Cátedra de Economía y
Finanzas Internacionales

Documento de Trabajo/Working Paper
*Serie Economía*

# Inequality of opportunity and growth

by

Gustavo A. Marrero
and
Juan G. Rodríguez

March 2011

*DT-E-2011-05*

ISSN: 1989-9440

Instituto Universitario de Desarrollo Regional, Facultad de Ciencias Económicas y Empresariales, Universidad de La Laguna, Camino de la Hornera s/n - 38071 La Laguna, Santa Cruz de Tenerife, Spain

**INEQUALITY OF OPPORTUNITY AND GROWTH**[1]

**This version: January 8th 2011**

Gustavo A. Marrero[(1)] and Juan G. Rodríguez[(2)]

(1) Departamento de Análisis Económico (Universidad de La Laguna, Spain) and FEDEA (Spain). Tel: +34 922 317123. E-mail: gmarrero@ull.es
(2) Departamento de Fundamentos del Análisis Económico I (Universidad Complutense de Madrid). Tel: +34 91 3942515. E-mail: juangabriel.rodriguez@ccee.ucm.es

Abstract

Theoretical and empirical studies exploring the effects of income inequality upon growth reach a disappointing inconclusive result. This paper postulates that one reason for this ambiguity is that income inequality is actually a composite measure of inequality of opportunity and inequality of returns to effort. They affect growth through opposite channels, so the relationship between inequality and growth depends on which component is larger. Using the PSID database for U.S. in 1970, 1980 and 1990 we find robust support for a negative relationship between inequality of opportunity and growth, and a positive relationship between inequality of returns to effort and growth.

**JEL** Classification: D63, E24, O15, O40.

**Key Words**: income inequality; inequality of opportunity; economic growth.

[1] We are grateful for assistance with the data base from, Marta Huerta, Nidia García and Elijah Baley. We also acknowledge helpful comments and suggestions of three anonymous referees, François Bourguignon, John Roemer, Francisco Ferreira, Shlomo Yitzhaki, Rafael Salas, the audiences at ECINEQ2009 (Buenos Aires), XXXIV Simposio de la Asociación Española de Economía (Valencia), XII Encuentro de Economía Aplicada (Madrid) and FEDEA (Madrid). This paper has benefited from the financial support of the Fundación Ramón Areces. The usual disclaimer applies.

## 1. Introduction

A surge of literature on the effect of income inequality on growth has emerged over the last two decades, leading to controversial conclusions.[2] This literature suggests many channels through which inequality can affect growth. Accumulation of savings (Galenson and Leibenstein, 1955), unobservable effort (Mirrless, 1971), and the investment project size (Barro, 2000) are some of the main routes through which inequality may enhance growth. On the contrary, inequality can negatively affect growth through the following channels: unproductive investments (Mason, 1988), levels of nutrition and health (Dasgupta and Ray, 1987), demand patterns (Marshall, 1988), capital market imperfections (Banerjee and Newman, 1993; Galor and Zeira, 1993), fertility (Galor and Zang, 1997), domestic market size (Murphy et al., 1989), political economy (Persson and Tabellini, 1994), and political instability (Alesina and Perotti, 1996). Thus, overall inequality would affect growth positively or negatively depending on the channels that dominate.

However, the vast empirical literature does not indicate that any of these channels has a predominant influence. As a result, the empirical relationship between inequality and growth is also ambiguous.[3] This ambiguity tends to be justified through the quality and type of data (Deininger and Squire, 1996; Panizza, 2002), the inconsistent nature of inequality measures (Knowles, 2001), the type of inequality index (Székely, 2003), the econometric method (Forbes, 2000), the model specification (Panizza, 2002) or the set of countries considered and their degree of development (Barro, 2000).[4] Moreover, as pointed out by Partridge (1997 and 2005), Barro (2000), Bleaney and Nishizama (2004) and Voitchovsky (2005), this ambiguity can be due to the fact that income inequality has distinct offsetting avenues affecting subsequent growth in different ways. For instance, the variation in the relationship between inequality and growth with income level and the presence of non-linearities could reflect these alternative offsetting ways by altering economic incentives.

[2] Surveys on this issue can be found in Bénabou (1996), Bourguignon (1996), Aghion et al. (1999), Bertola et al. (2005) and Ehrhart (2009).

[3] See Banerjee and Duflo (2003) on the inconclusiveness of the cross-country empirical literature on inequality and growth.

[4] Ehrhart (2009, p. 39) acknowledges that the overall rather inconclusive econometric results suggest that either the data and the instruments are not sufficient to estimate the true relationship between inequality and growth or the transmission mechanisms really at work are different from those mentioned in the literature.

Following this line of inquiry, we claim that an important reason for this ambiguous relationship is that income inequality is actually a composite measure of *inequality of opportunity* (IO) and inequality of returns-to-effort (IE).[5] Following Roemer (1993) and Van de Gaer (1993), IO refers to that inequality stemming from factors, called *circumstances*, beyond the scope of individual responsibility, like race and socioeconomic background (i.e., proxy by parental education or wealth), while IE defines the income inequality caused by individual responsible choices, like the number of hours worked or the occupational choice. Roughly speaking, total inequality can be viewed as the result of heterogeneity in circumstances, which involves individual initial conditions, and the exerted effort, which basically has to do with individual control variables. We hypothesize that these two types of inequality affect growth in opposite ways. On one hand, IO would reduce growth as it favors human capital accumulation by individuals with better social origins, rather than by individuals with more talent or skills (Loury, 1981 and Chiu, 1998).[6] On the other hand, income inequality among those who exert different effort can stimulate growth because it may encourage people to invest in education and effort (Mirrless, 1971). The main goal of this paper is to test this hypothesis. For this task, we combine the empirical growth literature from macroeconomics and the inequality-of-opportunity literature from microeconomics. A discussion on both these literatures and how they can be linked is presented in Section 2.

Data requirements for comparing inequality of income across states or countries are severe (Deininger and Squire, 1996), but comparisons of IO and its estimation are even more stringent (Lefranc et al., 2008). This is because empirical analysis of IO requires not only comparable measures of individual disposable income but also individual circumstances or social origins measured in a comparable and homogeneous way. Furthermore, there are only few databases with information on individual circumstances or social origins and in these cases the number of circumstances is usually small. In addition, to test for long-term effects on growth, we also need the value of IO for at least two distant periods of time, generally ten years (Barro and Sala-i-Martin, 1991). This last requirement limits even more the availability of databases. To the best of our knowledge, the Panel Survey Income Dynamics (PSID) database is the only exception

[5] Though not considered in this paper, another possible source of inequality is luck (Lefranc et al., 2009).
[6] A similar reasoning is found in World Bank (2006) and Bourguignon et al. (2007b).

that satisfies both requirements and is rich enough in terms of cross-sectional heterogeneity, variables, observations and circumstances. This and other databases are discussed in Section 3, and a sensitivity analysis with the IPUMS-USA database is considered in Section 5 (this database contains large samples but it only includes one circumstance, i.e., race). Given these database limitations, to fulfill the overall objective of the paper, we measure total inequality, IO, and IE at the U.S. state level (Section 3). For this task, we use refined data from the PSID database, and decompose total inequality into inequality across groups (classified by race and parental education, the two observed circumstances), and inequality within groups, by applying the Theil 0 decomposition technique. The first component will be the proxy for IO, while the second component will be the proxy for IE.

In Section 4 we present the results of standard linear pooled-OLS estimations for total inequality, IO, and IE according to a small model (where only a few number of controls are specified) and a base model (where a large set of controls are considered). In addition, Durlauf and Quah (1999), Panizza (2002), Partridge (1997 and 2005), and Barro (2002), among others, emphasize the need to include an extensive sensitivity analysis to supplement any reduced form regression analysis, in order to demonstrate how robust the found results are across alternative econometric techniques, model specifications and non-linearities. Following Panizza (2002) and Partridge (2005), a sensitivity analysis is also carried out (Section 5). In sum, we find that the impact of overall inequality on growth is positive, as in Partridge (1997 and 2005), although it is non-robust to alternative specifications, as in Panizza (2002). On the contrary, the impact of the IO component is negative and significant, while the impact of the IE component is positive and significant. Moreover, these correlations are highly robust to alternative model specifications, econometric techniques and non-linearities. Our results would therefore offer a unified explanation for the two opposite streams of empirical results by suggesting that the overall impact of total inequality on growth could be positive, negative or zero depending on which of the above two sources of income inequality dominates in the data.

Our results can be linked to a substantial amount of literature that treats the relationship between inequality and growth as an endogenous outcome that depends on initial conditions and capital market imperfections (Galor and Zeira, 1993, Banerjee and

Newman, 1993, and Aghion and Bolton, 1997, among others). In a multiple steady state framework with borrowing constraints, these authors propose that higher initial wealth inequality reduces the opportunity of accessing credit to promote many profitable investment projects, which would have a negative consequence on subsequent growth. Correspondingly, initial heterogeneity in certain circumstances could lead the economy into an undesirable equilibrium with high inequality of opportunity and low growth. With regards to initial heterogeneity, the history of slavery is an important one in the U.S., because it implied a large initial wealth inequality between black and white people. Nevertheless, with respect to the condition of race, the immigration of Latino and the history of native indigenous people are also relevant. With respect to socioeconomic status, access to high-quality education can be highly conditioned by the initial level of parental education or wealth, as we will further discuss in Section 2. The subsequent racial and educational barriers for accessing credit in the presence of multiple steady states would imply multiple paths of development for the different racial and parental educational groups and, as a result, a harmful impact of circumstance-based income inequality on growth. Our results would also contradict the alternative and competing hypothesis by Phelan (2006). In particular, this author states that in the absence of multiple steady states, unequal opportunity helps society to provide incentives to work hard, which would enhance growth.[7]

A final comment is worth noting. Our results call for a proper design of policy. In particular, general redistributive policies may increase investment across individuals and thus may increase growth, but also may discourage unobservable effort borne by agents. On the contrary, selected policies reducing IO will promote not only equity – in the sense of opportunity-, but also economic efficiency and growth. We will further discuss this issue in the final Section 6.

## 2. The inequality–growth debate and inequality of opportunity

The last decade has witnessed an intensive debate about the effects of inequality on growth. Meanwhile, the inequality-of-opportunity literature has also increased in

[7] Along this line, Rogerson (1985) has proposed that some restrictions on agents' access to credit is necessary to achieve a Pareto optimal outcome.

importance during the last decade.[8] This section attempts to bring the inequality-of-opportunity issue into the inequality–growth debate.

### 2.1. The inequality-growth debate

In the inequality–growth literature two sets of models have been proposed: models where inequality is beneficial for growth and models where inequality is harmful for growth. On one hand, we find three main reasons for a positive relationship between inequality and growth. First, income inequality is fundamentally good for the accumulation of a surplus over present consumption since the rich have a higher marginal propensity to save than the poor (Kaldor's hypothesis). Then, more unequal economies grow faster than economies characterized by a more equitable income distribution if growth is related to the proportion of national income that is saved.[9] Second, following Mirrless (1971), in a moral hazard context where output depends on the unobservable effort borne by agents, rewarding the employees with a constant wage, which is independent from output performance, will discourage them from investing any effort (Rebelo, 1991). Third, under imperfect credit markets, since investment projects often involve large sunk costs, wealth needs to be sufficiently concentrated in order for an individual to be able to initiate a new industrial activity. Barro (1997) proposes a similar argument for education. Accordingly, investments in physical or human capital have to go beyond a fixed degree to affect growth in a positive manner.

On the other hand, we find three main sets of models in which inequality can discourage growth. The first set refers to models of economic development where three general arguments can be found (Todaro, 1994): unproductive investment by the rich (Mason, 1988); lower levels of human capital, nutrition and health by the poor (Dasgupta and Ray, 1987); and biased demand pattern of the poor towards local goods (Marshall, 1988). The second set groups models of fertility, models of domestic market size and models of imperfect capital markets. According to the endogenous fertility approach, income inequality reduces per capita growth because of the positive effect

[8] Using the *Google Academic Search* tool, the term "inequality and growth" appears 608 times between 1990 and 1999 but 3,690 times between 2000 and 2009. The term "inequality of opportunity" is shown 696 times between 1990 and 1999 but 1,460 times between 2000 and 2009. However, the entry "inequality of opportunity and growth" is shown zero times. There is one academic document for each of the following entries: "inequality of opportunities and growth", "equality of opportunities and growth" and "equality of opportunity and growth". This search was made on May 26th, 2009.

[9] See Galenson and Leibenstein (1955), Stiglitz (1969) and Bourguignon (1981).

that inequality exerts on the rate of fertility.[10] Moreover, the production of manufactures is only profitable if domestic sales cover at least the fixed setup costs of plants. Consequently, redistribution of income may increase future growth by inducing higher demand of manufactures.[11] Wealth and human capital heterogeneity across individuals produces a negative relationship between income inequality and growth wherever capital markets are imperfect. The reason lies on the fact that a large fraction of indivisible investments that are nevertheless beneficial at the individual and aggregate levels cannot be undertaken because the access to the credit is limited to the non poor agents of the population.[12] Finally, the third set of models refers to the political economy literature, where two arguments can be found. First, in a median-voter framework, a more unequal distribution of income leads to a larger redistributive policy and thus to more tax distortion that deters private investment and growth.[13] Second, strong inequality may result in political instability.[14]

As a conclusion from the last two paragraphs, inequality may affect growth through a large variety of opposite routes. Therefore, from a theoretical perspective, the prevalence of a positive or negative relationship between overall inequality and growth depends on which channel predominates. This fact is clearly reflected by the empirical evidence linking income inequality to economic growth: cross-sectional and panel data studies are generally inconclusive. Cross-sectional analysis showing a negative relationship between both dimensions include, among others, Alesina and Rodrik (1994), Persson and Tabellini (1994), Clarke (1995), Perotti (1996), Alesina and Perotti (1996) and Alesina et al. (1996). However, other authors find a positive relationship between growth and income inequality, such as Partridge (1997 and 2005), Zou and Li (1998) and Frank (2009). Barro (2000) shows a very slight relationship between both variables when using panel data, while Forbes (2000) finds a positive relationship. In fact, Forbes' results suggest that overall inequality has a significant positive effect upon growth in the short and medium term, while Partridge (2005) finds that inequality is

[10] See Galor and Zang (1997), Dahan and Tsiddon (1998), Morand (1998), Khoo and Dennis (1999), Kremer and Chen (2002), and de la Croix and Doepke (2003).
[11] See Murphy et al. (1989), Falkinger and Zweimüller (1997), Zweimüller (2000) and Mani (2001).
[12] See Banerjee and Newman (1991), Galor and Zeira (1993), Bénabou (1996), Aghion and Bolton (1997) and Piketty (1997).
[13] See Perotti (1992 and 1993), Alesina and Rodrik (1994), Alesina and Perotti (1994) and Persson and Tabellini (1994).
[14] See Gupta (1990), Tornell and Velasco (1992), Alesina and Perotti (1996), Alesina et al. (1996), Svensson (1998) and Keefer and Knack (2002).

positively related to long-term growth, this relationship being less clear in the short-run. Panizza (2002), using a cross-state panel, finds some evidence in support of a negative relationship between inequality and growth, though this relationship is not robust to different econometric methods and regression specifications. And Barro (2008), using a non-linear equation, finds a negative and significant effect of inequality on growth for poor countries, while a positive but insignificant effect of inequality on growth for rich countries. Given these different findings in the literature, we propose to analyze the inequality and growth relationship using the IO concept.

### 2.2. Inequality of Opportunity and growth[15]

Traditionally, equality of opportunity was understood as the absence of barriers to access education, and all positions and jobs, and the fact that hiring was meritocratic. Race, class and gender should have no bearing on the merit of the individual. In this approach, individuals are completely responsible for their outcome (income, health, employment status, or utility), and, as a consequence, total inequality is due to individual responsible choices (Lucas, 1995). Rawls (1971) and Sen (1980 and 1985) challenged this traditional view to invoke a more general notion. They argued that equality of opportunity actually requires compensating persons for a variety of *circumstances* beyond one's control. This conception, which has been developed over the last two decades, considers that equal opportunity policies must create a "*level playing field*", after which individuals are on their own.[16] The "*level playing field*" principle recognizes that an individual's outcome is a function of variables *beyond* and *within* the individual's control, called circumstances (e.g., socioeconomic, cultural background or race) and effort (e.g., investment in human capital, number of hours worked and occupational choice), respectively.[17] IO refers to those outcome inequalities that are exclusively due to different circumstances. Individuals are, therefore, only responsible for their effort.

[15] It is commonly used in the literature either the concept of equality of opportunity or inequality of opportunity.

[16] See Roemer (1993, 1996, 1998 and 2002), Van de Gaer (1993), Fleurbaey (1995 and 2008), Roemer et al. (2003), Ruiz-Castillo (2003), Peragine (2002 and 2004), Betts and Roemer (2007), Moreno-Ternero (2007), Ooghe et al. (2007), Fleurbaey and Maniquet (2007), Bourguignon et al. (2007a and 2007b), Lefranc et al. (2008 and 2009), Rodríguez (2008), Ferreira and Gignoux (2008) and Checchi and Peragine (2010).

[17] Using the dynamic programming language, circumstances can be seen as state variables, while effort and other choice – or control – variables can be seen as functions of those state variables.

The important role of circumstances has been emphasized in the literature. For instance, Arrow et al. (2000), Hertz et al. (2008), Blume and Durlauf (2001), Durlauf (2003) and Loury (1989; 1999) have found strong evidence for persistent inequality, which is not attributable to discrimination between individuals, but rather to factors such as social networks, poor education quality and intergenerational inertia. Furthermore, Roemer (1998 and 2000) and Bowles et al. (2005), among others, have emphasized that circumstances can affect the realization of talent and, thus, the full achievement of a purely meritocratic society. These authors have shown that even if individuals have high inborn talent, the likelihood of their being able to realize the benefits of that talent (for example, in terms of admission to university or access to employment) will be affected by social conditions. Therefore, the meritocracy approach can be seen as an extreme case for which circumstances are not considered. In this paper, we adopt the more general and modern second approach, which distinguishes between total inequality, inequality of opportunity and inequality of returns-to-effort.

The literature on inequality-of-opportunity has identified the following channels through which parents can affect the income earning capacity of their children (Dardanoni et al., 2006): wealth; provision of social connections which are relevant in the labor market; formation of skills in children, through family culture and investment; genetic transmission (like native ability and race); and, instillation of preferences and aspirations. Given these factors and the restrictions imposed by data availability, this literature has widely used the level of parental education or occupational status as a proxy for the first three factors, and the ethnic group or race as a direct measure of the fourth factor.[18] In this paper, we use parental education and race as the causal determinants of IO. Now, we show several existing routes in the growth-inequality literature of how these two circumstances can affect economic growth.

Following the pioneering works of Banerjee and Newman (1991) and Galor and Zeira (1993), a consensus is emerging that in the presence of borrowing constraints, a higher initial wealth inequality has a negative effect on the long-term economic growth. These authors emphasize the existence of multiple steady-state equilibrium paths, which cause a convergence trajectory that will depend, among other things, on initial inequality. In

[18] See the references in footnote 16. Note that native ability or talent may be considered as a circumstance; however, this variable is controversial as it might reflect a person's past effort (as a child), and hence it is not obviously something for which a person should not be held accountable.

this context, people with unfavorable initial circumstances (because of their race and/or parent's education) will face considerable barriers for accessing credit, regardless of their talent and degree of effort exerted. As a result, they will obtain lower returns on their effort with a negative consequence on growth. Another general argument to support the negative effect of initial IO on growth can be inferred from Heathcote et al. (2008), which focus on the implications of missing insurance markets rather than on borrowing constraints. They emphasize that it is reasonable to believe that society cannot provide insurance against one's circumstances (in our case, race and parental education) and the non-pecuniary discrimination associated with them. As a consequence, income allocation based on those uninsurable factors would be inefficient and would harm growth.

Arguments based on one particular circumstance are also found in the literature. For instance, Chiu (1998) shows that, under liquidity constraints and decreasing marginal utility, a more equitable distribution of wealth among parents improves economic performance. If wealth is redistributed from rich to poor, rich parents would stop sending their less talented children to college, while, among poor parents, more talented children would be afforded the chance to go to college (Loury, 1981 and Bénabou, 1996). As a consequence, aggregate human capital will increase and, therefore, so will growth. Another example can be found in Ferreira (2001), where productivity and wages are determined by the quality of the school one attends (see also Bénabou, 2000). Under borrowing constraints, children from poorer families would not be able to attend private schools, and would thus go to public schools. On the contrary, richer families would send their children to private schools and, because they are willing to pay high fees, these schools would provide high-quality education. Then, if public school budgets are determined by the level of taxation, and the pivotal voter is wealthy enough to send their children to private school, the funds received by public schooling would be low. As a result, children from initially rich families will stay rich because they attend high-quality schools, while children from initially poor families will stay poor because they attend low-quality schools. Removing this inequality trap could enhance both equity and growth.

If we focus on a theoretical framework where the history-dependent initial conditions yield path-dependent outcomes, an obvious example in the U.S. is the history of race. A large initial wealth inequality between black slaves, indigenous natives and Latinos, and whites, combined with subsequent racial barriers for accessing credit, would justify a negative effect of inequality of opportunity (due to race) on growth under a model *à la* Galor and Zeira (1993). Alternatively, we can advocate the model proposed by Easterly and Levine (1997), which report a negative impact of ethnic heterogeneity on growth; or the model by Gradstein and Justman (2002), which emphasizes the negative effect of racial and ethnic heterogeneity on social norms that, in turn, lower the effectiveness of education on growth; or the model by Galor et al. (2009), where land concentration, which is highly correlated with the proportion of income inequality explained by race, adversely affects the implementation of human capital promoting institutions like public schooling and child labor regulations.

## 3. **Inequality of Opportunity in the U.S.**

In this section we estimate the IO in the U.S. by using depurated data of the Panel Survey Income Dynamics (PSID) database for 26 states in the 1970s, 1980s and 1990s. First, we describe the database; next, we present the method; and finally, after describing the sample design, we show the IO estimates.

### 3.1. The database

As commented in the Introduction, the data requirements for measuring IO indices are severe. We need microdata of comparable measures of individual disposable income and observed circumstances that span at least two decades and cover a large enough cross-section of countries or states. In this respect, it is worth noting the importance of having at least two observed circumstances when computing IO. Let us suppose that IO estimates reflect only one factor (i.e., race). In this case, too much *non-estimated* IO coming from omitted circumstances (i.e., parent's education) would remain in the IE component. In fact, by using only race information, estimates of IO will be close to zero for those countries or states with an overwhelmingly white population. Moreover, when including two or more circumstances, the IO estimates do not only reflect the relevance of each circumstance, but also their interactions (i.e., between race and parents' education). Individual poor opportunity may be caused by the concurrence of two or more unfavorable circumstances, instead of a single circumstance, because they reinforce each other.

While many databases satisfy some of the above restrictions, the Panel Survey of Income Dynamics (PSID) database (for U.S. states) is, to the best of our knowledge, the only exception that satisfies all of the above requirements and can be used to characterize the IO-growth relationship.[19] The PSID is a household panel which began in 1968 and is still running. The survey was conducted annually from 1968 to 1997, and then every other year. The initial sample for the PSID consists of two independent probability samples. The first one is an equal probability sample of households from the 48 contiguous states (based on a stratified multistage selection of the civilian noninstitutional population of the U.S.) drawn by the Survey Research Center (SRC); the second one is a national sample of low-income households drawn by the Survey of Economic Opportunity (SEO). The combination of both is also a probability sample, with unequal selection probabilities and, as a result, compensatory population weighting would be needed in the estimation of inequality indices. Fortunately, the PSID supplies these weights, which indicate how many persons in the U.S. population are represented by a given observation in the sample. These weights are designed to compensate for unequal selection probabilities and differential attrition.[20]

[19] Among other databases, the Survey on Income, Social Inclusion and Living Conditions in Europe (EU-SILC) database gives information on individual disposable income and circumstances for most European countries. However, this survey is valid only for 2005, since it is the only year for which information is available on parental level of education. The Integrated Public Use Microdata Series-CPS (IPUMS-CPS) database is an integrated set of data from 49 years (1962-2010) of the March Current Population Survey (CPS) in the U.S. (i.e., see King et al., 2010). Unfortunately, this database does not provide information on parent's education and it is not representative by states. The Integrated Public Use Microdata Series-USA (IPUMS-USA) database consists of a series of decennial individual-level representative samples of the U.S. for the years 1850-1880, 1900-2000, the annual American Community Surveys of 2000-2007, and the annual Puerto Rican Community Surveys of 2005-2007 (see Ruggles et al., 2010). This database represents all persons in each state but does not provide information on parents' education. Furthermore, racial categories have not been very consistent in time since non distinction was made between whites and Hispanics people before 1980. The studies in http://www.econ.umn.edu/~fperri/Cross.html, a project sponsored by the Review of the Economic Dynamics, consider databases with information on individual income; however, they do not feature information on individual circumstances such as parental education. Finally, Roemer et al. (2003), Lefranc et al. (2006b), Bourguignon et al. (2007a), Rodríguez (2008) and Cogneau and Mesplé-Somps (2009) have considered heterogeneous data with circumstances, but for a few countries and specific years.

[20] A representative sample of 2,043 Latino (Mexican, Cuban, and Puerto Rican) households was added to the PSID data in 1990. However, this sample missed out Asians, and because of this crucial shortcoming, and a lack of sufficient funding, the Latino sample was dropped after 1995. To avoid longitudinal inconsistencies, we have not considered the Latino sample in our study. For more information about the PSID database visit: http://psidonline.isr.umich.edu/Guide/.

The quality of the PSID database has been continually assessed by comparing different distributions from this database with their equivalent in other sources. For instance, Gouskova and Schoeni (2010) have compared estimates of family income between the PSID and the March Current Population Survey (CPS) for the entire history of the PSID (1968-2007).[21] They find that the distributions are in close agreement throughout the 39-year history of the PSID, above all in the range between the 5th and 95th percentiles. Therefore, the PSID database contains information on individual income and circumstances, and it is widely believed to be highly accurate. Nevertheless, a problem remains in that while the data are representative on a national level, they may not be at the state level. To minimize this problem, we have made a reasonable selection of data, states and decades, as commented below (Section 3.3.). Moreover, an extensive robustness analysis is carried out in Section 5 in order to evaluate the sample selection bias. In addition, in Section 5.5., we have replicated our main results for the IPUMS-USA database.

### 3.2. The estimation approach

Consider a finite population of discrete individuals indexed by $i \in \{1, \ldots, N\}$. As is standard in the inequality-of-opportunity literature, the individual income, $y_i$, is assumed to be a function of the amount of effort, $e_i$, and the set of circumstances, $C_i$, that the individual faces, such that $y_i = f(C_i, e_i)$. Effort is treated as a continuous variable, while, for each individual $i$, $C_i$ is a vector of $J$ elements, each element corresponding to a particular circumstance. Finally, circumstances are exogenous because they cannot be affected by individual decisions, while effort is influenced, among other factors, by circumstances. Consequently, individual income can be rewritten as $y_i = f\left[C_i, e_i(C_i)\right]$.

In order to estimate IO, the population is partitioned into a mutually exclusive and exhaustive set of types $\Gamma = \{H_1, \ldots, H_M\}$, where all individuals in each type $m$ share the same set of circumstances. That is, $H_1 \cup H_2 \cup \ldots \cup H_M = \{1, \ldots, N\}$, $H_r \cap H_s = \varnothing$, $\forall r$ and $s$, and $C_i = C_k$, $\forall i$ and $k \mid i \in H_m$ and $k \in H_m$, $\forall m$. Furthermore, let us assume that the distribution of effort exerted by individuals of type or group $m$ is $F^m$ and that $e^m(\pi)$ is the level of effort exerted by the individual at the $\pi^{th}$ quantile of that effort

[21] The CPS is the most widely used data source for cross-sectional estimates of family income in the U.S., which is the basis for the government's official estimates of income and poverty.

distribution. Given the type *m*, we can define the level of income obtained by the individual at the $\pi^{th}$ quantile as $v^m(\pi) = y^m[e^m(\pi)]$. Now, let $\pi \in [0,1]$, $v = (v^1, ..., v^M)$ be a partition of income into *M* groups, and $\bar{v} = \left(\int_0^1 v^1(\pi)\,d\pi, ..., \int_0^1 v^M(\pi)\,d\pi\right)$ be the *M*-dimensional vector of average incomes, where each element represents the expected income for each origin category or type *m*. Furthermore, let $\chi$ be the space of joint income distributions and circumstances {*y*, *C*} and $\delta$ the space of possible divisions of the population.

In order to fulfill the aim of this paper, we need to decompose overall inequality into IO and IE components. Following Moreno-Ternero (2007), Rodríguez (2008), Ferreira and Gignoux (2008) and Checchi and Peragine (2010), among others, we define $IO: \chi \times \delta \to R^+$ as $IO = I(\bar{v})$, where $I$ is an inequality index.[22] In this manner, whenever total inequality can be additively decomposed by population groups according to a set of circumstances, the IO term can be seen as a between-group inequality component, while the IE term can be interpreted as a within-group inequality component. Among all the possible inequality indices that fulfill the basic principles found in the literature on inequality,[23] only those of the Generalized Entropy class are additively decomposable into a between-group and a within-group component (Bourguignon, 1979, Shorrocks, 1980, and Cowell, 1980). Consequently, we adopt the mean logarithmic deviation or Theil 0 (*T*), because it belongs to the Generalized Entropy class, has a path-independent decomposition (Foster and Shneyerov, 2000), and uses weights based on the groups' population shares.[24] The Theil 0 index can be exactly decomposed as follows:

[22] In an early debate in the conceptual literature on equal-opportunity policies, Roemer (1993) proposed taking the minimum (across types) at each centile of the conditional distribution of income, and then averaging across centiles, in the so-called "mean of mins" approach. Alternatively, Van de Gaer (1993) proposed first averaging across centiles, and then taking the minimum across types (a "min of means" approach). Thus, Roemer's approach requires measuring income differences between types by centiles, while Van de Gaer's method only measures income differences between types at the mean.

[23] The principle of progressive transfers, symmetry, invariance to changes in scale and replication of the population (Cowell, 1995 and Sen and Foster, 1997).

[24] The path-independent property implies that the result of the decomposition is independent of the component that is eliminated first, the within-group inequality or the between-group inequality. The Theil 0 index has a value between 0 and ∞, with zero representing an equal distribution and higher values representing a higher level of inequality. For a distribution *X*, with mean $\mu_X$, the Theil 0 index is defined as:

$$T(X) = \sum_i w_i \ln \frac{\mu_X}{x_i},$$

$$T(v) = T(\bar{v}) + \sum_{m=1}^{M} p_m T(v^m), \qquad (1)$$

where $T(\bar{v})$ is the between-group component (the IO term), which is calculated by applying the Theil 0 index to the vector $\bar{v}$, and the second term is the within-group component (the IE term), which captures the income inequality within each type *m*, weighted by $p_m$, the frequency of type *m* in the population.[25] In this manner, the two sources of income differences, circumstances and effort, can be included separately in the inequality-growth regressions conducted in Section 4. For comparative purposes, we also consider the Gini index to estimate IO. However, this index does not belong to the Generalized Entropy class and, therefore, it is not additively decomposable into within-group and between-group components.[26] As we will see in Section 4, this could yield misleading results when using the Gini index.

The IO and IE components can be estimated non-parametrically (Lefranc et al., 2008, and Checchi and Peragine, 2010) and/or parametrically (Bourguignon et al., 2007a, and Ferreira and Gignoux, 2008). The convenience of using one or another approach generally depends on the available database and the issue under analysis. In this paper, we use the first approach for several reasons. First, the non-parametric method does not assume any particular functional form, while the parametric method usually assumes log-linear/linear specifications for its system of equations. Second, the parametric specification omits various possible interaction terms between circumstance and effort variables. This assumption requires that the returns-to-effort factors be orthogonal to the set of circumstances, which is an unrealistic assumption, as commented above and in Section 2. Third, the possible existence of a relevant number of unobserved circumstances and effort variables –correlated with the observed ones– may cause the

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where $w_i$ is the relative population weight of observation $x_i$.

[25] The remaining members of the General Entropy class (for example, the Theil 1 index or the square of the coefficient of variation) use weights based not only on the groups' population shares but also on the groups' income shares. These indices then would give, for two groups of the same population size, more importance to the group with higher incomes.

[26] The Gini index generally fails to decompose additively into between- and within-group components. Thus, the Gini decomposition is (see, among others, Lambert and Aronson, 1993):

$$G(v) = G(\bar{v}) + \sum_{m=1}^{M} p_m q_m G(v^m) + R,$$

where $p_m$ and $q_m$ are the population and income shares for type *m*, respectively. The first term is the between-groups Gini coefficient, the second term is the within-group component, and *R* is a residual that is zero only in the case that group income ranges do not overlap, which does not occur in our case.

residuals of the parametric regressions not to be orthogonal to the regressors. This would not be a problem if one is interested in a lower-bound estimation for the overall effect of all circumstances, as in Ferreira and Gignoux (2008).[27] However, it may be relevant for the accuracy of the estimates if one is interested in the effect of a specific observed set of circumstances on IO, as in Bourguignon et al. (2007a). Finally, the application of the non-parametric method is straightforward.[28] Nevertheless, IO and IE indices computed according to the parametric methodology proposed by Ferreira and Gignoux (2008), which is similar though less computing demanding than the methodology proposed by Bourguignon et al. (2007a), are also considered in the sensitivity analysis carried out in Section 5.3. We find that the main results are robust to the specific way in which IO estimates are generated.

Thus far, we have developed the measurement of absolute IO indices. However, Ferreira and Gignoux (2008) also propose the use of a relative IO term: the IO to total inequality ratio. The problem with the relative IO index, as these authors acknowledge, is that it depends, by construction, not only on opportunities but also on the returns-to-effort component. For example, if total inequality increases due to a higher IE component, the relative IO index would decrease, though IO has not changed. Therefore, the use of this relative index is problematic. Despite this shortcoming, and in order to check the robustness of our results to the specific way of measuring IO, we have also considered regressions using the IO ratio in Section 4.

### 3.3. The sample design

In order to estimate IO, we need to refine the PSID samples. First, we consider individuals who are household heads.[29] Correspondingly, gross income is computed as the household head's labor income plus the household capital income divided by the number of adults in the household (Roemer et al., 2003 and Rodríguez, 2008). Second, we remove the so–called composition effect: individuals with different ages are in different phases of the wage–earning time series. To do this, the common practice in the inequality–of–opportunity literature is the truncation of the samples. In particular,

[27] The true IO requires the observation of all circumstances, but this is unfeasible in practice. Consequently, the estimated IO should be interpreted as a lower bound of the true IO, while the estimated IE should be interpreted as an upper bound of the true IE (Ferreira and Gignoux, 2008).

[28] The disadvantage of the non-parametric method is that the frequency of sample observations per type tends to diminish as the number of types increases.

[29] The household head is male in married-couple families, but female or male, otherwise.

studies are usually restricted to household heads in a given age group, for example, 25 and 50 years old. However, in our case, this strategy could significantly reduce the number of available observations and, as a result, the accuracy of IO estimates. Alternatively, we just restrict the samples to household heads between 18 and 65 and, following Checchi and Peragine (2010), we regress actual gross incomes on *experience*, *experience squared* and *survey years*.[30] Then, we take residuals from this regression and, because they are centered around zero, we add a constant to match the minimum of the actual series.[31]

Third, as commented above, we consider two circumstances: the father's education and race. For the father's education, we assume four groups: no education, primary, secondary and tertiary education;[32] for race, we consider two groups: white and non-white.[33] Then, combing both circumstances, the sample is partitioned into 8 groups or types (i.e., $M$=8), and the estimated inequality-of-opportunity index is called *"IO-8group"*. Four, to neutralize possible outliers, inequality indices (overall inequality, IO and IE) in 1970, 1980 and 1990 are the average up to 2 years, that is, 1969 and 1970, 1979 and 1980, and 1989 and 1990, respectively.[34]

Finally, we disregard those states with fewer than 50 observations for each decade so as to have enough heterogeneity to estimate IO.[35] In this respect, it is worth noting that each observation in the PSID sample represents as many persons in the U.S. population as her/his weight indicates. As a result, 50 observations may represent a large proportion of the state's population. In this manner, the problem of dealing with states

[30] Due to the lack of information about actual experience, we have calculated potential experience as: age – age when finished education. The results of these partial regressions are available upon request.

[31] In a previous version of this paper (Marrero and Rodriguez, 2010) we truncated the samples to household heads between 25 and 50 years old. When comparing the results in this paper with those in the previous version, we see that they are robust to the applied sample selection rule.

[32] Information on mother's education is not available for the whole period. "No education" means 5 grades or less; "primary" education goes from 6 grades to 11; "secondary" education refers to 12 grades and 12 grades plus non-academic training; and, "tertiary" education refers to college with or without a degree. The results do not change significantly when three groups for the father's education (primary or no education; secondary education; and, tertiary education) are considered.

[33] We have split the population into white and others (instead of black and others) for two reasons. First, the history of slavery is an important one in the U.S., but the immigration of Latinos and the history of native indigenous people are also relevant. Second, by doing this we have more observations for some types, given the fact that white people are an overwhelming majority in many states. Nevertheless, we have replicated the regression analysis in Sections 4 and 5 for the black and non-black division, and the results are similar. Moreover, in Section 5.5., we consider the IPUMS-USA database, which has a larger sample size, and we apply both divisions: white and non-white, and black and non-black. For this database, IO estimates are practically equivalent.

[34] We do not average up to three years because the PSID data were subject to nonresponse (24% of the households) in 1968, the year of its implementation.

[35] The regression results in Section 4 do not vary significantly when the criterion of selection changes to 20, 30, 70 or 100 observations.

with fewer than 50 observations is not one of sample size, but rather the lack of sufficient heterogeneity in the sample to generate the different groups or types (8 in our case) to estimate IO accurately. Following this criterion, our final sample for 1970, 1980 and 1990 reduces to a set of 26 states distributed throughout the whole territory: Arkansas, California, Florida, Georgia, Illinois, Indiana, Iowa, Kentucky, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, New Jersey, New York, North Carolina, Ohio, Oregon, Pennsylvania, South Carolina, Tennessee, Texas, Virginia and Washington.

### 3.4. Inequality of opportunity in the U.S. by states

For our refined database, Table 1 shows the estimations of total inequality, the absolute IO and the relative IO (for the Gini and the Theil 0 indices) for 1970, 1980 and 1990.[36] Figures 1a, 1b and 1c show the decade averages of these three inequality measures, respectively, sorted from the highest to the lowest Gini index.

INSERT TABLE 1 ABOUT HERE

From Figure 1a, we see that the Theil 0 and the Gini reach a similar ranking for the different states, showing slight differences. It is worth noting that a direct comparison of these inequality indices with those published by the U.S. Census Bureau would be misleading. Notice first that data from the PSID were refined to estimate IO and not to estimate total inequality. Thus, Census data refer to families, while our data refer to individuals (household heads).[37] Second, our samples consider only individuals between 18 and 65 years old. Finally, we correct incomes due to the fact that individuals are at different phases of the wage–earning time series. Despite these transformations, the coefficient of correlation between our Gini and the Gini indices provided by the U.S. Census Bureau for 1970, 1980 and 1990 is 0.33 (0.39 if the composition effect is not corrected). These numbers, although smaller, are in line with other comparisons made in

[36] Due to space constraints, we have omitted from Table 1 the standard errors computed by bootstrapping (see Davison and Hinkley, 2005, and Cowell and Flachaire, 2007). The estimated standard errors for the income inequality and IO indices are rather precise. They are available from the authors upon request.
[37] As commented by Deininger and Squire (1996), variation in the definition of the variables used to measure inequality —in our case, individuals instead of families—can seriously affect the magnitude of the indicators of inequality and undermine the comparability of the data.

the literature.[38] Nonetheless, we are aware that some states, like Arkansas, Mississippi, Kentucky and Tennessee, run counter to well established –by the U.S. Census– expectations. To address this possible problem, we carry out in Section 5 an extensive robustness analysis. In particular, we run the IO-growth regressions by dropping all states in the sample one by one, with special focus on these problematic states. Moreover, we also compute the inequality and IO indices using the IPUMS-USA database, and then run the IO-growth regressions. In general, we show that the main results hold.

By comparing the income inequality and IO average results (Figures 1a, 1b and 1c), we observe substantial differences between their rankings. For example, there exists a group of states with high total inequality and rather low IO (absolute and relative), such as Massachusetts and California, while the opposite happens for states like Louisiana and Virginia. Additionally, there exist some states whose relative position remains basically unchanged, such as Oregon and Minnesota, which are at the lowest levels of both dimensions, and New Jersey, South Carolina and Florida, which are at the top of the three rankings. This finding is consistent with Figures 2.a and 2.b, which, for the entire pool of observations, show the relationship between the Theil 0 index and the estimated IO, and between IE and IO indices, respectively. Their coefficients of determination ($R^2$) are 0.45 in Figure 2.a, positive but far from unity, and only 0.21 in Figure 2.b. This result highlights how those factors affecting these two dimensions of inequality should be different. As a consequence, the impact on growth of each variable should be distinct, as commented in Section 2 and will be discussed in more detail in the next section.

Going back to Figure 1c, we observe that IO estimates represent a modest percentage of the total inequality, which is consistent with results in the literature.[39] The existence of

[38] For example, Panizza (2002) finds, for the U.S. states, a correlation of 0.44 between his Gini indices (using non refined data) computed from the annual reports, *Statistics of Income* (SOI), published by the Internal Revenue Service, and the Gini indices published by the U.S. Census Bureau. Another example that is even more revealing is the correlation found between the Gini indices computed by Deininger and Squire (1996) and the Gini indices computed by Gottschalk and Smeeding (1997) for a set of OECD countries using the same database (the LIS data set), which was 0.48.

[39] Ferreira and Gignoux (2008) find that between one fifth and one third of all income inequality is explained by opportunities in six countries in Latin America. Checchi and Peragine (2010) find that less than ten percent of all income inequality is explained by opportunities in Italy.

additional circumstances capturing differences in opportunity, other than race and parental education, could explain this result.[40]

INSERT FIGURES 1a, 1b AND 1c ABOUT HERE

Finally, we consider the temporal progression of the indices. As a general trend, we see that total inequality between the 70's and the 80's is stable, while it increases between the 80's and the 90's. Note that this progression is consistent with the information gathered from the U.S. Census Bureau. With respect to IO values, its temporal progression is less clear, and depends on each state. For example, between the 80's and 90's, IO drops significantly in Louisiana and Mississippi, while it increases considerably in Maryland, Georgia, Illinois, New Jersey, Massachusetts, Tennessee, Texas and Virginia. In all other states in the sample, IO is relatively constant between the 80's and the 90's. Accordingly, IO and IE evolve in one direction or another depending on the case. This finding is consistent with Figure 3. Looking at the entire data pool, the relationship between IO and IE annual changes is positive though small, showing a coefficient of determination of 0.35, and of just 0.15 if the two extreme values shown in the figure are omitted. Thus, a clear-cut endogenous individual behavior response of IE to IO changes (or vice-versa) cannot be inferred from this simple data analysis. Many other factors such as institutions or policy actions might be affecting the evolution of both components in one way or another. An extensive analysis of this issue deserves much more attention, which goes beyond the scope of this paper.

INSERT FIGURES 2a, 2b and 3 ABOUT HERE

## 4. Inequality, Inequality of Opportunity and Growth: An Empirical Analysis

In this section we carry out the main task of this paper, which is to characterize the effect of IO on growth. We start by presenting the results of standard linear pooled-OLS

[40] Recall that our IO estimates are interpreted as a lower bound. Another possibility is that income-based IO tends to underestimate IO because the higher measurement error and variance for transitory components in the distribution of income (in comparison with the distribution of consumption) may be effectively counted as inequality of returns to effort (see Ferreira and Gignoux, 2008).

estimate.[41] In the following section, we will show long-term cross-sectional results, standard random and fixed effects regressions, non-linearity estimates, and check for robustness to alternative specifications. As commented above, the benchmark analysis is limited to the 1970-2000 period and to a selected set of 26 U.S. states.[42] In all cases, the dependent variable is the growth rate of real personal income (adjusted by CPI), divided by total midyear population, $GY_{i,(t-s,t)}$, in the ensuing time period – i.e., ten years for the pooled-OLS, random and fixed effects regressions. Inequality indices, $INEQ_{i,t-s}$, and all other control variables, $X_{i,t-s}$, are all measured at the beginning of each decade, which help us to reduce endogeneity errors when applying standard regression techniques. The benchmark regression also includes lagged per capita real income, $Y_{i,t-s}$, which controls for conditional convergence across states.[43] Finally, we consider regional, $R_i$, and temporal, $T_t$, fixed effects.[44] The reduced-form regression is:

$$GY_{i(t-s,t)} = \alpha + \beta Y_{it-s} + \phi' INEQ_{it-s} + \delta' R_i + \varphi' T_t + \lambda' X_{it-s} + \varepsilon_{it}\,, \qquad (2)$$

where $\varepsilon_{it}$ encompasses effects of a random nature that are not considered in the model and is assumed to have the standard error component structure. The *INEQ* vector would include overall inequality, and/or alternative IE and IO measures, depending on the case considered.

[41] In the inequality–growth literature, the pioneering work of Benabou (2002) provides explicit structural equations identifying alternative sources through which inequality may affect growth. Moreover, Galor and Moav (2004) provide a unified theory with a testable implication (inequality affects growth positively in low income economies, while the impact is negative in high income economies) and Bandyopadhyay and Basu (2005) propose a calibration exercise applied to a dynamic general equilibrium model. Instead, this paper is based on an extensive sensitivity analysis of reduced-form regressions. In this manner, we try to establish some evidence on the empirical relationship between different types of inequality (IO and IE) and growth for future theoretical research.

[42] An advantage of dealing with states instead of with countries is that heterogeneity within states does not stem from the political process because, for the most part, it is similar across the different states. More importantly, institutional, cultural, religious and other differences are less intensive for U.S. states than for different countries (Partridge, 1997).

[43] As is the norm in the convergence literature, an implicit assumption is that economic growth is converging to an equilibrium path that is a function of initial conditions (Barro and Sala-i-Martin, 1991). This assumption may be important, as we will discuss to some extent in the following section.

[44] Time dummies included are those of the 70s and 80s, while the dummy for the 90s is omitted. We also use a standard and broad classification for regional variables (in parenthesis, our selected states for each region): *West* (California, Washington, Oregon and Missouri), *Midwest* (Minnesota, Iowa, Illinois, Indiana, Ohio and Michigan), *South* (Arkansas, Texas, Louisiana, Mississippi, Tennessee, Kentucky, Florida, Georgia, North and South Carolina, Virginia and Maryland) and *Northeast* (Massachusetts, New Jersey, New York and Pennsylvania). The omitted regional dummy is *Northeast*. In the pooled-OLS estimates, all models consider regional and time dummy variables, while time dummies are omitted in the long-run cross sectional estimates, and regional dummies are omitted in the fixed and random effects regressions.

We assume a 'small' and a 'base' version of (2), depending on the variables used in X. Perotti (1996), Panizza (2002) and Partridge (2005) emphasize the importance of considering this distinction. The 'small' or parsimonious version includes human capital variables, the percentage of people who live in metropolitan areas, and the percentage of the population above 65 years of age. The 'base' model accounts for human capital, industry mix, farm employment, welfare public expenditures and lag employment growth.[45] We also include the fertility rate at the beginning of the period as an additional explanatory variable in the 'base' specification.[46] Fertility has been proved to be an important channel through which initial inequality may reduce growth. Following Galor and Zhang (1997), the empirical evidence reveals that a rise in net fertility rate (Koo and Dennis, 1999 and Perotti, 1996) or higher differential fertility (de la Croix and Doepke, 2003) has a negative impact on growth. In keeping with this literature, we will show that fertility is one of the most significant variables (negative correlated) in explaining posterior growth.

The comparison between the 'small' and the 'base' models is important because inequality may affect growth not only directly, but also indirectly through other variables. A 'small' model would capture both direct and indirect effects, while a fully specified model would mainly capture the direct impact of inequality on growth. However, the 'base' model could introduce important multicollinearity problems in the regression. For these reasons, especially in a reduced-form exercise, it is convenient to show and compare results for both a fully-specified (base) model and a reduced (small) model.

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[45] As is standard in the literature, we consider three categories to measure human capital: the percentage of the population over 24 years of age who have graduated from high school but do not have a four-year college degree (*high school*); the percentage who have graduated from a four-year college (*college*); and the omitted category, which is the percentage of individuals who have not graduated from high school. To control for the initial economic sectoral mix of each state, the shares of nonagricultural employment are included for *mining*, *construction*, *manufacturing*, *transportation and public utilities*, *finance*, *insurance and real estate*, and *government*. *Traded goods and services* are the omitted sector, and thus the employment share coefficients should be interpreted as being relative to this sector. The percentage of the population who worked on a farm (*farm*) is included to account for the different importance of agriculture across states. In order to account for the possibility that growth in the previous decade could, in turn, influence growth in the following decade and be correlated with past inequality, we include the percentage change in nonagricultural employment in the preceding decade (e.g., employment growth in the 70s is used to explain per capita income growth in the 80s). Finally, welfare expenditure as a percentage of personal income is included as a proxy for the degree of distributive policy. More inequality would imply greater distribution, more welfare expenditure and that, in turn, would imply lower average growth rate (as some political economy models suggest). Hence, a negative growth–inequality relationship could appear if we omit this variable. See the Appendix for details on data sources.

[46] For the case of U.S. states, Panizza (2002) and Partridge (1997 and 2005) omit fertility in their regressions.

### 4.1 Inequality and growth: preliminary results

Tables 2.a and 2.b show the results of model (2) using our estimated Theil 0 and Gini indices, respectively. For each panel in the tables, the first column shows results for the 'small' model and the second one for the 'base' model. Although it is not the purpose of the paper, we start by commenting on the results for the total inequality–growth relationship (first panel in the tables). In all cases, we find a positive relationship between total inequality and per capita income growth.[47] However, the significance of this positive relationship is fragile to the sample used, which is a poor conclusion and undermines the effectiveness of general inequality policies on the economy.[48] We will test whether this result is due to the non-distinction between income inequality, IO and IE.

INSERT TABLES 2a and 2b ABOUT HERE

To conclude this preliminary analysis, we briefly discuss the estimates for the relationship between initial control variables and posterior decade income growth. The results are fairly robust and in line with the literature. For instance, the negative coefficient for lagged per capita income reflects conditional convergence, and its magnitude is in accordance with the pioneering work of Barro and Sala-i-Martin (1991). Future economic growth is expected to be positively correlated with the labor force's human capital. As in Partridge (1997 and 2005) and Panizza (2002), the relevant variable of education is *college*, which is highly positive and significant with respect to the omitted category (non-graduated). The coefficient of *high school* on growth is not significant for the small model, while it is negative and significant for the base model.[49] The coefficients on most of the initial industrial mix variables are negative, though

[47] Using the 'small' and 'base' specifications, we have compared our estimates with those obtained using the Gini index from the U.S. Census Bureau, which is the inequality measure used in Partridge (1997 and 2005). Using the Gini from the Census, for the entire sample (48 states and the 1960-2000 period) and for our reduced sample (26 states and the 1970-2000 period), the estimated coefficients of inequality are 106.44 and 71.05 and significant at the 1% and 10% levels for the "small" and "base" model, respectively. These coefficients are higher than our estimates, but they share the same sign. With respect to the other explanatory variables (lagged income, high school, etc.), the results are also similar.

[48] In a previous version of this paper (Marrero and Rodriguez, 2010), we focused only on the 80s and 90s, and the relationship was positive but non-significant. In this respect, it is worth to recall the unstable relationship between inequality and growth found by Panizza (2002).

[49] Perhaps, the base model is too parameterized, and as a result, there is some colinearity that is affecting the sign of the high school coefficient. In this respect, note that the coefficient for high school turns positive, though not significant, when fixed and random effects are used (not show in the paper). Moreover, for U.S. states, Partridge (1997 and 2005) and Panizza (2002) find that the sign of the high school variable is negative in some specifications. Finally, the coefficient for high school is positive and significant when the college variable is ommited from the regression.

basically only those of the *construction, manufacturing and finance* sectors are significant. Thus, states with greater initial shares in *services and traded goods* (the omitted category) have experienced higher economic growth, in general. The estimates for the *farm* variable are negative and no significant, in general, and a similar result is found for estimates of the labor growth in the preceding year and the percentage of welfare public expenditure to personal income. Finally, the relationship between initial *fertility* and subsequent growth is always negative and significant at a 1% level, which is in line with the related literature, as commented above. This relationship is strongly robust to all the specifications considered. Because fertility is an important channel through which inequality can affect growth, its inclusion in the model is crucial to measure properly measuring the direct relationship between overall inequality, IO, IE and growth. As we will see below, after controlling for fertility, the coefficients for IE and IO remain generally positive and negative, respectively. As a result, there would exist, apart from the fertility channel, additional ways through which IO and IE might affect growth.

### 4.2. Inequality of opportunity and growth

The aim of this section is to estimate the impact of IO on the long-term average growth of per capita income. We consider different ways to include IO into the INEQ vector in (2). The easiest way is to include IO together with total inequality (second panel in Tables 2a and 2b). When including the IO term, we control for the observed circumstances, i.e. the father's education and race. As a result, total inequality would now more clearly reflect the IE component and its coefficient should then be higher and more significant than before (compare the row for total inequality in Panel 1 and 2 in Tables 2a and 2b). Moreover, IO coefficients are always negative and, with the exception of the "small" model for the Gini index, they are always significant at the 1% and 5% levels.

However, IO and total inequality can be highly correlated, because IO is a part of total inequality, what can affect regression results. Alternatively, as proposed by Ferreira and Gignoux (2008), we can consider the IO to total inequality ratio in the regression (third panel in Tables 2a and 2b). We see that the main results remain unchanged. In particular, total inequality coefficients are always positive and highly significant, while those of the IO components are always negative and significant (the exception is again the 'small' model for the Gini coefficient).[50] However, as commented in section 3.2., the IO to total inequality ratio index depends, by construction, not only on opportunities but also on the returns-to-effort component.

For this reason, the third alternative (fourth panel in Tables 2a and 2b) is probably the most interesting one: to use the decomposition of total inequality into a between-group component (the absolute IO term) and a within-group component (the absolute IE term), as described in Section 3.2. In this manner, we isolate the effect of each component on growth. For the Theil 0 index (Table 2a), the IE coefficients are always positive and significant, and the IO coefficients are always negative and significant. However, for the Gini coefficient (Table 2b), the IE coefficients are always positive and significant, but the IO coefficients turn out positive, although only significant for the base model. As discussed in Section 3, the IO-IE decomposition is only exact for the Theil 0 case and, consequently, the decomposition of the Gini index may lead to misleading results. As a result, we will focus only on the Theil 0 decomposition from now on. In fact, in order to reduce the number of tables in the next section, we will just show results for the IO-IE decomposition.[51]

Therefore, the impact of the IO component is, in general, significantly negative and the impact of the IE component is, in general, significantly positive.[52] This result is especially robust for the Theil 0 case, which, as discussed in Section 3, is the most convenient way to decompose total inequality into their IO and IE components. While IO might be harmful for growth because it can reduce the access of individuals with lower opportunities to profitable investment plans, differential rewards to different levels of effort may have a positive effect on growth by encouraging individuals to invest in human capital and effort. This finding suggests that the overall impact of total

[50] Note that coefficients for IO in panel 2 and panel 3 are not comparable to each other because in panel 2 the IO term is a level, while it is a ratio in panel 3.

[51] The results in Section 5 are quite robust to the IO specification used, including the results for the Gini decomposition.

[52] Note that the IO component might be picking up the proportion of poor individuals in each state, because the latter variable could have a negative correlation with income growth. We acknowledge this observation to an anonymous referee. To check for this possibility, we have compared the IO coefficients with and without the proportion of poor individuals in each state (the poverty rate as provided by the U.S. annual census data base) in the 'base' model. The negative coefficient (and its significance) prevails for IO. Moreover, the poverty coefficients are negative in most cases and significant in some cases. Thus, although poverty and IO must be related, one does not exclude the other in explaining economic growth performance. They seem to capture different channels through which growth is negatively affected. Nevertheless, a fully understanding of the differences regarding how poverty and IO can affect growth deserves further analysis that goes beyond the scope of this paper, though it clearly constitutes a promising extension of the paper.

inequality on growth can be positive, negative or zero depending on which of the above two origins of income inequality (opportunity or effort) dominates in data. Accordingly, policies that equalize opportunity and promote individual effort will enhance growth.

The important implications of this result and the potential weakness of reduced-form regressions make mandatory the need to carry out an extensive sensitivity analysis of our results in several directions: ‘the assumption of convergence’ (i.e., how the $Y_{i,(t-s)}$ term is included in (2)); the econometric method used; the presence of non-linearities; the estates considered; the control variables included; the set of circumstances considered; the database used; and, some others. These analyses are performed in Section 5.

## 5. Robustness and sensitivity analysis

Durlauf and Quah (1999), Panizza (2002), Partridge (1997 and 2005), and Barro (2002), among many others, have emphasized the importance of including a sensitivity analysis to show how robust the findings of a reduced form regression are across alternative econometric techniques or model specifications. We address these concerns and include an extensive sensitivity analysis in order to supplement our reduced form regression analysis. Throughout this Section, in order to simplify the exposition of the results, we restrict the analysis to the IO-IE Theil 0 decomposition. Moreover, we just show estimates for the inequality coefficients and, in some cases, also for those of the income lag. Results for all other control variables are quite robust to these alternative specifications.

### 5.1. Income lag and econometric methods

An important check concerns the way the income term, $Y_{i,t-s}$, is included in regression (2). In neoclassical growth models, this term proxies the initial level of development. If all states are converging toward their own long-term equilibrium path, the $\beta$ coefficient in (2) would be negative, and all other variables would determine the steady-state level of real income. However, as applied to the U.S. states, several papers question the convergence process, or simply emphasize that states are quite close to their steady-states (Durfau and Quah, 1999, and Evans and Karras, 1996, among others). In this case, transitory cyclical conditions would dominate, and, whenever initial income is included, instrumental-variable approaches must be used or, alternatively, the income term must be omitted (Durfau and Quah, 1999). In addition, Panizza (2002) emphasizes that estimate might change depending on whether the level of income or its log is used. In keeping with Partridge (2005), who properly address these concerns, we consider five alternative models depending on the income term. Table 3 shows the results for the five income-lag versions (in columns): the first column shows results for the regression, including the lag-level of income (the benchmark specification), and the second column considers the lag-log of income; columns three and four account for the possible cyclical endogeneity problem (also for the level and the log case).[53] Finally, the fifth column reports results for the case in which the income lag variable is omitted (i.e., U.S. states are assumed to be in their balanced growth path). For all these possibilities, for the 'small' and 'base' model, we present (in rows) pooled-OLS, long-run cross-sectional, random and fixed effects estimates for the relationship between IO and growth, and check the robustness of the results.

INSERT TABLE 3 ABOUT HERE

In general, the results are highly robust to all of these alternative analyses: IE coefficients are positive and significant, while IO coefficients are negative and highly significant. Although some differences are observed between alternative specifications and econometric methods, they are in line with the theory and predictions outlined by other papers, and do not affect the policy and economic implications of the results of Table 2a. For example, for the pooled-OLS regression, IE and IO coefficients are similar whether the "base" model or the "small" model is used: they are always positive and negative, respectively, and significant at the 5% level for the 'small' model and at the 1% level for the 'base' model. Hence, main conclusions are similar regardless of which income lag variable is used. In particular, IE coefficients are more positive in columns 1 and 3, while IO coefficients are more negative in columns 3 and 4. Moreover, the income terms in columns 3 and 4 are smaller in magnitude, which is consistent with the expected negative bias when using $Y_{t-s}$ or its lag (Banerjee and

[53] As in Banerjee and Duflo (2000), we lag in both cases the income term one more period, which is a kind of simple instrumental variable estimator. A practical advantage of this alternative is that it does not introduce the dynamic panel data model bias in the first stage, when instrumenting $Y_{t-s}$ by their lagged terms (Partridge, 2005, footnote 15). Note that using lags not too far in past (5 or 10 years) also control for the level of initial development (Li and Zou, 1998). In fact, they are instrumental variable estimators when the cyclical component is correlated with $Y_{t-s}$ (Banerjee and Duflo, 2000, and Partridge, 2005).

Duflo, 2000, and Partridge, 2005). Thus, it seems that the possible endogeneity cyclical bias modestly affects our results.

As said above, we complement the pooled-OLS estimates with the results of the long-term cross-sectional model, and the Random Effects (RE) and Fixed Effects (FE) estimates. When variables mostly vary cross-sectionally, which is generally the case of income inequality, pooled-OLS and RE estimates would likely reflect long-term effects. The advantage of the long-term cross-sectional model is that cyclical effects are avoided, that is, only long-run responses are reflected. The inconvenience of this model is the resulting reduction in the number of degrees of freedom, which leads to less accurate estimations. For the 'small' and 'base' models, IE and IO coefficients are always positive and negative, respectively. IE coefficients are significant only for the 'base' model, while IO coefficients are significant regardless of the dimension of vector X. As in Partridge (1997 and 2005), coefficients are higher in the long-term cross-sectional regressions than in the pooled-OLS regressions, though their similarities suggest that both are mainly reflecting similar persistent growth-IO and growth-IE effects.

The RE model is a possible solution for the omitted variable bias in OLS models. But, because the RE approach uses cross-section variations, its results are expected to be similar to those of pooled-OLS regressions. Indeed, as is shown in Table 3, the IE and IO coefficients are always positive and negative, respectively, and except for the model that does not contain the lagged income term (column 5), they are, in general, significant at the 5% level.

The FE results are shown in the last two sets of rows in Table 3. Mairesee (1990), Durlauf and Quah (1999) and Forbes (2000), among others, emphasize that transitory and short-term changes are more relevant across time than across cross-sections, and because FE estimates basically capture the former types of changes, their estimates can be better interpreted as short/medium-term effects. With respect to the pooled-OLS method, the FE procedure eliminates the omitted variable bias when an unmeasured time-invariant factor is correlated with explanatory variables. However, regarding the FE approach, Banerjee and Duflo (2000), Barro (2000), Hsiao (1986) and Partridge (2005), among others, are aware of the important measurement-error problems that can bias estimates more than the pooled-OLS approach. The problem is that basically only within-state variability is used and, hence, FE estimates may produce inaccurate results for variables that mostly vary cross-sectionally.[54]

To check for this problem, Panizza (2002) suggests computing the proportion of inequality variability that is explained by time and regional variables by regressing inequality with respect to time and regional dummies. A high $R^2$ would reflect that there is a limited within-state and within-decade variability, which would lead to unstable FE estimates, as is the case in Panizza's and Partridge's results.[55] For our Theil 0, IE and IO estimates, the associated $R^2$ are 0.38, 0.50 and 0.18, respectively, which are clearly smaller than the 0.76 found for the Gini index in Panizza (2002). This better performance of FE regressions using the IO-IE decomposition is reflected by the robustness of our results regardless of the lagged income variable used.[56] In particular, IE and IO coefficients are always positive and negative, respectively, and, in general, significant at the 5% level.

### 5.2. Non-linearities

Linear regressions are motivated by the business-as-usual analysis made in the inequality-growth literature. However, in these type of studies, Benabou (2002), Panizza (2002) and Banerjee and Dufflo (2003), among others, have emphasized the importance of dealing with non-linearities. Considering the findings of these papers, we now test for non-linearities in the relationship between IO, IE and growth. Table 4 summarizes the results for the pooled-OLS, the long-term cross-sectional, the RE and the FE regressions for the level of lag income (our benchmark). While non-parametric estimates are beyond the scope of this paper, we augment equation (2) with a quadratic

[54] As in the case in Partridge (1997), especially for the Gini index from the U.S. Census, as emphasized by Panizza (2002).
[55] As emphasized by Panizza (2002), in Section 3.2., between 1960-1980, the state and decade dummies explain 86% of the variance of the Gini index in Partridge's data set (Census Data), while for his data set (the IRS data), the state and decade dummies explained less than 55% of the variance in the Gini index. These percentages are slightly reduced when 1990 is included in the analysis.
[56] The similarity of the results for models that either lag or omit the income term also suggests that the dynamic panel data bias is not strongly affecting the results (Partridge, 2005). This bias appears in models that include the income lag term, while it is not evident in models not including this term. In any case, we cannot apply GMM-based estimates because our time dimension is three, and a minimum of four is required (Arellano and Bond, 1991).

term for IE (first column in the table), a quadratic term for IO (second column in the table) and both quadratic terms (third column in the table).[57]

INSERT TABLE 4 ABOUT HERE

Although our analysis is still far from yielding a robust theory on the real relationship between IE, IO and growth, our empirical findings would support the following ideas. In the case of IO, its quadratic term is highly negative and, in most cases, significant at the 5% or 10% levels. Moreover, its linear term is always negative and significant when its quadratic term is omitted, while, when the quadratic term is included, it is close to zero, non-significant and its sign changes from negative to positive depending on the model specification. In the case of IE, results support the existence of an inverted U-shaped relationship: the linear term is, in general, positive and its quadratic term is negative. When both the linear and quadratic IO and IE terms are included in the model (third panel of columns in Table 4), similar results are found. Note, however, that the inclusion of both, linear and quadratic, terms may lead to important multicollinearity problems, which make estimates less significant than when both quadratic terms are not included in the regressions.

Just for illustrative purposes, we summarize in Figure 4 our findings. The relationship between IO and growth is negative and concave, while the relationship between IE and growth shows an inverted U-shaped curve. In this manner, both curves are consistent with an inverted U-shaped relationship between real per capita income and total inequality, as pointed out in Banerjee and Dufflo (1999), though it is clear that further research on this important issue, beyond the scope of this paper, is needed.

### 5.3. Further sensitivity analysis

A further sensitivity analysis is shown next. First, we run the regression in (2) by dropping one state at a time. In particular, we show in Table 5 the results when dropping those states that could be problematic, i.e., Arkansas, Mississippi, Kentucky and Tennessee (recall from Section 3.4.). We obtain that IO coefficients are basically the same, and their significance also remain unchanged. Possible measurement errors in their IO and IE estimates thus seem not to be affecting our regression results.

[57] To simplify the exposition, we do not show results for the remaining lag income variables, though they are quite similar. A cubic term was also considered, but it was always non-significant and it affected the significance of the linear and quadratic terms (i.e., it introduced more collinearity).

Second, we estimate the change in correlations when decade dummies are not included in the model. On one hand, the inclusion of time dummies can exacerbate the multicollinearity problem, especially for the FE estimation. Moreover, dropping time dummies implies that part of the fixed time-variant effect is now captured by temporal progression of IO and IE. On the other hand, the exclusion of time dummies can generate an omitted variable bias. In general, we observe that IO and IE estimates are similar, although more significant when time dummies are dropped from the regression. Third, we run regressions by dropping the 70's. Recall from footnote 48 (Section 4.1.) that dropping this decade weakened the correlation between overall inequality and growth. Nevertheless, the results for IE and IO are basically unchanged: their coefficients are positive and negative, respectively, although less significant in some cases.

INSERT TABLES 5, 6 AND 7 ABOUT HERE

Four, we run the regression in (2) for the 'base' model by dropping one control variable at a time. Significant changes in the IO and IE coefficients would indicate that the excluded variable is an important –indirect– channel through which these inequalities are affecting growth. In this exercise, we find that the most important variables are *fertility* and *college*, while the results remain basically unchanged when *welfare expenditure* and *industry mix* are dropped. Table 6 shows IE and IO estimates for selected specifications of model in (2) when these four variables are dropped.[58] Time and state dummies are included in all cases. By excluding fertility (first two rows in the table), we obtain, in general, more negative IO and less positive IE coefficients. Moreover, they tend to be more significant. These results would imply that fertility is an important indirect and negative channel through which IO and IE might affect growth, though it is not the only one. By excluding the college variable, we observe that the IO coefficients also become more negative and more significant, while changes in the IE coefficient depend on the econometric method. For example, it turns more positive for pooled-OLS, RE and FE, while it becomes even more negative for the long-term cross-sectional regression. However, excluding welfare expenditure basically has no influence on IO and IE estimates. A possible explanation for this result is that the impact of welfare expenditures on growth and its correlation with IO and IE depend on the

[58] IO and IE estimates remain basically unchanged when all other variables in the base model are dropped.

financing scheme and the composition of total public expenditure. Thus, a more precise analysis of this indirect channel would require considering the tax mix and the public expenditure composition as well, which goes beyond the scope of this paper. Finally, we see in Table 6 that excluding the industry mix basically keeps the estimated IO and IE coefficients unchanged.

As commented in Section 3.2., an alternative way to estimate the between-group and within-group components is to use a parametric approach. Here we apply the procedure proposed in Ferreira et al. (2008).[59] For illustrative purposes, we just show in Table 7 the IO and IE coefficients for the 'small' and 'base' models, the income-lag and log-income-lag specifications, and the pooled-OLS and FE procedures. For the pooled-OLS regressions, IE coefficients are always positive and significant, while IO coefficients are negative though non-significant for the 'base' specification. For the FE regressions, IE and IO coefficients show the expected signs and are always significant (in some cases at the 1% level).

### 5.4. The role of circumstances

As commented in Section 3, although race is a very important circumstance for the case of the U.S., it is especially appealing when combined with other circumstances (in our case, parents' education). Nevertheless, and for the sake of robustness, we also estimate IO using separately the circumstances of race (*IO-race*) and father's education (*IO-edu*). In the first case, to avoid zero estimates for those states with an overwhelmingly white population, we have imposed the following two criteria: white people must represent less than 95% of the state's population for 1970, 1980 and 1990; and, black people must represent at least 5% of the state's population for 1970, 1980 and 1990 (according to the statistics of the U.S. Census Bureau). This criterion ensures that we only work with those states that possess enough racial heterogeneity. Table A.1. in the Appendix shows the IO-race and IO-edu indices for our selected U.S. states in 1970, 1980 and 1990. We observe that both circumstances are relevant for explaining differences in opportunity. For example, on average for 1970, 1980 and 1990, the IO-race measure is higher than the IO-edu for about a half of the states, while it is smaller in practically the other half.

[59] For the 26 states, and the three decades considered, the correlation between the IO estimates computed in Section 3.4 and those calculated using this parametric approach is 0.74. Moreover, the ranking among the states is basically unchanged. The higher dispersion of non-parametric estimates may be the reason why IO coefficients are more significant when computed with the non-parametric procedure.

Moreover, their levels are significantly lower than those presented in Table 1 when both circumstances were considered.

Table 8 shows the pooled-OLS, long-run cross-sectional, RE and FE estimations for the level and the log of the income lag, when only one circumstance is considered (race or father's education). In general, qualitative results found in Section 5.1 are still valid, although they are now less significant. This is an expected result, since, in Section 5.1., we used the information of both circumstances and, therefore, IO were more accurately estimated. With only a few exceptions, IE coefficients are positive and IO coefficients are negative, regardless of the econometric method used or the way the income lag term is included in the model.

INSERT TABLE 8 ABOUT HERE

Lastly, it is worth noting that peculiar non-linearity effects of the educational structure on growth might lead to erroneous conclusions when using father's education as the unique circumstance.[60] Suppose that the explicative variables that measure the average skills of the labor force do not completely capture the effect of education upon growth. Then, it is possible that the estimated IO term, which relies on the distribution of people among four educational groups, is actually capturing part of the effect that education may have on growth. This fact might cause that the estimated impact of IO would be misleading. However, we have found that the negative impact of IO on growth cannot be completely ascribed to father's education because inequality coming from race differences has also a negative and significant impact on growth. Therefore, even if the proposed alternative channel through which education may affect growth is true, there is still room for a negative and significant impact of IO on growth.

### 5.5. The IPUMS-USA database

In this section we estimate IO using the IPUMS-USA database and consider race as the unique available circumstance. As a consequence, we apply the selection criteria commented in point 5.4. (i.e., states whose population is less than 95% white and more than 5% black for 1970, 1980 and 1990) to guarantee a minimum of racial heterogeneity. Taking the sample used in Section 3.4 as a point of reference, we exclude

[60] We are grateful for this suggestion from François Bourguignon.

Iowa, Minnesota, Oregon and Washington, and include Alabama, Connecticut, Kansas, Nevada and Oklahoma. Therefore, we work with a sample of 27 states.[61]

Given the large size of the IPUMS-USA samples, we consider three alternative race divisions: i) white and others (as in Section 3.4 and 5.4 for the PSID); ii) black and others, and iii) white, black and others, a 3-group division. The associated IO estimates are named IO-ipums1, IO-ipums2 and IO-ipums3, respectively. The third panel of Table A.1 shows total inequality and IO estimates (Theil 0 index) for these divisions in 1970, 1980 and 1990. The cross-correlation between IO-ipums1 and IO-ipums2 is 0.9767, while the cross-correlation between IO-ipums2 and IO ipums3 is 0.9977. Thus, the results are highly robust to the race division considered.

INSERT TABLE 9 ABOUT HERE

In Table 9 we show the IO and IE coefficients for the 'small' and 'base' models, the income-lag and log-income-lag specifications, the four alternative econometric methods considered in the paper, and these three alternative divisions. The results are not as robust as those shown in Table 3 (Section 5.1.), but only a few cases are contradictory. For example, for the pooled-OLS regression and the 'small' model, the IO and IE coefficients are always negative and positive, respectively, and they are generally significant at the 5% or 10% level. A similar result is found for the long-term cross-sectional regression and the 'base' model. For the RE and FE methods, the signs of the coefficients are as expected in most cases and they are significant for some specifications. However, we find some controversial results. For example, for the pooled-OLS regression and 'base' model, the IO coefficient is positive and significant when using IO-ipums1 and IO-ipums2 (for the income-lag specifications); when using IO-ipums3 (for the income-lag specification), its sign is also positive but non-significant. In a few other cases, IO and IE coefficients switch their signs, but they are never significant.

Most of these controversies can be resolved (i.e., the signs of the IO and IE coefficients turn out to be negative and positive, respectively) if a relevant control variable such as fertility (recall Section 5.3.) is excluded from the base model regression. By way of

[61] Due to the difficulties of collecting the entire set of regressors used in Section 4 for Delaware, the District of Columbia, Hawaii and Alaska, we do not analyze these states. Moreover, they are small and/or anomalous states, and their inclusion in the regression analysis might strongly bias the estimates.

illustration, in the last rows of Table 9, we show the results when fertility is dropped in the base model for pooled-OLS, RE and FE regressions. For all cases, IO coefficients become more negative and more significant. These results are consistent with the discussion in Section 5.3., which pointed out the relevance of fertility as an indirect channel through which IO (now IO because of race) can affect growth. In addition, to understand the more unstable results when using the IPUMS-USA database, we can return to the arguments made at the end of Section 5.1. In particular, the IO and IE indices from the IPUMS-USA data show a smaller within-state and within-decade variability than those indices estimated in Section 3.4 from the PSID data.[62] Thus, the proportion of inequality variability that is explained by time and regional dummies is relatively high, which might lead to unstable estimate results.

## 6. Concluding Remarks

Models exploring the incidence of income inequality upon economic growth do not reach a clear-cut conclusion. We postulate in this paper that one possible reason for this inconclusiveness is that income inequality indices are indeed measuring at least two different sorts of inequality: inequality of opportunity and inequality of effort. Though this distinction has already been emphasized in the inequality-of-opportunity literature, it has not yet been considered in the growth literature. In this manner, the present paper contributes to the literature on the relation between inequality and economic growth by incorporating the notion of inequality of opportunity in macro studies.

Using refined data of the PSID database for 26 U.S. states in 1970, 1980 and 1990, and applying pooled-OLS, long-run cross-sectional, random effects and fixed effects regressions we find robust support for a negative relationship between inequality of opportunity and growth, and a positive relationship between inequality of effort and growth. Hence, these two types of inequalities are affecting growth through opposite channels. On one hand, inequality of effort increases growth because it may encourage people to invest in education and to exert effort. On the other hand, inequality of opportunity decreases growth because it may not favor human capital accumulation of

[62] The regression of IO indices with respect time and regional dummies presents a $R^2$ of 0.37 for IO-ipums1, 0.40 for IO-ipums2 and 0.39 for IO-ipums3, while the $R^2$ for IO-8group of the PSID was only 0.18.

the more talented individuals. In fact, Van de Gaer et al. (2001) have pointed out that inequality of opportunity reduces the role that talent plays in competing for a position by worsening intergenerational mobility. Moreover, our findings would provide support for a general theoretical prediction of models with multiple steady-states and borrowing constraints. In this context, people with initial adverse circumstances would be likely exposed to barriers for accessing credit or education, independently of their talent or effort, which would undermine subsequent economic growth.

Making a distinction between inequality of income and inequality of opportunity can throw some light upon several intriguing empirical facts in the growth literature. Two examples are pointed out. Barro (2000) shows a positive relationship between growth and inequality within most developed countries, while this relationship is negative when looking at the poorest countries. He proposes, as a tentative explanation, the different role of capital markets. In particular, he considers that problems of information (moral-hazard and repayment enforcement problems) are larger in poor countries because they have less-developed credit markets. However, he does not find empirical evidence for this different role of capital markets. An alternative explanation that would arise from the present paper is that differences in opportunity are more important within less-developed countries. At this respect, some evidence is found in the inequality-of-opportunity literature as in Ferreira and Gignoux (2008), Rodríguez (2008), Cogneau and Mesplé-Somps (2009), and Checchi and Peragine (2010).

Secondly, some empirical studies have found that the effect of income inequality on growth is sensitive to the inclusion of some variables like regional dummy variables (Birdall et al., 1995). However, the relationship between initial land inequality and growth is negative and robust to the introduction of regional dummies and other explicative variables (Deininger and Squire, 1998). Our proposal offers an easy explanation for this empirical fact. Income inequality comes not only from unequal opportunities but also from different levels of effort. As a result, the effect of income inequality upon growth can have a different sign depending on the kind of controls that are introduced in the regressions. However, initial land inequality comes from unequal opportunities and has a clear-cut negative effect upon growth. In this respect, it is worth noting that Galor et al. (2009) have recently proposed a model for analyzing the effect that inequality in land ownership has on the accumulation of human capital. In their case, land inequality adversely affects the implementation of human capital promoting institutions like public schooling and child labor regulations. In this manner, land inequality contributes to the emergence of the Great Divergence in per capita income across countries.

It is clear from the discussion above that general redistributive policies may discourage unobservable effort borne by agents and, in this manner, decrease growth. On the contrary, policies that equalize opportunity for the acquisition of attributes necessary to compete for jobs and careers may promote not only equity, but also economic growth. One natural proposal for equalizing opportunity is the so-called ‘affirmative action’. For instance, Roemer (1998) proposes spending more educational resources, per capita, on children from disadvantaged groups. In the same vein, Bourguignon et al. (2007b) propose interventions focusing on the disadvantaged groups. Among other suggestions, they propose cash transfers conditional on specific behaviors, such as school attendance; interventions to increase learning rates at public schools; health interventions to increase basic knowledge of nutrition and hygiene; and, promotion of sports and arts to reduce the appeal of violence. Further research concerning these issues is clearly needed. However, we believe that a complete understanding of the relationship between inequality and growth requires more effort in constructing appropriated databases that properly represent social origins.

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## TABLES:

**Table 1.** Inequality of income and inequality of opportunity (8 groups) in 1970, 1980 and 1990.

| | | | | **Total Inequality** | | | | | | **IO (8 groups)[a]** | | | | | | **IO to Total Inequality Ratio[b]** | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **Observations** | | | **Gini** | | | **Theil 0** | | | **Gini** | | | **Theil 0** | | | **Gini** | | | **Theil 0** | | |
| **State** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** |
| Arkansas | 88 | 85 | 100 | 0.2355 | 0.2278 | 0.2215 | 0.0916 | 0.0894 | 0.0819 | 0.0905 | 0.0825 | 0.0584 | 0.0172 | 0.0157 | 0.0100 | 38.4 | 36.2 | 26.4 | 18.8 | 17.7 | 12.3 |
| California | 353 | 438 | 434 | 0.2365 | 0.2492 | 0.3280 | 0.0945 | 0.1046 | 0.1854 | 0.0505 | 0.0418 | 0.0487 | 0.0068 | 0.0046 | 0.0061 | 21.4 | 16.8 | 14.9 | 7.2 | 4.4 | 3.3 |
| Florida | 86 | 135 | 176 | 0.2772 | 0.2936 | 0.3556 | 0.1286 | 0.1464 | 0.2046 | 0.1395 | 0.1333 | 0.0965 | 0.0384 | 0.0341 | 0.0280 | 50.3 | 45.4 | 27.2 | 29.9 | 23.3 | 13.7 |
| Georgia | 91 | 129 | 144 | 0.2176 | 0.2406 | 0.2748 | 0.0798 | 0.1064 | 0.1278 | 0.0841 | 0.0924 | 0.1388 | 0.0176 | 0.0201 | 0.0311 | 38.7 | 38.4 | 50.5 | 22.0 | 18.9 | 24.3 |
| Illinois | 120 | 158 | 147 | 0.2247 | 0.2416 | 0.2888 | 0.0813 | 0.0966 | 0.1480 | 0.0571 | 0.0427 | 0.0759 | 0.0079 | 0.0045 | 0.0215 | 25.4 | 17.7 | 26.3 | 9.8 | 4.7 | 14.6 |
| Indiana | 84 | 133 | 130 | 0.1736 | 0.2109 | 0.2664 | 0.0553 | 0.0767 | 0.1207 | 0.0377 | 0.0510 | 0.0829 | 0.0031 | 0.0055 | 0.0143 | 21.8 | 24.2 | 31.1 | 5.7 | 7.1 | 11.9 |
| Iowa | 59 | 89 | 89 | 0.2356 | 0.2480 | 0.3128 | 0.0901 | 0.1025 | 0.1681 | 0.0373 | 0.0598 | 0.0428 | 0.0056 | 0.0129 | 0.0160 | 15.8 | 24.1 | 13.7 | 6.2 | 12.6 | 9.5 |
| Kentucky | 77 | 85 | 97 | 0.1992 | 0.1951 | 0.2519 | 0.0692 | 0.0653 | 0.1062 | 0.0339 | 0.0743 | 0.0921 | 0.0036 | 0.0097 | 0.0163 | 17.1 | 38.1 | 36.6 | 5.2 | 14.9 | 15.3 |
| Louisiana | 79 | 119 | 94 | 0.1914 | 0.2739 | 0.2428 | 0.0587 | 0.1251 | 0.0981 | 0.0841 | 0.1835 | 0.1065 | 0.0156 | 0.0578 | 0.0191 | 43.9 | 66.9 | 43.9 | 26.6 | 46.2 | 19.5 |
| Maryland | 111 | 168 | 211 | 0.1812 | 0.2451 | 0.3608 | 0.0549 | 0.1062 | 0.2355 | 0.1102 | 0.1044 | 0.1933 | 0.0206 | 0.0184 | 0.0590 | 60.8 | 42.6 | 53.6 | 37.6 | 17.3 | 25.0 |
| Massachusetts | 74 | 98 | 124 | 0.2122 | 0.2681 | 0.3795 | 0.0780 | 0.1289 | 0.2668 | 0.0434 | 0.0220 | 0.0902 | 0.0077 | 0.0020 | 0.0244 | 20.5 | 8.2 | 23.8 | 9.9 | 1.6 | 9.1 |
| Michigan | 177 | 220 | 245 | 0.2433 | 0.2763 | 0.3498 | 0.1031 | 0.1368 | 0.2094 | 0.0895 | 0.1088 | 0.1281 | 0.0175 | 0.0299 | 0.0350 | 36.8 | 39.4 | 36.6 | 17.1 | 21.9 | 16.7 |
| Minnesota | 58 | 74 | 88 | 0.1949 | 0.2300 | 0.2630 | 0.0712 | 0.0946 | 0.1223 | 0.0474 | 0.0388 | 0.0382 | 0.0069 | 0.0079 | 0.0066 | 24.4 | 16.9 | 14.6 | 9.8 | 8.4 | 5.5 |
| Mississippi | 101 | 195 | 204 | 0.2480 | 0.2117 | 0.2317 | 0.0987 | 0.0782 | 0.0885 | 0.1269 | 0.0842 | 0.0731 | 0.0309 | 0.0205 | 0.0097 | 51.2 | 39.8 | 31.6 | 31.3 | 26.3 | 11.0 |
| Missouri | 134 | 140 | 143 | 0.2112 | 0.2275 | 0.2673 | 0.0736 | 0.0880 | 0.1180 | 0.0287 | 0.0409 | 0.0493 | 0.0019 | 0.0077 | 0.0056 | 13.6 | 18.0 | 18.5 | 2.7 | 8.8 | 4.8 |
| New Jersey | 92 | 116 | 142 | 0.2361 | 0.2957 | 0.4785 | 0.0911 | 0.1488 | 0.4078 | 0.0728 | 0.1026 | 0.2211 | 0.0103 | 0.0285 | 0.0841 | 30.8 | 34.7 | 46.2 | 11.3 | 19.2 | 20.6 |
| New York | 193 | 217 | 228 | 0.2234 | 0.2695 | 0.2837 | 0.0849 | 0.1196 | 0.1416 | 0.0648 | 0.0624 | 0.0620 | 0.0107 | 0.0080 | 0.0084 | 29.0 | 23.2 | 21.9 | 12.6 | 6.7 | 5.9 |
| N. Carolina | 151 | 209 | 272 | 0.2299 | 0.2523 | 0.2965 | 0.0881 | 0.1074 | 0.1455 | 0.1262 | 0.1230 | 0.1401 | 0.0284 | 0.0295 | 0.0338 | 54.9 | 48.8 | 47.2 | 32.3 | 27.5 | 23.3 |
| Ohio | 170 | 234 | 200 | 0.2256 | 0.2287 | 0.2785 | 0.0911 | 0.0869 | 0.1325 | 0.0760 | 0.0308 | 0.0710 | 0.0272 | 0.0069 | 0.0118 | 33.7 | 13.5 | 25.5 | 29.9 | 8.0 | 8.9 |
| Oregon | 60 | 74 | 76 | 0.2003 | 0.2408 | 0.2329 | 0.0675 | 0.0984 | 0.0910 | 0.0291 | 0.0494 | 0.0214 | 0.0031 | 0.0063 | 0.0035 | 14.6 | 20.5 | 9.2 | 4.6 | 6.4 | 3.9 |
| Pennsylvania | 192 | 236 | 263 | 0.2052 | 0.2445 | 0.2631 | 0.0765 | 0.1042 | 0.1195 | 0.0289 | 0.0570 | 0.0524 | 0.0026 | 0.0078 | 0.0076 | 14.1 | 23.3 | 19.9 | 3.5 | 7.5 | 6.4 |
| S. Carolina | 155 | 215 | 288 | 0.3109 | 0.2386 | 0.2658 | 0.1675 | 0.0926 | 0.1170 | 0.1702 | 0.0809 | 0.1126 | 0.0686 | 0.0142 | 0.0238 | 54.6 | 33.9 | 42.4 | 40.9 | 15.4 | 20.3 |
| Tennessee | 51 | 75 | 88 | 0.1721 | 0.1847 | 0.2940 | 0.0506 | 0.0568 | 0.1521 | 0.0406 | 0.0446 | 0.1417 | 0.0054 | 0.0041 | 0.0355 | 23.6 | 24.1 | 48.2 | 10.8 | 7.3 | 23.3 |
| Texas | 210 | 286 | 308 | 0.2312 | 0.2390 | 0.2730 | 0.0902 | 0.0940 | 0.1224 | 0.0823 | 0.0498 | 0.1006 | 0.0135 | 0.0063 | 0.0196 | 35.6 | 20.8 | 36.8 | 15.0 | 6.8 | 16.1 |
| Virginia | 120 | 155 | 164 | 0.1837 | 0.1999 | 0.2645 | 0.0587 | 0.0698 | 0.1161 | 0.0560 | 0.0471 | 0.1155 | 0.0066 | 0.0061 | 0.0243 | 30.5 | 23.6 | 43.7 | 11.3 | 8.7 | 20.9 |
| Washington | 53 | 74 | 72 | 0.2541 | 0.2397 | 0.2863 | 0.1152 | 0.0961 | 0.1414 | 0.0656 | 0.0752 | 0.0466 | 0.0160 | 0.0111 | 0.0062 | 25.8 | 31.4 | 16.3 | 13.9 | 11.6 | 4.4 |
| **USA** | **3464** | **4706** | **5124** | **0.2327** | **0.2502** | **0.3175** | **0.0916** | **0.1058** | **0.1749** | **0.0485** | **0.0456** | **0.0677** | **0.0055** | **0.0046** | **0.0099** | **20.9** | **18.6** | **21.3** | **6.1** | **4.4** | **5.7** |

The values for 1970, 1980 and 1990 are actually the averages of 1969 and 1970, 1979 and 1980, and 1989 and 1990, respectively.
[a] IE estimates are the difference between total inequality and IO values. This decomposition is only exact for the Theil 0 index.
[b] In percentage.

**Table 2a.** Growth, inequality and inequality of opportunity: the Theil 0 index.

| | THEIL | | THEIL + IO | | THEIL + IO/THEIL | | IE + IO | |
|---|---|---|---|---|---|---|---|---|
| | Small | Base | Small | Base | Small | Base | Small | Base |
| cte | 52.010*** | 131.10*** | 50.372*** | 91.688*** | 54.261*** | 118.25*** | 50.372*** | 91.688*** |
| | (7.4927) | (4.1332) | (7.5785) | (2.8518) | (8.3441) | (3.8398) | (7.5785) | (2.8518) |
| Total inequality | 14.778 | 31.388*** | 72.152*** | 93.775*** | 31.579*** | 42.413*** | | |
| | (0.9433) | (3.2377) | (2.6788) | (4.57) | (2.8577) | (4.0018) | -- | -- |
| IE | | | | | | | 72.152*** | 93.775*** |
| | -- | -- | -- | -- | -- | -- | (2.6788) | (4.57) |
| IO | | | -178.02** | -201.71*** | -14.686* | -11.682** | -105.87* | -107.93*** |
| | -- | -- | (-1.88) | (-3.3319) | (-1.515) | (-1.6873) | (-1.5282) | (-2.5382) |
| income lag | -0.0061*** | -0.005*** | -0.0059*** | -0.0049*** | -0.006*** | -0.005*** | -0.0059*** | -0.0049*** |
| | (-6.1206) | (-6.9205) | (-6.8035) | (-7.0578) | (-6.8691) | (-7.0544) | (-6.8035) | (-7.0578) |
| High | -0.2292 | -0.4179* | -0.0754 | -0.2443 | -0.1321 | -0.3666* | -0.0754 | -0.2443 |
| | (-1.1498) | (-1.4288) | (-0.3819) | (-0.8802) | (-0.6699) | (-1.2700) | (-0.3819) | (-0.8802) |
| College | 2.374*** | 1.9764*** | 2.075*** | 1.9502*** | 2.222*** | 1.9764*** | 2.075*** | 1.9502*** |
| | (5.5182) | (6.4791) | (5.6076) | (6.1831) | (5.9664) | (6.6315) | (5.6076) | (6.1831) |
| Farm share | | -16.526 | | -24.420** | | -24.671* | | -24.420** |
| | -- | (-1.1208) | -- | (-1.6093) | -- | (-1.4606) | -- | (-1.6093) |
| Mining | | -88.447 | | -77.460 | | -96.190 | | -77.460 |
| | -- | (-1.0919) | -- | (-1.0567) | -- | (-1.2035) | -- | (-1.0567) |
| Construction | | -162.60*** | | -44.217 | | -110.36* | | -44.217 |
| | -- | (-2.7457) | -- | (-0.6894) | -- | (-1.5876) | -- | (-0.6894) |
| Manufacturing | | -45.034** | | -11.616 | | -34.574* | | -11.616 |
| | -- | (-1.6111) | -- | (-0.399) | -- | (-1.2524) | -- | (-0.399) |
| Transport & Pub. Util. | | 52.847 | | 141.86** | | 86.176 | | 141.86** |
| | -- | (0.8825) | -- | (1.9655) | -- | (1.2205) | -- | (1.9655) |
| Fin. Inst. & real Estate | | -121.67* | | -90.532 | | -121.32* | | -90.532 |
| | -- | (-1.2965) | -- | (-0.9758) | -- | (-1.286) | -- | (-0.9758) |
| Government | | -43.299 | | -17.474 | | -34.466 | | -17.474 |
| | -- | (-1.2143) | -- | (-0.4865) | -- | (-0.9874) | -- | (-0.4865) |
| % lag 10yr emp. Growth | | -0.0697 | | -0.1010** | | -0.0769 | | -0.101** |
| | -- | (-1.1076) | -- | (-1.6004) | -- | (-1.1478) | -- | (-1.6004) |
| % welfare exp./income | | 0.2156 | | 0.6475 | | 0.6696 | | 0.6475 |
| | -- | (0.2049) | -- | (0.6166) | -- | (0.5753) | -- | (0.6166) |
| Fertility rate | | -0.3802*** | | -0.3764*** | | -0.3618*** | | -0.3764*** |
| | -- | (-4.637) | -- | (-4.4555) | -- | (-4.2236 ) | -- | (-4.4555) |
| % age 65 | 0.4434 | | 0.0019 | | 0.1559 | | 0.0019 | |
| | (1.0828) | -- | (0.005) | -- | (0.4107) | -- | (0.005) | -- |
| % pop Metropolitan | 0.1461*** | | 0.1327*** | | 0.1396*** | | 0.1327*** | |
| | (3.1276) | -- | (3.2838) | -- | (3.4222) | -- | (3.2838) | -- |
| decade 80 | 1.5049 | -5.026** | 1.0711 | -5.863** | 1.159 | -5.0271** | 1.071 | -5.863** |
| | (0.6617) | (-1.784) | (0.4698) | (-2.2017) | (0.5162) | (-1.8031) | (0.4698) | (-2.2017) |
| decade 90 | 5.8603** | -4.380 | 4.0469 | -6.403* | 4.8452** | -4.7597 | 4.046* | -6.403* |
| | (2.3342) | (-0.889) | (1.3561) | (-1.3935) | (1.7846) | (-0.9678) | (1.3561) | (-1.3935) |
| south | -6.663*** | -0.403 | -3.5372* | 3.563 | -4.5592** | 1.1747 | -3.537* | 3.563 |
| | (-3.933) | (-0.12) | (-1.2585) | (1.0877) | (-1.8168) | (0.3429) | (-1.2585) | (1.0877) |
| midwest | -1.763 | 1.647 | -1.9063 | 2.765* | -1.8535 | 2.1948 | -1.906 | 2.765* |
| | (-0.8099) | (0.8699) | (-0.9065) | (1.3321) | (-0.8612) | (1.0209) | (-0.9065) | (1.3321) |
| west | -5.696*** | 0.961 | -6.5335*** | 0.8254 | -6.295*** | 0.7517 | -6.533*** | 0.8254 |
| | (-2.8543) | (0.4509) | (-3.0034) | (0.4396) | (-3.0959) | (0.3676) | (-3.0034) | (0.4396) |
| R2 | 0.4366 | 0.6013 | 0.4784 | 0.6409 | 0.4605 | 0.6125 | 0.4784 | 0.6409 |
| Num. Observ. | 78 | 78 | 78 | 78 | 78 | 78 | 78 | 78 |

t-statistics in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%

**Table 2b.** Growth, inequality and inequality of opportunity: the Gini index.

| | GINI | | GINI + IO | | GINI + IO/GINI | | IE + IO | |
|---|---|---|---|---|---|---|---|---|
| | Small | Base | Small | Base | Small | Base | Small | Base |
| cte | 53.885*** | 126.68*** | 47.155*** | 95.145*** | 50.830*** | 112.43*** | 47.155*** | 95.145*** |
| | (7.9252) | (3.9245) | (5.1373) | (2.867) | (7.1653) | (3.6736) | (5.1373) | (2.867) |
| Total inequality | 15.806* | 38.201*** | 41.530* | 72.144*** | 25.448* | 48.038*** | | |
| | (1.3649) | (2.9773) | (1.3871) | (3.9913) | (1.3965) | (4.1193) | -- | -- |
| IE | | | | | | | 41.530* | 72.144*** |
| | -- | -- | -- | -- | -- | -- | (1.3871) | (3.9913) |
| IO | | | -34.027 | -45.668** | -6.681 | -7.0915* | 7.502 | 26.476** |
| | -- | -- | (-0.8632) | (-1.9061) | (-0.7037) | (-1.3507) | (0.3792) | (1.6678) |
| income lag | -0.0062*** | -0.0050*** | -0.0058*** | -0.0046*** | -0.0059 | -0.0048*** | -0.0058*** | -0.0046*** |
| | (-6.3587) | (-7.0918) | (-5.6164) | (-6.1139) | (-5.8158) | (-6.6805) | (-5.6164) | (-6.1139) |
| High | -0.2156 | -0.4291* | -0.1479 | -0.3247 | -0.1701 | -0.3810* | -0.1479 | -0.3247 |
| | (-1.0904) | (-1.4717) | (-0.6955) | (-1.1765) | (-0.8026) | (-1.3644) | (-0.6955) | (-1.1765) |
| College | 2.3627*** | 2.0015*** | 2.1786*** | 1.9462*** | 2.2394*** | 1.9646*** | 2.1786*** | 1.9462*** |
| | (5.564) | (6.649) | (4.9887) | (6.4558) | (5.185) | (6.5646) | (4.9887) | (6.4558) |
| Farm share | | -15.809 | | -20.644* | | -20.821* | | -20.644* |
| | -- | (-1.0923) | -- | (-1.3674) | -- | (-1.3300) | -- | (-1.3674) |
| Mining | | -73.980 | | -66.495 | | -73.387 | | -66.495 |
| | -- | (-0.8898) | -- | (-0.8483) | -- | (-0.9041) | -- | (-0.8483) |
| Construction | | -180.32*** | | -117.25** | | -144.01** | | -117.25** |
| | -- | (-3.1184) | -- | (-1.9615) | -- | (-2.3113) | -- | (-1.9615) |
| Manufacturing | | -44.135* | | -21.990 | | -33.114 | | -21.990 |
| | -- | (-1.5744) | -- | (-0.7604) | -- | (-1.208) | -- | (-0.7604) |
| Transport & Pub. Util. | | 55.1766 | | 123.79* | | 89.8168 | | 123.79* |
| | -- | (0.9367) | -- | (1.5245) | -- | (1.1942) | -- | (1.5245) |
| Fin. Inst. & real Estate | | -125.77* | | -119.59* | | -124.49* | | -119.59* |
| | -- | (-1.3854) | -- | (-1.3428) | -- | (-1.375) | -- | (-1.3428) |
| Government | | -38.399 | | -15.727 | | -25.708 | | -15.727 |
| | -- | (-1.0679) | -- | (-0.4187) | -- | (-0.7152) | -- | (-0.4187) |
| % lag 10yr emp. Growth | | -0.0662 | | -0.0840* | | -0.0734 | | -0.0840* |
| | -- | (-1.0835) | -- | (-1.3987) | -- | (-1.1832) | -- | (-1.3987) |
| % welfare exp./income | | 0.0807 | | 0.4054 | | 0.3641 | | 0.4054 |
| | -- | (0.0757) | -- | (0.3721) | -- | (0.3167) | -- | (0.3721) |
| Fertility rate | | -0.3964*** | | -0.3921*** | | -0.3898*** | | -0.3921*** |
| | -- | (-4.8367) | -- | (-4.5615) | -- | (-4.5831) | -- | (-4.5615) |
| % age 65 | 0.4363 | | 0.1412 | | 0.2190 | | 0.1412 | |
| | (1.1036) | -- | (0.2833) | -- | (0.4436) | -- | (0.2833) | -- |
| % pop Metropolitan | 0.1488*** | | 0.1360*** | | 0.1394*** | | 0.1360*** | |
| | (3.244) | -- | (3.1083) | -- | (3.1807) | -- | (3.1083) | -- |
| decade 80 | 1.5700 | -5.601** | 1.2376 | -6.1924*** | 1.3197 | -5.7782** | 1.2376 | -6.1924** |
| | (0.6842) | (-2.0426) | (0.5332) | (-2.4003) | (0.5741) | (-2.1593) | (0.5332) | (-2.4003) |
| decade 90 | 5.9691*** | -5.097 | 4.8595** | -6.4874* | 5.2088** | -5.6134 | 4.8595** | -6.4874* |
| | (2.3571) | (-1.0553) | (1.6339) | (-1.4413) | (1.8583) | (-1.1978) | (1.6339) | (-1.4413) |
| south | -6.5781*** | -0.4829 | -4.7397* | 2.6102 | -5.2175** | 1.2210 | -4.7397* | 2.6102 |
| | (-3.8779) | (-0.1493) | (-1.4265) | (0.8156) | (-1.6734) | (0.3903) | (-1.4265) | (0.8156) |
| midwest | -1.7511 | 1.6842 | -2.1698 | 2.112 | -2.0604 | 1.9859 | -2.1698 | 2.1120 |
| | (-0.8113) | (0.8932) | (-1.032) | (1.0869) | (-0.9648) | (0.9919) | (-1.032) | (1.0869) |
| west | -5.6379*** | 0.873 | -6.4691*** | 0.4607 | -6.2653*** | 0.6297 | -6.4691 | 0.4607 |
| | (-2.8241) | (0.4143) | (-2.9737) | (0.2332) | (-2.9168) | (0.3079) | (-2.9737) | (0.2332) |
| R2 | 0.4385 | 0.6088 | 0.447 | 0.6246 | 0.4427 | 0.6149 | 0.447 | 0.6246 |
| Num. Observ. | 78 | 78 | 78 | 78 | 78 | 78 | 78 | 78 |

t-statistics in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%

**Table 3.** Growth, inequality of opportunity and inequality of returns-to-effort: alternative models.

| | Small model | | | | | Base model | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Yt-1 | log(Yt-1) | lag Yt-1 | log(lag Yt-1) | No income | Yt-1 | log(Yt-1) | lag Yt-1 | log(lag Yt-1) | No income |
| **Pool-OLS** | | | | | | | | | | |
| IE | 72.152*** | 56.172** | 72.223** | 68.619** | 68.088** | 93.775*** | 83.719*** | 96.026 *** | 99.148*** | 81.725*** |
| | (2.6788) | (1.9982) | (2.052) | (2.0111) | (1.9437) | (4.57) | (4.1551) | (3.3564) | (3.8322) | (2.728) |
| IO | -105.87* | -124.09** | -163.08** | -170.42** | -156.73** | -107.93*** | -120.47*** | -148.76*** | -173.69*** | -147.58*** |
| | (-1.5282) | (-1.8243) | (-2.2025) | (-2.2156) | (-2.2444) | (-2.5382) | (-3.1378) | (-3.0533) | (-3.7706) | (-2.9534) |
| Real pc income | -0.0059*** | -62.599*** | -0.0016* | -18.070** | | -0.0049*** | -61.556*** | -0.0033 *** | -40.936*** | |
| | (-6.8035) | (-5.264) | (-1.4539) | (-2.0461) | -- | (-7.0578) | (-7.2515) | (-4.5364) | (-5.5603) | -- |
| **Cross-section long-run** | | | | | | | | | | |
| IE | 64.660 | 57.615 | 84.254 | 71.013 | 24.409 | 212.66** | 290.18*** | 224.42*** | 292.97*** | -43.029 |
| | (0.6368) | (0.578) | (0.8377) | (0.722) | (0.2292) | (2.5107) | (3.2306) | (3.0389) | (3.6945) | (-0.3216) |
| IO | -148.37** | -150.66** | -220.14** | -234.73** | -122.03* | -277.86** | -319.82** | -398.89*** | -462.00*** | -181.81 |
| | (-1.9284) | (-1.9944) | (-2.5038) | (-2.4124) | (-1.4751) | (-2.4147) | (-2.8945) | (-3.4809) | (-3.9809) | (-1.0866) |
| Real pc income | -0.0056* | -41.478 | -0.0101** | -66.765** | | -0.0132*** | -150.77*** | -0.0147*** | -131.41*** | |
| | (-1.4639) | (-1.0193) | (-2.4637) | (-1.7269) | -- | (-7.0817) | (-6.9315) | (-8.6345) | (-8.3904) | -- |
| **RE panel regression** | | | | | | | | | | |
| IE | 61.059*** | 40.314** | 37.436** | 34.502** | 22.645** | 75.344*** | 63.096*** | 56.219** | 49.953** | 33.464 |
| | (2.5317) | (1.647) | (1.8493) | (2.1808) | (1.636) | (2.9702) | (2.5415) | (1.9456) | (1.8528) | (1.1663) |
| IO | -76.837** | -74.505** | -46.403* | -39.708* | -16.861 | -93.698** | -106.69** | -87.565* | -92.208** | -72.8395 |
| | (-1.7838) | (-1.5981) | (-1.2738) | (-1.3663) | (-0.6608) | (-1.8498) | (-2.1199) | (-1.5079) | (-1.6725) | (-1.2308) |
| Real pc income | -0.0034*** | -41.206*** | -0.0015*** | -13.310*** | | -0.0042*** | -52.921*** | -0.0027*** | -29.769*** | |
| | (-4.5465) | (-4.0964) | (-2.4242) | (-2.8976) | -- | (-5.7971) | (-5.9822) | (-3.0659) | (-3.8421) | -- |
| **FE panel regression** | | | | | | | | | | |
| IE | 68.868*** | 48.589** | 75.068** | 80.7512** | 80.056** | 52.455** | 39.7447** | 37.193 | 44.967* | 31.2446 |
| | (2.621) | (1.7117) | (1.7142) | (1.8505) | (1.8551) | (2.3268) | (2.0082) | (1.1379) | (1.4659) | (0.961) |
| IO | -71.484* | -101.24** | -211.17*** | -230.00*** | -226.48*** | -87.462** | -94.309*** | -116.38** | -145.18*** | -107.37** |
| | (-1.4979) | (-2.0002) | (-2.7127) | (-2.9455) | (-3.0233) | (-2.13082) | (-2.6055) | (-1.9526) | (-2.5552) | (-1.8052) |
| Real pc income | -0.0144*** | -165.62*** | 0.0017 | -3.195 | | -0.0109*** | -137.15*** | -0.0029* | -46.063*** | |
| | (-10.9593) | (-9.7417) | (0.7526) | (-0.1747) | -- | (-8.3441) | (-10.3259) | (-1.2736) | (-3.1728) | -- |

t-statistics in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%

**Table 4.** Growth, inequality of opportunity and inequality of returns-to-effort: non-linearities.

| | Quadratic for IE | | Quadratic for IO | | Quadratic for IE and IO | |
|---|---|---|---|---|---|---|
| | Small | Base | Small | Base | Small | Base |
| **Pool-OLS** | | | | | | |
| IE | 106.894*** | 181.65*** | 80.104*** | 107.69*** | 74.063* | 152.03*** |
| | (2.434) | (4.657) | (3.111) | (5.934) | (1.298) | (3.248) |
| IE^2 | -100.101 | -252.51** | | | 18.439 | -136.12 |
| | (-1.189) | (-2.344) | -- | -- | (0.123) | (-0.908) |
| IO | -105.974* | -107.46** | 33.619 | 85.821 | 39.923 | 43.783 |
| | (-1.500) | (-2.235) | (0.309) | (1.028) | (0.274) | (0.442) |
| IO^2 | | | -2182.1** | -3052.2*** | -2280.43 | -2385.9** |
| | -- | -- | (-1.779) | (-3.008) | (-1.22) | (-1.698) |
| Real pc income | -0.0060*** | -0.0050*** | -0.0060*** | -0.0050*** | -0.0060*** | -0.0050*** |
| | (-6.477) | (-6.666) | (-6.563) | (-7.373) | (-6.569) | (-6.951) |
| **Cross-section long-run** | | | | | | |
| IE | 542.49 | -1078.9** | 69.758 | 229.24** | 546.02 | -1342.7** |
| | (0.679) | (-2.686) | (0.68) | (2.44) | (0.642) | (-2.79) |
| IE^2 | -3578.9 | 9677.6** | | | -3607.2 | 11929.7** |
| | (-0.621) | (3.061) | -- | -- | (-0.582) | (3.00) |
| IO | -107.67 | -343.78*** | -85.022 | -143.11 | -110.36 | -60.72 |
| | (-0.994) | (-3.367) | (-0.331) | (-0.726) | (-0.426) | (-0.31) |
| IO^2 | | | -1083.0 | -2519.1 | 51.467 | -5578.6* |
| | -- | -- | (-0.295) | (-0.826) | (0.013) | (-1.59) |
| Real pc income | -0.0050* | -0.0140*** | -0.0060* | -0.0140*** | -0.0050* | -0.0200*** |
| | (-1.417) | (-7.09) | (-1.496) | (-6.389) | (-1.383) | (-5.98) |
| **RE panel regression** | | | | | | |
| IE | 66.072 | 145.58*** | 71.931*** | 92.885*** | 36.523 | 118.82** |
| | (1.175) | (2.529) | (2.847) | (3.516) | (0.598) | (1.999) |
| IE^2 | -18.314 | -209.93* | | | 114.04 | -86.347 |
| | (-0.12) | (-1.393) | -- | -- | (0.626) | (-0.509) |
| IO | -73.898** | -88.376** | 28.408 | 67.897 | 59.635 | 48.372 |
| | (-1.745) | (-1.784) | (0.313) | (0.723) | (0.572) | (0.474) |
| IO^2 | | | -1897.0* | -2812.6** | -2456.0* | -2430.1* |
| | -- | -- | (-1.313) | (-1.987) | (-1.425) | (-1.518) |
| Real pc income | -0.0030*** | -0.0040*** | -0.0030*** | -0.0040*** | -0.0030*** | -0.0040*** |
| | (-4.46) | (-5.671) | (-4.517) | (-5.576) | (-4.471) | (-5.534) |
| **FE panel regression** | | | | | | |
| IE | 74.528* | 78.789** | 84.467*** | 59.666*** | 53.934 | 72.727* |
| | (1.295) | (1.656) | (3.021) | (2.397) | (0.926) | (1.454) |
| IE^2 | -14.851 | -73.013 | | | 87.782 | -41.745 |
| | (-0.111) | (-0.629) | -- | -- | (0.598) | (-0.302) |
| IO | -71.786* | -89.466** | 59.066 | -35.891 | 85.317 | -51.306 |
| | (-1.491) | (-2.162) | (0.604) | (-0.424) | (0.793) | (-0.516) |
| IO^2 | | | -1999.3* | -782.52 | -2374.0** | -566.0 |
| | -- | -- | (-1.526) | (-0.697) | (-1.629) | (-0.422) |
| Real pc income | -0.0140*** | -0.0110*** | -0.0140*** | -0.0110*** | -0.0140*** | -0.0110*** |
| | (-10.589) | (-8.008) | (-10.832) | (-8.191) | (-10.694) | (-7.955) |

t-statistics in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%

**Table 5.** Sensitivity Analysis of IO estimates by dropping some controversial states.

| | | OLS-pool regression | | Cross-sectional long-run | | RE panel regression | | FE panel regression | |
|---|---|---|---|---|---|---|---|---|---|
| | | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) |
| Without Arkansas | Small | -105* | -124 | -146** | -149** | -76** | -75** | -71* | -96** |
| | Base | -114*** | -129*** | -293** | -344** | -88** | -103** | -87** | -94*** |
| Without Mississippi | Small | -98* | -108** | -122** | -127** | -76** | -72** | -74* | -77** |
| | Base | -129*** | -134*** | -281** | -328** | -91** | -98** | -86** | -92*** |
| Without Kentucky | Small | -96* | -116** | -116** | -172** | -79** | -76** | -75* | -105** |
| | Base | -112*** | -127*** | -302** | -359** | -100** | -113** | -109** | -112*** |
| Without Tennessee | Small | -103* | -126** | -130** | -133** | -82** | -84** | -86* | -128** |
| | Base | -108*** | -123*** | -272** | -314** | -94** | -109** | -86** | -100*** |

t-statistics, IE coefficients (almost the same than in Table 2) and decimals are ommited to simplify the table.
* significant at 10%; ** significant at 5%; *** significant at 1%

**Table 6.** Sensitivity Analysis of IO and IE estimates by dropping main control variables.

| | | OLS-pool regression | | Cross-sectional long-run | | RE panel regression | | FE panel regression | |
|---|---|---|---|---|---|---|---|---|---|
| | | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) |
| Without Fertility | IE | 83*** | 73*** | 182** | 199** | 60** | 40* | 55*** | 42** |
| | IO | -121*** | -136*** | -313*** | -383*** | -77* | -86* | -96*** | -111*** |
| Without College | IE | 119*** | 114*** | -117 | -116 | 71*** | 72*** | 68** | 71*** |
| | IO | -148*** | -145*** | -331** | -328** | -90** | -99** | -86* | -84* |
| Without Welfare exp. | IE | 92*** | 83*** | 187** | 276*** | 75*** | 63*** | 51*** | 40** |
| | IO | -104*** | -120*** | -262** | -310*** | -93** | -107** | -87*** | -95*** |
| Without Industry Mix | IE | 95*** | 84*** | 103 | 170* | 74*** | 62*** | 68*** | 55*** |
| | IO | -106*** | -125*** | -214** | -298** | -83** | -96*** | -71** | -76** |

Results are for the 'base' model. T-statistics and decimals are ommited to simplify the table.
* significant at 10%; ** significant at 5%; *** significant at 1%

**Table 7.** Sensitivity Analysis of IO and IE estimates: parametric estimates of IO.

| | Small model | | Base model | |
|---|---|---|---|---|
| | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) |
| **OLS-pool** | | | | |
| IE | 32.231*** | 15.323* | 41.780*** | 28.116** |
| | (3.1803) | (1.3429) | (3.2081) | (2.1101) |
| IO | -141.23** | -158.61** | -79.996 | -68.450 |
| | (-1.5872) | (-1.8941) | (-0.919) | (-0.8307) |
| Income lag | -0.006*** | -63.620*** | -0.0051*** | -63.480*** |
| | (-6.8118) | (-5.2126) | (-6.7363) | (-6.8366) |
| **FE panel regression** | | | | |
| IE | 33.386** | 12.558 | 18.168* | 3.9580 |
| | (2.0732) | (0.7399) | (1.3365) | (0.329) |
| IO | -130.84* | -189.01** | -158.07*** | -126.04** |
| | (-1.4977) | (-2.0603) | (-2.4479) | (-2.171) |
| Income lag | -0.0144*** | -165.08*** | -0.0114*** | -139.97*** |
| | (-10.7516) | (-9.6732) | (-8.7152) | (-10.3155) |

t-statistics in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%

**Table 8.** Growth, inequality of opportunity and inequality of returns-to-effort: the set of circumstances.

| | IO-edu (4-groups) | | | | IO-race (2-groups)[(+)] | | | |
|---|---|---|---|---|---|---|---|---|
| | Small | | Base | | Small | | Base | |
| | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) |
| **Pool-OLS** | | | | | | | | |
| IE | 29.926* | 10.266 | 67.412*** | 53.639*** | 36.796*** | 22.564** | 44.390*** | 28.297** |
| | (1.5135) | (0.489) | (5.4491) | (3.8591) | (2.5887) | (1.6158) | (2.7035) | (1.8038) |
| IO | -38.428 | -46.825 | -111.75*** | -113.87*** | -174.34* | -200.17** | -16.989 | -4.275 |
| | (-0.5083) | (-0.6089) | (-2.5529) | (-2.3608) | (-1.4324) | (-1.7712) | (-0.1513) | (-0.0461) |
| Real pc income | -0.0061*** | -65.128*** | -0.0051*** | -63.231*** | -0.0055*** | -58.778*** | -0.0055*** | -75.066*** |
| | (-6.3768) | (-4.9029) | (-6.8461) | (-6.7254) | (-5.5466) | (-4.6704) | (-7.922) | (-8.9332) |
| **Cross-section long-run** | | | | | | | | |
| IE | -21.891 | -44.069 | 97.2708 | 139.87* | 43.3114 | 58.630 | -69.609 | -68.442 |
| | (-0.1773) | (-0.347) | (1.0773) | (1.3766) | (0.6562) | (0.9752) | (-1.228) | (-1.242) |
| IO | -63.007 | -32.974 | -106.38 | -130.552 | -578.71** | -657.72** | -365.04 | -477.91* |
| | (-0.3679) | (-0.1815) | (-0.6519) | (-0.7711) | (-1.772) | (-2.1581) | (-1.202) | (-1.782) |
| Real pc income | -0.0053* | -35.653 | -0.0123*** | -133.67*** | 0.0005 | 29.164 | -0.0120*** | -140.40*** |
| | (-1.3902) | (-0.8887) | (-5.8787) | (-5.0238) | (0.1279) | (0.8588) | (-5.506) | (-6.238) |
| **RE panel regression** | | | | | | | | |
| IE | 34.540* | 15.500 | 50.018** | 35.853** | 42.995*** | 27.420* | 38.921** | 23.145 |
| | (1.4218) | (0.6475) | (2.3112) | (1.7043) | (2.4253) | (1.5085) | (1.9235) | (1.2015) |
| IO | -52.904 | -36.014 | -88.523* | -92.543* | -187.529** | -178.56** | -87.088 | -96.671 |
| | (-0.7021) | (-0.462) | (-1.244) | (-1.2991) | (-2.3435) | (-2.0837) | (-0.8201) | (-0.9419) |
| Real pc income | -0.0032*** | -38.891*** | -0.0042*** | -51.992*** | -0.0031*** | -38.473*** | -0.0045*** | -59.179*** |
| | (-4.1079) | (-3.8193) | (-5.6445) | (-5.7726) | (-4.2478) | (-3.7884) | (-5.3516) | (-5.7959) |
| **FE panel regression** | | | | | | | | |
| IE | 41.534** | 23.120 | 27.939* | 19.154 | 24.083* | 8.653 | 18.971 | 6.403 |
| | (1.8389) | (0.961) | (1.433) | (1.1161) | (1.2964) | (0.4336) | (1.1002) | (0.4294) |
| IO | -40.968 | -84.916* | -65.332* | -82.193** | -62.043 | -129.34* | -124.56* | -111.92* |
| | (-0.6651) | (-1.3011) | (-1.2497) | (-1.7922) | (-0.7065) | (-1.4067) | (-1.4497) | (-1.4672) |
| Real pc income | -0.0149*** | -171.40*** | -0.0107*** | -135.81*** | -0.0151*** | -168.04*** | -0.0114*** | -142.21*** |
| | (-11.2613) | (-10.1129) | (-7.9296) | (-9.9092) | (-10.5529) | (-9.4787) | (-7.3051) | (-9.0025) |

t-statistics in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%

(+): We exclude Iowa, Minnesota, Oregon and Washington from the sample

**Table 9.** Growth, inequality of opportunity and inequality of returns-to-effort: the IPUMS-USA database.

| | | White Vs. Others (2-groups) | | Black Vs. Others (2-groups) | | White Vs. Black vs. Others (3-groups) | | White Vs. Others (2-groups) | | Black Vs. Others (2-groups) | | White Vs. Black vs. Others (3-groups) | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | **Small model** | | | | | | **Base model** | | | | | |
| | | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) | Yt-1 | log(Yt-1) |
| **Pool-ols** | IE | 122.39*** | 74.133* | 99.269** | 51.531 | 108.58** | 60.029 | 225.16*** | 212.23*** | 243.89*** | 225.62*** | 242.00*** | 226.15*** |
| | | (2.3576) | (1.3721) | (1.922) | (0.9514) | (2.0674) | (1.0987) | (4.0783) | (3.5799) | (4.6866) | (4.0182) | (4.5561) | (3.918) |
| | IO | -104.23** | -176.14*** | -55.720 | -123.53* | -261.30** | -346.70** | 245.47** | 17.414 | 186.71** | -25.133 | 144.47 | -273.46 |
| | | (-1.6547) | (-2.3819) | (-0.701) | (-1.4017) | (-1.6736) | (-2.0304) | (1.9499) | (0.1187) | (1.7196) | (-0.2029) | (0.6463) | (-1.0271) |
| **Cross-section long-run** | IE | 34.270 | 8.034 | 29.533 | 3.963 | 31.461 | 5.801 | 281.99* | 230.14 | 318.28** | 280.02* | 315.60* | 275.80* |
| | | (0.1469) | (0.0343) | (0.1271) | (0.017) | (0.136) | (0.025) | (1.5999) | (1.2936) | (1.809) | (1.586) | (1.792) | (1.561) |
| | IO | -262.45 | -298.87 | -250.80 | -287.75 | -539.75 | -587.99 | -566.05** | -848.63** | -622.17** | -938.02** | -1542.2** | -2120.5** |
| | | (-1.0583) | (-1.121) | (-1.0189) | (-1.0863) | (-0.993) | (-1.021) | (-2.029) | (-2.4211) | (-2.203) | (-2.616) | (-2.264) | (-2.632) |
| **RE panel regression** | IE | 48.290 | 17.793 | 44.772 | 11.049 | 47.580 | 14.232 | 137.73** | -16.274 | 66.038* | 90.423** | 63.544* | 90.525** |
| | | (1.1069) | (0.4475) | (1.1008) | (0.3004) | (1.1388) | (0.3767) | (1.9215) | (-0.2191) | (1.3202) | (1.9574) | (1.2491) | (1.9184) |
| | IO | -77.121 | -119.06 | -71.363 | -90.903 | -211.17 | -222.39 | 49.886 | -108.70 | -41.835 | -240.33** | -123.51 | -563.93** |
| | | (-0.5149) | (-0.8601) | (-0.5135) | (-0.7097) | (-0.6953) | (-0.797) | (0.1926) | (-0.4082) | (-0.2984) | (-1.7424) | (-0.3967) | (-1.831) |
| **FE panel regression** | IE | 46.663 | 77.041* | 124.42** | -5.882 | 131.56** | -1.374 | 34.025 | 4.026 | 55.004 | 26.989 | 55.819 | 28.797 |
| | | (0.9127) | (1.6013) | (1.8126) | (-0.0822) | (1.8474) | (-0.0186) | (0.3898) | (0.0497) | (0.6737) | (0.3582) | (0.6625) | (0.3702) |
| | IO | 42.781 | -186.71 | 138.64 | -210.53 | 40.930 | -450.66 | -72.811 | -149.75 | -219.27 | -326.43* | -455.40 | -637.09* |
| | | (0.2843) | (-1.2157) | (0.5175) | (-0.7685) | (0.0702) | (-0.7569) | (-0.3229) | (-0.7146) | (-0.9742) | (-1.5624) | (-0.9391) | (-1.4188) |
| **Pool-ols without fertility** | IE | | | | | | | 190.98*** | 179.25*** | 183.68*** | 165.84*** | 192.62*** | 177.63*** |
| | | | | | | | | (3.6999) | (3.4172) | (3.7439) | (3.3392) | (3.8672) | (3.4962) |
| | IO | | | | | | | -64.498 | -284.31*** | -54.038 | -248.33** | -333.06** | -723.31*** |
| | | | | | | | | (-0.6391) | (-2.6363) | (-0.5244) | (-2.2536) | (-1.5914) | (-3.2424) |
| **RE panel regression without fertility** | IE | | | | | | | -19.380 | 6.020 | -13.593 | 0.1542 | -13.009 | 4.205 |
| | | | | | | | | (-0.3818) | (0.1243) | (-0.2852) | (0.0034) | (-0.2652) | (0.0908) |
| | IO | | | | | | | -78.561 | -302.90** | -110.59 | -296.35** | -207.70 | -614.62** |
| | | | | | | | | (-0.4922) | (-1.8422) | (-0.7282) | (-1.9392) | (-0.6136) | (-1.8006) |
| **FE panel regression without fertility** | IE | | | | | | | 32.531 | 2.223 | 41.325 | 9.6603 | 47.637 | 18.759 |
| | | | | | | | | (0.3631) | (0.0255) | (0.4954) | (0.1192) | (0.5527) | (0.2244) |
| | IO | | | | | | | -224.32 | -349.65** | -333.26* | -480.78** | -707.37* | -980.84** |
| | | | | | | | | (-1.0222) | (-1.6113) | (-1.4893) | (-2.1866) | (-1.4728) | (-2.0862) |

t-statistics in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%

The values for 1970, 1980 and 1990 are actually the averages of 1969 and 1970, 1979 and 1980, and 1989 and 1990, respectively.

[a] IE estimates are the difference between total inequality and IO values. This decomposition is only exact for the Theil 0 index.

[b] In percentage.

**FIGURES:**

**Figure 1a.** Income inequality in U.S.
(average 1970-1980-1990)

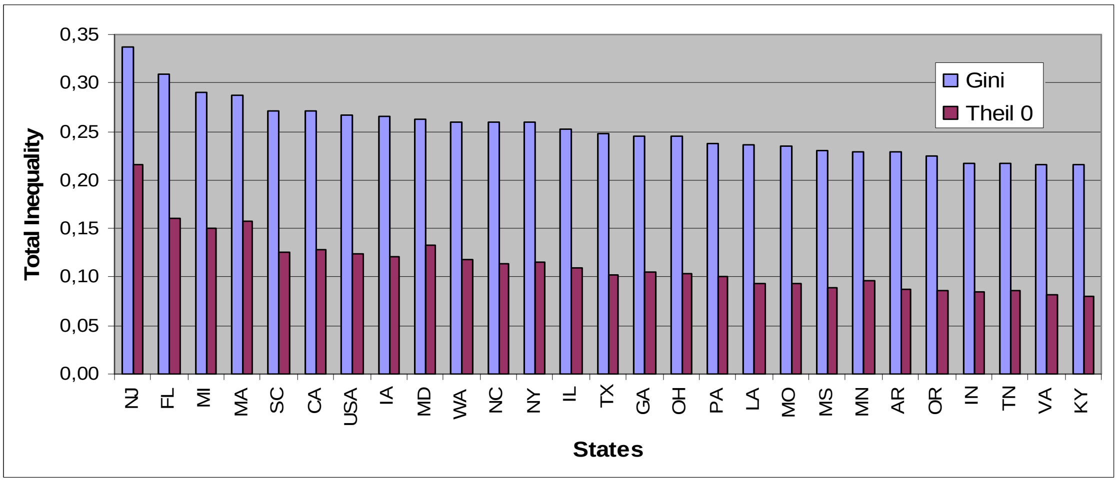



**Figure 1b.** Inequality of opportunity (8 groups) in U.S.
(average 1970-1980-1990)

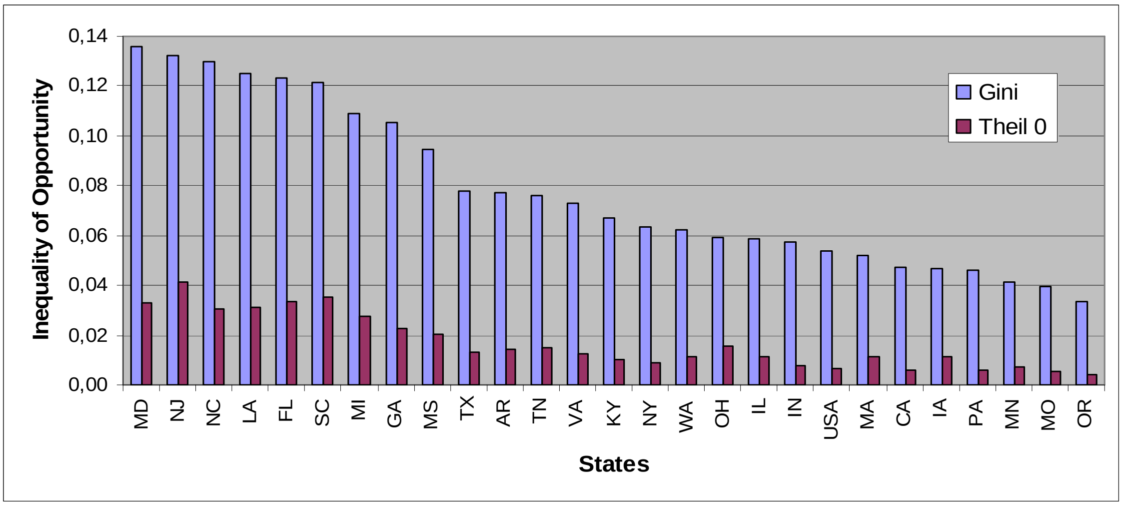



**Figure 1c.** Inequality of opportunity Ratio (8 groups) in U.S.
(average 1970-1980-1990)

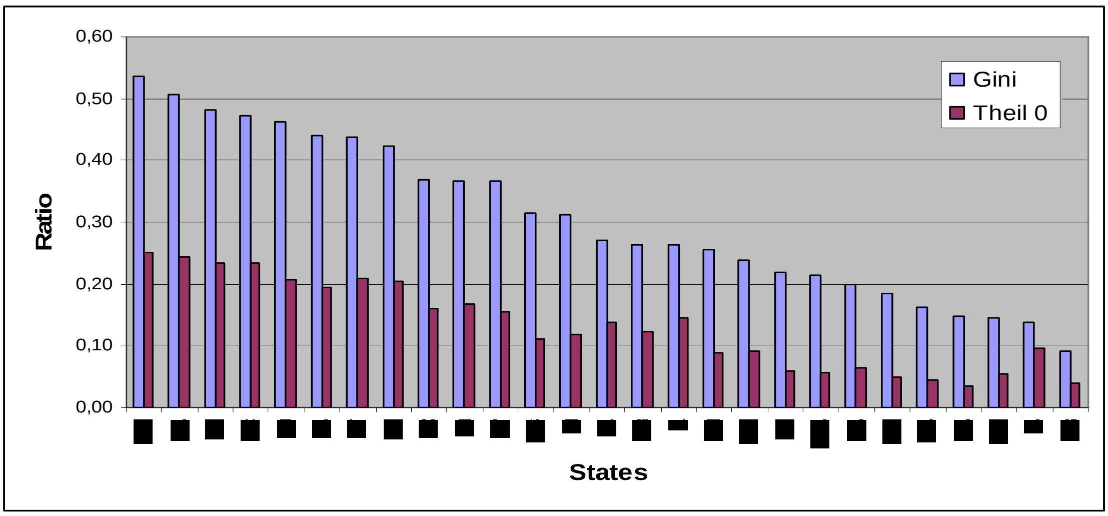


**Figure 2.a.** The scatter plot of total inequality and IO in U.S. (Theil 0).
(pool of observations: 1970,1980 and 1990)

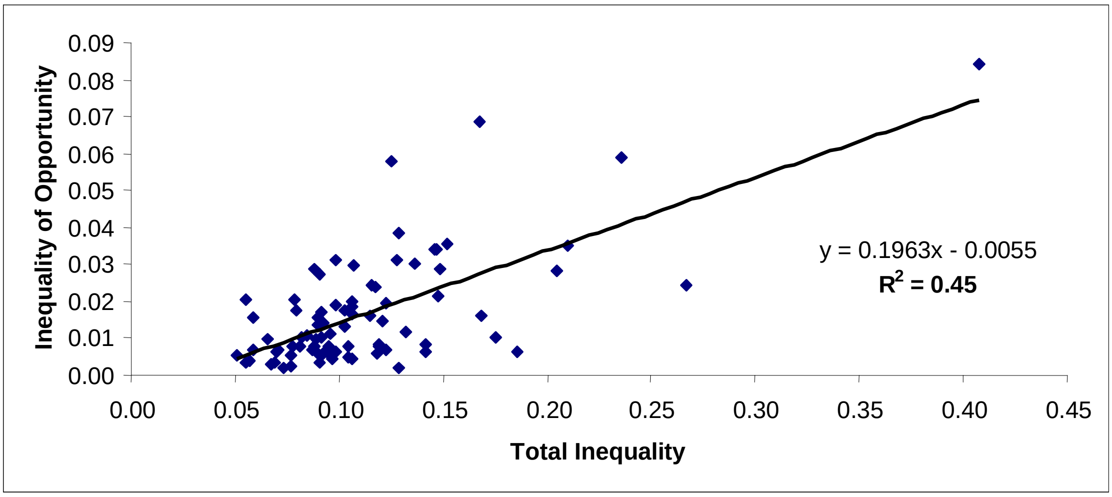



**Figure 2.b.** The scatter plot of IE and IO in U.S. (Theil 0).
(pool of observations: 1970, 1980 and 1990)

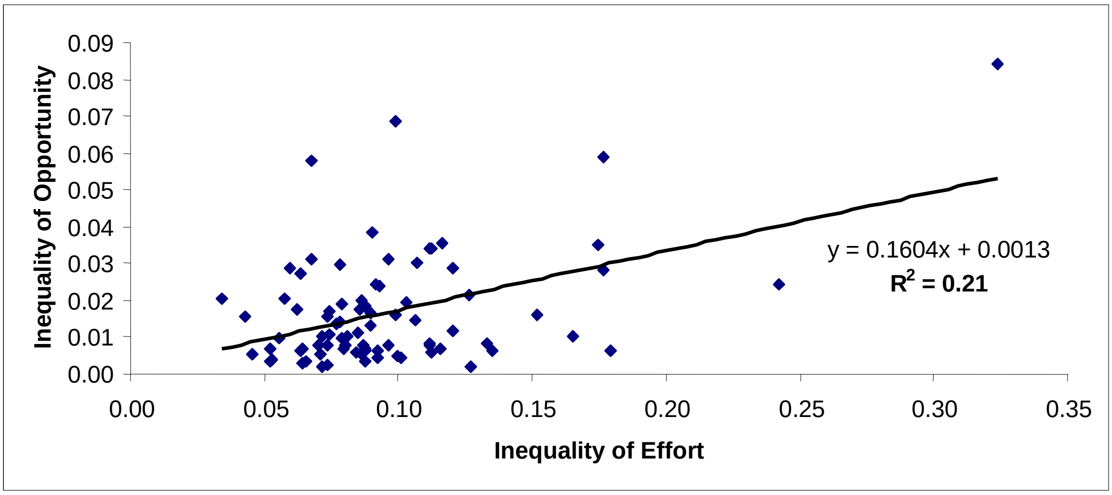



**Figure 3.** The scatter plot of IE and IO variations in U.S. (Theil 0).
(pool of observations: 1980-1970 and 1990-1980)

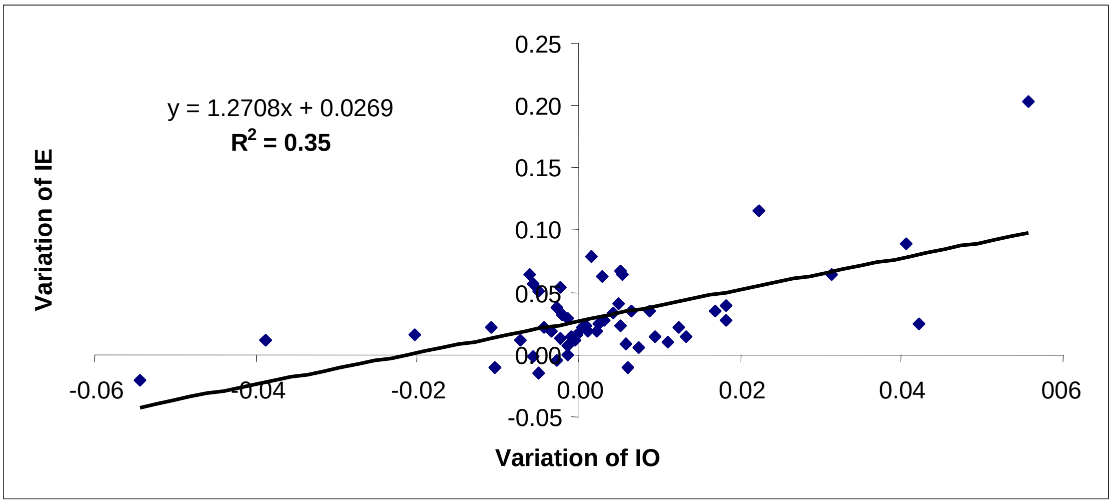
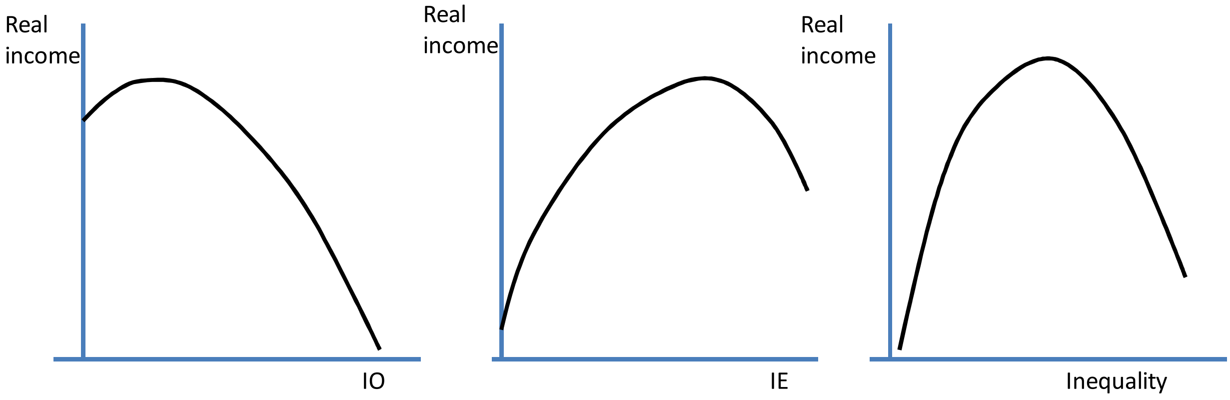


**Figure 4.** The relationship between IO, IE, total inequality and real income.




**APPENDIX: Data Sources**

**Population** (annual midyear population) and **personal income** data come from the Regional Economic Accounts of the Bureau of Economic Analysis (*U.S. Department of Commerce*, http://www.bea.gov/regional/spi/drill.cfm). Personal income is the income that is received by persons from all sources. It is calculated as the sum of wage and salary disbursements, supplements to wages and salaries, proprietors' income with inventory valuation and capital consumption adjustments, rental income of persons with capital consumption adjustment, personal dividend income, personal interest income, and personal current transfer receipts, less contributions for government social insurance. **CPI** data come from the U.S. Department of Labor (*All Urban Consumers CPI series*: http://www.bls.gov/data/#prices). **Employment** data (total and by type of industry) come from the Current Employment Statistics of the Bureau of Labor Statistics (*U.S. Department of Labor*: http://www.bls.gov/data/#employment). **Education** data comes from the Historical Census Statistics on Education Attained in the U.S., 1940 to 2000 (U.S. Census Bureau): http://www.census.gov/population/www/socdemo/ education/introphct41.html. **Fertility** is measures as the number of live births per 1000 women between the ages of 15 and 44 years (the general fertility rate), obtained from the Vital Statistics of the United States (http://www.cdc.gov/nchs/products/vsus.htm). Regarding the **poverty** rate, we follow the Office of Management and Budget's (OMB) Statistical Policy Directive 14, and use the statistics from the Census Bureau. Poverty rate is measured as the percent of families - and every individual in it – whose total income is less than a family's threshold. Data comes from the Census of Population, General Social and Economic Characteristics, 1960, 1970, 1980 and 1990, http://www.census.gov/hhes/www/poverty/census/cphl162.html. The percent of **Population above 65** years old and the percent **Metropolitan population** by states comes from the U.S. Census Bureau decennial Census of population, 1960-2000. The 1970, 1980 and 1990 state and **local welfare expenditure** data are from the U.S. Department of Commerce publication (Government Finances).

**Table A1.** Inequality of opportunity using the PSID (4 and 2 groups) and the IPUMS-USA in 1970, 1980 and 1990

| | **PSID** | | | | | | **IPUMS-USA** | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **IO-edu** | | | **IO-race** | | | **Theil 0** | | | **IO-ipums1** | | | **IO-ipums2** | | | **IO-ipums3** | | |
| **State** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** | **1970** | **1980** | **1990** |
| Alabama | -- | -- | -- | -- | -- | -- | 0.1582 | 0.1159 | 0.1519 | 0.0207 | 0.0120 | 0.0152 | 0.0207 | 0.0124 | 0.0158 | 0.0104 | 0.0062 | 0.0079 |
| Arkansas | 0.0137 | 0.0101 | 0.0023 | 0.0054 | 0.0078 | 0.0072 | 0.1503 | 0.1119 | 0.1518 | 0.0128 | 0.0072 | 0.0084 | 0.0130 | 0.0071 | 0.0085 | 0.0065 | 0.0036 | 0.0043 |
| California | 0.0021 | 0.0021 | 0.0039 | 0.0019 | 0.0022 | 0.0021 | 0.1394 | 0.1215 | 0.1774 | 0.0036 | 0.0040 | 0.0080 | 0.0036 | 0.0020 | 0.0019 | 0.0018 | 0.0014 | 0.0023 |
| Connecticut | -- | -- | -- | -- | -- | -- | 0.1348 | 0.1222 | 0.1737 | 0.0050 | 0.0068 | 0.0069 | 0.0049 | 0.0052 | 0.0052 | 0.0025 | 0.0028 | 0.0028 |
| Florida | 0.0159 | 0.0212 | 0.0084 | 0.0245 | 0.0027 | 0.0221 | 0.1575 | 0.1230 | 0.1618 | 0.0154 | 0.0073 | 0.0074 | 0.0158 | 0.0072 | 0.0075 | 0.0079 | 0.0037 | 0.0039 |
| Georgia | 0.0063 | 0.0172 | 0.0259 | 0.0132 | 0.0048 | 0.0079 | 0.1548 | 0.1190 | 0.1650 | 0.0246 | 0.0113 | 0.0138 | 0.0247 | 0.0122 | 0.0158 | 0.0124 | 0.0060 | 0.0079 |
| Illinois | 0.0012 | 0.0025 | 0.0036 | 0.0047 | 0.0015 | 0.0144 | 0.1255 | 0.1152 | 0.1639 | 0.0065 | 0.0067 | 0.0093 | 0.0068 | 0.0069 | 0.0083 | 0.0034 | 0.0036 | 0.0045 |
| Indiana | 0.0023 | 0.0013 | 0.0053 | 0.0002 | 0.0015 | 0.0048 | 0.1086 | 0.0986 | 0.1297 | 0.0025 | 0.0015 | 0.0018 | 0.0026 | 0.0017 | 0.0029 | 0.0013 | 0.0008 | 0.0014 |
| Iowa | 0.0056 | 0.0129 | 0.0157 | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- |
| Kansas | -- | -- | -- | -- | -- | -- | 0.1284 | 0.0922 | 0.1396 | 0.0027 | 0.0017 | 0.0031 | 0.0028 | 0.0017 | 0.0022 | 0.0014 | 0.0008 | 0.0012 |
| Kentucky | 0.0032 | 0.0052 | 0.0113 | 0.0002 | 0.0020 | 0.0021 | 0.1463 | 0.1146 | 0.1516 | 0.0035 | 0.0013 | 0.0017 | 0.0036 | 0.0020 | 0.0030 | 0.0018 | 0.0010 | 0.0015 |
| Louisiana | 0.0116 | 0.0222 | 0.0099 | 0.0052 | 0.0350 | 0.0043 | 0.1726 | 0.1342 | 0.1779 | 0.0281 | 0.0174 | 0.0235 | 0.0286 | 0.0182 | 0.0243 | 0.0143 | 0.0092 | 0.0123 |
| Maryland | 0.0108 | 0.0107 | 0.0274 | 0.0113 | 0.0054 | 0.0241 | 0.1409 | 0.1113 | 0.1447 | 0.0112 | 0.0074 | 0.0086 | 0.0123 | 0.0085 | 0.0093 | 0.0061 | 0.0043 | 0.0048 |
| Massachusetts | 0.0068 | 0.0016 | 0.0126 | 0.0010 | 0.0002 | 0.0081 | 0.1244 | 0.1062 | 0.1568 | 0.0023 | 0.0026 | 0.0042 | 0.0021 | 0.0023 | 0.0022 | 0.0010 | 0.0012 | 0.0013 |
| Michigan | 0.0069 | 0.0233 | 0.0223 | 0.0086 | 0.0059 | 0.0130 | 0.1168 | 0.1068 | 0.1554 | 0.0049 | 0.0032 | 0.0053 | 0.0049 | 0.0037 | 0.0066 | 0.0025 | 0.0019 | 0.0033 |
| Minnesota | 0.0051 | 0.0060 | 0.0048 | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- |
| Mississippi | 0.0188 | 0.0123 | 0.0052 | 0.0170 | 0.0045 | 0.0047 | 0.1772 | 0.1205 | 0.1521 | 0.0324 | 0.0187 | 0.0213 | 0.0323 | 0.0194 | 0.0221 | 0.0162 | 0.0097 | 0.0110 |
| Missouri | 0.0013 | 0.0008 | 0.0016 | 0.0004 | 0.0028 | 0.0013 | 0.1321 | 0.1030 | 0.1398 | 0.0050 | 0.0030 | 0.0034 | 0.0052 | 0.0032 | 0.0043 | 0.0026 | 0.0016 | 0.0022 |
| Nevada | -- | -- | -- | -- | -- | -- | 0.1448 | 0.0955 | 0.1712 | 0.0058 | 0.0016 | 0.0082 | 0.0052 | 0.0006 | 0.0045 | 0.0026 | 0.0004 | 0.0028 |
| New Jersey | 0.0073 | 0.0083 | 0.0492 | 0.0024 | 0.0145 | 0.0297 | 0.1302 | 0.1274 | 0.1812 | 0.0074 | 0.0082 | 0.0076 | 0.0077 | 0.0080 | 0.0087 | 0.0039 | 0.0042 | 0.0046 |
| New York | 0.0039 | 0.0038 | 0.0028 | 0.0056 | 0.0033 | 0.0061 | 0.1434 | 0.1279 | 0.1885 | 0.0088 | 0.0095 | 0.0125 | 0.0087 | 0.0063 | 0.0082 | 0.0044 | 0.0038 | 0.0050 |
| N. Carolina | 0.0223 | 0.0246 | 0.0218 | 0.0120 | 0.0126 | 0.0205 | 0.1474 | 0.1043 | 0.1369 | 0.0159 | 0.0076 | 0.0010 | 0.0155 | 0.0075 | 0.0102 | 0.0078 | 0.0038 | 0.0052 |
| Ohio | 0.0052 | 0.0011 | 0.0068 | 0.0047 | 0.0021 | 0.0006 | 0.1166 | 0.1019 | 0.1448 | 0.0041 | 0.0030 | 0.0039 | 0.0044 | 0.0037 | 0.0054 | 0.0022 | 0.0018 | 0.0027 |
| Oklahoma | -- | -- | -- | -- | -- | -- | 0.1309 | 0.1047 | 0.1393 | 0.0052 | 0.0035 | 0.0037 | 0.0044 | 0.0028 | 0.0022 | 0.0023 | 0.0015 | 0.0014 |
| Oregon | 0.0018 | 0.0049 | 0.0023 | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- |
| Pennsylvania | 0.0008 | 0.0049 | 0.0052 | 0.0014 | 0.0024 | 0.0018 | 0.1203 | 0.1062 | 0.1522 | 0.0041 | 0.0038 | 0.0035 | 0.0042 | 0.0044 | 0.0043 | 0.0021 | 0.0022 | 0.0021 |
| S. Carolina | 0.0568 | 0.0050 | 0.0084 | 0.0150 | 0.0100 | 0.0199 | 0.1424 | 0.1076 | 0.1337 | 0.0225 | 0.0103 | 0.0151 | 0.0224 | 0.0106 | 0.0160 | 0.0112 | 0.0053 | 0.0080 |
| Tennessee | 0.0046 | 0.0037 | 0.0307 | 0.0008 | 0.0004 | 0.0007 | 0.1538 | 0.1168 | 0.1518 | 0.0096 | 0.0048 | 0.0071 | 0.0094 | 0.0054 | 0.0074 | 0.0047 | 0.0027 | 0.0037 |
| Texas | 0.0102 | 0.0056 | 0.0023 | 0.0000 | 0.0018 | 0.0010 | 0.1528 | 0.1207 | 0.1686 | 0.0100 | 0.0066 | 0.0104 | 0.0101 | 0.0041 | 0.0048 | 0.0051 | 0.0025 | 0.0034 |
| Virginia | 0.0030 | 0.0043 | 0.0196 | 0.0041 | 0.0007 | 0.0075 | 0.1452 | 0.1084 | 0.1459 | 0.0140 | 0.0065 | 0.0088 | 0.0144 | 0.0072 | 0.0115 | 0.0072 | 0.0036 | 0.0057 |
| Washington | 0.0156 | 0.0108 | 0.0049 | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- | -- |
| **USA** | **0.0029** | **0.0023** | **0.0043** | **0.0029** | **0.0029** | **0.0069** | **0.1385** | **0.1145** | **0.1620** | **0.0071** | **0.0052** | **0.0064** | **0.0076** | **0.0050** | **0.0059** | **0.0038** | **0.0027** | **0.0032** |