Join to prosper?

An empirical analysis of EU membership and economic growth

Andersen, Thomas Barnebeck; Barslund, Mikkel; Vanhuysse, Pieter

Published in:
Kyklos

DOI:
10.1111/kykl.12200

Publication date:
2019

Document version:
Accepted manuscript

Citation for pulished version (APA):
Andersen, T. B., Barslund, M., & Vanhuysse, P. (2019). Join to prosper? An empirical analysis of EU membership and economic growth. *Kyklos*, *72*(2), 211-238. https://doi.org/10.1111/kykl.12200

Join to Prosper? An Empirical Analysis of EU Membership and Economic Growth*

Thomas Barnebeck Andersen** Mikkel Barslund† Pieter Vanhuysse‡

Friday, 24 January 2020

Summary: We explore whether EU membership has led to faster economic growth. Using different estimation strategies, different time periods, different country samples, and different datasets, we are unable to demonstrate the presence of a membership growth premium. This may reflect that GDP data are too noisy and/or causal identification too complicated, in which case we should remain agnostic about the EU's growth impact. Alternatively, it may reflect that EU membership simply has no effect on economic growth. Either way, these findings urge strong caution about one of EU membership's key alleged benefits: greater economic prosperity.

JEL classification codes: F43, F45, O4, P2

Keywords: EU membership, single market, economic growth

---

* A previous version of this paper was circulated under the title "Join to Prosper?".

** Department of Business and Economics, University of Southern Denmark, Campusvej 55, DK-5230 Odense M, Denmark, e-mail: barnebeck@sam.sdu.dk;

† Centre for European Policy Studies, 1 Place du Congres, 1000 Brussels, Belgium, e-mail: mikkel.barslund@ceps.eu

‡ Department of Political Science and Public Management, University of Southern Denmark, Campusvej 55, DK-5230 Odense M, Denmark, e-mail: vanhuysse@sam.sdu.dk

I. Introduction

In his 2005 magnum opus, the distinguished historian Tony Judt (2005, p. 32) concluded that "taken all in all, the EU is a good thing." In 2013, the equally distinguished historian Niall Ferguson called the European Union an "ignominious failure".[1] Others would object that reaching a verdict on the EU is a fool's errand: the historically unprecedented EU project is simply too complex and seeks to deliver on too long a list of outcomes. Nevertheless, a principled assessment of the European experiment in terms of well-defined outcomes is *a priori* reasonable. Such an assessment has, if anything, acquired greater relevance in light of Britain's 2016 decision to leave the EU and the sixtieth anniversary of the foundational Treaty of Rome in 2017. It also resonates with the wide-ranging agenda of evidence-based policy making, which is marked by a shift in focus from inputs or procedures (i.e., how the EU does things) to an evaluation of outputs or outcomes (i.e., what the EU delivers).[2]

So how should we evaluate the accomplishments of the EU? Staab (2013, p. 4) notes that the EU "has, to date, been a highly successful attempt at integration." But integration is only a means to an end: it is an example of how the EU does things. So, integration *per se* is of limited independent interest.[3] As Giddens (2014, p. 5) notes, the EU is a functionalist enterprise, driven by results. We need instead to identify what the EU promised to deliver in terms of measurable outcomes.

The great European post-war statesmen, including the EU founding fathers, clearly saw the European project as a way of taming the passions by serving the interests; an age-old argument in favour of market exchange (Hirschman 1977). That is, they envisaged the establishment of a common political and economic entity as a guarantor of both international peace and domestic economic progress. In his famous Zürich speech of 19 September 1946, Winston Churchill argued that "the sovereign remedy" to the plight of post-war Europe was "to recreate the European family, or as much of it as we can, and to provide it with a structure under which it can dwell in peace, in safety, and in freedom. We must build a kind of United States of Europe."[4] Four years later, on 9 May 1950, the epochal Schuman Declaration in turn stated that the pooling of coal and steel production had the double economic and political aim of "contributing to raising living standards and to promoting peaceful achievements." Creating such a "fusion of economic interests," Robert Schuman argued, "may be the leaven

[1] http://www.voxeurop.eu/en/content/article/3757341-european-project-total-failure

[2] Simply put, an evaluation gauges the changes in the well-being of subjects that can be attributed to a particular project, program, or policy. The fundamental challenge in carrying out good evaluations is therefore to identify the causal relationship between the project, program, or policy and the outcomes of interest (Gertler et al. 2010).

[3] In his chapter on the single market, Staab (2013) ponders whether the single market program has really delivered a 'single' market. However, while a single market may deliver increased welfare, the single market is (arguably) not inherently desirable. See however footnote 9 for a subtler view.

[4] See http://www.churchill-society-london.org.uk/astonish.html

from which may grow a wider and deeper community between countries long opposed to one another by sanguinary divisions."[5] In 1957, Article 2 of the Treaty of Rome explicitly talked about "raising the standard of living."

Half a century later, in its award to the EU of the Nobel Peace Prize for contributing "to the advancement of peace and reconciliation, democracy and human rights in Europe," the Nobel Committee noted that the EU's *single* most important result over six decades is "the successful struggle for peace and reconciliation and for democracy and human rights. The stabilizing part played by the EU has helped to transform most of Europe from a continent of war to a continent of peace."[6] The official webpage of the European Commission itself also proclaims that the "EU has delivered half a century of peace, stability and prosperity".[7] In recent decades, the EU's activities have expanded most in policy areas quite remote from the original mission of establishing a free internal market with common external trade policies (Alesina et al. 2005). At the same time, the EU itself mainly emphasizes growth when proclaiming general objectives, as is evident from its most ambitious recent policy agendas. In 2000, a stated aim of the Lisbon Agenda was to make the European economy the "most competitive and knowledge-based economy in the world, capable of sustainable economic growth with more and better jobs and greater social cohesion."[8] And all seven of the Flagship Initiatives adopted as part of the Europe 2020 Strategy were about growth—smart, sustainable, and inclusive.

Consequently, it should not be controversial to identify peace and prosperity as two crucial dimensions on which EU has set out to deliver.[9] In his wide-ranging account of Europe, Giddens (2014, p. 12) unambiguously states that "the EU is there to promote common economic prosperity." Niall Ferguson in turn focuses on both peace and prosperity. Rejecting the first factor, he argues that European integration has had absolutely nothing to do with peace in Europe since World War 2; that has been the achievement of NATO. If the EU was about war and peace, there would have been a European Defence Community, which was vetoed by the French National Assembly in 1954.[10] And Ferguson emphasizes that as European integration has proceeded, EU growth has

---

[5] http://europa.eu/about-eu/basic-information/symbols/europe-day/schuman-declaration/index_en.htm

[6] See http://www.nobelprize.org/nobel_prizes/peace/laureates/2012/eu-facts.html.

[7] http://europa.eu/about-eu/basic-information/about/index_en.htm

[8] http://www.europarl.europa.eu/summits/lis1_en.htm

[9] For a skeptical view on these two EU *finalitées* and others (such as mutual control, cultural diversity, and accountable government), see Offe (2015, Ch. 6) and Giddens (2014). Other (more political) dimensions of the European project are surely important as well. For the French, the EU has traditionally been viewed as a foreign policy lever. The Germans, on the other hand, saw it as a way to become a 'normal' country. Elites of smaller countries, like the Benelux, have surely seen the EU as a way to obtain perks and posts.

[10] http://www.voxeurop.eu/en/content/article/3757341-european-project-total-failure. This is also the view found in Judt (2005, p. 302). The European project had stalled by the mid-1950s; the push for a fresh start came from Belgium's Paul-Henri Spaak, who had realized that military and political integration were—at the time—*a cul-de-sac*, for which reason emphasis should be put on European economic integration. Interestingly, Ludwig Erhard—enthusiastic free-trader and Economics Minister under Konrad Adenauer—was critical of a neo-mercantilist European customs union, which in his view would distort prices and reduce German trade with external countries. A customs union was, according to Erhard, "macro-economic nonsense" (Judt 2005, p. 304).

declined. Moreover, since 1980 the EU has grown faster than the United States in only nine out of 32 years. Thus Ferguson's verdict: the EU is a failure. Judt (2005) also weighs in on peace and prosperity. In terms of prosperity, he simply claims that the "economic benefits of the single market have been real" (p. 732). With respect to peace, he concedes that the lesson that war is too high a price to pay for political or territorial advantage had been brought home to the victors after World War 1; it took World War 2 to also bring it home to the losers.[11] Nevertheless, the elites and institutions of the EU are now so intertwined and interdependent that armed conflict is all but inconceivable. Moreover, it would be difficult to deny that many of the Eastern European countries, which joined the EU in 2004 (cf. Supplementary Appendix Table A.1), saw the EU "as an escape route out of their past and an insurance policy for the future" (p. 734).[12] Thus Judt's verdict: the EU is a good thing.

Whatever one's thoughts about the peace issue, it is not well-suited for an empirical evaluation. There is no credible way of separating the influence of the EU from that of NATO (or from widespread conflict weariness, for that matter). One could perhaps focus on the main proximate link in the peace argument, which holds that trade reduces conflict.[13] However, while multilateral trade reduces the risk of a global war, it does not necessarily reduce the risk of bilateral conflicts. Because openness to multilateral trade decreases bilateral dependence vis-à-vis any given country, it may thus (depending on the international governance system) decrease the cost of a bilateral conflict (Martin et al. 2008). At the same time, if economic integration is a powerful force for lasting peace it is somewhat puzzling that there are some many within-country conflicts.[14] Add to this that the secular forces associated with modernity have steadily worked to diminish violence on a global scale (Pinker 2011). Consequently, the focus of the present paper will be on prosperity as the key outcome that the EU will be measured up against.[15] Even if one considers the peace issue to be central, an assessment of EU's growth record should still be of independent interest.

---

[11] A very similar view is found in Mazower (1999), who argues that the shock of being subjected to a regime of unprecedented violence in the few years between 1938 and 1945 instigated a sea change in Europeans' political and social attitudes; in fact, it made them rediscover the virtues of democracy. Mazower also argues that the post-war focus on full employment and state planning for the social good (as opposed to the inequities of prewar laissez-faire) provided one of the preconditions for post-war political stability.

[12] Political stabilization also played a role when the EU was enlarged with Greece and, a few years later, with Portugal and Spain (Staab 2013). In the motivation for the Nobel Peace Prize, the Nobel Committee also emphasizes the EU's benign role for political stability. The Committee furthermore believes that the question of EU membership bolsters the reconciliation process after the Balkan wars as well as the promotion of democracy in Turkey. See http://www.nobelprize.org/nobel_prizes/peace/laureates/2012/eu-facts.html.

[13] Judt's (2010) argument does not build on the foundation that war is less likely among countries that trade, an insight frequently associated with Emmanuel Kant's 1795 *Essays on Perpetual Peace*. However, a leading policy economist such as Larry Summers does not seem to question that trade leads to peace. See "Global trade should be remade from the bottom up," *Financial Times*, 11 April 2016.

[14] We thank the Editor for pointing this out.

[15] This is also the dimension that Ferguson identifies as the most important and the one which Judt (2010) and the European Commission both assert that the EU has delivered. Note, however, that the whole notion of pan-European economic cooperation goes back at least to the interwar years (Mazower 1999). The 1925 Locarno treaties and the 1927 Genova conference were perceived by some as a move towards "an economic league of Nations [sic] whose long-term goal…is the creation of a United States of Europe" (p. 111).

There is a small empirical literature which considers the growth effects of EU *membership*.[16] Landau (1995), for example, considers a sample of 17 OECD countries during the period 1950-1990. The estimating equation is a growth regression which includes e.g. GDP per capita, government debt to GDP, proxy for human capital, and a terms of trade measure. The independent variable of interest is a dummy variable taking the value 1 if the country is a member of the EU, zero else.[17] Landau does not find that the EU dummy is significantly correlated with economic growth, which leads him to conclude that "there has not been a statistically significant difference between the growth of EEC and non-EEC developed market economies" (p. 781). Henrekson et al. (1997) estimate a growth regression with right-hand side variables such as schooling, investment to GDP, and a trade policy proxy. The variable of interest is a dummy which takes the value 1 if the country is a member of either EU *or EFTA*, zero else. Using a sample of 115 countries over the period 1976-1985, they find that the EU/EFTA is correlated with economic growth at a five percent level of significance in their baseline specification. They conclude that "regional integration in Europe not only affects static efficiency but can also have […] significant growth effects" (p. 1555). Vanhoudt (1999) analyses the time series properties of weighted EU real GDP per capita growth from 1950 to 1990. He concludes that there is no unit root in the weighted EU economic growth (thus no scale effects), and he also shows that EU economic growth fell as the EU expanded from having six to having ten member countries. Such a fall in economic growth was not observed for the US economy over the said period. Vanhoudt also estimates a growth regression specification on a panel of 23 OECD countries over the 1950-90 period. Employing an EU membership dummy, he finds no differences in growth performance between EU and non-EU OECD countries. Vanhoudt thus concludes that there are "no growth effects associated with EU membership" (p. 214). Using synthetic control methods, Campos et al. (2014) construct a counterfactual (or synthetic) EU from a pool of 11 non-EU OECD and 24 middle-income countries. Comparing EU and synthetic counterfactuals, Campos et al. conclude that there is "strong evidence of positive pay-offs from EU membership" (p. 25).[18]

The chairman of the Genova conference envisaged this as the "sole economic formula which can fight effectively against the United States of America" (p. 113). Moreover, the idea of European economic integration with the creation of tariff-free zone was proposed by German Reich Minister of Economics (Reichswirtschaftsminister) Walther Funk in the summer of 1940, and idea which Goering seemed to approve (ibid).

[16] According to Campos et al. (2014, p. 2), the literature is "disappointingly small". Our brief review of the literature only considers the literature that analyses EU *membership*. The reason, which will be explained more fully in Section III.A, is that a more proximate measure such as trade integration leads to fundamental problems of causal interpretation.

[17] An interaction between the dummy variable and a time trend is also included in the specification.

[18] Campos et al. (2014) assert that they present "improved estimates". However, synthetic control methods—as the name suggests—are of course no panacea. There are non-trivial choices and assumptions involved. In any case, robust (causal) findings should be method neutral in the sense that they hold across different (reasonable) identification strategies (Angrist and Pischke 2009; Imbens 2014). As shown in the present paper, the finding that EU has a large impact on prosperity is not method free in this sense. In fact, we

The present paper departs from the existing literature in several ways. First, our analysis is informed by the potential outcomes literature. This requires that we must be able to specify a well-defined counterfactual with respect to our treatment variable. It also means that we do not add a long list of confounders to our empirical specifications. Second, we slice and dice the data in various ways as part of several distinct identification strategies. Third, we use larger datasets (more countries, longer time horizons) than existing studies. Finally, we deal explicitly with problems of statistical inference resulting from spatial and temporal dependencies. The conclusion that emerges upon looking comprehensively and systematically at the data is that we cannot reject the null hypothesis that EU membership has zero impact on economic growth. This may be the result of some combination of noisy data and the fact that causal identification is just too complicated in a non-experimental setting, or, alternatively, it may simply reflect that EU membership has no effect on prosperity. In any case, it suggests that an agnostic stance with respect to EU's growth contribution is justified.

Needless to mention, the EU provides many other *direct* benefits (or costs, depending on one's perspective) to the citizens of Europe. It may well be true that the scale, the multi-level policy interlocking, and the sheer bureaucratic opacity of some of the EU processes obscure these benefits (Benz and Stutzer 2004). But it also seems certain that, for example, the right to study, work, travel, and live in any EU country of one's choosing is a benefit that many Europeans value highly. And the EU has also contributed to, among many other things, consumer protection, workplace safety, regional convergence, and constitutional rights protection. By focusing exclusively on economic growth, we only consider the *indirect* 'growth value' of any of these many likely direct EU benefits. This is an obvious limitation of the present research.[19] But overall, we trust that an agnostic stance with respect to the growth contribution of EU membership is both justified empirically and non-trivial in terms of its political economy implications.

address the Campos et al. study in some detail in the Supplementary Appendix. Specifically, we show that the said study's "large positive effects" are not present when we employ nearest-neighbor matching techniques in the context of the same set of observables that they work with.

[19] On a more general level, we are, in effect, adopting an outcome-based ethical framework (consequentialism), see Wight (2015) and Hausman et al. (2017). Looking at the other benefits—right to work, study, travel, etc.—would not change this. Alternative ethical frameworks, however, could be brought to bear. A duty- and rules-based framework could argue that for historical reasons, European integration is simply a moral obligation. This has most certainly been a driving force for older generations of Europeans. A virtue-based ethical framework in turn could argue that "the EU is a normative power" (see Manners 2008). A key requirement here would be that the EU lives by "virtuous example", which means that it is coherent and consistent. Coherence entails that the EU does not just promote its own norms, but that the normative principles that constitute it and its external actions are part of a universalized strategy for the common good. Consistency in turn entails absence of hypocrisy in the sense that the EU must follow the norms it promotes (Manners 2008). It should be clear that the consequentialist framework is the only ethical framework that lends itself to rigorous empirical testing (see, however, Alesina et al. 2005 for an attempted quantitative evaluation of a non-consequentialist framework).

The paper is structured as follows. Section II offers some data plots, which provide the first indication that EU membership is no universal growth remedy. Section III offers a more systematic empirical analysis. Section IV offers concluding remarks.

II. Some Simple Plots

Turning to the data, consider first the comparative performance of the EU and the United States since 1980, which is the comparison that Niall Ferguson makes. The IMF's World Economic Outlook Database provides real GDP growth rates going back to 1980 for the EU (size weighted) and the US. These are plotted in Figure 1. The EU only managed to outperform the US economy in terms of real GDP growth in ten out of the 35 years between 1980 and 2015. The (arithmetic) average annual growth rate in real GDP was 0.026 and 0.019 in the United States and the EU, respectively. With these growth rates, the US economy would double its size every 27 years, whereas the corresponding number for the EU is 36 years. This hardly amounts to stellar performance on part of the EU. It is more or less on this basis (and the identification of NATO as the main cause of peace in Europe) that Niall Ferguson reaches his negative verdict on the EU.

[FIGURE 1 ABOUT HERE]

Two objections immediately present themselves. First, it is not obvious that we should size weight observations;[20] in the cross-country growth literature, weighting is practically never used. Second, it is not obvious the United States constitutes an appropriate comparison group. Consequently, we have (for the sample of 21 original OECD countries, cf. footnote 24 below) plotted unweighted real GDP *per capita* growth since 1961 for respectively the EU and the non-EU members in Figure 2. Inspection of the figure leads to the impression that these two groups of countries have performed at a comparable level. Indeed, average annual growth was 0.0227 in the EU group versus 0.0235 in the non-EU group.

[FIGURE 2 ABOUT HERE]

[20] In the IMF's World Economic Outlook, group aggregate series are obtained using GDP valued at purchasing power parities as a share of country group GDP size as weights.

Figure 2 admits another interesting comparison. The EU was enlarged by ten countries in 2004, two countries in 2007, and one country in 2013, cf. Appendix Table A.1. As we are considering the original OECD-21 sample, this enlargement represents an approximately *exogenous* expansion of the European single market for the EU group of OECD-21 countries.[21] If the single market is a powerful engine of economic growth, we would probably expect post-2004 economic growth to have been higher in the EU OECD-21 countries vis-à-vis the non-EU dittos. Inspection of Figure 2 reveals that this is not borne out by the data. Add to this that (unweighted) real GDP per capita growth in the EU is negatively correlated (corr. coef. $= -0.36$) with EU membership count, cf. Figure 3. This constitutes a sobering and transparent piece of evidence to suggest that EU membership is no pre-packaged growth recipe.

[FIGURE 3 ABOUT HERE]

Consequently, the brief discussion in this section cautions against upbeat expectations in terms of what EU membership has delivered with respect to economic growth. The next section will entertain a more systematic empirical analysis.

III. Empirical Analysis

A. Model Specification and Data

We will use real GDP per capita (or per worker) growth as our main dependent variable, as real GDP per capita growth is a better measure of changes in welfare than are changes in the size of the overall economy (Lequiller and Blades 2014). The US population, for example, is structurally more dynamic (e.g., it rises faster) than that of Europe, say, which means that US real GDP grows faster, all else equal.[22]

We will use EU membership as the independent variable of interest. A more proximate measure such as trade integration leads to problems of causal interpretation, as it does not provide a clear and well-defined counterfactual. In the language of the potential outcomes literature, there is *non-manipulability* of treatment (see Gerring 2012; Morgan and Winship 2015). Trade integration comes with EU membership; but so do many other

[21] To be sure, three non-EU OECD-21 countries may have gained from the 2004 enlargement, due to EEA (Iceland and Norway) and bilateral trade deals (Switzerland); yet four countries (Canada, Japan, Turkey and the United States) have not.

[22] Focusing on real GDP growth, as done in Figure 1, therefore puts the European economy at a disadvantage.

policy-induced changes, which may impact growth. This means that a counterfactual scenario of EU membership without trade integration makes little empirical sense, and thus (according to a strict philosophical judgement) trade integration (not being manipulable) is not causal in nature.[23] EU membership, on the other hand, is amenable to manipulation, for which reason it has a causal interpretation. According to this line of reasoning, it is preferable to use the deep variable 'EU membership' as the treatment effect of interest.

Consider then the following very simple regression equation:

$$(1) \quad \Delta \log(y_{it}) = \mu_i + \delta_t + \rho EU_{it} + \varepsilon_{it}.$$

In equation (1), $\Delta \log(y_{it})$ is the annual (continuously compounded) growth rate of real GDP per capita (per worker) of country i in year t; μ_i is a country fixed effect; δ_t is a time fixed effect; and EU_{it} is a binary indicator taking the value 1 if country i is an EU member in year t, zero otherwise. In equation (1), ρ therefore tells us the average annual growth contribution of EU provided that $E(EU_{it}|\varepsilon_{it}) = 0$.

We start by estimating equation (1) on a global sample of more than 200 countries. This approach bears a resemblance to the well-known difference-in-differences estimator, where the group of countries that never entered the EU serves as an untreated control group for the countries that did enter. To be sure, it is not obvious that the group of all non-EU countries constitutes an appropriate control group for retrieving an estimate of EU's growth contribution. For instance, poorer countries may reap advantages of backwardness (Abramovitz 1986) and/or conditional convergence may be operative (Barro and Sala-i-Martin 2004). Put differently, at any given time countries that join the EU are richer than the 'average' world country, which likely invalidates $E(EU_{it}|\varepsilon_{it}) = 0$. We deal with this issue in two ways.

First, we estimate equation (1) on the sample of "original" OECD countries (henceforth OECD-21), which joined the OECD in the early 1960s.[24] Among the OECD-21, seven countries never joined the EU; these seven countries will therefore serve as control group in the difference-in-differences type setup that equation (1) amounts to.[25] The said countries are likely to constitute an appropriate control group. A complication that arises in the context of using the OECD-21 sample, however, is that the panel becomes long. This means that asymptotics should rely on fixed *N* and large *T*. We therefore use both pooled least squares with long panel-

[23] This 'no causation without manipulation' may appear at bit extreme for practical purposes. Indeed, Gerring (2012) argues that manipulability should only be considered a desirable trait, among others, in a causal analysis.

[24] These are Austria 1961, Belgium 1961, Canada 1961, Denmark 1961, France 1961, Germany 1961, Greece 1961, Iceland 1961, Ireland 1961, Italy 1962, Japan 1964, Luxembourg 1961, Netherlands 1961, Norway 1961, Portugal 1961, Spain 1961, Sweden 1961, Switzerland 1961, Turkey 1961, United Kingdom 1961, and the United States 1961.

[25] These countries are Canada, Iceland, Japan, Norway, Switzerland, Turkey, and the United States.

corrected standard errors as well as the Prais-Winsten FGLS estimator with AR(1) errors. That is, as the covariance matrix elements, σ_{ij}, are estimated using those observations that have common time periods, these estimators achieve their asymptotic behaviour as the T_i's approach infinity. While we include country fixed effects in the long panel context, we use a linear time trend instead of time fixed effects, as advised by Cameron and Trivedi (2010).

Second, we also estimate a standard growth regression (see Barro and Sala-i-Martin 2004) on the global sample. The growth regression looks as follows:

$$(2) \quad \Delta \log(y_{it}) = \alpha + \delta_t + \gamma \log(y_{it-1}) + \rho EU_{it} + \epsilon_{it}.$$

The inclusion of the lagged income term, $\log(y_{it-1})$, is meant to control for convergence effects (distance from a country's steady state); indeed, this can be given a structural interpretation (Barro and Sala-*i*-Martin 2004). A more mundane interpretation is that it picks up more general advantages of backwardness (Abramovitz 1986). That is, where estimation of equation (1) on the OECD-21 sample seeks to eliminate confounders via simple stratification logic, the growth regression aims to do so via the inclusion of lagged income per capita. While stratification is likely more convincing as a strategy to rule out confounding, the growth regression has the advantage that it can be estimated on a much larger sample.

Equations (1) and (2) are not nested. Of course, we could ensure nesting by estimating the following equation:

$$(3) \quad \Delta \log(y_{it}) = \alpha + \mu_i + \delta_t + \gamma \log(y_{it-1}) + \rho EU_{it} + \nu_{it}.$$

Unfortunately, the conditions for consistent estimation of equation (3) are much more demanding than those required for estimation of equations (1) and (2) (see e.g. Wooldridge 2010, Cameron and Trivedi 2005, or Roodman 2009). The dynamic panel-data model in equation (3) can be estimated using the somewhat complicated machinery of difference and system GMM estimators. These estimators do not require external instruments, but instead rely on lags of the instrumented variables (so-called internal instruments). Consequently, they allow the unobserved country fixed effects to be correlated with the lagged dependent variable[26]; it is this

[26] To see that $\log(y_{it-1})$ is a lagged dependent variable use that $\Delta \log(y_{it}) = \gamma \log(y_{it-1})$ is mathematically equivalent to $\log(y_{it}) = (\gamma + 1) \log(y_{it-1})$.

correlation that makes standard estimators inconsistent, provided of course that (3) is the correct specification. We will estimate equation (3) on a global sample using these dynamic panel methods.[27]

The appropriate length of the panel is not clear-cut. The Eurozone both exacerbated the pre-2008 boom and the post-2010 crisis. It exacerbated the boom because, as Walters (1990) warned long ago, with the nominal interest being beyond any single country's own control, the real interest rate falls with rising inflation, and vice versa. This rendered the system potentially unstable. It exacerbated the bust due to the incompleteness of the Eurozone edifice (Sinn 2014; De Grauwe 2014).[28] Thus it may make some sense to consider what happens when we discard the years 2010-2015 from the observation window. On the other hand, it probably makes less sense to discard the global financial crisis years 2008-09, as basically all rich countries were hit hard and because it represented a warranted correction.[29] Consequently, we will consider three periods. The full sample period 1961-2015 (1951-2011) is the result of using all available data in the World Development Indicators [WDI] 2015 (Penn World Tables [PWT] 8.1); that is, this is the maximum length of the panel.[30] The sample period 1961-2009 (1951-2009) removes the Eurozone crisis period, whereas the sample period 1961-2007 (1951-2007) removes the global financial crisis.

A few words on the pros and cons of WDI versus PWT: The PWT panel is longer than (more years), but not as wide as (fewer countries), WDI. Moreover, a recent assessment of measures of economic activity, based on nighttime lights data, concludes that the WDI are as good, and often better, measures of unobserved true income as are any recent vintages of the PWT (Pinkovskiy and Sala-i-Martin 2016). WDI is therefore our preferred data source. However, the advantage offered by PWT for present purposes is that the EU_{it} variable varies for all EU countries. As the WDI sample starts in 1961, the 'original six' do not contribute to the *within variation* in EU_{it}.[31]

---

[27] Both estimators assume no second-order autocorrelation in the differenced errors, so this will be tested. Moreover, there is the problem of 'instrument proliferation', so the number of instruments will be reduced by setting the number of usable lags to two. The two estimators basically differ in that the difference estimator instruments differences with levels, whereas the system estimator instruments levels with differences.

[28] According to Brunnermeier et al. (2016) it was difficult to decide prior to the crisis whether growth was sound or driven by cheap credit. They invoke "the naked swimmer" metaphor to describe this: "Only when the tide goes out and the water level recedes does it become apparent whether the swimmer is naked" (p. 118).

[29] Of course, the Eurozone crisis also represented a warranted correction, as many Spanish *cajas* and German *sparkassen* had lend aggressively to fund property booms in peripheral Eurozone economies.

[30] We use GDP per capita in constant local currency from World Development Indicators 2015. The WDI indicator code is NY.GDP.PCAP.KN. From PWT 8.1, we extract real GDP per worker. We construct it as real GDP at chained PPP divided by employment (i.e., rgdpe/emp in PWT notation). PWT Data, which can be downloaded at http://www.rug.nl/research/ggdc/data/pwt/pwt-8.1, are from Feenstra et al. (2015).

[31] Note, however, that EU-28 becomes EU-27 in PWT, as Croatia first entered the EU in 2013.

Figure 4 displays the correlation (correlation coefficient is 0.63) between annual growth in real GDP per worker from PWT and annual growth in real GDP per capita from WDI.[32]

[FIGURE 4 ABOUT HERE]

B. Results

Table 1 reports estimates of growth of real GDP per capita (columns 1-3) and GDP per worker (columns 4-6) on the binary EU membership variable and country and time fixed effects; i.e., it estimates equation (1). Here we basically measure pre- and post-entry growth for the EU countries up against the growth trajectories of all other countries. Inspection of the table reveals that EU membership is associated with lower economic growth in all columns. In column 1, where we use the maximum length WDI sample (i.e., 1961-2015), EU entry is associated with a statistically significant growth reduction of roughly 1.8 percentage points per year. When we remove the period associated with the sovereign debt crisis in the Eurozone (i.e., 2010-15), the reduction remains significant but is lower (1.27 percentage points per year). Finally, when we remove the global financial crisis of 2008-09, the reduction (which is now statistically insignificant) is 0.5 percentage points per year. Using GDP per worker growth from PWT gives roughly similar results, cf. columns 4-6. Consequently, in a difference-in-differences type setting EU entry seems to have reduced economic growth.[33]

[TABLE 1 ABOUT HERE]

As alluded to in Section III.A, this negative finding is likely a result of convergence dynamics.[34] Consequently, in Table 2 we focus on the OECD-21 sample where all countries are to some degree (structurally) similar; indeed, a *t*-test (unequal variances) fails to reject the null that the difference in means in (log) real GDP per capita in 1961 between EU and non-EU OECD-21 countries is zero (*p*-value 0.22). In this OECD-21 sample we find neither an economically nor a statistically significant difference between economic growth in EU and

[32] Mathematically, GDP per capita can be written as GDP/P, where P is the population. GDP per worker (or labour productivity), on the other hand, is equal to GDP/L, where L is the labor force. This means that GDP per capita equals GDP per worker times the fraction of the population in the labor force; i.e., $GDP/P = (GDP/L)(L/P)$. As the L/P ratio varies across countries, GDP per capita and GDP per worker are related but different concepts.

[33] Table A.2 of the Supplementary Appendix investigates what happens when we allow for treatment heterogeneity by allowing the five successive waves of EU enlargement to have differential impacts on economic growth. Except for the enlargement in 1973 where Denmark, Sweden and the UK entered, results are negative, as in the restricted model reported in Table 1.

[34] Formally, if $COV(EU_{it}, \log(y_{it-1})) > 0$ and $\gamma < 0$ (where γ is the population coefficient associated with the omitted term $\log(y_{it-1})$) then plim $\hat{\rho} < \rho$ when estimating equation (1).

non-EU OECD member countries, cf. Table 2.[35] Moreover, point estimates shift between being positive and negative when we use WDI data, cf. panel A of Table 2. All in all, this strongly suggests that the EU has performed at a level comparable to its closest peers.[36]

[TABLE 2 ABOUT HERE]

Focusing attention on OECD-21 (as done in Table 2) seeks to balance the data using stratification. This amounts to *indirect* covariance control, which (in many cases) is a credible causal strategy. In Table 3, we turn to the less credibly strategy of *direct* covariance control via estimation of the growth regression associated with equation (2) on the global sample. Inspection of the table reveals that WDI data tends to find a negative effect of EU membership, cf. columns 1-3. However, this impression is turned on its head once we use PWT data, where the EU variable is always positive and significant. The difference between WDI and PWT are neither driven by the sample reduction of 39 countries going from WDI to PWT nor by the change in panel length. In fact, when we estimate equation (2) on the sample generated by the intersection of the WDI sample and the PWT sample (results not reported), the EU variable is positive and significant in the PWT sample ($\rho_{PWT} = 0.006$, p-value = 0.062) but negative and significant in the WDI sample ($\rho_{WDI} = -0.005$, p-value = 0.046). This probably reflects nontrivial uncertainties surrounding national accounts data (see footnote 44 for more on this issue).

[TABLE 3 ABOUT HERE]

We cannot rigorously decide between equations (1) and (2), as the two equations are non-nested. Consequently, in Table 4 we turn to dynamic panel estimation of equation (3), which nests equations (1) and (2). Inspection of Table 4 reveals a negative impact of EU membership when the difference GMM estimator is employed. This is regardless of whether we use real GDP per capita from WDI (cf. panel A, columns 1-3) or real GDP per worker from PWT (cf. panel B, columns 1-3). However, the effect is always statistically insignificant. When system GMM is employed, we find evidence of a growth premium in the WDI context when the sample ends in 2009 (cf. panel A, column 5). However, as we reject the null of no second order autocorrelation, the conditions for consistent estimation are not met in this case. Moreover, when we use PWT's real GDP per

[35] In the Appendix, we exclude the US and Japan from the OECD-21 sample. This makes no difference to our results, as inspection of Appendix Table A.7 reveals.

[36] Of course, we cannot rule out that a rising EU has "lifted all boats" in the OECD-21 sample.

worker in panel B, there is no effect. Overall, the basic message from the dynamic panel estimations reported in Table 4 is that the EU entry has no measurable impact on economic growth.

[TABLE 4 ABOUT HERE]

Let's sum up the results so far. First, when we estimate equation (1) on the global sample (using either WDI or PWT) we tend to find a growth drag associated with EU entry, cf. Table 1. The growth drag is not driven by the Eurozone crisis; it also obtains when the years 2010-15 are removed from the dataset. However, the growth drag is only found in the global sample, where confounders are more likely to taint results; it is never found in the preferred OECD-21 sample, where economic and statistical insignificance obtains in all columns, cf. Table 2. Second, when we turn to equation (2), WDI and PWT yield opposing results, cf. Table 3. As noted above, these opposing findings are not driven by sample differences. Finally, when we estimate equation (3) using dynamic panel methods, we also conclude that there is no growth effect of EU membership, cf. Table 4. Overall, this suggests the conclusion that the EU has performed at a level comparable to its peers; or, put differently, EU membership does not appear to increase prosperity.

In Section C below, we subject this conclusion to a list of auxiliary robustness checks.

C. Additional Robustness Issues[37]

1. EU Accession Dynamics

Accession to the EU is not automatic. The applicant country must prepare itself before joining the EU, which means complying with accession criteria and implementing the *acquis communitaire*.[38] The accession criteria, also known as the Copenhagen criteria, are political stability (e.g., institutions guaranteeing democracy, rule of law, human rights), economic criteria (e.g., a functioning market economy), and administrative and institutional criteria (e.g., the capacity to take on the obligations of membership).

Between March 1994 and January 1996, for example, ten Central and Eastern European countries submitted their applications to the EU. Cyprus and Malta had already submitted their applications in 1990. Ten of these twelve countries became EU members in 2004. In the run-up to accession, the countries undertook many reforms and received help from the EU to modernize agriculture, infrastructure, and so on. To the extent that

[37] For brevity, we now restrict attention to WDI data. Note in addition that, as shown by Pinkovskiy and Sala-i-Martin (2016), WDI are likely superior to PWT data in terms of estimating true income.

[38] https://ec.europa.eu/neighbourhood-enlargement/policy/glossary/terms/accession-eu_en

these reforms improved the economy, it may be misleading to use the date of actual accession in an empirical analysis. That is, part of the growth premium of EU membership may be realized *prior* to actual membership. Note, however, that if there were a pre-entry growth effect, but no post-entry effect, EU membership would merely offer a *level effect* (or, equivalently, a temporary growth effect), not a *permanent* growth effect. To explore potential pre-entry growth effects, we lag the EU membership variable 10, 9…, 1 years (that is, we substitute EU_{it-j} $(j = 10, 9, \dots, 1)$ for EU_{it} in our estimating equations).

Table A.3 of the Appendix reveals the importance of the length of the panel. If we use the full length (1961-2015) there is no evidence of a pre-entry growth boost, while the evidence of a growth drag remains. When we instead drop the years of the eurozone crisis, 2010-15, from the sample the picture is less clear: There is always insignificance in the growth regressions (cf., columns 11-12), but in the OECD-21 sample there is now evidence of a growth boost at lags 8 to 10.

Overall it is therefore safe to conclude that trying to take pre-accession dynamics into account does little to alter the conclusion from Tables 1-4. The conservative conclusion that the EU appears to have no impact on prosperity remains intact.

2. Formerly Planned Economies

Yet another way to assess the economic growth contribution of EU membership is to look at the sample of the Eastern European countries, which were granted membership in 2004 or later. These countries are Bulgaria, Croatia, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovakia and Slovenia, cf. Appendix Table A.1. All these countries were previously planned economies, although they were also, on the whole, geographically closer to Western Europe than other non-EU acceding countries further to the East (Kopstein and Reilly 2003). It therefore makes sense to compare these new EU member' growth record with that of 18 *non-EU* countries that were also connected with the Soviet orbit.[39] That is, in this section we ask whether growth picked up in the new Eastern European EU countries after accession vis-à-vis growth in 18 formerly planned non-EU countries.

Before turning to regression based statistical inference, it may be instructive to take a brief look at economic growth before and after the EU accession in the accession countries. Of the 11 accession countries, not even one had higher *average* annual real GDP per capita growth in the period after the EU accession as

[39] The full sample consists of the following 29 countries: Albania, Armenia, Azerbaijan, Bulgaria, Bosnia & Herzegovina, Belarus, Croatia, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kosovo, Kyrgyz Republic, Latvia, Lithuania, Macedonia, Moldova, Montenegro, Poland, Romania, Russia, Serbia, Slovakia, Slovenia, Tajikistan, Turkmenistan, Ukraine, and Uzbekistan.

compared to the period before, cf. Figure 5. This means that a simple difference estimator would find a negative effect; one that is statistically insignificant, as it turns out (not reported).

[FIGURE 5 ABOUT HERE]

The financial crisis of 2008-09 and the Eurozone crisis certainly confound any attempt at inference, as crises tend to elevate growth prior to their onset and the reverse after. However, once we turn to difference-in-differences type regressions as in equation (1), where the comparison group consists of the formerly planned non-EU accession countries, we control for the potentially confounding impact of crises, as the non-accession countries were also influenced by them. This is of course the advantage of the difference-in-differences setup. If we calculate real GDP per capita growth for the group of 18 formerly planned economies that did not enter the EU before and after 2004 (the year in which eight of the 11 accession economies joined the EU), we get respectively −0.8% and 4.7%. Doing the same calculation for the accession countries, we get respectively 3.32% and 2.99%. Thus, not only was the growth rate in the non-accession countries faster after 2004 compared to before, but these countries (as a group) also grew faster than accession countries after 2004. In fact, save for the year 2014, growth has been higher every year since 2004 in non-accession countries compared to accession countries, cf. Figure 6.[40] Such simple considerations should instil moderation with respect to what we can expect in terms of an EU-induced growth premium in the formerly planned EU accession economies.

[FIGURE 6 ABOUT HERE]

Let us now turn to regression results, which are reported in Appendix Table A.4. As is immediately obvious upon inspection of the table, all EU coefficients are negative. Moreover, they are statistically significant

[40] The better pre-2004 growth performance of the (subsequent) EU accession countries (evident especially between 1990 and 1996) may be due to more radical post-communist transition reforms in this fast-reformer subset of former communist states. Alternatively, or additionally, it could be due to economic diffusion and network effects due to these EU-accession countries' closer proximity to the EU-15, as compared to countries in the Western Balkans and the former Soviet Republics (Kopstein and Reilly 2003; Krugman 1995). By contrast, the worse post-2004 growth performance of EU accession countries may represent greater exposure to the Eurozone crisis in the latter group of countries. Alternatively, it may reflect entirely different models of economic development adopted by the less democratically advanced polities in the Western Balkans and the former USSR (Acemoglu and Robinson 2012). Recent research by the IMF may also shed light on our findings (Atoyan et al. 2016). Large and persistent emigration from Eastern Europe has hurt sending countries' economies. Labor outflows—particularly outflows of young, skilled workers—lowered productivity growth, pushed up wages, and slowed growth and income convergence. Simultaneously, remittance inflows may have reduced incentives to work and led to exchange rate appreciations, eroding competitiveness. The departure of young, skilled workers also added to the fiscal pressures of already aging populations in Eastern Europe. In the Supplementary Appendix, we discuss the consequences of migration for our empirical analysis in some detail.

when equations (1) and (2) are used (cf. columns 1-2, 4-5), but statistically insignificant in equation (3) (cf. columns 3 and 6). This once again suggests that the EU has not added anything in terms of higher economic growth.

3. Spatial Error Correlation

So far, we have assumed that growth rates are statistically independent across countries. This is potentially problematic, as pointed out long ago by among others De Long and Summers (1991). The concern is that unobservables may be correlated across countries, for which reason each country does not necessarily provide as informative and independent an observation as any other.[41] Such spatial dependencies distort conventional standard errors, which leads to unreliable statistical inference. It is not possible to obtain precise knowledge about spatial dependencies across countries. However, the fact that we use country fixed effects should reduce the problem somewhat, as the country fixed effects pick up correlation among slow-moving and time-invariant unobservables. Nevertheless, it may be of some interest to address the issue more directly in order to gauge whether statistical inference is based on a (fairly) safe footing.

A widely used approach to address the problem is to assume that the physical distance between countries reflect their proximity in terms of unobservables. Under this assumption, Conley (1999) shows how to calculate consistent standard errors through what is essentially an application of Hansen's (1982) GMM estimator to spatial error autocorrelation.[42] In what follows, we apply the Conley estimator to the difference-in-differences type regressions used in Table 1, columns 1-3, in an attempt to gauge the consequences of spatial dependencies.[43] Appendix Table A.5 reports the results. Since we are not using instruments, parameters are always estimated using just-identified GMM. This means that allowing for spatial dependencies will only affect standard errors; it never affects point estimates. Interestingly, Conley standard errors are slightly less conservative (i.e., they are smaller and thus rejects the null hypothesis more frequently) than the standard clustered standard errors. In any case, as the ratio of Conley to clustered standard error is between 0.75 and 0.92 (cf. Table 1, columns 1-3, and Appendix Table A.5), spatial dependencies do not appear to be important for results reported in this paper.

---

[41] De long and Summers exemplify the problem using Belgium and Holland, whose respective growth rates are unlikely ever to deviate significantly. Of course, the growth rates within the EU are also highly dependent.

[42] The time-series analogue of this approach is roughly that of Newey and West (1987).

[43] We use Stata code developed by Hsiang (2010) and subsequently amended by Thiemo René Fetzer (see http://www.trfetzer.com/conley-spatial-hac-errors-with-fixed-effects/).

4. Panel Averages

In the cross-country growth literature, it is not uncommon to generate panel averages to prune the data for business cycles. As we always include a time trend or a full set of time dummies, this should not make any difference to the results reported in the paper. For completeness, however, Appendix Table A.6 reports results from the global sample and the OECD-21 sample when the panel is averaged into 8-year epochs. Columns 1-2 contain results for the global sample and columns 3-4 for the OECD-21 sample. A quick comparison between respectively Tables 1 (columns 1 and 3) and A.6 (columns 1-2) and Tables 2 (panel A, columns 1 and 3) and A.6 (columns 3-4) reveals that results do not change with 8-year epochs.

IV. Concluding remarks

This paper has been unable to reject the null hypothesis that 'EU membership has *zero* impact on economic growth'. Of course, we cannot rule out that our inability to reject the null is a consequence of unsurmountable difficulties with respect to approximating the true counterfactual outcome. This objection applies to all observational studies. Moreover, we cannot rule out that our inability to reject the null hypothesis simply reflects noisy data. Although all OECD countries refer to the same international system of national accounts, in practice the quality of their statistical systems varies somewhat (Lequiller and Blades 2014). The OECD concludes that different measurement methods imply that a growth discrepancy between the United States and another OECD country of less than 0.3 percentage points should not be considered statistically significant (Lequiller and Blades 2006).[44] Add to this that Gordon (2016) and Feldstein (2017) argue respectively that GDP fails to account for true growth in prosperity historically and today.

Whichever way one chooses to interpret our results, the inability to reject the null hypothesis is not inconsequential. Indeed, many official reports arrive at the opposite inference: the OECD's Brexit report, for example, claims that the EU has contributed in no small measure to British prosperity;[45] the Danish government recently commissioned a study, which found that EU membership had made Danes much richer;[46] and

[44] When we turn to emerging markets, we are on a substantially less solid base. Indeed, for some countries one may reasonably doubt whether GDP data make much sense at all (Jerven 2013). In Zambia, for example, just one man was responsible for preparing national income accounts in 2010; at the same time, incentives were biased against producing estimates. Moreover, data collection is often politicized with measurement often "taking the backseat" (Jerven 2013). For many African countries, base years for GDP series even date back some 20 years.

[45] See http://www.oecd.org/eco/the-economic-consequences-of-brexit-a-taxing-decision.htm.

[46] See https://erhvervsstyrelsen.dk/det-indre-markeds-oekonomiske-betydning-danmark

Netherlands Bureau for Economic Policy Analysis, an independent part of the Ministry of Economic Affairs, found that EU membership had made the Dutch much richer.[47] Our findings suggest an agnostic position vis-à-vis the growth effects of EU membership would be more appropriate. Moreover, this is consistent with the latest thinking on growth strategy. The EU can create a level playing field but there is no off-the-shelf blueprint when it comes to growth policies. Policies to address country-specific binding constraints on growth must be tailored to local context, and so it becomes the remit of national policymakers (Rodrik 2010).

The pertinence of context has transformed how the economics profession thinks about growth strategy. Until recently, international institutions such as the IMF and the World Bank would have recommended a *non-contextual* growth strategy. This presumed that it was possible *ex ante* to settle on a unique set of appropriate institutional arrangements (best-practices) and that convergence towards these institutional arrangements would lead to higher economic growth. Today, these same international actors recognize the importance of context, as elucidated in the growth diagnostics framework (see IMF 2013). Indeed, the IMF highlights that a number of its recent country reports and papers have applied growth diagnostics to identify country-specific binding constraints on growth. The governing idea behind growth diagnostics is that a few focused policy interventions (i.e., the removal of a small number of binding constraints on growth) are far superior to the traditional approach based on implementing a fairly long list of reforms, which has failed to deliver in terms of economic growth.[48] The aim of growth diagnostics is to identify the constraints with the largest direct effects, in the hope that removal of these will stimulate economic growth (i.e., the direct effects will overshadow any offsetting indirect – or general equilibrium – effects).[49]

---

[47] See https://www.cpb.nl/sites/default/files/publicaties/download/internal-market-and-dutch-economy-implications-trade-and-economic-growth.pdf

[48] This is backed by empirical results reported in Hausmann et al. (2005), who have studied a set of growth accelerations (i.e., increases in growth of GDP per capita) that were sustained for at least eight years, that had a post-acceleration growth rate of at least 3.5% per year, and (to rule out cases of pure recovery) that had post-acceleration output exceeding the pre-episode peak level of income. Using a large cross-country sample spanning the period since the 1950s, Hausmann et al. find episodes of growth accelerations. Of these growth accelerations, only 14.5% are accompanied by standard macroeconomic reforms. That is, more than 85% of all growth accelerations are not associated with reforms. Hausmann et al. conclude that growth accelerations are mainly caused by idiosyncratic, and often small-scale, changes. This is consistent with the idea that a set of country-specific binding constraints may in fact be holding down the economy's growth rate.

[49] This can be formalized using the theory of second best, which teaches that the removal of an arbitrarily chosen distortion in an economy with multiple distortions is not guaranteed to be welfare (i.e., growth) enhancing. The only way a reformer can be sure to increase economic growth is by across-the-board reform. This is clearly not an operational growth strategy, as it requires complete knowledge of *all* distortions in the economy. In an economy with multiple distortions, a small reduction in a single distortion leads to a *direct effect* on economic growth. The more the distortion binds, the larger is the direct effect. There are also *indirect effects* from reducing the said distortion, which may *offset* the direct effect. The aim of growth diagnostics is to remove the distortions with the largest direct effects, in the hope that this will overshadow any offsetting indirect effects. Interestingly, Jones (2011) constructs a growth model in which linkages and complementarities can amplify the macroeconomic effects of small microeconomic distortions. He argues that in his model the approach of growth diagnostics would work well.

Consider now joining the EU. As already noted, this means complying with the accession criteria and implementing the EU *acquis*, which is essentially a non-contextual reform strategy. The growth diagnostics framework suggests that there can be no presumption that this will target the binding constraints on growth, let alone the binding constraints with the largest direct effect. By extension, there can be no presumption that EU membership will have a discernible effect on the economic growth of its member states. The latest thinking on growth strategy thus provides no reason to expect that EU membership will affect economic growth. Put differently, joining the EU does not make for a growth strategy *per se*. The results reported in this paper are consistent with this line of thinking.

References

Abramovitz, Moses (1986). Catching Up, Forging Ahead, and Falling Behind, Journal of Economic History. 46: 385-406.

Acemoglu, Daron and James Robinson. 2012. Why Nations Fail: The Origins of Power, Prosperity and Poverty. New York: Profile Books.

Alesina, Alberto, Ignazio Angeloni and Ludger Schuknecht (2005). What does the European Union do?" Public Choice. 123: 275-319.

Angrist, Joshua D. and Jörn-Steffen Pischke (2009). Mostly Harmless Econometrics. Princeton, NJ: Princeton University Press.

Atoyan, Ruben, Lone Christiansen, Allan Dizioli, Christian Ebele, Nadeem Iladi, Anna Ilyina, Gil Mehrez, Haonan Qu, Faezeh Raei, Alaina Rhee and Daria Zakharova (2016). Emigration and Its Economic Impact on Eastern Europe, IMF Staff Discussion Note. SDN 16/07.

Barro, Robert J. and Xavier Sala-i-Martin (2004). Economic Growth, 2nd Edition. Cambridge, MA: MIT Press.

Benz, Matthias and Alois Stutzer (2004). Are Voters Better Informed When They Have a Larger Say in Politics? Evidence for the European Union and Switzerland, Public Choice. 119: 31-59.

Brunnermeier, Markus, Harold James and Jean-Pierre Landau (2016). The Euro and the Battle of Ideas. Princeton, NJ: Princeton University Press.

Cameron, A. Colin and Pravin K. Trivedi (2005). Microeconometrics. New York, NY: Cambridge University Press.

Cameron, A. Colin and Pravin K. Trivedi (2010). Microeconometrics Using Stata. Revised Edition, College Station, TX: Stata Press.

Campos, Nauro F., Fabrizio Coricelli and Luigi Moretti (2014). Economic Growth and Political Integration: Estimating the Benefits from Membership in the European Union Using the Synthetic Counterfactuals Method, IZA Discussion Paper, No. 8162, IZA.

Conley, Timothy G. (1999). GMM Estimation with Cross Sectional Dependence, Journal of Econometrics. 92: 1-45.

Cuaresma, Jesus Crespo, Doris Ritzberger-Grünwald and Maria Antoinette Silgoner (2008). Growth, convergence and EU membership, Applied Economics. 40: 643-656.

De Grauwe, Paul (2014). The Economics of Monetary Union, 10th Edition. Oxford: Oxford University Press.

De Long, J. Bradford and Lawrence Summers (1991). Equipment Investment and Economic Growth, Quarterly Journal of Economics. 106: 445-502.

European Commission (2012). 20 Years of the European Single Market. http://ec.europa.eu/internal_market/publications/docs/20years/achievements-web_en.pdf

Feenstra, Robert C., Robert Inklaar and Marcel P. Timmer (2015) The Next Generation of the Penn World Table, American Economic Review. 105: 3150-3182

Feldstein, Martin (2017). Underestimating the Real Growth of GDP, Personal Income, and Productivity, Journal of Economic Perspectives. 31: 145-164.

Guay, Terrence R. (2014). The Business Environment of Europe. Cambridge: Cambridge University Press.

Gerring, John (2012). Social Science Methodology. Cambridge: Cambridge University Press.

Gertler, Paul J., Sebastian Martinez, Patrick Premand, Laura B. Rawlings and Christel M. J. Vermeersch (2011). Impact Evaluation in Practice. Washington, DC: The International Bank for Reconstruction and Development.

Giddens, Anthony (2014). Turbulent and Mighty Continent: What Future for Europe? Cambridge: Polity Press.

Gordon, Robert J. (2016). The Rise and Fall of American Growth. Princeton, NJ: Princeton University Press.

Guo, Shenyang and Mark W. Fraser (2010). Propensity Score Analysis. Los Angeles, CA: Sage.

Hansen, Lars Peter (1982). Large sample properties of generalized methods of moments estimators, Econometrica. 50: 1029-1054.

Hausman, Daniel, Michael McPherson, and Debra Satz (2017). Economic Analysis, Moral Philosophy, and Public Policy. Cambridge: Cambridge University Press.

Hausmann, Ricardo, Lant Pritchett and Dani Rodrik (2005). Growth Accelerations, Journal of Economic Growth. 10: 303-329.

Henrekson, Magnus, Johan Torstensson and Rasha Torstensson (1997). Growth effects of European Integration, European Economic Review. 41: 1537-1557.

Hirschman, Albert O. (1977). The Passions and the Interests: Political Argument for Capitalism before its Triumph. New Jersey: Princeton University Press.

Hsiang, Solomon M. (2010). Temperatures and cyclones strongly associated with economic production in the Caribbean and Central America, Proceedings of the National Academy of Sciences of the United States, PNAS. 107: 15367-15372.

Imbens, Guido W. (2014). Matching Methods in Practice, Journal of Human Resources. 50: 373-419.

IMF (2013). Jobs and Growth: Analytical and Operational Considerations for the Fund. http://www.imf.org/external/np/pp/eng/2013/031413.pdf

Jerven, Morten (2013). Poor Numbers: How We Are Misled by African Development Statistics and What to Do about It. Ithaca, NY: Cornell University Press.

Jones, Chad (2011). Intermediate Goods and Weak Links in the Theory of Economic Development, American Economic Journal: Macroeconomics. 3: 1-28.

Judt, Tony (2010). Postwar. London: Vintage Book.

King, Mervin (2016). The End of Alchemy. London: Little, Brown.

Kopstein, Jeffrey and David Reilly (2003). Postcommuist Spaces: A Political Geography Approach to Explaining Postcommunist Outcomes, in: Grezegorz Ekiert and Stephen Hanson (eds.), Capitalism and Democracy in Central and Eastern Europe. Cambridge: Cambridge University Press: 120-155.

Krugman, Paul (1995). Geography, Development and Economic Theory. Cambridge, Mass.: MIT Press.

Landau, D. (1995). The Contribution of the European Common Market to the Growth of Its Member Countries: An Empirical Test, Weltwirtschaftliches Archiv. 131: 774-782.

Lequiller, Francois and Derek Blades (2006). Understanding National Accounts. Paris: OECD Publishing.

Lequiller, Francois and Derek Blades (2014). Understanding National Accounts: Second Edition Revised and Expanded. Paris: OECD Publishing.

Manners, Ian (2008). The normative ethics of the European Union, International Affairs. 84: 45-60.

Martin, Philippe, Thierry Mayer and Mathias Thoenig (2008). Make Trade Not War?" Review of Economic Studies. 75: 865-900.

Mazower, Mark (1999). Dark Continent. London: Penguin Book.

Meyer, Bruce D. (1995). Natural and Quasi-Experiments in Economics, Journal of Business & Economic Statistics. 13: 151-161.

Morgan, Stephen L. and Christopher Winship (2015). Counterfactuals and Causal Inference. Cambridge: Cambridge University Press.

Newey, Whitney K. and Kenneth D. West (1987). A simple, positive semi-definite, heteroscedasticity and autocorrelation consistent covariance matrix, Econometrica. 55: 703-708.

Offe, Claus (2015). Europe Entrapped. Cambridge: Polity Press.

Ozgen, Ceren, Peter Nijkamp, and Jacques Poot (2010). The effect of migration on income growth and convergence: Meta-analytical evidence, Papers in Regional Science. 89: 537-561.

Pinker, Steven (2011). The Better Angels of our Nature. New York, NY: Viking.

Pinkovskiy, Maxim and Xavier Sala-i-Martin (2016). Newer Need Not Be Better: Evaluating the Penn World Tables and the World Development Indicators Using Nighttime Lights, NBER Working Paper, No. 22216.

Rodrik, Dani (2010). Diagnostics before Prescription, Journal of Economic Perspectives. 24: 33-44.

Roodman, David (2009). How to do xtabond2: An introduction to difference and system GMM in Stata, The Stata Journal. 9: 86-136.

Sinn, Hans-Werner (2014). The Euro Trap. Oxford: Oxford University Press.

Staab, Andreas (2013). The European Union Explained. 3E. Bloomington, IN: Indiana University Press.

Vanhoudt, Patrick. (1999). Did the European Unification Induce Economic Growth? In Search of Scale Effects and Persistent Changes, Weltwirtschaftliches Archiv. 135: 193-220.

Walters, Alan A. (1990). Sterling in Danger: The Economic Consequences of Pegged Exchange Rates. London: Fotana Press.

Wight, Jonathan B. (2015). Ethics in Economics. Stanford, CA: Stanford University Press.

Wooldridge, Jeffrey M. (2010). The Econometrics of Cross Section and Panel Data, 2nd Edition. Cambridge, MA: MIT Press.

SUPPLEMENTARY APPENDIX

Campos et al. (2014) study

In this supplementary appendix, we address the Campos et al. (2014) findings in the context of matching methods. Concretely, we use nearest-neighbour matching (NNM) on the covariates. NNM estimators impute the missing counterfactual for each country by using an average of the countries that are most similar (based on the vector of covariates), but are in the alternative treatment state, to predict unobserved potential outcomes. Consequently, the causal effect of treatment is computed as the average of the difference between observed and imputed potential outcomes for each country.[50] NNM is nonparametric in that no parametric form is assumed for either the outcome model or the treatment model.[51] Textbook treatments of NMM include Wooldridge (2010, Section 21.3.5) and Guo and Fraser (2010, Chapter 6).

Our approach entails looking at the nine EU members as of 1973, cf. Appendix Table A.1. This constitutes the treatment group. The control group is all other countries except for the 19 countries that eventually became EU members after 1973; these latter countries are excluded. We then use NNM to gauge the causal impact of EU membership on economic growth.

The covariates we start with are the pre-treatment variables used by Campos et al. These include the investment share of per capita GDP, population growth, initial income, share of agriculture in value added, share of industry in value added, secondary gross school enrolment, and tertiary gross school enrolment. We operate with two covariate sets: CS1, the smallest set, which is just initial income (log of real GDP per capita); and CS2, which is the full set of covariates. The outcome variable is average annual growth in real GDP per capita (PWT 8.1) between 1973 and 2011. The treatment variable records whether the country was one of the nine 1973 EU members (treatment) or not (no treatment). Pre-treatment population growth is calculated from 1960 to 1973, whereas the remaining variables are set to their 1973 value.

To conduct matching on the covariates we must make some subjective decisions. To minimize such decisions, we will mainly rely on the default settings in Stata's **teffects nnmatch**. This means *inter alia* using the Mahalanobis distance function and reporting Abadie-Imbens (robust) standard errors. The only non-default choices we make are to use bias-adjustment (which is recommended when covariates are continuous, cf. Guo and Fraser 2010) and to focus on the average treatment effect on the treated (ATET). We focus on ATET

[50] Under so-called unconfoundedness (i.e., selection-on-observables), the comparison of units with different treatments but identical pretreatment variables can be given a causal interpretation.

[51] This flexibility comes at a price in the sense that a parametric model converges faster to the "true" value as compared to the nonparametric NNM estimator.

because we are interested in whether EU membership benefitted actual EU members as opposed to whether it will benefit countries that will never become EU members. Consequently, the analysis entails minimum discretion on our part.

Table S.1 reports the results from the matching analysis. When we use the full 'Campos et al.' list of covariates (i.e., CS2), ATET is not significantly different from zero. This is the case regardless of whether we only use one match per treated observation (column 2) or two (column 3). The same goes for CS1, where we only use initial income as covariate, cf. column 1. In columns 4 and 5 of the table we report OLS results, as recommended by Imbens (2014). OLS results with respect to the EU variable are always statistically insignificant. Overall, the results in Table S.1 corroborate the findings reported in the main paper.

[SUP. APP. TABLE S.1 ABOUT HERE]

Migration

Recent research by the IMF may be of relevance to our findings. Concretely, Atoyan et al. (2016) argue that large and persistent emigration from Eastern Europe has hurt sending countries. Labor outflows—particularly outflows of young, skilled workers—lowered productivity growth, pushed up wages, and slowed growth and income convergence. Simultaneously, remittance inflows may have reduced incentives to work and led to exchange rate appreciations, eroding competitiveness. The departure of young, skilled workers also added to the fiscal pressures of already aging populations in Eastern Europe. At the same time, if citizens with above average income in a poor country migrate to a rich country and work there for a below average income, it might well be that average income declines in both countries. Could such an ecological fallacy work to taint our findings?

To explore this question, we have gathered data on net migration from World Development Indicators, where net migration is the number of immigrants less the number of emigrants (including both citizens and noncitizens). We normalize this number with the size of the population, and we call the resulting variable *migration*. This variable can be constructed for many countries at five year intervals, as migration is only available every fifth year.

Figure S.1 shows the values of migration for EU in 2012. As expected, poorer (richer) EU countries have negative (positive) net migration.

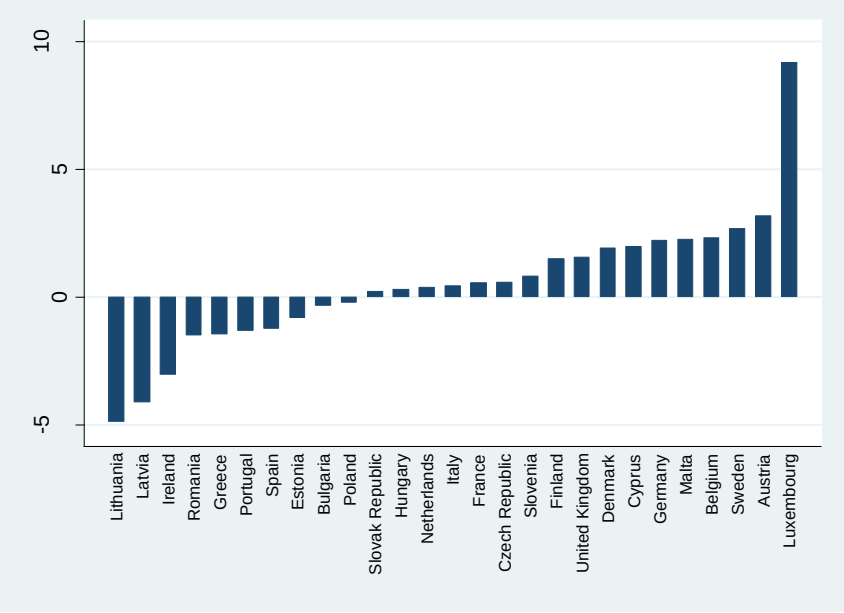

Figure S.1: Net migration as a percentage of total population for EU-27 in 2012. *Notes*: Source is the World Development Indicators 2017.

If there is an ecological fallacy, we would expect that the migration variable turns up statistically significant when introduced in our regressions. This is not the case, cf. Supplementary Appendix Table S.2. In the table, migration is always insignificant. Note also that every other column excludes the migration variable but estimates the EU variable on the exact same sample. When we compare same-sample estimations with and without the migration variable, we see that the migration variable has no impact on the EU variable. Thus, insignificance of the EU variable when migration is present is not due to multicollinearity.

[SUP. APP. TABLE S.2 ABOUT HERE]

We have further reasons to be skeptical that an ecological fallacy taints our results. First, if there is such an effect, it is likely to be quite small. Indeed, in a meta-analytical review of existing econometric studies on net migration, income growth and convergence, Ozgen, Nijkamp, and Poot (2010) find that a one percentage point increase in net migration increases the rate of real income per capita growth by only 0.1 percentage point. Second, the issue of an ecological fallacy probably only became an issue as of 2004, when the EU was enlarged with countries that had income levels significantly below the pre-2004 EU average. In all tables in the main paper, we report results obtained when the sample ends in 2007, so these results are not affected by an ecological fallacy. Third, poorer member countries are net recipients of EU funds, which works to counter the effects arising from an economic fallacy.

APPENDIX TABLES

[TABLE A.1]

[TABLE A.2]

[TABLE A.3]

[TABLE A.4]

[TABLE A.5]

[TABLE A.6]

[TABLE A.7]

FIGURES

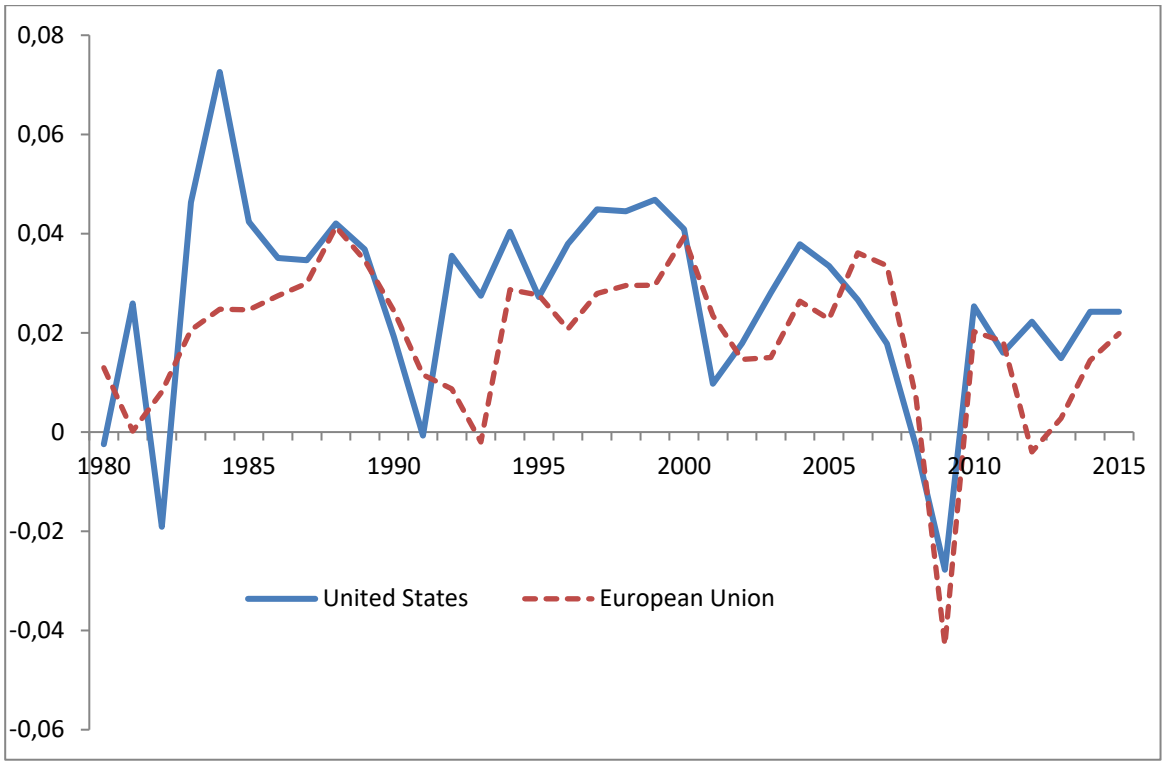

Figure 1: Annual real GDP growth in the European Union (broken line) and the United States (solid line).

Notes: The source is the IMF's World Economic Outlook Database, April 2016.

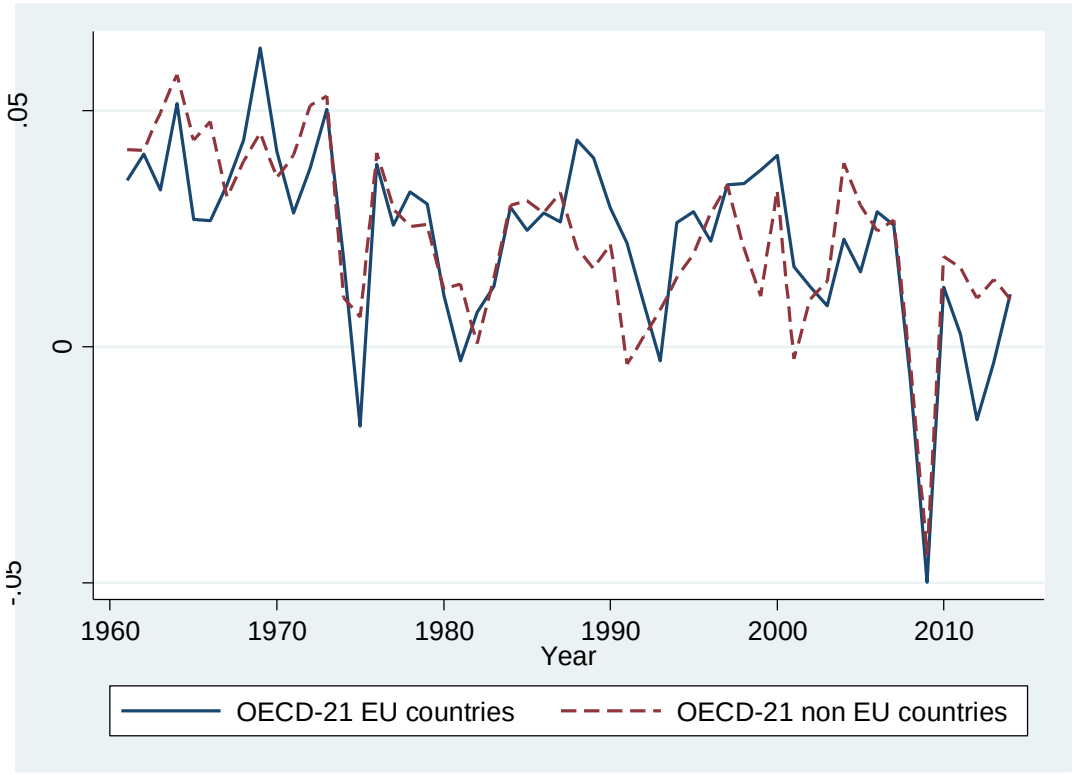

Figure 2: Annual real GDP *per capita* growth in the OECD-21 country sample for EU (solid line) and non-EU (broken line) countries, respectively. *Notes*: Series are unweighted averages. The source is World Development Indicators 2015.

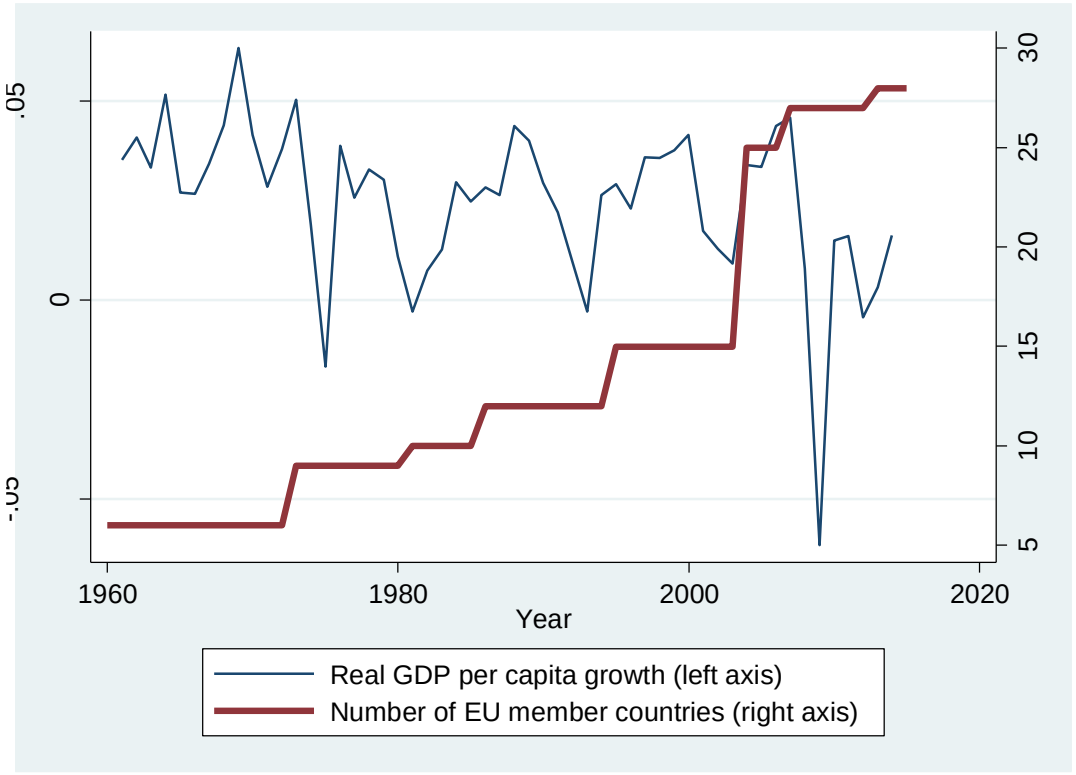

Figure 3: Unweighted EU real GDP per capita growth (left axis) and EU membership count (right axis). *Notes*: The period is 1960-2015. The growth rate is calculated as the unweighted sum of all EU countries; that is, EU-6 until 1973, EU-9 until 1981, and so on and so forth. The correlation between growth and membership count is -0.36. The sources are World Development Indicators 2015 and Staab (2013).

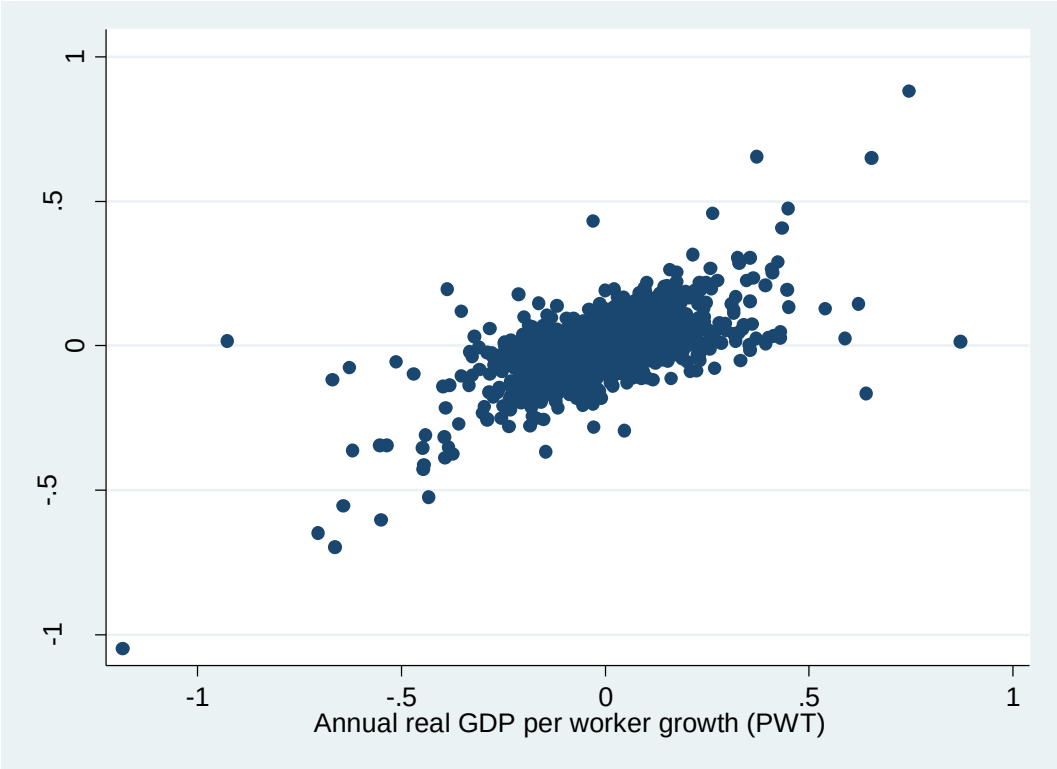

Figure 4: Scatter plot of PWT-based real GDP per worker growth (x-axis) and WDI-based real GDP per capita (y-axis). The period is 1960-2011, the correlation is 0.63, and the number of observations is 6,056. *Notes*: The sources are World Development Indicators 2015 and Penn World Tables 8.1 (see Feenstra et al. 2015).

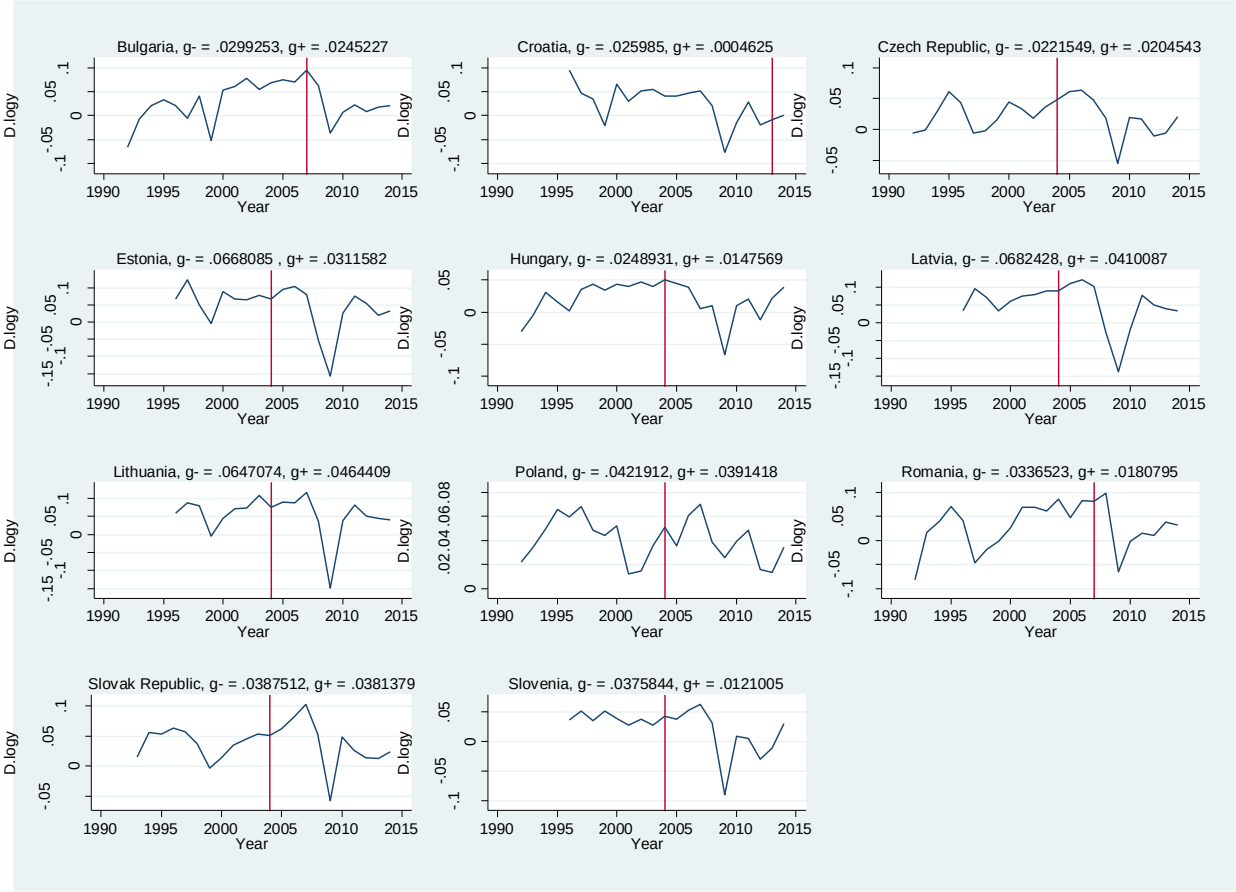

Figure 5: Real GDP per capita growth in formerly planned economies before and after the EU accession. *Notes*: Average annual growth before and after is given by respectively *g*- and *g*+. The vertical line is the date of accession. The data source is World Development Indicators 2015.

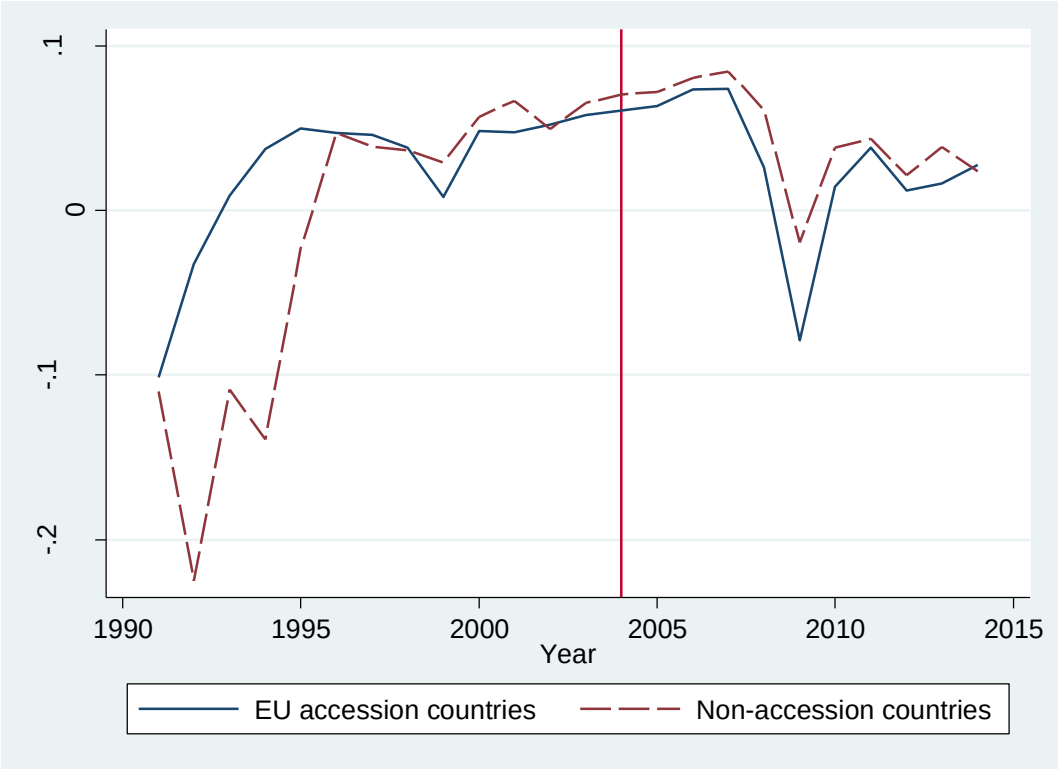

Figure 6: Real GDP per capita growth in formerly planned EU accession (solid line) and non-accession (broken line) countries before and after 2004. *Notes*: The vertical line is placed at year = 2004. The data source is World Development Indicators 2015.

Table 1: Estimation of equation 1 on global sample

VARIABLES	(1) Δlogy	(2) Δlogy	(3) Δlogy	(4) Δlogy	(5) Δlogy	(6) Δlogy
EU	-0.0179***	-0.0127***	-0,0054	-0.0119***	-0.0103**	-0.0106**
	(0.0040)	(0.0037)	(0.0037)	(0.0044)	(0.0043)	(0.0047)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,403	7,463	7.076	7.026	6,701	6,374
R-squared	0.056	0.061	0,053	0,051	0,051	0,047
Number of countries	205	205	204	166	166	166
Sample	Global sample 1961-2015	Global sample 1961-2009	Global sample 1961-2007	Global sample 1951-2011	Global sample 1951-2009	Global sample 1951-2007
Data source	WDI	WDI	WDI	PWT	PWT	PWT

Notes: Dependent variable is annual growth in real GDP per capita (worker) in columns 1 to 3 (columns 4 to 6). Robust standard errors clustered at the country level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include country and time fixed effects.

Table 2: Estimation of equation 1 on OECD-21 sample

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator		Pooled OLS			Prais-Winsten	
VARIABLES	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy
			Panel A			
EU	-0.0002	0.0017	-0.0005	-0.0005	0.0020	-0.0008
	(0.0032)	(0.0029)	(0.0028)	(0.0043)	(0.0045)	(0.0043)
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1.094	989	947	1,094	989	947
R-squared	0.169	0.149	0.104	0.113	0.105	0.068
Number of countries	21	21	21	21	21	21
Autocorrelation coefficient				0,270	0,316	0,284
Sample	OECD-21	OECD-21	OECD-21	OECD-21	OECD-21	OECD-21
	1961-2015	1961-2009	1961-2007	1961-2015	1961-2009	1961-2007
Data source	WDI	WDI	WDI	WDI	WDI	WDI
			Panel B			
EU	0.0030	0.0039	0.0023	0.0029	0.0041	0.0021
	(0.0039)	(0.0040)	(0.0039)	(0.0048)	(0.0051)	(0.0051)
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1.274	1.232	1.190	1.274	1.232	1.190
R-squared	0,112	0,117	0,094	0,085	0,085	0,067
Number of countries	21	21	21	21	21	21
Autocorrelation coefficient				0,176	0,219	0,215
Sample	OECD-21	OECD-21	OECD-21	OECD-21	OECD-21	OECD-21
	1951-2011	1951-2009	1951-2007	1951-2011	1951-2009	1951-2007
Data source	PWT	PWT	PWT	PWT	PWT	PWT

Notes: Dependent variable is annual growth in real GDP per capita (worker) in panel A (panel B). Standard errors in columns 1-3 are long panel-corrected errors, which allow heteroskedasticity and correlation over i. Standard errors in columns 4-6 in addition assume AR(1) for each i. The option *pairwise selection* in Stata's **xtpcse** routine is invoked in order to use all observations that can be matched by period between panels i and j. *** p<0.01, ** p<0.05, * p<0.1. All regressions include country fixed effects and a linear time trend.

Table 3: Estimation of equation 2 on global sample

VARIABLES	(1) Δlogy	(2) Δlogy	(3) Δlogy	(4) Δlogy	(5) Δlogy	(6) Δlogy
EU	-0.0050*	-0.0021	0.0030	0.0055*	0.0072**	0.0062*
	(0.0027)	(0.0025)	(0.0027)	(0.0032)	(0.0032)	(0.0035)
log(y_{it-1})	-0.0006	-0.0009	-0.0009	-0.0039***	-0.0047***	-0.0046***
	(0.0006)	(0.0008)	(0.0009)	(0.0014)	(0.0015)	(0.0015)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	No	No	No	No	No
Observations	8,403	7,463	7,076	7.026	6.701	6.374
R-squared	0,049	0,054	0,046	0,053	0,052	0,047
Number of countries	205	205	204	166	166	166
Sample	Global sample 1961-2015	Global sample 1961-2009	Global sample 1961-2007	Global sample 1951-2011	Global sample 1951-2009	Global sample 1951-2007
Data source	WDI	WDI	WDI	PWT	PWT	PWT

Notes : Dependent variable is annual growth in real GDP per capita (worker) in columns 1 to 3 (columns 4 to 6). Standard errors clustered at the country level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include time fixed effects.

Table 4: Estimation of equation 3 on global sample

	(1)	(2)	(3)	(4)	(5)	(6)
		Difference GMM			System GMM	
VARIABLES	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy
			Panel A			
EU	-0.0021	-0.0012	-0.0015	0.0261	0.0518**	-0.0543
	(0.0056)	(0.0053)	(0.0048)	(0.944)	(0.0226)	(0.2827)
log(y_{it-1})	-0.2646***	-0.2666***	-0.2477***	0.0167	0.0160	0.0156
	(0.0802)	(0.0681)	(0.0746)	(0.0393)	(0.0122)	(0.0130)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of instruments	160	145	139	213	193	185
Test for second order autocorrelation (p-value)	0,3172	0,1038	0,0713	0,1666	0,0246	0,0184
Test for overidentifying restrictions (p-value)	0,4119	0,4228	0,4000	0,2043	0,1335	0.1253
Observations	8,195	7,256	6,870	8.403	7.463	7.076
Number of countries	204	203	203	205	205	204
Sample	Global sample 1961-2015	Global sample 1961-2009	Global sample 1961-2007	Global sample 1961-2015	Global sample 1961-2009	Global sample 1961-2007
Data source	WDI	WDI	WDI	WDI	WDI	WDI
			Panel B			
EU	-0.0244	-0.0229	-0.0077	-0.0116	-0.0103	-0.0104
	(5.0689)	(4.1628)	(2.0061)	(11.2275)	(7.3333)	(6.8406)
log(y_{it-1})	-0.2562	-0.2374	-0.2775	0.0324	0.0347	0.0319
	(7.9848)	(8.1390)	(6.1276)	(0.3546)	(0.5829)	(0.3193)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of instruments	181	175	169	241	233	225
Test for second order autocorrelation (p-value)	0,9367	0,9276	0,9088	0,1001	0,1268	0,0602
Test for overidentifying restrictions (p-value)	0,2577	0,1549	0,0861	0,9991	0,9925	0.9799
Observations	6.860	6.535	6.208	7.026	6.701	6.374
Number of countries	166	166	166	166	166	166
Sample	Global sample 1951-2011	Global sample 1951-2009	Global sample 1951-2007	Global sample 1951-2011	Global sample 1951-2009	Global sample 1951-2007
Data source	PWT	PWT	PWT	PWT	PWT	PWT

Notes: Dependent variable is annual growth in real GDP per capita (worker) in columns 1 to 3 (columns 4 to 6). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include country and time fixed effects. Arellano-Bond test of the null of zero autocorrelation in first-differenced errors. The null in Sargan test of overidentifying restrictions is that overidentifying restrictions are valid; the Sargan test is calculated without cluster robust standard errors. Columns 1-3 are estimated using **xtabond** in Stata, whereas columns 4-6 are estimated using **xtdpdsys** in Stata. All columns are estimated with the twostep option, which means that we report robust standard errors that are obtained using the Windmeijer WC-robust estimator.

Table A.1: Chronology of EU enlargement

Year	Membership count	Countries
1958	Original six	(West) Germany, France, Italy, Belgium, Luxembourg, the Netherlands
1973	9	Britain, Denmark, Ireland
1981	10	Greece
1986	12	Portugal, Spain
1995	15	Sweden, Finland, Austria
2004	25	Malta, Cyprus, Estonia, Latvia, Lithuania, Poland, Hungary, Czech Republic, Slovakia, Slovenia
2007	27	Bulgaria, Romania
2013	28	Croatia

Source : Staab (2013).

Table A.2: Heterogenous enlargement effects

VARIABLES	(1) Δlogy	(2) Δlogy	(3) Δlogy	(4) Δlogy	(5) Δlogy	(6) Δlogy
EU wave 1				-0.0068	-0.0059	-0.0051
				(0.0065)	(0.0066)	(0.0064)
EU wave 2	0,0011	0,0036	0,0062	0.0060*	0.0061	0.0079*
	(0.0060)	(0.0057)	(0.0058)	(0.0036)	(0.0038)	(0.0041)
EU wave 3	-0.0233***	-0.0159***	-0.0134***	-0.0123**	-0.0094	-0.0096
	(0.0057)	(0.0047)	(0.0049)	(0.0059)	(0.0059)	(0.0061)
EU wave 4	-0.0141***	-0.0110***	-0.0058	-0.0273***	-0.0273***	-0.0259***
	(0.0040)	(0.0040)	(0.0052)	(0.0058)	(0.0055)	(0.0063)
EU wave 5	-0.0225***	-0.0187***	-0.0033	-0.0162**	-0.0149*	-0.0232**
	(0.0071)	(0.0074)	(0.0089)	(0.0080)	(0.0078)	(0.0101)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,403	7,463	7.076	7.026	6,701	6,374
R-squared	0,047	0,053	0,048	0,053	0,052	0,049
Number of countries	205	205	204	166	166	166
Sample	Global sample 1961-2015	Global sample 1961-2009	Global sample 1961-2007	Global sample 1951-2011	Global sample 1951-2009	Global sample 1951-2007
Data source	WDI	WDI	WDI	PWT	PWT	PWT

Notes: Dependent variable is annual growth in real GDP per capita (worker) in columns 1 to 3 (columns 4 to 6). The five enlargements refer to respectively the 1958 original six (wave 1), the 1973 enlargement (wave 2), the 1981-86 enlargement (wave 3), the 1995 enlargement (wave 4), and the 2004 and beyond enlargement (wave 5), cf. Table A.1. Standard errors clustered at the country level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regression include country and time fixed effects.

Table A.3: Pre-Accession Dynamics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU t-10	-0.0132***	-0.0075	-0.0013	0.0047	0.0084**	0.0091***	0.0041	0.0072	0.0079*	-0.0049	-0.0012	0.0027
EU t-9	-0.0140***	-0.0073	-0.0015	0.0053	0.0088***	0.0091***	0.0058	0.0096**	0.0094**	-0.0051	-0.0010	0.0028
EU t-8	-0.0157***	-0.0079	-0.0027	0.0034	0.0065**	0.0063**	0.0037	0.0069	0.0062	-0.0058*	-0.0012	0.0022
EU t-7	-0.0153***	-0.0092*	-0.0044	-0.0006	0.0030	0.0022	-0.0002	0.0021	0.0010	-0.0055*	-0.0017	0.0015
EU t-6	-0.0153***	-0.0096*	-0.0051	-0.0003	0.0019	0.0007	-0.0013	0.0006	0.0008	-0.0059**	-0.0019	0.0011
EU t-5	-0.0202***	-0.0163***	-0.0036	0.0004	0.0027	0.0013	0.0001	0.0025	0.0008	-0.0080***	-0.0053	0.0017
EU t-4	-0.0204***	-0.0166***	-0.0030	0.0017	0.0041	0.0027	0.0016	0.0044	0.0024	-0.0078**	-0.0052	0.0022
EU t-3	-0.0191***	-0.0144***	-0.0026	0.0029	0.0054*	0.0039	0.0037	0.0070	0.0047	-0.0069**	-0.0039	0.0029
EU t-2	-0.0183***	-0.0134***	-0.0025	0.0024	0.0047	0.0030	0.0027	0.0056	0.0032	-0.0061**	-0.0032	0.0034
EU t-1	-0.0183***	-0.0132***	-0.0049	-0.0003	0.0017	0.0002	-0.0013	0.0008	-0.0016	-0.0055**	-0.0027	0.0028
Sample width	Full sample			OECD-21			OECD-21			Full sample		
Sample length	1961-2015	1961-2009	1961-2007	1961-2015	1961-2009	1961-2007	1961-2015	1961-2009	1961-2007	1961-2015	1961-2009	1961-2007
Specification	Diff-in-diff			Diff-in-diff			Diff-in-diff			Growth regression		

Notes: Dependent variable is annual growth in real GDP per capita. Columns 1-3 corresponds to columns 1-3 of Table 1; columns 4-6 corresponds to columns 1-3 of Table 2; columns 7-9 corresponds to columns 1-3 of Table 3; and columns 10-12 corresponds to columns 1-3 of Table 4. Standard errors clustered at the country level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See notes below the said tables for further details.

Table A.4: Formerly planned economies

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy
EU	-0.0390***	-0.0173***	-0.0181	-0.0434***	-0.0218**	-0.0123
	(0.0115)	(0.0066)	(0.0167)	(0.0126)	(0.0087)	(0.0146)
$\log(y_{it-1})$		-0.0012	-0.1850		0.0021	-0.1896
		(0.0016)	(0.1093)		(0.0020)	(0.1210)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	No	Yes	Yes	No	Yes
Observations	642	642	613	497	497	468
R-squared	0.4259	0.4358		0.4371	0.4445	
Number of countries	29	29	29	29	29	29
Estimation method	Fixed effects	Random effects	Difference GMM	Fixed effects	Random effects	Difference GMM
Sample	1991-2015	1991-2015	1991-2015	1991-2009	1991-2009	1991-2009

Notes: Dependent variable is annual growth in real GDP per capita. Columns 1 and 4 estimate equation (1); columns 2 and 5 estimate equation (2) ; and columns 3 and 6 estimate equation (3) . Standard errors clustered at the country level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Conley standard errors

VARIABLES	(1) Δlogy	(2) Δlogy	(3) Δlogy
EU	-0.0179***	-0.0127***	-0.0054*
	(0.0028)	(0.0035)	(0.0030)
Year fixed effects	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes
Observations	8,403	7,463	7.076
R-squared	0.056	0.061	0,053
Number of countries	205	205	204
Sample	Global sample 1961-2015	Global sample 1961-2009	Global sample 1961-2007

Notes: Dependent variable is annual growth in real GDP per capita. *** p<0.01, ** p<0.05, * p<0.1. The table estimates Conley standard errors for columns 1-3 of Table 1. Spatial correlation cutoff is set at 500 km. *** p<0.01, ** p<0.05, * p<0.1. All regressions include country and time fixed effects.

Table A.6: 8-year epochs

VARIABLES	(1) Δlogy	(2) Δlogy	(3) Δlogy	(4) Δlogy
EU	-0.020***	-0.007	-0.005	-0.002
	(0.0068)	(0.0062)	(0.0069)	(0.007)
Time fixed effects	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
Observations	1,151	955	143	122
R-squared	0,039	0,055	0,572	0,328
Number of countries	205	204	21	21
Sample	Global sample 1961-2015	Global sample 1961-2007	OECD-21 1961-2015	OECD-21 1961-2007

Notes: Dependent variable is annual growth in real GDP per capita. Columns 1-4 estimate respectively columns 1 and 3 of Tables 1 and 2 (panel A) using 8-year epoch. Standard errors clustered at the country level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include country and time fixed effects. See notes below the said tables for further information.

Table A.7: Fixed effects estimation on OECD-19 sample (Japan and US excluded)

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator		Pooled OLS			Prais-Winsten	
VARIABLES	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy
			Panel A			
EU	-0.0012	0.0001	-0.0022	-0.0014	0.0006	-0.0024
	(0.0032)	(0.0030)	(0.0028)	(0.0044)	(0.0047)	(0.0044)
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	986	891	853	986	891	853
R-squared	0.157	0.134	0.091	0.104	0.095	0.060
Number of countries	19	19	19	19	19	19
Autocorrelation coefficient				0,270	0,312	0,283
Sample	OECD-19	OECD-19	OECD-19	OECD-19	OECD-19	OECD-19
	1961-2015	1961-2009	1961-2007	1961-2015	1961-2009	1961-2007
Data source	WDI	WDI	WDI	WDI	WDI	WDI
			Panel B			
EU	0.0025	0.0033	0.0016	0.0024	0.0036	0.0015
	(0.0043)	(0.0043)	(0.0043)	(0.0050)	(0.0054)	(0.0053)
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,152	1,114	1,076	1,152	1,114	1,076
R-squared	0.098	0.103	0.079	0.078	0.078	0.058
Number of countries	19	19	19	19	19	19
Autocorrelation coefficient				0,148	0,192	0,191
Sample	OECD-19	OECD-19	OECD-19	OECD-19	OECD-19	OECD-19
	1951-2011	1951-2009	1951-2007	1951-2011	1951-2009	1951-2007
Data source	PWT	PWT	PWT	PWT	PWT	PWT

Notes: Dependent variable is annual growth in real GDP per capita. Standard errors in columns 1-3 are long panel-corrected errors, which allow heteroskedasticity and correlation over i. *** p<0.01, ** p<0.05, * p<0.1. Standard errors in columns 4-6 in addition assume AR(1) for each i. The option *pairwise selection* in Stata's **xtpcse** routine is invoked in order to use all observations that can be matched by period between panels i and j. All regressions include country fixed effects and a linear time trend.

Table S.1: Nearest-neighbour matching (NNM) and OLS regressions

	(1)	(2)	(3)	(4)	(5)
Estimator	NNM			OLS	
VARIABLES	Average annual growth in real GDP per capita 1973-2011				
Treatment effect (ATET)	0,0031	-0.0058	-0.0076		
	(0.0041)	(0.0046)	(0.0066)		
EU				0,0047	0.0006
				(0.0029)	(0.0040)
Real GDP per capita (log)				-0.0005	-0.0189***
				(0.0014)	(0.0043)
Investment share					0.0289***
					(0.0132)
Population growth					0.3729**
					(0.1420)
Agriculture's share of value added					-0.0011***
					(0.0003)
Industry's share of value added					0.00004
					(0.0002)
Secondary school enrolment					0.00005
					(0.0001)
Tertiary school enrolment					0.0004***
					(0.00007)
Observations	132	53	53	132	53
R-squared				0,004	0,720
Matches per treated	1	1	2		
Sample	S1	S2	S2	S1	S2

Notes: Dependent variable is average annual growth in real GDP per capita 1973-2011. Columns 1-3 estimates ATET using nearest-neighbour matching, whereas columns 4 and 5 reports OLS results. In the OLS regressions we do not report the control variables, which are S1 and S2, and S2 in respectively columns 4 and 5. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are robust.

Table S.2: Migration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Diff-in diff				Growth regression			
	Panel A							
VARIABLES	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy	Δlogy
EU	-0.0201***	-0.0195***	-0.0091	-0.0080	-0.0019	-0.0023	0.0048	0.0043
	(0.0057)	(0.0055)	(0.0058)	(0.0052)	(0.0045)	(0.0043)	(0.0037)	(0.0035)
log(y_{it-1})					-0.0019	-0.0012	0.0001	0.0008
					(0.0020)	(0.0014)	(0.0015)	(0.0011)
Migration	0.1116		0.1592		0.0734		0.0805	
	(0.1524)		(0.1771)		(0.1039)		(0.1006)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	No	No	No	No
Observations	2,008	2,008	1,779	1,779	2,008	2,008	1,779	1,779
R-squared	0.086	0.084	0.090	0.085	0.065	0.065	0.077	0.075
Number of countries	231	231	230	230	231	231	230	230
Sample	Global	Global	Global	Global	Global	Global	Global	Global
	1961-2015	1961-2015	1961-2007	1961-2008	1961-2015	1961-2015	1961-2007	1961-2008
	Panel B							
EU	-0.0039	-0.0033	-0.0004	-0.0003	-0.0024	-0.0023	0.0005	0.0005
	(0.0073)	(0.0072)	(0.0066)	(0.0068)	(0.0031)	(0.0030)	(0.0027)	(0.0028)
log(y_{it-1})					-0.0110***	-0.0097***	-0.0116***	-0.0114***
					(0.0030)	(0.0028)	(0.0036)	(0.0032)
Migration	0.1252		0.0074		0.0813		0.0094	
	(0.0975)		(0.1387)		(0.0716)		(0.1294)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	No	No	No	No
Observations	223	223	202	202	223	223	202	202
R-squared	0.432	0.427	0.362	0.362	0.428	0.426	0.375	0.375
Number of countries	21	21	21	21	21	21	21	21
Sample	OECD-21	OECD-21	OECD-21	OECD-21	OECD-21	OECD-21	OECD-21	OECD-21
	1961-2015	1961-2015	1961-2007	1961-2007	1961-2015	1961-2015	1961-2007	1961-2007

Notes : Dependent variable in all columns is quinquennial real GDP per capita growth. Panel A provides estimates on the global sample, whereas panel B only uses the OECD-21 sample. With respect to the OECD-21 sample, we do not use long panel-corrected standard errors. The reason is that the migration variable is only available every five years, for which reason the panel is wide, not long. Cluster robust (at country level) standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Odd columns include migration, whereas even exclude it. Columns 1&2, 3&4, 5&6, 7&8 respectively are estimated on the same sample, the only difference being that even columns in a given pair of columns exclude the migration variable.