

A Response to Arel-Bundock, Blais, and Dassonneville

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This version: September 2018

In their letter, Arel-Bundock, Blais and Dassonneville (2019, ABD hereafter) present an alternative model specification to test the reference point theory of economic voting proposed in Aytaç (2018).¹ ABD claim that the resulting evidence for the electoral importance of relative domestic performance (which they call historical benchmarking) is rather mixed, and that incumbents' relative international performance (cross-national, or international, benchmarking) does not seem consequential for election results. A correct reading of their analytical results, however, actually does provide robust evidence for the theory laid out in Aytaç (2018), and confirms reported results therein. ABD's erroneous conclusions are due to a neglect of evidence that they themselves report and a mischaracterization of the claims in Aytaç (2018).

Relevance of Relative Domestic Performance

ABD employ what they claim to be simpler specification (see Kayser and Peress 2019 for a dispute of this claim) to replicate the analyses in Aytaç (2018). Using this specification, they report strong evidence for the electoral relevance of incumbents' relative domestic performance. Model (1) in Table 1 below reproduces their analyses (Table 2 in ABD, column 6). We see that the variable of interest, G_h , which ABD denote as a country's historical level of growth, is negative and statistically significant ($p < 0.01$). Therefore, there is evidence that voters reward (punish) incumbents on whose watch the economy performs relatively better (worse) in domestic comparisons, just as reported in Aytaç (2018). Unlike the analyses in Aytaç (2018), ABD do not use election-year growth (G_y) together with growth during the incumbent's full term (G_t) in their specifications. When we add that to the model as well (Model 2 in Table 1), the results are robust – G_h is still negative and statistically significant. Moreover, once we account for relative performance, election-year growth (G_y) ceases to be a predictor for incumbent vote.

¹ ABD also present a critique of Kayser and Peress (2012). See Kayser and Peress (2019) for a response.

Table 1. The Impact of Relative Economic Performance on Incumbent Vote – Using Specification Proposed in Arel-Bundock, Blais and Dassonneville (2019).

DV: Incumbent vote	(1)	(2)
G_t	1.492*** (0.273)	1.292*** (0.287)
G_i	-0.410 (0.288)	-0.421 (0.280)
G_h	-0.572*** (0.202)	-0.574*** (0.204)
G_y		0.216 (0.143)
Vote share lag	0.673*** (0.063)	0.679*** (0.063)
Coalition	0.148 (1.023)	0.129 (1.015)
ENP	-1.569*** (0.385)	-1.536*** (0.381)
Presidential	-4.790*** (1.100)	-4.820*** (1.103)
Re-run	12.869*** (2.387)	12.661*** (2.393)
Constant	13.362*** (3.435)	13.040*** (3.405)
Observations	460	460

OLS estimates with robust standard errors clustered by country in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

At this point ABD resort to something very unusual: they ignore the replication results above using their own specification, and switch to a different model that is not espoused in Aytac (2018). The authors adopt a model where they only use election-year growth, i.e., G_t is ignored (Table 2 in ABD, columns 1–3). This model is theoretically problematic as there is no room for historical benchmarking anymore: the model implicitly assumes a comparison of election-year growth with the average growth during the previous incumbent’s term in office. The authors do not provide any theoretical argument to justify such a comparison. Basically, ABD ignore their own replication of Aytac (2018) and look for evidence for historical benchmarking with a specification where historical benchmarking does not make sense to start with. This alternative model they put and the theoretically-driven model in Aytac (2018) are simply incomparable.

Relevance of Relative International Performance

ABD's second major claim is that analyses with their alternative specification do not lend evidence for the electoral importance of incumbents' relative international performance, or cross-national benchmarking. This claim is first based on Model (1) of Table 1 above: The coefficient for G_i is in the correct direction (negative) but not statistically significant at conventional levels. Second, ABD assert that there is "no evidence that...education increase[s] the salience of comparative economic assessments" (p. XX, see also Figure 2 in ABD).

ABD is correct that in their baseline specification G_i does not reach conventional levels of statistical significance though it is quite close ($p = 0.14$). The crucial point, however, is that I do not expect cross-national benchmarking to be a universal phenomenon. The second hypothesis in Aytac (2018: 23) is clear: "The availability of information about international economic outcomes is a necessary condition for the electoral importance of relative international performance." I take the average level of schooling in a country as a proxy for the availability of information about international economic outcomes and find that relative international performance has a positive and statistically significant effect on incumbent vote *only when* voters have at least about eight years of schooling on average (See Figure 2 in Aytac 2018).

ABD's claim that there is no evidence that education increases the salience of relative international performance is based on an analysis where the authors employ an interaction specification of G_i with levels of education. Thus, they look for evidence of a *linear* relationship between presence of cross-national benchmarking and education levels – a claim that is not made by Aytac (2018). The hypothesis stated above does not expect relative international performance to become more salient as education levels increase; it states that relative international performance is electorally relevant only in countries with a sufficient availability of information about international economy (proxied by education levels). In all fairness, the analyses in Aytac (2018) also adopted a multiplicative interaction regression to investigate the proposed hypothesis because of a lack of a theoretically-driven a priori threshold. But the hypothesis does not call for a continuous, positive mediating effect of schooling on cross-national benchmarking. Therefore, ABD seek evidence for a claim that only they themselves put forward and report null results.

Given the nature of my hypothesis, a proper replication of the results in Aytac (2018) with ABD's specification would be the following: Divide the sample into two groups – one group consisting of countries with at least eight years of average schooling, and the rest in the other

group. This cut-off point is derived from the analyses in Aytac (2018), and the groups can be labeled as countries with highly- and lowly-educated populations, respectively. Aytac (2018) shows presence of cross-national benchmarking in the former group but not in the latter.

Table 2 below presents this analysis using ABD's own specification. Model (1) consists of all countries in the sample, Model (2) considers only countries with highly-educated populations, and Model (3) countries with lowly-educated populations. The predictions of Aytac (2018) are supported: there is evidence for both international *and* historical benchmarking in Model (2), that is, among countries with highly-educated populations – as expected by ABD, the coefficients of G_i and G_h are negative and statistically significant. In contrast, among the lowly-educated countries (Model 3) there is evidence for historical benchmarking only.

Table 2. The Electoral Relevance of Relative International Performance is Conditional – All Countries versus Countries with Highly- and Lowly-Educated Populations.

DV: Incumbent vote	(1) All Countries	(2) Countries with Highly-Educated Populations	(3) Countries with Lowly-Educated Populations
G_t	1.492*** (0.273)	1.651*** (0.467)	1.353*** (0.325)
G_i	-0.410 (0.288)	-0.561* (0.290)	-0.253 (0.566)
G_h	-0.572*** (0.202)	-0.494* (0.285)	-0.581* (0.292)
Vote share lag	0.673*** (0.063)	0.650*** (0.084)	0.676*** (0.088)
Coalition	0.148 (1.023)	0.068 (1.159)	0.012 (1.694)
ENP	-1.569*** (0.385)	-1.697*** (0.561)	-1.650*** (0.514)
Presidential	-4.790*** (1.100)	-4.908*** (1.529)	-4.305** (1.626)
Re-run	12.869*** (2.387)	8.321*** (2.830)	15.221*** (3.213)
Constant	13.362*** (3.435)	14.831*** (4.768)	12.978** (5.179)
Observations	460	242	218

OLS estimates with robust standard errors clustered by country in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

In short, ABD do find evidence for the presence of cross-national benchmarking among the group of countries specified by Aytacı (2018) – those with a sufficiently high levels of average schooling.

Conclusion

As highlighted by Kayser and Peress (2019), ABD propose a mathematically identical regression specification to revisit the analyses in Aytacı (2018). Thus, it should not come as a surprise that they reach identical empirical results as the original article, and thereby confirm the validity of the reference point theory of economic voting. Yet they neglect positive results in their own analyses and mischaracterize the arguments in Aytacı (2018), leading them to reach erroneous conclusions.

References:

Arel-Bundock, Vincent, André Blais, and Ruth Dassonneville. 2019. “Do Voters Benchmark Economic Performance?” Forthcoming, *British Journal of Political Science*.

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