Replicating Sachs and Warner's Working Papers on the Resource Curse

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January 2013

"This is an Accepted Manuscript of an article published in *Journal of Development Studies* on 17 July, 2013, available online:
http://dx.doi.org/10.1080/00220388.2013.807501

Abstract

This paper reports on my attempt to replicate Sachs and Warner's 1995 and 1997 resource curse working papers. The 1995 paper is not replicable for lack of a data archive. Pure replication of the 1997 paper is achieved. Statistical replication determines that the proposed institutional causes of the resource curse are not robust to country sample. Scientific replication shows that findings of a resource curse are not sensitive to different measures of resource intensiveness, though they are sensitive to estimation technique. Typographical errors in the published paper reveal the value of researchers making both their data *and* code available.

Keywords: replication, Sachs and Warner, resource curse, decline effect.

JEL Codes: O13, Q32, Q33, B40, C80

Replicating Sachs and Warner's Working Papers on the Resource Curse

Introduction

Replication is an essential part of the scientific method. Hammermesh (2007) defines three types of replication in economics: pure replication, 'to make or do something again in exactly the same way' (a verification exercise); statistical replication, 'different sample, but the identical model and underlying population' (a type of validation); and scientific replication, 'different sample, different population, and perhaps similar but not identical model' (an extension). This paper reports attempts to replicate the 1995 and 1997 Sachs and Warner working papers that were the first to empirically establish a 'resource curse.'

All available evidence indicates that most published empirical economic research is not purely replicable. The main reason is the failure of authors to make their data and code publically available. Yet even the majority of economic research that provides publically available data and code cannot be reproduced. In investigations of two distinct samples of empirical papers published in the *Journal of Money, Credit and Banking* and for which data archives existed, only 22 per cent (Dewald et al., 1986) and 23 per cent (McCullough et al., 2006) could be purely replicated. A similar analysis of papers published in the Federal Reserve Bank of St. Louis *Review* yielded an 8 per cent rate of pure replicability (McCullough et al., 2008).[1] Glandon (2011) is unable to purely replicate any of a sample of nine papers with data and code published in the *American Economic Review*, though five were replicated 'almost exactly.'

Even where it is purely replicable, economics research on economic growth is notorious for being sensitive to country sample, meaning that it is often not statistically replicable

(Temple, 1998; Easterly, 2005; Knabb, 2005; Norman, 2009). In the context of the resource curse literature, which relies almost exclusively on growth diagnostics, results have indeed been found to be ‘sensitive to changing the sample period, the sample of countries, or the definition of various explanatory variables’ (van der Ploeg, 2011: 381) The results also appear to be highly sensitive to empirical specification, with recent reworking of the data finding that there may be no curse at all, and even a resource blessing.

Table 1 about here

In this light, and given the rising recognition that such a ‘decline effect’ is often the result of bias or errors in the early research (Lehrer 2010), how are we to think about the classic but unreplicated Sachs and Warner (1995, 1997a) working papers first finding evidence of a resource curse? The papers have never been published, and yet together have been cited 2,654 times, more than any of Sachs and Warner’s published papers on the resource curse (Table 1).[2] To put this level of citation in perspective, Feder (1983) is the most cited paper published in the *Journal of Development Economics*, the top journal in the field, with 1,811 cites (Google Scholar). In the physical sciences, such path-breaking and highly cited work would have been replicated dozens of times by now. In the social sciences, and in economics in particular, replication of results is rare; there is a lack of demand for replication in economics, such that undertakings on the supply side carry risks that are not compensated with the reward of publication (Hammermesh, 2007; McCullough et al., 2008). As McCullough et al. (2006: 1093) muse, ‘If Pons and Fleischman had published their cold fusion results in an economics journal, the world would still be awaiting lower utility bills.’

Given the practical limitations to replicating all published economics work, there must be a rational strategy for the expenditure of labour on such tasks. One strategy is to select those works that have had greatest influence in their field, are still relevant, have not been fully replicated, and for which replication is likely to be an issue. The Sachs and Warner working papers fit these criteria. First, they have been incredibly influential. Second, worries about the resource curse continue to invoke policy incentives that dissuade the production of primary products in favour of manufacturing. One regularly reads in the popular media that nascent primary resource exporters like Brazil, Mongolia, Mozambique and Afghanistan have grave concern about their development prospects. Sweeping reviews of the resource curse literature are produced regularly (e.g., Stevens, 2003; Davis and Tilton, 2005; Frankel, 2010; van der Ploeg, 2011). Third, even though there have been many extensions of the Sachs and Warner papers, the papers have not been purely or statistically replicated. Schonger (2002), Mehlum (2006), and Davis (2011) purely replicate some of the 1997 paper's results, but Stijns' (2005) reports an inability to purely replicate these same results. This discrepancy has yet to be resolved. As a final motivating factor, we have evidence of a decline effect combined with a report that some of the results in the contemporaneous Sachs and Warner (1997c) paper on the resource curse in Africa, which uses almost the same model and data as the 1997 working paper, cannot be replicated (Davis, 2012). This makes replication of the original Sachs and Warner working papers of more than passing interest.

A Review of the 1995 and 1997 Sachs and Warner Resource Curse Studies

In 1995, Sachs and Warner (hereafter SW) produced their first paper examining primary resources' role in economic growth (Sachs and Warner 1995).[3] Their purpose was to investigate what they variously call ‘a conceptual puzzle,’ ‘a surprising feature of economic life,’ and an ‘oddity:’ namely, the negative association identified by previous researchers between the intensity of a country’s natural resource (agriculture, mining, and fuels) production and subsequent economic growth. The paper examines the impact of resource intensity in 1971 against per capita economic growth from 1970 to 1989. Resource intensiveness is measured as the 1971 share of agricultural, mining, and fuel exports in GDP (variable SXP; see the Appendix for a full glossary of variable abbreviations). They measure economic growth as the average annual change in real GDP per capita from 1970 to 1989 (variable G7089) and regress G7089 on SXP using ordinary least squares (OLS) linear regression, controlling for convergence effects via the log of the 1970 real GDP per capita (variable LGDP70). The regression shows that, conditional on initial income levels, those economies with higher levels of initial resource exports grew more slowly from 1970 to 1989. This is the resource curse.

A second working paper completed two years later, SW (1997a), updates the growth period to 1990, changes the measure of economic growth to the average annual change in real GDP per economically active population (variable GEA7090), and changes the measure of resource intensiveness to the 1970 share of agricultural, mining, and fuel exports in GNP (again variable SXP). These changes have little impact on the results. The resource curse is maintained as additional conditioning variables are added in a series of twelve additional OLS regressions in Tables I, III, IV, V, VI, and VII.

SW (1997a, Tables VIII, X, and XI) produce three additional tables of regressions exploring indirect routes through which primary exports may be causing the slower economic growth. The results support their hypothesis, presented in detail in the 1995 paper, that a shrinking manufacturing sector is to blame. These regressions also test for the impact of institutions on growth. They show that growth is impacted by good institutions and that institutional quality is a function of resource intensity. SW do not make much of this result, however, as they conclude that the indirect effects of resources on institutions and of institutions on growth are small compared to the direct effect of resources on growth.

Are the Results in the 1995 and 1997 Papers Purely Replicable?

Despite an intensive search I could find no data archive for the 1995 paper, and so it joins the many other economics papers that are not replicable. For the 1997 paper SW provide a readme file and four data files: a STATA (version 5) Do file that they apparently use to do the econometric analysis; a STATA data set; and Excel and HTML files with the data. The five files are currently available at http://www.cid.harvard.edu/ciddata/ciddata.html. There is no description of the codes used for the variables in the data file, though many of these can be found in an Appendix in the 1997 paper. I used EViews 5.1 with an Intel Pentium M 1.86 GHz processor to conduct the replication using the data presented in the downloadable Excel file.

As SW note in footnote 13 of their paper, they first check their sample for outliers. They regress GEA7090 on LGDPEA70, SXP, and SOPEN (a measure of trade openness), which is regression 1.2 in Table I, and determine outliers based on the DFITS statistic computed in STATA. The DFITS results are included as a column in the data files. While the paper states that an observation is excluded if DFITS > $2\sqrt{k/n}$ where k is the number of regressors (inclusive of the constant) and n is the sample size, the STATA code actually tests for $|\text{DFITS}| > 2\sqrt{k/n}$ since DFITS can be negative. There is complete data for GEA7090, LGDPEA70, SXP, and SOPEN for 91 of the 211 countries in the sample, and so $k = 4$ and $n = 91$. On the basis of this test they exclude Chad, Gabon, Guyana, and Malaysia as outliers. I am able to replicate their list of outliers by computing the DFITS statistic using STATA version 11.2.[4]

There are 95 countries in the SW data set with complete growth and resource export data. The relationship between growth and resource exports is depicted in SW's Figure 1. SW claim that the figure shows the relationship between growth and resource exports for 95 developing countries (p. 2). The figure in fact includes both developing and developed countries.

Table 2 about here

Table I in SW presents five different specifications testing for the resource curse. The specifications are reproduced in Table 2 above. In each of these regressions they draw from the 87 countries for which complete data exist for regressions 1.2 and 1.3. I list the 87 countries and the data for each country in the online Appendix. SW do not include in their paper this list of countries, though I discovered that the data file codes the included countries as excl1 = 0.00 and the excluded countries as excl1 = 1.00. One would not be able to come up with the correct country sample for regression 1.1, for which there is complete data for 94 countries, if one did not read the STATA file to know which additional four countries to exclude from that regression.[5]

Given that we now know the country sample regression 1.1 can be replicated except for two differences in the second decimal place of the reported t-statistic values in regression 1.1.[6] Regression 1.2 was also successfully replicated.

An initial problem with replicating regression 1.3 had to do with the reported regressor INV7089 (the investment to GDP ratio averaged over the period 1970-1989). The raw data file contained both INV7089 and LINV7089, the latter being the natural log of the former. The regression table in the SW paper reports that INV7089 was used in regression 1.3. The SW STATA Do file shows that the correct regressor is LINV7089. With LINV7089 as the independent variable, regression 1.3 is also exactly replicated.[7] This is what tripped up Stijns (2005) in his replication attempt – I can replicate regressions SW3 through SW5 in Stijns by using INV7089 in SW regressions 1.3 through 1.5.

The country size drops to 71 in regressions 1.4 and 1.5 because there are 16 countries for which no rule of law (RL) data is available (see the online Appendix). I am also able to replicate regressions 1.4 and 1.5. There is an error in the title of Table I in SW: it should be 'Partial Associations Between Growth (1970-90) and Natural Resource Intensity (1970),' not 'Partial Associations Between Growth (1970-90) and Natural Resource Intensity (1971).'[8]

SW then go on to note in a footnote to their Table I that if the four outliers are not excluded the estimated coefficients on SXP 'range from -6.0 to -8.5, with t-ratios always exceeding 4 in absolute value.' I have verified that this is the case, though since Chad does not have rule of law (RL) data it is technically not added back to regressions 1.4 and 1.5 in this exercise.

The remaining 27 regressions in the SW paper revert to the full population of 211 countries, including the four outliers. This is not made clear in the paper, but is clear in the STATA file. The sample size for each regression is now simply the number of countries for which the data for the set of regressors was complete.

The next table of regression results is Table III, which tests whether the resource curse finding in Table I is dependent on the measure of resource intensity. Table III introduces three alternative resource intensity measures, SNR (share of mineral production in GNP in 1971), PXI70 (ratio of primary exports to total merchandise exports in 1970), and LAND (natural logarithm of the ratio of total land area to population in 1971), and sequentially replaces SXP with these in regression 1.5. The resource curse remains regardless of the measure of resource intensity. I was able to replicate the regressions in Table III exactly save for the t-statistic on LAND in regression 3.4. It should be -4.08, not -3.78. From the STATA file the dependent variable in these regressions is actually GEA7090, not GEA7089 as reported by SW in the table. The SNR variable is reported in the table notes as being mineral production divided by GNP in 1970. The paper's text and variable descriptions state that the data is from 1971. Davis (2011) confirms that the data is from 1971.

Given that Table III finds that the measure of resource intensiveness is immaterial, SW stay with their preferred measure, SXP, in the remaining regressions. Table IV adds more conditioning variables. The dependent variable in this table is actually GEA7090, not

GEA7089 as reported by SW in the table. The table notes refer to variable INV7090, and yet the regressor is reported as INV7089 in both the table and the STATA file. The data files only contain data for INV7089, and so this is the correct regressor. Note that the investment variable is not logged in this table.

Tables V, VI, and VII add still different control variables to check for the robustness of the resource curse. I managed to exactly replicate the regressions in these tables. In Table V the independent variables are actually KLLSEC and KLLLY70, not LSEC and LLY70 as listed by SW in the table. In Tables VI and VII the dependent variable is actually GEA7090, not GEA7089 as reported by SW in the tables.

Table VIII regresses sectoral data against SXP and additional conditioning variables looking for a causal mechanism for the resource curse. I had some trouble replicating regression 8.2 in Table VIII until I realized that the independent variable is LGDPNR70 (natural log of GNP produced in sectors other than the natural resource sector) per the table notes on p. 32 and the STATA file, not LGDPEA70 as reported by SW in the table.

Table IX tests to see whether the resource curse differed between the 1970s and 1980s. In regression 9.2 variable SXP80 is used in the regression, not SXP as reported by SW in the regression and in the table note. With this change I replicated regressions 9.1 and 9.2. Note that the column headings should be Growth 1970 – 1980 and Growth 1980 – 1990, not Growth 1970 – 1979 and Growth 1980 – 1989 as reported by SW in the table.

In footnote 17 SW mention that policy in earlier periods may be driving the resource curse results seen in the 1970s and 1980s. They state that they test for this by controlling for growth in the 1960s, but that ‘growth in the 1960's does not enter the regression significantly, and does not alter the significance of the SXP coefficient’ (p. 20). It is not clear from the footnote text which regression they test. The note comes in the same paragraph as discussions of regressions 9.1 and 9.2, which test for a changing resource curse effect between the 1970s and the 1980s. The STATA file makes it clear that they in fact returned to regression 1.5, additionally controlling for GR6070 (average annual real growth per capita from 1960 to 1970).[9] I confirm that that GR6070 is statistically insignificant in regression 1.5 and that the significance of the SXP coefficient only goes down from -6.89 to -6.51 when GR6070 is included as an independent variable. However, if I add GR6070 to regression 9.1, which tests for the resource curse in only the 1970s, SXP becomes statistically insignificant, suggesting that there may be something to their theory, at least for the resource curse of the 1970s. Adding GR6070 to regression 9.2, which tests for the resource curse in the 1980s, does not cause SXP80 to become statistically insignificant. GR6070 is, however, statistically significant in that case. Table 3 below provides the comparison of regressions 9.1 and 9.2 with and without GR6070 as a conditioning variable.

Table 3 about here

Table X investigates the relationship between natural resource intensity and other economic variables. Table XI investigates the relationship between natural resource intensity and the quality of institutions. I replicated all of the regressions in these tables.

The conclusion of this section is that SW’s results can be exactly purely replicated once the countries included in the regressions are determined and the errors in the paper’s reported regressors are corrected. Both adjustments required information from the STATA file. The replication revealed three inconsequential differences in the reported t-statistics (two in regression 1.1 and one in regression 3.4). There is no doubt that the SW data allows them to measure a resource curse in the1970s and 1980s that is robust to various sets of conditioning variables. The only question mark given their model and econometric method is the robustness of the resource curse in the 1970s, since conditioning on growth in the previous decade (a proxy for policy in the previous decade) causes the coefficient on SXP in that regression to become insignificant. This is an important finding, as it indicates that the slow growth in that decade may be a result of legacy policies rather than a result of extensive primary production.

The replication attempt took longer than it should have because of the many inconsistencies between the reported regressors in the paper and the actual regressors in the STATA file. Without the STATA file the replication may have been impossible.

Statistical Replication of the 1997 Paper

Statistical replication involves testing robustness to sample and sample period using exactly the same model and underlying population. Many researchers have tested for the robustness of a resource curse over different samples and sample periods, but without the benefit of using the same regressors and estimation technique as in SW. This is arguably not statistical replication. Here I test whether SW’s growth regression results are sensitive to country sample and sample period given their regressors and estimation technique.

SW do not maintain a consistent country sample across the various regressions. For example, SW regressions 3.1 through 3.4 test for whether the resource curse is sensitive to the specification of primary production by replacing SXP in regression 1.5 with other measures of resource intensiveness. However, three outliers (Gabon, Guyana, and Malaysia) omitted from the sample in regression 1.5 are added back in the Table III regressions, increasing the sample size to 74.[10] Given the noted sample sensitivity of these types of regressions, the inconsistency of country selection across regressions is a troubling aspect of SW's analysis. For the record, regressions 1.1 through 1.5 sample from the set of 87 countries listed in the online Appendix, while the regressions in Table III onwards sample from the full 211 countries in the data base and use the set of countries for which complete data for that regression is available. There are only 22 countries that are common across all 32 regressions.

SW may feel comfortable changing the sample across their regressions due to some preliminary testing that they do in this regard confirming that the oil economies in the sample are not driving the results. In further examining sample effects, I can confirm that the resource curse associated with the alternative indicators of resource intensiveness listed in Table III is not changed when the three outlier countries are excluded. I can also confirm that it is not Singapore and Trinidad & Tobago that are driving the resource curse result, as suggested by Lederman and Maloney (2007b); the results in Tables I and III are robust to the removal of those two countries.

The country subsamples used in the resource curse tests in Tables I and III only include three of the top ten resource-intensive economies (as measured by SXP) in the data set. Resource intensive countries such as Iraq, Oman, Saudi Arabia, Botswana, Niger, and Zaire have no growth data and so are not included. These countries' omission is not inconsequential. SW suggest that Botswana is an example of a natural resource-abundant economy that grew rapidly, and that its exclusion may be biasing the results towards a resource curse. Indeed, when Sala-i-Martin et al. (2004) find that the fraction of mining (and oil and gas) in GDP in 1988 has a *positive* effect on overall economic growth from 1960-1996, contrary to the received wisdom of the resource curse, they posit that this may be because of the inclusion of the 'outlier' Botswana. Botswana's growth was astounding over this period, and it certainly could be considered an outlier. But with its low resource intensity in 1970 (SXP = 0.05) it was more like Hong Kong (SXP = 0.03) and Singapore (SXP = 0.03) than a resource-abundant economy. Given the model set-up and the definition of resource intensiveness Botswana is an example of a non-resource abundant economy that grew rapidly in the 1970s and 1980s, *supporting* the resource curse. Somalia, Tanzania, Barbados, Haiti, and Myanmar, some of the poorer performing developing economies in the non-resource intensive category, also lack growth data, and so are excluded. Adding these countries to the sample may weaken the resource curse.

To see whether the findings of a resource curse in Table I are influenced by the omissions of these resource-intensive countries I expand the data sample for Table I's regressions by adding back the four outliers and filling in the missing growth data for Oman, Saudi Arabia, Botswana, Niger, Zaire, Somalia, Tanzania, Barbados, Haiti, and Myanmar using GEA7089 data rather than GEA7090, taken from the same data source as GEA7090. I also add back the four selectively omitted countries from regression 1.1 (see endnote 5). The five regressions in Table I, which now have a sample size of 105, 101, 101, 79, and 79, still show a strongly statistically significant resource curse, supporting Davis's (2011) proposition that Sala-i-Martin et al. (2004) find a resource blessing not because of the inclusion of Botswana in their sample, but because they measure resource intensity near the end of the growth period and are picking up reversion in production levels.

I also use the SW 91-country subsample listed in the online Appendix to test the regressions in Tables VIII, X, and XI, which explore indirect routes through which primary exports may be causing the slower growth found in Table 1 using this same subsample. This seems more appropriate than the approach SW take, which is to include all countries which have data for a given regression, since one is interested in the indirect routes operating in the country subsample which was used to identify the resource curse in the first place. It turns out that delimiting the investigation to the 91-country subsample matters in seven of the regressions. First, the SXP coefficient in regression 8.1, which now has an *n* of 80 rather than 89, turns from statistically significant to statistically insignificant due to the removal of nine countries from the sample. This weakens SW's conjectures about the resource curse operating through a decline in manufacturing and lost external economics of scale. Second, the SXP^2 coefficient in regression 10.4, which drops from a sample size of 104 to 91, turns from statistically significant to statistically insignificant. This creates a lack of support for SW's claim that there is a U-shaped relationship between resource intensity and trade openness and that the heavily resource-intensive economies are less protectionist than the less resource-intensive economies. The SXP coefficient turns from significant to insignificant in each of regressions 11.1 through 11.5 due to the elimination of 10 or more countries from the sample, depending on the regression (Regressions 11.1 and 11.2 now only have 55 countries in the sample, down from 65, and regressions 11.3 through 11.5 now have 74 countries, down from 85). These results weaken SW's proposition that higher resource intensity is related to poorer institutional quality, and that this is part of the reason for slower growth. That the measured impact of resource extraction on institutional quality is dependent on country subsample is consistent with Norman's (2009) observations for a similar data set.

The second part of statistical replication is sample period. SW test whether sample period matters by restricting their regressions to 1970 - 1980 and 1980 - 1990 (Table IX), and find that the resource curse holds in both periods. Mehlum et al. (2006) use the data in Sachs and Warner (1997c) to test SW regression 1.2 (see Table 2 above) for a growth period from 1965 to 1990 for the same 87 countries as in the original SW regression. They find the resource abundance coefficient is still negative and statistically significant. Lederman and Maloney (2007b) find a resource curse from 1980 to 1999 when using the SW model, though they also change the independent variable to real growth per capita. When I extend the growth period in the SW Table I regressions to 2010, which is the latest data available, I find that the coefficient on SXP is negative and statistically significant at the 5 per cent level in all but regression 1.2.[11]

The results of this section indicate that SW's finding of the resource curse is surprisingly robust to different country samples and for the most part to different sample periods. They are not reporting outlying results. On the other hand, their evidence that the resource curse is a result of resource production's impact on institutional quality and manufacturing output is not robust to country sample.

Scientific Replication of the 1997 Paper

The majority of research that extends SW resource curse analysis proposes different control variates, different samples, and different estimation techniques. As I noted in the Introduction, much of the recent work has brought doubt upon the existence of slow growth in resource-rich countries (e.g., Lederman and Maloney, 2007a; Alexeev and Conrad, 2009; van der Ploeg and Poelhekke, 2010; Smith, 2012).[12] Other work suggests that growth in resource rich countries may be slower in the short term but not in the long term due to a temporarily lagging resource sector (Schonger, 2002; Davis, 2011; James and James, 2011). Mehlum et al. (2006) develop a model that shows that one must interact resource dependence with institutional quality. Once this is done, resources could be a blessing or a curse, depending on institutional quality. In all of the scientific replications of the resource curse, only Schonger (2002), Mehlum et al. (2006) and Davis (2011) begin with a pure replication of the relevant SW (1997a) results. Given my success at purely and statistically replicating all of the SW results, we now know that the different outcomes in these scientific replications are truly a result of differing empirical models and specifications, rather than differences in sample.

Another area of investigation in the scientific replications of SW is whether the SW results are sensitive to the measure of resource dependence (e.g., Lederman and Maloney, 2007b; van der Ploeg and Poelhekke, 2010). Since these efforts do not begin with the same data and empirical specification as in SW, it is difficult to know whether the results are exposing sensitivity to only the measure of resource dependence. Here I make use of my ability to purely replicate the SW results to test this question for SW's country sample and sample period. The flow variable that SW use to define initial primary production intensity, SXP, has been suggested to pick up trade patterns not necessarily related to endowments (Lederman and Maloney, 2007b). The measure may also be endogenous, with high values the result of, rather than a cause of, underdevelopment (Mehlum et al., 2006; Alexeev and Condrad, 2009). Resource production is thus a preferred indicator of resource abundance. Minerals and energy have been particularly targeted as being responsible for the resource curse (Butkiewicz and Yanikkaya, 2010), and are more likely to be the target of resource-grabbing institutions (Mehlum et al., 2006). Given this, I first replace SXP with the share of mineral production in GNP (SNR) in each of the nine SW resource curse regressions that contained GEA7090 as the dependent variable and SXP as an independent variable. The difference between the statistical significance of the two variables in each regression is minimal under both the original sample of 91 countries and under the full country sample with growth data for 10 countries added.[13] This is also the case when the resource intensity variable PXI70 (ratio of primary exports to total exports in 1970) replaces SXP, with the exception that PXI70 is statistically insignificant in regression 5.2 when using the 91 country sample. This latter measure is less likely to have endogeneity problems, but it may be a proxy for export concentration rather than resource abundance (Lederman and Maloney, 2007b).

I also test the five resource curse regressions in SW Table I using initial mineral and energy production per worker (M71/EAPOP71), as suggested by Alexeev and Conrad (2009). The data are taken from Davis (2011). In these regressions I follow Alexeev and Conrad's suggestion of controlling for initial level of per capita income using fitted values so as not to contaminate the initial condition with the boom of the resource sector. The fitted per capita income values are computed using Alexeev and Conrad's equation (3). In each regression, regardless of whether or not I control for initial income, the coefficient on per worker mineral and energy production is negative and statistically significant at the 5 per cent level.[14]

Some resource curse researchers argue that economies should be characterized by their initial resource stocks rather than their initial resource flows. When I replace the flow measure SXP with the stock measure LAND in each of the nine SW growth regressions that contain SXP as an independent variable, LAND remains negative and statistically significant.[15] The fact that a high initial level of LAND promotes a resource curse is contrary to the findings by Ding and Field (2005) and van der Ploeg and Poelhekke (2010), who use a different model and different empirical estimation technique to show that large resource endowments do not result in a lower level of economic growth.

My replication analysis makes it clear that the SW analysis is robust to the measure of resource intensity. The scientific replications that find otherwise appear to be confounding new measures of resource intensity with new empirical specifications and analytic techniques. This finding is one of the benefits of replication: ‘Only through replication of the results of others can scientists unify the disparate findings of various researchers in a discipline into a defensible, consistent, coherent body of knowledge’ (Dewald et al., 1986: 600).

Conclusions

Overall, my pure and statistical replication of the 1997 Sachs and Warner working paper confirms their finding that countries with intensive primary resource sectors as of 1970 subsequently grew more slowly than equivalent economies that did not have intensive primary resource sectors. The result is invariant to how initial primary resource intensity is measured, to country sample, to how and whether one controls for initial income, and to sample period. This positive replication outcome should be of some comfort to the thousands who have cited Sachs and Warner’s results in support of a resource curse and to the many more who have been influenced by them in terms of development policy. It also warrants the myriad of research that the original findings spawned, as the Sachs and Warner work did indeed find a conundrum that needed to be solved.

Pure and statistical replication does not test the validity of the econometric specification or model that Sachs and Warner use; it only confirms that their reported results are consistent with the data that they provide. Their regressions may have endogeneity problems (Alexeev and Conrad, 2009), bias associated with cross-country estimation of dynamic growth effects (Lederman and Maloney, 2007b; Manzano and Rigobón, 2007; Smith, 2012), or bias as a result of dropping countries for which there are missing values (Norman, 2009).[16] Correcting these empirical problems causes the resource curse to weaken or even disappear. van der Ploeg and Poelhekke (2010) find that there is no evidence for a resource curse in cross-country analysis once certain variables are instrumented and additional determinants of economic growth are added to the model. My replication of the original Sachs and Warner work allows us to conclude that these empirical and modelling extensions provide meaningful scientific replication failures as opposed to a decline effect arising from exaggerated or erroneous claims in the original research.

As a final note, given the low replicability equilibrium of economics research it is surprising that pure replication was possible. Success would have been impossible had Sachs and Warner not publically posted their data and an appropriately commented STATA file for the 1997 paper. This reinforces the calls in the replication literature for authors to make available their data *and* code (Anderson et al., 2008).

[*]I thank Bruce McCullough for the encouragement to write up and communicate my initial efforts on this replication, which I completed in 2004, and for his comments on an earlier draft of the paper. I also thank Laura Camfield and Richard Palmer-Jones for taking interest in replication studies and for inviting me to participate in their panel on replication in development studies at the EADI/DSA General Conference 2011 in York, UK. Two anonymous referees and the editor provided valuable suggestions that led to this version of the paper.

[1] The lack of ability to replicate does not necessarily mean that a paper is wrong; there may be technical details in data storage or the presentation of results that represent innocent errors in the original research. See Dewald et al. (1986) and the subsequent exchange between Merritt (1988) and Dewald et al. (1988) for an example.

[2] The papers are nearly perfect substitutes, and so one would either cite the 1995 paper or the 1997 paper, but not both. Hence, there is no double counting here. Portions of the 1997a paper are reproduced in Meier and Rauch (2000: 161-167).

[3] The 1995 NBER paper was previously produced as HIID Development Discussion Paper No. 517a, October 1995.

[4] I thank Arturo Vazquez Cordano and Michael Heeley for assisting me with this portion of the replication attempt.

[5] Cape Verdi Islands, Iceland, Fiji, and Panama are excluded from Regression 1.1 so as to make a consistent 87 country sample across the first three regressions.

[6] The t-statistic on initial productivity (LGDPEA70) should be -0.54 rather than 0.55. The t-statistic on primary exports in GNP in 1970 (SXP) should be -4.74 rather than -4.75.

[7] Eleven years ago I and graduate students Jean-Philippe Stijns at University of California, Berkeley and Martin Schonger at Bonn shared our frustrations at not being able to replicate Table I. It was Martin who eventually realized that the independent variable in Table I should be LINV7089, not INV7089 (Schonger, 2002).

[8] The notes to the data state that SXP is for 1970, taken from a 1995 World Bank data diskette. I have verified that the diskette only contains 1970 data for fuel and non-fuel exports. It does not contain 1971 data.

[9] The STATA file incorrectly refers to footnote 18 instead of footnote 17. Footnote 18 is a discussion about Botswana.

[10] The fourth outlier, Chad, does not have rule of law data, and so is not part of the sample here. There is no PXI70 data for Zimbabwe in regression 3.3, resulting in a sample size of 73 in that regression.

[11] I compute the share of economically active population in 1970 (sea70) from the raw SW data files, and then compute the growth in GDP per economically active population from 1970 to 2010 using the PWT 7.1 RGDPCH series and sea10 data from World Development Indicators http://data.worldbank.org/indicator/SP.POP.1564.TO.ZS. I also replace the original LGDPEA70 conditioning variable with LGDPEA70NEW computed from the PWT 7.1 data.

[12] Brunnschweiler and Bulte (2008) also find against the resource curse, though their work has since been shown to be empirically flawed (van der Ploeg and Poelhekke, 2010).

[13] Davis (2011) notes that he was unable to replicate the SW SNR data series from the primary sources listed by SW, and so regressions that include this variable may be unreliable.

[14] For the regressions that omit the initial income variable the sample size is now 113, 107, 107, 78, and 78 across regressions 1.1 through 1.5. For regressions that include the initial income variable the sample size is 112, 107, 107, 78, and 78.

[15] This holds for both the 91 country sample and the augmented sample. Wood and Berge (1997) and Owens and Wood (1997) find that land per worker is a reasonable proxy for mineral stock endowments.

[16] Listwise deletion will yield biased coefficient estimates when the missing data is correlated with the level of the dependent variable, in this case economic growth. Such a correlation seems likely for this type of analysis: in the 91 country sample in SW, the 17 countries that are missing rule of law (RL) data and therefore excluded from all regressions containing that covariate have an average growth rate of 0.46 per cent, while the 74 that have RL data have an average growth rate of 1.34 per cent.

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Appendix: Alphabetic list of Sachs and Warner regression variables mentioned in this paper

DTT7080 Average annual growth of the natural logarithm of the external terms of trade between 1970 and 1980.

DTT8090 Average annual growth of the natural logarithm of the external terms of trade between 1980 and 1990.

DTT7090 Average annual growth of the natural logarithm of the external terms of trade between 1970 and 1990.

G7089 Average annual growth of purchasing power adjusted GDP per person between years 1970 and 1989.

GEA7080 Average annual growth of purchasing power adjusted GDP per person aged 15-64 (economically active population) between the years 1970 and 1980.

GEA8090 Average annual growth of purchasing power adjusted GDP per person aged 15-64 (economically active population) between the years 1970 and 1980.

GEA7089 Average annual growth of purchasing power adjusted GDP per person aged 15-64 (economically active population) between the years 1970 and 1989.

GEA7090 Average annual growth of purchasing power adjusted GDP per person aged 15-64 (economically active population) between the years 1970 and 1990.

GR6070 Average annual real per-capita growth between 1960 and 1970.

LAND Natural logarithm of the ratio of total land area to population in 1971.

LGDP70 Natural logarithm of real GDP per capita in 1970.

LGDPEA70 Natural logarithm of real GDP per person aged 15-64 in 1970.

LGDPEA80 Natural logarithm of real GDP per person aged 15-64 in 1980.

LGDPNR70 Natural logarithm of GNP produced in sectors other than the natural resource sector in 1970.

LINV7079 Natural logarithm of the ratio of real gross domestic investment (public plus private) to real GDP, averaged over the period 1970-1979.

LINV8089 Natural logarithm of the ratio of real gross domestic investment (public plus private) to real GDP, averaged over the period 1980-1989.

LINV7089 Natural logarithm of the ratio of real gross domestic investment (public plus private) to real GDP, averaged over the period 1970-1989.

M71/EAPOP71 Mineral and energy sales per economically active population in 1971 based on 1971 USD prices.

PXI70 Ratio of primary exports to total merchandise exports in 1970.

RL Index for rule of law ranging from 0 (low) to 6 (high) measured as of 1982.

sea70 The share of total population in 1970 aged 15 – 64.

SOPEN The fraction of years during 1970-1990 in which Sachs and Warner rate an economy as open.

SOPEN7 The fraction of years during 1970-1980 in which Sachs and Warner rate an economy as open.

SOPEN8 The fraction of years during 1980-1990 in which Sachs and Warner rate an economy as open.

SNR Share of mineral production in GNP in 1971.

SXP Share of primary products exports in GDP in 1971 (Sachs and Warner, 1995) or in GNP in 1970 (Sachs and Warner, 1997a).

SXP80 Share of primary products exports in GNP in 1980.

Table 1: Citations and Citation Rate for the Seven Sachs and Warner Natural Resource Curse Papers

Paper	Place of Publication	Citations to date	Average annual citation rate
Sachs and Warner 1995, 1997a	Unpublished*	2,654	147
Sachs and Warner 1997b	AER	681	43
Sachs and Warner 1997c	JAE	962	57
Sachs and Warner 1999a	JDE	523	38
Sachs and Warner 1999b	EE Book Chapter	2	0
Sachs and Warner 2001	EER	1,506	108

Notes: Based on Publish or Perish software (Harzing 2007), v 3.8.1.4675, which scrapes Google Scholar, search conducted on November 1, 2012. AER: *American Economic Review*; JAE: *Journal of African Economies*; JDE: *Journal of Development Economics*; EE: Edward Elgar; EER: *European Economic Review.*

*Google Scholar combines cites for the 1995 paper with the 1997a paper, and lists these as the 1995 paper.

Table 2: Regressions in Table I of Sachs and Warner (1997a) Testing for the Resource Curse

Regression	Specification	*n*
1.1	$GEA7090 = \alpha_0 + \alpha_1 LGDPEA70 + \alpha_2 SXP + \varepsilon$	87
1.2	$GEA7090 = \alpha_0 + \alpha_1 LGDPEA70 + \alpha_2 SXP + \alpha_3 SOPEN + \varepsilon$	87
1.3	$GEA7090 = \alpha_0 + \alpha_1 LGDPEA70 + \alpha_2 SXP + \alpha_3 SOPEN + \alpha_4 INV7089 + \varepsilon$	87
1.4	$GEA7090 = \alpha_0 + \alpha_1 LGDPEA70 + \alpha_2 SXP + \alpha_3 SOPEN + \alpha_4 INV7089 + \alpha_5 RL + \varepsilon$	71
1.5	$GEA7090 = \alpha_0 + \alpha_1 LGDPEA70 + \alpha_2 SXP + \alpha_3 SOPEN + \alpha_4 INV7089 + \alpha_5 RL + \alpha_6 DTT7090 + \varepsilon$	71

Table 3: Sachs and Warner Regressions 9.1 and 9.2. Dependent variable: Average annual growth in real GDP per economically active population, 1970 – 1980 (GEA7080) in 9.1 and 1980 – 1990 (GEA8090) in 9.2.

	(9.1)	(9.1a)	(9.2)	(9.2a)
Constant	8.21 (3.65)	7.91 (3.34)	13.05 (4.70)	14.05 (5.16)
Initial productivity (LGDPEA70)	-1.25 (-4.07)	-1.23 (-3.81)	-1.88 (-5.00)	-1.99 (-5.47)
Primary exports in GNP (SXP, SXP80)*	-3.89 (-2.43)	-2.64 (-1.36)	-6.15 (-3.42)	-5.59 (-3.21)
Openness (SOPEN7, SOPEN8)*	1.82 (3.10)	1.89 (3.05)	2.51 (4.14)	2.11 (3.50)
Growth in Terms of Trade (DTT7080, DTT8090)*	0.11 (3.23)	0.11 (3.20)	0.02 (0.17)	0.04 (0.43)
Ln of Investment Ratio (LINV7079, LINV8089)*	1.51 (4.15)	1.52 (3.60)	0.63 (1.25)	0.39 (0.78)
Rule of Law Index (RL)			0.55 (3.34)	0.58 (3.60)
Growth in 1960s (GR6070)		-0.02 (-0.18)		0.23 (2.02)
Adjusted R^2	0.31	0.28	0.60	0.61
Sample Size	101	96	73	71
Standard error	2.04	2.08	1.52	1.46

Notes: *SXP, SOPEN, DTT, and LINV are all synchronized with the dependent variable, and so regression 9.1 uses the data for the 1970s and regression 9.2 uses the data for the 1980s. RL is for the 1980s, and so is not used in the 1970s regression. t-statistics in parentheses.