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Zusammenfassung/ Abstract

Although a large body of research has examined the effects of unions on the wage distribution, surprisingly little attention has been devoted to the effects of unions on the distribution of income. This paper examines the long-run relationship between unionization and income inequality for a sample of 20 countries. Using heterogeneous panel cointegration techniques, we find that (i) unions have, on average, a negative long-run effect on income inequality, (ii) there is considerable heterogeneity in the effects of unionization on income inequality across countries (in about a third of cases the effect is positive), and (iii) long-run causality runs in both directions, suggesting that, on average, an increase in unionization reduces income inequality and that, in turn, higher inequality leads to lower unionization rates.

JEL-Klassifikation / JEL-Classification: J51; D31; C23

Schlagworte /Keywords: unions; income inequality; cross-country heterogeneity; causality; panel cointegration

1. INTRODUCTION

Although a large body of research has examined the effects of unions on the wage distribution, surprisingly little attention has been devoted to the effects of unions on the distribution of income. More recent studies of wage inequality tend to find that unions compress the wage distribution (see, e.g., Kahn, 2000; Card et al., 2004; Koeniger et al., 2007). The results of the few studies on unions and income distribution are mixed—with negative, positive, and insignificant relationships between the level unionization and the level of income inequality (see, e.g., Alderson and Nielsen, 2002; Checchi and García-Peñalosa, 2010, 2008).

Economic theory is also not clear on the impact of unions on income inequality. Unions can affect the distribution of income by different mechanisms, including wage inequality, unemployment, and the wage share. Since the signs of the effects of unions on these variables are theoretically ambiguous, and since the signs may also differ between the mechanisms, the net effect on income distribution is theoretically indeterminate (Checchi and García-Peñalosa, 2008, 2010).

Moreover, there are good reasons to assume that both the contributions of these mechanisms to income inequality and the effects of unions on these mechanisms are country-specific. Suppose, for example, that higher unemployment increases the proportion of people receiving unemployment benefits. Because unemployment benefits are typically lower than wage income, unemployment increases income inequality through its effect on the proportion of individuals with low incomes (provided that this proportion is not too high).[1] The magnitude of the inequality increasing effect of unemployment, however, depends on the size of the gap between high and low incomes and thus on the level of unemployment benefits, which is country-specific. Unemployment, in turn, depends, among other factors, on the ability of unions to raise wages above market clearing levels. The ability of unions to raise wages, in turn, depends not only on union power in wage bargaining

[1] When the unemployment rate is very high, a large fraction of the population is receiving unemployment benefits and thus nearly the same income. In this (unrealistic) case, an increase in unemployment might cause a decrease in average income inequality simply because an increasing proportion of people have the same income (Checchi and García-Peñalosa, 2008).

(unionization), but also on labor market institutions such as bargaining coverage and the level of bargaining centralization (Layard et al., 2005, p. XV). Since labor market institutions differ across countries, the effect of unions on employment and consequently on income inequality may also vary across countries.

Also, it is not clear whether the level of unionization is influenced by income inequality. For example, inequality-averse union members may perceive unions as unable to reduce wage inequality and to influence redistributive policy when inequality increases. If these individuals feel that their expectations regarding the efficacy of unions in reducing inequality have been disappointed, a decline in union membership may be a consequence of an increase in wage and income inequality (see, e.g., Checchi et al., 2010). Alternatively, risk-averse individuals may perceive unions as effective in providing job and income security (see, e.g., Checchi and Visser, 2005). If greater inequality results in greater unemployment and income insecurity, such individuals may join unions in times of inequality. Accordingly, an increase in income inequality could also lead to an increase in union membership.

It is thus likely that the impact of unionization on income distribution is heterogeneous across countries and that the level of unionization is endogenous with respect to income inequality in many countries. However, existing cross-country panel studies on income inequality and unionization use homogeneous panel data models, which, by definition, are unable to capture the potential heterogeneity in the relationship between unionization and income inequality across countries. Moreover, the simple pooled models used in some of these studies may yield misleading results in the presence of such heterogeneity. Several country-specific factors may induce apparent differences in the effect of unions on income inequality. If these factors (that are correlated with both unionization and income inequality) are not controlled for, biased estimates may result; this is the classical omitted-variables problem. In addition, it is well-known that even if unobserved time-invariant country-specific effects are controlled for, homogeneous panel estimators produce inconsistent and potentially misleading estimates of the average values of the parameters in dynamic models when the slope coefficients differ across cross-section units (Pesaran and Smith, 1995).

The potential endogeneity problem implies, on the one hand, that one cannot completely exclude the possibility that the results reported in some studies reflect a causal effect of income inequality on unionization, rather than a causal effect of unionization on income inequality. On the other hand, it is also possible that decreased unionization leads to increased inequality, which, in turn, feeds back into decreased or increased unionization. In this case, simple OLS and fixed/random effects estimates of the causal effect of unionization on income inequality conflate these two effects; that is, they are biased and the direction of the bias cannot be predicted. As pointed out by Scheve and Stasavage (2009, p. 235): "The big question … is whether … [the correlation between union density and income inequality] also reflects a causal relationship whereby union density determines inequality." This is one question addressed here.

Our objective is to examine whether and how the level of unionization (or union strength) is related to the level of income inequality using heterogeneous panel cointegration techniques. The advantage of the cointegration framework is that the resulting estimates are robust to a variety of estimation problems that often plague empirical work, including omitted variables and endogeneity (see, e.g., Pedroni, 2007; Coe et. al, 2009). Not only does such an analysis provide new evidence on the average effect of unions on income inequality across countries, it also allows us to examine potential country differences in the effect of unions on the distribution of income. Moreover, we not only estimate the cointegrating or long-run relationship between the level of unionization and the level of inequality, both for the sample as a whole and for each country individually, but we also examine the direction of long-run causality between the variables.

To preview our main results, we find that (i) unions have, on average, a negative long-run effect on income inequality, but (ii) there are large differences across countries (in about a third of cases the effect is positive), and (iii) long-run causality runs in both directions, suggesting that, on average (or in general), an increase in unionization tends reduces income inequality and that, in turn, higher inequality leads to lower unionization.

The rest of the paper is organized as follows: In Section 2, we discuss the theoretical background and related empirical literature. Section 3 sets out the basic empirical model and describes the data. The econometric implementation and the estimation results are presented in Section 4. Section 5 concludes.

2. THEORETICAL BACKGROUND AND RELATED EMPIRICAL LITERATURE

Three mechanisms have been proposed in the literature by which unions can alter the distribution of income: by affecting the distribution of wages, by affecting employment, and by affecting the wage share (Checchi and García-Peñalosa, 2008, 2010). In this section, we discuss these mechanisms in more detail (Subsections a – c) and review the empirical literature on the impact of unions on income inequality (Subsection d).

(a) *Effects of unions on wage inequality*

Overall income inequality can be decomposed by income sources, such as wage, capital, and transfer income, implying that changes in wage inequality, will, ceteris paribus, lead to changes in income inequality. This prediction is supported by the work of Checchi and García-Peñalosa (2010) who find a significant positive correlation between wage and income inequality.

Unions affect the distribution of wages in two main ways. First, unions push up the wages of low-skilled workers more than those of high-skilled workers and, thereby, they reduce wage dispersion (Freeman, 1980, 1982; Blau and Kahn, 1996; Card, 1996). Accordingly, the pursuit of standard wage policies has the effect of compressing the wage distribution of union members (and those non-members covered by union agreements). A large body of research shows that wage dispersion is generally lower among union workers than their non-union counterparts (Freeman, 1980; Freeman and Medof, 1984; Blau and Kahn, 1996), implying that unions exert an inequality-reducing “within-sector” effect associated with the fact that wage dispersion is different in the union and non-union sectors (Card, 1998). Second, while unions reduce wage inequality within the union sector, they increase the wages of their members (and non-union workers covered by collective agreements) relative to the wages of non-unionized workers. This is the inequality-increasing “between-sector” effect which is associated with the wage gap between union and non-union workers (Card, 1998). Most studies conclude that, overall, unions tend to reduce wage inequality because the magnitude of the inequality-reducing within-sector effect dominates the inequality-increasing between-sector effect (see, e.g., Freeman, 1980; Kahn, 2000; Card et al., 2004; Koeniger et al., 2007).

(b) *Effects of unions on employment*

As discussed in the Introduction, higher unemployment increases the proportion of people receiving unemployment (or other social security) benefits. Since income from unemployment benefits is typically less than income from work, an increase in unemployment will raise the proportion of people with low incomes, and this will widen the gap between the top and the bottom of the income distribution (Checchi and García-Peñalosa, 2008, 2010). Several studies find that higher unemployment is associated with higher income inequality (see, e.g., Björklund 1991; Mocan, 1999; Checchi and García-Peñalosa, 2010).

As far as the role of unionization is concerned, the traditional view is that an increase in union membership increases the bargaining power of unions, which enables unions to raise wages above the competitive market clearing level. When unions fix wages at a level in excess of that at which all workers can be employed, some of these workers will become unemployed (Freeman and Medoff, 1984).

Several theoretical models predict that unions maximize the wage income of their members without taking into account possible negative consequences for employment (see, e.g., Shishter, 1943; Dunlop, 1944; Oswald, 1986), implying that wages are rigid downwards. This is consistent with recent empirical evidence by Dickens et al. (2007) and Holden and Wulfsberg (2008, 2009), who document that wage rigidity is related to the degree of unionization, measured as the percentage of employed wage and salary workers who are union members—union density.

Given these findings, one would expect that unionization reduces employment. However, the evidence is not so clear. While many studies find that greater unionization is associated with higher unemployment rates (see, e.g., Scarpetta, 1996; Nickel, 1997; Blanchard and Wolfers, 2000; Nickel et al., 2005; Checchi and García-Peñalosa, 2008; Checchi and García-Peñalosa, 2010), others find that unionization has no statistically significant effects on employment (see, e.g., OECD, 1999; Baker et al., 2005; Bassanini and Duval, 2009). The latter finding supports an alternative view: that unions care about both (real) wages and employment, and that in an imperfectly competitive labor market environment there is a scope for unions to increase wages (at the expense of reductions in profits) without adverse impacts on employment (see, e.g., Alogoskoufis et al., 1988).

(c) *Effects of unions on the labor share*

While both wage inequality and unemployment tend to increase income inequality, the effect of the wage share is ambiguous. On the one hand, a higher wage share reduces both the contribution of inequality in capital income to total income inequality (Daudey and García-Peñalosa, 2007) and the income differential between those individuals who have capital incomes (supplementing their labor incomes) and those individuals who do not receive income from capital (Checchi and García-Peñalosa, 2008). On the other hand, a higher wage share increases the weight of wage inequality in total income inequality (Checchi and García-Peñalosa, 2008). Since the inequality of capital income is likely to exceed the inequality of wage income (Glyn, 2009), it is reasonable to assume that an increase in the labor share decreases income inequality. This is what empirical studies on factor shares and income inequality show (see, e.g., Daudey and García-Peñalosa, 2007; Checchi and García-Peñalosa, 2010).

As discussed in Blanchard (1997) and Bentolila and Saint-Paul (2003), unions can positively or negatively affect the labor share, depending on their impact on wages and employment. If unions and firms bargain over both wages and employment (the efficient bargaining model), and workers, as a result, are able to obtain a higher wage without suffering a decrease in employment, the labor share will increase. If unions and firms instead bargain only over wages, leaving firms free to determine employment unilaterally (the right-to-manage model), higher union wages may combine with lower employment. In this case, an increase in union power can lead to a lower wage share.

While several studies fail to find evidence that union power affects the labor share (see, e.g., Simler, 1961; Ahlseen, 1990), others show that union density is associated with a higher labor share (see, e.g., Macpherson, 1990; Fichtenbaum, 2011). According to the results of Checchi and García-Peñalosa (2010), union density exerts a negative, but insignificant effect on the labor share.

(d) *Effects of unions on income inequality*

From the above discussion, it can be concluded that unions may affect the distribution of wages, unemployment, and the labor share, which may, in turn, affect the distribution of income. Obviously, many (positive and negative) effects are possible, implying that the net effect of unions on income inequality is an empirical matter. However, studies on the relationship between unionization and inequality are relatively scarce.

Gustafsson and Johansson (1999), by analyzing unbalanced panel data for 16 industrialized countries between 1966 and 1994, find that unionization is significantly negatively related to income inequality in most specifications. This is consistent with the results of Alderson and Nielsen

(2002) who, using unbalanced panel data for 16 OECD countries over the period from 1967 to 1992, find that unionization is associated with a statistically significant decrease in income inequality. Similarly, Bradley et al. (2003) find in an unbalanced panel of 14 developed countries over the period from 1967 to 1997 that unionization has a significant equalizing effect. Mahler (2004) employs a pooled cross-sectional time-series model based on unbalanced panel data covering 14 developed countries between 1981 and 2000 (without controlling for country-specific effects). Consistent with the above studies, he finds that unionization is negatively associated with income inequality. Similarly, the results of Calderón et al. (2005) suggest, based on panel data for 121 developed and developing countries between 1970 and 2000, that union density has a significant equalizing effect on the distribution of income.

Checchi and García-Peñalosa (2008), in contrast, use unbalanced panel data for 16 industrial countries over the period from 1969 to 2004 and find no significant relationship between unionization and income inequality. Scheve and Stasavage (2009) report results for an unbalanced panel of 13 industrialized countries over the period from 1916 to 2000. According to their estimates, the relationship between unionization and income inequality is negative, but not significant for the period from 1976 to 2000. Similarly, a study by the International Labor Organization (ILO, 2008) suggests, based on unbalanced panel data for up to 43 countries from 1989-2003, that changes in union density are generally not significantly related to changes in income inequality. Unions density seems to have a significant negative impact on income inequality only in Central and Eastern European countries (Hungary and Poland), while, remarkably, the fixed-effects coefficient on union density in Latin America and advanced industrial countries is positive, although not significant.

Finally, Checchi and García-Peñalosa (2010) apply panel data estimation techniques to unbalanced panel data for 14 (and 11) industrialized economies over the period from 1960 to 2000.

Their results suggest that unionization increases income inequality and that the inequality increasing effect is due to the negative effect of unionization on employment.

Summarizing, it can be said that existing empirical results are mixed. However, it is not clear whether some of the findings also reflect a causal effect of income inequality on unionization. The theoretical model of Acemoglu et al. (2001), for example, suggests a negative feedback relationship between union membership and income inequality. More specifically, the model predicts that if skill-biased technical change is the main cause of income inequality, the compressed wage structure in the union sector will give skilled workers an incentive to look for jobs in the non-union sector where market forces cause the real wages of skilled workers to increase relatively faster (because of the increase in the productivity of skilled workers relative to unskilled workers). The resulting decline in unionization, in turn, will produce a widening wage distribution as more workers choose to become skilled and move from a sector where wages are more compressed (the union sector) to a sector where they are less compressed (the non-union sector). Moreover, as discussed in the Introduction, income inequality may be a determinant of unionization if union members feel that their expectations regarding the efficacy of unions in reducing income inequality have been disappointed in the face of increasing inequality, or unions are perceived as an effective mechanism for securing income in times of economic uncertainty caused by economic inequality. Thus, an increase in income inequality may be both a cause and a consequence of a decrease or an increase in union membership. However, the question of causality is not explicitly addressed in the existing literature. In addition, it is well known that conventional estimation methods generally produce estimates that are biased in the presence of endogenous variables.

Another limitation of the existing studies is that they do not adequately account for the potential heterogeneity in the relationship between unionization and income inequality across countries. Rather, they implicitly assume that the effect of unionization on income inequality is the same for all or for certain groups of countries. Given the complexity of the relationship between these two factors and given that countries differ in economic structure, this assumption may be too restrictive. Relaxing this assumption may help to identify important differences in the effect of unionization on income inequality between countries.

3. EMPIRICAL MODEL AND DATA

The analysis will examine the relationship between unionization and income inequality using heterogeneous panel cointegration and causality techniques (i) to account for the potential cross-country heterogeneity in the effects of unionization and (ii) to explicitly test the direction of causality among the variables. In this section, we present the basic empirical model and discuss some econometric issues (Subsection a). Then, we describe the data and report some descriptive statistics (Subsection b).

(a) *Basic empirical model and econometric issues*

Following common practice in (panel) cointegration studies (Pedroni, 2007; Herzer, 2008; Chintrakarn et al., 2012), we consider a parsimonious model which includes only the two variables of empirical interest: income inequality and unionization. Specifically, the model takes the form

$$INEQUALITY_{it} = a_i + \beta UNION_{it} + \varepsilon_{it}, \tag{1}$$

where $i = 1, 2, ..., N$ is the country index, $t = 1, 2, ..., T$ is the time index, and the a_i are country-specific fixed effects, capturing any country-specific omitted factors that are relatively stable over time. $INEQUALITY_{it}$ stands for the estimated household income inequality (EHII) in Gini format and $UNION_{it}$ is the most commonly used measure of unionization or union strenght—the percentage of employed wage and salary workers who are union members, union density. As in previous studies, we do not use logs, but in the robustness section, we also present results based on log-transformed data.

Figures 3 and 4 (discussed below) show that the underlying variables are trended, i.e., they are nonstationary. Given that the behavior of most economic variables can be approximated by a stochastic, rather than a deterministic, process, we assume that the trends in $INEQUALITY_{it}$ and $UNION_{it}$ are 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 nonstationary integrated variables must be stationary, or, in the terminology of Engle and Granger (1987), $INEQUALITY_{it}$ and $UNION_{it}$ must be cointegrated. If the two variables are not cointegrated, there is no long-run relationship between inequality and unionization; Equation (1) would in this case represent a spurious regression in the sense of Granger and Newbold (1974). 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. The requirement for the above regression not to be spurious is thus that the two (integrated) variables cointegrate.[2]

A regression consisting of cointegrated variables yields superconsistent estimates of the long-run parameters. Superconsistency means that the parameter estimates are not only consistent but converge to their true values at a faster rate than is normally the case, namely rate T rather than $\sqrt{T}$ (Stock, 1987). Estimates obtained under cointegration are thus more accurate than is possible using conventional methods. In addition, as shown by Stock (1987), the estimated coefficients are superconsistent even in the presence of temporal and/or contemporaneous correlation between the stationary error term and the regressor(s). The important implication is that cointegration estimates are not biased by omitted *stationary* variables (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. On the one hand, any omitted nonstationary variable that is part of the cointegrating relationship would become part of the error

[2] 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 the data and leads to misspecification if a cointegrating relationship between the levels of the variables exists (see, e.g., Granger 1988).

term, thereby producing nonstationary residuals, and thus leading to a failure to detect cointegration (Everaert, 2011). If, on the other hand, there is cointegration between a set of variables, then this stationary relationship also exists in extended variable space. In other words, cointegration relationships are invariant to model extensions (Lütkepohl, 2007). The important implication of finding cointegration is thus that no additional variables are required to control for omitted variable bias because such a bias does not exist under cointegration. Of course, there are several nonstationary factors (such as trade and foreign direct investment) that may affect income inequality and/or union density. Adding further nonstationary variables to the model may therefore result in further cointegrating relationships—that would have to be identified and estimated. The original cointegrating relationship, however, will not be affected by the presence (or absence) of additional variables (Juselius, 2006).

A related issue is the inclusion of individual time trends in the regression. It is common practice in panel (cointegraion) studies to include country-specific deterministic time trends to control for any country-specific omitted factors that evolve smoothly over time. However, as just discussed, the finding of cointegration (without individual time trends) implies that there are no missing trending variables and that therefore no additional variables, such as time trends, are required in the model. Moreover, there is always a certain degree of collinearity between stochastic and deterministic trends (in small samples), and that, therefore, depending on the degree of collinearity, the inclusion of a time trend can lead to biased estimates. Given the collinearity concerns between $UNION_{it}$ and the deterministic time trends, our preferred specification is the model without individual time trends—that is, Equation (1). In the robustness section, however, we also estimate the model with individual time trends.

The superconsistency of the estimates of the cointegrating relationship also implies that the potential endogeneity of the regressors does 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, and its *t*-ratio is not asymptotically standard normal. As discussed above, there are good reasons to assume that union density is endogenous. To deal with this problem, we use a dynamic OLS (DOLS) estimator (discussed in more detail in Section 4) that corrects for serial correlation and the potential endogeneity of union density to estimate Equation (1).

Second, we know from the Granger representation theorem (Engle and Granger, 1987) that the existence of cointegration implies long-run Granger causality in at least one direction (Granger, 1988), but cointegration says nothing about the direction of the causal relationship between the variables. A statistically significant cointegrating relationship between $INEQUALITY_{it}$ and $UNION_{it}$ does therefore not necessarily imply that, in the long run, changes in union density cause changes in income inequality. It may well be that that causality runs in the opposite direction. The empirical implication is that it is important not only to deal with the potential endogeneity of union membership, but also to explicitly test the direction of causality.

Another important issue is the potential cross-country heterogeneity in the relationship between unionization and inequality. Countries differ in terms of economic and institutional characteristics (and other factors). The implicit assumption of traditional, homogeneous panel estimators that the coefficients on the variables of interest are the same across all countries can therefore be unduly restrictive. For this reason, we use heterogeneous panel techniques.

A final econometric issue is the potential cross-sectional dependence in the panel through common time effects. Standard panel cointegration techniques assume cross-sectional independence and can be biased if this assumption does not hold. Therefore, we not only apply standard panel cointegration procedures, but we also use recently developed panel unit roots and cointegration tests that are robust to cross-sectional dependence and we explicitly test for cross-sectional dependence in our panel cointegration regressions.

(b) *Data and descriptive statistics*

We now discuss the data employed in the empirical analysis and present some descriptive statistics. Following Visser and Checchi (2009) and Checchi and García-Peñalosa (2010), among others, we use as the dependent variable the union density rate from the ICTWSS database compiled by Jelle Visser at the Amsterdam Institute for Advanced Labor Studies at the University of Amsterdam (available at http://www.uva-aias.net/208). The union density rate is defined as "net union membership [total union members minus retired and unemployed members] as a proportion of wage and salary earners in employment" Visser (2011, p. 8).

As is well known, however, union density does not fully capture the ability of unions to influence wages. The extent of membership, or union density, reflects the ability of unions to exert pressure on employers, but in many countries union wage negotiations determine the wages of workers who are not explicitly part of the union. Consequently, collective bargaining coverage—that is, the proportion of employees covered by collective agreements (regardless of whether they are union members or not)—can, and does, differ from union density, as discussed in more detail below. Collective bargaining coverage measures the real extent to which salaried workers are subject to union-negotiated terms and conditions of employment, while union density measures the potential power of the unions in bargaining (OECD, 2004; Visser, 2006). Both factors affect the ability of unions to influence wages. Unfortunately, data on union coverage are not available for a sufficiently large number of countries over a sufficiently large period of time, so that we have to rely on the density variable to capture the impact of unionization on income inequality.

As far as data on income inequality are concerned, Alderson und Nielsen (2002) and Calderón et al. (2005) use the Gini coefficient dataset constructed by Deininger and Squire (1996).

However, it is well known since the work of Atkinson and Brandolini (2001) that the Deininger-Squire data suffer from deficiencies such as sparse coverage, problematic measurements, and the combination of diverse data types into a single dataset, thus limiting the comparability—not only across countries but also over time. Other studies therefore rely on Gini data from the Luxembourg Income Study database (see, e.g., Gustafsson and Johansson, 1999; Mahler, 2004; Checchi and García-Peñalosa, 2008) or the World Income Inequality Database (see, e.g., Checchi und García-Peñalosa, 2010). The problem with all these sources is the lack of consistent and continuous inequality data over time (Galbraith, 2009).

In this study, we use the estimated household income inequality (EHII) dataset developed by the University of Texas Inequality Project (UTIP) (available at http://utip.gov.utexas.edu/data.html). The EHII data are comparable across countries and over time (Galbraith and Kum, 2005). In addition, the EHII data are available for a reasonably large number of countries over a sufficiently long and continuous time period.

The EHII index, which is in Gini format (measured on a 0 to 100 scale), is estimated by combining information from the UTIP-UNIDO dataset with information from the Deininger-Squire dataset. The former is a set of measures of manufacturing wage inequality, using the between-groups component of a Theil index, measured across industrial categories in the manufacturing sector based on the Industrial Statistics database of the United Nations Industrial Development Organization (UNIDO). Specifically, the EHII index is constructed by regressing the Deininger-Squire Gini indices on the UTIP-UNIDO Theil inequality measures (and on several control variables), and then using the predicted values as (estimated) Gini coefficients. The intention of this procedure is to separate the useful from the doubtful information in the Deininger-Squire dataset (Galbraith and Kum, 2005).

Many of the more recent income inequality studies use the EHII Gini coefficient (Meschi and Vivarelli, 2009; Gimet and Lagoarde-Segot, 2011; Herzer and Vollmer, 2012). The inherent limitation of this index is that it is estimated, and estimates may be biased (for several reasons). In the robustness section, we therefore examine the sensitivity of the results to the measure of inequality by using top-decile income shares, thus following Scheve and Stasavage (2009) who also use top income shares as a proxy for inequality. The data we use are from Leigh (2007) (available at http://people.anu.edu.au/andrew.leigh/) who adjusts top incomes series from 13 different papers to produce a comparable dataset. Unfortunately, these data are available only for a relatively small number of countries, so that the EHII Gini is our preferred measure of income inequality.

The identification and estimation of cointegrating relationships requires the use of continuous data over a sufficiently long period of time. Panel cointegration procedures exploit both the time-series and cross-sectional dimensions of the data and can therefore be implemented with shorter data spans than their time-series counterparts. Consequently, a period of 26 years should be more than sufficient for our purposes; several panel cointegration studies are based on shorter time periods (see, e.g., Guellec and Van Pottelsberghe, 2004; Apergis et al., 2008; Apergis and Payne, 2011). We include all countries for which complete data are available over the period 1970-1995 (the longest period for which data are available for a large number of countries), resulting in a balanced panel of 520 observations on 20 countries. In the robustness section, we also estimate the model for 9 countries over the period 1963-2000 (342 observations).

Table 1 lists the countries along with the average values for $INEQUALITY_{it}$ and $UNION_{it}$ over the period 1970-1995. Israel was the country with the highest inequality, followed by Singapore, Korea, and Ireland, while Sweden had the lowest inequality level in our sample, followed by Finland, Denmark, and the United Kingdom. Union density was highest in Sweden, followed by Israel, Denmark, and Finland; the country with lowest union density was Korea, followed by the USA, Singapore, and Japan. Altogether, it appears that countries with lower union density tend to have higher inequality levels.

[Table 1]

This conclusion is supported by Figure 1, which shows the scatter plot of the period-averages of the EHII Gini coefficients versus the period-averages of the union density rates for the 20 countries in our sample along with the regression line; the slope is negative (with a *t*-value of -2.10).

[Figure 1]

Figure 2 shows the cross-sectional averages of the variables over the period 1970-1995. While income inequality declined between 1970 and 1979, there has been a positive inequality trend between 1979 and 1995. Union density shows exactly the opposite pattern: it first increased between 1970 and 1979 then decreased from 1980 to 1995. Again, this could reflect a negative association between unionization and inequality (on average in the sample).

[Figure 2] [Figure 3] [Figure 4]

Figures 3 and 4 plot the two variables for each country. As can be seen from Figure 3, the EHII Gini coefficient increased in all countries between 1970 and 1995, with the exception of Denmark, Finland, Italy, the Republic of Korea, and Singapore where inequality declined. In Finland, however, after a fall in inequality, taking the period between 1971 and the early 1980s as a whole, there has been an increasing inequality trend since about 1980 (or even earlier). Similarly, the data show an increasing trend since 1980 for Italy, while there is no clear trend in the Gini data for Denmark. Noteworthy are also the Netherlands and Sweden: for the Netherlands, the EHII data show a decreasing trend till 1981 and then an increasing trend. Similarly, Sweden's Gini coefficient first declined and then increased above the original level.

Comparison with Figure 4 shows positive trends in both the union density and the income inequality series of Belgium, Canada, Finland, Norway, and Sweden, while both series for Singapore exhibit a negative trend; this could suggest that the relationship between $INEQUALITY_{it}$ and $UNION_{it}$ is positive in these countries. In other countries, such as Australia, Austria, Ireland, Israel, Japan, New Zealand, the United Kingdom, and the United states, we observe a negative trend in union density and a positive trend in income inequality; this could be an indication that the two variables are negatively related in these countries. Finally, both positive and negative trends are apparent for union density in Germany, Italy, the Republic of Korea, Luxembourg, and the Netherlands. Altogether the pattern in Figures 3 and 4 suggests that there could be large differences in the unionization-inequality relationship across countries.

Lastly, in Table 2 union density and collective bargaining coverage rates (also from the ICTWSS database) are presented for 1990 or other years (if collective bargaining coverage data are not available for 1990). As discussed above, union membership and collective bargaining coverage rates differ in the same country in the same year. In countries with decentralized bargaining arrangements (bargaining at the firm level), such as the United States, Canada, and the United Kingdom, the union-negotiated contract applies only to union members and non-union members in the same workplace (or bargaining unit). Since workers who are covered by a union contract at their workplace may choose not to join the union that represents them, union density rates in these countries are typically somewhat lower than collective bargaining coverage rates (see also Visser and Checchi, 2009). By contrast, in most European countries, multi-employer bargaining and public policies extend the negotiated contract to non-organized firms, implying very high coverage rates that are far in excess of union density rates (Visser, 2006). Austria, Germany, and the Netherlands are examples of such countries where the bargaining coverage rate significantly exceeds the union density rate. Finally, there are also countries, such as Belgium, Denmark, Finland, Norway, and Sweden, where both the union coverage rate and the union density rate are high (union density rates are above 50 percent and union coverage rates are 70 percent or higher). Notably, four of these countries (Belgium, Finland, Norway, and Sweden) exhibit the highest degree of downward real wage rigidity according to the results of Dickens et al. (2007).[3]

[3] A reason for the high union density in some of these countries is the "Ghent system", in which unions manage the (primarily) publicly financed unemployment insurance system (Scruggs, 2002). As argued by Van Rie et al. (2011) the Ghent system provides incentives such as relatively high unemployment insurance benefits for union members that induce workers to join the unions and to discourage them from leaving, especially under conditions of rising

[Table 2]

Unions in countries with both high union density and high bargaining coverage are likely to be especially influential in affecting the labor market. It would therefore not be surprising if there are significant differences in the nature of the unionization-inequality relationship between these and other countries.

4. EMPIRICAL ANALYSIS

The pre-tests for unit roots and cointegration, which are reported in the Appendix, suggest that the variables are nonstationary and cointegrated, as assumed in Equation (1). In this section, we provide estimates of the cointegrating relationship between unionization and income inequality (Subsection a) and test the robustness of the estimates (Subsection b). We also investigate the direction of long-run causality between the two variables (Subsection c) and examine the degree of heterogeneity in the long-run effects of unions on income inequality across countries (Subsection d).

(a) *Panel cointegration estimates*

The long-run unionization-inequality coefficient for the sample is estimated using the between-dimension group-mean panel DOLS estimator that Pedroni (2001) argues has a number of advantages over the within-dimension approach. First, it allows for greater flexibility in the presence of heterogeneous cointegrating vectors, whereas under the within-dimension approach, the cointegrating vectors are constrained to be the same for each country; this is an important advantage for applications such as the present one because there is no reason to assume that the effect of unions on income inequality is the same across countries. Further, the point estimates provide a more useful interpretation in the case of heterogeneous cointegrating vectors, as they can be

unemployment and economic insecurity. The countries with the Ghent system are Denmark, Finland, and Sweden, all of interpreted as the mean value of the cointegrating vectors, which does not apply to the within estimators. In addition, between-dimension estimators suffer from much lower small-sample size distortions than is the case with the within-dimension estimators.

The DOLS regression in our case is given by

$$INEQUALITY_{it} = a_i + \beta_i UNION_{it} + \sum_{j=-k_i}^{k_i} \Phi_{ij} \Delta UNION_{it} + \mu_{it}, \tag{2}$$

where Φ_{ij} are coefficients of lead and lag differences, which account for possible serial correlation and endogeneity of the regressor(s), thus yielding unbiased estimates. Thus, an important feature of the DOLS procedure is that it generates unbiased estimates for variables that cointegrate even with endogenous regressors. In addition, the DOLS estimator is superconsistent under cointegration, and it is also robust to the omission of variables that do not form part of the cointegrating relationship.

From Regression (3), the group-mean DOLS estimator for β is constructed as

$$\hat{\beta} = \left[N^{-1} \sum_{i=1}^{N} \left(\sum_{t=1}^{T} z_{it} z'_{it} \right)^{-1} \left(\sum_{t=1}^{T} z_{it} \tilde{s}'_{it} \right) \right]_1, \tag{3}$$

where z_{it} is the $2(K+1) \times 1$ vector of regressors z_{it} = ($UNION_{it} - \overline{UNION}_i, \Delta UNION_{it-K}$, …, $\Delta UNION_{it+K}$), $\tilde{s}_{it} = s_{it} - \bar{s}_i$, and the subscript 1 outside the brackets indicates that only the first element of the vector is taken to obtain the pooled slope coefficient. Because the expression following the summation over the *i* is identical to the conventional time-series DOLS estimator, the between-dimension estimator for β can be calculated as

$$\hat{\beta} = N^{-1} \sum_{i=1}^{N} \hat{\beta}_i, \tag{4}$$

where

$$t_{\hat{\beta}} = N^{-1/2} \sum\nolimits_{i=1}^{N} t_{\hat{\beta}_i} \tag{5}$$

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whom have increased density throughout the period 1970-1995.

is the corresponding t-statistic of $\hat{\beta}_i$, and $\hat{\beta}_i$ is the conventional DOLS estimator applied to the ith country of the panel. The DOLS t-statistic calculated using heteroskedasticity and autocorrelation-consistent (HAC) standard errors has a standard normal distribution.

Accordingly, implementing the mean-group DOLS estimator involves two steps (Perdoni, 2001). The first is to estimate country-specific DOLS regressions (using HAC robust standard errors). The second step is to average the coefficients from the individual country regressions and test the statistical significance of the average coefficient(s).

Given the short time-series dimension of the data, we use one lead and lag in all DOLS regressions to preserve degrees of freedom, as is common practice in small T samples (see, for example, Spilimbergo and Vamvakidis, 2003; Thorbecke and Smith, 2010; Herzer et al., 2012).

However, the DOLS procedure does not account for potential cross-sectional dependence in the residuals induced by common shocks that influence all panel units at the same time. Common time effects may lead to inconsistent coefficient estimates if they are correlated with the explanatory variables. In order to exclude the possibility of bias due to cross-sectional dependence, we compute Pesaran's (2004) CD test for cross-sectional dependence, which is based on an average of the pair-wise correlations of the residuals from the individual regressions in the panel. The CD test statistic is defined as

$$CD = \sqrt{\frac{2T}{N(N-1)}}\left(\sum_{i=1}^{N-1}\sum_{j=i+1}^{N}\hat{\rho}_{ij}\right), \tag{6}$$

where

$$\hat{\rho}_{ij} = \hat{\rho}_{ji} = \frac{\sum_{t=1}^{T}\hat{\mu}_{it}\hat{\mu}_{jt}}{\left(\sum_{t=1}^{T}\hat{\mu}_{it}^2\right)^{\frac{1}{2}}\left(\sum_{t=1}^{T}\hat{\mu}_{jt}^2\right)^{\frac{1}{2}}} \tag{7}$$

is the sample estimate of the pair-wise correlation of the residuals, and $\hat{\mu}_{it}$ are the estimated (DOLS) residuals (computed for each i separately). The CD test statistic is normally distributed under the null hypothesis of no cross-sectional dependence.

As can be seen from column 1 of Table 3, the null hypothesis of no cross-sectional dependence is not rejected with a p-value of 0.32. This is consistent with the study of Bradley et al. (2003) who also found no evidence of common time effects in their inequality data set.

[Table 3]

The DOLS group-mean point estimate of the effect of unions on inequality is presented in column 1 of Table 3. The effect is highly significant, and the point estimate implies that a decrease in union density of one percent increases the Gini coefficient by 0.081 units. Thus, we find, on average across the countries in our sample, that unions decrease income inequality.

(b) *Robustness*

We perform several robustness checks. First, we examine whether the negative effect of unionization on inequality is robust to different estimation techniques. Specifically, we adopt Pedroni's (2001) group-mean fully modified OLS (FMOLS) estimator. While the DOLS estimator employs a parametric correction for endogeneity and serial correlation using leads, lags, and contemporaneous values of the differenced $I(1)$ regressors, the FMOLS estimator incorporates a semi-parametric correction to the OLS estimator to eliminate endogeneity and serial correlation bias based on the OLS residuals and the first differences of the regressors. Like the group-mean DOLS estimator, the group-mean FMOLS estimator allows the slope coefficients to vary across countries. The FMOLS results are reported in column 2 of Table 3. The CD test does not reject the null hypothesis of no cross-sectional dependence in the residuals, and the coefficient on union density is negative and highly significant. From this we conclude that our results are robust to alternative estimation methods.

Although simulation evidence suggests that the DOLS estimator performs better than the FMOLS estimator (Stock and Watson, 1993; Kao and Chiang, 2000; Caporale and Cerrato, 2006; Wagner and Hlouskova, 2010), the results of the DOLS procedure may be sensitive to the number of leads and lags included. As noted above, we use just one lead and lag in the baseline regression to preserve degrees of freedom. To assess whether our baseline results are sensitive to this decision, we re-estimate the DOLS regression using two leads and lags. The results are presented in column 1 of Table 4. As can be seen, the coefficient is almost identical to our baseline coefficient in Table 3.

[Table 4]

In Table 4, we also test the robustness of our results to the inclusion of individual time trends (column 2), the use of log-transformed data (column 3), and the use of an alternative measure of inequality (column 4). As far as the latter is concerned, we employ the top decile income share series ($TopDecile_{it}$) from Leigh (2007) over the period 1961-1996 for 9 countries.[4] Regrettably, balanced panel data for more countries are not available for a sufficiently long time period. As can be seen, all coefficients suggest that union density has a statistically significant negative effect on inequality.

Given the relatively small number of countries in our sample, we also need to ensure that the estimated effects are not due to individual outliers. To this end, we re-estimate the DOLS regression, excluding one country at a time from the sample. The sequentially estimated coefficients and their *t*-statistics are presented in Figure 5. Each number on the horizontal axes represents the country omitted from DOLS regression; on the vertical axes we plot the respective coefficients and *t*-statistics of the explanatory variables in the remaining sample of 19 countries. As can be seen, the estimated coefficients are relatively stable and always significant at the one-percent level, suggesting that our results are not due to potential outliers.

[Figure 5]

[4] The countries are: Australia, Canada, France, Germany, the Netherlands, Sweden, Switzerland, the UK, and the USA.

Finally, we study the sensitivity of our results to the period length. For this purpose, the DOLS regression is re-estimated over the longest period for which data were available, 1963-2000. We include all countries with complete data over this period (9 countries).[5] As Table 5 shows, the results do not change qualitatively when a longer period is considered, consistent with the sensitivity test using the top income share over the period 1961-1996 (see column 4 of Table 4).

[Table 5]

(c) *Long-run causality*

The above interpretation of the estimation results is based on the assumption that long-run causality runs from union density to income inequality. 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, we enter the residuals from the individual long-run relationships (estimated by DOLS),

$$ec_{it} = INEQUALITY_{it} - [\hat{a}_i + \hat{\beta}_i\, UNION_{it}], \tag{8}$$

as error-correction terms into a panel vector error correction model (VECM) of the form

$$\begin{bmatrix} \Delta INEQUALITY_{it} \\ \Delta UNION_{it} \end{bmatrix} = \begin{bmatrix} c_{1i} \\ c_{2i} \end{bmatrix} + \sum_{j=1}^{k_i} \Gamma_{ij} \begin{bmatrix} \Delta INEQUALITY_{it-j} \\ \Delta UNION_{it-j} \end{bmatrix} + \begin{bmatrix} a_{1i} \\ a_{2i} \end{bmatrix} ec_{it-1} + \begin{bmatrix} \varepsilon_{1it} \\ \varepsilon_{2it} \end{bmatrix}, \tag{9}$$

where the error-correction term, ec_{it-1}, represents the error in, or deviation from, the equilibrium, and the adjustment coefficients a_{1i} and a_{2i} capture how $INEQUALITY_{it}$ and $UNION_{it}$ respond to deviations from the equilibrium relationship. From the Granger representation theorem (Engle and Granger, 1987) it follows that at least one of the adjustment coefficients must be non-zero if a long-run relationship between the variables is to hold. A significant error-correction term also suggests long-run Granger causality, and thus, long-run endogeneity (Hall and Milne, 1994), whereas a non-

[5] The countries are: Australia Canada, Finland, Ireland, Japan, Korea, Netherlands, Norway, and Sweden.

significant adjustment coefficient implies long-run Granger non-causality from the independent to the dependent variable(s), as well as weak exogeneity.

To allow for complete heterogeneity in the adjustment coefficients and short-run dynamics, we follow Herzer (2013) and test for weak exogeneity of the variables (and thus for long-run Granger non-causality between union density and income inequality) by estimating the VECM separately for each country. For each country, the lag length of the short-run dynamics is selected using the "t-sig" approach suggested by Perron (1997). More specifically, we set an upper bound of $k = 3$ for the lag length and test down until a significant (at the five-percent level) lag is found. The panel weak exogeneity test is then conducted using the Fisher statistic proposed by Madalla and Wu (1999). This statistic is defined as

$$\lambda = -2\sum_{i}^{N} \log(p_i), \tag{10}$$

where p_i is the p-value of the standard likelihood ratio test of the null hypothesis $a_{1,2} = 0$ for country i. The Fisher statistic is distributed as χ^2 with $2 \times N$ degrees of freedom.

Column 1 of Table 6 presents the results for the sample as a whole. In column, 2 we also report weak exogeneity tests for a subsample of high-density, high-coverage countries (discussed in more detail in the next subsection). As far our total sample is concerned, the Fisher statistic rejects the null hypothesis of weak exogeneity for both $INEQUALITY_{it}$ and $UNION_{it}$ at the one-percent significance level, implying that the statistical long-run causality is bidirectional, on average. From this it can be concluded that, on average in the sample of countries considered, increased inequality is both a consequence and a cause of decreased unionization.

[Table 6]

(d) *Estimates for individual countries*

The results reported thus far indicate that union density has, on average, a negative long-run effect on income inequality (and vice versa). This finding for the sample as a whole does not imply, however, that unions exert inequality-reducing effects in each individual country. Figure 6 plots the individual country DOLS estimates of the coefficients on $UNION_{it}$, $\hat{\beta}_i$.

[Figure 6]

Although these estimates must be interpreted with caution given the relatively limited number of observations for each country, it can be concluded that there is considerable heterogeneity in the effects of union density on income inequality across countries. The coefficients range from –0.44 in South Korea to +0.38 in Singapore. Thus, unions do not have an equalizing effect on income distribution in all countries. Of the 20 countries in our sample, there are seven—Belgium, Canada, Denmark, Finland, Norway, Singapore, and Sweden—for which we find positive unionization-inequality coefficients.

The reasons why unions contribute to inequality may be complex and multifactorial. It is therefore difficult (if not impossible) to find an adequate explanation for the magnitude and sign of each individual country coefficient, and an attempt to do so would go beyond the scope of this paper. What is striking, however, is that in all countries where both the union coverage rate and the union density rate are high—Belgium, Denmark, Finland, Norway, and Sweden—, the relationship between unionization and inequality is positive.

To examine whether this positive relationship in fact reflects a causal effect of unionization on income inequality (and not vice versa), we perform the long-run weak exogeneity test described in Subsection c for these high-density, high-coverage countries. As can be seen from column 2 of Table 6, union density can be regarded as weakly exogenous while the weak exogeneity hypothesis of income inequality is again decisively rejected. From this it can be concluded that union density exerts a positive, unidirectional long-run influence on income inequality in high-density, high-coverage countries.

One explanation for this result could be the presence of downward wage rigidity in these countries. Recent evidence suggests that unemployment is positively related to downward wage rigidity (Fehr and Goette, 2005; Bauer et al, 2007) and that downward wage rigidity, in turn, is positively related to both union density (see, e.g., Dickens et al., 2007; Holden and Wulfsberg, 2008, 2009) and bargaining coverage (Messina et al., 2010). Similarly, Layard et al, (2005, p. XV) argue that, "greater union power and coverage can be expected to exert upward pressure on wages, hence raising equilibrium unemployment". Thus, the combination of high union density and extensive bargaining coverage could, through inducing wage rigidity, have strong negative long-run effects on employment that cause income inequality to rise.

5. CONCLUSION

The purpose of this study was: (i) to provide new evidence on the average effect of unions on income inequality across countries; (ii) to study the direction of causality between unionization and income inequality; and (iii) to examine the degree of heterogeneity in the effects of unions on income inequality across countries. To this end, heterogeneous panel cointegration techniques (robust to omitted variables and endogenous regressors) were applied to a sample of 20 countries. We found that union density has, on average, a negative long-run association with income inequality. This finding is robust to alternative estimation techniques, different empirical specifications, alternative measures of income inequality, potential outliers, and the length of the sample period used. Our results also show that the causality of the average long-run relationship between unionization and income inequality is bidirectional, suggesting that, on average or in general, an increase in unionization reduces income inequality and that, in turn, higher inequality leads to lower union density. However, there are large differences in the long-run unionization-inequality coefficient across countries. More specifically, we found that a decrease in unionization is associated with an increase in income inequality in 65 percent of the countries, while in the remaining 35 percent of the cases, the association is positive.

What is striking is that the relationship between unionization and inequality is positive in all those countries where both the union coverage rate and the union density rate are high (Belgium, Denmark, Finland, Norway, and Sweden). A possible explanation for this result could be that the combination of high union density and extensive bargaining coverage induces wage rigidity and thereby unemployment, which in turn causes a rise in income inequality. In this sense, this result is consistent with those of Checchi and García-Peñalosa (2010) who find that union density increases income inequality due to negative employment effects.

The general conclusion from our results is that unionization has, on average, a negative long-run causal effect on income inequality, but there is considerable heterogeneity in the effect of unionization on income inequality across countries. A natural question is: What factors determine the long-run effect of unionization on income distribution? One factor could be bargaining coverage. But differences in bargaining coverage are certainly not the only explanatory factor for the differences in the effects of union density across countries; many other factors (such as bargaining centralization, the generosity of unemployment benefits, minimum wage laws, etc.) are potentially important for some, but not necessarily all, countries. We leave this issue for future research.

Appendix A1. Panel unit root tests

To examine the time-series properties of the data, we use the heterogeneous panel unit root test developed by Im, Pesaran, and Shin (2003) (IPS). This test is based on the augmented Dickey-Fuller (ADF) regression

$$\Delta x_{it} = z_{it}'\gamma + \rho_i x_{it-1} + \sum_{j=1}^{k_i} \varphi_{ij} \Delta x_{it-j} + \varepsilon_{it}, \qquad \text{(A.1)}$$

where k_i is the lag order, z_{it} represents deterministic terms, such as fixed effects or fixed effects and individual time trends, and ρ_i are country-specific first-order autoregressive parameters. The null hypothesis is that each series has a unit root, $H_0 : \rho_i = 0$ for all i, while the alternative hypothesis is that at least one of the individual series in the panel is (trend) stationary, $H_1 : \rho_i < 0$ for at least one i. Accordingly, the individual first order autoregressive coefficients are allowed to vary under the alternative hypothesis by estimating the ADF equation separately for each country. The unit root null hypothesis is tested against the alternative of (trend) stationary using the standardized t-bar statistic

$$\Gamma_{\bar{t}} = \frac{\sqrt{N}[\bar{t}_{NT} - \mu]}{\sqrt{v}}, \tag{A.2}$$

where $\bar{t}_{NT}$ is the average of the individual ADF t-statistics, and μ and v are, respectively, the mean and variance of the average of the individual t-statistics, tabulated by Im et al. (2003). The standardized t-bar statistic converges to a standard normal distribution as N and $T \to \infty$.

However, the standard IPS test can lead to spurious inferences if the errors, ε_{it}, are not independent across i (for example, due to common shocks or spillovers between countries). Therefore, we also employ the cross-sectionally augmented IPS test proposed by Pesaran (2007), which is designed to filter out the (potential) cross-sectional dependence by augmenting the ADF regression with the cross-sectional averages of lagged levels and first-differences of the individual series. Accordingly, the cross-sectionally augmented ADF (CADF) regression is given by

$$\Delta x_{it} = z_{it}'\gamma + \rho_i x_{it-1} + \sum_{j=1}^{k_i} \varphi_{ij} \Delta x_{it-j} + \alpha_i \bar{x}_{t-1} + \sum_{j=0}^{k_i} \eta_{ij} \Delta \bar{x}_{t-j} + v_{it}, \tag{A.3}$$

where $\bar{x}_t$ is the cross-section mean of x_{it}, $\bar{x}_t = N^{-1} \sum_{i=1}^{N} x_{it}$. The cross-sectionally augmented IPS (CIPS) statistic is the simple average of the individual CADF statistics:

$$CIPS = t\text{-bar} = N^{-1} \sum_{i=1}^{N_i} t_i, \tag{A.4}$$

where t_i is the OLS t-ratio of ρ_i in Equation (A.3). Critical values are tabulated by Pesaran (2007).

Table A.1 reports the results of these tests for the variables in levels and in first differences. The test statistics do not reject the null hypothesis that $INEQUALITY_{it}$ and $UNION_{it}$ have a unit root in levels, whereas the unit root hypothesis is rejected for the first differences, implying that both $INEQUALITY_{it}$ and $UNION_{it}$ are integrated of order one, $I(1)$. This is consistent with results of Herzer and Vollmer (2012) and Checci and Visser (2005); while the former find that the EHII Gini coefficient is a nonstationary $I(1)$ process, the latter report that the union density rate is $I(1)$.

[Table A.1]

Appendix A2. Panel cointegration tests

We test for cointegration using the Larsson et al. (2001) approach, which is based on the Johansen (1988) full information maximum likelihood procedure. Like the Johansen time-series cointegration test, the Larsson et al. panel test treats all variables as potentially endogenous, thus avoiding the normalization problems inherent in residual-based cointegration tests. The Larsson et al. approach involves estimating the Johansen vector-error-correction model for each country separately and then computing the individual trace statistics $LR_{iT}\{H(r)|H(p)\}$; this allows us to account for heterogeneous cointegrating vectors across countries. The null hypothesis is that all countries have the same number of cointegrating vectors r_i among the p variables, $H_0: rank(\Pi_i) = r_i \leq r$, and the alternative hypothesis is $H_1: rank(\Pi_i) = p$, for all $i = 1, \ldots, N$, where Π_i is the long-run matrix of order $p \times p$. To test H_0 against H_1, a panel cointegration rank trace test is constructed by calculating the average of the N individual trace statistics,

$$\overline{LR}_{NT}\{H(r)|H(p)\} = \frac{1}{N}\sum_{i=1}^{N} LR_{iT}\{H(r)|H(p)\}, \tag{A.5}$$

and then standardizing it as follows:

$$\Psi_{\overline{LR}}\{H(r)|H(p)\} = \frac{\sqrt{N}\left(\overline{LR}_{NT}\{H(r)|H(p)\} - E(Z_k)\right)}{\sqrt{Var(Z_k)}} \Rightarrow N(0, 1). \tag{A.6}$$

The mean $E(Z_k)$ and variance $Var(Z_k)$ of the asymptotic trace statistic are tabulated by Breitung (2005) for the model we use (the model with a constant in the cointegrating vector and a linear trend in the data). As shown by Larsson et al. (2001), the standardized panel trace statistic has an asymptotic standard normal distribution as N and $T \rightarrow \infty$.

However, it is well-known that the Johansen trace statistics are biased toward rejecting the null hypothesis in small samples. To avoid the Larsson et al. test, as a consequence of this bias, also overestimating the cointegrating rank, we follow Herzer et al. (2012) and compute the standardized panel trace statistics based on small-sample corrected country-specific trace statistics. Specifically, we use the small-sample correction factor suggested by Reinsel and Ahn (1992) to adjust the individual trace statistics as follows:

$$LR_{iT}\{H(r)|H(p)\}\times\left[\frac{T-k_i\times p}{T}\right], \tag{A.7}$$

where k_i is the lag length of the models used in the test.

A potential problem with Larsson et al. approach, however, is that it does not take into account potential error cross-sectional dependence, which could bias the results. To test for cointegration in the presence of possible cross-sectional dependence we follow Holly et al. (2010) and adopt a residual-based two-step approach in the style of Pedroni (1999). Unlike Pedroni, we use the common correlated effects (CCE) estimation procedure developed by Pesaran (2006) in the first-step regression. Like the cross-sectionally augmented IPS test, the CCE estimator allows for cross-sectional dependencies that potentially arise from multiple unobserved common factors and permits the individual responses to these factors to differ across countries. In our case, the cross-sectionally augmented cointegrating regression (for the ith cross-section) is given by

$$Inequality_{it} = a_i + \beta_i Union_{it} + g_{i1}\overline{Inequality_t} + g_{i2}\overline{Union_t} + e_{it}, \tag{A.8}$$

where the cross-section averages $\overline{Inequality_t} = N^{-1}\sum_i^N Inequality_{it}$ and $\overline{Union_t} = N^{-1}\sum_i^N Union_{it}$ serve as proxies for the unobserved factors. In the second step, we compute the cross-sectionally augmented IPS statistic for the residuals from the individual CCE long-run relations, $\hat{\mu}_{it} = Inequality_{it} - \hat{\beta}_{it} Union_{it}$, including an intercept. This allows us to account for unobserved common factors that could be correlated with the observed regressors in both steps. If the presence of a unit root in $\hat{\mu}_{it}$ is rejected, it can be concluded that there is a cointegrating relationship between income inequality and unionization.

The results of these tests are presented in Table A.2. For completeness, we also report the standard panel and group rho, PP, and ADF statistics suggested by Pedroni (1999, 2004). As can be seen, all tests suggest that $INEQUALITY_{it}$ and $UNION_{it}$ are cointegrated. The standardized trace statistics clearly support the presence of one cointegrating vector. Also, the CIPS, the rho, the PP, and the ADF statistics reject the null hypothesis of no cointegration at least at the 5% level, implying that there exists a long-run relationship between income inequality and union density.

[Table A.2]

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Table 1. Countries and summary statistics

	Average of $INEQUALITY_{it}$	Average of $UNION_{it}$
Australia	33.46	44.32
Austria	34.63	53.05
Belgium	36.30	51.82
Canada	35.99	34.23
Denmark	30.94	73.83
Finland	31.26	68.99
Germany	32.70	33.59
Ireland	37.47	59.51
Israel	40.36	77.27
Italy	35.14	43.31
Japan	36.63	29.88
Korea, Rep. of	38.93	14.68
Luxembourg	32.92	47.91
Netherlands	32.51	31.05
New Zealand	34.99	54.25
Norway	32.18	56.37
Singapore	39.09	20.25
Sweden	27.54	79.10
United Kingdom	29.69	44.81
USA	37.19	19.79

Table 2. Union density and collective bargaining coverage in 1990 or other years (in parenthesis)

	Union density	Collective bargaining coverage
Australia	39.55	80.00
Austria	46.93	98.00
Belgium	53.94	96.00
Canada	34.02	38.00
Denmark	75.34	84.00
Finland	72.55	81.00
Germany	31.22	72.00
Ireland	56.75	60.00
Israel	84.00 (1982)	82.00 (1982)
Italy	38.81	83.00
Japan	26.13	23.00
Korea	10.85 (2002)	11.00 (2002)
Luxembourg	46.36	60.00
Netherlands	24.34	82.00
New Zealand	48.83	61.00
Norway	58.53	70.00
Singapore	18.10 (2006)	17.50 (2006)
Sweden	81.79 (1991)	89.00 (1991)
United Kingdom	39.32	54.00
USA	15.45	18.30

Table 3. Estimates of the long-run effect of unions on income inequality

Independent variable	(1) Group-mean DOLS estimator (Pedroni, 2001)	(2) Group-mean FMOLS estimator (Pedroni, 2001)
$UNION_{it}$	-0.081**	-0.067**
	(-7.51)	(-9.43)
CD statistic (p-value)	0.98 (0.32)	1.57 (0.12)
No. of observations	460	500

Note: The dependent variable is $INEQUALITY_{it}$. *t*-statistics are in parenthesis. ** indicate significance at the one-percent level. The DOLS regression was estimated with one lead and one lag.

Table 4. DOLS estimates with two leads and lags, individual time trends, log-transformed data, and the top decile income share as an alternative measure of inequality

Independent variable	(1) With two leads and lags Dependent variable: $INEQUALITY_{it}$	(2) With individual time trends Dependent variable: $INEQUALITY_{it}$	(3) With log-transformed data Dependent variable: log($INEQUALITY_{it}$)	(4) With an alternative measure of inequality Dependent variable: $TopDecile_{it}$
$UNION_{it}$	-0.080**	-0.096**		-0.119**
	(-7.84)	(-3.56)		(-4.44)
$Log(UNION_{it})$			-0.086**	
			(-6.73)	
Leads and lags	2	1	1	1
Time trends	No	Yes	No	No
No. of observations	420	460	460	324

Note: *t*-statistics are in parenthesis. ** indicate significance at the one-percent level.

Table 5. DOLS estimation over the period 1963-2000

Independent variable	Mean group estimate	No. of observations
$UNION_{it}$	-0.135**	315
	(-5.77)	

Note: The dependent variable is $INEQUALITY_{it}$. *t*-statistics are in parenthesis. ** indicate significance at the one-percent level. The DOLS regression was estimated with one lead and one lag.

Table 6. Weak exogeneity tests / long-run causality tests

Variable (Coefficient)	(1) Total sample	(2) Subsample of high-density, high-coverage countries
$INEQUALITY_{it}$	181.32	63.65
(α_1)	(0.000)	(0.000)
$UNION_{it}$	93.05	15.65
(α_2)	(0.000)	(0.110)

Note: The reported statistics are Fisher statistics, which are distributed as χ^2 with $2 \times N$ degrees of freedom. *p*-values are in parentheses. The models were estimated with up to three lags. The subsample of high-density, high-coverage countries includes Belgium, Denmark, Finland, Norway, and Sweden.

Table A.1. Panel unit root tests

Variable	Deterministic terms	IPS statistics	CIPS statistics
Levels			
$INEQUALITY_{it}$	c, t	0.108	-2.341
$UNION_{it}$	c, t	-0.336	-2.116
First differences			
$\Delta INEQUALITY_{it}$	c	-5.427**	-2.575**
$\Delta UNION_{it}$	c	-3.191**	-2.234*

Note: c (t) indicates that we allow for different intercepts (and time trends) for each country. Three lags were selected to adjust for autocorrelation. The IPS statistic is distributed as $N(0, 1)$. The relevant 1% (5%) critical value for the CIPS statistics is -2.88 (-2.27), with an intercept and a linear trend, and -2.38 (-2.20) with an intercept. ** (*) denote significance at the 1% (5%) level.

Table A.2. Panel cointegration tests

	Cointegration rank	
	$r = 0$	$r = 1$
Standardized panel trace statistics; $\Psi_{\overline{LR}}\{H(r)\|H(2)\}$	5.64**	1.28
CIPS statistic	-2.96**	
Panel rho statistic	-4.24**	
Panel PP statistic	-5.35**	
Panel ADF statistic	-2.64**	
Group rho statistic	-2.53*	
Group PP statistic	-4.58**	
Group ADF statistic	-2.90**	

Note: The panel trace, rho, PP, and ADF statistics are asymptotically normally distributed. Under the alternative hypothesis, the panel trace statistic diverges to positive infinity so that the right tail of the normal distribution is used to reject the null hypothesis. The relevant 5% (1%) critical value for the CIPS statistic is -2.20 (-2.38). The panel statistics pool the autoregressive coefficients across different countries during the unit root test on the residuals of the static cointegrating regression, whereas the group statistics are based on averaging the individually estimated autoregressive coefficients for each country. The panel ADF statistic is analogous to the Levin et al. (2002) panel unit root test. The group ADF statistic is analogous to the IPS panel unit root test. The rho and PP statistics are panel versions of the Phillips and Perron (PP) rho statistic and t-statistic, respectively. One lag was used in all tests. ** (*) indicate a rejection of the null hypothesis of no cointegration at the 1% (5%) level.

Figure 1. Scatter plot of income inequality versus union density

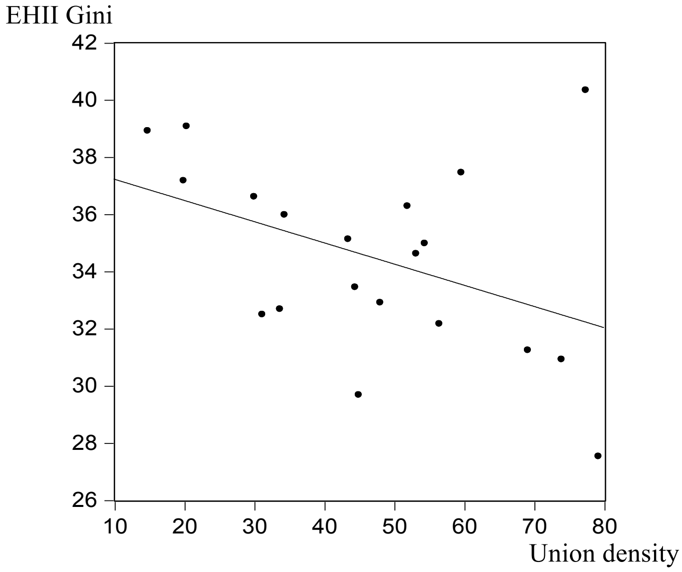


Figure 2. Cross-sectional averages of the variables, 1970-1995

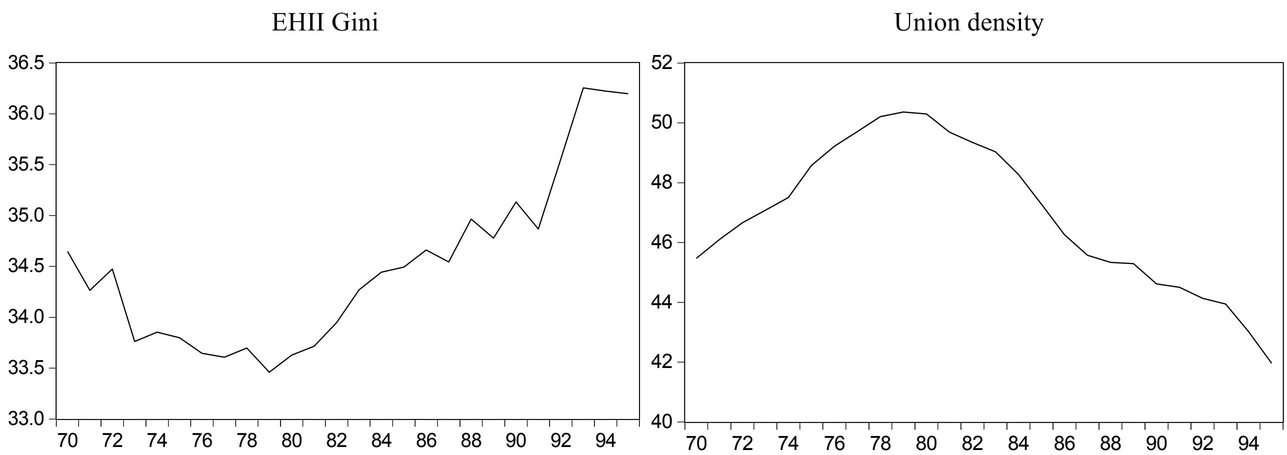
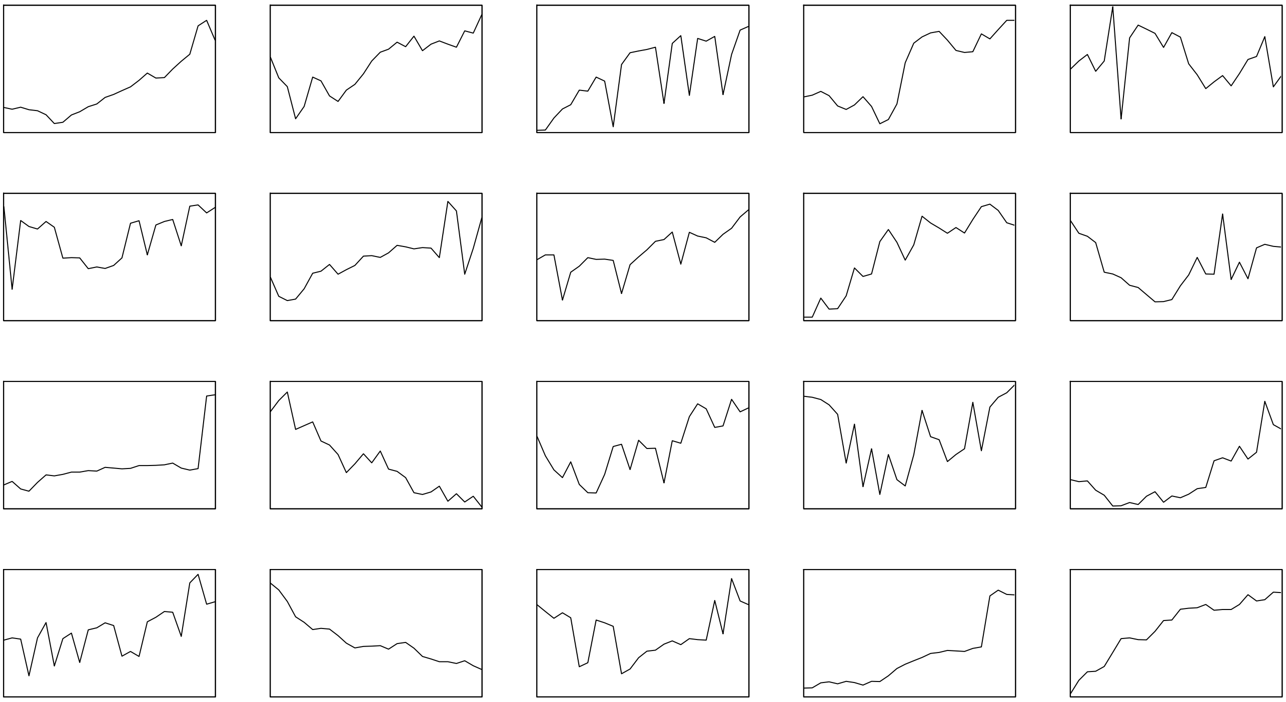
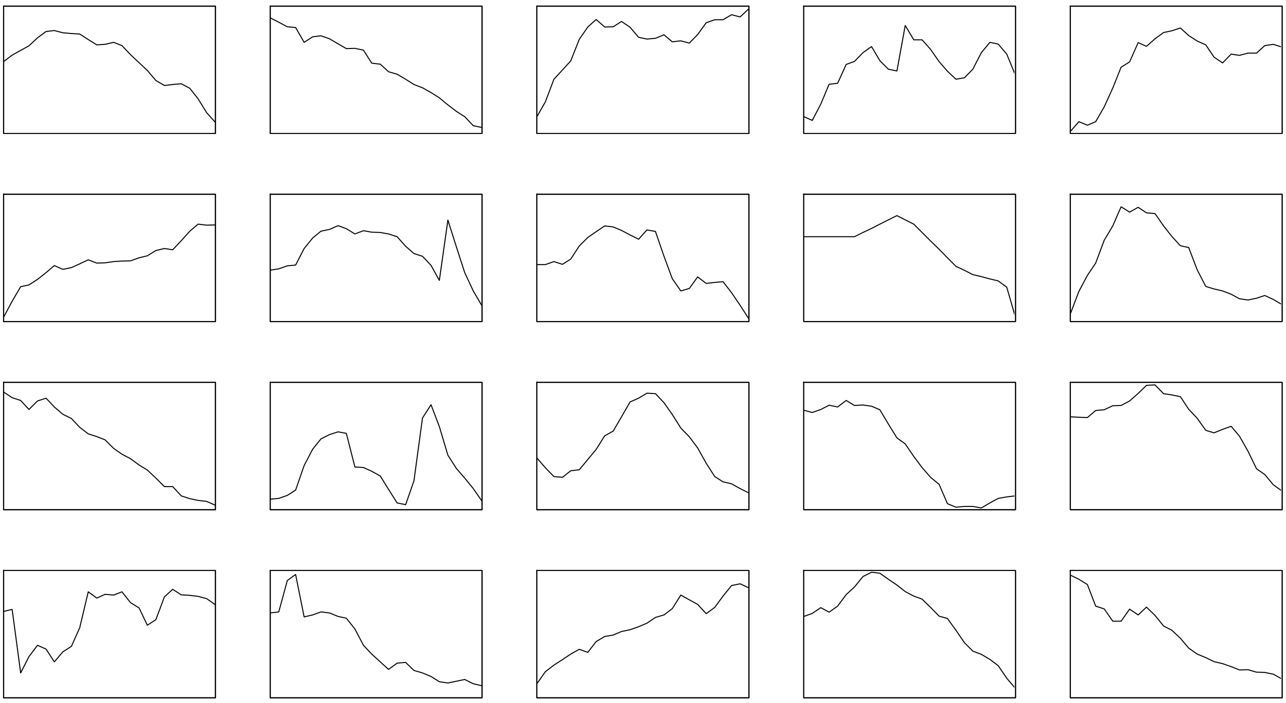


Figure 3. EHII Gini index by country over the period 1970-1995, $INEQUALITY_{it}$



Notes: The countries from left to right are: Australia, Austria, Belgium, Canada, Denmark, Finland, Germany, Ireland, Israel, Italy, Japan, Republic of Korea, Luxembourg, Netherlands, New Zealand, Norway, Singapore, Sweden, UK, and USA.

Figure 4. Union density by country over the period 1970-1995, $UNION_{it}$



Notes: The countries from left to right are: Australia, Austria, Belgium, Canada, Denmark, Finland, Germany, Ireland, Israel, Italy, Japan, Republic of Korea, Luxembourg, Netherlands, New Zealand, Norway, Singapore, Sweden, UK, and USA.

Figure 5. DOLS estimation with single country excluded from the sample

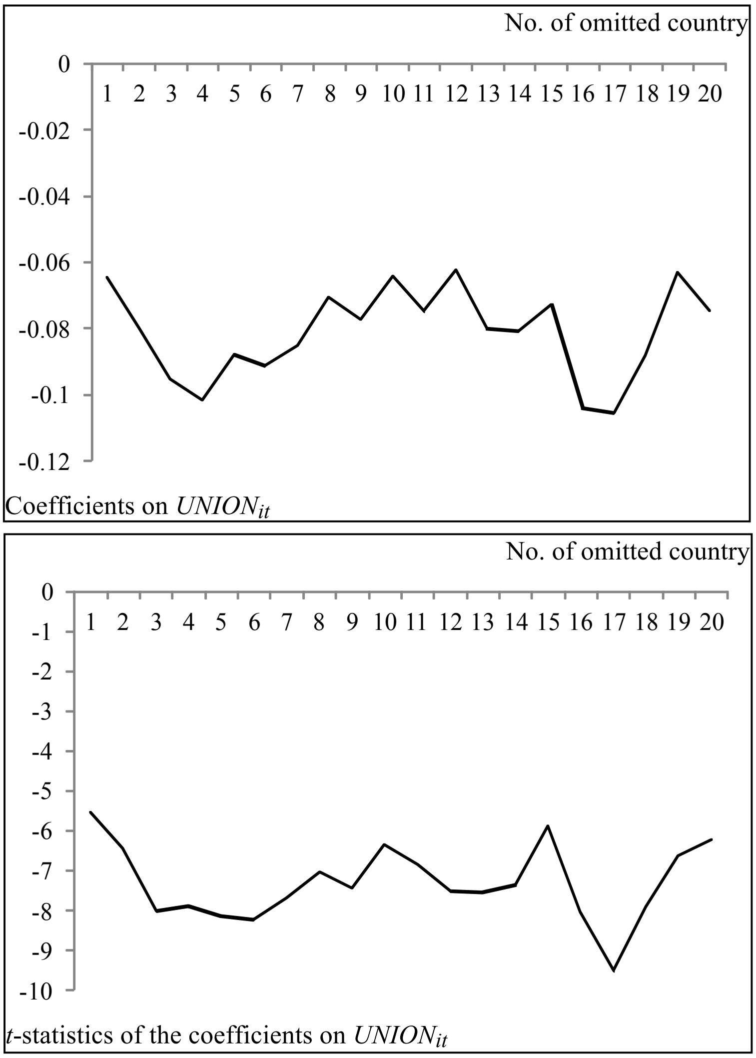
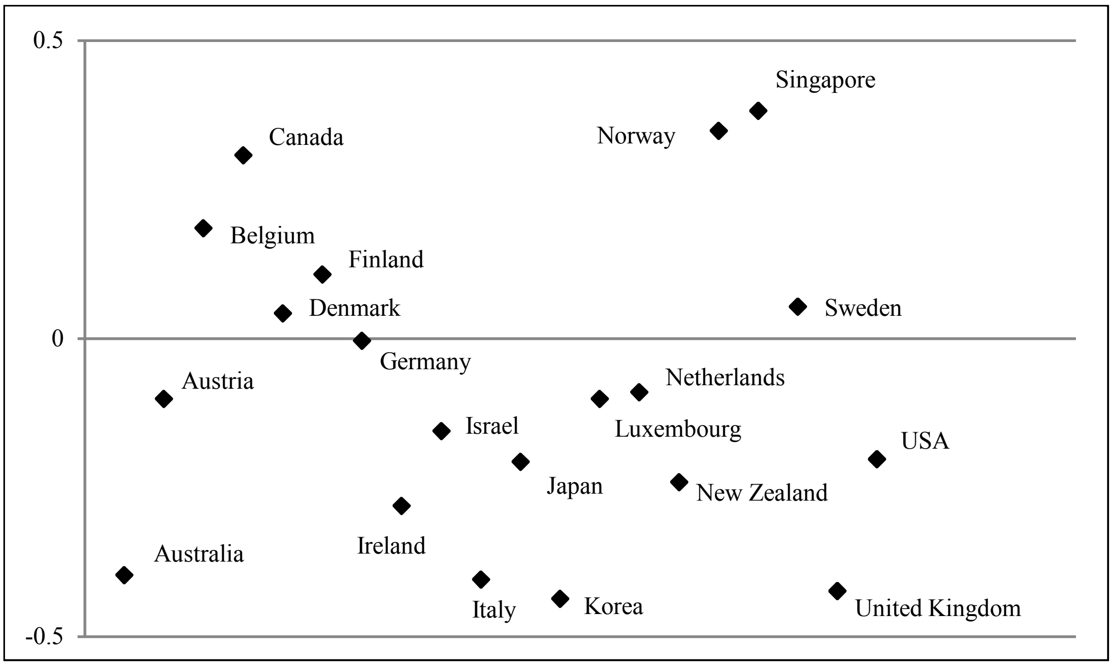


Figure 6. Individual country DOLS estimates of the long-run effect of unions on inequality



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