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# The long-run relationship between trade and population health: evidence from five decades

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**Abstract.** In recent years, the increase in international trade has sparked a debate about the impact of international trade on population health. To date, however, there has been very little econometric research on the relationship between these two variables. This paper examines the long-run relationship between trade openness and population health for a sample of 74 countries over five decades, from 1960 to 2010. Using panel time-series techniques, it is shown that international trade in general has a robust positive long-run effect on health, as measured by life expectancy and infant mortality. This effect tends to be greater in countries with lower development levels, higher taxes on income, profits, and capital gains, and less restrictive business and labor market regulations. The results also show that long-run causality runs in both directions, suggesting that increased trade is both a consequence and a cause of increased life expectancy.

*Keywords:* international trade; population health; panel cointegration, causality

*JELClassification:* O11; F40; I12

## 1. Introduction

The world has witnessed a dramatic increase in trade over the last fifty years. This increase has sparked a debate, mainly among epidemiologists and political scientists, about the impact of international trade on health. In 2009, *The Lancet*, a leading public health journal, published a series on trade and health, calling for increased attention to the potential adverse consequences of trade for health. In an introductory comment to *The Lancet Series on Trade and Health*, MacDonald and Horton (2009) state: “[T]he fact that trade directly and indirectly affects the health of the global population with an unrivalled reach and depth undoubtedly makes it a key health issue that the global health community can no longer ignore” (p. 273).

However, although the World Health Assembly passed a resolution in 2006 requesting the Director-General of the World Health Organization to work with other international organizations to generate and share evidence on the relationship between trade and health (World Health Assembly, 2006, p. 38), only two published studies have empirically evaluated the effect of total trade on population health. Owen and Wu (2007), using panel data based on 5-year intervals for the period 1960-1995, find that increased trade openness is associated with lower infant mortality rates and higher life expectancies, especially in developing countries. Stevens et al. (2013) confirm this finding based on 5-year data for the period 1970-2005.

Given the small number of time-series observations (every 5 years), the results of these studies are primarily driven by the cross-sectional rather than time-series variation in the data. However, both trade and health are dynamic phenomena that vary considerably across countries and over time. It is therefore an open question whether these results hold when more attention is paid to the time-series dimension of the data, and this is one question the present study attempts to answer.

The objective of this paper is to examine the long-run relationship between trade and health by applying panel time-series methods to annual data from 74 countries over the period 1960-2010. The paper makes three contributions.

The first and main contribution is to estimate the long-run effect of trade on health using panel cointegration estimation methods. These methods are known to be robust (under cointegration) to a variety of estimation problems that often plague empirical work, including omitted variables and endogeneity (as discussed in more detail in Section 3). To preview the main result, I find that trade openness has a statistically and quantitatively significant positive impact on population health. This result is robust to a variety of sensitivity tests, including the use of different cointegration techniques, potential outliers, different samples and sample periods, alternative

definitions of the dependent and independent variables, and the inclusion of additional variables (such as GDP per capita and GDP per capita squared).

Another question this study addresses is whether countries with certain characteristics benefit more, on average, from trade than others. To answer this question, and thus gain insight into possible factors that determine the effect of trade on life expectancy on average across countries, I present panel estimates of the long-run trade-life expectancy coefficients for selected groups of countries. This is the second contribution of this paper. My results suggest that the effect of trade on life expectancy income tends to be greater in countries with lower development levels, higher taxes on income, profits, and capital gains (as a percentage of GDP), and less restrictive business and labor market regulations.

The third contribution of this paper is to address the question of causality: is trade an exogenous determinant of life expectancy or is increased trade both a cause and a consequence of increased life expectancy? There are good theoretical reasons to assume that there is a bidirectional relationship between trade and health (as discussed in Section 2), and this is what I find: long-run Granger causality runs in both directions, suggesting that, over time, an increase in trade raises life expectancy and that, in turn, higher life expectancy increases trade.

The paper is organized as follows. In Section 2, I discuss hypotheses concerning the effects of trade on health and vice versa. Section 3 sets out the basic empirical model and describes the data. The econometric implementation and the estimation results are presented in Section 4, and Section 5 concludes.

## **2. Hypotheses about the effects of trade on health and vice versa**

### **2.1. The effect of trade on health**

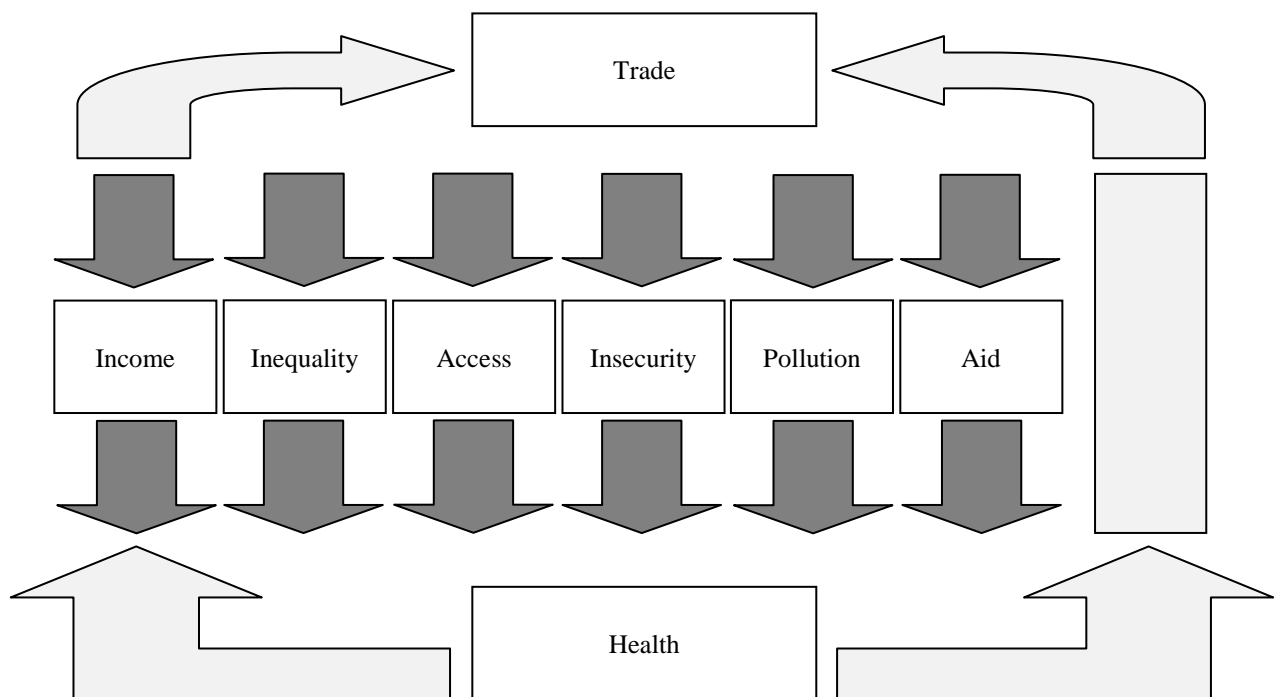
Epidemiologists, political scientists, and economists have proposed several mechanisms by which increased trade could result in a change in health status over time (see also Dollar, 2001; Blouin et al., 2009). In the following, I group these mechanisms under six headings, as depicted in Figure 1.

#### **2.1.1. The income mechanism**

If an increase in income leads to an increase in expenditures, both public and private, on goods that improve health (such as food, clean water and sanitation, education, and medical care), the effect of trade on population health through changes in the level of income will be positive unless trade has no effect on income or the effect of trade on income is negative.

According to the textbook model of international trade, with perfect factor mobility, perfect competition, and no market failures (such as external effects or increasing returns), trade has a positive effect on real income. It is well known, however, that if these standard assumptions do not hold, the theoretical effect of trade on real income is not necessarily positive, which is also supported by empirical evidence. Borrmann et al. (2006) and Freund and Bolaky (2008) find that trade does not increase income in economies with excessive business and labor regulations. According to the results of Herzer (2013), trade raises income in most countries, but in some countries, particularly in the developing world, increased trade is associated with lower levels of income. Herzer also finds that many developing economies are highly regulated and speculates that this “may, at least partly, explain why [...] some developing countries lose from trade” (p. 209).

Figure 1. Effects of trade on health and vice versa



Another qualification is the observation that increases in income are strongly associated with increases in life expectancy among poorer countries, but as income rises, the relationship flattens out, and is weaker or even absent among the richest countries (Deaton, 2003, 2004). If health gains from an extra unit of income diminish as income increases, as the concave cross-country relationship between life expectancy and income suggests (see also Preston, 1975), it may well be that health is affected by the standard of living mainly in developing countries, while an increase in income has no or little effect on health in high-income countries. If higher income is associated with longer working hours and consequently less time for social interactions, more stress, less sleep,

increased consumption of health-damaging products (such as alcohol and tobacco), and increased consumption of unhealthy foods (such as burgers, French fries, sandwiches, and pizza), it could even be that the income-health relationship becomes negative at high levels of income.

The implication of this discussion is that the long-run effect of trade on health that operates through the level of income is theoretically ambiguous and depends on factors that affect both the trade-income relationship (such as perhaps excessive business and labor regulations) and the income-health relationship (such as perhaps the level of development).

### **2.1.2. The inequality mechanism**

Textbook analysis tells us that trade increases the real return to the factor that is relatively abundant in each country and reduces the real return to the scarce factor—known as the Stolper-Samuelson theorem. In countries that are abundant in skilled labor, real wages of less-skilled workers should thus decline, both relative to more-skilled workers and in absolute terms. In countries that are well-endowed with unskilled labor, real wages of more-skilled workers should decline, both relative to less-skilled workers and in absolute terms. The implication is that inequality should rise with trade in developed countries, while in developing countries an increase in trade should have an equalizing effect on income distribution. In contrast to the standard textbook analysis, newer theories, which focus on heterogeneous firms and bargaining, labor market frictions, and incomplete contracts, suggest several mechanisms by which increased trade may increase inequality in both developed and developing countries.<sup>1</sup>

Existing empirical studies, however, do not provide clear evidence that trade increases inequality. Dollar and Kraay (2002), for example, find in a sample of developed and developing countries that trade openness is not significantly associated with inequality, while the results of Dutt and Mukhopadhyay (2005) suggest that trade causes inequality worldwide. Calderon and Chong (2001) find that greater openness reduces inequality in developing countries; the results of Barro (2000) and Ravallion (2001) suggest that trade raises inequality in low-income countries and reduces inequality in high-income countries; and Herzer and Vollmer (2013) find in a panel of high-income countries that trade increases inequality.

It is also not clear what impact inequality has on health. If the health function has diminishing marginal returns to income, a mean-preserving transfer from a richer person to a poorer person raises the health of the poorer person more than it lowers the health of the richer person, implying that a more equal distribution of income should improve population health; this is often

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<sup>1</sup> A discussion of these mechanisms is beyond the scope of this paper; a review of the recent literature can be found in Harrison et al. (2010).

referred to as the absolute income hypothesis (Leigh et al., 2007). Given, however, that an increasing share of income is often spent on health care and healthy foods (such as lean meat, fish, vegetables, and fruits) as income rises, the health function does not necessarily have diminishing returns, as discussed in Herzer and Nunnenkamp (2011).

Another prominent hypothesis is the so-called relative income hypothesis according to which individuals assess their well-being by comparing themselves to others with more income than themselves, so that that income inequality leads to ill health via “stressful social comparisons” (Kawachi and Kennedy, 1999). However, as argued, for example, by Leigh et al. (2007), people typically belong to various reference groups and tend to reduce stress by deriving self-esteem from the reference group where their ranking is highest.

Further arguments for and against the hypothesis that health is negatively linked to inequality are discussed by Leigh et al. (2007) and Herzer and Nunnenkamp (2011). While the former find no statistically significant relationship between inequality and health in their fixed-effects regressions, the latter find that income inequality has a small but positive impact on population health, but most studies document a negative relationship between the two variables (see, for example, Zheng, 2012). Kondo et al. (2010: 8) conclude from a meta-analysis of published self-rated studies that “that there is an association between higher income inequality and worse health outcomes.”

### **2.1.3. The access mechanism**

Trade gives domestic consumers and producers access to a wider range of goods and services at lower cost than under autarky, which can have both negative and positive effects on health. An obvious negative consequence is the international spread of infectious diseases through trade in infected animals and products (Timoney, 2000; Deaton, 2004; Huynen et al., 2005). Another obvious threat to health is trade in (and consumption of) health-damaging products, such as alcohol, tobacco, and unhealthy foods (Dollar, 2001; Blouin et al., 2009; Labonté et al., 2011).

A positive consequence is that countries, particularly developing countries, gain access to medical goods such as pharmaceuticals (e.g. vaccines and antibiotics) or medical equipment (e.g. surgical instruments) (Owen and Wu, 2007), that is, R&D-intensive goods. Papageorgiou et al. (2007: 411) argue that, “similar to capital goods in manufacturing, medical goods [...] embody R&D-induced technology.” Thus, another positive implication for health is that trade in such goods may result in international spillovers of medical knowledge (see also Deaton, 2004; Owen and Wu, 2007, Cornia et al., 2008). Papageorgiou et al. (2007) find that medical imports from countries that are major exporters of medical technology are positively correlated with the health status in

countries that do not perform medical R&D. They conclude that medical knowledge embodied in medical imports contributes to significant improvements life expectancy.

#### **2.1.4. The insecurity mechanism**

Several studies document that economic insecurity has detrimental effects on health (see, for example, Ferrie et al., 1995, 2002; Rugulies et al., 2008). The implication is that international trade will negatively affect population health if trade increases economic insecurity (holding all else constant).

In an influential study, Rodrik (1998) argues that countries that trade more, *ceteris paribus*, are subject to greater volatility, thus leading to greater income and employment insecurity. He finds that external risk from trade, measured by the interaction between trade openness and the standard deviation of a country's terms of trade, is positively correlated with income volatility. However, this finding is not supported by the work of Iversen and Cusack (2000), who present evidence that, at least for developed economies, there is no relationship between trade openness and volatility in output, earnings, or employment.

But even if the hypothesis that that trade increases economic insecurity is correct, it must be qualified in that increased insecurity may lead to increased demand for social insurance that compensates workers for the higher income and employment risk. Based on this consideration, Rodrik (1998) argues that government spending provides social insurance against external risk, and finds that trade openness has a statistically significant positive association with government spending. Ram (2009), for example, supports this finding, while Benarroch and Pandey (2008) find no support for the positive causal relationship between trade openness and government spending. To my knowledge, there are no published studies showing that trade openness decreases government spending (although a decline in government revenues from tariffs associated with increased trade openness could mean less money for public health care).

#### **2.1.5. The pollution mechanism**

There is an enormous body of research on the impact of environmental pollution on health. According to Briggs (2003), about 8-9% of the total disease burden worldwide may be attributed to environmental pollution.

As far as the role of trade is concerned, the so-called pollution haven hypothesis predicts that developing countries, which tend to have weak environmental standards relative to developed countries, will specialize in dirty production (and thus become pollution havens), while developed



countries will specialize in clean production. As a result, pollution in developing countries will rise with trade, and pollution in developed countries will fall with trade.

An alternative hypothesis is the so-called factor endowments hypothesis according to which trade expands the polluting, capital-intensive industry in countries that are well endowed with physical capital, implying that pollution rises in developed countries, while pollution falls in capital-scarce developing countries (Copeland and Taylor, 2004).

Finally, it is argued that, on the one hand, free trade involves an increase in pollution associated with the transportation of goods (Cristea et al., 2013), and that, on the other hand, free trade increases access to environmentally-friendly technologies (Lovely and Popp, 2008).

Recent empirical work by Antweiler et al. (2001) and Frankel and Rose (2005) suggests that the overall impact of trade on the environment tends to be positive. Begun and Eicher (2008), however, find that trade reduces pollution only in developed countries, while trade increases pollution in developing countries.

#### **2.1.6. The aid mechanism**

Owen and Wu (2007) argue and find evidence that more open economies receive more development aid, and that foreign aid, particularly for water supply and sanitation, is associated with lower infant mortality and higher life expectancy. However, other evidence does not support this. The results of Nowak-Lehmann D. et al. (2009), for example, suggest that aid leads to trade and not vice versa, and Wilson (2011) finds that aid, including aid for water projects and aid for health programs, has no effect on mortality.

The conclusion from the discussion above is that trade can have both positive and negative effects on health. The net long-run effect is an empirical matter and may depend on several factors. One of these factors could be the level of development, so that the effects vary between developed and developing countries. Another possible determining factor could be the level of redistribution through taxes and transfers. In particular, taxes on income, profits, and capital gains (which are typically progressive) may mitigate negative health effects of income inequality and insecurity by reducing income differences between the winners and losers from trade and by financing public health services and social security benefits. Finally, it can be hypothesized that the effect of trade on health differs between less regulated countries and more regulated countries.

#### **2.2. The effect of health on trade**

I now present some ideas on the effects of health on trade. These ideas are grouped under two headings.

### **2.2.1. Direct effects**

The direct effects are obvious. First, countries tend to import less pharmaceuticals when health is good (Papageorgiou et al., 2007; Owen and Wu, 2007), which implies a negative effect of health on trade. Second, it is well known that only the most productive firms export (while the less productive firms serve only the domestic market) and it is also well known that better health is associated with productivity gains, implying that better health may lead to higher exports and, as a consequence, to more trade—a positive effect of health on trade.

### **2.2.2. Indirect effects through income**

As argued by Frankel and Romer (1999), it is reasonable to assume that countries with higher income levels have better infrastructure and transportation systems which allow them to facilitate trade. Moreover, high-income countries generally have the institutions and resources needed to tax domestic economic activity, and thus need not rely on tariffs to finance government spending. In addition, high-income countries tend to demand a greater variety of products that are traded internationally. And finally, as noted by Herzer (2013), high-income countries typically offer more opportunities for companies to acquire the knowledge and resources necessary to enter export markets. Thus, there are good reasons to assume that the volume of trade increases as income increases.

Health can affect income growth through a number of channels. Howitt (2005), for example, provides a theoretical framework for incorporating health improvements into an innovation-based growth model, and shows that health has multiple effects on growth through factors such as productive efficiency, school attendance, learning efficiency in school, and research efficiency. Zhang and Zhang (2005) develop an overlapping-generations model that shows that rising longevity reduces fertility, raises saving, and increases schooling duration.

Most empirical studies find, using life expectancy as an indicator of health, that life expectancy has a positive effect on growth (see, for example, Aghion et al., 2011; Cervellati and Sunde, 2011; Bloom et al., 2014). An exception is the study by Acemoglu and Johnson (2007), who conclude from their results that “the increases in life expectancy [...] appear to have reduced income per capita” (p. 975). The rationale for this result is that while health improvements may increase human capital and worker productivity, lower mortality may increase population size and thereby reduce other factor inputs on a per capita basis.

In summary, it is reasonable to hypothesize that causality runs in both directions: not only from trade to health but also from health to trade. However, the expected sign of the causal effects is not clear a priori.

### 3. Model and data

The analysis will examine the long-run relationship between trade and population health using panel cointegration and causality techniques. This section first presents the basic empirical model and discusses some issues related to the estimation of nonstationary panel models (Subsection 3.1). The section then describes the data (Subsection 3.2) and reports unit root and cointegration test results (Subsection 3.3).

#### 3.1. Basic empirical model and econometric issues

Following common practice in (panel) cointegration studies (see, for example, Pedroni, 2007; Herzer, 2008; Moscone and Tosetti, 2010), I consider a parsimonious model which includes only the two variables of empirical interest: trade and health. Thus, the basic model takes the form

$$\log(LE_{it}) = a_i + \beta \log(T_{it}) + \varepsilon_{it}, \quad (1)$$

where  $i=1, 2, \dots, N$  is the country index,  $t=1, 2, \dots, T$  is the time index, and the  $a_i$  are country-specific fixed effects.  $LE_{it}$  is the most commonly used summary measure of health status—life expectancy at birth (Henderson, 2009), and  $T_{it}$  represents the standard measure of trade openness—the (percentage) ratio of trade (imports plus exports) in current prices to GDP in current prices. In the robustness section, I use the infant mortality rate as an alternative measure of population health and the ratio of nominal trade to purchasing power parity (PPP) adjusted GDP as an alternative measure of trade openness. The latter is called by Alcalá and Ciccone (2004) “real openness”. Both variables are log-transformed, implying that equation (1) incorporates a relationship in growth rates. To see this, differentiate equation (1) to obtain the growth rate of life expectancy  $dLE_{it}/LE_{it}$  as a function of the growth rate of the trade/GDP ratio  $dT_{it}/T_{it}$ . In addition, the log-transformation implies that the coefficient  $\beta$  can be interpreted as the long-run elasticity of life expectancy at birth with respect to trade openness, measuring the percentage change in life expectancy due to a 1 percent change in the trade/GDP ratio. As the principal interest is on the long-run elasticity, it is not essential to be concerned about the variable lags through which trade will impact life expectancy.

I now turn to econometric issues. The first issue is that the underlying variables are trending over time—that is, they are nonstationary (as shown in Figures 2 and 3). Given that most economic time series are characterized by a stochastic rather than deterministic nonstationarity, it is plausible to assume that the trends in  $\log(LE_{it})$  and  $\log(T_{it})$  are also stochastic (through the presence of a unit root) rather than deterministic (through the presence of polynomial time trends). If this assumption

is correct, the linear combination of these stochastically trending (or integrated) variables must be stationary, or, in the terminology of Engle and Granger (1987),  $\log(LE_{it})$  and  $\log(T_{it})$  must be cointegrated. If the two variables are not cointegrated, then there is no long-run relationship between life expectancy and trade openness, and equation (1) would represent a spurious regression in the sense of Granger and Newbold (1974). Standard regression output must therefore be treated with extreme caution when variables are nonstationary, since the estimated results are potentially spurious (see also Eberhardt and Teal, 2013). As shown by Entorf (1997) and Kao (1999), the tendency for spuriously indicating a relationship may even be stronger in panel data regressions than in pure time-series regressions. Thus, the necessary condition for the existence of a non-spurious long-run relationship between  $\log(LE_{it})$  and  $\log(T_{it})$  is that the two integrated variables cointegrate.<sup>2</sup>

A regression consisting of nonstationary cointegrated variables has the property of superconsistency such that coefficient estimates converge to the true parameter values at a faster rate than they do in standard regressions with stationary variables, namely rate  $T$  rather than  $\sqrt{T}$  (Stock, 1987). The important point in this context is that the estimated cointegration coefficients are superconsistent even in the presence of temporal and/or contemporaneous correlation between the stationary error term,  $\varepsilon_{it}$ , and the regressor(s) (Stock, 1987), implying that cointegration estimates are not biased by omitted *stationary* variables (see also Bonham and Cohen, 2001).

The fact that a regression consisting of cointegrated variables has a stationary error term also implies that no relevant *nonstationary* variables are omitted. Any omitted nonstationary variable that is part of the cointegrating relationship would become part of the error term, thereby producing nonstationary residuals, and thus leading to a failure to detect cointegration (see, for example, Everaert, 2011).

If there is cointegration between a set of variables, then this stationary relationship also exists in extended variable space. In other words, the cointegration property is invariant to model extensions (see also Lütkepohl, 2007), which is in stark contrast to regression analysis where one new variable can alter the existing estimates dramatically (Juselius, 2006, p. 11). The important implication of finding cointegration is thus that no additional variables are required to account for the classical omitted variables problem; the result for the long-run relationship between life expectancy and trade would also hold if additional variables were included in the model (see also Juselius, 1996).

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<sup>2</sup> The standard time-series approach is to first-difference the variables to remove the nonstationarity in the data and to avoid spurious results. However, this approach precludes the possibility of a long-run or cointegrating relationship in

Of course, there are several other factors that may affect life expectancy and/or trade openness. Therefore, adding further non-stationary variables to the model may, on the one hand, result in further cointegrating relationships. If, however, there is more than one cointegrating relationship, identifying restrictions are required to separate the cointegrating relationships. Otherwise, multicollinearity problems may arise. On the other hand, adding further nonstationary variables to the regression model may result in spurious associations. More specifically, if a nonstationary variable that is not cointegrated with the other variables is added to the cointegrating regression, the error term will no longer be stationary. As a result, the coefficient of the added variable will not converge to zero, as one would expect of an irrelevant variable in a standard regression (Davidson, 1998). Although these considerations justify a parsimonious model such as equation (1), I nevertheless check the robustness of the results to the inclusion of additional variables, such as GDP per capita and GDP per capita squared (in logs). This allows me to test the hypothesis that health gains from an extra unit of income diminish as income rises.

The superconsistency of the cointegration estimation also implies that the potential endogeneity of the regressors should not affect the estimated long-run coefficients; the estimated long-run coefficients from reverse regressions should be approximately the inverse of each other due to the superconsistency (Engle and Granger, 1987). Nevertheless, there are two problems:

First, although the standard least-squares dummy variable estimator is superconsistent under panel cointegration, it suffers from a second-order asymptotic bias arising from serial correlation and endogeneity in finite samples. As a consequence, its  $t$ -ratio is not asymptotically standard normal. To deal with this problem, one has to employ an asymptotically efficient (cointegration) estimator. Examples of such estimators include panel versions of the dynamic OLS (DOLS) and fully modified ordinary least squares (FMOLS) methods. As shown by Wagner and Hlouskova (2010), the panel DOLS estimator of Kao and Chiang (2000) outperforms other asymptotically efficient estimators. Therefore, this estimator is preferred here, but in the robustness section I also present results based on alternative estimation procedures.

Second, although the existence of cointegration implies long-run Granger causality in at least one direction (Granger, 1988), cointegration says nothing about the direction of the causal relationship between the variables. A statistically significant cointegrating relationship between  $\log(LE_{it})$  and  $\log(T_{it})$  does therefore not necessarily imply that, in the long run, changes in trade openness cause changes in life expectancy. The causality may run in the opposite direction, from  $\log(LE_{it})$  to  $\log(T_{it})$ , or in both directions. The empirical implication is that it is important not only

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the data and leads to misspecification if a long-run relationship between the levels of the variables exists (see, e.g., Granger, 1988).

to employ an asymptotically efficient cointegration estimator (to account for the potential endogeneity of trade), but also to explicitly test the direction of long-run causality. As is common practice in testing long-run Granger causality between cointegrated variables, I use a vector error correction model (VECM) to identify cause and effect in the sense of Granger (1988).

### 3.2. Data

I now describe the data and present unit root and cointegration tests. The data on life expectancy at birth are from the World Development Indicators (WDI) 2013 online database.<sup>3</sup> Life expectancy at birth indicates the number of years a newborn infant would live if prevailing patterns of mortality at the time of its birth were to stay the same throughout its life. Life expectancy is the most widely used indicator of health status and has also several advantages over other measures of health, including the following: (i) it depends on both infant mortality and other mortality rates, thus incorporating mortality rates at all stages in life; (ii) it is not biased by age structure; and (iii) data on life expectancy at birth are available for a reasonably large number of countries and time periods.<sup>4</sup> I therefore employ life expectancy as the preferred measure of population health. The infant mortality rate (per 1,000 live births), also from the WDI 2013 online database, is used as a robustness check (as noted above).

The data on trade openness, measured as nominal trade as a percentage of nominal GDP, are also from the WDI 2013 online database. I use the WDI database because it covers a larger set of countries than the Penn World Tables (PWT). Openness data from the PWT are employed in the robustness analysis. More specifically, I construct the so called “real openness” by multiplying price level of GDP (GDP in exchange rate US\$ relative to GDP in PPP US\$) from the Heston, Summers, and Aten (2012) Penn World Table Version 7.1 by nominal openness, also from the Penn World Table Version 7.1.<sup>5</sup> While Alcalá and Ciccone (2004) argue that *nominal* openness yields biased estimates of the effect of trade on income, Rodrik et al. (2004) argue that the use of *real* openness leads to biased estimates of the relationship between trade and income. Since the objective of this study is to examine the relationship between trade and health, rather than the relationship

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<sup>3</sup> Available at: <http://data.worldbank.org/data-catalog/world-development-indicators>.

<sup>4</sup> The use of life expectancy as an indicator of health can be criticized on the grounds that a longer life expectancy does not necessarily translate into better health. While this is theoretically correct, I find that life expectancy at birth and healthy life expectancy at birth, i.e., life expectancy at birth adjusted for morbidity and time spent in poor health, are highly correlated. According to data from the Institute for Health Metrics and Evaluation (available at: <http://ghdx.healthmetricsandevaluation.org/global-burden-disease-study-2010-gbd-2010-data-downloads>), the cross-country correlation coefficient between life expectancy at birth and healthy life expectancy at birth for women (men) in 1990 is 0.994 (0.994), and 0.990 (0.999) in 2000. Therefore, I do not believe that the results would change significantly if healthy life expectancy were used in place of life expectancy. Unfortunately, adequate data on healthy life expectancy are not available to conduct a meaningful panel data analysis.

<sup>5</sup> Available at: [https://pwt.sas.upenn.edu/php\\_site/pwt71/pwt71\\_form.php](https://pwt.sas.upenn.edu/php_site/pwt71/pwt71_form.php).

between trade and income, my preferred measure is the conventional (nominal) openness measure, that is, the ratio of trade to GDP at current prices. Real openness, that is, trade relative to GDP at PPP, is used in the robustness section.

In the robustness section, I also use GDP per capita (in constant 2005 US dollars) and GDP per capita squared (in logs) as additional explanatory variables (as noted above). These data are also from the WDI 2013 online database.

Given that the WDI data span a long period (1960-2012), it is inappropriate to use conventional small  $T$  panel data models that ignore the potential nonstationarity of the variables (see also Phillips and Moon, 2000). The appropriate approach is to use panel time-series techniques to account for the time-series properties of the variables and to avoid spurious results by testing for cointegration. Cointegration estimates are not only robust to omitted variables and endogenous regressors (as discussed above) but also robust to non-systematic measurement errors (Stock, 1987). The latter is an important advantage for applications such as the present one, because life expectancy is estimated, and thus subject to measurement error.

A potential disadvantage, however, is that the estimators I use are designed for balanced panels, while the underlying data set is unbalanced in the sense that the number of time-series observations per country varies. To construct a balanced panel, which entails a trade-off between the time span and number of countries in the sample, I select all countries for which complete time-series data are available over the period 1960-2010—the longest time period with complete data for a reasonably large number of countries. This yields a sample of 74 countries and 51 time-series observations per country (3774 total observations). In the robustness section, I estimate the long-run relationship between  $\log(LE_{it})$  and  $\log(T_{it})$  for a larger sample of countries (101) over a shorter period of time (1970-2000).

Table 1 lists the 74 countries in the main sample along with the average values for  $\log(LE_{it})$  and  $\log(T_{it})$  over the period 1960-2010. Iceland had the highest average life expectancy, followed by Sweden, Japan, and Norway; the country with lowest average life expectancy was Niger, followed by Rwanda, Malawi, and the Central African Republic. The average degree of trade openness was highest in Hong Kong, Luxembourg, and Puerto Rico. The countries with the lowest degree of trade openness were (in descending order) Brazil, the United States, and India.

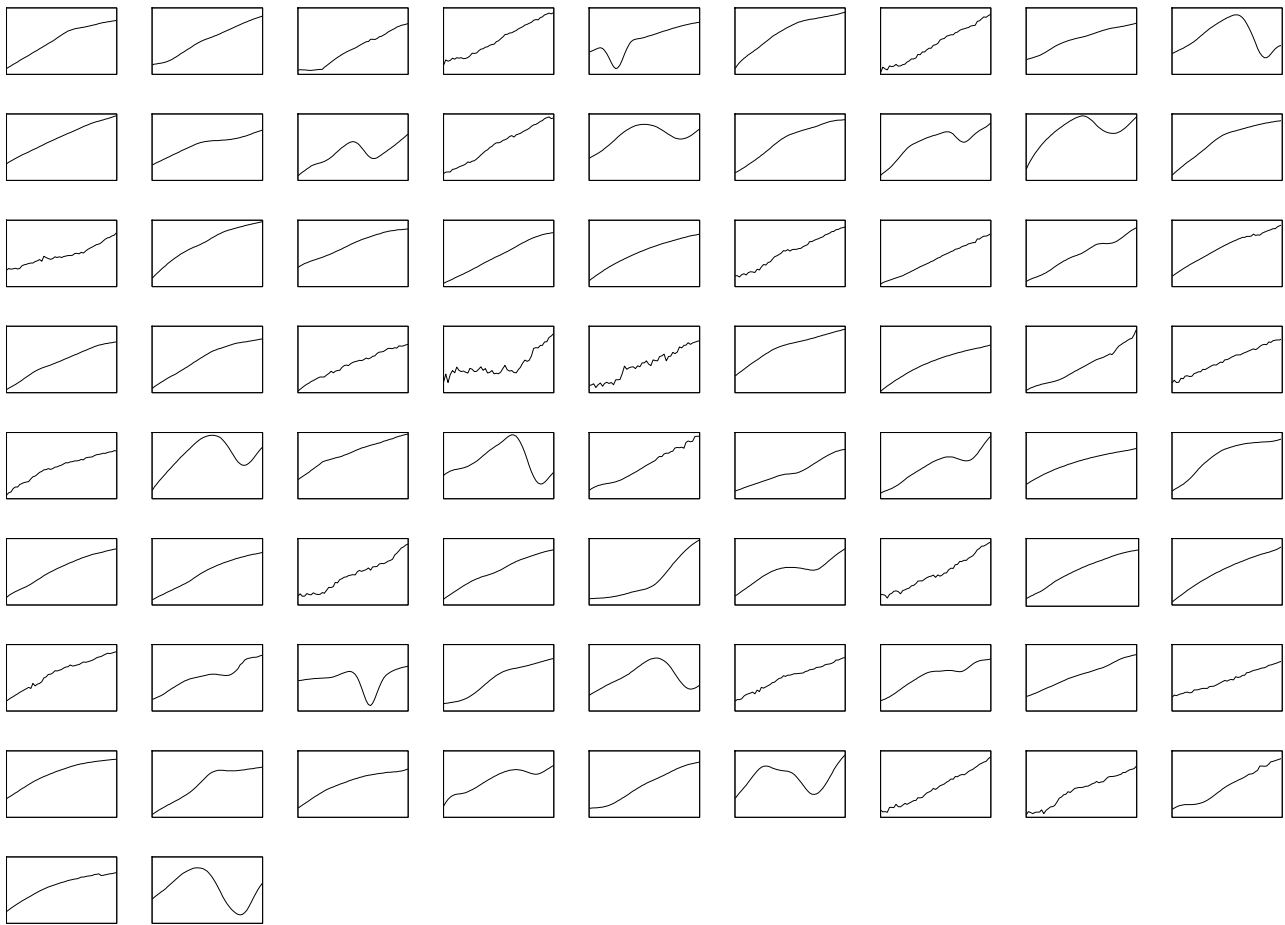
Figures 2 and 3 plot the variables for the period 1960-2010. As can be seen, life expectancy has increased in most countries at most times. Given, however, that life expectancy grew at different rates across countries, many countries, particularly in sub-Saharan Africa, are still characterized by low life expectancy, while other countries, particularly developed countries, have relatively high life expectancy (see Table 1).

Table 1. Countries and summary statistics

	Average of $\log(LE_{it})$	Average of $\log(T_{it})$		Average of $\log(LE_{it})$	Average of $\log(T_{it})$
Algeria	4.12	4.03	Kenya	4.00	4.09
Argentina	4.25	2.89	Korea, Rep.	4.22	3.99
Australia	4.32	3.47	Lesotho	3.93	4.81
Austria	4.31	4.22	Luxembourg	4.30	5.28
Bangladesh	4.01	3.18	Madagascar	3.92	3.77
Barbados	4.28	4.77	Malawi	3.80	4.12
Belgium	4.31	4.76	Malaysia	4.22	4.80
Benin	3.83	3.74	Mauritania	3.96	4.48
Botswana	4.03	4.55	Mexico	4.22	3.43
Brazil	4.16	2.87	Morocco	4.09	3.95
Burkina Faso	3.83	3.44	Netherlands	4.33	4.67
Burundi	3.82	3.42	Nicaragua	4.11	4.09
Canada	4.33	3.98	Niger	3.75	3.66
Central African Republic	3.80	3.90	Nigeria	3.80	3.83
Chile	4.24	3.83	Norway	4.34	4.29
Congo, Dem. Rep.	3.81	3.77	Peru	4.12	3.58
Congo, Rep.	4.00	4.65	Philippines	4.15	4.04
Costa Rica	4.28	4.26	Portugal	4.27	4.00
Denmark	4.32	4.26	Puerto Rico	4.30	4.84
Dominican Republic	4.16	4.05	Rwanda	3.77	3.41
Ecuador	4.18	3.65	Senegal	3.89	4.06
Egypt	4.07	3.87	South Africa	4.01	3.95
Fiji	4.15	4.64	Spain	4.33	3.56
Finland	4.30	4.03	Sri Lanka	4.21	4.16
France	4.32	3.70	Sudan	3.93	3.27
Ghana	3.99	3.79	Sweden	4.34	4.13
Greece	4.32	3.76	Syria	4.20	3.96
Guatemala	4.08	3.75	Thailand	4.20	4.14
Honduras	4.11	4.32	Togo	3.91	4.42
Hong Kong	4.33	5.40	Trinidad and Tobago	4.21	4.50
Hungary	4.25	4.42	Turkey	4.08	3.17
Iceland	4.35	4.31	Uganda	3.88	3.57
India	4.01	2.78	United Kingdom	4.32	3.92
Indonesia	4.07	3.75	United States	4.30	2.86
Ireland	4.30	4.68	Uruguay	4.27	3.62
Italy	4.32	3.68	Venezuela, RB	4.22	3.85
Japan	4.34	3.07	Zambia	3.86	4.35



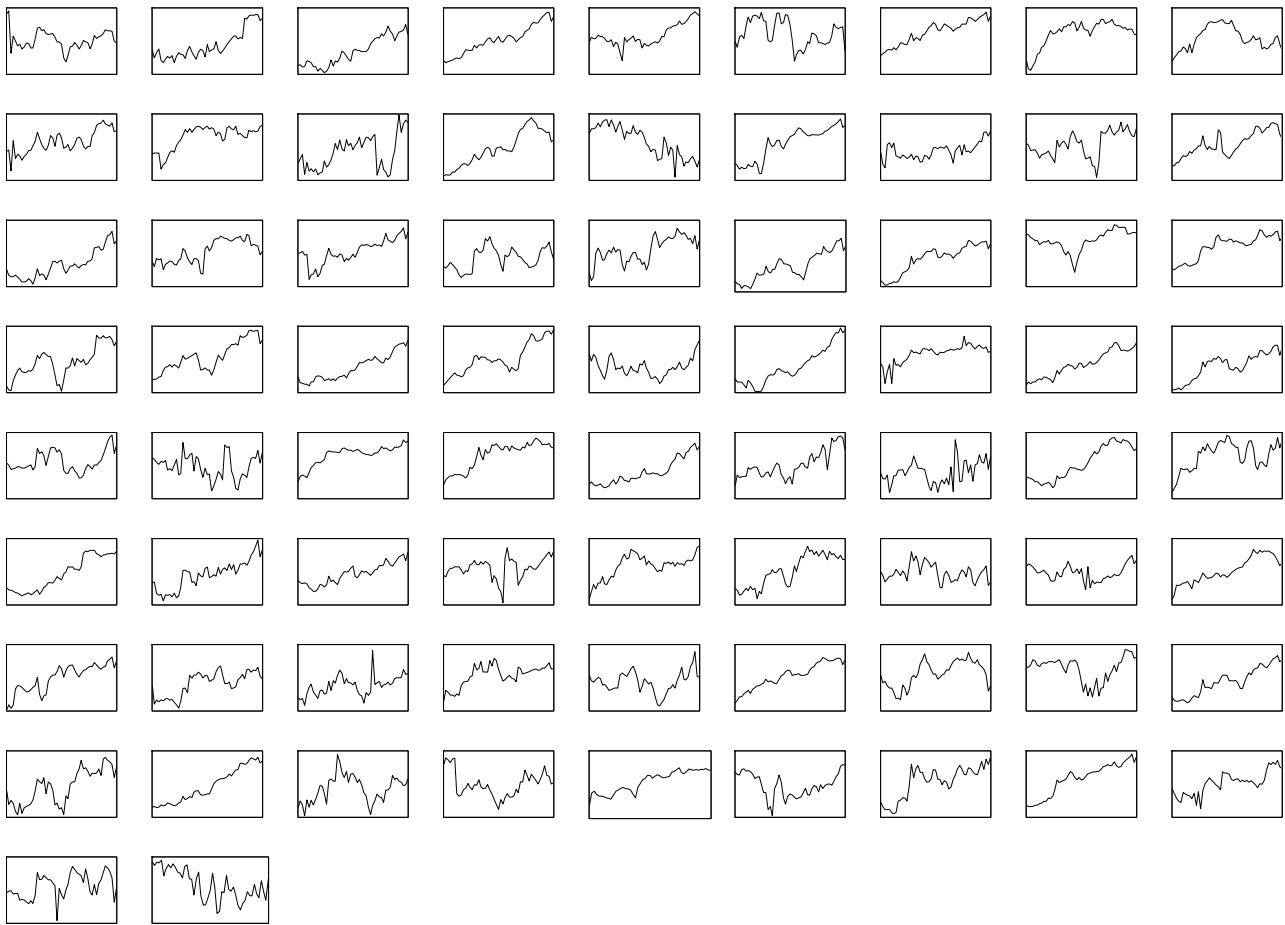
Figure 2. Log life expectancy over the period 1960-2010,  $\log(LE_{it})$



*Notes:* The countries from left to right are: Algeria, Argentina, Australia, Austria, Bangladesh, Barbados, Belgium, Benin, Botswana, Brazil, Burkina Faso, Burundi, Canada, Central African Republic, Chile, Dem. Rep. of Congo, Rep. of Congo, Costa Rica, Denmark, Dominican Republic, Ecuador, Egypt, Fiji, Finland, France, Ghana, Greece, Guatemala, Honduras, Hong Kong, Hungary, Iceland, India, Indonesia, Ireland, Italy, Japan, Kenya, Republic of Korea, Lesotho, Luxembourg, Madagascar, Malawi, Malaysia, Mauritania, Mexico, Morocco, Netherlands, Nicaragua, Niger, Nigeria, Norway, Peru, Philippines, Portugal, Puerto Rico, Rwanda, Senegal, South Africa, Spain, Sri Lanka, Sudan, Sweden, Syrian Arab Republic, Thailand, Togo, Trinidad and Tobago, Turkey, Uganda, United Kingdom, United States, Uruguay, Venezuela, and Zambia.

As with life expectancy, trends in trade openness are far from uniform across countries. While a clear positive trend in trade openness is evident for many countries over the last five decades, particularly in the developed world, growth in trade relative to GDP was less stable in other countries; in some countries, particularly in the developing world, the share of trade in GDP even declined during this period. Overall, the time-series evolution is consistent with the possibility that  $\log(LE_{it})$  and  $\log(T_{it})$  are integrated and cointegrated. I address these issues in the next subsection.

Figure 3. Log trade over the period 1960-2010,  $\log(T_{it})$



*Notes:* The countries from left to right are: Algeria, Argentina, Australia, Austria, Bangladesh, Barbados, Belgium, Benin, Botswana, Brazil, Burkina Faso, Burundi, Canada, Central African Republic, Chile, Dem. Rep. of Congo, Rep. of Congo, Costa Rica, Denmark, Dominican Republic, Ecuador, Egypt, Fiji, Finland, France, Ghana, Greece, Guatemala, Honduras, Hong Kong, Hungary, Iceland, India, Indonesia, Ireland, Italy, Japan, Kenya, Republic of Korea, Lesotho, Luxembourg, Madagascar, Malawi, Malaysia, Mauritania, Mexico, Morocco, Netherlands, Nicaragua, Niger, Nigeria, Norway, Peru, Philippines, Portugal, Puerto Rico, Rwanda, Senegal, South Africa, Spain, Sri Lanka, Sudan, Sweden, Syrian Arab Republic, Thailand, Togo, Trinidad and Tobago, Turkey, Uganda, United Kingdom, United States, Uruguay, Venezuela, and Zambia.

### 3.3. Panel unit root and cointegration tests

In recent years, a number of panel unit root tests have been developed. The most commonly used are the so-called first-generation panel unit root tests. These, however, can exhibit severe size distortions in the presence of cross-sectional dependence due to common shocks or spillovers among countries at the same time. Therefore, I employ a second-generation panel unit root test to account for potential cross-sectional dependence. More specifically, I use the panel unit root test developed by Pesaran (2007). This test is an Augmented-Dickey-Fuller (ADF) type test and is based on an average of the individual country ADF  $t$ -statistics. The idea of the test is to filter out the cross-sectional dependence by augmenting the individual country ADF regressions with the cross-

sectional averages of lagged levels and first differences of the individual series as proxies for the unobserved common factors.

Table 2 reports the results of the test for the variables in levels and in first differences. The test statistics do not reject the null hypothesis that  $\log(LE_{it})$  and  $\log(T_{it})$  have a unit root in levels, whereas the unit root hypothesis is rejected for the first differences. It can therefore be concluded that both  $\log(LE_{it})$  and  $\log(T_{it})$  are nonstationary, integrated of order 1 series.

Table 2. Pesaran (2007) panel unit root tests

Variable	Levels ( <i>c, t</i> )	First differences ( <i>c</i> )
$\log(LE_{it})$	-2.424	-2.353***
$\log(T_{it})$	-2.481	-3.428***

Notes: *c* (*t*) indicates that I allow for different intercepts (and time trends) for each country. Three lags were used to adjust for autocorrelation. Large negative values lead to rejection of a unit root in favor of (trend) stationarity. The relevant 5% (1%) critical value is -2.581 (-2.679) with an intercept and a linear trend, and -2.089 (-2.193) with an intercept. The critical values are calculated from the response-surface estimates in Otero and Smith (2013). \*\*\* indicate rejection of the null hypothesis of a unit root at the 1% level.

In order to ensure that the relationship between  $\log(LE_{it})$  and  $\log(T_{it})$  is not spurious, I test for cointegration using the second-generation cointegration test recently proposed by Banerjee and Carrion-i-Silvestre (2011). This test is a residual-based cointegration test which involves four steps. The first is to estimate the coefficient  $\beta$  of the cointegrating equation (1) using the pooled common correlated effects (CCE) estimation technique advanced by Pesaran (2006). The pooled CCE estimator allows for cross-sectional dependence through common factors by augmenting the cointegrating regression with the cross-sectional averages of the dependent and independent variables as proxies for the unobserved common factors; these averages are interacted with country-dummies to allow for country-specific parameters. In the second step, the estimated long-run coefficient is used to construct the residuals from the long-run relationship,  $\hat{\mu} = \log(LE_{it}) - \hat{\beta} \log(T_{it})$ . Then, these long-run residual series are regressed on country dummies to compute the OLS residuals from this regression as  $\hat{e}_{it} = \hat{\mu} - \hat{D}_i$ . Finally, the Pesaran (2007) unit root test is computed for the estimated OLS residuals. If the presence of a unit root in  $\hat{e}_{it}$  can be rejected, it can be concluded that there is a cointegrating relationship between the variables. Given that residual-based cointegration tests are generally not invariant to the normalization of the cointegration vector, I compute the Banerjee-Carrion-i-Silvestre test for both  $\log(LE_{it})$  as the dependent and  $\log(T_{it})$  as the dependent variable.

However, it is well known that residual-based (panel) cointegration tests require that the long-run and short-run elasticities are equal, and that failure of this common factor restriction causes significant loss of power (see, for example, Westerlund, 2007). Therefore, I also use the four

panel cointegration tests of Westerlund (2007), which have high power relative to residual-based panel cointegration tests. The Westerlund tests are conditional error correction model (ECM) based tests that (like their time-series counterparts) evaluate the significance of the lagged dependent variable ( $\log(LE_{it-1})$ ) in the conditional ECM, which in our case is given by

$$\Delta \log(LE_{it}) = b_{1i} + b_{2i} \log(LE_{it-1}) + b_{3i} \log(T_{it-1}) + \sum_{j=1}^{k_i} \gamma_{1ij} \Delta \log(LE_{it-j}) + \sum_{j=0}^{k_i} \gamma_{2ij} \Delta \log(T_{it-j}) + \xi_{it} \quad (2)$$

The group-mean statistics denoted  $G_\tau$  and  $G_\alpha$  (using the nomenclature in Westerlund, 2007) test the null of no cointegration against the alternative that there is cointegration for at least one cross-sectional unit, whereas the panel statistics  $P_\tau$  and  $P_\alpha$  test the null of no cointegration against the simultaneous alternative that the panel is cointegrated. To account for cross-sectional dependence, I use the bootstrap approach of Westerlund (2007). Because the ECM procedure assumes that the explanatory variables are weakly exogenous, both  $\Delta \log(LE_{it})$  and  $\Delta \log(T_{it})$  are employed as dependent variables.

Table 3. Panel cointegration tests

	(1) Assumed endogenous variable: $\log(LE_{it})$	(2) Assumed endogenous variable: $\log(T_{it})$
Banerjee and Carrion-i-Silvestre (2011)	-2.150*	-2.252**
Westerlund (2007)		
$G_\tau$	-1.729*** (0.005)	-2.768*** (0.000)
$G_\alpha$	-2.014 (0.733)	-10.798*** (0.003)
$P_\tau$	-32.224*** (0.003)	-20.123*** (0.000)
$P_\alpha$	-4.953** (0.028)	-8.246*** (0.000)

Notes: Bootstrap  $p$ -values (based on 400 replications) in parentheses. To avoid overparametrization and the resulting loss of power, only one lag was included in the tests. The 5% [10%] critical value for the Banerjee and Carrion-i-Silvestre (2011) statistic (for  $T = 50$  and  $N = 50$ ) is -2.18 [-2.11]. Banerjee and Carrion-i-Silvestre (2011) do not report 1% critical values. \*\*\* (\*\*) [\*] indicate rejection of the null hypothesis of no cointegration at the 1% (5%) [10%] level.

The results of these tests are reported in Table 3. While the test statistics reported in column (1) are based on the assumption that  $\log(LE_{it})$  is endogenous in the long run, column (2) assumes that  $\log(T_{it})$  is endogenous on the long run. Eight (nine) of the ten statistics reject the null hypothesis of no cointegration at least at the 5 (10) percent level, indicating that there is a long run relationship between life expectancy and trade. The fact that the results in both columns overall indicate that  $\log(LE_{it})$  and  $\log(T_{it})$  are cointegrated is also a first indication that the long-run

relationship between the two variables is bidirectional. This issue is examined in more detail in the next section.

## 4. Empirical analysis

This section presents estimates of the long-run relationship between trade and population health (Subsection 4.1) and tests the robustness of the estimates (Subsection 4.2). The section also presents estimates of the long-run coefficients for certain country groups (Subsection 4.3) and investigates the direction of long-run causality between the two variables (Subsection 4.4).

### 4.1. Panel cointegration estimates

I use the panel DOLS estimator suggested by Kao and Chiang (2000) to estimate the long-run relationship between life expectancy and trade openness. The DOLS estimator is superconsistent, asymptotically unbiased, and normally distributed, even in the presence of endogenous regressors. Moreover, recent Monte Carlo evidence by Wagner and Hlouskova (2010) suggests that Kao and Chiang's panel DOLS estimator outperforms other estimators.<sup>6</sup> The idea behind this estimator is to account for possible serial correlation and endogeneity of the regressors by augmenting the cointegrating regression with lead, lag, and current values of the first differences of the I(1) regressors. Accordingly, the DOLS regression in this study is given by

$$\log(LE_{it}) = a_i + \beta \log(T_{it}) + \sum_{j=-k}^k \theta_{ij} \Delta \log(T_{it-j}) + \varepsilon_{it} . \quad (3)$$

The DOLS procedure is applied to both the raw data and to data that have been demeaned over the cross-sectional dimension; that is, in place of  $\log(LE_{it})$  and  $\log(T_{it})$ , I also use

$$\begin{aligned} \log(LE_{it})' &= \log(LE_{it}) - \overline{\log(LE_t)} \quad \text{and} \quad \log(T_{it})' = \log(T_{it}) - \overline{\log(T_t)}, \quad \text{where} \\ \overline{\log(LE_t)} &= N^{-1} \sum_{i=1}^N \log(LE_{it}) \quad \text{and} \quad \overline{\log(T_t)} = N^{-1} \sum_{i=1}^N \log(T_{it}) \end{aligned} \quad (4)$$

---

<sup>6</sup> An extension of the panel DOLS estimator is the dynamic seemingly unrelated regression (DSUR) estimator proposed by Mark et al. (2005), which was not included in the simulation study by Wagner and Hlouskova (2010). Unfortunately, the DSUR estimator is seriously biased or not applicable when, as in this study, the number of time periods is small relative to the number of cross-sectional units (Mark et al. 2005, Di Iorio and Fachin, 2012).

to account for cross-sectional dependence due to common shocks or spillovers among countries at the same time. The demeaning procedure is equivalent to including time dummies in equation (3).

However, a problem with this procedure is that it may, on the one hand, be ineffective in eliminating the cross-sectional correlation problem when the individual responses to the common shocks differ across countries. On the other hand, the demeaning procedure may introduce cross-sectional correlation among the error terms when it is not already present (Carpurale and Cerrato, 2006). I therefore explicitly test for cross-sectional dependence (CD) in the residuals of the estimated DOLS models using the CD test of Pesaran (2004).

Columns 1 and 2 of Table 4 present the results of the panel DOLS estimation for the raw and demeaned data, including the CD statistics. Both the results for the raw and demeaned data show a positive and statistically significant relationship between life expectancy and trade openness. However, the CD test rejects the null hypothesis of no cross sectional dependence for the raw data (see column 1), while the null of no cross sectional dependence is not rejected for the demeaned data (see column 2). Thus, the estimated coefficient in column 1 is likely to be biased due to cross-sectional dependence, and therefore I focus on the long-run effect implied by the coefficient in column 2.

Table 4. DOLS estimates of the long-run effect of trade openness on life expectancy

	(1) Unadjusted data	(2) Demeaned data
Coefficient on $\log(T_{it})$	0.171*** (21.33)	0.048*** (5.92)
CD statistic	86.95***	0.79
Observations	3552	3552

*Notes:* The dependent variable is  $\log(LE_{it})$ . The DOLS regressions were estimated with one lead and one lag ( $k = 1$ ). The CD test statistic is normally distributed under the null hypothesis of no cross-sectional dependence.  $t$ -statistics in parenthesis. \*\*\* indicate significance at the 1% level.

What can be said about the magnitude of the estimated effect in column 2? Multiplying the coefficient of  $\log(T_{it})$  (0.048) with the average value of  $\Delta\log(T_{it})$  in the sample yields value of 0.000574, implying that trade openness has contributed 0.0574 percentage points to the annual increase in life expectancy for the average country in the sample. With an average growth rate of life expectancy of 0.4979 percent, this means that trade openness has been responsible for 11.53 percent of the annual increase in life expectancy in the country sample considered here—a quantitatively significant effect. With an average increase in life expectancy of 0.297072 years, the estimated effect implies that the average gain in life expectancy was about 12 days per year.

## 4.2. Robustness

I perform several robustness checks (using demeaned data to account for cross-sectional dependence). First, I examine whether the positive relationship between trade and population health is robust to alternative estimation techniques. A potential problem with the pooled results in Table 4 could be that they are based on the implicit assumption of homogeneity of the long-run parameters. While efficiency gains from the pooling of observations over the cross-sectional units can be achieved when the individual slope coefficients are the same, pooled estimators may yield inconsistent and potentially misleading estimates of the sample mean of the individual coefficients when the true slope coefficients are heterogeneous. Although a comparative study by Baltagi and Griffin (1997) concludes that the “the efficiency gains from pooling appear to more than offset the biases due to intercountry heterogeneities” (p. 317), I nonetheless allow the long-run coefficients to vary across countries by using the group-mean panel DOLS estimator suggested by Pedroni (2001). This estimator involves estimating separate country-specific DOLS regressions and averaging the individual coefficients. In addition, I use the panel FMOLS estimator suggested by Phillips and Moon (1999). Like the time-series FMOLS estimator, the panel FMOLS estimator incorporates a semi-parametric correction to the OLS estimator, which eliminates the second order bias induced by the endogeneity of the regressors. For completeness, I also apply the group-mean panel FMOLS estimator of Pedroni (2000), which, like the group-mean panel DOLS estimator, is based individual country time-series regressions. I report the results of these estimation methods in columns 1 and 2 of Table 5. As can be seen, the estimated coefficients are significant at the 1 percent level. The panel and group-mean estimators produce similar coefficients, and the FMOLS coefficient estimates in Table 5 are almost identical to their panel and group-mean DOLS counterparts in Tables 4 and 5. Thus, the results are robust to alternative estimation methods.

Table 5. Alternative estimators

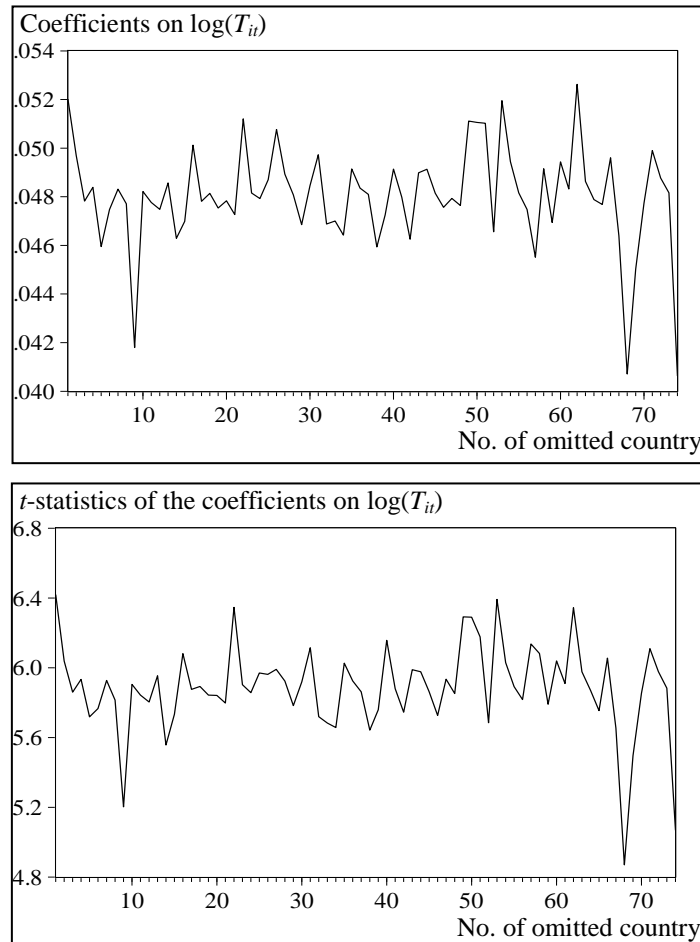
	(1) Group-mean panel DOLS estimator (Pedroni, 2001)	(2) Panel FMOLS estimator (Phillips and Moon, 1999)	(2) Group-mean panel FMOLS estimator (Pedroni, 2000)
Coefficient on $\log(T_{it})$	0.034*** (4.05)	0.049*** (6.63)	0.033*** (4.12)
Observations	3552	3700	3700

*Notes:* The dependent variable is  $\log(LE_{it})$ . The DOLS regressions were estimated with one lead and one lag ( $k = 1$ ).  $t$ -statistics in parenthesis. \*\*\* indicate significance at the 1% level.

To verify that the positive effect of trade on life expectancy is not due to individual outliers, the panel DOLS regression is re-estimated excluding one country at a time from the sample. The sequentially estimated coefficients and their  $t$ -statistics are presented in Figure 4. Each number on

the horizontal axes represents the country omitted from DOLS regression; the vertical axes plot the coefficients and  $t$ -statistics in the remaining sample. As can be seen, the sequentially estimated coefficients are relatively stable and always significant at the 1 percent level, suggesting that the results are not due to potential outliers.

Figure 4. DOLS estimation with single country excluded from the sample



In Table 6, I check whether my results are sensitive to the selection of countries and the sample period. In column 1, I estimate the coefficient on  $\log(T_{it})$  using a larger sample of 101 countries over the period 1970-2000. In columns 2 and 3, I re-estimate the DOLS regression for two non-overlapping sub-periods (of equal length) from 1960 through 1985 and 1986 through 2010 using the original sample. The coefficients are always positive and statistically significant.



Table 6. DOLS estimates for a larger sample of countries and for different sub-periods

	(1)	(2)	(3)
Independent variable	Larger country sample 1970-2000	First sub-period (1960-1985)	Second sub-period 1986-2010
$\log(T_{it})$	0.018** (2.44)	0.023*** (3.42)	0.055*** (4.35)
Countries	101	74	74
Observations	2828	1776	1776

Notes: The dependent variable is  $\log(LE_{it})$ . The DOLS regressions were estimated with one lead and one lag ( $k = 1$ ). The results are based on demeaned data.  $t$ -statistics in parenthesis. \*\*\* (\*\*) indicate significance at the 1% (5%) level.

I also examine whether the results are robust to alternative measures of trade openness and population health. Alcalá and Ciccone (2004) propose real openness as an alternative trade measure, while many health studies use infant mortality as an indicator of the general health status of the population. Table 7 presents the results of the panel DOLS regressions using these two different measures, labelled  $\log(RT_{it})$  and  $\log(IMR_{it})$ , both separately and jointly. Again, the results suggest that trade has a positive effect on population health.

Table 7. DOLS estimates using different measures of trade openness and population health

	(1)	(2)	(3)
Independent variable	Different openness measure [Dependent variable: $\log(LE_{it})$ ]	Different health measure [Dependent variable: $\log(IMR_{it})$ ]	Different openness and different health measure [Dependent variable: $\log(IMR_{it})$ ]
$\log(T_{it})$		-0.348*** (-10.04)	
$\log(RT_{it})$	0.010*** (3.04)		-0.429*** (-16.97)
Countries	72	65	63
Observations	3456	3120	3024

Notes:  $\log(RT_{it})$  is the log of real openness, defined as (the log of) trade (exports plus imports) relative to GDP at PPP.  $\log(IMR_{it})$  is the log of the infant mortality rate. All countries with complete time series for the period 1960-2010 were included. The DOLS regressions were estimated with one lead and one lag ( $k = 1$ ). The results are based on demeaned data.  $t$ -statistics in parenthesis. \*\*\* indicate significance at the 1% level.

As discussed in Section 3, the finding of cointegration implies that there are no missing trending variables and that therefore no additional variables are needed to produce unbiased estimates. Nevertheless, I check the robustness of the results to the inclusion of GDP per capita and GDP per capita squared. As can be seen from columns 1 and 2 of Table 8, the impact of trade on life expectancy remains positive and statistically significant when I control for GDP per capita and GDP per capita squared. While in column 1 the coefficient on GDP per capita has an unexpected sign, the signs of the coefficients on both GDP per capita and GDP per capita squared in column 2 are as expected: the coefficient on GDP per capita is positive and statistically significant and the coefficient on GDP per capita squared is negative and statistically significant. This is consistent with the hypothesis (discussed in Subsection 2.1) that increases in income are associated with

increases in life expectancy, but the gain in life expectancy resulting from increased income diminishes as income increases.

Table 8. DOLS estimates with additional control variables

Independent variables	(1)	(2)
$\log(T_{it})$	0.049*** (5.94)	0.050*** (5.79)
$\log(GDP_{it})$	-0.014* (-1.85)	0.256*** (7.91)
$\log(GDP_{it}) \times \log(GDP_{it})$		-0.018*** (-8.84)
Countries	69	69
Observations	3312	3312

Notes: The dependent variable is  $\log(LE_{it})$ . All countries with complete time series for the period 1960-2010 were included. The DOLS regressions were estimated with one lead and one lag ( $k = 1$ ). The results are based on demeaned data.  $t$ -statistics in parenthesis. \*\*\* (\*) indicate significance at the 1% (10%) level.

### 4.3. Estimates for sub-samples

I now present estimates of the effect of trade openness on life expectancy for sub-samples of countries in order to assess whether countries with certain characteristics benefit more, on average, from trade in terms of gains in life expectancy than others. Or, to put it differently, I investigate possible factors that could influence the effect of trade on health on average across countries (though not necessarily in each country).

One of these factors could be the level of development. Several arguments discussed in Section 2 suggest that the effect of trade on health may differ between developed and developing countries. In panel 1 of Table 9, I estimate the effect for these two country groups separately to investigate this issue.<sup>7</sup> I use both the pooled DOLS estimator and the group-mean panel DOLS estimator to ensure the robustness of the conclusions. As can be seen, the pooled DOLS coefficient for developing countries is somewhat larger than the pooled DOLS coefficient for developed countries. According to the group-mean results, the estimated coefficient on  $\log(T_{it})$  for developing countries is more than double the coefficient for developed countries. Thus, it can be concluded that the effect of trade on life expectancy is larger in developing countries than in developed countries.

In panel 2 of Table 9, I present separate DOLS estimates for the countries with the highest share of taxes on income, profits, and capital gains in GDP and for the countries with the lowest share of taxes on income, profits, and capital gains in GDP (listed in Appendix A).<sup>8</sup> As speculated

<sup>7</sup> I follow the IMF (2013) classification of advanced (or developed) countries and developing countries, including emerging market economies. See Table A.1 in Appendix A for a list of the countries in these groupings.

<sup>8</sup> The classification is based on average data over the period 1990-2010. The data are from the WDI online database. Taxes on income, income, profits and, capital gains as a percentage of GDP were calculated by multiplying taxes on income, profits, and capital gains as a percentage of total government revenue by government revenue as percentage of GDP (divided by 100). The WDI database reports data on taxes on income, profits, and capital gains for all countries in the sample, with the exception of Malawi, Mauritania, and Puerto Rico.

in Section 2, increasing taxes on income, profits, and capital gains may mitigate the negative effects that trade may have on health through its impact on income (and health) inequality by reducing inequality and financing public health services. Increasing taxes may also mitigate the negative health effects of economic insecurity by financing social security. The estimates in panel 2 support this speculation. In the pooled DOLS regressions, the trade coefficient for the countries with the highest taxes is highly significant and more than 20 times larger than the statistically insignificant coefficient for the countries with the lowest taxes. The group-mean regression coefficient is even negative (although only significant at the 10 percent level) in the sample of countries with low taxes on income, profits, and capital gains. Thus, the beneficial effect of trade is greater in countries with higher taxes on income, profits, and capital gains; one may even conclude that trade in general does not increase life expectancy in countries with low taxes on income, profits, and capital gains (as a percentage of GDP).

Table 9. DOLS estimates for sub-samples

	log( $T_{it}$ )		No. of countries	No. of obs.
	Pooled estimator	Group-mean estimator		
1. Developed countries	0.041*** (5.18)	0.023*** (4.25)	22	1056
Developing countries	0.054*** (5.64)	0.057*** (5.81)	52	2496
2. Countries with the highest share of taxes on income, profits, and capital gains in GDP	0.103*** (10.96)	0.068*** (6.48)	36	1728
Countries with the lowest share of taxes on income, profits, and capital gains in GDP	0.005 (0.38)	-0.023* (-1.67)	35	1680
3. Countries with the most business-friendly regulations (according to the ease-of-doing-business index)	0.106*** (8.95)	0.055*** (6.20)	36	1728
Countries with the least business-friendly regulations (according to the ease-of-doing-business index)	0.021** (2.09)	0.026*** (2.86)	37	1776
4. Countries with the most restrictive labor regulations (according to the rigidity-of-employment index)	0.038*** (3.21)	0.011 (0.94)	37	1776
Countries with the least restrictive labor regulations (according to the rigidity-of-employment index)	0.057*** (4.98)	0.044*** (3.78)	36	1728

Notes: The dependent variable is  $\log(LE_{it})$ . The DOLS regressions were estimated with one lead and one lag ( $k = 1$ ). The results are based on demeaned data.  $t$ -statistics in parenthesis. \*\*\* (\*\*) indicate significance at the 1% (5%) level.

In panels 3 and 4 of Table 9, I examine the possibility that the effect of trade on life expectancy depends on the level of regulation. Government regulations may impede the reallocation of resources from import competing to export industries, thereby reducing the gains from trade (Freund and Bolaky, 2008). In scenarios of severe market imperfections which limit both the mobility of factors between sectors and the flexibility of factor prices, it is even theoretically possible that trade leads to income losses (see, for example, Haberler, 1950; Edwards, 1988; Krishna and Yavas, 2005; Chang et al., 2009). If lower (higher) income is associated with lower

(higher) life expectancy, the effect of trade on life expectancy should be greater in less regulated countries. To investigate this issue, I consider two forms of regulation: business regulation and labor regulation. In panel 3, I present separate estimates for the countries with the most business-friendly regulations and for the countries with the least business-friendly regulations (listed in Appendix A).<sup>9</sup> In panel 4, I report separate estimates for the countries with the highest labor market rigidities and for the countries with the lowest labor market rigidities (also listed in Appendix A).<sup>10</sup> The estimated trade coefficients for the most regulated countries are always smaller than the coefficients for the least regulated countries. The coefficient on trade in the group-mean regression for the countries with the most restrictive labor regulations is even insignificant. From these results it can be concluded that trade-induced gains in life expectancy are, in general, greater in less regulated countries.

#### 4.4. Long-run causality

The above interpretation of the results is based on the assumption that long-run causality runs from trade to health. Cointegration implies causality in at least one direction. However, cointegration says nothing about the direction of the causal relationship between the variables, as discussed above. To test the direction of long-run causality, I follow common practice in the applied panel cointegration literature (see, for example, Canning and Pedroni, 2008; Apergis et al., 2010; Herzer et al., 2012) and employ a two-step procedure. In the first step, I use the (pooled) DOLS estimate of the long-run relationship to construct the disequilibrium term

$$ec_{it} = \log(LE_{it}) - [\hat{a}_i - 0.048\log(T_{it})], \quad (5)$$

In the second step, I estimate the error correction model

$$\begin{aligned} \Delta \log(LE_{it}) &= c_{1i} + a_1 ec_{it-1} + \sum_{j=1}^k \varphi_{11ij} \Delta \log(LE_{it-j}) + \sum_{j=1}^k \varphi_{12ij} \Delta \log(T_{it-j}) + e_{it}^{\log(LE)} \\ \Delta \log(T_{it}) &= c_{2i} + a_2 ec_{it-1} + \sum_{j=1}^k \varphi_{21ij} \Delta \log(LE_{it-j}) + \sum_{j=1}^k \varphi_{22ij} \Delta \log(T_{it-j}) + e_{it}^{\log(T)} \end{aligned} \quad (6)$$

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<sup>9</sup> The classification is based on the ease-of-doing-business index, averaged over the period 2004-2010. The data are from the Doing Business reports of the World Bank (available at <http://www.doingbusiness.org/>). The ease-of-doing-business index is not available for Barbados.

The error-correction term,  $ec_{it-1}$ , represents the error in, or deviation from, the equilibrium, and the adjustment coefficients  $a_1$  and  $a_2$  capture how  $\log(LE_{it})$  and  $\log(T_{it})$  respond to deviations from the equilibrium. From the Granger representation theorem (Engle and Granger, 1987) it follows that at least one of the adjustment coefficients must be nonzero if a long-run relationship between the variables is to hold. A statistically significant error correction term also implies long-run Granger causality from the explanatory variables to the dependent variables (Granger, 1988), and thus that the dependent variables are endogenous in the long run. An insignificant error correction term implies long-run Granger non-causality, and thus that the explanatory variables are weakly exogenous (Hall and Milne, 1994). Because all variables in the model, including  $ec_{it-1}$ , are stationary (because the level variables are cointegrated), a conventional likelihood ratio chi-square test can be used to test the null hypothesis of weak exogeneity,  $H_0 : a_{1,2} = 0$ .

Table 10. Tests for long-run causality/weak exogeneity

Number of lags of the differenced variables	(1)	(2)
	Weak exogeneity of $\log(LE_{it})$ (Significance of $\alpha_1$ )	Weak exogeneity of $\log(T_{it})$ (Significance of $\alpha_2$ )
1	327.854*** (0.000)	7.817*** (0.005)
2	289.782*** (0.000)	4.307** (0.038)
3	135.358*** (0.000)	4.898** (0.027)

Note: Column values are chi-square statistics (with one degree of freedom).  $p$ -values in parentheses. The results are based on demeaned data. \*\*\* indicate rejection of the null hypothesis of weak exogeneity at the 1% (5%) level.

Table 10 reports the results (based on demeaned data to account for the likely cross-sectional dependence through common time effects). Given that results from Granger-causality tests are sensitive to the selection of the lag length, I present results for different lags ( $k = 1, 2, 3$ ). As can be seen from column 1, the null hypothesis of weak exogeneity is rejected for  $\log(LE_{it})$  at the 1 percent level no matter which lag length is chosen. Column 2 shows that the null hypothesis of weak exogeneity is also rejected at least at the 5 percent level for  $\log(T_{it})$ . From this it can be concluded that the statistical long-run causality is bidirectional, implying that increased trade is both a consequence and a cause of increased life expectancy.

<sup>10</sup> The classification is based on the rigidity-of-employment index, averaged over the period 2003-2010. The data are from the Doing Business reports of the World Bank (available at <http://www.doingbusiness.org/>). For Barbados, the rigidity-of-employment index is not reported.

## 5. Conclusion

In this study, I examined the long-run relationship between international trade and population health for a sample of 74 countries over the period 1960-2010, a relationship that has received little empirical attention. Using panel time-series techniques, I found that trade openness in general has a positive long-run effect on health, as measured by life expectancy and infant mortality, and that increased trade is both a consequence and a cause of improved health. Thus, this study suggests that there is, in general, a virtuous cycle in which improved health leads to more trade, and increased trade further benefits population health. However, the strength of this relationship seems to vary across countries. My results show that the beneficial effect of trade on health tends to be greater in countries with lower development levels, higher taxes on income, profits, and capital gains, and less business and labor market regulations.

One reason why the effect differs between developed and developing countries could be the existence of diminishing health returns to increases in income—that is, increases in income are associated with increases in life expectancy, but the gain in life expectancy resulting from increased income diminishes as income increases. Another reason could be access to medical goods and medical technology. A possible reason for the greater effect of trade on health in countries with higher taxes on income, profits, and capital gains is that such taxes (which are typically progressive) may mitigate negative health effects of income inequality and insecurity by reducing income differences between the winners and losers from trade and by financing public health services and social security benefits. Finally, a plausible reason why the effect differs between more and less regulated countries is that regulations may limit both the mobility of factors between sectors and the flexibility of factor prices, thereby reducing the gains from trade.

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## Appendix A

### Countries in the sub-samples

Table A.1. *List of countries and their classification*

Algeria	1, 4, 5, 7	Ghana	1, 4, 6, 8	Nigeria	1, 4, 6, 8
Argentina	1, 4, 5, 8	Greece	2, 4, 5, 8	Norway	2, 3, 5, 7
Australia	2, 3, 5, 7	Guatemala	1, 4, 6, 8	Peru	1, 3, 5, 8
Austria	2, 3, 5, 8	Honduras	1, 4, 5, 8	Philippines	1, 4, 6, 7
Bangladesh	1, 4, 5, 8	Hong Kong	2, 3, 6, 7	Portugal	2, 3, 5, 8
Barbados	1, 7	Hungary	1, 3, 6, 8	Puerto Rico	1, 3, 6
Belgium	2, 3, 5, 7	Iceland	2, 3, 6, 7	Rwanda	1, 3, 6, 8
Benin	1, 4, 5, 8	India	1, 4, 6, 7	Senegal	1, 4, 5, 8
Botswana	1, 3, 5, 7	Indonesia	1, 4, 5, 7	South Africa	1, 3, 6, 7
Brazil	1, 4, 6, 8	Ireland	2, 3, 6, 7	Spain	2, 3, 5, 7
Burkina Faso	1, 4, 5, 8	Italy	2, 3, 5, 7	Sri Lanka	1, 4, 6, 8
Burundi	1, 4, 5, 8	Japan	2, 3, 6, 7	Sudan	1, 4, 6, 8
Canada	2, 3, 5, 7	Kenya	1, 4, 6, 7	Sweden	2, 3, 5, 8
Central African Rep.	1, 4, 6, 8	Korea, Rep.	2, 3, 5, 7	Syria	1, 4, 6, 7
Chile	1, 3, 5, 7	Lesotho	1, 4, 6, 8	Thailand	1, 3, 6, 7
Congo, Dem. Rep.	1, 4, 5, 8	Luxembourg	2, 3, 5, 7	Togo	1, 4, 5, 8
Congo, Rep.	1, 4, 5, 8	Madagascar	1, 4, 5, 8	Trinidad and Tobago	1, 3, 6, 7
Costa Rica	1, 4, 5, 8	Malawi	1, 4, 6	Turkey	1, 3, 6, 7
Denmark	2, 3, 6, 7	Malaysia	1, 3, 6, 7	Uganda	1, 4, 6, 8
Dominican Republic	1, 3, 6, 8	Mauritania	1, 4, 5	United Kingdom	2, 3, 6, 7
Ecuador	1, 4, 6, 7	Mexico	1, 3, 5, 7	United States	2, 3, 6, 7
Egypt	1, 4, 6, 8	Morocco	1, 4, 5, 7	Uruguay	1, 4, 6, 8
Fiji	1, 3, 6, 7	Netherlands	2, 3, 5, 7	Venezuela	1, 4, 5, 7
Finland	2, 3, 5, 8	Nicaragua	1, 4, 6, 8	Zambia	1, 3, 6, 7
France	2, 3, 5, 8	Niger	1, 4, 5, 8		

*Notes:* The number “1” [“2”] indicates that the country was included in the sub-sample of 52 [22] developing [developed] economies (according to IMF (2013) classification). The number “3” [“4”] indicates that the country was included in the subsample of 36 [37] countries with the most [least] business-friendly regulations. The number “5” [“6”] indicates that the country was included in the subsample of 37 [36] countries with the most [least] restrictive labor regulations. The number “7” [“7”] indicates that the country was included in the subsample of 36 [35] countries with the highest [lowest] share of taxes on income, profits and capital gains in GDP.