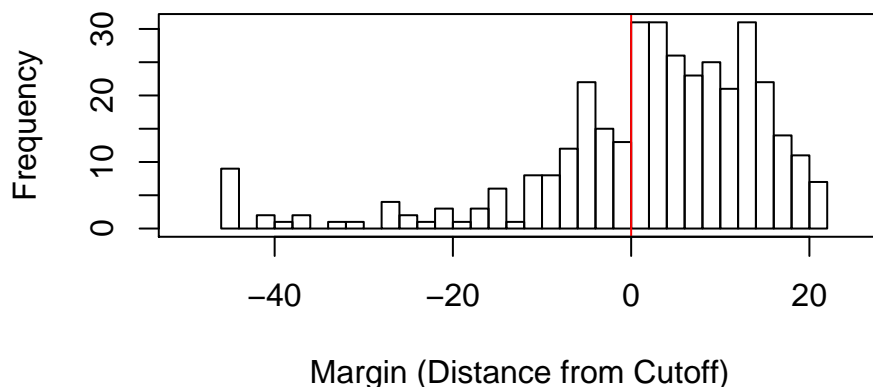
Figure 3. RD Plot (selecting bins using QSMV)



Manipulation

The density of the firms receiving grants with scores immediately to the right of the cutoff, and those not receiving grants with scores immediately to the left, is different. The authors argue this is not because firms manipulated their scores, but because application evaluators avoided awarding scores just below 75 to avoid appeals by these firms. 254 firms were awarded grants (Score > 75), and 103 were not under this program.

Figure 4. Density Check



Continuity & Placebo Checks

The authors check the continuity assumption and run placebo checks, but not until the robustness section, where they cite to an online appendix that does not include code or additional data. They check if firm characteristics that should not be determined by research expenditures, like profitability and the cost of debt, are continuous across the cutoff. They argue “if we do not observe jumps it is plausible that the outcome variable would also have been continuous without the treatment” (pg. 128). I confirm this using their estimation strategy (unweighted OLS of equation (1)): Profitability is smooth across the cutoff.

Similarly, they also check if the “outcome variable before the program is smooth across the cutoff?” This is misleading, however. They don’t give data for their main outcome variable—investments over sales—that is pre-treatment. Sales is pre-treatment, but investment is post-treatment. There’s no way for me to replicate this. However, I can re-estimate equation 1 using their estimation strategy using each of the three pre-treatment outcome variables I do have: assets, sales, and capital. None of the linear estimates for the indicator at the cutoff are statistically significant. Of course, this conclusion relies on equation (1) being correctly specified.

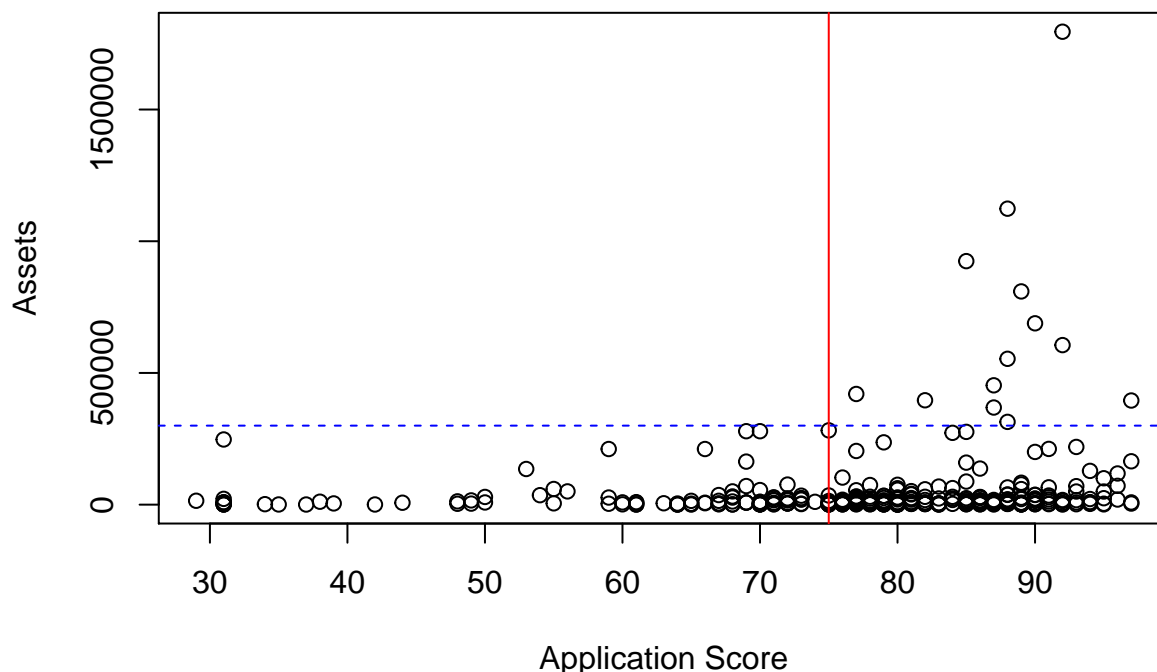
Balance

If covariates of firm characteristics change at the cutoff, then our data will be unbalanced in covariates: in this case, the treated and control units are different in ways other than whether they received treatment, confounding our estimated treatment effect. The authors check for balance, finding that untreated and treated firms are generally similar, but that treated firms are larger—with higher sales and assets (Table 1, below). We can see this visually as well, in Figure 5 (below). No treated firm had pre-treatment assets greater than €300,000 (the horizontal blue dashed line), while numerous treated firms had assets above €300,000.

Table 1: Balance Table

Variable	Difference in Means	P-Value
Sales	44694.0947940	0.0009865
Assets	33328.9567694	0.0073659
Roa	0.0193112	0.1542015
Labor_Sales	-0.0509187	0.2606673
Investment_Sales	0.0121740	0.3289892
TInvestment_Sales	0.0079053	0.4353576
ITInvestment_Sales	0.0042687	0.5369329

Figure 5. Firm Assets by Score



Estimation Results

The paper’s main result is that their estimate for β from equation (1), $\hat{\beta}$, is not distinguishable for zero at a 5% confidence level when using the full sample. However, they find that $\hat{\beta}$ is significant from zero when using a wide-window and narrow-window sample and a third order polynomial. But these coefficients have opposite signs: the narrow window suggests a jump up in investment by firms receiving grants, and the wide window suggests a jump down.

Next, they examine whether firm size affects the RD estimate. Their reasoning is that small firms typically have trouble accessing capital markets at reasonable terms, and that grants to small firms may be more likely to fund marginal projects. We also know that covariates capturing firm size (such as assets or sales) are unbalanced. To do this, they update equation (1) to include firm-size dummies interacted with the treatment T_i . The model becomes:

Equation (2):

$$\begin{aligned} Y_i = & (1 - T_i) \sum_{k=1}^2 \alpha_k Size_i^k + T_i \sum_{k=1}^2 \beta_k Size_i^k \\ & + (1 - T_i) \sum_{k=1}^2 \sum_{p=1}^3 \gamma_{kp} Size_i^k (S_i)^p \\ & + T_i \sum_{k=1}^2 \sum_{p=1}^3 \gamma'_{kp} Size_i^k (S_i)^p + \eta \end{aligned}$$

where $Size_i = 1$ if the firm is above the median, and zero otherwise, such that k (which indexes firm size) is either 1 or 2.

The authors report large and statistically significant estimates of the treatment effect for small firms, using an unweighted second-order polynomial OLS regression of equation (2) for both the full sample and the wide-window sample: $\hat{\beta} = 0.149$ and $\hat{\beta} = 0.178$ respectively. However, the estimate of the narrow-window is $\hat{\beta} = 0.053$ and not reported as statistically significant from zero.

Applying the Potential Outcomes Framework

Let’s rename the variables so that the running variable and treatment more closely match the RDD literature. Keep T_i for the treatment. Rename $Score_i$ as X_i , such that $\bar{x}_i = 75$ is the cutoff. Since this is a Sharp RD design, $D_i = T_i = 1(X_i > \bar{x}_i)$. $Y_i(1)$ and $Y_i(0)$ are the potential outcomes, which are levels of investment normalized for pre-treatment sales for treated and untreated firms respectively. The Sharp RD treatment effect is:

$$\tau_{SRD} = E[Y_i(1) - Y_i(0) | X_i = \bar{x}]$$

We can condition on a covariate Z (i.e., pre-treatment assets), such that the Sharp RD treatment effect becomes:

$$\pi_{SRD} = E[Y_i(1) - Y_i(0) | X_i = \bar{x}, Z].$$

Weaknesses of Bronzini and Iachini (2014)

Bronzini and Iachini’s main estimation result is that “For the sample as a whole we find no significant increase in investment” because of the research subsidy (pg. 100). I too come to this empirical conclusion, but also see several weaknesses in Bronzini and Iachini’s research.

Global Polynomial Estimation Strategy

Bronzini and Iachini use a global polynomial estimation strategy, which “it is now widely recognized [that this global polynomial approach] does not deliver point estimators and inference procedures with good properties

for the main object of interest: the RD treatment effect.” (Catteneo et. al. 2017, pg. 47) As a result, I re-estimate the RD treatment effect using a low-order polynomial approximation near the cutoff, and examine how the order of the polynomial and kernel choice effect the results.

Regardless of the polynomial order ($p = 1, 2$, or 3), I estimate RD treatment effects (τ_{SRD}) that are small, between -0.024 and 0.072 . We fail to reject the null hypothesis that the RD treatment effect is zero for each estimate; all of the bias corrected confidence intervals contain 0. Similarly, changing the kernel (from triangular to Epanechnikov or uniform) does not affect this result. Table 2, below, reports Bronzini and Iachini’s estimation results and, in the final row, my estimate of τ_{SRD} using an MSE-optimal bandwidth and a third-order polynomial (which is significant in the paper).

Table 2: RDD Estimates (replication & MSE-optimal)

Bandwidth	Estimated RDD Effect
Full Sample	0.06400
Wide-Window	0.11000
Narrow-Window	-0.07900
MSE Optimal	0.07188

Arbitrary Bandwidth

The authors choose arbitrary local estimation bandwidths, covering 35% and 50% of observations. The MSE-optimal bandwidth for τ_{SRD} is 7.36 points on each side of the cutoff, or 46% of observations. It’s smaller than the “wide-window” (50%), and larger than their “narrow-window” (35%).

“Robust” Inference

Comparing estimates from my MSE-optimized bandwidth, local low-order polynomial estimation strategy and that deployed in the paper leads to another criticism: The authors rely on “robust standard errors clustered by score” to make inferences, which are not actually robust, but only calculated to be heteroscedasticity-consistent. Of course, this is a common way to make results seem more compelling than they are (i.e., do “, r” in Stata)—and is something that I’ve also done in the past. It’s misleading, and the inference is wrong.

Misrepresentation of the RD estimate

The authors refer to their RD estimate incorrectly: They write, “the average treatment effect of the program is assessed through the estimated value of the discontinuity at the threshold” (pg. 107). This is incorrect. The estimated value of the discontinuity at the score threshold, denoted above as τ_{SRD} , is “only the average effect of treatment for units LOCAL to the cutoff.” (Catteneo et. al. 2017)

The authors continue, writing: “If model (1) is correctly specified, the OLS estimate of the parameter $\hat{\beta}$ measures the value of the discontinuity of the function $Y(S)$ at the cutoff point, corresponding to the unbiased estimate of the causal effect of the program.” (pg. 108) But why should we think model (1) is correctly specified? This is probably a common refrain but doesn’t make sense—it’s a very strong assumption. Moreover, they proceed with inference after estimating coefficients as if their model is correctly specified (i.e., parametric).

Controlling for Covariate Imbalances

In the robustness section, the authors estimate an RDD conditional on covariates. In theory, controlling on an imbalanced covariate makes sense. But they re-do the main estimates adding in ALL pre-treatment

variables, and admit that this amounts to mindless p-value hunting by writing: “we introduce pre-treatment firm-observables in models (1) and (2) to increase the precision of our estimates.” (pg. 127)

Interplay between theory and empirical results

This paper is largely meant to show how a Sharp RDD can be used to get an estimate the effectiveness of research grants. The authors provide little explanation or theories why many firms are able to use these research grants for inframarginal investments. I would have liked to know more about how firms do this, to design a more effective policy program.

References

- Bronzini, Raffaello, and Eleonora Iachini. 2014. “Are Incentives for R&D Effective? Evidence from a Regression Discontinuity Approach.” *American Economic Journal: Economic Policy*, 6(4): 100-134.
- Cattaneo, Matias D, Idrobo, Nicolas, and Rocio Titiunik. 2017. “A Practical Introduction to Regression Discontinuity Designs.” Monograph, forthcoming in *Cambridge Elements*: 9-102.