Recommendation: Reject  
  
Comments:  
Dear authors,  
  
I read your paper carefully and think you have a good article. However, many issues are important to address because they are very fundamental I decided to reject the paper. I hope my comments can help you to improve the paper.  
  
I see as fundamental three points:  
  
1 - Scope of the paper: It seems that you do not have a focus in the paper when discussing funding usage and inequality across the schools. I suggest using the funding usage as auxiliary results (maybe an appendix) and focusing on the inequality across schools.  
  
2 - Methods: I believe that the differences-in-differences approach might not be the most compelling method to analyze your questions. If I understood correctly, everyone got some funding. However, some municipalities get more funding their others. In some sense, everyone is being treated simultaneously but at different intensities. Here, you should choose a different method or a different control group. If you stay on the DiD approach, I would suggest using a different control group (e.g., municipalities not in Ceara State or school performance of grades not used in the funding formula).  
  
3 - Results: It is fundamental to test the differences in the coefficients statistically when discussing whether they are different. You do not present a formal test (results from Table 3), and draw important conclusions.  
  
Some minor comments (but important to consider):  
  
A - Grammar: there are several passages with grammar errors. Please, check the text carefully.  
  
B - Data description: It was unclear how many years you used in the SPAECE. Please, review it.  
  
  
  
Additional Questions:  
Originality: Does the paper contain new and significant information adequate to justify publication?: The paper analyzes an important educational policy in the context of Ceara, Brazil. The policy they analyzed (quota-part) changed how the state transfers funding for municipalities. Before the policy, the state transferred funding to municipalities based on the population size. After the policy, the state created performance rules to transfer funding for municipalities, including performance in education. While the policy is relevant, several articles have already analyzed this policy (as cited by the authors in the introduction).  
  
  
The authors aim to present additional analysis looking at the changes in the expenditures of municipalities and inequality in education performance across schools (within municipalities). I do not see as significant the findings related to total expenditures. I cannot understand why a municipality that gets more funding would not expend more money. If the authors present these results, they should discuss why they are important. Also, it seems natural to use only part of the new resources in education. I cannot see why the authors expect larger increases in education expenditures. Altogether, I see as less important these first results.  
  
On the other hand, I believe the most significant information is related to education performance. According to the authors, the new rules can create incentives for municipalities to increase investments in better schools and eventually increase inequality. This is is an interesting and important question to ask.  
  
In sum, the authors have two questions. They seem very different from each other. The questions regarding the changes in expenditures seem auxiliary (maybe they could move it to an appendix). The second question seems more relevant.  
  
Relationship to Literature: Does the paper demonstrate an adequate understanding of the relevant literature in the field and cite an appropriate range of literature sources?  Is any significant work ignored?: The paper cited an appropriate range of literature sources.  
  
Methodology: Is the paper's argument built on an appropriate base of theory, concepts, or other ideas? Has the research or equivalent intellectual work on which the paper is based been well designed?  Are the methods employed appropriate?: In terms of data description, I think the authors could improve how they describe it, the sources they are using, and the period. Especially for the education performance results, I missed more details about the data.  
  
For the methods, I do not believe the methods are appropriate to the question they are asking. They use a DiD approach. Their treated group is the municipalities with a high education performance before the state introduced the funding law. The control group is the municipalities with a low education performance before the law. The DiD approach does not seem to fit the analysis because the treatment is continuous. I understood from their description that while a municipality that performs well in education will get more funding, a municipality with a poor performance still gets funding. Therefore, it seems everyone is treated, but the treatment intensity varies. On top of that, the treated groups differ in several dimensions. Even if the authors follow the DiD strategy, I would appreciate it if they could include a better discussion on how all the differences do not create problems for the parallel trends assumption. In sum, I suggest using an alternative approach.  
  
Results: Are results presented clearly and analysed appropriately?  Do the conclusions adequately tie together the other elements of the paper?: The authors present several different descriptive statistics and results of their models.  
  
Regarding descriptive statistics, I missed a better description of the distribution of education performance and the funding received across municipalities. These indicators are important to evaluate the DiD strategy.  
  
When presenting the results related to the total expenditures, it would be good to show a table with the main coefficients of DiD (showing the aggregated effects). Table 2 shows only the treatment coefficient (check the stars, it seems they were incorrectly dropped). Also, I did not understand the economic cycle fixed effects and how they are not colinear to the year and municipality fixed effects. Check it because you might report you are estimating both together, but your software (Stata, R, etc.) might be dropping some of these coefficients.  
  
The discussion about the changes in performance across low and high-performing schools is interesting. However, the authors could improve the way they report the results.  I have four suggestions here. First, even for the current results, the authors should test if the coefficients are statistically different and not only compare the point estimates. Looking at the point estimates and the standard errors, I do not believe we can affirm that high-performing schools increase more than low-performing schools. Second, I do not understand which years of school performance the authors used in the analysis. Municipalities could "game" the incentives and increase inequality across schools, especially after 2009.  Therefore, it would be very important to be more parsimonious when looking at the coefficients. Third, it is not clear why we should expect that high-performing municipalities (in terms of education outcomes) would try to manipulate more their results than the low performing municipalities. Their discussion makes it unclear why we should expect changes in some direction. Fourth, it would be good to look at other education indicators not included in the index to redistribute funding. For example, the formula uses results in the state test; could you not use the national exam (prova brasil / provinha brasil) to check whether results are improving in one test and not in the other?  
  
Practicality and/or Research implications:  Does the paper identify clearly any implications for practice and/or further research?  Are these implications consistent with the findings and conclusions of the paper?: The results related to inequality across schools are very interesting and have practical implications. The conclusions of the paper might be reviewed when the authors address the issues I pointed out when discussing the results. I would say the most important point is the comparison of coefficients across regressions in Table 3. I believe the conclusions will change if they test the differences of these coefficients statistically and find they cannot reject the null hypothesis.  
  
Quality of Communication: Does the paper clearly express its case, measured against the technical language of the field and the expected knowledge of the journal's readership?  Has attention been paid to the clarity of expression and readability, such as sentence structure, jargon use, acronyms, etc.: The paper has several sentences that are hard to read. Not because of technical language or jargon use, but because of grammar issues. I suggest the authors to pay attention on these issues.  
  
Abstract: Does the abstract relate to the article? Does it provide an adequate summary of the content? Is the language easy to understand and clear for the reader?: The abstract is clear.  
  
Associate Editor  
Comments to the Author: Reject.  
(There are no comments.)