Lower Volatility, Higher Inequality: Are They Related?

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Abstract

This thesis is divided into three chapters. In the first chapter, I identify and explore the fundamental relationship between income inequality and GDP volatility. I give theoretical insight of this relationship alongside empirical evidence from a sample of industrialized countries. In the second chapter, in regression estimates relating inequality to the variables of interest, I suggest that rather than aggregate inequality, the average growth rate of within-cohort inequality data should be used. In the light of my findings I then try to explore the effect of international trade on inequality in the US and UK. In the last chapter, I carry out a Monte Carlo study. This compares efficiencies of impulse response matching and GMM estimators at identifying reduced form coefficients and structural parameters on a DSGE model.

Resumen

Esta tesis está dividida por tres capítulos. En el primer capítulo, llevo al interés que hay una relación fundamental entre la desigualdad de ingresos y la volatilidad de PBI. Doy pruebas teóricas para esta relación, así como empíricas de una muestra de países industrializados. En el segundo capítulo, sugiero que mejor que la desigualdad agregada, la tasa de crecimiento media de dentro de desigualdades de cohorte debería estar usada en las estimaciones de regresión que relaciona la desigualdad con las variables del interés. Entonces trato de explorar el efecto del comercio internacional en la desigualdad en los EE.UU y en el Reino Unido a la luz de mis conclusiones. En el último capítulo, realizo un estudio de Monte Carlo para comparar la eficiencia de la Correspondencia de respuesta de Impulso y peritos GMM en la identificación de los coeficientes de forma reducidos y parámetros estructurales en un modelo de DSGE.

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Preface

This thesis is divided into three chapters. In the first chapter, I bring to attention the fundamental relationship between income inequality and GDP volatility. This relationship arises from the fact that both of the variables have a common root: individuals' incomes. Income inequality, being a measure of the dispersion of individuals' incomes, already uses individual income data. On the other hand, GDP, that is aggregate income, can also be seen as a collection of individuals' incomes. Not only this basic relation, I additionally show that identical dynamics of the individual income data lie behind both variables. Empirical evidence supporting this is found from a sample of industrialized countries. It shows that there have been simultaneous changes in volatility and inequality across these countries. Recognizing the relationship between variables, among other things, is important, as one of the measures to evaluate the welfare effect of macro policies is their effect on GDP volatility. If volatility and inequality are related, inequality outcomes of these policies should also be taken into account.

This is the first study that brings these two literatures together, and it also helps us to understand the recent changes in data. Concerning these changes, the recurring pattern across industrialized countries is an increase in inequality. To explain these phenomena, the income inequality literature uses the increase in the variance of individuals' income shocks, as this leads to more dispersed incomes and higher inequality. However, if this was the sole cause we would expect aggregate volatility to increase as well. This is contrary to the observed general decline in volatility, called the Great Moderation. Hence, I claim that it is the decline in the correlation of the individuals' income shocks which is responsible for changes in both data, and find empirical evidence favouring it from US data.

The second chapter was inspired by a finding from the first chapter. I discovered evidence showing that changes in the structure of the economy at the time of Great Moderation influenced US income inequality for several decades. This enduring response of aggregate inequality occurs due to the cohort structure of the population, and is important since it may lead to relationships between inequality and other variables going undetected in linear regression estimates, or in similar methods measuring linear correlation. In this chapter I analyze this issue further, and suggest an alternative measure of inequality to be used in regression estimates: the average growth rate of within-cohort inequalities. I first construct within-cohort inequalities from US and UK micro data, I reexamine the effect of globalization on inequality. I replicate some estimates of IMF World Economic Outlook with the cohort inequality data I constructed from US and UK micro data. My emphasis is on trade liberalization, as current regression estimates using aggregate inequality do find a weakening effect of trade liberalization on inequality, inconsistent with the general consensus.

The final chapter of the thesis is distinct from the first two chapters in both motivation and subject. It addresses identification problems in estimating reduced form and structural parameters of DSGE models, which have been the subject of most literatures. In my project, I try to contribute to this literature by employing the Impulse Response Matching (IRM) estimation in one of these models, a hybrid new Keynesian general equilibrium model, using Monte Carlo study. There is literature that uses this estimator, but to the best of my knowledge there is no literature that compares it with another estimator. To this end, I compare it with the GMM, a widely-used estimator on this class of models.

The DSGE model I choose is liable to all the identification problems in the literature, and this makes me search for the best way of applying each estimator before comparing them. For the GMM, I measure the extent of the following four issues on identification: autocorrelated error terms (that create a weak instrument problem); the presence of forward-looking variables (that we have at the time of estimation and need to instrument for); whether to apply the theoretical restrictions implied by the structural form of the model on reduced form coefficients; and either to use just or over-identifying moment conditions. For the IRM, I discuss how to find the right identification to apply on a sample VAR.

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Lower Volatility, Higher Inequality: Are They Related?

I. Lower Volatility, Higher Inequality: Are They Related?

1.1 Introduction

In the income inequality literature, an income of a representative individual is modeled as having a constant long-term growth rate (Moffitt and Gottschalk 1998, Guvenen 2009). As individuals' incomes are subject to changes through their lifetimes, this deterministic part of the model is supplemented with the individual income shocks. As these shocks differ across individuals, they create an income inequality.

Early inequality literature recognizes three important features of these shocks affective on inequality (Deaton and Paxson 1994). First, their variance: if their variance increases, they would lead to higher inequality compared to previous years. Second, the degree they are correlated among individuals: if they are perfectly correlated for instance, we may even further assume that everybody gets the same shock in percentage terms; these shocks would not affect inequality regardless of their variance. The final important feature of the shocks is that any change in either variance or correlation lead to a long lasting change on inequality. This is because of the cohort structure of the population. Since at the time of a change in any of the parameters, old cohorts, or old people, have already been affected from the previous realization of the shocks, the change in inequality lasts until the today's youngest cohort becomes the oldest cohort.

Although early inequality literature recognizes these three channels, empirical inequality literature tends to ignore the last two: the correlation and cohort structure channels. On the one hand, if papers' objective is to explain the change in (within cohort) inequality by using the changes in the individuals' income dynamics, they only use change in the variance of the shocks to explain it, not their correlations (from the US: Moffitt and Gottschalk 1998, 2008; Primiceri and van Rens 2002, 2009). I suspect that the underlying cause of the lack of concern for correlation term is that the correlation in the data is very low. In this study, I show that what matters is the percentage change in that term, not its level. On the other side of the literature, if paper's objective is to explain the change in inequality by relating it to changes in some other variables by using time series regressions, as these estimations look for a simultaneous correlation between the variables, they ignore the cohort effect. I suspect the underlying reason could be that the effect of cohort structure of a population on the dynamics of the aggregate inequality has never been quantified before. My calibration exercise shows that it took decades for the US income inequality to adjust the changes in the structure of the economy at the time of Great Moderation.

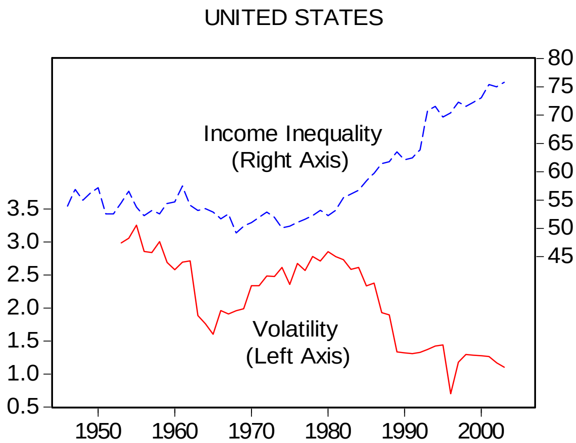
To show the importance of correlation of income shocks and cohort structure of the population on the evolution of income inequality, I first recognize that they are the same individual income shocks creating inequality that also create volatility in real GDP. This is because these shocks create fluctuations in individuals' incomes. As individuals' incomes fluctuate, so does aggregate income. If the variance of the shocks increases, we can easily expect higher volatility in the data. So volatility, like inequality, is positively correlated with the variance of the shocks. However, if the correlation of shocks increases, as everybody gets the shocks at the same frequency, they would affect GDP a lot. The whole mechanism is summarized in the following table.

Table 1

	Variance of shocks	Their correlation
Volatility	+	+
Inequality	+	−

Figure 1 illustrates GDP volatility and income inequality for the US. The dashed line shows the measure of income inequality that is used throughout this paper: cross-sectional variance of individuals' income distribution, measured in logs. This is shown on the right axis of the figure. The solid line shows the measure of volatility I use: standard deviation of per capita real GDP growth, calculated using a 10 year centered rolling window and shown on the left axis of the figure. Figure 1 reveals that there have been concurrent changes in the time series behavior of the variables, occurring in the 1980's. This coincidence points out the common dynamics behind the variables. Then inequality starts to increase, and volatility starts to decrease. I infer that these happenings can only occur due to the decline in correlation of shocks, not in their variance as it is commonly employed explanation for the changes in the variables. Besides, the change in inequality lasts longer than the change in volatility, which I prove as to happen due to the cohort structure of the economy.

Figure 1



Volatility: Standard Deviation of Per Capita Real GDP Growth
Income Inequality: Variance of Log Incomes

Recognizing the relation of the variables is important because decline in volatility, as being the subject of the Great Moderation, is a common phenomenon across industrialized countries (Stock and Watson 2005, Summers 2005, Cabanillas and Ruscher 2008. And increase in within country income inequality is also common observation through these countries (Deininger and Squire 1996, Smeeding 2002, Sala-i-Martin 2002, Atkinson 2003, IMF World Economic Outlook 2007 Ch 4).

To analyze the relation of volatility and inequality with individuals' incomes in detail, I use a simple individual income process and aggregate it for volatility and inequality. The prediction of the model is that common dynamics that govern the changes in the variables are the second moment characteristics of income shocks. The opposite movements in the variables in last decades arise from the decline in the correlation of these shocks among individuals. I also find an empirical evidence for each of these predictions.

The sample consists of 9 industrialized countries. These countries have been chosen on the basis of availability of the inequality data that is long enough to carry out structural break analysis[1] However, even for these countries there is no unique data source of inequality that covers relatively long periods of time. This problem has been overcome by combining different data sources. In this manner I obtain, to my knowledge, the longest existing income inequality data index in the literature.

With regard to the empirical evidence for the relation of the variables, I predict that Table 1 shows that if there occurs a change in variance, the variables would evolve in the same direction. If a change occurs in correlation, variables will evolve in opposite ways. So we couldn't expect a long-term stable correlation between the variables, and this implies that time series regressions are useless to relate them. But alternatively, Table 1 shows that if there occurs a change either in variance or correlation, this change should be immediately reflected in both variables; meaning that if observe a change in any of two variables, we should also observe a simultaneous change in the other one. The existence of structural changes in the volatility data is already known from great moderation literature, and here I discuss that there exist contemporaneous changes in the inequality data with volatility data throughout countries. Accordingly, I employ structural break estimation to find the time of the changes in each data, and test if the changes in the data occur at the same time. My results constitute strong evidence for

[1] The data was also available for a few of developing countries. However, I wanted to concentrate only on the countries having experienced similar structural changes in their economies in the recent decades. So that common results and policy recommendations can be made.

the presence of concurrent changes in volatility and inequality in the last three decades. This is my empirical evidence for the long-term relation of the variables. I interpret it as a long-term as I do not refer to the direction of the changes in data realized for specific country, but only coincidences over time. Hence, it is expected to hold for all realizations in the data, and for any country.

With regard to the empirical evidence I obtain through analyzing the changes in the data in the recent decades, I show that such an analysis could give quantitative results for the change in variance and correlation leading to these changes. I run this experiment for the US, and estimate the changes in variance and correlation before and after the Great Moderation in the US. Because only for this country there are also available empirical studies investigating variance and correlation in the micro income data that I can compare my findings. My results fairly match the changes found in the micro data. I find that the decline in correlation parameter has decreased around 80% around the time of Great Moderation. This finding exactly matches with Gorbachev (2007)'s empirical observation from micro data. I also find that the variance of shocks has increased 50%, though Nichols (2008) uses micro data and shows that this figure is 30%. I believe the discrepancy arises as he smooths the data to calculate the variance, whereas I do not. These fairly consistent findings both supports my derivations of volatility and inequality in terms of variance and cross sectional correlation of the shocks, and also proves the importance of the cohort structure of the economy, as I take it into account along these derivations. My calibration exercise shows that it took 25 years for the US inequality to adjust the changes in the second moments of the shocks due to the cohort structure. The finding that the changes in the data of the US in the last decades have mainly occurred due to the decline in the correlation of the shocks is further important because the recurring pattern in the data across industrialized countries is the same with that of the US, an increase in inequality, and a decline in volatility, pointing the decline in correlation across countries. Since they are the aggregate shocks that affect every individual in a parallel way and create correlation, this means there has been a decline in the weight of aggregate shocks in the total income shock of the individuals.

I also contribute to the volatility literature. The common practice in explaining the reduction in volatility by means of disaggregating data to micro level is to investigate sector and firm level volatilities (see Comin and Philippon 2006). Gorbachev (2007) is the only study, to my knowledge, which goes beyond this. It uses US micro and aggregate income data and associates the reduction in the correlation among income changes of families with the reduction in aggregate volatility. The paper also emphasizes that these are the same shocks creating inequality. However, her empirical study neither refers to any income process nor to the type of the shocks affecting individuals.

To the best of my knowledge, this paper is the first to relate two large literatures about the increase in inequality and the decrease in volatility. Uren (2008) recognizes that an increase in cross-sectional wage inequality is accompanied by a decrease in volatility in the US, but he uses the increase in efficiency of the labor market to explain both features of the data. He does not refer to, or attempt to explain, the connection of variables via individual income shocks, ignoring a class of models that can explain phenomenon. I, by showing the connection of the variables, show that Great Moderation is not costless and income inequality is one of the variables that is needed to taken into account in evaluating welfare implications of the policies leading to that.

There are also studies considering the linear and causal relation between inequality and volatility through cross section and panel studies; such as Calderón and Yeyati (2007), Konya and Mouratidis (2005), Iyigun and Owen (2004), Breena and Garcia-Peñalosa (1999). However these studies make no reference to underlying relation. Iyigun and Owen (2004), Breena and Garcia-Peñalosa (1999) also use structural models in which both volatility and inequality change, but in the model of Iyigun and Owen income inequality is found to cause volatility, while in the model of Breena and Garcia-Peñalosa causality is in reversed.

The rest of the paper is organized as follows: The next section presents graphical evidence from representative series of inequality and volatility data with the aim of directing the reader's attention to their seemingly simultaneous movements. Sections 3 and 4 aim to link the variables. Section 3 explains the theoretical link between the variables by aggregating two different specifications of individual income processes, with and without the transitory shocks but permanent shocks in both, for the variables. This provides two frameworks on which to apply structural break estimation in Section 4. Sections 5 aim to analyze the changes in the variables occurred in the last decades. To this end, volatility and inequality data from the US are used to infer about the changes in variance and correlation terms. Section 6 concludes.

1.2 International Evidence

Figure 2 in the Appendix shows graphs analogous to Figure 1, but drawn for all the sample countries. These countries have been chosen on the basis of availability of inequality data that is long enough to carry out empirical analysis. The robust pattern the graphs reveal is the coincidence of changes in the time series behavior of the variables, which is seemingly apparent for every country, possibly other than Sweden and Netherlands. Yet, here I use 10 years rolling window to calculate volatility, as it is one of the standard measured in the literature. This implies that these graphs are not most useful to infer exact timing of the break dates as this measure captures the deviations in data 5 years ahead at any time. However, to estimate the time of changes in this variable I use the raw data, and then coincidences of changes in the variables become more obvious.

Apart from coincidences of changes in the variables, whether variables display a parallel or opposite trend following those changes gives hint as to the specific cause of these changes. The graphs reveal that for many countries the volatility index follows a downward trend beginning in the 1980's, and the reverse is true for the inequality index. This, I presume, could only occur due to the decline in the correlation of the shocks. There are countries like Italy and Denmark, which seem to be exceptions to this generalization as inequality has declined in these countries; however, even for them we observe stop in that decline. If you look at data from this perspective, there is regular pattern in the data, inequality either increases, or stops declining. Thus, possible source of decline in correlation of income changes, as it leads to dispersion in the incomes, account for much of the changes in data. At the second chapter of this thesis I try to find the source of change in the correlation. There I discuss that, for each country, the change in the pattern of data, including stop in the decline in inequality, occurs at the time that country experienced substantial increase in trade globalization.

1.3 A Reduced Form Model of Aggregation

1.3.1 Individual Income Process

In this section I define, and try to rationalize, a reduced form income process that will be used to derive inequality and volatility in the next section. The income process defines a path for log real income y for N individuals i over time periods t.

$$y_{it} = \alpha + y_{it-1} + v_{it}$$

I do not take a stance on what determines these individuals' incomes, except to say that they are subject to shocks having zero mean across individuals and time, $E_{i,t}\left(v_{it}\right) = 0$, having the same variance in time across individuals, $Var_{ts}\left(v_{it}\right) = \sigma_t^2$. To start with, I assume that the shocks are uncorrelated with their past values, $Corr\left(v_{it}, v_{it-1}\right) = 0$, but correlated among individuals

$$Corr\left(v_{it}, v_{jt}\right) = \rho_t$$

Since these second moments may not be constant over time, I denote them with a subscript t[2].

A change in the variance of the individual income shocks or in their correlation between individuals changes both the shape and mean income growth rate of the individual income distribution. To see how, consider two extreme cases for

[2] Notice that here the error term has a permanent effect on income. Later I will also employ transitory shocks in the income process. Permanent shocks have been chosen to start with since the intuition for the mechanism stressed in this paper is best understood in a model with only this type of shock. Also using models with and with and without transitory shocks will supply two separate frameworks on which to apply the structural break analysis, so that I will have opportunity to check for robustness of our results.

the degree of correlation. In one extreme case I assume that all individuals experience an equal percentage shock to incomes, which makes the shocks perfectly correlated as well. Then what follows is that the mean of the income distribution will shift from its long run growth rate, which would cause volatility, the degree of which is parallel to the variance of the shocks, but the shape of the distribution will not be affected so that inequality would not change. In the other extreme case of no correlation between shocks, they will cancel each other out while aggregating the country shock. This happens due to the law of large numbers, so that volatility will not be observed in the mean income. However, as people receive diverse shocks under this case, higher cross-sectional variance should be observed for any given level of variance of the shocks. To sum up: while increasing variance of the shocks increases both of these variables, increasing correlation increases volatility but has a reverse affect on inequality.

One specification of the income process that may seem important is keeping α, the long term growth rate individual income, constant across individuals. By assuming this, I use one of the two models used in the literature to estimate the true nature of individuals' income processes. This is called "Restricted Income Profile" model. It assumes that individuals face similar life-cycle income profiles; while the income divergence among them occurs as a result of very persistent shocks to which they are subject. The point estimate for this persistency is found to be at least 0.95 by both recent, Storesletten et al.(2004a) and Guvenen (2009),and earlier, MaCurdy (1982), studies. So it has become common practice to take it as 1 (unit root) in the related literature, as in this paper

The second branch of the literature use the so called "Heterogeneous Income Profile" model. In this model α is allowed to change across individuals and as a result, part of the income inequality is explained by the difference in life-cycle income profiles. Accordingly, in this model there is a smaller role for the persistence of shocks to account for the differences in earnings of individuals and a smaller persistence parameter is found for the effect of shocks; 0.8 by Guvenen (2009).

However, whether the restricted or heterogeneous income profile model is assumed, it is common practice to use a common persistence parameter for the effect of shocks. This is because the estimated variance for a heterogeneous persistence parameter is much smaller than in the long term growth component, see Baker (1997).

Although I use the "Restricted Income Profile" model instead of the "Heterogeneous Income Profile", it is only for the sake of tractability of the next section, which involves estimating the structural break in the data. Following my derivations for inequality and volatility, it is becoming obvious that even if I allow α to change across time the result of my estimations wouldn't change. This is intuitive as any parameter that doesn't change over time, whether α or the unit root persistency parameter, even if it differs across individuals, would not lead to a change in the time series behavior of the variables.

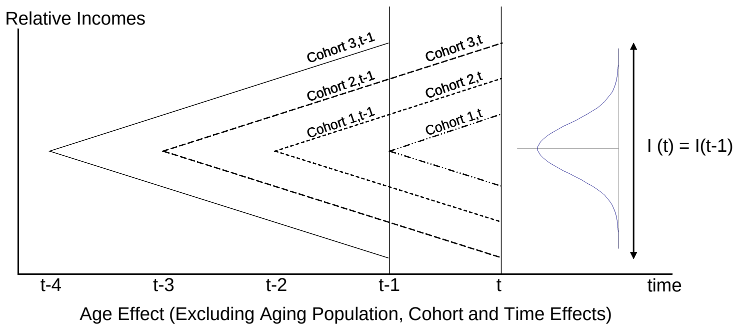
Now I have an individual income model. But before deriving volatility and inequality from that, I need to take a closer look at theincome inequality dynamics. This is because individuals have a limited life span and shocks cannot be expected to create ever increasing inequality between them. The common practice in explaining aggregate inequality is to divide it into within and between cohort inequalities.

1.3.2 Dynamics of Aggregate Inequality

Within cohort inequality measures the inequality within individuals, born in the same year, who have been subject to different income shocks over time. This is called the "Age Effect" on inequality. Yet, even though within cohort inequality increases by time, as long as the rate of this increase is constant, within country income inequality stay the same level over time. This is because old cohorts are replaced by younger ones and there exist similar cohorts constituting the population at each point in time.

This is demonstrated in Figure 3 below. Vertical axis shows the relative incomes of individuals. There cohorts, each having life time of 3 years, are used and the within country income inequalities at time t and $t-1$ are analyzed. As an example for the notation, (Cohort 2,t) indicates the people who are 2 years old at time t[3]. The graph shows that inequalities are equal in these two years: $I(t) = I(t-1)$

Figure 3 : Illustration of Evolution of Income Distribution



[3] Although I used discrete time for notation, the lines ad graphs are drawn consistent with continuous time. The normally distributed incomes are attained because of the presence of a higher number of individuals around the mean income.

Here we can infer how changes in within cohort inequality, affect aggregate inequality. The difference in income growth rate of individuals, cannot lead to change in aggregate inequality since it is constant over time. It would only explain part of the existing inequality. In the same manner, the levels of ρ and σ, as long as they stay constant over time, affect within cohort inequality, but not within country inequality. But what happens if there exist a change in correlation and variance terms? The following figure shows the effect of decline in correlation at time $t-1$.

Figure 4 : Illustrative Effect of a Decline in the Correlation on Inequality

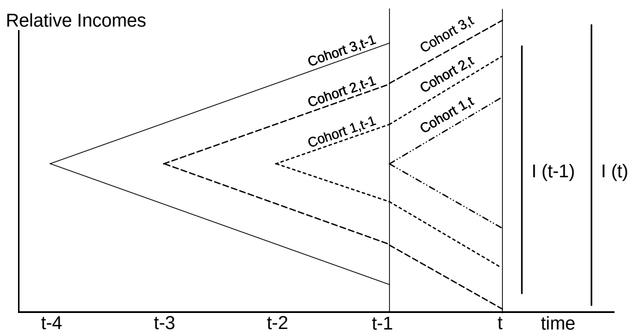


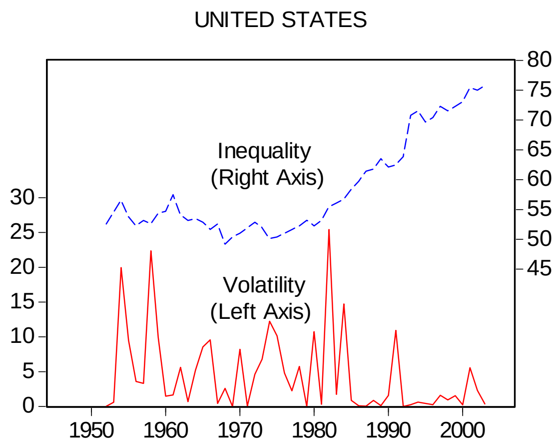
Figure 4 shows that following a decline in correlation, an increase in inequality is observed at time t compared to time $t-1$. As this decline affect both within cohort and aggregate inequalities. This implies that changes in the aggregate inequality can be taken as evidence for the changes in correlation and variance parameters.

Even though by now I have shown that changes in variance and correlation lead to changes in both volatility and inequality, I cannot trust time series regression to relate the variables. This is because depending on the changing parameter, variance or correlation, variables can move in the similar, or in reverse directions. So a long-term stable correlation between the variables cannot be expected. Rather, knowing that changes in the parameters should be immediately reflected in variables, we can infer that if we observe a change in any of the variables, this change should be immediately reflected in both variables; meaning that if we observe a change in one of the variables, we should observe a change in the other one as well. We already know the existence of structural changes in the pattern of volatility in the last decades from the Great Moderation literature, and in this study I discuss that there exist structural changes in the inequality data occurring at the same time with that of volatility. I find the time of the changes in each data by structural break estimation, and test if the changes in each data are simultaneous.

Another important inference that can be inferred from this graph is that the effect of such a level shift in parameters on within country inequality should last for three periods. this happens since at the t,me of decline n correlation, the old cohorts have already been affected from the previous realizations of the shocks; hence, only three years later, at t,me $t+2$, all the cohorts will completely adapt to these changes, and then within country income inequality will reach its new steady state.

This can also be seen from Figure 5 below, which includes the same variables with Figure 1, but using squared deviations from the mean growth rate of per capita GDP instead of a 10 year-rolling window to calculate the volatility, so that it lets us capture the time of change in the data more accurately. We observe that the reduction in the volatility occurs instantly in the 1984 (upon the sudden changes in ρ), though from that time on inequality continues to increase until its reaches its new steady state. As I predict to occur due to the cohort structure of the population. Although not every country graph reveals such a clear pattern, it may easily be result of non-existence of sudden changes in ρ and σ in a time.

Figure 5



Volatility: 10.000 * Squared Deviations from the Mean GDP Growtl

Income Inequality: 100 * Variance of Log Incomes

So any change in parameters will immediately and totally reflected in volatility, but it will have long lasting effect on inequality. This observation is enough for me to realize that I cannot trust time series regression estimation to relate volatility and inequality. Later I also find empirical evidence showing the importance of the transition period. There I will claim that these egressions cannot be used to relate inequality to any other variable of interest. This is because these estimates simultaneous correlation between variables.

Rather, I use the structural break estimation and find the time of change in volatility, and time of start of change in inequality. I show that they occur at the same time. I check if a break in one series stays in the confidence interval of a break in the other series.

As a final remark, it can again be seen from the Figure 4 that even if the α is not the same across individuals, as long as its distribution is constant over time, it would only lead to constant inequality. Its effect on volatility is also very similar. If it changes across individuals but not over time, as there will not be fluctuations in individual incomes, there will be no volatility either. Such change in individual income trends is called income mobility and Nichols (2008,2) shows that in the US, compared to the risk of volatility, the effect of mobility is very small.

However, time varying parameters, like α, are specific to the income process employed. There is more general classification of possible sources affecting within country income inequality other than the age effect. To summarize such sources, it might be said that the change in the within cohort inequality, which I use is equally representative for the change in total inequality if and only if

· there is no population effect; which means the birth rate is constant over time, so that the weight of any cohort on inequality is fixed

· there is no cohort specific effect; i.e. the shocks have equal distribution between cohorts

· there is no time effect; if this condition is not satisfied it means cohorts are affected by unexpected changes in income distribution at that time (example would be a tax reform or change in social security system.

Given these conditions, one can intuitively derive the effect each may have on this paper´s analysis. As a sample of industrialized countries is used, I assume that none would have a strong effect on my results.

1.3.3 Volatility and Inequality of Income Growth

In this section the volatility and inequality implications of the income model are derived.

Volatility arises from deviation of the average individual income growth from its long term mean

$$Vol = Var_{ts}\left(E_{cs}\left(\Delta y_{it}\right)\right) = \lim_{T \longrightarrow \infty} \frac{1}{T} \sum_{t=1}^{T} (\overline{\Delta y_t} - E_{ts}(\overline{\Delta y_t}))^2,$$

where TS denotes time series, and CS cross section. So volatility is time series variance of the cross-sectional average of individual income growths, which, at time t, is

$$E_{cs}\left(\Delta y_{it}\right) = \overline{\Delta y_t} = \lim_{N \rightarrow \infty} \frac{1}{N} \sum_{i=1}^{N} \Delta y_{it}.$$

Then I add the chosen income process as written above to that definition

$$\Delta y_{it} = \alpha + v_{it} \quad \Rightarrow \quad Vol = \lim_{T \longrightarrow \infty} \frac{1}{T} \sum_{t=1}^{T} ((\alpha + \sum_{i=1}^{N} \frac{v_{it}}{N}) - \alpha)^2,$$

which finds that

$$Vol = \lim_{T \longrightarrow \infty} \frac{1}{T} \sum_{t=1}^{T} \left[\lim_{N \longrightarrow \infty} (\frac{1}{N^2} (N\sigma_t^2 + N(N-1)\rho_t \sigma_t^2) \right] = \rho_t \sigma_t^2.$$

Thus, the model creates two separate effects on volatility. The first one, $N\sigma^2$, is the direct effect of individual error variances on volatility that tends to zero when multiplied by $1/N^2$, which means that by the law of large numbers idiosyncratic shocks cancel each other out. While the second term, $N(N-1)\rho\sigma^2$, coming from the correlation of shocks across individuals, is the one causing volatility. This result shows that only if there is a correlation between individual shocks, there is also the volatility. This correlation, later on, will be assumed to occur due to the aggregate shocks.

Now I need to derive inequality from the income model. In principle, the change in inequality arises due to the difference in income gains of individuals. However, as intuition given in the previous sections, inequality does not increase always due to the population dynamics of the society. Hence, to understand this dynamics of the income inequality, the common practice is to divide it into within and between cohort inequalities. Within cohort inequality is is the inequality within individuals, born in the same year, who have been subject to different income shocks over time. I start by analyzing this one. I also need to analyze the "change in inequality" instead of "inequality". Since even if there is no change in inequality in the given year, inequality from previous years will prevail in the data. For volatility this was not the case.

The change in within cohort inequality can be shown by the term $E_{ts}(\Delta Var_{cs}(y_{it}))$, the change in the variance of log income from one year to the next. However, when error terms are uncorrelated with their past values as is the case with this model, this measure is directly equal to $E_{ts}\left(Var_{cs}\left(\Delta y_{it}\right)\right)$ (this equivalence is shown in Appendix A). That measures the variance of the changes in individual incomes, which we can directly calculate from the income model.

$$C.I. = E_{ts}\left(Var_{cs}\left(\Delta y_{it}\right)\right) = \lim_{N\to\infty,T\to\infty}\frac{1}{T}\sum_{t=1}^{T}\frac{1}{N}\sum_{i=1}^{N}\left(\Delta y_{it} - E_{cs}\left(\Delta y_{jt}\right)\right)^2$$

This is the second moment condition relevant for this study than that of volatility: dispersion of individual's income growth rates around their cross-sectional mean. After inserting the income process into this definition

$$\Delta y_{it} = \alpha + v_{it} \quad \Rightarrow \quad C.I. = \lim_{N\to\infty,T\to\infty}\frac{1}{T}\sum_{t=1}^{T}\frac{1}{N}\sum_{i=1}^{N}\left((v_{it} - \sum_{j=1}^{N}\frac{v_{jt}}{N}\right)^2$$

I find that

$$C.I. = \lim_{T\to\infty}\frac{1}{T}\sum_{t=1}^{T}(\sigma^2 - 2\rho\sigma^2 + \rho\sigma^2) = (1-\rho_t)\sigma_t^2$$

showing that inequality is positively correlated with the variance of the shocks, but negatively so with the correlation between them[4].

Testing Model with the data:

For volatility, the model implied that

$$Vol = \frac{1}{T}\sum_{t=1}^{T}(\overline{\Delta y_t} - E_{ts}(\overline{\Delta y_t}))^2 = \frac{1}{T}\sum_{t=1}^{T}(\sum_{i=1}^{N}\frac{v_{it}}{N})^2.$$

[4] Note that if we allow heterogeneity in the growth rate of incomes of individuals, an additional term appears on the right hand side of the equation: $\lim_{N\to\infty}\sum_{i=1}^{N}(\alpha_i - \overline{\alpha_i})^2/N$. This is time invariant and cannot change within country income inequality. Such heterogeneity is not even reflected in the derived volatility equation.

To find the structural break in the data consistent with this measure I use per capita GDP growth as a proxy for average income growth of individuals

$$(\Delta y_t^{pc} - \overline{\Delta y_t^{pc}})^2 = (v_t)^2 = c^n + {\varepsilon_{vt}}^5, \tag{1}$$

where each n shows the value of constant within the time interval separated by two breaks. These intervals are called regimes. With m break dates, $[T^1, ..., T^m]$, there are $m+1$ regimes, $n = 1, ..., m+1$, and the regime n includes the time interval between break dates $[T^{n-1}+1, T^n]$. In equation (1) I look for a change in the variance of deviations from the mean of the data, i.e. in unconditional variance of the data. For the details of the types of structural estimation, whether it is on (un)conditional means or conditional variance of the data, please refer to Eksi (2009).

For inequality, the implication of the model was

$$C.I. = E_{ts}\left(\Delta Var_{cs}\left(y_{it}\right)\right),$$

and the way I use the data consistent with this measure is

$$Var_{cs}\left(y_{it}\right) = c^n + \beta^n t + \varepsilon_{it} \tag{2}$$

Although the theoretical measure of the change in inequality requires taking the difference of the variance of log income, instead of taking the difference of the

[5] Occasionally in the related literature, to avoid overemphasis on outliers, the absolute value of the left hand side of equation (1) is taken (Stock & Watson (2002)). Breaks in this paper were also estimated in the same manner but merely resulted in slightly different results than those documented here.

data, it is regressed on both a constant and a time trend, and a break is tested for in both. The reason for this is two fold. First, although within cohort inequality has been driven theoretically, within country inequality data is used to test for a break. The relation between them is explained in the previous section. There we see that when there is a change in the rate of increase in within cohort inequality at time t, there will be level shift in this measure; however, inequality will start to follow a trend[6]. Second, taking the difference of the data results in smoothing and makes the estimation liable to outliers in the data.

1.3.4 Autocorrelation: Transitory and Permanent Shocks

Now volatility and inequality are derived for the income process including the transitory income shock, u_{it}, together with the permanent shock, v_{it}. Like I do here, it is the usual procedure to test different income processes on the data, as there is no consensus on which process represents the true progression of income. Moffitt and Gottschalk (1998), (2008) find that around half of the increase in the cross-sectional variance of male earnings has arisen from an increase in the variance of transitory shocks. On the other hand, Primiceri and Van Rens (2009) find that permanent changes in income explain all of the increase in inequality in the 1980s and 90s supporting the previous model that uses only permanent shocks[7]. In reality, these two models are not in competition; rather this model

[6] Notice that a change in the growth rate of a variable can always be determined by regressing this change on a time trend. This is because: $\Delta Var_{cs}\left(y_{it}\right) = c^{n} + \beta^{n} t + \varepsilon_{it}$

$$\Rightarrow Var_{cs}\left(y_{it}\right) = Var_{cs}\left(y_{i0}\right) + \sum_{s=t-a}^{t} c_s = Var_{cs}\left(y_{i0}\right) + (t-a)c = \left[Var_{cs}\left(y_{i0}\right) - ac\right] + tc$$

, which justifies looking for a break in both constant and trend terms.

[7] If for any individual i, when the income process is defined as $y_{it} = y_{it}^{p} + u_{it}$ & $y_{it}^{p} = \alpha + \lambda y_{it-1}^{p} + v_{it}$ (although I assumed $\lambda = 1$ before, as it was already emphasized its empirical value is around 0.98 for the US), then

is the extension of the first. But the implications and findings of this model still are of interest to this paper.

I define today's income for individual i as the sum of the permanent component of his income and a transitory income shock

$$y_{it} = y_{it}^{p} + u_{it},$$

where the permanent component of income follows the same process I defined before

$$y_{it}^{p} = \alpha + y_{it-1}^{p} + v_{it}.$$

Together they imply that

$$\Rightarrow \quad y_{it} = \alpha + y_{it-1} + \Delta u_{it} + v_{it}$$

so the effect of u_{it} on y_{it} lasts only one period, while v_{it} affects it in the manner it does before. Here each shock has zero mean across individuals and time, $E_{i,t}\left(v_{it}\right) = 0$ & $E_{i,t}\left(u_{it}\right) = 0$, and has the same variance in time across individuals, $Var_{ts}\left(v_{it}\right) = \sigma_{v,t}^{2}$ & $Var_{ts}\left(v_{it}\right) = \sigma_{u,t}^{2}$. These shocks are uncorrelated with their past values, $Corr\left(v_{it}, v_{it-1}\right) = 0$ & $Corr\left(u_{it}, u_{it-1}\right) = 0$, but also among themselves, $Cov\left(v_{it}, u_{it}\right) = 0$. I define a correlation ρ for each type of shock among individual in the cross-section

$$Var_{ts}\left(y_{it}\right) = Var_{ts}\left(y_{it}^{p}\right) + Var_{ts}\left(u_{it}\right) = \frac{Var_{ts}(v_{it})}{(1-\lambda)^2 = 0.02^2} + Var_{ts} u_{it}$$

so rather than the variance of temporary shocks, the variance of permanent shocks should be expected to create the main dispersion between income of individuals.

$$Cov\left(v_{it}, v_{jt}\right) = \rho_v \sigma_v^2 \qquad Cov\left(u_{it}, u_{jt}\right) = \rho_u \sigma_u^2 \text{ for } i \neq j.$$

Given these specifications, volatility and the change in inequality consistent with this income process can be found as

$$Vol = \rho_u \sigma_u^2 + \rho_v \sigma_v^2. \qquad C.I. = (1-\rho_v)\sigma_v^2.$$

Derivations can be found in Appendix A. Here is while volatility is affected by both of the shocks, inequality is only effected by the permanent income shock. The transitory income shock at time t could affect the inequality in principle but we don't observe the effect of it as the effect of similar shock hitting the income process at time $t-1$ cancel out at time t[8].

An important implication of using transitory shocks is that it reminds us that today's error term can include past shocks as well as today's. If this is the true income process generating the data, then structural break estimation applied on previous model cannot accurately find the exact timing of the breaks. This is shown below:

$$Vol = \frac{1}{T}\sum_{t=1}^{T}(\overline{\Delta y_t} - E_t(\overline{\Delta y_t}))^2 = \frac{1}{T}\sum_{t=1}^{T}(\sum_{i=1}^{N}\frac{v_{it}}{N} + \frac{\Delta u_{it}}{N})^2,$$

which includes u_{it-1} as well. However, instead of a permanent and transitory decomposition of the error terms, the income process still can be written as

[8] One unpleasant implication of this result will be that by using only two variables, volatility and inequality, identification of variance and correlation of each type of shock, which constitute four unknowns in total, is not possible. However, the inferences for the way correlation and variance of error terms affect inequality and volatility are still valid.

$$\Rightarrow \; y_{it} = \alpha + y_{it-1} + \Delta u_{it} + v_{it}$$

in the following MA(1) form

$$y_{it} = \alpha + y_{it-1} + \mu_{it} - \beta\mu_{it-1}. \tag{3}$$

In Appendix A it is shown that when μ_t is an i.i.d. shock with $var\,(\mu_{it}) = var\,(v_{it}) + var\,(u_{it})$ and $\beta = var\,(u_{it})\,/\,(var\,(v_{it}) + var\,(u_{it}))$, this transformation preserves the time covariance of shocks, and approximates their variance. The intuition behind this transformation is that the original process includes a temporary shock that has a one period memory, just like an MA(1) process.

Now I can use equation (3) and insert lags of Δy_{it} into that equation to remove past shocks. Equation (4) obtained.

$$\rightarrow \Delta y_{it} = (1 + \beta + \beta^2)\alpha - \beta\Delta y_{it-1} - \beta^2\Delta y_{it-2} + \mu_{it} - \beta^3_{it-3}\mu_{it-3}. \tag{4}$$

This equation includes a further lag of the past shock, which may be ignored now as it is multiplied by the higher powers of β. As result, the date of break(s) found in the variance of residuals of this regression can be attributed to the real time changes in the variance of shocks more confidently. Consistent with this equation, I regress per capita GDP growth on its lags

$$\Delta y^{pc}_t = c^n + \beta^n(L) * \Delta y^{pc}_t + e(v)_t. \tag{6}$$

I collect errors to check for a break in their variances (consistent with Stock and Watson 2002):

$$| e(v)_t |= c^n + \xi_{vt} \tag{8}$$

For the variance of log income, a similar operations was executed and resulted in the following:

$$\rightarrow Var_{cs}(y_{it}) = (1-\beta)Var_{cs}(y_{it-1}) + 3\beta Var_{cs}(y_{it-2}) + Var_{cs}(\mu_{it}) + e.c. \tag{5}$$

where e.c. is the error component and includes the terms I omitted, lags of variance of log income from 2 periods onward. Consistent with this measure, I run regressions (7) and obtain error terms,

$$Var_{cs}(y_{it}) = c^n + \beta^n(L) * Var_{cs}(y_{it}) + e(i)_t \tag{7}$$

which will be used in searching for a break in the innovation variances.

$$| e(i)_t |= c^n + \xi_{it}. \tag{9}$$

As β is liable to vary over time, breaks in the regression coefficients are allowed for in both (5) & (7). Although there is no a constant term in the equation (5), while running the equation (7) I allow for a constant because it corresponds to the other terms that were mentioned before, such as cohort, time and population effects. These do not affect derived within cohort inequality, but do affect inequality in the data employed. In effect, the whole process of regressing the variables on their past values should leave error terms only with today's shocks whether the error term follows an MA or AR process[9].

In the next section I define data sources, and in the following one, I estimate the breaks in equations (1), (2), (8) and (9). Note that whether volatility and inequality is derived from the process without transitory shocks (equations(1) & (2)), or from the process with transitory shocks (equations(8) & (9)), the final aim is to search for the break dates in the two series, and then to try to state statistically whether break dates in these series occurs at the same time or not. i.e. if a break in one of the series stays in the confidence interval of the break in the other series. In this manner I can find econometric evidence for the seemingly coincidence of breaks.

1.4 Empirical Approach

1.4.1 Data

I use data for Sweden, Canada, Denmark, Finland, Italy, Netherlands, Norway, the United Kingdom and the United States. These countries have been chosen on the basis of availability of inequality data. The data must cover all time periods from the 1980s, the period associated with the moderation in volatility of BC's and also with increasing inequality. Although a few more developing countries could have been included in the sample, industrialized countries are more likely to have similar experiences of globalization.

[9] It is shown that why memory in the individual income shocks requires searching for a break in innovation variance. And here I derived everything by taking aggregate income shock as a collection of individual income shocks. However, even if we think the other way around, i.e. if the aggregate shocks are taken as affecting income of each individual in some proportion, the way of searching break in data would have been the same. It is because once these aggregate shocks are also assumed have autocorrelated pattern, as it is usually assumed, then they will create autocorrelation in inequality and income growth as well.

I collected data of the Gini index, which is the revealed inequality index most of the time, and then I mapped, transformed, this data to the variance of log income. I needed to this mapping since the variance of log income is comparable measure with volatility. The way of doing this is shown by the work of van Rens and Teulings (2008). This mapping is possible because both of the index data are calculated via second moments of the income distribution. The only necessary assumption to do that is to assume log normal distribution for individuals' incomes, which is a standard assumption in the literature.

The Gini index itself is also "constructed" from UNU/WIDER (World Institute for Development Economics Research of the United Nations University) World Income Inequality database (WIID), which is the latest updated version of Deininger and Squire's well-known data set that measures income inequality. Essentially, one contribution of this paper is the construction of the Gini Index. As is familiar to those who have worked with this index data, there is no common source that supplies this index for a relatively long period of time, and this is true for almost any country, with the US and UK being the only exceptions in the sample. In fact popularity of the WIID type databases arises from this very fact. Data is collected for each country from different sources and summarized (if the Gini index is based on income or expenditure, includes only urban or also rural area, uses household or individuals, etc.). Additionally, quality indexes are given to each source, based on how reliable the data is (if covers the entire country or not, the size of survey sample used is high enough or not, etc.).

Accordingly, I have multiple sources of the Gini index data for countries, each of which may cover different time spans. Furthermore, as the way of measuring inequality of each source is different (urban or rural area, uses household or individuals, before or after tax ...), so does the level of inequality they reveal. For the purpose of handling these problems and constructing the longest possible time series data for each country, I use the index data source which covers the longest time period, and extended it by using the yearly growth rate of the data belonging to other sources. The notion was that although the level of indexes for different sources were different, as are all inequality indexes, changes in these indexes should be informative and reflect change in overall inequality.

In choosing which data sources to combine, those with a quality indicator bigger than 2 (over 4) were rejected, and a variety of "income" inequality indexes were used, but never "consumption or expenditure" based indexes. This was to avoid the effect of 'consumption smoothing'[10]. Finally, if there are more than 2 sources which cover the same time period in addition to the principal source, which is often the case, the source which demonstrates similar growth rates with the principal in the time periods in which they were both available was chosen[11].

The data for GDP, stock of FDI, amount of Cross Border Asset and Liabilities, Exports and Imports in constructing trade share of countries and population are all from International Financial Statistics web page of IMF. As inequality indexes are collected yearly, I use yearly GDP in the structural break estimations as well.wage. I also use wage differential (W90/W10) as a measure of wage inequality. This is the ratio of wages of two labors, one is in top 10 percent of wage

[10]But since WIDER collection does not include sufficiently long consumption or expenditure based Gini index data for the countries used, this does not cause any loss of information.

[11]During presentations of this paper, questions have arisen about the lack of consideration of volatility in Personal Incomes in favour of GDP, as the aforementioned may seem more consistent when considering inequality between Personal Incomes. However, for most of the sample countries, income inequality metrics are calculated based on before-tax incomes of individuals. And the total of those incomes should correspond to countries' GDPs, especially in developed countries, in which states' shares in production are limited. Finally, and in practical terms, using GDP data to measure volatility made this work more easily comparable with existing literature.

distribution, and the other is in bottom 10 percent. It is from OECD Employment Statistics database. The data for mean income of cohorts from Current Population Survey of U.S. Census Bureau, Table P-10.

1.4.2 Structural Break Estimation

In this section I explain the methodology to estimate breaks; firstly in equations (1) & (2), and then in equations (8) & (9).

1.4.2.1 Estimating Breaks in the Unconditional Means

Volatility:

The equation that is going to be tested with real data is found to be

$$(\Delta y_t^{pc} - \overline{\Delta y_t^{pc}})^2 = (v_t)^2 = c^n + \varepsilon_{vt}. \tag{1}$$

There are two considerations in testing this equation for breaks. First, the stationarity of the data is essential to obtain efficient estimates, but this is verified for all countries' GDPs growth rates[12]. Second, if there have been changes in the mean growth rate of the data, $\overline{\Delta y_t^{pc}}$, the Equation (1) would give biased estimates for volatility. To account for those changes, I first regressed Δy_t^{pc} on a constant for each county and found Sweden to be the only country for which there is a significant break in the data (in 1971), shown in Figure 6. Accordingly, for this country I calculated deviations from two sample means[13].

[12] Notice that here I use raw growth data to find the break in the volatility, while an alternative would be finding volatility ex-ante, or using s.d. of the data, as employed in Figure 1 for illustrative purpose. But since s.d. catches deviations in 5 periods ahead, I do not follow this procedure.

[13] Finding a significant break in the Swedish data alone would seem surprising as reduction in productivity is a well-known phenomenon in both Europe and the USA around 1970 (Bai,

In the identification of the break(s), I used Perron and Qu (2007)'s code,as discussed below. However, before proceeding further it is wise to make general comment about the sample period employed and the number and location of breaks that I find in the data throughout my estimations. Whether breaks are found or not depends on the length of the data among other things. The break point found in relatively short data easily be part of a long term trend and may be unidentifiable in longer data sources. Although related papers succeed in proving that estimators are able to pin down break date independent of sample size (like this program does), I believe this should be read as "given the break, the length of the uniformly continuous data on the right and left of the break point is unimportant in allocating the break", which is not the case in practice. So the shorter the data used, the more the probability that every discontinuity in data will be perceived as a break. As a result, for the countries with a limited number of observations in the country sample I employed, I cannot assert that all break dates found can be taken as given. However, for the US and UK which have relatively longer data, break dates will coincide with those found in previous literature.

However, whether the breaks I found are defined as structural changes over a longer time period is irrelevant for the aim of this study, as it is not to name breaks in the data. Rather it is to analyze two series simultaneously and try to observe concurrent structural changes. And as long as the same observation period is used for each series, estimations will be affected equally by sample size.

Lumsdaine & Stock (1998)). However, these authors, like others, act upon this knowledge and make special effort to identify the date of the break, for example through using GDP, Consumption and Investment data. In fact, such a break in the data (especially in the growth rate required for this paper's purpose) is not obvious within the sample period used and shown between 2 lines in Figure 13.

Consequently, although more extensive GDP data is available for the majority of the sample countries, while searching for a break, only the portion of this data covering the years for which inequality estimates are available was used. That portion is also shown between two lines in Figure 6. For some countries those portions do not cover even the early 1970s period that is supposed to include breaks in mean growth rates.

Inequality:

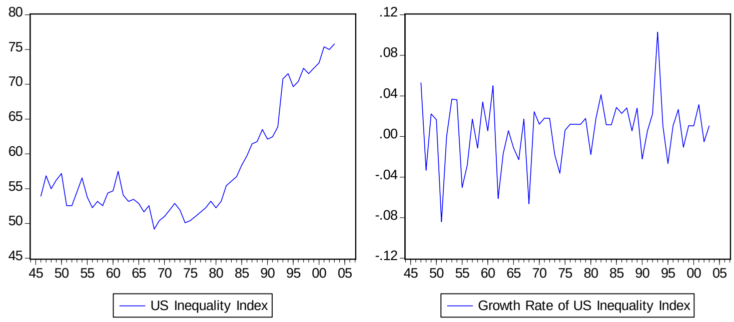
The equation that will be tested with inequality data was found to be

$$Var_{cs}\left(y_{it}\right) = c^{n} + \beta^{n} t + \varepsilon_{it} \tag{2}$$

There is a problem in estimating this equation. For 8 out of 10 countries variance of log incomes is found to be non-stationary, while checking for a break in a series requires stationarity in residuals. Furthermore, this cannot be assumed to be the case after accounting for the break in the series, and the reason is that the null hypothesis states there is no break in the data.

One way of overcoming the problem of nonstationarity would be to take log difference of the variance of log income, and to look for a break in the growth rate of the series. However, I already emphasized that following a break, a trendy pattern in the series should be expected. Moreover, If I take the difference of the data, as there is no clear pattern in the inequality indexes throughout countries, the break procedure tends to catch any kind of one time deviation in the data rather than finding a change in mean growth, as can be deduced from Figure 7 below. There it finds a break in 1994. Figures for the rest of the countries show worse patterns as the available data for them is shorter than the data of the US.

Figure 7



So taking the log difference of the data to make it stationary is unhelpful, but I can overcome this problem through other means. It is known that the variance of log income is mapped from the Gini index, and this index is, by definition, bounded between 0 and 1, and has to be stationary. However, even for the Gini index, the null-hypothesis of the unit root could also not be rejected for the same countries.

This contradictory picture is a common problem in the structural break literature. Perron (1989) shows that under structural breaks, even if the data follows a trend stationary process with a break in time, the usual Unit Root tests will reveal it as non-stationary. This can be seen in the variance of log income in the left panel, which is a motivation of this literature. There it seems that index follows one stationary and one trend stationary pattern, while the combination of both gives a non-stationary result. This pattern is similar for many other countries. A detailed literature review is presented on the tests used to differentiate (trend) stationary process with structural break(s) from the presence of non-stationarity in the data in a companion paper to this one, Eksi (2009).

Whether the data is nonstationary or stationary with breaks is determined by applying Lee and Strazicich (2004)'s methodology that allows two-breaks in time series data while testing for a unit root. For details of this test, and also similar ones, please refer to Eksi(2009). This test also reveals break dates as a by-product[14]. It estimates the break date(s) endogenously from the data and assuming breaks(s) both under the null and alternative hypotheses. This test revealed that out of 8 series of non-stationary variance of log income data initially estimated as being non-stationary, the following is true: 6 of them are trend stationary with a break, 1 is stationary with mean shift, and only 1 is found to remain non-stationary. I disregard the last finding since theory states that the inequality index should be stationary (through almost linear mapping from Gini index to variance of log income). So I take all the variables as stationary with breaks. Given that the data is actually stationary with breaks, I conclude that both of the following equations are ready to be tested for structural breaks.

$$(\Delta y_t^{pc} - \overline{\Delta y_t^{pc}})^2 = c^n + \varepsilon_{vt} \tag{1}$$

$$Var_{cs}\left(y_{it}\right) = c^n + \beta^n t + \varepsilon_{it}. \tag{2}$$

Now I shall explain my methodology for testing structural breaks Although there are break estimators which consistently find the dates of breaks under several assumptions, there is no efficiency condition in these estimations, which means there is no guarantee for finding the smallest confidence interval around the break dates. This consideration is important in this paper. Since a large confidence interval of a break date in one series would indisputably enclose the

[14]The break dates that are found for the variance of log income and wll be discussed below show nearly 1 to 1 similarity of Lee and Strazicich 's test results.

break in another, results can only be as strong as the efficiency of the estimator. In the light of this, a thorough review of the literature was needed and many different estimators were tested. Consequently, this paper is very specific in choosing the estimator. The reader is directed to Eksi (2009) for further details.

Perron and Qu (2007)´s code is applied to search for breaks for each of the equations. This code uses a multiple equation model and is able to find breaks even if they only exist in one of these equations. The reason for using multiple equations is that, in their words, "there can be an increase in the precision of the estimates as long as the correlation between error terms of equations is different from zero. A poorly estimated break in one regression affects the likelihood function through the residual variance of that equation but also via the correlation with the rest of the regressions". So I use one additional ancillary equation for each of the equations estimated. Actually using 2 equations at a time is no different to using an SUR (seemingly unrelated regression) model for linear regression instead of OLS, where the former is at least as efficient as the latter. Further details on the estimation are can be found in Appendix B. Here I only mention that the trimming parameter that specifies the minimal length of a segment that stays between two breaks is set to be 15% of the sample size, but only if the data is long enough to satisfy the criteria of the program. In other cases, it was set to higher proportions.

To check for the robustness of the results, I also used Perron and Qu (2006)'s code, which again looks for multiple breaks at once, but uses a single equation to estimate them. Though there were only slight differences between break dates found with the two methodologies, this code encountered the problem of larger confidence intervals, like many other papers in the subject. Actually, because of this problem it is common for research to reveal percentage confidence intervals of less than 90% (even research using quarterly data distinct from ours).

1.4.2.2 Estimating Breaks in the Variance of Innovations

In this section I test the volatility and inequality implications of the income process including both permanent and transitory shocks. This process, to remind, requires searching for a break in conditional variance. The absolute value of residual term in regressions (5) & (6) are then regressed on a constant, which is again to check if the mean of absolute deviations in the series changes in time or not. Further details are again in Appendix B. The relevant equations are

For volatility:

$$| e(v)_t |= c^n + \xi_{vt} \tag{8}$$

For inequality:

$$| e(i)_t |= c^n + \xi_{it} \tag{9}$$

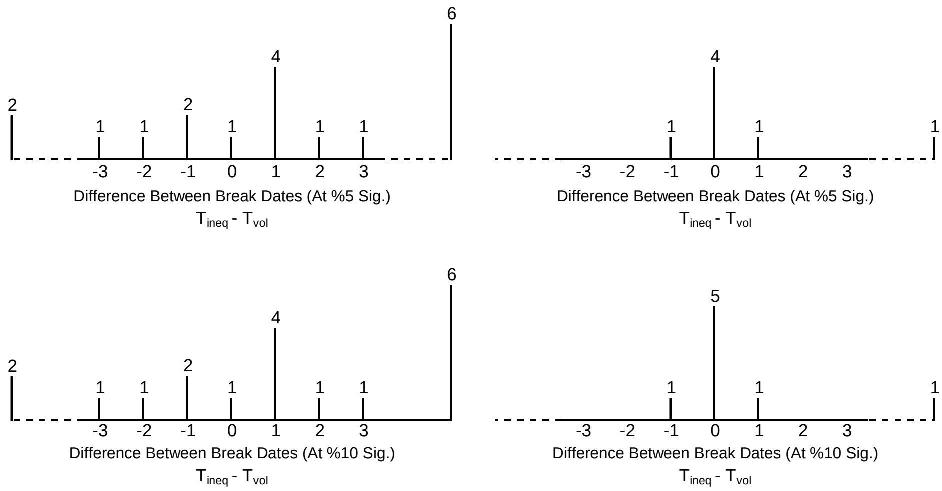
1.4.3 Results: Estimates of the Break Dates

Figure 8 in Appendix shows the breaks found in the unconditional means of the data, i.e. breaks found in the time series of volatility and inequality when those are derived by using an income process only with permanent shocks. As it is hard to see the complete picture of coincidence of breaks from this graph, a summary of the results will be provided further on together with results of the model with transitory shocks. However, it is still evident that most breaks in the data occur at the same time. Resulting break dates are also consistent with those found in existing studies using the same series and countries for volatility (Stock and Watson (2005)), while, to the author's knowledge, there exist no similar studies for inequality. One important result of this study is the attainment of the smallest confidence intervals encountered in any related literature.

Figure 9 shows the breaks found in the variance of innovations when endogenous variables are regressed on their lags as well as a constant term. This is consistent with the income process both with permanent and transitory shocks. Now the results are even more eye-catching. Not only do we observe coincidence of breaks, but also for countries without apparent breaks, the patterns of error processes are surprisingly similar. Results will be summarized together with the previous ones. But it should be noted that for some of countries such as the US, estimated break dates in the error term of volatility are the same as those estimated in unconditional variances. The reason for this is straightforward. For these countries the explanatory power of the regressors in equations (6) & (7) is weak. Furthermore, there is no break in regression coefficients. As a result, the series and its residuals are similar, together with the break dates in conditional and unconditional variances. Summary of the results are below.

Figure 10 : Coincidence of Breaks and Causality Inferences

Model without time correlated errors　　Model with time correlated errors



Histograms in Figure 10 summarize the results of the structural break estimation. The horizontal axis of each histogram shows the time difference between break dates found in inequality and volatility. The columns show the number of breaks found in the data throughout countries with the same order of occurrence. The histograms on the left-hand column show the results when estimating based on the income process with only a permanent shock. The right-hand column shows analogous results when both transitory and permanent shocks are used in the income process. The first row shows the break dates significant at 5% and the second row shows those significant at 10%. The vertical lines and the numbers written on top of them shows the number of countries for which the distance between the break dates in two series is the same. While looking for coincidence of breaks, I used 90% confidence intervals around the break dates.

For instance, the first pair of numbers on the left part of the top left histogram, (1,-2), indicates that in the first model there is only one country for which the break date in the variance of log income occurred 2 years before that in the volatility, while both breaks are significant at 5%. There is also a column on the left-hand side of the same histogram. It shows that there are two breaks in volatility data that cannot be associated with the breaks in inequality data. Overall, this histogram shows that for 11 out of 13 breaks in volatility coincide with 11 out of 17 breaks in inequality. Although this is not a statistical result, I believe it is pretty a strong one given the rareness of breaks in the macro data. The finding that there are more breaks in inequality than volatility makes sense as well. It is because, as it is already explained in this paper, inequality is subject to change due to effects like; population, time and cohort.

With regard to the histograms on the right-hand column, in the previous sections I said that there is an advantage of using transitory shocks, because today's error in the individual income process can include past shocks as well as today's. In order to dismiss past shocks so as to find precise results regarding the date of breaks in the variance of error terms, I regressed variables on their lags. As a result the residuals of these regressions will only include today's shocks. This was the intuition behind the model with transitory shocks, since these shocks were creating memory in the error terms. Now the results show that I reached my aim. The break dates in the models of volatility and inequality consistent with this income process are mostly coincident, although there is an average of around one year difference between the break dates found by the first income process. The number of breaks should have decreased due to the fact that in the first column I look for breaks in the aggregate data but in the second one in the residuals. And I allow for breaks in the coefficients of regressions that I used to obtain the error terms. Given that compared to the number of countries tested, which is 9, I have found at least 6 coincident breaks in the data, there is little reason to doubt the coincidence of breaks in the data. I believe that the importance of these results is that they do not show evidence contrary to the theory. This theory, in turn, can explain the relation between inequality and volatility by means of simple arithmetic concerning to second moment characteristics of individual income shocks.

By looking at the time difference between breaks, a further step can be taken beyond investigation of coincidence of breaks, and causality inferences can be made. As structural break estimation is frequently used for this purpose. Since breaks are distributed around 0, no causality is observed from one variable to the other in either model.

Although the results regarding the coincidence of breaks are strong, what is seen on graphs in Figure 8 is, nevertheless, not totally in accordance with my inference for reduction in correlation. This is because we observe that volatility doesn't always follow a downward trend. But notice that this is due to the fact that, for all countries there was a jump in this series in the beginning of the 1990's. That is a known recession period experienced by counties, which can also be seen from Figure 6. Once it is assumed this jump does not exist in the data, that decline in volatility is a common pattern for every country other than Netherlands. And to my view, such a one-off jump in data can neither be attributed to the increase in the time variance of shocks, nor the decline in correlation among individual income shocks in that specific year. It is rather a high realization. So I believe the decline in correlation is still a fair inference from the data for the sample of countries. Besides, the fact that data shows different pattern for

Netherlands is neither surprising, nor contradicts with my inferences. This is because in the second chapter I discuss that globalization, either it is financial or trade globalization, may have caused the decline in correlation. However, this is only expected to occur in the initial stages of globalization. But Netherlands, among the countries in my sample, is the one having the highest share of globalization indicators in its GDP. For example, the sum of its exports and imports is already equal to its GDP in the year the first inequality data became available for this country. Under this case, I discuss that further globalization should cause to increase in correlation between individuals' incomes as dominating fraction of them becomes subjected to foreign shocks.

1.4.4 Generality of the Results

In the following two subsections I mention about the generality of my results regarding the outcomes of the coincidences of the changes in inequality and volatility.

1.4.4.1 Why Permanent and Transitory Decomposition?

Since I apply structural break estimation on the time series model of the variables derived through the income But do these results depend on the specified income process? Although there are alternative decompositions which can be used instead of permanent and transitory ones, (e.g. splitting error terms to business cycle and idiosyncratic components, or using a shock with MA or AR behaviour), the decomposition employed here, while being consistent with the literature, Moffitt and Gottschalk 1998 show that it has good fit to data, it also practically equivalent to main alternative decompositions.

This decomposition nestles the alternative decomposition of using business cycle and idiosyncratic shock, because I have already defined correlation between the individuals' error terms, and the correlated part of the errors shall correspond to the effect of the business cycle shock in the one alternative decomposition. This is because business cycle shocks, unlike the idiosyncratic one, affects individuals in the parallel manner and creates correlation between incomes.

Using permanent and transitory decomposition also practically equivalent to the use of an error term with MA or AR behavior. Because both decompositions includes memory in the error term and as a result require us to apply structural break estimation in the same way, regressing the variables on their lags and obtaining today's shocks in the residuals. Actually the combination of permanent and transitory shocks was already MA(1).

1.4.4.2 Why Income Inequality but not Wage Inequality?

I have also been asked for why I have not use wage inequality instead of income one, as the most of the income inequality arises from it and wage inequality data is in the main interest of the economists. I used income inequality data because it is comparable measure with GDP volatility; besides, it was more abundant than the wage inequality data. However, the results I derived in this paper from the time series structure of this index data remain valid when wage inequality is considered. It is simply because of this very fact that majority of income inequality arises from differences in wage earnings. As a result, these two data series follow very similar patterns. This is seen in Figure 11. Finland is an exception but being a relatively smaller country, it has potential to display irregular pattern.

1.5 Changes in the Variance and Correlation of Income Shocks

In this section, I explain how to use volatility and inequality data to find the changes in variance and correlation of income changes around the break dates. Then the resulting estimates will be compared with the ones calculated from US micro data. As I will show they are consistent, this will constitute the second piece of evidence for my theoretical results.

In the previous sections, I derive

	Volatility	Change in Within Cohort Inequality
Model without transitory shocks	$\rho\sigma^2$	$(1-\rho)\sigma^2$
Model with transitory shocks	$\rho_u\sigma_u^2 + \rho_v\sigma_v^2$	$(1-\rho_v)\sigma_v^2$

where u & v represent transitory and permanent shocks respectively. Below I use the model without transitory shocks. Then it will be also obvious that the other model cannot be employed because of under-identification problem.

If "V" represents the volatility in the data, measured by $(\Delta y_t^{pc} - \overline{\Delta y_t^{pc}})^2$, it can be equalized to the theoretical measure of inequality found for the first model. That is:

$$Vol = \frac{1}{T}\sum_{t=1}^{T}(\overline{\Delta y_t} - E_t(\overline{\Delta y_t}))^2 = \rho\sigma^2$$

If I use *pre* and *post* to refer to the average values of parameters before and after the break date, it follows that:

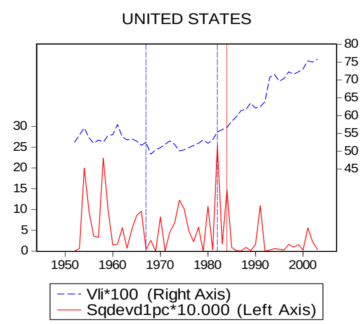
- $V_{pre} = \rho_{pre}\sigma_{pre}^2$ before the break date.

- $V_{post} = \rho_{post}\sigma_{post}^2$ after the break date.

For inequality, I only derived the change in within cohort inequality. However, a change in inequality in data should come from a change in the within cohort income inequality, which is $(1-\rho)\sigma^2$. Then simple intuition requires that change in within country Inequality shall be parallel to $\Delta(1-\rho)\sigma^2$ $(=(1-\rho_{post})\sigma^2_{post}-(1-\rho_{pre})\sigma^2_{pre})$. Thus, the available 3 data series, $(V_{pre}, V_{post}, Change$ $in\ Inequality)$, are not sufficient to pin down all the parameters of interest $(\rho_{pre}, \rho_{post}, \sigma_{pre}, \sigma_{post})$. Moreover, as inequality data does not show a sudden jump at the time of the break, but rather starts to follow a new trend, a yearly change in the data at the beginning of trend cannot be used.

The way I proceed is that, since the existing inequality should have come from the correlations and variances of previous years, I can try to decompose the existing level of inequality and use information hidden in that. But this can be done only at the expense of keeping rho and sigma constant for the years before the break data. Figure 2 shows that assuming a pair of steady state values before and after the breaks in the data seems reasonable for some countries, but not for countries such as the Netherlands, Norway and Denmark. The approach would be feasible, for example, for the US data, which is also shown in Figure 12 below.

Figure 12



Based on this, I should proceed as follows: the average values of the volatility and inequality data between two break dates shown by dashed lines can be taken as their steady state values before the break date. I use their means from 1968 to 1984. The average value of volatility after the second break date can be taken as its new steady state value. I use the mean of the data from 1985 to 2003. However, the new steady state value of the inequality should be taken as its value around 2000. I use the time period between 2000 and 2003. Now I can try to decompose the level of inequality at the steady state.

Within country income inequality can be derived from these values of within cohort inequalities by using variance decomposition (intuition can be gained from Figure 3):

$$\begin{aligned}\text{Variance of log incomes in the population} = \\ &\text{Mean of the variances of log incomes within cohorts} + \\ &\text{Variance of the means of log incomes within cohorts}\end{aligned}$$

which is true as long as cohorts are equally sized. I assume this is to be the case.

I already have the data for aggregate inequality, the first term written above. I calculate the third term by using the data from Current Population Survey of U.S. Census Bureau, which reveals the mean income of people at different ages and over time. From the difference of these two data I calculate the second term: mean of the variances of log incomes within cohorts. Now I need to find its theoretical correspondence. I know that the variance of log incomes within cohorts increases at a rate $1-\rho)\sigma^2$. This implies that, if "n" denotes the number of cohorts:

Inequality within the youngest cohort is	$(1-\rho)\sigma^2$
Inequality within the second youngest cohort is	$2(1-\rho)\sigma^2$
...	
Inequality within the oldest cohort is	$n(1-\rho)\sigma^2$

Now I can calculate their means, and equate it to data. All the findings are summarized below for before and after the break in 1984.

	pre-84	post-84
Measure of volatility	$\rho_{pre}\sigma^2_{pre}$	$\rho_{post}\sigma^2_{post}$
Volatility in the data	$62.8E-05$	$15.8E-05$
Measure of mean within cohort ineq.	$\frac{(n+1)(1-\rho_{pre})\sigma^2_{pre}}{2}$	$\frac{(n+1)(1-\rho_{post})\sigma^2_{post}}{2}$
Variance of log incomes in the population	0.52	0.75
Variance of the cohorts means	0.20	0.26
Mean within cohort ineq. in the data	0.32	0.49

Now lines (1) and (2) are comparable, together with lines (3) and (4).

Please note that now there are 5 parameters $(\rho_{pre}, \rho_{post}, \sigma_{pre}, \sigma_{post}, n)$[15], but 4 data series. Hence, I estimate the parameters of my interest with two different levels of n. In one of them, I calibrate it to 45. As people enter the labor force in their 20s and retire when they are around 65, here I assume that cohorts are populated by the people at the same age. And the other one assumes another practice in the literature, grouping people around the same age, and in

[15] If cohorts are assumed to be subject to different ρ & σ s at each point in time, inequality within the oldest cohort can be found by the term $(1-\rho_1)\sigma_1^2 + (1-\rho_2)\sigma_2^2 + ... + (1-\rho_n)\sigma_n^2$. In this case, it can be inferred that the information hidden in the inequality data wouldn't be useful for the purpose of this paper.

this one I use 15 cohorts. The results tabulated below show that although n affects the estimates for the levels of correlation and variance, it nearly does not have any effect on the percentage changes of these parameters. Furthermore, one can infer from my derivations that it is the percentage change in each parameter that can explain the changes in inequality and volatility.

	1968-1984		1985-2003			
	ρ_{pre}	σ^2_{pre}	ρ_{post}	σ^2_{post}	$\frac{\rho_{post}-\rho_{pre}}{\rho_{pre}}$	$\frac{\sigma^2_{post}-\sigma^2_{pre}}{\sigma^2_{pre}}$
United States (n=45)	0.043	0.015	0.007	0.021	%-83	%48
United States (n=15)	0.015	0.041	0.003	0.061	%-83	%51

These numbers can be compared with the finding of Nichols (2008) for the volatility in log family incomes, and with the finding of Gorbachev (2007) for the correlation between changes in the income of families. With regards to correlation, here it is found that there has been an approximately 80% decrease in this term between two time periods. This result is totally consistent with the finding of Gorbachev (2007). With regards to variance, this paper finds that there has been an approximately 50% increase in this term between two time periods, while Nichols (2008) shows that it has increased around 30%. Furthermore, even the bias in the estimates of the change in the variance terms makes sense. This is because I use relatively short data that could be easily affected from high realizations that can be seen any distribution. And as it was already indicated before and can be shown in Figure 6, in the beginning of 1990's all of the countries suffer from a recession that creates high realization of deviation from the mean growth rate of GDP, including for the US. So once volatility is calculated from this data, we may find a value higher than its long term average value. Besides, this may not be reflected in the variance of individual income shocks found by Nichols, as he calculates them at time t by a using a time window using plus and minus 2 years of information. Finally, when I calibrate my finding for the level of correlation to that in the data found by Gorbachev, I find the number of cohorts as 25.

The effect of the increase in the variance of the shocks is shown to increase both volatility and inequality. The effect of the decline in the correlation of the shocks is shown to decrease volatility and increase inequality. This result complements the inequality literature that only uses the increase in the variance of the shocks to explain the recent changes in the data. With regard to volatility, it points out that the decline in correlation outweighs the effect of the increase in the variance.so that the decline in the aggregate volatility data can be observed. Finally, the finding that it took 25 years for the US inequality to adjust the changes in the second moments of the shocks due to the cohort structure, questions the result of time series regressions that relate aggregate inequality to any other variable of interest. As these regressions estimate comtemporaneous relation between the variables of interest.

I only try to find the changes in the variance and correlation terms for the US, as there is existing empirical estimates from the micro data for this country that I can compare my results with. However, I already pointed out that the decline in volatility and increase in inequality is the common pattern of the data throughout the countries[16]. Hence, the evidence from the US, together with my

[16] The Netherlands is an exception to this generalization. However, in the second chapter of this thesis I discuss that the increase in the trade liberalization is responsible from the structural changes in these economies and the decline in correlation. And at the time inequality data started to be available for the Netherlands, the share of exports and imports in GDP for this country had already exceeded %100, the point which I further discuss that the effect of the liberalization shall have reversed.

derivations for inequality and volatility in terms of variance and correlation of the shocks, let me infer that the decline in the correlation is mainly responsible for the changes in data.

1.6 Concluding Remarks

This study investigates the link between inequality and volatility, and also the source behind the increase in inequality and decrease in volatility seen across many of industrialized countries in the recent decades. I show that inequality and volatility are connected through the variance and cross-sectional correlation of individuals' income shocks. I decompose the changes in the variables into the changes in these parameters and deduce that the main source behind the recent changes in the variables in this sample is the decline in the correlation. This last experiment also reveals the dynamics of aggregate inequality. It, both theoretically and quantitatively, shows that this variable gives a long-lasting response to the structural changes in the economy, happening due to its structure based on replacement of cohorts. This finding questions the result of time series regressions that relate aggregate inequality to any other variable of interest. As these regressions estimate comtemporaneous relation between the variables of interest.

I believe my result indicating the decline in the correlation of individuals' incomes has a potential to guide new theoretical models explaining the changes in inequality and volatility. It might also put shadows on some of the current explanations for each phenomenon if they do not address the changes in micro data correctly. Finding the true source of the changes in the data, whether they occur due to a change in the volatility of individuals' incomes or a change in the correlation of these fluctuations, is important as each possible source would require different precautionary policies to implement. For example, one of the measures to evaluate welfare effect of macro policies is the effect of these policies on GDP volatility. If volatility and inequality are related, inequality outcomes of these policies should also be taken into account.

I have used income inequality data throughout my estimations. However, the results derived in this paper from the time series structure of this index data remain valid when wage inequality is considered. This is because majority of income inequality arises from differences in wage earnings and these two data series follow very similar patterns. This is seen in Figure 11.

My sample consists of industrialized countries which provide sufficient inequality data to carry out structural break analysis. Based on the facts documented in this paper, the resulting theory used to identify changes in variance and correlation terms can henceforth be applied to countries with relatively limited inequality data. However, throughout this paper some assumptions were kept for derivations, such as constant distribution of annual growth rate of individuals' incomes, or the lack of population effect. If the results are applied to developing countries, it should be taken into account that those countries may have experienced sharp changes in the distribution of those parameters, which either due to their relatively unstable fiscal or monetary policies, or to their population growth rates.

1.7 APPENDIX

A-Details on the Derivations

Proposition 1 *if* $y_{it}= \alpha + y_{it-1}+v_{it}$ *and* $Cov(y_{it-1}, v_{it}) = 0$, *then* $E_{ts}\left(Var_{cs}\left(\Delta y_{it}|t\right)\right) = E_{ts}\left(\Delta Var_{cs}\left(y_{it}|t\right)\right)$

Proof. The right hand side of the equation can be written as

$$E_{ts}\left(\Delta Var_{cs}\left(y_{it}\right)\right) = E_{ts}\left(Var_{cs}\left(y_{it}\right) - Var_{cs}\left(y_{it-1}\right)\right).$$

Using the income process we have, $y_{it}= \alpha + y_{it-1}+v_{it}$, the term $E_{ts}(Var_{cs,t}\left(y_{it}\right))$ can be written so that:

$$y_{it}= \alpha + y_{it-1}+v_{it} \quad \Rightarrow \quad E_{ts}(Var_{cs}\left(y_{it}\right))= E_{ts}(Var_{cs}\left(y_{it-1}\right) +Var_{cs}\left(v_{it}\right)),$$

which is true if and only if v_{it} is uncorrelated with y_{it-1}, as I assumed for permanent shocks. This equation says that today's variance can be written as previous term's variance plus the variance of shocks affecting individuals today. Inserting this term into the first equation finds that

$$\Rightarrow \quad E_{ts}\left(\Delta Var_{cs}\left(y_{it}\right)\right) = E_{ts}\left(Var_{cs}\left(v_{it}\right)\right),$$

implying that

$$\Rightarrow \quad E_{ts}\left(\Delta Var_{cs}\left(y_{it}\right)\right) = E_{ts}(Var_{cs}\left(v_{it}\right) = E_{ts}(Var_{cs}\left(\Delta y_{it}\right)).$$

■

Proposition 2 *if* $y_{it} = \alpha + y_{it-1} + \Delta u_{it} + v_{it}$ *and* μ_t *an i.i.d. shock with* $var(\mu_t) = var(v_t) + var(u_t)$ *and let* $\beta = var(u_t) / (var(v_t) + var(u_t))$ *, then* $Var(\mu_t - \beta\mu_{t-1}) \simeq Var(v_t + \Delta u_t)$ *and* $Cov(\mu_t - \beta\mu_{t-1}, \mu_{t-1} - \beta\mu_{t-2}) = Cov(v_t + \Delta u_t, v_{t-1} + \Delta u_{t-1})$

Proof. The variance of the transformed process can be found by

$$Var(\mu_t - \beta\mu_{t-1}) = (1+\beta^2)var(\mu_t) = \frac{(var(v_t) + var(u_t))^2 + (var(u_t))^2}{(var(v_t) + var(u_t))^2}((var(v_t)+var(u_t)),$$

so that

$$Var(\mu_t - \beta\mu_{t-1}) = \frac{(var(v_t) + var(u_t))^2 + (var(u_t))^2}{var(v_t) + var(u_t)}.$$

The variance of the original process is equal to

$$Var(v_t + \Delta u_t) = var(v_t) + 2var(u_t),$$

which further can be written as

$$Var(v_t + \Delta u_t) = \frac{(var(v_t) + var(u_t))^2 + (var(u_t) * (var(u_t) + var(v_t))}{var(v_t) + var(u_t)},$$

so $Var(\mu_t - \beta\mu_{t-1})$ equals to $Var(v_t + \Delta u_t)$ if and only if

$$var(u_t)\, var(v_t) \simeq 0,$$

which should be a reasonable approximation. It further implies

$$\Delta y_t = v_t + \Delta u_t \simeq \mu_t - \beta\mu_{t-1.}$$

Covariance of the transformed process can be found by

$$\begin{aligned} Cov\left(\mu_t - \beta\mu_{t-1}, \mu_{t-1} - \beta\mu_{t-2}\right) &= -\beta var\left(\mu_{t-1}\right) \\ &= -\frac{var\left(u_{t-1}\right)}{var\left(v_{t-1}\right) + var\left(u_{t-1}\right)}\left(var\left(v_{t-1}\right) + var\left(u_{t-1}\right)\right) \end{aligned}$$

that reduces to

$$Cov\left(\mu_t - \beta\mu_{t-1}, \mu_{t-1} - \beta\mu_{t-2}\right) = -var\left(u_{t-1}\right).$$

Covariance of the original process, on the other hand, is equal to

$$Cov\left(v_t + \Delta u_t, v_{t-1} + \Delta u_{t-1}\right) = -var\left(u_{t-1}\right),$$

so they are same. ■

Proposition 3 *if* $y_{it} = \alpha + y_{it-1} + \Delta u_{it} + v_{it}$ *where* v_{it} & u_{it} *are i.i.d. shocks with* $Var_{ts}\left(v_{it}\right) = \sigma_{v,t}^2$ & $Var_{ts}\left(v_{it}\right) = \sigma_{u,t}^2$, $Cov\left(v_{it}, v_{jt}\right) = \rho_v\sigma_v^2$ & $Cov\left(u_{it}, u_{jt}\right) = \rho_u\sigma_u^2$ *, then* $Vol = \frac{1}{T}\sum_{t=1}^{T}(\overline{\Delta y_t} - E_t(\overline{\Delta y_t}))^2 = \rho_u\sigma_u^2 + \rho_v\sigma_v^2$

Proof. Volatility is defined as deviation of the average individual income growth from its long term mean, α

$$Vol = Var_{ts}\left(E_{cs}\left(\Delta y_{it}\right)\right) = \lim_{T\to\infty}\frac{1}{T}\sum_{t=1}^{T}\left(\overline{\Delta y_t}-\alpha\right)^2 \quad \text{for each } t,$$

where the average individual income growth at time t is

$$\overline{\Delta y_t} = \lim_{N\to\infty}\frac{1}{N}\sum_{i=1}^{N}\Delta y_{it}.$$

If I add chosen income process into this definition, it finds

$$\overline{\Delta y_t} = \lim_{N\longrightarrow\infty}\frac{1}{N}\sum_{i=1}^{N}\Delta y_{it} = \lim_{N\longrightarrow\infty}\frac{1}{N}\sum_{i=1}^{N}(\alpha+\Delta u_{it}+v_{it}) = \lim_{N\longrightarrow\infty}(\alpha+\sum_{i=1}^{N}\frac{\Delta u_{it}}{N}+\sum_{i=1}^{N}\frac{v_{it}}{N}).$$

volatility reduces to

$$Vol = \lim_{T\longrightarrow\infty}\frac{1}{T}\sum_{t=1}^{T}\left[\lim_{N\longrightarrow\infty}(\sum_{i=1}^{N}\frac{u_{it}}{N}+\sum_{i=1}^{N}\frac{v_{it}}{N})^2\right].$$

Inserting the second moment characteristics of the shocks into this equality

$$Vol = \lim_{T\longrightarrow\infty}\frac{1}{T}\sum_{t=1}^{T}\left[\lim_{N\longrightarrow\infty}(\frac{1}{N^2}(N\sigma_u^2+N(N-1)\rho_u\sigma_u^2)+\frac{1}{N^2}(N\sigma_v^2+N(N-1)\rho_v\sigma_v^2))\right],$$

implies that

$$Vol = \lim_{T\longrightarrow\infty}\frac{1}{T}\sum_{t=1}^{T}(\rho_u\sigma_u^2+\rho_v\sigma_v^2) = \rho_u\sigma_u^2+\rho_v\sigma_v^2.$$

■

Proposition 4 *if* $y_{it} = \alpha + y_{it-1} + \Delta u_{it} + v_{it}$ *where* v_{it} & u_{it} *are i.i.d. shocks with* $Var_{ts}(v_{it}) = \sigma^2_{v,t}$ & $Var_{ts}(v_{it}) = \sigma^2_{u,t}$, $Cov\left(v_{it}, v_{jt}\right) = \rho_v \sigma^2_v$ & $Cov(u_{it}, u_{jt}) = \rho_u \sigma^2_u$ *, then* $C.I. = E_{ts}(\Delta Var_{cs,t}(y_{it})) = (1-\rho_v)\sigma^2_v$

Proof.

$C.I.(within\ cohort) = E_{ts}(\Delta Var_{cs}(y_{it})) = E_{ts}(Var_{cs}(y_{it})) - E_{ts}(Var_{cs}(y_{it-1}))$

Using the defined income process, $y_{it} = \alpha + y_{it-1} + \Delta u_{it} + v_{it}$, I can write $Var_t(y_{it})$ as

$$Var_{cs}(y_{it}) = Var_{cs}(y_{it-1}) + Var_{cs}(\Delta u_{it} + v_{it}) + 2Cov_{cs}(y_{it-1}, \Delta u_{it} + v_{it}),$$

so the first term reduces to

$$\Rightarrow \quad E_{ts}(\Delta Var_{cs}(y_{it})) = E_{ts}(Var_{cs}(\Delta y_{it})) + 2E_{ts}(Cov_{cs}(y_{it-1}, \Delta u_{it} + v_{it})).$$

Hence, the change in the variance of log income arises from the variance of the changes in the log incomes, plus the covariance term. If the latter term is positive; i.e. if individuals with higher previous incomes also get larger positive income shocks, the change in inequality will be higher. I can start from deriving the first term affecting inequality:

$$E_{ts}(Var_{cs}(\Delta y_{it}) = \lim_{T \longrightarrow \infty} \frac{1}{T} \sum_{t=1}^{T} \lim_{N \longrightarrow \infty} \frac{1}{N} \sum_{i=1}^{N} (\Delta y_{it} - \overline{\Delta y_t})^2,$$

which measures the dispersion of individual income growth rates around their mean. Inserting the income process into this definition:

$$E_{ts}(Var_{cs}(\Delta y_{it}) = \lim_{T\longrightarrow\infty}\frac{1}{T}\sum_{t=1}^{T}\lim_{N\longrightarrow\infty}\frac{1}{N}\sum_{i=1}^{N}(\Delta u_{it}+v_{it}-\sum_{j=1}^{N}\frac{\Delta u_{jt}}{N}-\sum_{j=1}^{N}\frac{v_{jt}}{N})^2$$

$$=\lim_{T\longrightarrow\infty}\frac{1}{T}\sum_{t=1}^{T}\lim_{N\longrightarrow\infty}\frac{1}{N}\sum_{i=1}^{N}(u_{it}-\sum_{j=1}^{N}\frac{u_{jt}}{N})^2+(-u_{it-1}+\sum_{j=1}^{N}\frac{u_{jt-1}}{N})^2+(v_{it}-\sum_{j=1}^{N}\frac{v_{jt}}{N})^2$$

implies that

$$\lim_{T\longrightarrow\infty}\frac{1}{T}\sum_{t=1}^{T}2(\sigma_u^2-2\rho_u\sigma_u^2+\rho_u\sigma_u^2)+(\sigma_v^2-2\rho_v\sigma_v^2+\rho_v\sigma_v^2)=2(\sigma_u^2-\rho_u\sigma_u^2)+(\sigma_v^2-\rho_v\sigma_v^2).$$

Now I will derive the second, covariance, term affecting change in the inequality

$$Cov_{cs}(y_{it-1},\Delta u_{it}+v_{it})=E_{ts}((\Delta u_{it-1}-\sum_{j=1}^{N}\frac{\Delta u_{jt-1}}{N})(\Delta u_{it}-\sum_{j=1}^{N}\frac{\Delta u_{jt}}{N}),$$

which further, and maybe more intuitively, can be written as

$$Cov_{cs}(y_{it-1},\Delta u_{it}+v_{it})=E_{ts}((u_{it-1}-\sum_{j=1}^{N}\frac{u_{jt-1}}{N})(-u_{it-1}+\sum_{j=1}^{N}\frac{u_{jt-1}}{N}))=-(\sigma_u^2-\rho_u\sigma_u^2).$$

Now I can insert both of the findings into the main equation

$$\Rightarrow\quad E_{ts}\left(\Delta Var_{cs}\left(y_{it}\right)\right)=E_{ts}(Var_{cs}\left(\Delta y_{it}\right))+2E_{ts}(Cov_{cs}(y_{it-1},\Delta u_{it}+v_{it}))=\sigma_v^2-\rho_v\sigma_v^2.$$

■

B-Details on the Structural Break Estimation

Estimating Breaks in the Unconditional Means

The first equation to estimate is

$$Var_{cs}\left(y_{it}\right) = c^n + \beta^n t + \varepsilon_{it}. \tag{2}$$

However, there a problem with estimating this equation with the program employed: it doesn't allow use of non-stationarity regressors i.e. to include trend in equation (2). It allows usage of a scaled trend, t/T, but then limiting distributions are not valid. However, this problem can be overcome within the framework of this paper by using t/T. But relying only on the consistency of this estimator in finding the break dates, but not on the confidence intervals around them. Then if the break date within this series stays in the confidence interval of the break within the other series (but not vice versa), I can proceed. So the modified version of the equation 2 is

$$Var_t\left(y_{it}\right) = c + \beta(t/T_L) + \varepsilon_{it}, \tag{2*}$$

where T_L is the total length of the data and $t = 1, ..., T_L$ is the time trend.

There are still 2 crucial points to be careful about in application of Perron and Qu (2007) code that uses multiple equations at a time[17].

[17] In application of this code, and throughout all the estimations, both different moment matrixes of the regressors and different variance of the residuals across segments are separated by breaks are allowed for. In fact the change in the covariance matrix between segments means a change in regression coefficients is not only being tested for, but the model regime is switched. But it is the usual option while searching for a break in the regression coefficients in similar programs (including Quandt LR test). Since not doing that is equivalent to forcing a program

1) Along with the main equation, an ancillary must be produced, the residual of which is better to be correlated with residual of the main one

2) The ancillary equation should not show any break, so that the program finds the break in the main equation[18]

Before explaining the method, the equations are re-written consistent with searching for multiple breaks:

$$(\Delta y_t^{pc} - \overline{\Delta y_t^{pc}})^2 = c^n + \varepsilon_{vt} \tag{1}$$

$$Var_t\,(y_{it}) = c^n + \beta^n(t/T_L) + \varepsilon_{it} \tag{2*}$$

With m break dates, $[T^1, ..., T^m]$, there are $m+1$ regimes $n = 1, ..., m+1$. The regime n includes the time interval between break dates $[T^{n-1}+1, T^n]$[19].

Now I have to choose data to be used in ancillary equations along with equations (1) and (2*), this is to increase the precision of the estimates regarding the size of the confidence intervals around the break dates. A necessary condition for these equations to satisfy is that their mean should be constant and the residual should be correlated with ε_{vt} or ε_{it}. For that purpose, to test for a break in equation (1), the residuals of equation (7) were used, and to test for a break

to keep the same covariance matrix of error terms between segments, this would lead a bias in the estimations. This point should be obvious from Figure 2 in Eksi (2009).

[18] The program can actually be restricted to prohibit the search for a break in the ancillary equation, so that it is only able to find a break in the main equation even if there is one in the second. However, if it is not restricted and furthermore if this equation does not inherit a break (as checked by testing auxiliary equation previously), then precision of the estimates increases.

[19] Although a common notation for number of breaks (and for name of the regimes) is used, it is clear that their actual number in one series is independent from the other.

in equation (2), the residuals of equation (6) were used, which were

$$\Delta y_t^{pc} = c^n + \beta^n(L) * \Delta y_t^{pc} + e(v)_t, \tag{6}$$

$$Var_t\,(y_{it}) = c^n + \beta^n(L) * Var_t\,(y_{it}) + e(i)_t. \tag{7}$$

Hence, while testing for a break in volatility, the whole system of equations becomes:

$$(\Delta y_t^{pc} - \overline{\Delta y_t^{pc}})^2 = (v_t)^2 = c^n + \varepsilon_{vt}, \tag{1}$$

$$e(i)_t = c^n + \epsilon_{gt}, \tag{10}$$

and to search for a break in the variance of log income index they are

$$Var_t\,(y_{it}) = c^n + \beta^n(t/T_L) + e(i)_t. \tag{2*}$$

$$e(v)_t = c^n + \epsilon_{yt}. \tag{11}$$

While running equations (6) & (7), two lags of the endogenous variables were used. This is because yearly data is used and further lags weren't significant; besides, using more lags would just reduce the sample size. As multiple breaks are allowed for in equations (6) & (7), the mean of their residuals is expected to be 0, which is one of the required conditions (this result is confirmed by applying structural break analysis once again to the residuals). As both variables are based on income, it can also be expected that the residual of one variable is correlated with the other.

But would using the same data for both of the equations cause a bias? The employed program code demonstrates the lack of bias with regard to the consistency of estimates. Yet, I still compare the results of these estimations with the Perron and Qu(2006) estimator that use one equation at a time but also with the results found by Lee and Strazicich (2004)'s methodology, and they all show almost one-to-one similarity. However, it is not certain that doing so could affect the confidence interval of the estimates, since in this case the break dates in two different series are not identified independently. Yet, possible third and fourth variables to be used in ancillary equations were not chosen since this approach would not be feasible. It should be noted that alternative estimators found larger confidence intervals around the break dates, so their findings for the coincidence of breaks were even stronger.

If there is any question left concerning the estimation procedure, it would be whether the errors in the regressions are normally distributed, as QML was used. However, QML doesn't make the normality assumption, but rather operates as if the world is normal, and is in many cases robust to non-normality[20].

Estimating Breaks in the Variance of Innovations

According to the income model with transitory income shocks, the system of equations I use for volatility are:

$$\mid (e(v)_t \mid = c^n + \xi_{vt}, \tag{8}$$

$$e(i)_t = c + \epsilon_{gt}, \tag{10}$$

[20] In addition, the null hypothesis of normality was also tested. The Anderson-Darling test was applied for this purpose, which is known for its effectiveness in small samples (≤ 25). For most of the cases, this test could not reject the null that residuals are normally distributed. For the cases in which the test rejected normality, a normality test was run again within each segment of the data separated by the break and this time nearly all remaining data passed the tests.

and for inequality

$$| (e(i)_t |= c^n + \xi_{it}, \tag{9}$$

$$e(v)_t = c + \epsilon_{yt}. \tag{11}$$

Tables & Figures

Figure 2 : Inequality Index and Standard Deviation of GDP Growth

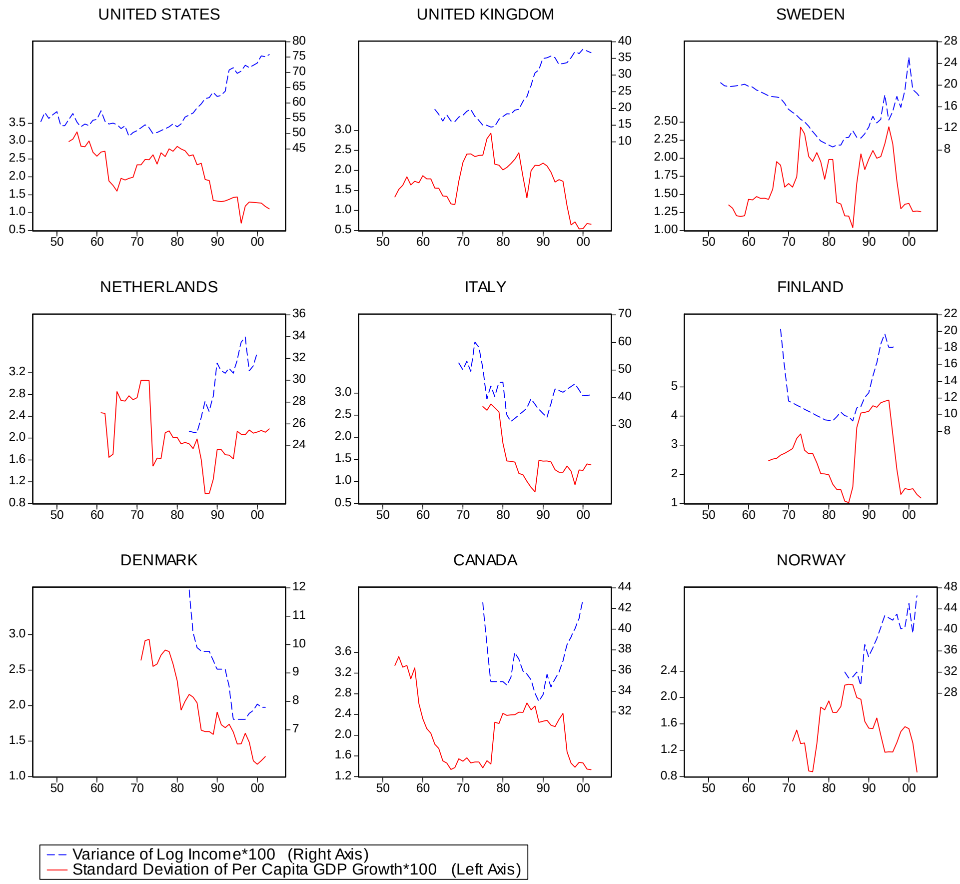


Figure 6 : Breaks in Per Capita GDP Growths

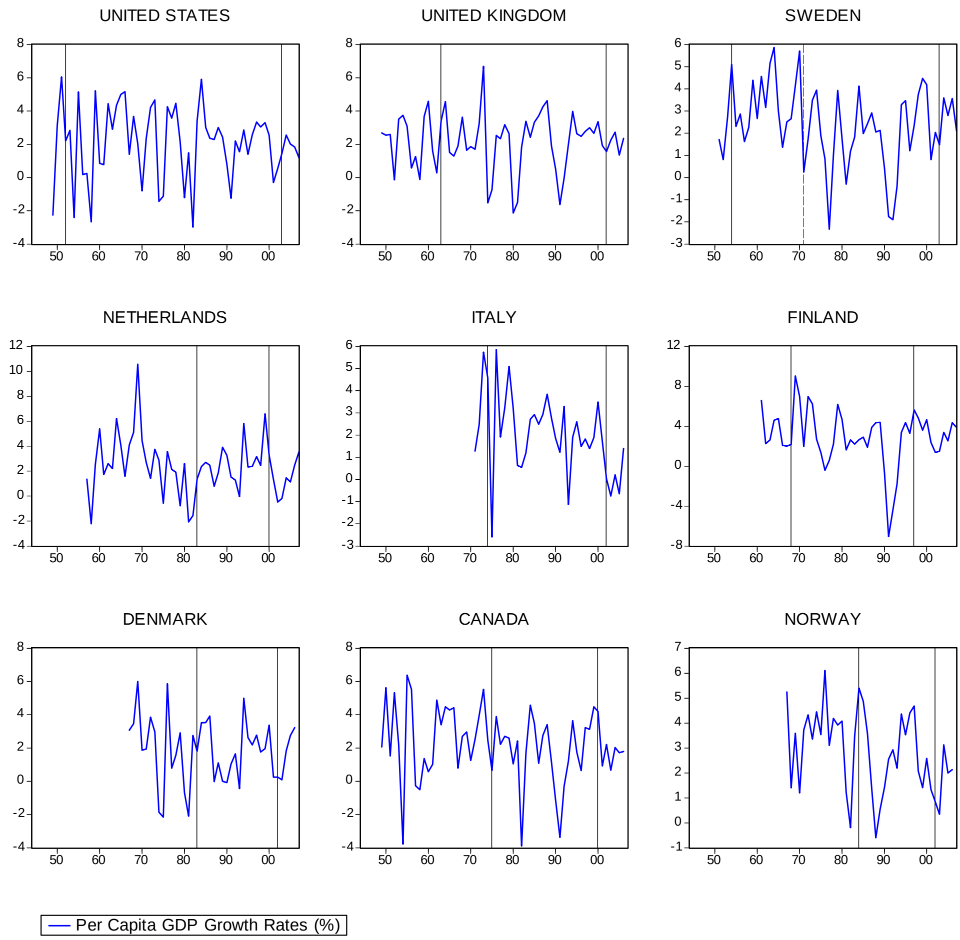
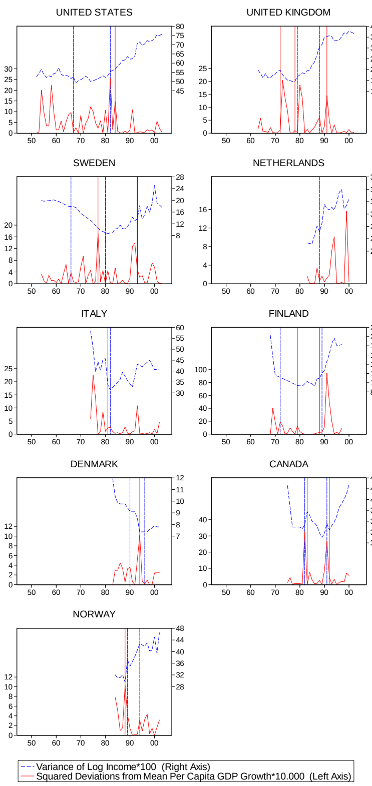


Figure 8 : Breaks in the Unconditional Means



* breaks that are significant in %10 are shown

* thicker lines demonstrate coincident breaks

INEQUALITY

	BREAK DATES			CORR.
UNITED STATES				
	1967	**1982**		-0.08
	63 68	81 83		
	64 68	81 83		
UNITED KINGDOM				
	1979	**1988**		0.00
	78 80	87 89		
	78 80	87 89		
SWEDEN				
	1966	**1980**	**1993**	0.14
	65 67	79 80	92 99	
	65 67	79 80	92 98	
NETHERLANDS				
	1988			-0.39
	87 89			
	87 89			
ITALY				
	1982			0.28
	81 83			
	81 83			
FINLAND				
	1972	**1989**		0.24
	72 73	86 87		
	72 73	86 87		
DENMARK				
	1990	**1996**		-0.58
	89 92	95 97		
	89 91	95 97		
CANADA				
	1982	**1991**		-0.73
	81 83	90 92		
	81 83	90 92		
NORWAY				
	1989	**1994**		0.58
	88 90	93 94		
	88 90	93 94		

VOLATILITY

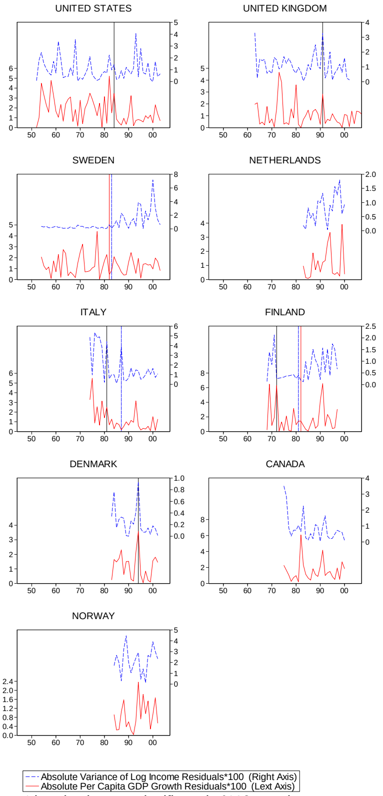
	BREAK DATES			CORR.
UNITED STATES				
	1984			0.02
	73 86			
	80 86			
UNITED KINGDOM				
	1972	**1978**	**1991**	-0.02
	71 73	77 79	87 92	
	71 73	77 79	89 92	
SWEDEN				
	1977	**1993**		0.18
	80 86	92 95		
	80 85	92 94		
NETHERLANDS				
				-0.16
ITALY				
	1981			-0.07
	75 82			
	76 82			
FINLAND				
	1979	**1988**		0.15
	74 80	87 94		
	76 80	87 92		
DENMARK				
	1994			-0.68
	92 96			
	93 96			
CANADA				
	1983	**1992**		0.16
	78 84	91 93		
	79 84	91 93		
NORWAY				
	1988			-0.29
	87 89			
	87 89			

* bold numbers indicates the ones statistically significant in %5, others in %10

* first row indicates %95 confidence interval and the second one includes %90

* "Corr." columns show the correlations between error terms of estimated eq.s

Figure 9 : Breaks in the Variance of Innovations

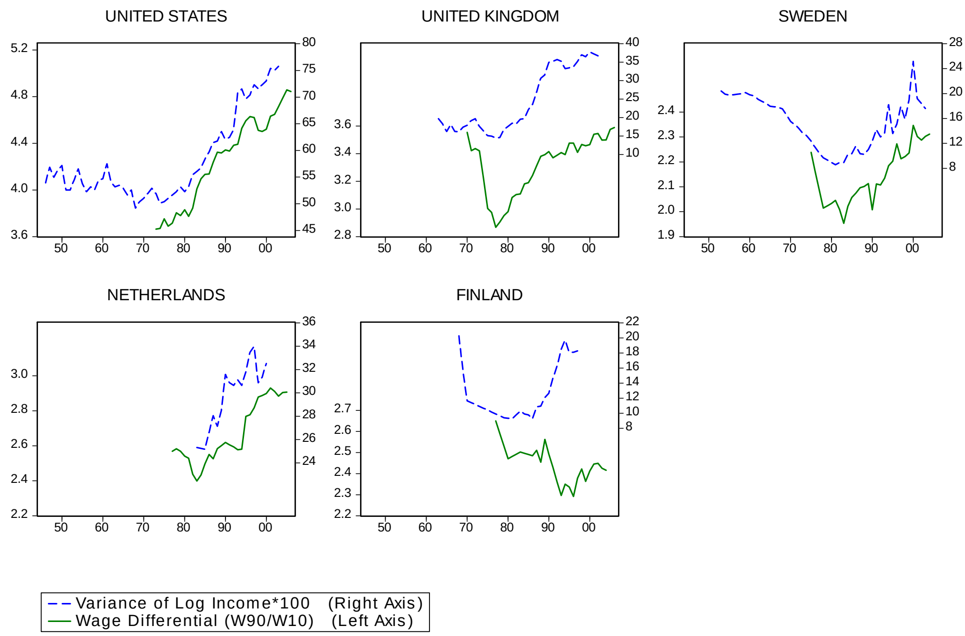


* breaks that are significant in %10 are shown
* thicker lines demonstrate coincident breaks

	INEQUALITY BREAK DATES			INEQUALITY CORR.	VOLATILITY BREAK DATES			VOLATILITY CORR.
UNITED STATES	**1984** 83 85 83 85			-0.21	**1984** 76 89 78 87			-0.15
UNITED KINGDOM	1991 85 92 87 92			0.10	1991 89 92 90 92			-0.03
SWEDEN	**1983** 82 85 82 84			0.13	**1982** 81 90 81 88			0.11
NETHERLANDS				0.21				-0.12
ITALY	**1981** 77 82 78 82	**1987** 86 88 86 88		0.33	**1981** 76 82 78 82			0.29
FINLAND	**1972** 71 73 71 73	**1981** 80 82 80 82		-0.12	**1972** 70 73 71 73	**1982** 80 83 80 83		0.14
DENMARK	**1994** 93 95 93 95			0.52	**1994** 87 95 89 95			-0.58
CANADA				-0.04				0.32
NORWAY				-0.02				-0.14

* bold numbers indicates the ones statistically significant in %5, others in %10
* first row indicates %95 confidence interval and the second one includes %90
* "Corr." columns show the correlations between error terms of estimated eq.s

Figure 11: Income and Wage (W90/W10) Inequalities



II. Trade and Inequality in the US and UK: Cohort Perspective

2.1 Introduction

There is much literature regarding the effect of trade liberalization on increasing inequality across developed countries, and a substantial part of this literature explains this effect through the rising skill premium. This link between trade and inequality is based on predictions of trade theory. According to Stolper-Samuelson (1941), trade globalization should increase within-country earning inequalities in the developed regions of the world due to the increase in demand for goods produced by skilled labour (North and South trade). Earlier inequality literature analyses trade in outputs however finds little support for this theory. Most of these studies use US data, and although some studies find a significant effect (Wood 1994, Leamer 1996), most conclude that trade volume is not sufficient to explain much of the increase in inequality (Lawrence and Slaughter 1993, Sachs and Shatz 1996, Katz and Murphy 1992). More recent literature also considers intermediate input trade (Feenstra and Gordon 1995, 1996); this is theoretically motivated by two factors. First, by the increasing returns to scale for intermediate inputs which concentrates each input production in one place, and secondly by the variety of trade which increases the number of inputs used in the production of the final good, which leads to an increase in the amount of the final good produced, consequently creating an incentive for trade (Krugman 1979, 80).

Most of this type of trade takes place between developed countries (Miroudot et al. 2009). Even though new trade theory is more successful in explaining the rise in earning inequality, it is still unable to account for much of this rise. Moreover, trade literature tends to indicate that skill-biased technological change has been a determinant of inequality more important than trade itself (Acemoglu 2000, Krugman 2000).

However the skill premium explanation is only one dimension of wage inequality. It measures wage dispersion between two groups; skilled and unskilled labor. But if we divide the labor force into such groups, we also need to consider that wage inequality could change due to the changes in within-group variances. Both Juhn, Murphy and Pierce (1993), and Gottschalk (1997) document that inequality increased not only between experience and education categorized groups, but also within these groups. Gottschalk (1997) further documents that these two sources are equally important to the rise of earning inequality. Finally, trade can also affect earning inequality by changing the relative share of skilled and unskilled labor in the labour force (as it shifts labour force away from agriculture for example).

There are also factors through which trade may affect income but not wage inequality. Giovanni and Levchenko (2006) document that the productions of sectors that are open to international trade are less correlated with the rest of the economy. This is intuitive as traders are subject to foreign shocks (exporters may well be subject to foreign demand shocks and importers to the foreign supply shocks). This openness to shocks, as I discuss in the previous chapter, is expected to increase income inequality. Comin and Philippon (2006) address the increase in the turnover of market shares resulting from increasing competition in the product market. One can also make several arguments on how these factors affect income inequality.

Contrary to the large amount of literature attempting to explain the effect of trade on the between-group wage inequality (skill premium), which does so with little success, the literature regarding the other channels through which trade can affect inequality is scarce. To study the change in inequality taking into account all the possible channels, there are regression analyses that link trade to aggregate inequality. These studies also find weak support for the same question, some even show that trade moderates the inequality across a sample of industrialized countries (IMF WEO, 2007). Hence, referring both to studies exploiting between-group and aggregate information, one may conclude that international trade is not a substantial determinant of the rise in inequality.

In this chapter I present contrary evidence showing trade as an important determinant of inequality. I first provide graphical evidence showing that changes in inequality occur at the time of increasing trade (and financial) liberalization across sample of advanced countries. I also discuss that trade may be a relevant factor in explaining inequality due to the decline in the correlation of income shocks, as found in the previous chapter. I then explain the lack of support for the variable relation found in time series regression estimates. My conjecture is based on a finding of mine in the first chapter of this thesis. There, I discuss how aggregate inequality gives enduring response to the changes in the structure of the economy so long as these changes affect the income pattern of individuals. I claim that at the time of change in income patterns, the old cohorts have already been affected by the previous realizations of incomes; hence, the change in inequality should last until the old cohorts leave the population and are replaced by today's young cohorts. Consequently, regression estimates that measure instantaneous correlation between trade and inequality show insignificant results. I further analyze this issue and suggest an alternative measure of inequality to be used in regression estimates: the growth rate of within-cohort inequality. I begin by, analyzing the inequality measures and derive the difference between average growth rate of within-cohort inequalities and growth in aggregate (average within-cohort) inequality. My results indicate that the measure of inequality to be used in regression estimates is the average growth rate of within-cohort inequalities, though using the growth rate of the aggregate Gini would also improve the estimates significantly. Then I take my derivations to the data, and revisit the effect of trade on increasing inequalities in the US and UK. I chose these countries on the basis of availability of high quality individual income data which I use to construct within-cohort inequalities.

I construct within-cohort inequality data from the repeated cross sectional data sets (as it is called pseudo panel or synthetic cohort data). I start my estimation by replicating IMF WEO (2007) estimates which explain the change in the Gini coefficient across a sample of countries by using a set of variables including the trade share of GDP. I first use the aggregate Gini index for the purpose of replicating the IMF results for the US and UK, and then use a panel of within-cohort Gini indexes from these countries in the same regression. The results found by aggregate indexes are consistent with IMF estimates: trade has led to a decrease in inequality in these countries, although the estimated coefficients are not significant. Using the growth rate of the aggregate inequality data, on the other hand, , substantially improves the estimates in terms of standard errors, and also the coefficient of trade becomes less negative in the US and positive in the UK. Finally, the results obtained with the growth rate of cohort inequalities indicate that trade had a positive and significant effect on inequality in the US and UK.

Using within-cohort dispersion of incomes is not a perfect measure of inequality. This is because people in the same cohort age simultaneously, meaning that they have the same level of experience throughout their life spans. Hence, inequality cannot be expected to increase due to changes in the return to experience. However, return to experience is part of the skill premium, and the trade effect on skill premium has already been shown significant by the previously described literature. As a result, compared to the aggregate inequality index, the cost of using within-cohort inequality data should be small.

The rest of the paper is organized as follows. Section 2 reviews the inequality measures and explains the difference between average growth in within-cohort inequality and growth in aggregate (average within-cohort) inequality. In section 3, I discuss why trade would be relevant in explaining the recent increases of inequality in the US and UK, i.e., how it may cause the decline in the correlation between income shocks, which I find in the previous chapter. Section 4 presents the framework for the empirical analysis. The data and its sources are summarized in Section 5 and Section 6 presents the empirical results. Section 7 concludes.

2.2 Average Growth in Within-Cohort Inequality vs Growth in Average Within-Cohort (Aggregate) Inequality

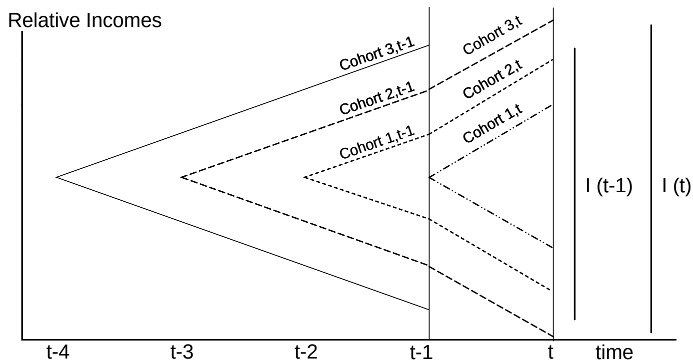
The main measures of inequality used in the literature are the skill premium, the Gini coefficient, and the variance of log wages (incomes). The skill premium measures the dispersion between earnings of two groups of individuals, not the change in the variance of earnings within each group. The rest of the inequality indexes can be used to account for a change in the aggregate inequality. However I claim and discuss in this section that regression estimates using aggregate inequality data can be subject to problems that would stem from the time effect. I propose the use of average growth in within-cohort inequality data instead of aggregate inequality datas, which can help us to overcome that problem. To this end, I need the refer to the section "Reduced Form Model of Aggregation" of the first chapter, where I define a path for log real income y for N individuals i over time periods t

$$y_{it} = \alpha_i + y_{it-1} + v_{it}$$

In that section I explain how the variance and cross sectional correlation characteristics of the income shocks, v_{it}, can affect the aggregate inequality through the cohort structure of the economy[1]. At that time I only needed to give an intuitive explanation for the evolution of aggregate inequality and for that purpose I used Figures 3 & 4. For the sake of convenience I repeat Figure 4 here.

[1] In principle, the change in cross-sectional difference in the long term growth rate of incomes, α_{it},in time can also be used to model the change in the income distribution of individuals rather than the change in variance and correlation of individuals' income shocks, but there is no such practice in literature. Also in the first chapter suppress the (i) index but not here. This is because there I made it obvious that this term cannot affect the aggregate inequality. But since the concern here is the within cohort inequality, I prefer to keep it, though eventually it will not change the results here either.

Figure 4 (Ch1): Illustrative Effect of a Decline in the Correlation on Inequality



This figure uses three cohorts, each having life span of 3 years. (Cohort 2,t) indicates those individuals who are 2 years old at time t. The vertical axis on the left shows the relative income of individuals. I use this figure to compare the income inequalities at time t and $t-1$ upon the decline in the cross sectional correlation of individuals' incomes at time $t-1$. Figure () shows an increase in inequality from $t-1$ to t in both within-cohort and aggregate inequalities. An important inference from this graph would be that the effect of such a level shift in correlation on the aggregate inequality should last for three periods. This happens since at the time of decline in correlation, the old cohorts have already been affected from the previous realizations of the shocks; hence only three years later, at time $t+2$, all the cohorts would completely adapt to that decline and reach its new steady state. In the previous chapter I find quantitative evidence justifying this. In this section I further derive its implication on the measures of inequality.

Consider an economy that is populated by individuals i of different ages a. An individual of age a at time t belongs to cohort $c = t - a$, where we index cohorts by birth year. Log income of each individual $y_{i,c,t}$ evolves according to

$$y_{i,c,t} = \alpha_i + y_{i,c,t-1} + \varepsilon_{it}$$

where ε_{it} is i.i.d. mean zero over time and has variance σ_t^2 and correlation ρ_t across individuals (we allow these moments to change over time) and α_i is an idiosyncratic trend, which is uncorrelated with everything else and has constant variance σ_α^2.

Calculating the variance within a cohort over time (related derivations are in the Appendix of the same first chapter):

$$D_{c,t} = D_{c,t-1} + \sigma_\alpha + (1 - \rho_t)\, \sigma_t^2 \tag{1}$$

where

$$D_{c,t} = var\left(y_{i,t,a}\right)$$

is within-cohort inequality.

Therefore, the changes in σ_t^2 and ρ_t affect inequality by means of the term: $(1-\rho_t)\,\sigma_t^2$, and the correct measure for this term is $D_{c,t} - D_{c,t-1}$. If we want to explore what caused changes in ρ_t or σ_t (or both), we should regress $\Delta D_{c,t}$ on candidates X_t.

$$\Delta D_{c,t} = \beta_0 + \beta_1 X_t + u_{c,t}$$

where we would expect $\beta_0 = \sigma_\alpha^2$ and $\beta_1 = d\left[(1-\rho_t)\,\sigma_t^2\right]/dX_t$. Clearly, this regression is equivalent (in terms of point estimates, not standard errors) to

$$\left(\overline{\Delta D_{c,t}}\right)_t = \beta_0 + \beta_1 X_t + \left(\overline{u_{c,t}}\right)_t \tag{3}$$

so we can also use the average change in within-cohort inequality.

What would happen if we used the change in average within-cohort inequality rather than the average change in within-cohort inequality?

$$\Delta\left(\overline{D_{c,t}}\right)_t = \gamma_0 + \gamma_1 X_t + v_t \tag{4}$$

Notice that if the population is constant and if shocks are uncorrelated across cohorts, then aggregate inequality equals average within-cohort inequality. This should give us fairly good insight into what happens if we use aggregate inequality data.

To answer this question we need to make some additional assumptions. Assume that a new cohort is born in each period and that everyone dies when they turn A years old. That means that at any point in time t, there will be in total A cohorts alive. Assume further that all cohorts have the same level of inequality at birth, which I will denote by D_0. Iterating the expression for $D_{c,t}$ backward until birth gives

$$\begin{aligned} D_{c,t} &= D_{c,t-1} + \sigma_\alpha^2 + (1-\rho_t)\,\sigma_t^2 \\ &= D_{c,t-2} + 2\sigma_\alpha^2 + (1-\rho_t)\,\sigma_t^2 + \left(1-\rho_{t-1}\right)\sigma_{t-1}^2 \\ &= D_0 + a\sigma_\alpha^2 + \sum_{s=1}^{a}\left(1-\rho_{t-a+s}\right)\sigma_{t-a+s}^2 \qquad (2.1) \end{aligned}$$

Then,

$$\left(\overline{D_{c,t}}\right)_t = \frac{1}{A}\sum_{a=0}^{A-1} D_{c,t} = \frac{1}{A}\sum_{a=0}^{A-1}\left(D_0 + a\sigma_\alpha^2 + \sum_{s=1}^{a}\left(1-\rho_{t-a+s}\right)\sigma_{t-a+s}^2\right)$$

Notice that the cohort born in the period t dies in the period $A-1$. The change in this term can be found as

$$\Delta\left(\overline{D_{c,t}}\right)_t = \frac{1}{A}\sum_{a=0}^{A-1}\sum_{s=1}^{a}\left[\left(1-\rho_{t-a+s}\right)\sigma_{t-a+s}^2 - \left(1-\rho_{t-1-a+s}\right)\sigma_{t-a+s}^2\right]$$

Suppose, for simplicity, there is a structural change in ρ_t and σ_t in period $\tilde{t}$, i.e. $(1-\rho_t)\,\sigma_t^2 = (1-\rho_0)\,\sigma_0^2$ if $t < \tilde{t}$ and $(1-\rho_t)\,\sigma_t^2 = (1-\rho_1)\,\sigma_1^2$ if $t \geq \tilde{t}$. Then the previous equation reduces to

$$\Delta\left(\overline{D_{c,t}}\right)_t = \frac{A-1}{A}\left[(1-\rho_1)\,\sigma_1^2 - (1-\rho_0)\,\sigma_0^2\right]$$

and one period later

$$\Delta\left(\overline{D_{c,t+1}}\right)_{t+1} = \frac{A-2}{A}\left[(1-\rho_1)\,\sigma_1^2 - (1-\rho_0)\,\sigma_0^2\right]$$

where the right hand side of the expression dies out linearly for further increases in the period. This occurs due to the fact that the changes in ρ_t and σ_t are most effective on inequality at time $\tilde{t}$, as can be inferred from the Figure 4 of chapter 1 that is also presented above (in a further attempt to make these derivations more apprehensible, in the Appendix I give a numerical example by calibrating A to 4). his means that the effect of the changes at time $\tilde{t}$ do not disappear in one period. These derivations imply that

$$\Delta\left(\overline{D_{c,t}}\right)_t \neq \left(\overline{\Delta D_{c,t}}\right)_t$$

as upon a structural change in ρ_t and σ_t in period $\tilde{t}$, the right hand side of the equation can be derived as

$$\begin{aligned}
\left(\overline{\Delta D_{c,t}}\right)_t &= \sigma_\alpha^2 + (1-\rho_0)\,\sigma_0^2 \quad \text{when } t < \tilde{t} \\
\left(\overline{\Delta D_{c,t}}\right)_t &= \sigma_\alpha^2 + (1-\rho_1)\,\sigma_1^2 \quad \text{when } t \geq \tilde{t}
\end{aligned}$$

This means that regression the change in average within-cohort inequality on the X's (equation 2) is not the same as running equation 3, which uses the average of differences in within-cohort inequalities on the X's.

The changes in either variance or correlation at certain point in time are called time effect[2]. This effect implies that the recent increase in inequality can also be explained by 'the rate of increase' of within-cohort inequality with age, and I just showed that using within-cohort inequality data can also help us to pin

[2] Even if this change will be there from time $\tilde{t}$ on, meaning it is not specific to only time $\tilde{t}$, it still affects only some time interval, which is: $t \geq \tilde{t}$. So it is still called time effect, This is at least how Guvenen (2009) interprets it.

down the true effect of structural changes in the economy that affect the income pattern of individuals and the income distribution of the society. Yet, using within-cohort dispersion of incomes is not a perfect measure of inequality. This is because people in the same cohort age simultaneously, meaning that they have the same level of experience throughout their life spans. Hence, inequality cannot be expected to increase due to changes in the return to experience. However, return to experience is part of the skill premium, and in the following section I investigate the effect of trade on inequality, and the trade effect on skill premium has already been shown to be significant by the previously described literature.

2.3 Why Trade?

I claim that aggregate inequality indexes give an enduring response to almost all types of structural changes in the economy so long as they affect the income pattern of individuals. Hence, I do not expect international trade to be the only variable whose effect on inequality can be justified through the use of within cohort inequality. The reason for me to concentrate on trade is that in the previous chapter I derived the changes in inequality and volatility in terms of variance and cross-sectional correlation of individuals' income shocks, and I also discussed that the decline in the correlation has been the driving element behind the changes in inequality and volatility across industrialized countries in the previous decades. In an attempt to find what type of structural change could cause this phenomenon, I find a set of evidence, which I am about to present, demonstrating that trade would result from these changes. This set of evidence should be taken seriously as they are consistent with the predictions of the economic theory that I discuss in the introduction of this chapter, but contrary to the results of regression estimates obtained through the sample of industrialized countries (IMF, 2007). It was this discrepancy which motivated me to concentrate my attention on trade.

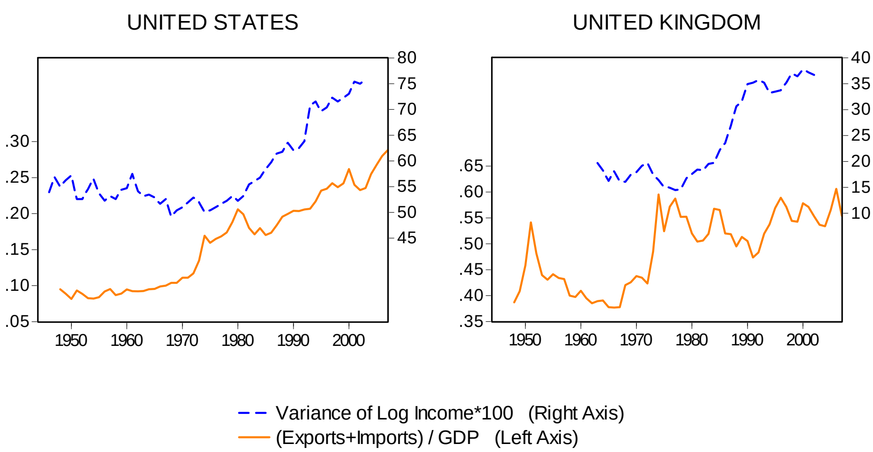
With regard to the decline in the correlation of income shocks that I find in the previous chapter, they are aggregate shocks affecting every individual in a parallel manner and as a result create correlation between them. Therefore if there has been a decline in the correlation of income shocks, either there has been a decline in the variance of aggregate shocks, an increase in the variance of idiosyncratic shocks, or a new shock has been introduced so that the weight of the aggregate shock in the total variance of the shocks has declined. Given that the changes in the data occurred in previous decades, a likely candidate seems to be the last explanation, where the new shock introduced is the foreign shock appearing through globalization. These shocks could possibly be the demand shocks that may affect exporters, or the supply shocks that may affect importers. In general, my prediction is that if it is either trade or financial integration, the way they should affect the inequality should be the same: they are both forms of opening up a country to foreign shocks. In this way the incomes of individuals who are (or industries of whom are) directly integrated with the rest of the world become less correlated with the rest of the individuals living in the same country, i.e., whose incomes are only affected from general equilibrium effects of integration. This intuition finds support from Figures 1, 2 and 3[3] which show that the changes in inequality in the sample of countries occur when these countries experience an increase in financial and trade integration with the rest of the world. What is seen in these figures is that as soon as an increase in any type globalization indicator is observed, an increase in inequality occurs throughout the countries, which I presume to happen through a decline in correlation. Italy and Denmark seem to be exceptions to this generalization; however a close look at their figures reveals that the predicted effect of globalization is the same. It causes a decline in correlation, which stops an initial decline in inequality in each country. From this point of view we can say that the inequality follows a U-shaped pattern across the sample of countries I present, and it is worthwhile to mention these countries are only chosen based on the availability of inequality data. The reason I concentrate on trade rather than financial liberalization is that the share of trade globalization in GDP is much greater than the share of globalization with respect to financial indicators across countries, making it easier to rationalize its effects on inequality. Besides, financial integration has already

[3] These are not the countries used in the sample of this chapter but of the previous one. Yet, they are chosen to demonstrate the relationship between inequality and trade in a larger sample of countries than the US and UK. These figures also use the data I constructed in the previous chapter.

found to have a negative effect on inequality through regression estimates as it is shown and summarized again by IMF 2007.

If we think that trade liberalization could be one of the X's mentioned in the previous section that lead to a long lasting increase in aggregate inequality, the incentive of this study can be seen from the following subset of Figure 1 that includes the countries I use in the sample of this chapter

Figure 4



where $\tilde{t} = 1971$ (or later) in the US and $\tilde{t} = 1971$ in the UK according to my predictions. That is to say that in the UK, trade increases suddenly up to some constant level, but it takes a decade for the aggregate inequality to respond this change. Hence, these graphs could explain the lack of strong evidence between trade and inequality found by regression estimates.

Once again, I do not discuss that only trade would create long lasting effect on inequality. I discuss and account for some other possible channels in my panel estimates, as well as that the increase in the volume of trade shows sudden jumps over time for many developed countries. This fact makes it harder to associate it to the increasing inequality though this issue seems to be less serious in the US data.

2.4 Framework for the Empirical Analysis

My estimations are based on the framework of the Chapter 4 of the IMF WEO (2007), which explains the change in the Gini coefficient across a sample of countries by using a set of variables including the trade share of GDP. Due to high quality the micro data limitations, I only concentrate on the US and UK. I first use the aggregate Gini index they use, replicate their estimations and obtain the results specific to these two countries. Then I carry out the same regression by replacing the dependent variable, the aggregate Gini index, with the one I constructed from the micro income data, and then compare the results of these two estimations. This exercise aims to justify the inequality data I constructed. In the third step I use the growth rate of the aggregate inequality instead of its level. Finally I use the growth rates of within-cohort Gini indexes in a panel using cohorts as id variables.

The basic IMF model relates the Gini index to several measures of globalization, using a set of control variables. They use the following equation as the basic specification for the analysis:

$$
\begin{aligned}
\ln(Gini) = \alpha_1 + \alpha_2 \ln(\frac{X+M}{Y}) + \alpha_3(100 - Tariff) + \alpha_4 \ln(\frac{A+L}{Y}) \\
+\alpha_5 Kaopen + \alpha_6 \ln(\frac{K_{ICT}}{K}) + \alpha_7 \ln(\frac{Credit}{Y}) + \alpha_8 Popshare \\
+\alpha_9 \ln(H) + \alpha_{10} \ln(\frac{E_{AGR}}{E}) + \alpha_{11} \ln(\frac{E_{IND}}{E}) + \varepsilon
\end{aligned}
$$

where X and M are non-oil exports and imports, Y is GDP, TARIFF is the average tariff rate, A and L are cross-border financial assets and liabilities (this variable is replaced with the FDI stock in their auxiliary regressions), KAOPEN is the capital account openness index, KICT is the ratio of information communication technology capital stock to the total capital stock, CREDIT is credit to the private sector by deposit money banks and other financial institutions, POPSHARE is the share of population ages 15 and older with secondary or higher education, H is average years of education in the population ages 15 and older, EAGR and EIND are employment in agriculture and industry, and E is total employment. IMF authors run a panel regression with fixed effects (capturing cross-country differences) and time dummies. The authors also use the logarithm of the Gini index by indicating that they pursue the aim of making this bounded variable behave more like a normally distributed variable than its level, hence making it more amenable to OLS estimation. They also run a robustness check by using a logistic transformation of the Gini index (making the variable completely unbounded). However, in spite of this attempt, they still do reveal the estimates obtained through the use of the log of the variable. What I infer from this practice is that a conversion of the data attempting to make the variable unbounded did not change their results significantly, as I may have expected it not to.

I chose the IMF report as a baseline model simply because they are interested in the effect of globalization on inequality as I am. Their interest is on the effect of trade and financial liberalization. I already explained the channels through which trade liberalization may affect the inequality in the advanced countries in the introduction of this chapter. For the literature on the effect of financial liberalization on inequality, World of Work Report 2008 gives a thorough summary. In short, financial integration, in theory, helps to reduce within country income inequality by lowering the borrowing constraints of the households with the least access to financial markets. But it can also lead to an increase in inequality either through increased demand for skilled labour, or by increasing the likelihood of financial crises, which may disproportionately hurt the poor. Empirical evidence tends to suggest that both lead to increase in within country income inequality in these countries. Regarding the effect of FDI, Stolper-Samuelson assume international immobility of factors of production. FDI weakens this assumption. Inward FDI tends to take place in more skill- and technology- intensive sectors in the advanced economies, outward FDI does the same for these economies, as they invest in their relatively less-skill intensive sectors. So both helps to incease relative demand for the skilled labor. Empirical evidence suggest that both inward and outward FDI contributed to increasing inequality.

The other variables used by the basic model of the IMF report are chosen aas control variables. A particularly interesting control variable is the share of information, technology and capital stock in the total stock of capital. It is so because this variable is indicator of technological development, and it is not easy to separate the effect of globalization and technology from one another as technological advances are shown to have helped deepen trade and financial linkages between countries, while globalization has helped spread the use of technology. A similar concern between globalization and control variables arises for the sectoral share of employment. This is because globalization can move labor away from the agricultural activities in the advanced countries. I will be more specific on the estimates of these variables when I compare my results with those of IMF in the later sections.

2.5 Data

2.5.1 The Macro Data

For both the US and UK, I collect data from the same sources as IMF (2007), and where necessary, replicate their modifications. The sample period that the IMF chapter uses is 1981-2003. One of their measures of trade globalization is a de facto measure and uses the sum of imports and exports of goods and services over GDP. This data and sector trade data on agriculture, manufacturing, and services are from the World Bank's World Development Indicators database. The other measure of trade globalization is a de jure measure and calculated as 100 minus the tariff rate, which is the mean of the average unweighted tariff rate and of the effective tariff rate (tariff revenue/import value). I collect the data for the average unweighted tariff rate from the World Bank's World Development Indicators database, and tariff revenue data from the OECD statistics database.

The de facto measure of financial globalization is the share of cross border assets and liabilities in GDP, and from the "External Wealth of Nations Mark II" created by Lane and Milesi-Ferretti (2006). The components of this share include (for both assets and liabilities) FDI, portfolio equity, debt, financial derivatives and total reserves minus gold (assets only). The de jure measure of financial globalization is the capital account openness index, and as does IMF (2007), I obtain this from Chinn and Ito (2006). IMF (2007) uses the capital stock series prepared by Fajnzylber and Lederman (1999), and ICT (Information Communication Technology) capital stock series prepared by Jorgensen and Vu (2005) to calculate the share of ICT in the total capital stock as a measure of technological development. However, this series is no longer available. Instead I use both the total and ICT capital stock from the joint work of Dale Jorgenson and Khuong Vu of Harvard University and the Total Economy Growth Accounting Database of the Groningen Growth and Development Centre. These series only cover the period after 1980. To measure a countries' financial depth, IMF uses the ratio of credit to the private sector by deposit money banks and other financial institutions to GDP. The source is the Financial Structure database prepared by Beck, Demirgüç-Kunt, and Levine (2000) and revised in March 2007. Data on educational attainment of the population aged 15 +, the average schooling years in the population, and the share of the population with secondary and/or higher education are from Barro-Lee´s (2000) dataset. The data is available for the years between 1980 and 2000 but only in 5 yearly intervals. For the years in between I interpolate them linearly for both countries, and for years 2000-06 I extrapolate them linearly. To replicate the IMF estimation, I use data on employment shares in agriculture and industry from the World Bank's World Development Indicators database. However this data is only available from 1980 onwards. To employ in my extended sample, I use data from OECD statistics database, going back to1965. To calculate labour productivity in agriculture and industry (with respect to total labour productivity), I use the employment share of these industries again both from World Bank and OECD, and use value-added share of these industries in GDP (in current US dollars), from the World Bank database.

2.5.2 The Micro data

Consistent with the sources of aggregate inequality indexes used by IMF (2007), the micro income data I use to construct within-cohort and aggregate inequality indexes is based on CPS (Current Population Survey) for the US, and on FES (Family Expenditure Survey) for the UK. The CPS data is extracted from IPUMS (Integrated Public Use Microdata Series), and the yearly FES data is downloaded from the ESDS (Economic and Social Data Service) of the UK Government distributed by the UK Data Archive, University of Essex, Colchester. I constructed the data for the years 1969-2009. Neither of these sources supplies panel data, Therefore they are not exactly the same households that are followed to construct the within-cohort inequality from one year to the other; rather I construct the synthetic within-cohort inequality data from these cross sectional data sets. If, as an example, within-inequality in 1969 is calculated for the households, heads of which are between 21-25 years old, the next year I will calculate for the households, heads of which are between 22-26 years old. However, these two groups of households do not have to be the same ones.

The micro income data for the US is the gross monetary income of the households, unadjusted for the household size. The micro income data of the UK is the disposable income of the households, adjusted for the household size by following the standards of HBAI (Households Below Average Income). This is produced by the UK Department for Work and Pensions. There are two types of HBAI scales that are used to weigh the household members in the adjustment of household size. I use the one that is also used by FES, and can be seen from Table 1 of the Appendix. I constructed the data for the years 1969-2001. The data for the UK also excludes Northern Ireland (hence it is Great Britain data); however the IMF (2007) also refers to Great Britain for extending their data set. Excluding Northern Ireland should not create big differences in estimation results; Frosztega et al. (2000) compare income distribution of Great Britain and the UK and show there is a negligible difference between them. They also show that including Northern Ireland has a negligible effect on the main estimates presented in HBAI. It is important to know that the FES changed its calendar year in 1994. Before this year, it collected data for the previous calendar year. Therefore, I used the data for 1993 to extract micro income data for this year. Following 1994, it started to collect the data at the same time March CPS did. The data covering the period from the 1994 April to 1995 March is called 1994-1995 data. I labeled such data from the CPS supplement as 1995 data, and I did the same for the FES. As a result, I have missing data for 1994. I constructed the inequality data for this year by averaging inequalities in 1993 and 1995 for each cohort and aggregate inequalities. Finally, UK data covers the period between 1969 and 2001, and US data between 1967-2009. However, to be able to follow the inequality of the same cohorts, while maintaining the consistency between estimates, I also use the US data starting from 1969.

The Gini coefficient is calculated according to the formula:

$$Gini = \frac{1}{n}(n+1-2(\frac{\sum_i (n+1-i)y_i}{\sum_i y_i}))$$

where the data is in a nondecreasing order: $y_i \leqslant y_{i+1}$.

2.5.3 Analysis of Constructed Inequality Data

2.5.3.1 Evolution of Aggregate Inequality in the US and the UK

Figure 5 of the Appendix compares the aggregate inequality data I constructed with that IMF (2007) uses from the World Income Inequality Database (WIID) for the US and UK. Regarding the US data, even though the trend and level of both data show great similarity, there is still a clear discrepancy between them arising due to the timing of changes in each data around the 1990s. This is interesting as I constructed this data from the IPUMS extraction of a CPS series. Furthermore, as the original inequality data source (US Census Bureau web) do not use any adjustment for size of the household, nor do I. However, since it is not the replication of the original data that I am interested in, but whether using within-cohort data improves estimates (at least in terms of standard errors) with respect to aggregate data. As long as I use aggregate and within-cohort inequalities formed from the same source, the primer discrepancy should not constitute a significant problem. On the other hand, for the UK, the WIID data and my construction show nearly a perfect match. There is a small difference between the level of the constructed and original series; however as this difference seem to stay constant over the years, the series are indifferent from the point of view of this study. Given the discrepancy in the US data, the good match of UK data is surprising. This is because to construct this data, I needed to download the survey data for each year and further combine different variables to construct a correction for household size. Moreover, WIID used two data sources to construct the data, whereas I use only one.

2.5.3.2 Evolution of Within-Cohort Inequalities in the US and the UK

Figures 6 and 7 show the evolution of within-cohort inequalities in the US and the UK respectively. I divide population in to 5-years cohorts, and from those I only use the ones aged between 16 and 65 (though for the sake of space I do not reveal the graphs belonging to the last two cohorts in Figures 6 and 7). In calculating inequalities I further eliminated the cohorts having sample size less than 200. The number of households in each cohort (except from the last one that is not included in the final estimations) and each year are in Tables 1 and 2.

Figures 6 and 7 reveal is clear pattern; within cohort inequalities increases by age (or by time). This has already been predicted in the previous sections. UK data is not as smooth as US data, but this is understandable given the limited number of cohort cells compared to US data used to calculate the inequalities for this country. This can be seen from Tables 1 and 2. The increase in inequalities seem to slow down when cohorts (or household heads) start to exceed their 55s, but this is understandable as these are yearly gains and there are well known intuitive explanations, such as retirement, for this decline[4].

Figures 6 and 7 are drawn for the full period of the data, so that we are able to observe the pattern of inequalities for the longest period of time possible. Moreover, I use this data to compare different measures of inequalities. However, to replicate the IMF estimations, which start with 1981, I construct a new full set of inequalities by using the set of cohorts, youngest of which are between 16-20 years old in 1981. Again for the sake of space I do not reveal similar figures and cell sizes for these cohorts. I do not use the cohorts that are used to construct

[4]In the US data, the within-cohort inequality also decreases in the initial periods of the youngest cohorts, and this pattern remains the same across observations repeated at different points in time. Although it is not in the interest of this paper, this observation is still interesting as it possibly point outs that around the time cohorts are expected to enter the labor force, the inequality among these people weakens. And this may well be the case if these people were previously living alone and funded by their families.

Figures 1 and 2 in the regression estimates, as such a practice would imply the use of cohorts, youngest of which is aged between 28 and 32 in 1981. Currently I am trying to compose larger dataset from macro data to use with the full period of within-cohort inequality data; however, I have not finished it yet.

2.6 Comparison of Different Measures of Inequality

I have already given a theoretical discussion on the difference between average growth in within-cohort inequality and growth in aggregate (average within cohort) inequality. I show this difference on the real data for the US and UK in Figure 7. The graph for each country uses 3 measures of inequality. One of them is the growth rate of aggregate inequality, where the latter is constructed in accordance with the WIID data. Another is the average growth rate of within-cohort inequalities. Since this data uses only the cohorts aged between 16-65, I also calculate aggregate inequality, the first measure, by using this sample of heads-of-household.

Graphs in Figure 7 point out two things. First, aggregate inequalities that are calculated by the use of full sample and the sample of heads-of-household aged between 20 and 65 are very close to one other. Hence, I do not lose information by using cohorts between these ages. Second, the lines do not show substantial difference between the average growth of within cohort inequality and growth of aggregate inequality. This, at first, seems to conflict my claim that aggregate data behaves different that the data averaging within-cohort inequalities, especially for the US. But these figures are drawn for the period 1969 onward. And Figure 4 already shows that the US inequality data keeps increasing for that period, and do not stabilize. Then it could be normal that the cohort and aggregate data in the above graphs show similar patterns for the US. For the UK on the other hand, Figure 4 shows that the inequality data stabilizes around 1990. And if we look carefully for the UK graph in Figure 7 , this is exactly the period where the cohort data deviates from the aggregate data.

There is also a final reminder that can be said on Figure 7, which can also be another explanation for the lack of difference between the measures of interest. The average of growth of within cohort inequality data in Figure 7 uses the cohorts that are constructed from 1969 onwards. The youngest cohort in 1969 is between 16-20 years old. And these people get older. Hence, the youngest cohort in 1970 is between 17-21. This means I do not include people that are 16 years old in 1970. This further means that at the time of change in aggregate inequality in the UK, which is 1990, the youngest people that I consider are 37 years old, and the average household age is around 50 (as I drop the ones above 65 years old). Hence, I can easily underestimate the effect of any structural change in the economies on the change in average growth of cohort inequalities occurring after 1970. To account for that, the graphs can be corrected by using, for example, a rolling window of cohorts. However, currently I prefer to keep this as a future work.

2.7 Panel Estimates

2.7.1 Summary of the IMF Results

The IMF World Economic Outlook (2007) results indicate that in the advanced economies technology has equally contributed to increasing inequality with globalization, whereas other control variables, which are indicator of financial development, education level, changes in the agricultural and industry employment share overall have mitigating effect on inequality. Decomposing the effect of globalization, the authors find the effect of trade on inequality as negative, i.e., it decreases the inequality. This effect is more prominent in the volume of exports than on the tariff liberalization. And the effect of financial liberalization is positive, i.e., it increases the inequality. This effect is stronger with the outward FDI than the inward FDI. These can be seen in Table 4.10, and Figures 4.9 and 4.10 of the publication. These estimates cover the period 1981-2003.

2.7.2 Re-Estimating the IMF Estimates

In the following pages I re-estimate the summary model of IMF Table 4.10. I start with replicating their estimation by using the US and UK aggregate inequality data; first separately, then both at a time through panel regressions. For the purpose of this replication I use the same inequality data source with IMF, which is WIID. The results are on the first columns of Tables 3, 4 and 5. Table 3 shows the results for the US, Table 4 for the UK, and Table 5 shows the result of regressions including both the US and the UK.

Even though I propose the growth rate of within-cohort inequality as the right measure to use, I do not change the independent variable from the level of the WIID based aggregate index data to this one instantly. Rather I do stepwise procedure and make a one change at a time. Hence, the second columns of Tables 3, 4 and 5 are devoted to the results of estimations using the growth rate of WIID Gini index data. The third columns use the growth rate of aggregate Gini index data I constructed. The fourth columns are the last ones and they use the growth rate of within-cohort inequality indexes, again constructed by me.

I already expect the second and third columns to improve the results significantly compared to the ones seen on the first columns. This is because I expect the main change in the growth rate of aggregate Gini to occur when there is a change in the determinants of inequality. For example what is seen on Figure 4 and for the UK is that this country experiences a substantial increase in the volume of trade in a short period of time, together with the change in the growth rate of inequality, happening around 1973. The change in inequality is reflected, at least according to my predictions, both in the level and in the growth rate of the data. However, the problem with the level of the data is that following 1973 it continues to increase, which I presume to occur due to the cohort structure of the population. On the other hand, the growth rate of the data can be found more or less constant during the decades following the change, so it can be associated to the trade volume better than the level of the variable does. Theoretical background for this phenomena can be obtained from Equations 5 and 6, where the aggregate Gini is approximated by the averages of within-cohort Gini indexes. On the other hand, there is also a problem with the growth rate of aggregate data, which is that it captures the deviations in the period that inequality stabilizes, around 1990, even though the volume of trade still stays constant at that time period. As a result I expect the last columns to improve the results with respect to the columns 2 and 3 on each table, as the growth rate of within cohort inequalities shall reflect the changes in the structure of the economy concurrently, which can be seen of Figure 4 of the previous chapter and Equation 7.

Unfortunately neither the IMF sample period (1981-2003), nor the ICT data they use to measure technological development goes before 1980s. Hence, I am not be able to catch the changes in data occuring around 1973. Extending the data set, by replacing the ICT data with the number of patents granted is something I am currently working on. However, looking at the graphs and focusing on only on some part of the data would already have been data mining. Thus for the current purposes I stick on the IMF sample.

In the estimation of first three columns of Tables 3,4 and 5, I use the sample size the IMF uses, which is 22 years; however, I am not able to use 10 explanatory variables at a time as they do. Since I do not use as many countries as they do and this would leave the regressions with very few degrees of freedom. I find another way of proceeding. IMF uses two control variables to measure the effect of some channels that may affect inequality other than trade. So I eliminated one out of two control variables and number of explanatory variables has redued to 8. In deciding which of two to eliminate, I chose the less significant variables in the seen on the IMF table. I eliminated the percentage of population with at least secondary education, but used average year of schooling, and also eliminated agricultural share of employment and used industry share (even though these are not complements). Finally, de jure measure of financial globalization the IMF uses is the capital account openness index. However, as this data stays constant for the countries I employ during the sample period, I avoid using it as well. Finally I use the same data conversion with IMF. Natural logarithm of all the dependent and independent variables except from 100 minus tariff rata and share of population with at least secondary education. I use the latter variable for the robustness check.

There is one final and important remark on the estimates. On each table I compare the panel estimates using cohorts as id variables, with time series regression estimates using the level of the inequality. Hence, even though it is standard to use year dummies in the panel estimations, as they do not appear in the time series regressions, I do not use them in the panel estimations as well. I discuss its implication on the panel estimates when when I include these dummies in robustness check.

2.7.3 Results

Table 3 shows the results for the US and indicates that when the level of Gni index that is taken from WIID is used, both trade volume and financial liberalization found to increase inequality. The coeeficient of trade changes when the growth rates of either WIID Gini or the Gini of my construction is used. Indeed the estimates found by WIID data and mine show a fairly good match. Finally when the growth rate of within-cohort inequalities are used, both of the trade and financial globalization are found to lead to an incrase in income inequality. So there is a discrepancy between the results obtained through the growth rate of aggregate inequality and within-cohort inequalities, though I am not be able to interpret this difference. The coefficients of the variables on the last column are higher with respect to previous two estimates, and unlike them they are highly significant now. Even though these improvements in the standard errors does not necessarily mean that growth rate is a better measure of inequality, as they may simply reflect the increase in the number of observations employed, they needed to be evaluated seriously given my derivations in the previous sections, and also predictions of the economic theory on the determinants of inequality that are also used in my estimates. The ICT capital share is another variable strongly associated to the increasing inequality, and financial development is found to be negatively related with that.

Results for the UK are on Table 4. The first column, using the level of aggregate inequality, finds mitigating effect of trade and worsening effect of financial liberalization on inequality. Just like the IMF paper does across sample of industrialzied countries. However, using the growth rate of this variables in the columns 2 and 3, especially using the growth rate of within-cohort indexes in the last column, caues the coefficients of these variables change their signs, together with the signs of most of the control variables. This is expected given the pattern of the UK data I explained in the previous section. Finally Table 5 shows the estimates using both countries at a time. As expected, results are close to being average of the previous two tables, and compared to the results of the first column, the effect of trade and financial liberalizations have reversed in the last one.

I use the last coloumn estimates of Table 5 as baseline for the robustness check. Hence, it is better to give its thorough summary. The coefficient of trade share in GDP is positive and significant (it has a aggravating effect on GDP). The effect of the other trade liberalization indicator, tariff rate, is the same, though insignificant. The coefficient of ratio of inward FDI stock to GDP is negative and insignificant, and again much smaller than that of the trade share. The coefficient of ratio of information, communication and technology capital in total capital stock is positive and highly significant, meaning the skill biased technological change explanation for the rise in earnings inequality finds strong support from my estimates. The coefficient of financial development, as it is measured by ratio of credit to pricate sector in GDP, is negative, but only significant in 10%. The other variable coefficient of which is found significant at this level is average years of education. Its sign is negative. The final variable is the industry share of employment. It has positive and insignificant sign, the only unexpected sign found.

2.7.3.1 Robustness Check

I check for the robustness of the results seen in the last column of Table 5, which I will call the 'Base' estimate from now on. In that estimation I use cohort and country dummies. Then I make first robustness check by running the same regression with the addition of year, then country-cohort dummies. Results are on

Table 6. Adding country-cohort dummies, seen on the last column, do not cause nearly any change neither of the coefficient estimates nor on the standard errors. However, addition of year dummies improves the results in terms of standard errors significantly, and most of the variables are significant now. Moreover, other than that of the tariff rate, other coefficients preserve ther signs. Mostly the same, these results still indicate that both trade liberalization caused to increase in inequality, and financial liberalization caused to decline in it, and still contrary to the IMF results.

The results of the second and last robustness check are seen on Table 7. The first column is again devoted to the Base estimate. In the second and third ones I change the indicator of financial liberalization from the share of Inward FDI stock in GDP, to the shares of Inward FDI Flow and Cross Border Assets and Liabilities in GDP. I do this practice as I suspect the mitigating effect of financial liberalization on inequality. The coefficients of these variables stay nearly the same across regressions; minus and insignificant. Moreover, changing the FDI with other liberalization indicators does not cause a noticeable change in the coefficients of other variables. Finally, the fourth and last column includes other control variables IMF uses including the ones not included in the Base estimate. The results of this regression are not much different for the variables that are also used by the Base estimate. For the newly added variables, results indicate that the share of the population with at least a secondary education has a negative and significant effect on inequality. However, the other measure of education, average years of schooling, is highly correlated with this variable and this may have affected the result for this variable. The other newly added variable is agriculture employment share, and we see that it has mitigating effect on inequality. This is unexpected, as was the effect of industry employment share.

Overall, robustness checks give considerable support to the conclusions that are drawn from the Base estimates. Trade has led to increase in the income inequalities in the US and UK, but given the trade effect, the effect of financial liberalization is negative and insignificant. When I do not include trade share in GDP but only a financial liberalization indicator, I find its coefficient insignificant but positive. This change possibly occurs due to the high correlation of the variables.

2.8 Concluding Remarks

This study begins with discussing the right measure of inequality to be used in the regression estimates; this part concludes that it is the average growth rate if within-cohort inequalities. The study revisits the relationship between inequality and trade by using US and UK data. Contrary to the negative and insignificant relation found between the variables obtained by regressions through the use of aggregate inequality data, the relationship between the variables are found to be positive and significant using the proposed measure of inequality, which is consistent with trade theory and a substantial part of the trade literature. The changes in between and within-cohort inequalities used to be explained by changes in the distribution of the individuals' characteristics, such as experience and education. To the best of my knowledge, this is the first study that relates them to variables such as trade.

Through the cohort estimates, the average number of years of education is found to lead to a decrease in inequality, whereas increases in technological advances, industry share of employment, financial development and tariff rates are found to increase inequality. The effect of financial liberalization is not robust to a change in the variable chosen to represent it. The share of inward FDI in GDP seems to increase inequality, but the respective share of the cross border assets and liabilities is found to have the opposite effect. Actually it is difficult to differentiate the effect of trade and financial liberalization through the regression estimates as they are highly correlated. However, my results remain valid in the set of robustness tests. Moreover, the share of trade globalization in GDP is much higher than the share of financial globalization indicators across countries, making it easier to rationalize its effects on inequality. In general, even though trade is found to have worsening effect on inequality, there are many advances occurring at the same time with the increase in trade globalization which may have caused the change in inequality, such as technology diffusion or the international movements of factors of production. Even other domestically oriented explanations for the changes in inequality may have started to play a role following an integration of a country with the rest of the world. Abstracting as much as possible from these discussions, my aim in this study is to bring to attention that changes in inequality coincide with large changes in trade and financial indicators. And the estimated changes in variance and correlation parameters from the US in the first chapter of this thesis are consistent with the implications of trade theory. Finally, the relationship between trade and inequality cannot be discerned based on simple regression estimates.

Throughout the paper it is argued that using within-cohort inequality data in the regressions can help us to overcome the problems that would stem from the time effect on aggregate inequality. This, on the other hand, may not be the only positive gain of using within cohort inequality data. Time effect indicates a change in the income pattern of all individuals at any point of time and onwards. By definition it accepts all cohorts are equally affected from such a change. As a result, using average growth rate of within-cohort inequality is the right measure to use. However, other than the time-effect, there is also the well known cohort effect, indicating the situation where the income structure of some cohorts changes with respect to that of the rest. This may be well the case if they are the young cohorts who are able to adapt to the changes in the structure of the economy.

And even if it is not in the interest of this paper, separate use of within-cohort inequalities in the regressions can also be used to test this prediction.

2.9 APPENDIX

A-Derivations of Average Growth in Within-Cohort Inequality and Growth in Average Cohort (Aggregate) Inequality

In this section I calibrate the number of cohorts, A, to 4 and derive the change in average growth in within-cohort inequality and growth in aggregate inequality upon on a change in in ρ_t and σ_t in period $\tilde{t}$, i.e. $(1-\rho_t)\,\sigma_t^2 = (1-\rho_0)\,\sigma_0^2$ if $t < \tilde{t}$ and $(1-\rho_t)\,\sigma_t^2 = (1-\rho_1)\,\sigma_1^2$ if $t \geq \tilde{t}$ where

$$y_{i,c,t} = \alpha_i + y_{i,c,t-1} + \varepsilon_{it}$$

and ε_{it} is i.i.d. mean zero over time and has variance σ_t^2 and correlation ρ_t across individuals.

For any cohort the equation (1), the

$$D_{c,t} = D_{c,t-1} + \sigma_\alpha + (1-\rho_t)\,\sigma_t^2$$

implies that

$$\Delta D_{c,\tilde{t}+T} = \sigma_\alpha^2 + (1-\rho_0)\,\sigma_0^2 \quad \text{when } T<0$$
$$\Delta D_{c,\tilde{t}+T} = \sigma_\alpha^2 + (1-\rho_1)\,\sigma_1^2 \quad \text{when } T\geqslant 0$$

so that average of growth in within-cohort inequalities also equal to

$$\overline{\Delta D_{c,\tilde{t}+T}} = \sigma_\alpha^2 + (1-\rho_0)\,\sigma_0^2 \quad \text{when } T<0$$
$$\overline{\Delta D_{c,\tilde{t}+T}} = \sigma_\alpha^2 + (1-\rho_1)\,\sigma_1^2 \quad \text{when } T\geqslant 0$$

For the growth in average cohort inequality, on the other hand, if $D_{\tilde{t}-4,\tilde{t}-1}$ represents the inequality within the oldest cohort at time $\tilde{t}-1$, and if all the inequalities within the cohorts living at time $\tilde{t}-1$ are: $D_{\tilde{t}-4,\tilde{t}-1}$, $D_{\tilde{t}-3,\tilde{t}-1}$, $D_{\tilde{t}-2,\tilde{t}-1}$, $D_{\tilde{t}-1,\tilde{t}-1}$, the equation (2)

$$D_{c,t} = D_0 + a\sigma_\alpha^2 + \sum_{s=1}^{a}\left(1-\rho_{t-a+s}\right)\sigma_{t-a+s}^2$$

implies that

$$\begin{aligned}
D_{\tilde{t}-4,\tilde{t}-1} &= D_0 + 3\sigma_\alpha^2 + 3\,(1-\rho_0)\,\sigma_0^2 & D_{\tilde{t}-3,\tilde{t}} &= D_0 + 3\sigma_\alpha^2 + 2\,(1-\rho_0)\,\sigma_0^2 + (1-\rho_1)\,\sigma_1^2\\
D_{\tilde{t}-3,\tilde{t}-1} &= D_0 + 2\sigma_\alpha^2 + 2\,(1-\rho_0)\,\sigma_0^2 & D_{\tilde{t}-2,\tilde{t}} &= D_0 + 2\sigma_\alpha^2 + (1-\rho_0)\,\sigma_0^2 + (1-\rho_1)\,\sigma_1^2\\
D_{\tilde{t}-2,\tilde{t}-1} &= D_0 + \sigma_\alpha^2 + (1-\rho_0)\,\sigma_0^2 & D_{\tilde{t}-1,\tilde{t}} &= D_0 + \sigma_\alpha^2 + (1-\rho_1)\,\sigma_1^2\\
D_{\tilde{t}-1,\tilde{t}-1} &= D_0 & D_{\tilde{t},\tilde{t}} &= D_0
\end{aligned}$$

and

$$\begin{aligned}
D_{\tilde{t}-2,\tilde{t}+1} &= D_0 + 3\sigma_\alpha^2 + (1-\rho_0)\,\sigma_0^2 + 2\,(1-\rho_1)\,\sigma_1^2 & D_{\tilde{t}-1,\tilde{t}+2} &= D_0 + 3\sigma_\alpha^2 + 3\,(1-\rho_1)\,\sigma_1^2\\
D_{\tilde{t}-1,\tilde{t}+1} &= D_0 + 2\sigma_\alpha^2 + 2\,(1-\rho_1)\,\sigma_1^2 & D_{\tilde{t},\tilde{t}+2} &= D_0 + 2\sigma_\alpha^2 + 2\,(1-\rho_1)\,\sigma_1^2\\
D_{\tilde{t},\tilde{t}+1} &= D_0 + \sigma_\alpha^2 + (1-\rho_1)\,\sigma_1^2 & D_{\tilde{t}+1,\tilde{t}+2} &= D_0 + \sigma_\alpha^2 + (1-\rho_1)\,\sigma_1^2\\
D_{\tilde{t}+1,\tilde{t}+1} &= D_0 & D_{\tilde{t}+2,\tilde{t}+2} &= D_0
\end{aligned}$$

then the average within-cohort inequalities are:

$$\left(\overline{D_{c,\tilde{t}-1}}\right)_{\tilde{t}-1} = [4D_0 + 6\sigma_\alpha^2 + 6\,(1-\rho_0)\,\sigma_0^2]/4$$
$$\left(\overline{D_{c,\tilde{t}}}\right)_{\tilde{t}} = \qquad [4D_0 + 6\sigma_\alpha^2 + 3\,(1-\rho_0)\,\sigma_0^2 + 3\,(1-\rho_1)\,\sigma_1^2]/4$$
$$\left(\overline{D_{c,\tilde{t}+1}}\right)_{\tilde{t}+1} = [4D_0 + 6\sigma_\alpha^2 + (1-\rho_0)\,\sigma_0^2 + 5\,(1-\rho_1)\,\sigma_1^2]/4$$
$$\left(\overline{D_{c,\tilde{t}+2}}\right)_{\tilde{t}+2} = [4D_0 + 6\sigma_\alpha^2 + 6\,(1-\rho_1)\,\sigma_1^2]/4$$

(*the coefficients above go symmetric once A is chosen to be an odd number)

Hence, we see that if a change occurs in ρ_t and σ_t in period $\tilde{t}$, it takes 3 years for the average within-cohort inequality to reach the new steady state, as it is claimed in the text. The year to year changes in these averages can also be found as:

$$\left(\overline{D_{c,\tilde{t}}}\right)_{\tilde{t}} - \left(\overline{D_{c,\tilde{t}-1}}\right)_{\tilde{t}-1} \qquad = 3[(1-\rho_1)\,\sigma_1^2 - (1-\rho_0)\,\sigma_0^2]/4 > 0$$
$$\left(\overline{D_{c,\tilde{t}+1}}\right)_{\tilde{t}+1} - \left(\overline{D_{c,\tilde{t}}}\right)_{\tilde{t}} \qquad = 2[(1-\rho_1)\,\sigma_1^2 - (1-\rho_0)\,\sigma_0^2]/4 > 0$$
$$\left(\overline{D_{c,\tilde{t}+2}}\right)_{\tilde{t}+2} - \left(\overline{D_{c,\tilde{t}+1}}\right)_{\tilde{t}+1} = [(1-\rho_1)\,\sigma_1^2 - (1-\rho_0)\,\sigma_0^2]/4 > 0$$
$$\left(\overline{D_{c,\tilde{t}+T}}\right)_{\tilde{t}+T} - \left(\overline{D_{c,\tilde{t}+T-1}}\right)_{\tilde{t}+T-1} = 0 \quad \text{when } T > 3 \text{ and } T < -1$$

Tables & Figures

Table 1: US Cohorts, Cell Sizes by Cohort and Year

Average age of reference person in 1969

Year	Total	16-20	21-25	26-30	31-35	36-40	41-45	46-50	51-55	56-60
1969	47510	541	3289	4333	4213	4540	4787	4836	4511	4151
1970	45444	918	3699	4212	4114	4308	4490	4571	4258	3900
1971	46438	1545	4300	4214	4166	4317	4552	4638	4341	3870
1972	45412	2345	4524	4155	3808	4049	4372	4480	4135	3747
1973	44899	3161	4645	4246	3697	4132	4197	4306	3793	3550
1974	44852	3790	4715	4142	3657	3920	4186	4109	3657	3472
1975	44165	4047	4713	4096	3648	3785	4068	4023	3598	3283
1976	46368	4711	5027	4313	3874	3941	4230	4068	3626	3326
1977	55540	6259	6060	5138	4594	4550	4772	4672	4298	3833
1978	54762	6281	5912	5005	4398	4446	4533	4614	4255	3738
1979	54891	6479	5775	4988	4232	4337	4501	4501	4138	3612
1980	65238	7748	6963	5883	4959	5090	5230	5096	4657	4008
1981	65731	7789	7048	5692	5032	5105	5276	5057	4502	3896
1982	59276	6922	6392	5116	4491	4496	4669	4583	3936	3498
1983	59211	6904	6275	5131	4448	4448	4547	4427	3919	3418
1984	59171	6977	6089	5020	4379	4418	4495	4330	3868	3151
1985	59799	7173	6183	5015	4347	4473	4366	4280	3828	3113
1986	58935	6883	6075	4818	4176	4169	4242	4147	3683	2974
1987	58279	6689	5995	4742	4054	4007	4180	3978	3465	2717
1988	58975	6670	6020	4736	4062	4080	4155	4030	3421	2637
1989	55335	6189	5429	4304	3639	3852	3851	3703	3184	2398
1990	59941	6823	5899	4696	3960	4053	4161	3925	3140	2329
1991	59929	6767	5874	4636	3953	3962	4112	3804	2993	2125
1992	59219	6646	5702	4522	3802	3854	3920	3683	2776	2001
1993	58970	6530	5570	4511	3731	3747	3740	3541	2698	1843
1994	57079	6223	5294	4166	3606	3598	3476	3281	2520	1578
1995	56941	6091	5131	4221	3496	3546	3386	3076	2325	1411
1996	49682	5307	4414	3523	3015	2994	2950	2614	1864	1024
1997	50311	5140	4493	3475	3095	2993	2895	2516	1735	910
1998	50353	5201	4477	3453	3082	2910	2726	2326	1559	740
1999	50785	5277	4406	3374	2944	2932	2675	2191	1408	859
2000	51016	5257	4323	3357	2929	2887	2577	2042	1268	708
2001	49633	4976	4139	3206	2847	2639	2491	1824	1152	538
2002	78265	7720	5895	4434	3937	3403	3104	3917		
2003	78310	7479	5825	4469	3722	3271	2911	3563		

Table 2: UKS Cohorts, Cell Sizes by Cohort and Year

Average age of reference person in 1969

Year	Total	16-20	21-25	26-30	31-35	36-40	41-45	46-50	51-55	56-60
1969	7008	35	374	551	634	644	661	703	641	682
1970	6391	77	478	542	534	605	590	600	524	630
1971	7239	173	596	612	646	607	660	685	608	725
1972	7017	227	644	659	606	639	620	693	613	623
1973	7124	319	684	582	597	551	616	714	595	664
1974	6695	366	647	609	606	528	593	591	587	612
1975	7203	502	727	663	614	577	604	640	597	702
1976	7203	580	711	622	573	585	605	655	600	668
1977	7196	628	776	617	604	569	585	634	617	660
1978	7001	651	731	608	570	533	647	621	572	648
1979	6776	700	706	587	543	510	531	601	498	632
1980	6944	631	772	616	558	533	594	580	580	608
1981	7524	722	834	681	601	590	596	681	610	618
1982	7423	687	847	635	571	590	610	635	576	590
1983	6973	719	732	605	542	529	560	611	559	537
1984	7081	707	688	623	527	540	629	656	567	486
1985	7012	711	737	570	531	554	564	621	523	549
1986	7178	700	712	585	549	517	532	657	540	449
1987	7396	722	745	565	536	525	594	688	535	412
1988	7265	671	779	572	531	537	568	613	471	467
1989	7410	673	729	546	546	571	593	622	501	390
1990	7046	643	670	498	508	518	569	581	436	358
1991	7056	655	621	526	530	529	527	561	447	344
1992	7418	683	718	561	540	513	627	547	425	284
1993	6979	624	665	512	513	486	530	527	365	246
1994	0	0	0	0	0	0	0	0	0	0
1995	6853	604	588	479	468	486	528	458	314	201
1996	6797	620	628	447	509	479	482	438	317	161
1997	6415	590	555	428	388	441	463	419	222	123
1998	6409	552	591	473	444	449	394	376	224	106
1999	6630	585	603	485	464	471	437	359	179	92
2000	7097	638	596	513	501	472	468	391	164	74
2001	6637	646	577	458	500	475	426	279	162	59

Table 3: Results for the US

	WIID	WIID Growth	Author Growth	Cohort Growth
	(1)	(2)	(3)	(4)
Ratio of exports and imports to GDP	0.1	-.11	-.07	0.23
	(0.06)*	(0.07)	(0.11)	(0.05)***
Ratio of inward FDI stock to GDP	0.02	0.03	0.02	0.04
	(0.02)	(0.02)	(0.02)	(0.02)**
100 minus tariff rate	-.007	-.007	0.01	0.008
	(0.01)	(0.01)	(0.02)	(0.007)
Share of ICT in total capital stock	0.03	0.02	-.02	0.18
	(0.05)	(0.04)	(0.08)	(0.04)***
Credit to private sector (percent of GDP)	-.05	-.10	-.09	-.53
	(0.12)	(0.12)	(0.15)	(0.11)***
Average years of education	0.17	0.96	0.51	-.70
	(0.65)	(0.82)	(1.01)	(0.46)
Industry employment share	-.23	0.06	-.08	0.09
	(0.08)***	(0.07)	(0.14)	(0.06)
Obs.	23	23	23	192
R^2	0.96	0.2	0.16	0.31
F statistic	297.6	1.64	0.49	4.7

*Independent Variables: (1) WIID Gini index, (2) Its Growth Rate, (3) Growth rate of Author's Gini (4) Growth rate of Within-Cohort Gini Indexes

*Sample: 1981-2003

*Panel estimates use cohort dummies

*Standard errors are robust

Table 4: Results for the UK

	WIID (1)	WIID Growth (2)	Author Growth (3)	Cohort Growth (4)
Ratio of exports and imports to GDP	-.17	0.15	0.14	0.2
	(0.08)**	(0.08)**	(0.15)	(0.15)
Ratio of inward FDI stock to GDP	0.12	0.06	-.07	-.13
	(0.05)**	(0.06)	(0.1)	(0.1)
100 minus tariff rate	0.0009	0.006	0.001	-.01
	(0.008)	(0.009)	(0.02)	(0.01)
Share of ICT in total capital stock	-.11	0.04	0.11	0.21
	(0.05)**	(0.06)	(0.11)	(0.1)**
Credit to private sector (percent of GDP)	0.13	0.05	0.07	0.13
	(0.02)***	(0.03)	(0.07)	(0.06)**
Average years of education	1.88	-1.43	-1.41	-2.49
	(0.47)***	(0.7)**	(1.36)	(1.11)**
Industry employment share	0.15	0.17	0.2	0.31
	(0.09)	(0.12)	(0.23)	(0.27)
Obs.	22	22	21	169
R^2	0.99	0.53	0.26	0.07
F statistic	212.14	1.86	1.64	0.72

*Independent Variables: (1) WIID Gini index, (2) Its Growth Rate, (3) Growth rate of Author's Gini (4) Growth rate of Within-Cohort Gini Indexes

*Sample: 1981-2003

*Panel estimates use cohort dummies

*Standard errors are robust

Table 5: Results for the US and the UK

	WIID	WIID Growth	Author Growth	Cohort Growth
	(1)	(2)	(3)	(4)
Ratio of exports and imports to GDP	-.10	-.009	0.05	0.17
	(0.05)**	(0.04)	(0.07)	(0.08)**
Ratio of inward FDI stock to GDP	0.04	-.004	-.01	-.03
	(0.02)**	(0.01)	(0.02)	(0.02)*
100 minus tariff rate	-.004	0.004	0.008	-.002
	(0.006)	(0.006)	(0.01)	(0.009)
Share of ICT in total capital stock	-.06	0.01	-.0007	0.03
	(0.02)***	(0.03)	(0.05)	(0.03)
Credit to private sector (percent of GDP)	0.14	0.02	0.008	0.03
	(0.02)***	(0.02)	(0.04)	(0.03)
Average years of education	1.11	-.55	-.37	-.65
	(0.32)***	(0.36)	(0.61)	(0.39)*
Industry employment share	-.13	0.03	-.01	0.09
	(0.08)*	(0.09)	(0.13)	(0.09)
Obs.	45	45	44	361
R^2	0.99	0.27	0.17	0.05
F statistic	1175.07	1.38	0.82	0.74

*Independent Variables: (1) WIID Gini index, (2) Its Growth Rate, (3) Growth rate of Author's Gini (4) Growth rate of Within-Cohort Gini Indexes

*Sample: 1981-2003

*Panel estimates use cohort and country dummies

*Standard errors are robust

Table 6: Robustness Check (1), Changes in the Dummies

	Base (1)	A (2)	B (3)
Ratio of exports and imports to GDP	0.17	0.37	0.17
	(0.08)**	(0.17)**	(0.08)**
Ratio of inward FDI stock to GDP	-.03	-.04	-.03
	(0.02)*	(0.07)	(0.02)*
100 minus tariff rate	-.002	0.02	-.002
	(0.009)	(0.01)**	(0.009)
Share of ICT in total capital stock	0.03	0.45	0.03
	(0.03)	(0.13)***	(0.03)
Credit to private sector (percent of GDP)	0.03	0.14	0.04
	(0.03)	(0.07)**	(0.03)
Average years of education	-.65	-2.74	-.67
	(0.39)*	(1.29)**	(0.4)*
Industry employment share	0.09	0.3	0.09
	(0.09)	(0.19)	(0.09)
Obs.	361	361	361
R^2	0.05	0.16	0.05
F statistic	0.74	2.26	0.82

*Independent Variable: The Growth Rate of Within-Cohort Inequality

*Coloumn 1: Country and Cohort Dummies (Same with previous estimates)

*Coloumn 2: Country, Cohort and Year Dummies

*Coloumn 3: Country, Cohort and Country*Cohort Dummies

Table 7: Robustness Check (2), Changes in the Control Variables

	Base (1)	FDIflow (2)	CBAL (3)	Full (4)
Ratio of exports and imports to GDP	0.17	0.09	0.11	0.19
	(0.08)**	(0.05)*	(0.06)*	(0.09)**
Ratio of inward FDI stock to GDP	-.03			-.05
	(0.02)*			(0.03)
Ratio of inward FDI flow to GDP		-.003		
		(0.006)		
Share of Cross Border Assets and Liabilities in GDP			-.02	
			(0.04)	
100 minus tariff rate	-.002	-.000	0.00	-.002
	(0.009)	(0.008)	(0.008)	(0.01)
Share of ICT in total capital stock	0.03	0.01	0.01	0.08
	(0.03)	(0.03)	(0.04)	(0.07)
Credit to private sector (percent of GDP)	0.03	0.01	0.01	0.05
	(0.03)	(0.03)	(0.02)	(0.04)
Population share with at least a secondary education				-.000
				(0.003)
Average years of education	-.65	-.24	-.27	-.90
	(0.39)*	(0.32)	(0.39)	(0.68)
Agriculture employment share				0.06
				(0.06)
Industry employment share	0.09	0.16	0.12	0.11
	(0.09)	(0.11)	(0.1)	(0.13)
Obs.	361	352	361	361
R^2	0.05	0.04	0.04	0.05
F statistic	0.74	0.65	0.88	1.1

*Independent Variable: The Growth Rate of Within-Cohort Inequality

Figure 1 : Inequality Index and Share of Trade in GDP

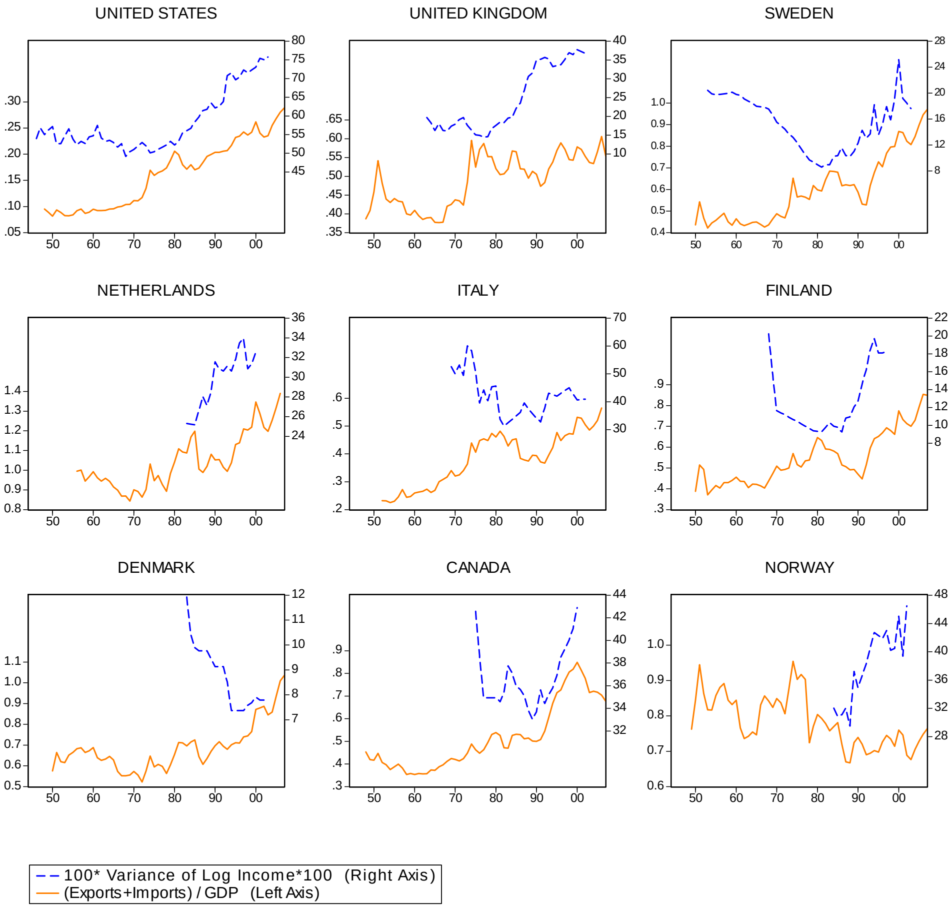


Figure 2 : Inequality Index and Share of Stock of FDI Intake in GDP

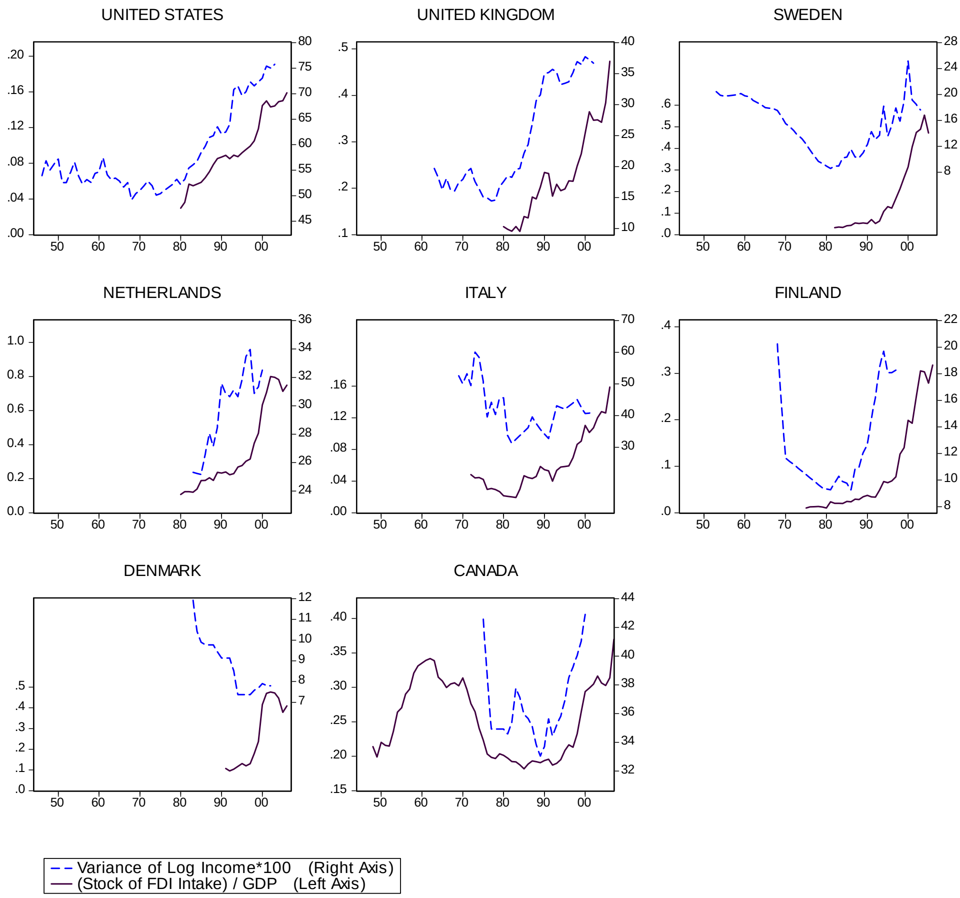


Figure 3: Inequality Index and Share of Cross Border Assets and Liabilities in GDP

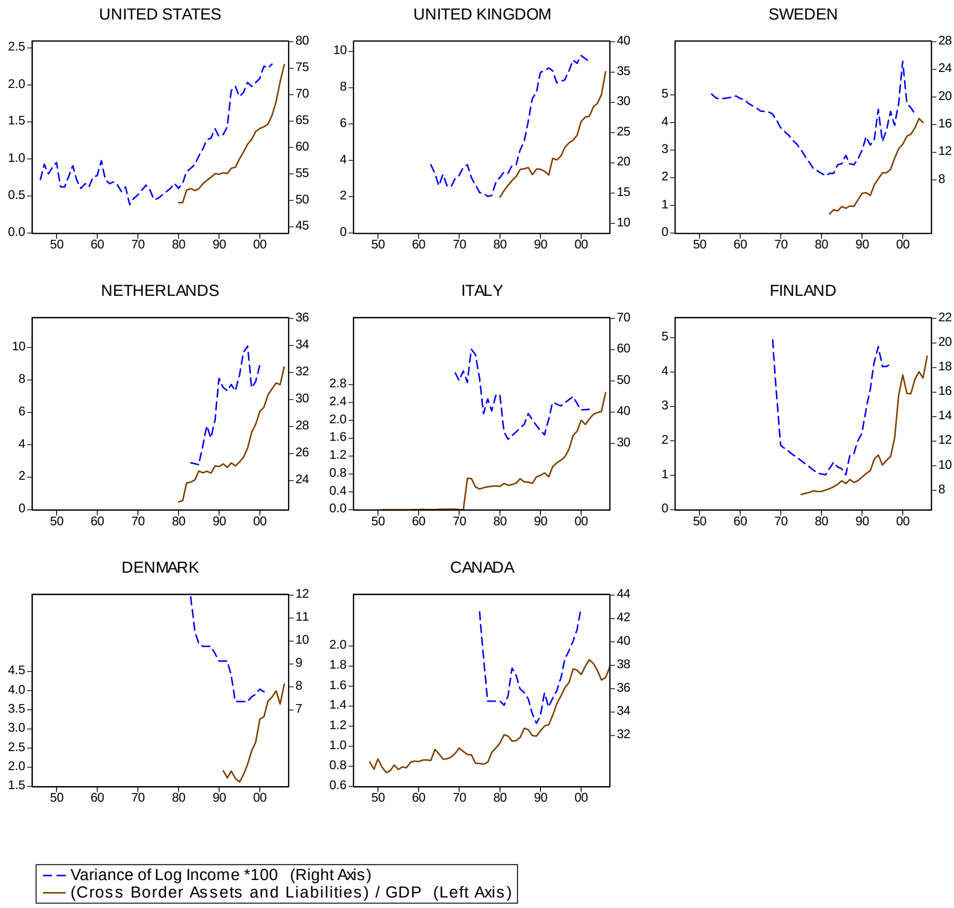
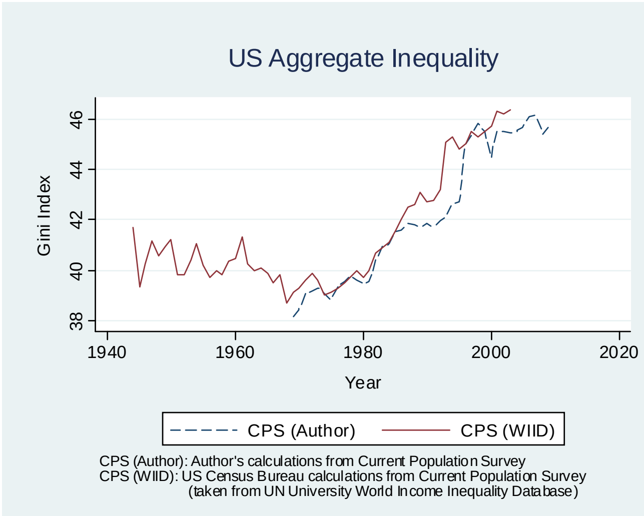
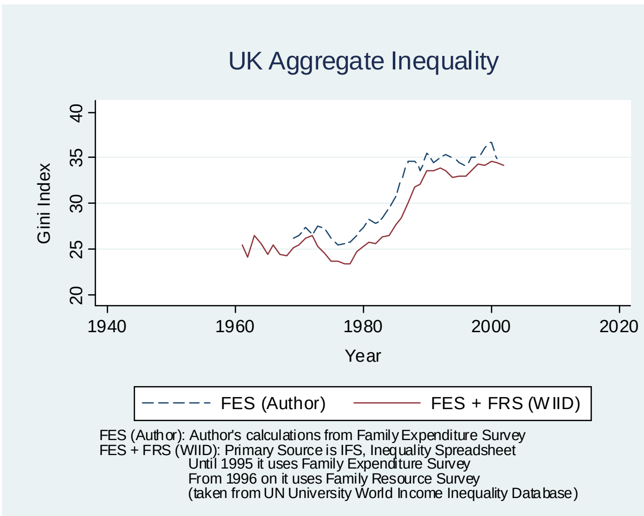


Figure 5



US Aggregate Inequality
Gini Index
38
40
42
44
46
1940
1960
1980
2000
2020
Year
CPS (Author)
CPS (WIID)
CPS (Author): Author's calculations from Current Population Survey
CPS (WIID): US Census Bureau calculations from Current Population Survey
(taken from UN University World Income Inequality Database)



UK Aggregate Inequality
Gini Index
20
25
30
35
40
1940
1960
1980
2000
2020
Year
FES (Author)
FES + FRS (WIID)
FES (Author): Author's calculations from Family Expenditure Survey
FES + FRS (WIID): Primary Source is IFS, Inequality Spreadsheet
Until 1995 it uses Family Expenditure Survey
From 1996 on it uses Family Resource Survey
(taken from UN University World Income Inequality Database)

Figure 6: Within Cohort Inequalities in the US (from 1969 on)

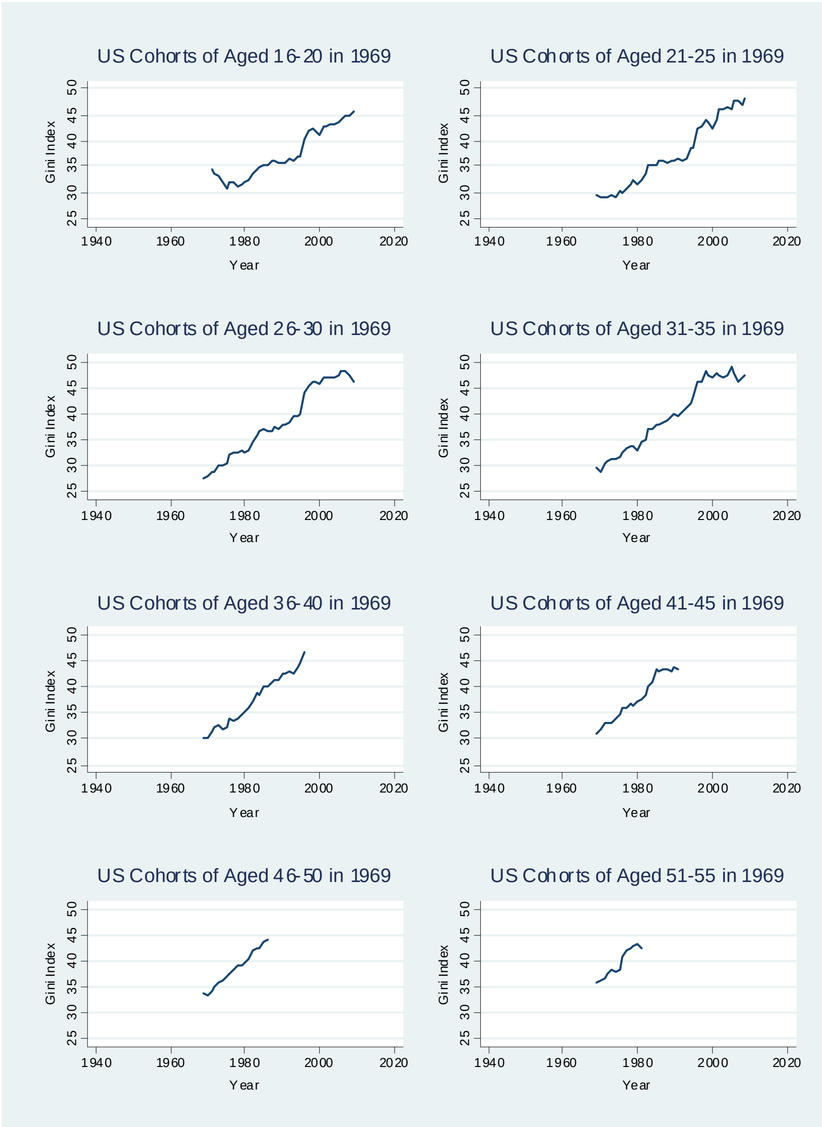


Figure 7: Within Cohort Inequalities in the UK (from 1969 on)

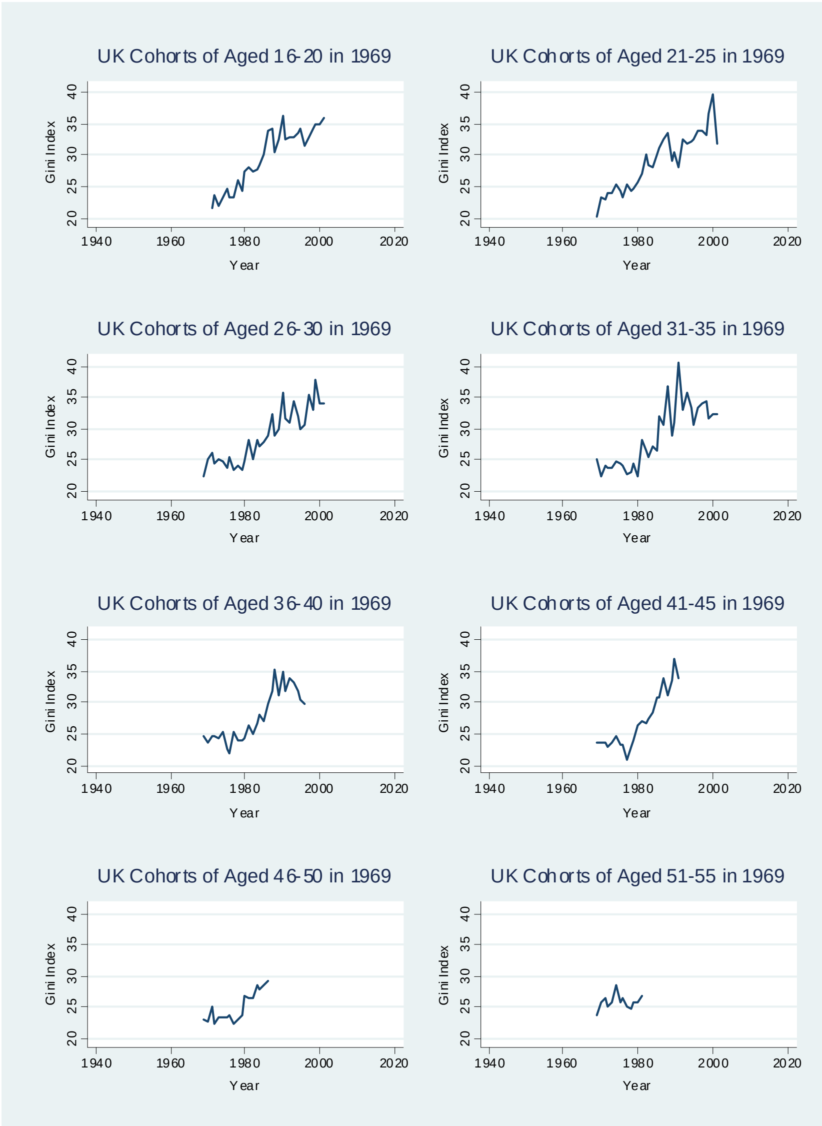
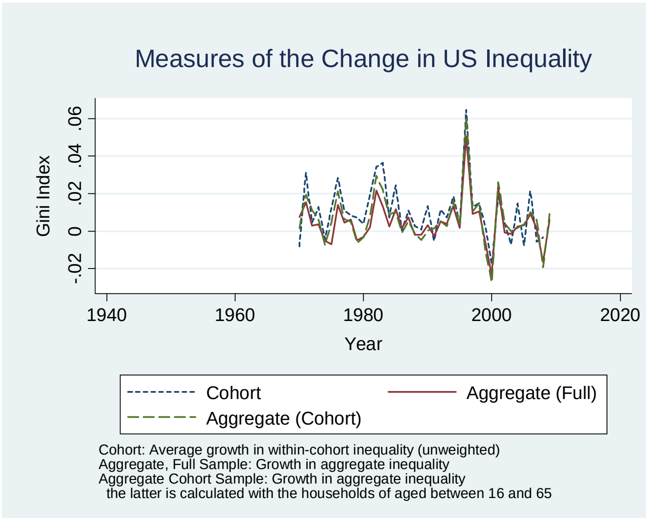
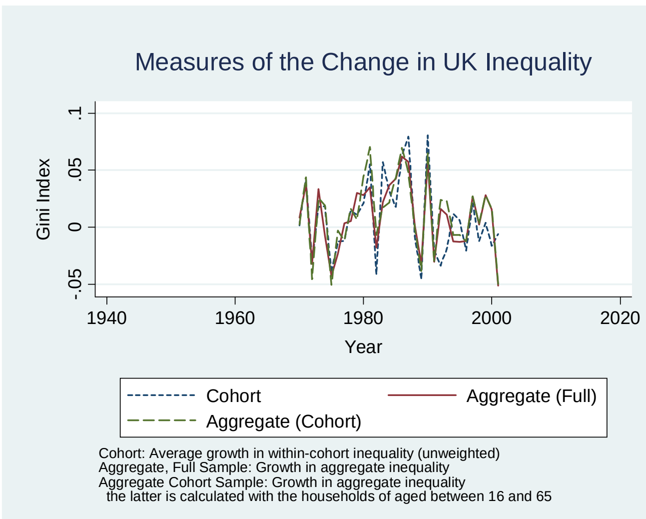


Figure 8: Average growth in within-cohort inequality vs growth in aggregate (average within cohort) inequality





III. Impulse Response Matching and GMM Estimation in Weakly Identified Models

3.1 Introduction:

Identification of structural parameters of DSGE models has been the subject of much literature. Recently, Canova and Sala (2006) documented evidence on the problems encountered in the area. To this end, they use an objective function that measures the distance between model impulse responses, generated with some parameter values, and estimated impulse responses, generated with parameters assigned from the accepted empirical range. Estimation rests on choosing the set of parameters that creates responses closest to model impulse response. The bias in the parameter estimates are used to derive results on identifiability. Here the general process is called IRM estimation and initiated by Rotemberg and Woodford (1997).

The main interest of this paper is not to derive conclusions on identifiability, but rather to measure the efficiency of IRM estimation. More specifically, whether it can yield better results than GMM estimation in a DSGE model with identification problems. I test this with a Monte Carlo study. I choose GMM for comparison with IRM as it is a widely used estimator on DSGE models. Not only the structural parameters of the model, but also the reduced form estimates are of my interest. This further points out the crucial difference between the estimators. IRM estimation uses all the system of equations simultaneously, and directly estimates the structural parameters of the model. In contrast, GMM uses one equation at a time and estimates reduced form coefficients. Thus, if the structural parameters are of interest, GMM requires their mapping from the reduced form estimates. After obtaining the results of this comparison, I make comparative statistics by changing the level of persistency of the structural error terms of the model, which also differentiates the paper from the sizable literature that uses GMM.

The DSGE model I choose is the 3 equation New Keynesian model. This model includes the Euler condition, Philips curve and monetary policy equations. I use both backward and forward looking terms in the first two equations (following Gali and Gertler 1999), and also autocorrelated error terms. So within staying in a closed economy context, this employed model includes all main specifications in the literature. This means it also inhabits all the identification problems in the area.

Since I am interested in comparison of estimators but not in identifiability, I have to follow a different approach than the one in Canova and Sala (2006). Instead of using the model impulse responses, generated with true parameter values, I first simulate data from the model, then run VAR on this data and extract structural shock responses from the reduced form shock responses of VAR.

Then like they do I try to match these responses with the ones I generate with parameters chosen from the accepted empirical range. As a result, compared to their study, my estimation suffers from two additional problems in identification: one arises from using a limited sample size, and the other arises from the identification of structural shocks from the VAR residuals. Therefore, my results have also identification implications and are more informative to be used on real data.

Before applying IRM estimation, I also choose the best identification scheme to apply structural VAR on the simulated data. For that, I choose to consult to the model impulse responses, and compare them with the simulated data impulse responses that are acquired with several identification schemes. From this comparison I obtain the first result of the experiment, which generalizes the finding of Chari, Kehoe and McGrattan (2005) and yields different results than theirs. Authors apply the same method on an another model, but use population responses instead of small sample ones. They conclude that, because of misspecifications, there can be quite significant differences between model and population responses. From this perspective they also support Sims's (1989) approach, which says that the right method could be to compare the impulse responses generated from the actual and simulated data so that both could suffer from the same misspecifications. My results show that all these results actually depend on the model and the related identification schemes. Chari et al. (2005) use long run and sign restrictions, and once again on a different model to mine.

I also show that with sign restrictions there is also a significant bias between simulated data and model impulse responses in my model. However, I further demonstrate that there is a high variance in the estimated responses with sign identification. This further implies that using wrong identification on both actual and simulated data does not necessarily create symmetric misspecifications as Sims (1989) suggested. In my paper I recommend the Choleski decomposition and demonstrate that the difference between the model and the simulated data impulse responses is quite small even with the small sample size I do employ, and this difference is shrinking further with a corresponding increase in the sample size.

I have also yielded results with regard to application of GMM estimation. I find that the most effective way of applying this reduced form coefficient estimator is to apply the theoretical restrictions on these coefficients that come via structural parameters of the model.

Finally I compare the results of the IRM and the GMM estimations and find that the presence of forward looking terms causes significant bias in estimations (especially true for the GMM estimation results). However, I also find that the key determinant that dominates comparative efficiency of estimations is the persistency of structural shocks. In the absence of an autocorrelation, GMM results in more precise estimates. However, this result is reversed for higher degrees of autocorrelation. In this case, GMM starts to suffer from a weak instruments problem, as is suggested by Stock, Wright and Yogo (2002), and Nason and Smith (2005), whereas the IRM benefits from the longer response to shocks. This is because longer lasting responses of shocks increase sensitivity in the matching process. Therefore, IRM estimation uses this undesired property to its advantage.

3.2 Model

It is a small scale hybrid New Keynesian model that includes Monetary Policy, Philipps Curve and Euler condition equations

$$i_t = \lambda_r i_{t-1} + (1-\lambda_r)(\lambda_\pi \pi_{t-1} + \lambda_y y_{t-1}) + e_{1t} \tag{1}$$

$$\pi_t = \frac{\omega}{1+\omega\beta}\pi_{t-1} + \frac{\beta}{1+\omega\beta}E_t\pi_{t+1} + \frac{(\phi+\theta_n)(1-\zeta\beta)(1-\zeta)}{(1+\omega\beta)\zeta}y_t + v_{1t} \tag{2}$$

$$y_t = \frac{h}{1+h}y_{t-1} + \frac{1}{1+h}E_t y_{t+1} - \frac{1}{\phi}(i_t - E_t\pi_{t+1}) + v_{2t} \tag{3}$$

where $v_{1t} = \rho_1 v_{1t-1} + e_{2t}$ $v_{2t} = \rho_2 v_{2t-1} + e_{3t}$ and e_{1t}, e_{2t}, e_{3t} are i.i.d., h is habit persistence parameter in consumption, ϕ and θ_n are relative risk aversion coefficients for consumption and labor respectively, ω is the degree of price indexation (fraction of backward looking firms), ζ is the degree of Calvo type price stickiness and λ's are policy parameters.

I simulate the data of output gap, inflation and nominal interest rate from this model at lengths 80, 160 and 320. Consequently, my estimations have small sample implications. In the simulations I use parameter estimates of Rabanal and Rubio-Ramirez (2005) from the US data as do Canova and Sala (2006). These parameters are

h	ϕ	ω	β	ζ	λ_r	λ_π	λ_y	ρ_1	ρ_2	θ_n
0.85	2.0	0.25	0.985	0.68	0.2	1.55	1.1	0.65	0.65	3.0

In all the parameter estimations written below, I assume true model is known, but not these parameters.

3.3 Impulse Response Matching Estimation

IRM is an indirect inference estimator and tries to estimate structural parameters of the model by quantitatively matching conditional dynamics of the data and model. It first requires running VAR on the data, and finding impulse responses to structural shocks. Then these responses are compared with the responses generated from the model with many sets of parameters chosen from the accepted empirical range. The set of parameters creating the closest response to that of the data becomes the set of estimated parameters[1].

Following this methodology, any identification problem that could emerge with this estimation should be related to, firstly, the chosen objective function

[1] For the recent advances in the method, please refer to Hall et al. 2009

(since there are 3 structural shocks, there are so many impulse responses that could be used for matching). Secondly, to the model itself (even though all shocks are used, some parameters could still be under identified depending on model). Thirdly, to sample size (which is effective in the quality of VAR estimation results) and finally, to the identification of structural shocks on VAR residuals (since this requires both specification of number of lags to be used in VAR and choosing type of restrictions to apply on VAR residual covariance matrix).

As it is mentioned in the introduction, instead of obtaining VAR responses from the simulated data, Canova and Sala (2006) use impulse responses from the model by using, what they call, 'true' parameters values. Then they try to find set of parameters that creates the closest impulse response to this. Using the model impulse responses in their objective function, they abstract themselves from last 2 problems mentioned above. As a result, they are able to indicate problems in identification of parameters which arise from the structure of the model under different objective functions[2]. They show that even if true impulse responses are used in matching, there still remains bias in the estimation of parameters. I will also justify this and show related bias. I also predict that the extra bias in estimations that I obtain through using sample impulse responses should

[2]In fact they also use a sample VAR approach and find impulse responses to monetary shocks to infer conclusions related to the sample size. However, as I am going to mention, this will be the only shock that could be correctly identified from the model. Here I also need to identify other shocks and use full information to mitigate weak and under identification problems.

be parallel to the degree of quantitative closeness of this to the model impulse responses. Thus, the practice of choosing the best specification scheme to use with sample VAR should be looking this quantitative difference obtained under different identifications of structural shocks and under different lag lengths used for modeling the VAR[3]. Later in this paper I prove my intuition by comparing true parameters with the parameter estimates found by impulse responses that are in different distances to them.

I compare Choleski decomposition and sign restriction for identification. Choleski is right to identify monetary shocks when the interest rate takes the first order in VAR, but incorrect for the other two shocks while alternative sign restrictions are correct but very generic. So comparing their results, I want to investigate the trade-off between choosing wrong specification and using less information.

In choosing the set of parameters that gives the best match of simulated data responses to those of the true model, the objective function I use is to minimize sum of the squared differences between these two responses[4]. For this

[3]Notice that this practice, comparison of sample impulse responses obtained via different identification schemes with true responses in deciding which scheme to use, has potential to be applied in many projects scopes of which are different than this one. This is because each DSGE model may require different identification scheme than the other; hence, before taking these models to the real data, this simulation exercise could give the best scheme to be employed.

[4]The qualitative properties of the results were not much of a different when I used the absolute difference between these two responses.

aim I use responses for 20 quarters length and calculate

$$M = \frac{\sum(IR\_data - IR\_model)^2}{\sum(IR\_model)^2}$$

In this equation, IR_model will stay constant for each shock but IR_data will change under different specifications used to identify that shock. This equation penalizes more the large deviations from the model responses, and also reveals deviations in percentage terms.

I start my practice by using the Choleski decomposition type of identification. First, I check for appropriate lag length in VAR. The presence of both forward and backward looking components indicates that running VAR with 2 lags should be enough to approximate to the reduced form solution of this structural model. But I try each lag up to 4 (though for the sake of space I only reveal the results of first three).

The results are documented in Table 1. As mentioned, the numbers in the table show bias between responses of sample VAR and the true model. I obtain these results for any of the objective function, i.e. type of the shock, that can be used to generate impulse responses. They are the median values of 100 generations, and standard errors are calculated from the difference between these estimates. Since these standard errors are large compared to the estimated medians, 100 replications may not seem to be enough. However, this procedure is very computationally demanding and the main aim here is not to pin these numbers down but rather compare them to decide on the type of specification. Hence, I do not proceed any further for higher numbers of replications.

Table 1 shows the results obtained by changing the sample size and lag length specification of VAR. We observe that for any lag length, as sample size increases, data responses get very close to the true model ones for each of the objective function I use. Also, for any of the sample lengths the results obtained with using one and two lags in VAR are much closer to the true impulse responses. Even though it seems there is no clear difference between the results found with one and two lags, while sample size increases, using two lags seem to give better results. As a result, as it is also supported by the theory I use two lags in running VAR to employ Choleski decomposition[5].

Instead of taking separate shock responses in forming an objective function, we can try to minimize the distance between impulse responses of all shocks at the same time, which is called the full information approach. Thus, I also formed a vector from impulse responses of all shocks and calculated its distance to the same vector created with true impulses, results denoted by 'Total' in the tables. By careful examination, we can see that the results of this practice are the average

[5] At this point reader may wonder why I did not utilize a standard test to decide for the appropriate number of lags, such as Akaike. This is because my measure fits better to my aim of catching true responses with the data. Surely having the program file that I use for comparing different identification schemes facilitated this practice.

of results of each separate minimization problem. Later in this paper I show its implications for parameter estimation.

The last results in Table 1 are to check the correctness of my specification in terms of ordering of variables in VAR. They show the results when the orders of inflation and output gap are changed. I consider doing this worthwhile as both of the equations affected from all of three shocks contemporaneously. However, this practice did much worse in identification of responses.

As I indicated above we can identify the monetary shock correctly with Choleski decomposition. But in the table, responses of that shock deviated from the true model responses more than the other sample VAR impulse responses. In this paper, what I conclude as a reason behind this deviation is the lack of autocorrelation in the error term of monetary equation. This makes the effect of this shock on the system not long lasting, which further makes it harder to be identified.

The second identification structure that I use to extract structural responses from the data is sign restriction, and I further apply the same restrictions on both reduced form (Wold) and Choleski impulse responses. The directions of these restrictions are obtained from model impulse responses. The results are on Table 2. It shows that all the bias in estimations are larger than the ones I obtained with Choleski decomposition. Therefore I prefer not to give details on this table. The theoretical reason for Choleski to dominate sign identification should be related to the fact that sign restrictions pin down a plane and not a point. As a result, there is a larger degree of uncertainty.

To see the implication of deviations found by Choleski decomposition on the parameter estimates, I estimated parameters by using both the true model and the sample VAR responses under different shocks. For my results not to be sensitive to the initial parametrization in the optimization routine (that minimizes the objective function), for each type of shock I make 75 initial parametrizations and get the median estimates of the resulting parameter estimates. Then I repeated this entire process 20 times for different samples. Final estimates are the median values of this last replication and standard errors are also calculated from these estimates. A final remark on this estimation would be that the structural parameters ζ and θn enter the system once and within the same reduced form coefficient, therefore one of them is not identifiable. I try to overcome this partial identification problem by fixing θn to its true value of 3.0.

Table 3 presents the results. The numbers under the parameters show percentage bias in their estimates from their true values, this way is chosen because of high number of entries in the table. For each type of shock, the first line shows the bias if I use true model responses in the objective function. We see that even if I use true responses, there exists a strong bias in estimations that would result from observational equivalence, weak, partial and under identification problems that also depend on the chosen objective function[6]. In general, while β and ζ are identifiable in similar degree under different shocks, the results for the rest of the parameters suffer strongly from the chosen one. We also observe that when full information is used in constructing the objective function, the bias decrease significantly. Notice that this occurs in spite of the fact that on Table 1, responses obtained via full information are found to be at average distance of other responses to the model responses. Pointing out that using full information ensures more regular fit to true impulse responses. To save some space, I do not reveal the estimation results that I obtain with the sample length of 320. This is a harmless decision as the pattern observed while passing through the sample length of 80 to 160 is the same between samples of 160 and 320

When sample VAR is used, due to the sample size and shock identification problems, bias are much higher. However, as sample size increases, they lessen significantly. Standard errors lessen with increasing sample size too, while this is not robust to estimation of every parameter. In fact, since comparing the estimations of 10 parameters at a time is a hard task, if we want to investigate length effect on estimation, we can directly look for the percentage difference between sample and true model impulse responses for different sample lengths,

[6] If it is monetary shock, as an example, weak identification exists for monetary policy parameters, while under identification is relevant for the estimation of autocorrelation terms that are not affected from this shock.

shown in column 3. My claim was that the closeness of the former to the latter will be a determining factor in estimations. For example for IS shocks, when I use 80 observations, this difference is 8.62, but it decreases with longer sample sizes, to 2.86 (and further to 1.38 when the sample length is 320). Similar findings exist for all shocks without exception, and this decrease is also reflected in parameter estimates.

Chari, Kehoe and McGrattan (2005), among other things, follow my approach with the difference that instead of using short samples of data simulated from the model, they use population impulse responses. They indicate that due to misspecifications, there can be quite significant differences between model and population responses, even though the latter is not subject to small sample bias. From this perspective, they also support Sim's (1989) approach, who says the right approach could be comparing impulse responses from actual and simulated data, so that both could suffer from the same misspecifications. But here I discuss that all these inferences actually depend on the model and related identification schemes. In their paper authors use long run and sign restrictions on different model. I also show that with sign restrictions there is significant bias between simulated data and model impulse responses with my model. But I also show that there is high variance in the estimated responses with sign identification, so using this wrong identification on both actual and simulated data does not necessarily create symmetric misspecifications.. Here I recommend Choleski, and in conflict to their argument, I show that the difference between theoretical impulse responses of model and responses from simulated data are quite small. It decreases even further with increasing sample length. Hence, my approach of comparing model and simulated data responses should yield correct specification of shocks and has important implications for use with real data; at least it turned out to be so in the model I employed.

One interesting result that I reveal in Table 3 is that there is 8.62 percent difference between sample and true model IRs. After I estimate parameters, I generate the impulses from the model by using these estimates. Resulting difference between these and true impulse responses is 7.64, less than 8.62. Pointing out that the new responses are closer to the true ones compared to the sample responses used to estimate them. From this finding what I infer is that model fits the data to itself.

Examining the percentage differences in parameter estimates and in between model and sample VAR responses; I decide to use full information results to compare with GMM estimates.

3.4 GMM Estimation

For the GMM estimation there are two model specific issues that would cause identification problems. First, in equations (2) & (3) expectation terms appear, and we are only equipped with realizations of the data. Since GMM, unlike IRM, is directly applied to estimate reduced form parameters of these equations, we should expect bias when we apply this estimation. To overcome this issue, I have used several specifications, which I am going to explain soon equation by equation.

The second problem is the weak instrument problem. This is a general problem in the GMM estimation but its relevance is further amplified in the simulation studies. This is because this problem arises when there is a correlation between instruments and error terms, and simulated data restrict us to use only the same 3 variables (and their lags) as an instrument. Since this is a complete and dynamic model, this means that all variables are endogenous and chosen instruments include information from past errors as well. Given that error terms are also autocorrelated, it turns out that these instruments should also be correlated with today's error terms. In an attempt of overcoming this problem, I could try to use more lagged values of instruments but this would cause efficiency loss. Later in the paper I make comparative statistics by taking autocorrelation in the error terms zero, so that I will be able to search for the effect of this problem.

Although the problem of weak instruments is well recognized in the literature, I could not encounter much of a source that carries out comparative statistics for the effect of autocorrelation. Rather, the literature is concentrated on the ways of overcoming this problem. To this end, approaches like optimal GMM (Fuhrer and Olivei 2004) or Maximum Likelihood (Linde 2005) is used. As a result, these estimations should still include bias both of the problems mentioned above; presence of forward looking variables and autocorrelated error terms. Hence, they do not address how to differentiate between these two biases so long as they do use autocorrelated error terms.

Other than these specification-based problems with GMM, there are also structural differences between estimators that lead them to have comparative advantage over each other. These are the following

• While IRM uses structural parameters in estimation, GMM is a reduced form estimator. Therefore it also requires mapping of structural parameters from reduced form ones. This problem would be amplified if the formers are complicated function of the latter's.

• GMM is a single equation method and requires recursive recovering of structural parameters that appear in more than one equation. This asserts use of already biased estimates, obtained in the initial steps of estimation, utilized in the later equations.

• GMM is a single equation method. Hence, if there exists any mis-specified equation, so long as its parameters do not appear in the other equations, GMM would give biased estimates only for the mis-specified equation. But since IRM uses the full system, the mis-specified equation will affect estimates of all the parameters (there are implications of the first two conditions in my results but of the last one. This is because I employ the correct model with the simulation approach).

Below, I try to address the model specific problems and try to find out the best way of estimating each equation by GMM. So that remaining differences between estimates of the GMM and IRM would be the result of the structural differences between the estimators.

3.4.1 GMM Estimates of the Equation 1

I start with estimating monetary policy equation. This equation does not suffer from the two model specific problems mentioned above. Furthermore, it has 3 parameters and 3 reduced form coefficients, thus it is just identified. I use the moment condition with different instruments, i.e. z_t's

$$E_t \left\{ \left[i_t - \lambda_r i_{t-1} - (1-\lambda_r)\lambda_\pi \pi_{t-1} - (1-\lambda_r)\lambda_y y_{t-1}) \right] * z_t \right\} = 0 \tag{4}$$

Table 4 presents results. In the estimations I chose instruments and their lags by testing them on a sample length of 1000. They are written below the table. In GMM estimation of parameters, I use Hansen's (1982) optimal weighting matrix that uses inverse of the spectral density matrix of the calculated moments, so that more weight is given to moment conditions with less uncertainty. As an initial weighting matrix, I use inv(Z'Z). Since there is no autocorrelation in the error term of this equation, I include White's (1980) kernel based estimator, which accounts for heteroskedasticity in the calculation of a spectral density matrix. For every sample length I carry out 250 simulations. Table shows the median and standard errors of these 250 estimates. After mapping each 250 reduced form coefficients to structural parameters, I again followed the same procedure.

Table 4 shows that the structural parameter estimates of GMM are very close to true parameter values. The standard errors are negligible as well. With an increase in the sample size, both bias and standard errors decrease as expected. It is further observed that for all the samples GMM outperforms IRM estimation. This is expected since this equation is free from the above mentioned model specific problems and required me to use very appropriate moment condition for GMM. IRM, on the other hand, uses all the system of equations to estimate these parameters.

3.4.2 GMM Estimates of the Equation 2

For this equation I use the following moment condition.

$$E_t\left\{\left[\pi_t - \frac{\omega}{1+\omega\beta}\pi_{t-1} - \frac{\beta}{1+\omega\beta}E_t\pi_{t+1} - \frac{(\phi+\theta_n)(1-\zeta\beta)(1-\zeta)}{(1+\omega\beta)\zeta}y_t\right] * z_t\right\} = 0 \quad (5)$$

Estimation of this equation suffers both from a weak instrument problem and the presence of a forward looking component. To solve them, I use what are sometimes referred to as conventional methods. There are several other approaches that is used to estimate this model with GMM, but they are not only very specific, but also proposed as an alternative to conventional methods, and to this end I propose IRM[7].

As errors are autocorrelated, in construction of weighting matrix I make correction by using Newey and West (1987) kernel for the spectral density matrix. For that I use $abs(T^{1/3})$ number of lags, where T is the sample length. Regarding the number of moment conditions to use, Canova (Ch 5) indicates than even though large set of moment conditions improves asymptotic efficiency, they increase small sample biases. Hence, I use both over- and just-identified number of moment conditions.

[7] In fact I tried one of these approaches. Djoudad and Gauthier (2003) uses Fair and Taylor (1983) methodology to solve rational expectations on a very similar model to mine; their method differed in the inclusion of i.i.d error terms. In practice, this method requires inclusion of more lags of forward looking variable in the estimation. Eventually, I could not get better estimates with this method compared to conventional GMM estimation. Later in this paper I find out that the real problem arising with GMM estimations is not the presence of an expectational term, but rather its co-existence with autoregressive error terms, which creates a weak instrument problem unable to be solved by using this alternative approach.

3.4.2.1 *1st Specification:*

This is the basic GMM estimation on the equation (5)

$$E_t \left\{ [\pi_t - A\pi_{t-1} - BE_t\pi_{t+1} - Cy_t] * z_t \right\} = 0 \tag{6}$$

The instruments that I use are written under the estimation results.

3.4.2.2 *2nd Specification:*

I estimate equation (6) by using the constraint $A + B = 1$. This is implied by the structural form of this equation, equation (5), and says that the sum of forward and backward looking components is set to 1. This is supposed to improve parameter estimates under the weak instrument problem.

3.4.2.3 *3rd & 4th Specifications:*

Now I try to address the presence of forward looking variable. Although $E_t\pi_{t+1}$ is unknown, future expectations should be based on today's states of the model, which are i_t, y_t and π_t. Hence, we can try to eliminate this variable. However, solving the system of equations and obtain reduced form solutions in terms of states is very computationally demanding and should not be the economical way of carrying out GMM. Instead, π_t can be regressed on the first n lags of these variables and estimated regression coefficients can be used to predict the lead of the variable by using current and $n-1$ lags of the variables. I followed this approach, and to this end used first two lags of variables, which I already found to be appropriate lag length in the VAR. Specifications 3 & 4 are obtained as counterparts of specifications 1 & 2 with expectation term is instrumented.

I will present the estimation results for equation 2 together with those for equation 3. Once again, for both of equations I will reveal the median estimates of 250 replications and their standard errors, like I did for equation 1.

3.4.3 GMM Estimates of the Equation 3

The moment condition for this equation is

$$E_t \{[y_t - Dy_{t-1} - E(E_t y_{t+1}) + F(i_t - E_t \pi_{t+1})] * z_t\} = 0 \tag{7}$$

To estimate this equation with GMM I apply exactly the same specifications I used for equation 2, including setting the sum of coefficients of backward and forward looking components to 1. For specifications 3 & 4, now both of the future variables are instrumented in the same way.

Table 5 shows reduced form parameter estimates of these equations. Both yield common results;

a) Over-identified estimates give better results than just identified ones (so from now on I will narrow out attention to over identified ones)

b) Imposing the restrictions gives better estimates, though they are still highly biased (notice that in specifications 2 & 4, the sum of first two reduced form coefficients is equal to 1)

c) Instrumenting for forward-looking components does not necessarily give better estimates (as I discuss later in this paper, the above mentioned bias come from the autocorrelation in the error terms, not presence of forward looking variables)

d) Sample size is affective on standard errors, but not much on the median parameter estimates

In the light of these findings, in mapping the reduced form parameter estimates to the structural ones, I restrict myself to over identified case with specifications 2 & 4, which use restrictions brought by structural form of equations.

Structural parameter estimates are again mapped from reduced form coefficients[8]. However, the structural parameters hidden in the reduced form coefficient of the output gap, C, are not identified from this equation. Therefore, I need to either fix 2 of 3 parameters, which are ϕ, θ_n, and ζ, by using their true values, or alternatively I can use the estimate of ϕ from the equation 3 and fix either θ_n or ζ. To be fair in comparison of GMM with IRM, I use ϕ's biased estimate from equation 3 and fix θ_n to its true value, 3.0.

[8] I also mapped reduced form results of other specifications to structural parameters and verified that they do not bring better estimates.

Table 6 shows the structural parameter estimates of both GMM and IRM for the equations 2 & 3. Contrary to the equation 1, IRM estimates of structural terms are definitely better for both of the equations, both in terms of point estimates, but also they have much smaller standard errors[9]. Even though for equation 3 and with the sample of size 80 GMM seems to yield the better estimate, this is not consistent with other samples. These results may induce us to use GMM for the model equations expected to have very small degrees of autocorrelation in its error terms, then using the resulting parameters estimates in the application of IRM, or of another estimator of interest, by keeping them fixed. Finally, we also see that some of the terms in GMM estimations equal to 1. This is because the reduced form coefficients include second degree equations of structural parameters, and to find the root of these within the desired range, I restricted β to less than 1. This, in fact, implies that the actual estimate of structural parameters should be even less accurate.

3.5 Comparative Statistics: i.i.d. Error Terms

Table 7 is analogous to Table 5, but with autocorrelation parameter set to 0 in simulating the data. It shows that results regarding over identification are still better than just identified ones. Besides, specifications 2 & 4, which are

[9] It is worth to remind that these estimates obtained under the presence of autocorrelation in the error terms, with the degree of persistence $\rho = 0.65$.

estimated with imposing theoretical restrictions, still give better estimates than 1 & 3. More importantly, they are also much better than the ones in Table 5 for all parameters. They are even very close to the true values. This means that it has to be autocorrelated error terms creating bias in the prior estimates. Finally, the results of specification 2 are better than those of specification 4 for both of the equations. Since I instrumented the forward looking variables in specification 4, unlike the ones in specification 2, this result points out that the main problem with GMM estimation should be the weak instrument problem, not to the presence of the forward looking term.

Table 8 is analogous to Table 6 and shows the structural parameter estimates of GMM and IRM, again assuming non-autocorrelated error terms. Once again, structural parameter estimates of the GMM are acquired by mapping of reduced form estimates. These GMM results reveal that the second specification is still better than the fourth one, but they are also better than the IRM estimates. For equation 3, they are also close to true parameter values, while for equation 2 they most likely suffers from using an estimate of ϕ from equation 3. An important result here is that GMM does better than IRM not only because it does not suffer from the instrument problem this time, but also as IRM results are now worse than in the previous case. The reason is that when error terms are non-autocorrelated, impulse responses lives shorter and it makes the matching process of sample VAR responses to the model responses less precise. We also see that the increase in bias with IRM estimation occurs contrary to the fact that the number of parameters to estimate decreased by 2. This is better reflected in equation 2. Yet, these biases decrease significantly with increasing sample size. To remind, under performance of the IRM estimation is also reflected in a different way in Tables 1 & 2. They reveal that the distance between model and sample VAR impulse responses in response to monetary shocks is huge compared to responses to other shocks, in spite of the fact that the monetary equation is the only one with an i.i.d. error term. As a result, I conclude that for lower values of autocorrelation in error terms GMM gives better results; however, as autocorrelation increases IRM starts to yield similar results.

I do the last comparative statistics on the structural parameter estimates of equation 2. In one case I obtain them by inserting ϕ from its estimate from equation 3, and in the other I use its true value. I assume no autocorrelation in both cases in simulating the data. The results are in Table 9. They show that the estimates for equation 2 are much more close to true values. In fact, this simple result reminds us that depending on the model, small bias in the estimation of reduced form parameters can be amplified after reduced form parameters are mapped to structural ones. This, indeed, can be also observed from the presence of large standard errors in the estimates of structural parameters compared to standard errors in the estimates of reduced form parameters. Hence, GMM results should be evaluated carefully if there are model parameters existing in more than one equation and requires recursive estimates of parameters.

3.6 Concluding Remarks

In the comparison of the GMM and the impulse response matching (IRM) estimators in identifying the structural parameters of a DSGE model, I used three equation hybrid New Keynesian macro model and employed a Monte Carlo study. Firstly, I searched for the best way of applying each estimator. For the GMM this resulted in applying all the theoretical restrictions implied by the structural parameters of the model on the reduced form coefficients and in using over-identifying moment conditions. For the IRM, on the other hand, it required me to use, firstly, the responses of all the shocks, full-information, in the objective function. This makes sense as different objective functions may have different information about the parameters. As a second, to use the Choleski decomposition in identification of structural shocks from the VAR residuals instead of sign identification. I further discuss that in an attempt to take model responses to that of the data, the right practice does not necessarily have to be to compare the impulse responses generated from the simulated and actual data so that both could suffer from the same misspecifications. At least this did not work out by sign identification, as this method resulted in high variance in the estimated responses. But instead, model responses can be compared with the responses generated from the simulated data obtains with different techniques of identification and the best scheme can be chosen.

Both IRM and GMM estimations resulted in significant bias caused from presence of forward looking terms, especially the GMM estimation. However, the key determinant that dominates the comparative efficiency of estimators turned out to be the persistence of structural shocks. In the absence of autocorrelation, the GMM gives more precise estimates. But for higher degrees of autocorrelation, not only the GMM starts to suffer from weak instruments problem, but also IRM estimation benefits from the longer response to shocks. I also show that GMM estimation suffers strongly from using one equation at a time that requires recursive recovery of parameters if they exist in more than one equation. I document the clear advantage of IRM in this respect as it uses all equations simultaneously.

3.7 APPENDIX

Table 1: Percentage Differences Between Sample VAR and Model Impulse Responses

Identification With Choleski Decomposition

	1 lag				*3 lags*		
(Sample Size)	80	160	320		80	160	320
IS	9,25	5,42	3,98	IS	14,52	7,26	4,47
	4,11	3,09	1,46		5,75	2,73	1,83
Cost Push	16,65	11,64	10,25	Cost Push	16,53	11,14	7,65
	5,88	3,19	2,56		6,12	3,14	3,21
Monetary	19,64	10,40	7,80	Monetary	67,64	27,49	11,17
	11,01	5,92	3,28		31,83	11,76	5,52
Total	12,44	7,82	6,31	Total	19,13	10,03	6,01
	3,21	2,06	1,36		4,41	2,08	1,53

	2 lags				*2 lags, wrong iden.*		
	80	160	320		80	160	320
IS	11,35	5,15	3,82	IS	26,27	24,97	23,92
	3,93	1,87	1,57		6,61	5,35	3,63
Cost Push	15,79	9,04	5,81	Cost Push	32,72	26,90	30,03
	9,65	2,84	1,44		18,73	10,04	8,54
Monetary	26,87	12,57	6,04	Monetary	26,41	10,72	5,37
	15,79	8,59	3,22		19,79	7,18	4,85
Total	14,96	7,44	4,86	Total	27,90	24,34	24,44
	3,55	1,32	0,93		7,20	4,84	3,63

The first rows show the median values of 100 estimates,
and the second ones shows their standard errors

Table 2: Percentage Differences Between Sample VAR and Model Impulse Responses

Sign Identification Results

	Sign Restrictions on Choleski Impulses				Sign Restrictions on Wold Impulses		
		1 lag				*1 lag*	
(Sample Size)	80	160	320		80	160	320
IS	18,75	18,30	17,80	IS	31,12	31,11	30,56
	5,12	4,08	3,44		3,95	2,63	2,27
Cost Push	15,82	11,61	10,14	Cost Push	35,91	33,38	34,19
	3,65	2,36	1,61		5,51	6,25	5,43
Monetary	105,36	90,93	129,66	Monetary	66,49	65,38	71,93
	85,36	73,23	70,03		42,16	50,46	40,89
Total	36,00	31,67	38,09	Total	38,62	37,64	38,39
	12,22	9,54	8,95		3,84	4,57	3,55
		2 lags				*2 lags*	
	80	160	320		80	160	320
IS	21,09	16,96	14,71	IS	32,25	29,71	28,47
	6,19	2,65	2,44		4,17	2,56	2,78
Cost Push	20,76	14,93	11,62	Cost Push	38,49	38,93	41,04
	5,24	4,02	3,49		8,45	8,16	10,40
Monetary	104,18	102,80	102,30	Monetary	68,56	66,46	77,11
	67,67	79,27	80,22		50,87	55,00	57,83
Total	38,80	34,16	31,64	Total	40,39	38,75	40,02
	10,17	10,33	11,03		5,35	4,48	5,93

Table 3

Impulse Response Matching Estimates of Parameters (Percentage Bias)

		*	**	h	ϕ	ω	β	ζ	λ_r	λ_π	λ_y	ρ_1	ρ_2
Monetary	*Model*	*0,00*	*0,00*	*0,30*	*0,04*	*0,36*	*0,52*	*0,24*	*7,24*	*8,97*	*12,50*	*6,34*	*27,63*
	80	23,78	7,74	39,31	20,28	296,00	1,52	4,00	196,24	54,87	330,46	8,29	25,28
		9,54	3,68	25,13	6,38	64,41	0,05	3,79	30,13	68,57	66,90	8,00	5,19
	160	17,95	6,60	16,47	16,10	296,00	1,52	8,28	167,92	34,84	328,00	7,31	22,93
		15,52	4,10	18,68	10,17	69,72	0,14	3,33	58,90	7,13	100,91	4,54	5,42
IS	*Model*	*0,00*	*0,00*	*0,00*	*0,02*	*0,61*	*0,28*	*0,07*	*2,58*	*1,62*	*3,54*	*0,02*	*27,63*
	80	8,62	7,6	32,05	30,06	96,00	1,52	12,72	19,90	31,31	47,61	18,15	25,28
		2,94	2,66	26,83	23,18	38,09	0,03	4,01	23,95	10,58	26,01	8,98	1,87
	160	2,86	2,25	50,75	31,82	91,30	1,44	9,59	20,69	13,86	38,48	10,27	25,28
		1,07	1,02	21,44	24,33	42,35	0,04	1,92	18,75	7,58	17,85	5,83	1,87
Cost Push	*Model*	*0,00*	*0,29*	*1,58*	*0,41*	*20,48*	*0,19*	*1,02*	*1,30*	*0,08*	*0,28*	*6,34*	*6,57*
	80	15,98	9,71	16,47	47,76	28,49	1,32	5,83	95,00	15,50	80,58	7,31	5,33
		3,09	3,02	19,45	43,18	16,53	0,19	11,71	45,09	35,44	106,88	4,19	16,73
	160	8,92	5,15	8,53	11,20	93,56	1,52	4,39	106,34	41,55	104,32	7,31	6,92
		2,07	1,19	4,04	13,58	76,23	0,00	2,57	50,47	33,93	98,50	4,19	4,92
Total	*Model*	*0,00*	*0,00*	*0,02*	*0,00*	*0,08*	*0,07*	*0,03*	*0,04*	*0,01*	*0,00*	*0,00*	*0,01*
	80	13,75	6,96	3,41	0,19	62,46	1,03	9,93	69,13	9,88	27,12	21,35	4,05
		3,09	3,10	28,08	8,69	17,46	0,07	7,08	31,97	3,79	23,71	11,12	11,13
	160	6,28	2,95	3,10	1,50	5,91	1,20	3,08	6,99	3,55	16,76	5,67	7,32
		3,22	1,47	5,67	5,00	39,07	0,04	2,08	19,43	2,69	9,09	4,92	5,37

$*$: Percentage difference between sample and true model impulse response

$**$: Percentage difference between generated and true model impulse response

Table 4: Estimates For Equation 1

		Reduced Form Coefficients			Structural Parameters		
		λ_r	$(1-\lambda_r)\lambda_\pi$	$(1-\lambda_r)\lambda_y$	λ_r	λ_π	λ_y
		0,200	1,240	0,880	0,200	1,550	1,100
80	GMM	0,203	1,237	0,878	0,203	1,541	1,110
		0,04	0,07	0,08	0,04	0,10	0,13
	IRM				0,258	1,397	1,398
					0,09	0,10	0,24
160	GMM	0,206	1,237	0,879	0,206	1,559	1,098
		0,03	0,05	0,06	0,03	0,06	0,10
	IRM				0,205	1,495	1,284
					0,03	0,03	0,07
320	GMM	0,201	1,233	0,878	0,201	1,544	1,099
		0,02	0,03	0,04	0,02	0,05	0,07
	IRM				0,182	1,428	1,290
					0,03	0,04	0,07

Instruments for GMM: c, interest rate and 3 lags of all the variables

Table 5: Reduced Form Coefficient Estimates of Equations 2 & 3 with GMM

		Equation 2			Equation 3		
	Reduced Form	*A*	*B*	*C*	*D*	*E*	*F*
	True Values	0.2006	0.7943	0.6234	0,459	0,541	-0,500
80	GMM Over Iden.						
	1	0,383	0,529	0,076	0,439	0,641	-0,189
	2	0,377	0,623	0,065	0,472	0,528	-0,183
		0,12	0,12	0,09	0,08	0,08	0,12
	3	0,144	1,419	0,141	0,428	0,753	-0,240
	4	0,249	0,751	0,097	0,450	0,550	-0,283
		0,17	0,17	0,11	0,14	0,14	0,17
	GMM Just Iden.						
	1	0,521	0,471	0,086	0,243	0,986	-0,136
	2	0,496	0,504	0,024	0,257	0,743	-0,196
	3	0,153	1,709	0,264	0,495	0,319	-0,070
	4	0,428	0,572	0,048	0,427	0,573	0,058
160	GMM Over Iden.						
	1	0,342	0,597	0,036	0,370	0,835	-0,148
	2	0,317	0,683	0,012	0,422	0,578	-0,183
		0,09	0,09	0,05	0,06	0,06	0,05
	3	0,074	1,592	0,146	0,381	0,908	-0,202
	4	0,227	0,773	0,041	0,398	0,602	-0,270
		0,12	0,12	0,08	0,09	0,09	0,12
	GMM Just Iden.						
	1	0,543	0,556	0,196	0,214	0,957	-0,160
	2	0,504	0,496	0,018	0,223	0,777	-0,214
	3	0,060	1,742	0,197	0,432	0,554	-0,115
	4	0,274	0,726	-0,059	0,437	0,563	0,038

Table 5 (cont.)

		Equation 2			Equation 3		
	Reduced Form	*A*	*B*	*C*	*D*	*E*	*F*
	True Values	0.2006	0.7943	0.6234	0,459	0,541	-0,500
320	GMM Over Iden.						
	1	0,319	0,654	0,001	0,271	10,763	-0,121
	2	0,301	0,699	-0,008	0,409	0,591	-0,174
		0,05	0,05	0,03	0,04	0,04	0,03
	3	0,068	1,602	0,120	0,266	11,239	-0,132
	4	0,258	0,742	0,020	0,347	0,653	-0,227
		0,08	0,08	0,05	0,07	0,07	0,06
	GMM Just Iden.						
	1	0,554	0,401	0,117	0,208	0,930	-0,173
	2	0,453	0,547	0,016	0,201	0,799	-0,207
	3	-0,044	1,931	0,186	0,386	0,419	-0,093
	4	0,447	0,553	0,010	0,405	0,595	0,053

	Instruments for Equation 2	Instruments for Equation 3
Over identified case		
Specifications 1&2	c, 4 lags of variables	c, 4 lags of variables
Specifications 3&4	c, 6 lags of variables	c, 6 lags of variables
Just identified case		
Specifications 1&2	c, 3. and 4. lags of inflation	c, 1 and 2. lags of inflation
Specifications 3&4	c, 4. and 5. lags of inflation	c, 2. and 3. lags of inflation

Table 6: Structural Parameter Estimates of Equations 2 & 3

		ω	β	ζ	h	ϕ
	True Values	0.25	0.985	0.68	0.85	2,000
80	GMM Over Iden.					
	2	0,626	1,000	0,909	0,846	5,460
		0,28	0,08	0,08	0,55	6,21
	4	0,284	1,000	0,879	0,799	3,524
		0,28	0,05	0,09	0,30	41,73
	IRM	0,094	0,975	0,748	0,879	1,996
		0,07	0,01	0,03	0,22	0,20
160	GMM Over Iden.					
	2	0,499	1,000	0,952	0,730	5,466
		0,18	0,02	0,07	0,12	0,07
	4	0,241	1,000	0,938	0,660	3,706
		0,22	0,01	0,08	0,23	2,00
	IRM	0,094	0,975	0,748	0,876	1,970
		0,07	0,01	0,03	0,12	0,09
320	GMM Over Iden.					
	2	0,454	1,000	0,979	0,692	5,749
		0,11	0,00	0,05	0,29	1,93
	4	0,331	1,000	0,943	0,531	4,399
		0,14	0,00	0,06	0,23	2,70
	IRM	0,136	0,976	0,715	0,91	1,975
		0,09	0,01	0,01	0,01	0,23

Table 7: Reduced Form Coefficient Estimates of Equations 2 & 3 with GMM

(under non-autocorrelated error terms)

		Equation 2			Equation 3		
	Reduced Form	A	B	C	D	E	F
	True Values	0.2006	0.7943	0.6234	0,459	0,541	-0,500
80	GMM Over Iden.						
	1	0,010	0,032	0,279	0,351	-0,018	-0,426
	2	0,241	0,759	0,481	0,463	0,537	-0,455
		0,13	0,13	0,10	0,17	0,17	0,30
	3	-0,037	-11,936	0,220	0,368	-0,476	-0,458
	4	0,131	0,869	0,424	0,527	0,473	-0,551
		0,15	0,15	0,12	0,21	0,21	0,28
	GMM Just Iden.						
	1	0,055	0,764	0,395	1,361	-0,248	-0,713
	2	-0,004	1,004	0,299	1,346	-0,346	-0,750
	3	0,169	-0,935	-0,038	0,482	-0,374	-0,476
	4	0,505	0,495	0,341	0,542	0,458	-0,521
160	GMM Over Iden.						
	1	0,010	-0,145	0,310	0,373	-0,001	-0,463
	2	0,203	0,797	0,512	0,503	0,497	-0,463
		0,07	0,07	0,06	0,14	0,14	0,23
	3	-0,085	-10,473	0,188	0,398	-0,346	-0,468
	4	0,108	0,892	0,452	0,508	0,492	-0,511
		0,09	0,09	0,08	0,13	0,13	0,19
	GMM Just Iden.						
	1	0,007	0,763	0,323	0,979	-0,349	-0,676
	2	-0,032	1,032	0,267	1,286	-0,286	-0,737
	3	0,195	-0,685	0,139	0,371	-0,331	-0,485
	4	0,363	0,637	0,129	0,504	0,496	-0,513

Table 7 (cont.)

Reduced Form	Equation 2 *A*	*B*	*C*	Equation 3 *D*	*E*	*F*
True Values	0.2006	0.7943	0.6234	0,459	0,541	-0,500
320 GMM Over Iden.						
1	0,022	-0,065	0,342	0,391	-0,170	-0,477
2	0,196	0,804	0,538	0,474	0,526	-0,488
	0,05	0,05	0,04	0,13	0,13	0,21
3	-0,027	-10,433	0,251	0,393	-0,375	-0,487
4	0,162	0,838	0,486	0,488	0,512	-0,530
	0,06	0,06	0,05	0,11	0,11	0,18
GMM Just Iden.						
1	0,047	0,739	0,327	1,376	-0,282	-0,738
2	0,016	0,984	0,289	1,311	-0,311	-0,735
3	-0,026	-1,923	0,003	0,437	-0,205	-0,502
4	0,244	0,756	0,166	0,490	0,510	-0,510

	Instruments for Equation 2	Instruments for Equation 3
Over identified case		
Specifications 1&2	c, 4 lags of variables	c, 4 lags of variables
Specifications 3&4	c, 6 lags of variables	c, 6 lags of variables
Just identified case		
Specifications 1&2	c, 1. and 2. lags of inflation	c, current and 1. lag of inflation
Specifications 3&4	c, 2. and 3. lags of inflation	c, 1. and 2. lags of inflation

Table 8: Structural Parameter Estimates of Equations 2 & 3

(under non-autocorrelated error terms)

		ω	β	ζ	h	ϕ
	True Values	0.25	0.985	0.68	0.85	2,000
80	GMM Over Iden.					
	2	0,305	0,996	0,736	0,853	2,200
		0,32	0,15	0,11	0,18	0,45
	4	0,226	0,997	0,716	1,107	1,816
		0,33	0,13	0,13	0,24	3,86
	IRM	0,078	0,996	0,783	0,793	2,167
		0,09	0,01	0,04	0,213	0,314
160	GMM Over Iden.					
	2	0,222	0,997	0,706	1,010	2,161
		0,34	0,18	0,09	0,20	0,52
	4	0,129	0,999	0,749	1,032	1,956
		0,25	0,06	0,10	0,32	0,36
	IRM	0,126	0,996	0,784	0,682	2,120
		0,09	0,01	0,03	0,133	0,258
320	GMM Over Iden.					
	2	0,186	0,997	0,725	0,901	2,049
		0,27	0,07	0,09	0,16	0,24
	4	0,113	0,999	0,730	0,953	1,886
		0,33	0,13	0,13	0,16	0,29
	IRM	0,079	0,999	0,792	0,676	2,128
		0,05	0,00	0,02	0,188	0,248

Table 9: Structural Parameter Estimates of Equations 2

(under non-autocorrelated error terms)

		if estimate of φ is used			if true value of φ is used		
		ω	β	ζ	ω	β	ζ
	True Values	0.25	0.985	0.68	0.25	0.985	0.68
80	GMM Over Iden.						
	2	0,305	0,996	0,736	0,314	0,996	0,716
		0,32	0,15	0,11	0,35	0,22	0,11
	4	0,226	0,997	0,716	0,151	0,999	0,748
		0,33	0,13	0,13	0,35	0,17	0,13
	IRM	0,078	0,996	0,783	0,078	0,996	0,783
		0,09	0,01	0,04	0,09	0,01	0,04
160	GMM Over Iden.						
	2	0,222	0,997	0,706	0,253	0,996	0,711
		0,34	0,18	0,09	0,32	0,15	0,09
	4	0,129	0,999	0,749	0,120	0,999	0,729
		0,25	0,06	0,10	0,26	0,10	0,11
	IRM	0,126	0,996	0,784	0,126	0,996	0,784
		0,09	0,01	0,03	0,09	0,01	0,03
320	GMM Over Iden.						
	2	0,186	0,997	0,725	0,242	0,997	0,695
		0,27	0,07	0,09	0,23	0,04	0,08
	4	0,113	0,999	0,730	0,192	0,997	0,715
		0,33	0,13	0,13	0,20	0,01	0,10
	IRM	0,079	0,999	0,792	0,079	0,999	0,792
		0,05	0,00	0,02	0,05	0,00	0,02

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