

Returns to Investing in Sovereign Debt: a Response to Alvarez Nogal and Chamley

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Abstract

Alvarez-Nogal and Chamley (2015) analyze one debt contract signed by lenders to Philip II, previously discussed in Drelichman and Voth (2014). They re-examine cash flows and challenge our interpretation of this particular contract's profitability. A closer look reveals that the alleged differences between their and our calculations simply reflect the use of conservative assumptions on our part, which systematically biased estimates of profitability downwards – as good scholarship requires if one is to argue that high profits were one of the main reasons why people lent to Philip II. We also question their use and reading of archival documents, as well as their use of basic financial economics. Finally, we document a continuing pattern of academic misconduct, including plagiarism, the misrepresentation of our findings, and the complete fabrication of a quote in order to discredit our work.

I. Alvarez Nogal and Chamley's Critique

Why do investors continue to put their money into sovereign debt, despite a long history of defaults and payment stops? In a series of articles and a book, we have analyzed the famous case of Philip II. He defaulted four times during his reign, and allegedly ruined scores of bankers who had taken his word at face value.

In a detailed exercise, we examined the profitability of 435 short-term loan contracts. Our main conclusion was that bankers continued to lend to Philip II despite his defaults because it was profitable to do so. Lenders earned an average of 11.6% p.a. under the most conservative assumptions, almost 4.5% above the return on long-dated debt (Drelichman and Voth, henceforth DV, 2011, p. 8).

Carlos Alvarez Nogal and Christophe Chamley (henceforth AC) focus on one particular contract, signed by the Maluenda brothers in July 1595, shortly before Philip II's last default. They argue that we: i. underestimate the rate of return of the contract; ii. misread the planned structure of cash-flows iii. ignore material information found in the archives; iv. that because of i-iii, our reconstruction of Castile's fiscal position between 1566 and 1596 is invalid; and v. that it is impossible to obtain any estimate of profitability for loans made to Philip II (AC 2015). We refute these claims in turn. We also show that, continuing a pattern of academic misconduct that dates back several years, AC misrepresent our arguments and conclusions, set up a straw man in order to discredit our

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work, and attribute to us –in quotation marks– statements that we never made, in print or otherwise.

i. Underestimating profitability

The argument in DV (2011, 2014) is that bankers lent to Philip II because it was profitable to do so despite the defaults:

We calculate returns on lending to the Castilian crown, taking into account defaults and the bankers' cost of funds. Our calculations demonstrate that loans to Philip II were highly profitable. Defaults and reschedulings reduced the rate of return, but profitability net of these losses was still high—and markedly higher than the return on alternative investments. This was true on average and also applied to the vast majority of banking dynasties individually. As a consequence, few financiers ever stopped lending to Philip II. Profitable lending also explains why the number of exits from the stage after the 1575 default was no greater than before (DV 2014)

In the text, we explain at length the numerous assumptions that we made to *bias results against our hypothesis of finding high rates of return*. These include relatively pessimistic assumptions on haircuts, rescheduling, reinvestment rates, and the bankers' financing rate (DV 2014, p. 177). Even with these conservative assumptions, we concluded that out of 78 families in the contracts, only 5 might have lost money. The Maluenda overall were not part of the group with negative returns; we calculate their rate of return as 20.6% – assuming the settlements were applied in full, and the ex-ante contractual agreements were otherwise honored.

AC (2015) now argue that

Because they have misread the contract and do not refer to the attachments next to it, and the audits in the CMC, Drelichman and Voth are reduced to idle speculations. The archives flatly refute their conclusion that "the Maluenda brothers lost money on this particular contract."

If AC are correct – something that we dispute – our conclusions are not reduced to “idle speculations”. AC would have demonstrated that we did indeed pick very conservative assumptions, as we pointed out. They would have shown that a contract that according to our methodology may have lost money even made some, despite being caught up in a severe sovereign debt crisis. Nothing could more eloquently demonstrate both the cautious nature of our assumptions and the robustness of our conclusions.

ii. Misreading the nature of cash flows

AC argue that ‘DV see only "cash flows that swing from positive to negative multiple times" without discussing the reasons. This characterization misses the essential features of the structure of the contract.’ They complain in particular that our estimated disbursement and repayment dates are uniformly off by one month; and that we missed the “three-part structure” of the contract (AC 2015, p. 20).

Since AC helpfully transcribed most of the details of the contract in question, readers can readily verify that the “tranches” they accuse us of missing are simply three

disbursements that we capture, in the exact same amount and with identical timing, in our own analysis (DV 2014, p. 174 ff.). Whether they are called “tranches” or not is absolutely irrelevant (and they were most certainly not called as such at the time, since the language to describe the segmenting of financial instruments was still centuries in the future). Similarly, our analysis captures all promised repayments, and hence allows for the construction of complete agreed-upon cash flows. What AC call “the contract’s three-part structure” is also irrelevant. Money is fungible, and a gold coin does not suddenly acquire a different value because it is attached to a fictitious “tranche B” of a loan. The rate of return of a financial instrument depends only on its net cash flows, and cannot be affected by whether disbursement and repayments are labeled as tranches, treasure chests, or egg hunts.

The offset of one month, highlighted by AC, is a deliberate choice on our part, and not a mistake – as our book readily describes. The reason is that we felt that a month’s delay for disbursement etc. was reasonable given what we know about payment practices at the time. Needless to say, changing the date of every disbursement and repayment uniformly by one month has no implications whatsoever for calculating its profitability over the life of a contract.

iii. Disregarding material information in the archives

AC want their readers to believe that we cherry-picked our evidence and ignored the important information from audits conducted by the CMC (Contaduría Mayor de Cuentas). These were carried out in 1606, almost a decade after the death of Philip II. The Spanish monarchy had a habit of “shaking the tree” after major events, such as a default or the death of a ruler. Auditors routinely looked for technical violations, disallowed repayments, and generally sought to reduce the monarchy’s liabilities. The classic example of this type of behavior is the general audit that took place after the 1575 suspension of payments, when royal auditors disallowed all returns in excess of the (legal but typically not enforced) maximum interest of 12%, even when the king himself had signed off on payment schedules that implied much higher rates. In the end, the 1577 settlement that resolved the suspension of payments was a political, not a technical solution. The audit had merely been a power tactic in order to make bankers more amenable to a negotiated settlement. The audit of the Maluenda contract, which AC accept as an objective description of reality, was performed ten years after the original loan agreement, after a payment suspension and on the eve of another, and under a different ruler. Its figures are subject to at least as much uncertainty as the ex-ante agreed upon cash-flows, if not more.

Even if the 1606 audits on which AC rely were trustworthy, the purpose of our work is to understand the incentives that enticed bankers to lend to Philip II. Whatever happened in 1606, eight years after Philip II’s death, is irrelevant. Bankers were making decisions based on ex-ante expectations, not with perfect foresight of future realizations. Of course, these expectations would have been coloured by the performance of earlier loans –and our assumptions fully take that aggregate performance into account.

Finally, as AC describe, the total documentation for the contract in question runs to 354 pages; the actual contract only accounts for 47 of them, of which several are devoted to formulaic language. The majority of the additional documentation relates to the exercising of options granted in the original contract. While such activities obviously affected the ex-post return of the contract, they would not have altered its ex-ante value, which priced in the expected value of these options. Since our focus was on ex-ante pricing, and we had to deal with 435 contracts, we decided to focus on the main contracts themselves.

Just how difficult is the analysis that AC take us to task for not having performed? The Maluenda contract is one of the simpler and shorter ones in the archival record, but, for the sake of argument, let's assume that it is typical. AC devote a 30 page article to examining all the available documentation (and, as we discuss below, to come to the enlightening conclusion that "we just don't know"). Examining each of the 435 contracts in all their complexity would have required reading and coding tens of thousands of manuscript pages, making it logistically infeasible to produce estimates of overall profitability at all (our work is based on approximately 5,000 manuscript pages, which took almost four years to analyze fully). We look forward to AC coding up in detail the remaining 434 contracts that have escaped their attention so far, and to comparing our estimates with theirs; we are confident that not one of our main arguments will be materially affected by the results.

iv. Our reconstruction of Castile's fiscal position

AC argue that our reconstruction of Castile's fiscal position in DV 2010 is flawed because we used simplifying assumptions:

Their claim to have provided a "reconstruction of the year-to-year movements in Philip II's fiscal position" rests on speculations that would have been unnecessary if they had drawn on all the available documents. (AC 2015, p. 22).

Besides the innuendo implicit in calling our reconstruction a "claim", AC fail to grasp the fact that our only use of asiento cash-flows in determining Castile's fiscal position is to calculate interest on short-term debt. Short-term loans were never the dominant component of Castile's debt –in fact, they averaged about 10% of total liabilities. As a consequence of this, interest on short-term debt averages 14% of overall government expenditures over a 30-year period.⁴ That is, if the rates of return on short-term debt were, say, a completely implausible 50% higher than our estimates, this would only impact our estimates of the overall fiscal balance by about 7% of the total. Needless to say, when dealing with almost 500 year old fiscal accounting, such differences are well within the margin of error. Of course, AC are welcome to refine our calculations once they obtain rates of return for the remaining 434 asientos using the 35,000 pages of archival documentation they chastise us for having overlooked. We look forward to the added precision in the first or second decimal place.

⁴ Calculations based on the data from DV 2010, available at http://econ.sites.olt.ubc.ca/files/2013/05/excel_article_mauricio-drelichman-dataset-sustainable-debt.xls

v. Estimating rates of return

AC argue that:

Even if the rate of return on the balances of the asiento were properly computed, we cannot know whether the Maluenda brothers made or lost money. As mentioned repeatedly in the contract, the funds can be raised in the credit market. The contract provides no information on the equity of the Maluenda brothers, as for any other asiento in DV's database. The Maluendas may just have been underwriters for the instruments of credits, the letters of exchange, and the sales of the juros, earning all their income from fees. We just do not know. (AC 2015, p. 22)

Having raised a large number of questions about mostly irrelevant aspects of our reading of the Maluenda contract, AC now want us to believe that no meaningful calculation of rates of return or profitability is possible because the “Maluendas may just have been underwriters for instruments of credits, the letters of exchange, and the sales of the juros...”. The key argument is that the Maluenda brothers may have made money as underwriters rather than investors, refinancing their participation in the loan by selling parts of it to other investors.

This criticism is remarkable for its incoherence, for three reasons:

1) We already wrote at length about the way in which financiers like the Maluenda brothers refinanced themselves, including the possible revenues from underwriting fees. Both in our book (DV 2014) and in a book chapter (DV 2015a) we traced in detail the source and route of funds borrowed by Philip II, from individual investors in Genoa to international financiers in Madrid, passing through a range of intermediaries in between (DV 2014, pp. 194-206). We analyzed the fees charged at each stage and used them to provide bounds on the influence of intermediation on the profitability of loans (DV 2014, pp. 192-194). AC are, of course, intimately familiar with our analysis of financial intermediation, having plagiarized it at length in a recent manuscript (AC 2014b).⁵

2) Every calculation of historical debt profitability we know, from the classic studies by Eichengreen and Portes (1989a; 1989b) to the works of Sturzenegger and Zettelmayer (2006; 2008), uses exactly the same measure of profitability that we use –the gross returns to investors of sovereign debt contract payouts, not the net profitability of financiers. Since there is almost no information on the business side, the refinancing costs of intermediaries, or the opportunity costs of private investors, there is no other way to discuss profitability at all. We leave it to the

⁵ The original version of AC 2014b, which had been under consideration at a specialized journal, contained an entire section plagiarizing our analysis of financial intermediation. After we lodged a formal complain with the editors of the journal in question, AC withdrew the first version of the manuscript from circulation. They have now posted a second version that does not discuss financial intermediation. Both the first version of the manuscript containing the plagiarized section and our note documenting the plagiarism are available from us upon request.

reader to conclude whether the entire historical literature on sovereign debt profitability is meaningless because of AC's esoteric objection.

3) The conclusion "we just do not know" strikes us not as a triumphant summary of serious doubts about the possibility of performing accurate calculations; it is a simple capitulation before facts and figures to which a substantial degree of uncertainty is attached. With similar justice, one could argue that nobody should try to determine what the weather in Madrid was like in December 2014 based on a large sample of newspaper reports – occasionally, it rained, and then the sun shone. For some days, we wouldn't even know. True. Averages are always "wrong" –there are observations above and below. Estimates are estimates because not every detail is observed. Even GDP figures today are estimates. However, only an open disregard for the meaning of numbers and facts in economic history would lead to the conclusion that we can never come up with a range of plausible profit rates –that "we just do not know". While we will never know the profitability of each and every contract to the last digit, it is perfectly possible to produce estimates and confidence intervals of overall returns that allow us to provide economically meaningful answers to important questions.

In a separate passage, AC point out that, in one sentence of our 2011 article, we claim that we can know "with certainty" that the Maluenda brothers lost money on their particular contract (AC 2015, p. 21). We should arguably have said "with high likelihood"; even a cursory reading of the immediate context shows that we are referring to estimates, and of course did not claim to know precise values without a shadow of a doubt, as AC imply. In our 2014 book, we clarify our meaning by being more guarded in our wording.

II. Misrepresentation of argument and made-up quote

AC's central argument is that our calculation of the rate of return of the Maluenda contract is flawed because we neglect to consider the documents that supposedly document how the contract was implemented.

"The misreading of the contract has fatal consequences for DV's analysis of its implementation. [...] Their neglect of all these documents leads DV to a description of the implementation of the Maluenda asiento that has no relation with what actually happened." (AC 2015, pp. 20-21)

AC construct a straw man to attack our work. Nowhere in our articles or our book do we discuss the *implementation* of specific contracts. Our entire analysis focuses on the ex-ante incentives of financiers to lend to the king, and is hence based on ex-ante agreed-upon contractual clauses, not on ex-post implementation. We use conservative assumptions to explore the impact of individual renegotiations and global reschedulings on our estimates of aggregate profitability, but never claim that these assumptions can

reflect the definitive ex-post rates of returns on individual contracts.⁶ Our use of ex-ante cash flows is openly acknowledged and abundantly signposted throughout our work (eg. DV 2014, p.p. 173, 178, 180). Most of AC's attacks are therefore aimed at a methodology that we never actually employ.

On page 22 of their paper, AC write:

Because some of the contracts allow for significant flexibility, the monitoring documents are obviously essential. DV have preferred to make their own assumptions on events, about which they are "absolutely sure."

By placing the expression "absolutely sure" in quotation marks, AC imply that we wrote those exact words when referring to our assumptions. Nowhere in our entire body of work do we use the expression "absolutely sure", nor do we refer to our assumptions with any other expression to similar effect. Our assumptions are clearly signposted throughout our writing and, as all assumptions, they are obviously subject to uncertainty. AC fabricate a quote, attributing to us wording we never used to discredit our work.

III. Conclusion: a note on intellectual dishonesty and academic misconduct

In a 2014 article, AC duplicated several of our earlier ideas and results without attribution, misrepresented our arguments at length, and attributed several of our findings to other scholars (AC 2014a). In our response, now forthcoming in the *Economic History Review*, we amply document these behaviours (DV 2015b).⁷ In a separate manuscript, AC plagiarized our analysis of financial intermediation, while continuing to claim several of our results as their own (AC 2014b). That manuscript was withdrawn from circulation after we submitted a complaint to the journal that was considering it for publication.

Clearly, up to late 2014, AC judged our analysis and results to be of high enough quality to claim them as their own without attribution. In a recent reversal of opinion, which coincidentally took place after having been twice caught engaging in this type of misconduct, they now claim that our work is based on a partial reading of archival sources and on dubious calculations, and is therefore invalid. In support of their argument they offer an anachronistic reading of archival documents, a deficient understanding of financial economics, straw men, plenty of innuendo, and even a fabricated quote. Students face severe disciplinary consequences for misconduct of this kind. It should under no circumstances be tolerated from established scholars.

⁶ Such a "final value" would have to take into account the ex-post implementation of every single contract with an individual lender, as new contracts often included clauses to compensate the lender for earlier breaches of the letter of an *asiento*. Needless to say, this would then imply that until a lender stopped doing business with Philip II, we cannot calculate a precise figures for profitability – even in the presence of detailed information on ex-post cash flows.

⁷ Pre-press version available at http://econ.sites.olt.ubc.ca/files/2015/09/pdf_drelichman_duplication_without_constraints.pdf

Should we reconsider any of our conclusions about the debts and defaults of Philip II in the light of AC's criticisms? Even if their analysis of rates of return were correct (which we dispute), it would only strengthen our conclusion — lending to Philip II was highly profitable, and even contracts caught up in bankruptcies did not suffer large losses. If they took their own arguments seriously, AC would have to conclude that our results are even more robust than our published work claims.

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