International Trade and Labor Income Risk in the United States*

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Abstract

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JEL Classification: F13, F16, D52, E21

Keywords: Trade, Import Penetration, Labor Income, Idiosyncratic Income Risk, Income Volatility

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I. Introduction

A vast empirical literature has examined the effects that globalization may have on workers in the domestic economy with particular focus on the important question of how trade might affect, on average, the wages of workers in different human capital or occupational categories. This impressive literature has uncovered many interesting findings regarding the "mean" effects of globalization on labor markets. However, for the most part, this literature has not addressed a broadly expressed public concern regarding another possible channel through which globalization might affect labor markets: Openness to international trade may expose workers to riskier economic environments with greater volatility (variance) in their incomes.[1]

How might trade openness affect labor income volatility? The literature has suggested several ways in which the exposure of the domestic economy to international trade can impact income volatility. For instance, in the standard neoclassical models of international trade, changes in the level of trade openness, or changes in global patterns of comparative advantage will cause a reallocation of factors of production across sectors within the domestic economy. When firms within a sector are heterogeneous in their productivity (as in Melitz (2003)), trade openness causes intra-sectoral reallocation of factors of production across firms. Importantly, the process of labor reallocation across sectors and across firms within a sector may not be an orderly or costless one; to the extent that similar workers experience different outcomes in the process, they are exposed to labor income risk (defined as the variance of unpredictable changes in income).

Importantly, going beyond the short term reallocation effects of trade as described in these theories, the exposure of the import competing sectors to trade may have implications for long-run volatility in these sectors as well. For instance, Rodrik (1997) has argued that openness may lead to a permanent increase in volatility because increased import competition, which increases the elasticity of the demand for goods, also raises the elasticity of the derived labor demand, in turn implying a larger variation in wages and employment in response to economic shocks (such as shocks to productivity or output demand) in the domestic economy. Along the same lines, Krishna, Mitra and Chinoy (2001) show, theoretically, that if the import competing sector were characterized by monopolistic

[1] Exceptions include Krebs, Krishna, and Maloney (2010), which studies Mexico, diGiovanni and Levchenko (2009), which provides interesting cross-country evidence regarding the links between trade and sectoral output volatility, and McLaren and Newman (2002), which studies how globalization may weaken domestic institutions for risk-sharing.

competition, trade openness would again result in an increased elasticity of firm-level labor demand (See Appendix A for details).[2] In this setting, if productivity shocks are at the firm-level, we will see a permanent increase in firm level volatility of employment and wages with greater openness. Furthermore, different firms may be hit by different productivity shocks, or may be otherwise heterogeneous. In either case, different workers employed in these different firms will be affected differently, implying a permanent increase in idiosyncratic income volatility for workers employed in the industry.

Similarly, Newbery and Stiglitz (1984) have argued that while in a closed economy, domestic price adjustments insulate producers against supply shocks (as shocks to output lead to offsetting movements in goods prices that stabilize incomes), in an open economy world prices do not play this role since such shocks have no impact on the world price. This implies that domestic productivity shocks will have a smaller equilibrium effect on sectoral output and employment in a closed economy compared to an open one, implying a permanently more volatile domestic economy.[3] If the adjustment process for labor reallocation following productivity shocks is such that workers experience different labor market outcomes, the economy will again be characterized by a permanently higher level of labor income risk.

Finally, openness implies that the domestic economy is also exposed to international markets and will be continuously affected by changes in shocks to (and trends in) the productivity and demand patterns abroad. As Rodrik (1998) points out, there are competing factors at play. On the one hand, since the world economy as a whole is *less* volatile than the economy of a single country (due to the law of large numbers), greater exposure to the international economy may lower risk. On the other hand, increased openness to trade is associated with increased specialization (through the forces of comparative advantage), resulting in a production structure that is less diversified, generating a more unstable stream of income from domestic production and thus in greater risk.[4] These aggregate changes may have idiosyncratic impacts on different domestic workers, leading to greater or lower levels of individual income risk with openness. Thus, as Rodrik (1998) notes, whether greater

[2] The hypothesis that greater openness leads to greater elasticity of factor demand has found empirical support in Slaughter (2001) and Senses (2012) in the U.S, and Hasan, Mitra and Ramaswamy (2007) and Hijzen and Swaim (2010) for India and a set of OECD countries, respectively.

[3] Specifically, Newbery and Stiglitz (1984) show that, while trade liberalization between two countries with negatively correlated outputs may reduce price volatility, it can also increase volatility in income to an extent that leaves all groups in both countries worse off. See also Epifani and Gancia (2009).

[4] Rodrik (1998) also provides empirical evidence that the latter channel dominates and that external risk is positively (and significantly) associated with aggregate income volatility for different measures of income.

exposure to external risk with openness is accompanied by higher or lower levels of risk in the domestic economy is largely an empirical matter.

This paper conducts an empirical analysis of the link between trade and individual labor income risk in the United States.[5] We use longitudinal data on workers to estimate idiosyncratic labor income risk and to study the role of trade in explaining the variation in risk across workers employed in different industries.[6] In estimating labor income risk, we employ specifications of the labor income process that account for the shocks to labor income that workers receive and that distinguish between transitory and persistent shocks to income. This distinction between transitory and persistent shocks is important. Workers can effectively "self-insure" against transitory shocks through borrowing or own savings, and the welfare effects of such shocks are quite small (Heaton and Lucas (1996), Levine and Zame (2002)). In contrast, highly persistent or permanent income shocks have a substantial effect on the present value of future earnings and therefore lead to significant changes in consumption. Thus, from a welfare point of view, it is the persistent income shocks that matter the most and it is on these shocks that we focus our attention.

In our analysis, we combine industry-level, time-varying estimates of the persistent component of labor income risk with measures of industry exposure to international trade to estimate the relationship between labor income risk and trade. We also repeat this analysis for different sub-samples of workers, such as those who switched to a different industry or sector, or those who remained in the same industry throughout the sample. Finally, we use our empirical estimates to conduct a simple welfare analysis to obtain indicative estimates of the benefits or costs of trade through the income risk channel. We note here that our paper builds on an earlier paper by Krebs, Krishna, and Maloney (2010) on trade openness and income risk in Mexico. While we broadly follow their methodological approach, the richer availability of data in our empirical context (for the United States) enables us to introduce

[5] We should note at the outset that labor income risk, which measures the variance of income *changes* is a distinct concept from wage inequality, which has been the focus of a large theoretical and empirical literature in international trade. For instance, while the distribution of incomes could stay the same between two time periods (i.e. with no change in inequality), workers could stochastically exchange positions with each other under the same income distribution, thus experiencing risk. A number of researchers have examined the implications of the theoretical "Stolper-Samuelson" prediction that trade openness will lead to an increase in earnings of abundant factors and a reduction in the earnings of scarce factors (see for instance, Lawrence and Slaughter (1993), Leamer (1996), Feenstra and Hanson (1999) and Goldberg and Pavcnik (2005)). While the links between trade, wage levels and wage inequality are clearly important issues to study, our focus is on a different dimension of the labor market experience – the variability (risk) in incomes experienced by workers.

[6] We use the Survey of Income and Program Participation (SIPP) in our analysis. SIPP contains longitudinal panels on individuals, with each panel ranging roughly three years in duration. We use data from 3 SIPP panels (the 1993–1995, 1996–1998 and 2001–2003 panels) in our study.

several methodological improvements, which allow for a more precise and robust evaluation of the links between trade openness and income risk.[7]

Our empirical results for the United States can be summarized as follows. First, we find that income risk is increasing over time for both the full sample of workers as well as workers in each sub-sample. Second, we find that those workers who switched industries (moving to a different manufacturing industry or to the non-manufacturing sector) experience higher income risk compared to those who stayed in the same industry throughout the sample. Among switchers, risk for those who switched to the non-manufacturing sector is higher than those who switched within manufacturing.[8] Finally, and most importantly, we find that within-industry changes in income risk are strongly related to changes in import penetration over the corresponding time-periods. This relationship between trade and income risk is robust to accounting for the endogeneity of import penetration, which we ascertain both by by implementing an instrumental variable approach (where we use the sectoral volume of exports from China to high-income OECD countries other than the US as an instrument for sectoral import penetration in the US) and, by separately controlling for a range of time-varying industry specific factors (such as exports, skill-biased technological change, offshoring, unionization, productivity and, growth) that are potentially correlated with both income risk and import penetration. Quantitatively, estimates from our preferred specification suggest that an increase in import penetration by ten percent is associated with an increase in the standard deviation of persistent income shocks of about 20 to 25 percent, for the full sample of workers.[9] Our welfare calculations suggest that these effects are economically significant, even after we evaluate them by considering a number of variations from our benchmark estimates and using a wide range of values of underlying parameters.

We should emphasize that our analysis focuses *exclusively* on the link between trade and

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[7] In our study, we use SIPP panels have a much longer longitudinal dimension than the Mexican data used by Krebs, Krishna, and Maloney (2010). As we discuss in the paper, this allows for important methodological improvements in the estimation of permanent income shocks. Furthermore, we estimate risk faced by various sub-samples of workers and study the differential association of trade exposure on workers in these groups. Finally, we use the greater availability of data in the United States on a variety of industry characteristics to include necessary controls in our econometric analysis, as discussed later in the paper. In sum, the present study, conducted in the context of the United States economy, uses a stronger methodological approach and superior breadth of data to arrive at more precise evaluation of the association between international trade and income risk than any previous analyses of this topic.

[8] As we will discuss in detail later in the paper, the estimates of income risk for the different groups of workers reflect the differences in worker characteristics and the endogenous actions that place workers in these different sub-samples, and should be interpreted with this qualification in mind.

[9] The same increase in import penetration is associated with an increase in income risk of about 30 percent and 20 percent for workers who remained in the same manufacturing industry throughout and those who switched, respectively.

individual income risk. Hence, our results should be taken together with the findings of a large literature on international trade exploring the many ways in which trade may affect the economy positively, through improved resource allocation, access to greater varieties of intermediate and final goods, greater exploitation of external economies and by possibly raising growth rates, *inter alia*. Specifically, the results presented here should not be interpreted as suggesting that exposure to trade results in welfare reduction, but instead as evidence that the costs of increased labor income risk ought to be taken into account when evaluating the total costs and benefits of trade and trade policy reform.

II. Labor Income Risk

The first stage of our analysis concerns the estimation of individual income risk and its separation into transitory and persistent components. As we have discussed earlier, it is this focus on income risk that separates our analysis from much of the earlier literature that has examined instead the "mean" effect of trade on wages of workers in different skill and occupational categories.[10] Furthermore, as we have indicated earlier, the separation between transitory and persistent shocks is essential for multiple reasons. First, consumption smoothing through borrowing or own savings works well for transitory income shocks but not when income shocks are highly persistent or permanent. Thus, highly persistent income shocks have a large effect on consumption volatility and welfare, whereas the effect of transitory shocks is relatively small. Second, the transitory term in our econometric specification of the income process will absorb the measurement error in individual income. For these reasons, we will focus on persistent shocks and their relation to trade exposure.[11]

II.1. Data

To estimate the risk in incomes faced by individuals, longitudinal data tracking individual

[10] We thank an anonymous referee for suggesting the following example to highlight this distinction: If the steel industry is exposed to a global glut in steel, our analysis seeks to capture the variance in outcomes experienced by different steel workers (roughly speaking) and not the downward trend in average wages that all steel workers may experience in common.

[11] We note two points here. First, there may be (and indeed are) circumstances under which transitory shocks also have welfare impact, for instance, when individuals are credit constrained or are otherwise restricted from borrowing or saving. However, the inclusion of these costs will only raise the welfare estimates we report in this paper. Second, as a practical matter, in our analysis of the data transitory income shocks are uncorrelated with trade exposure. This should not be too surprising given that the estimates of transitory income shocks are large and noisy, being that they are contaminated by measurement error in income data (as has been extensively discussed in the literature).

income transitions is very useful to have as it is the variance in income changes at the individual level that reflect the risk to which workers are exposed. In this paper, we use longitudinal data on individuals from the 1993–1995, 1996–1999 and 2001–2003 panels of the Survey of Income and Program Participation (SIPP). Each panel of the SIPP is designed to be a nationally representative sample of the US population and surveys thousands of workers. The interviews are conducted at four-month intervals over a period of three years for the 1993 panel, four years for the 1996 panel and three years again for the 2001 panel.[12] At each interview, data on earnings and labor force activity are collected for each of the preceding four months.

SIPP has several advantages over other commonly used individual-level datasets in that it includes monthly information on earnings and employment over a long panel period for a large sample. Although the Current Population Survey (CPS) provides a larger sample, individuals are only sampled for 8 months over a two-year period in comparison to 33 months in the SIPP. While the Panel Study of Income Dynamics (PSID) provides a much longer longitudinal panel, it has a significantly smaller sample size compared to the SIPP and therefore does not support the estimation of risk at the industry level.

In our analysis, we restrict the SIPP sample to respondents of age 16 to 65 who were not enrolled in school during a given month. Following previous literature, we exclude all observations for individuals whose earnings in any month were less than 5% or higher than 195% of the individual's average monthly earnings.[13] Table 1 presents a summary description of the workers surveyed in each panel. The summary statistics calculated for the first month of each panel are reported separately for the whole sample and for the manufacturing sector only. Worker's earnings represent amounts actually received in wages and salary and/or from self-employment, before deductions for income and payroll taxes, union dues, Medicare premiums, etc.

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[12] We limit our main analysis to data from the first three years of the 1996 panel to ensure comparability of our risk estimates from the other two panels. As we discuss later, we do exploit the additional year of data in the 1996 panel in our analysis of robustness.

[13] This results in the omission of approximately 13% of the respondents of each panel from our sample. Our results are robust to alternate criteria, such as when we exclude individuals whose earnings in any month were less than 1 percent or higher than 199 percent of the individual's average monthly earnings. Our results also remain robust if, instead of excluding individuals whose income in any period is outside of the set thresholds of 5 percent and 195 percent, we exclude only those income observations that are outside these thresholds. This latter procedure results in a loss of merely 0.1 percent of observations, and the main results remain very similar to those reported in this paper. However, we should also note that not all of our results are robust to "no cleaning". This is because not cleaning the data at all results in the inclusion of workers with income variability that is implausibly large, with risk estimates an order of magnitude higher than those for the rest of the sample.

II.2. Specification

To estimate labor income risk, we follow the approach taken in previous empirical work on this topic (see for instance, Carroll and Samwick (1997), Gourinchas and Parker (2002) and Meghir and Pistaferri (2004)).[14] Our survey data provide us with earnings (wage rate times number of hours worked) of individuals. We assume that the log of labor income of individual i employed in industry j in time period (month) t, log y_{ijt}, is given by:

$$\log y_{ijt} = \alpha_{jt} + \beta_t \cdot x_{ijt} + u_{ijt} \qquad (1)$$

In (1) α_{jt} and β_t denote time-varying coefficients, x_{ijt} is a vector of observable characteristics (such as age, age-squared, education, marital status, occupation, race, gender and industry), and u_{ijt} is the stochastic component of earnings. Changes in the stochastic component u_{ijt} represent individual income changes that are *not* due to changes in the return to observable worker characteristics. For example, income changes that are caused by an increase in the skill (education) premium are not contained in changes in u_{ijt}. In this sense, changes in u_{ijt} over time measure the unpredictable part of changes in individual income.[15]

We assume that the stochastic term is the sum of two (unobserved) components, a permanent component ω_{ijt} and a transitory component η_{ijt} :

$$u_{ijt} = \omega_{ijt} + \eta_{ijt} \qquad (2)$$

Permanent shocks to income are fully persistent in the sense that the permanent component follows a random walk:

$$\omega_{ijt+1} = \omega_{ijt} + \varepsilon_{ijt+1} \qquad (3)$$

[14] We should note that these papers have pursued the empirical estimation of labor income largely at broader levels of aggregation. None has examined the variation of income risk across different manufacturing industries nor, importantly, has focused on the relationship between labor income risk and international trade, as we do in this paper.

[15] Since income risk is calculated as the variance of unpredictable changes in earnings, it is understood that any time-invariant individual specific component of earnings will be purged out from our risk estimates. As such, the inclusion of individual-fixed effects in specification (1) should not and do not alter our risk estimates.

where the innovation terms, $\{\varepsilon_{ijt}\}$, are independently distributed over time and identically distributed across individuals, $\varepsilon_{ijt} \sim N(0, \sigma^2_{\epsilon js})$, where *s* denotes the SIPP panel (i.e., one of the 1993–1995, 1996–1998 or 2001–2003 panels). In this basic specification, transitory shocks have no persistence, that is, the random variables $\{\eta_{ijt}\}$ are independently distributed over time and identically distributed across individuals, $\eta_{ijt} \sim N(0, \sigma^2_{\eta js})$. Note that the parameters describing the magnitude of both transitory and persistent shocks are assumed to depend on the sector *j* and the SIPP panel *s*, but do not depend on *t*. That is to say, they are assumed to be constant within a SIPP panel, but allowed to vary across panels. Estimation of $\sigma^2_{\varepsilon js}$ and $\sigma^2_{\eta js}$ will therefore give us industry specific, time varying estimates of transitory and persistent income risk faced by individuals.

Notice that in (1), we allow the coefficient β_t to vary over time. Doing so takes out of income risk calculations any changes in income that may have occurred due to changes in returns to observable characteristics. Another possibility is to treat these changes as unpredictable by requiring the coefficients β to be time-invariant within a panel. In this case, estimated income risk will include any changes in the returns to observable characteristics that take place in reality. Which set of estimates to use will depend on whether we think of changes in the coefficients on observable worker characteristics to be predictable or not. While this is an interesting conceptual issue, in practice, estimates of the parameters representing income risk do not seem to depend very much on whether the changes in returns to observable characteristics are accounted for by allowing β to be time varying, or not, in estimating (1) – the correlation between the two sets of estimates is very high (around 0.99).[16]

Finally, notice that the inclusion of industry dummies in (1) filters out mean income changes

[16] Further, to allow for differential changes in returns to skills that vary by the level of skill and industry, we have also estimated various other versions of the first stage Mincer regressions by including, on the right hand side, education-level-specific time trends and a more detailed set of education-industry-specific time trends. Our results concerning the links between trade openness and risk remain robust to these changes. This is perhaps not too surprising as the correlation between the risk estimates obtained with the additional education and industry-specific time trends (as described above) and the estimates presented in the paper is quite high (ranging from 0.71 to 0.96). We note also that if unobservable skills are correlated with observable education levels, as a large literature in labor economics has previously observed, the estimated coefficient of education in the Mincer regression will capture the returns to both education and the component of unobserved skill that is correlated with education. While this is typically a problem with Mincer regressions when the goal is to estimate returns to education, this is not an issue for us, as we would like to take away from the residual any returns that are predictable to the worker but unobservable to us. Thus, to the extent that we allow for returns to observable characteristics to vary over time, we also implicitly allow for returns to unobservables correlated with observable characteristics to vary over time. In this case, the changes in returns to these unobservable characteristics over time should also not contaminate our risk estimates.

in an industry and thus any volatility in the changes of the mean industry earnings from our measure of individual risk. Our risk estimates therefore measure idiosyncratic income risk (effectively individual variation around the industry mean, conditional on the other covariates in (1)).[17]

II.2.1 Filtering out Shocks of Short Duration

Our specification of the labor income process (Equations (1)–(3)) describes shocks to income to be either purely transitory or purely persistent and is in accordance with other empirical work on US labor income risk. However, this specification does not capture shocks that have duration greater than one period (i.e., are not purely transitory) but that are also not permanent (i.e., last for a finite amount of time). Estimation of permanent income risk in this case requires us to filter out such shocks of longer duration (See Meghir and Pistaferri (2004)). To achieve this, we admit into the specification some moving average terms:

$$u_{ijt} = \omega_{ijt} + \sum_{k=0}^{K} \eta_{ijt-k}, \qquad (2')$$

with K indicating the number of moving average terms. In addition to the specification where transitory shocks have no persistence (K=0), we consider two alternative specifications of the labor income process that allow for transitory shocks that last up to six months (K=6) and, separately, up to a year (K=12). We denote the corresponding parameters estimating permanent income risk by $\sigma^2_{\varepsilon,k=0}$, $\sigma^2_{\varepsilon,k=6}$, and $\sigma^2_{\varepsilon,k=12}$ respectively. Note that we expect the estimates of permanent income risk to be smaller in magnitude when shocks of shorter duration have been filtered out; that is, we expect $\sigma^2_{\varepsilon,k=0} > \sigma^2_{\varepsilon,k=6} > \sigma^2_{\varepsilon,k=12}$ (See Meghir and Pistaferri (2004)). While we report our results obtained for each value of K, we place greater emphasis on results from specification (2') with K=12. $\sigma^2_{\varepsilon,k=12}$ is our preferred risk estimate because we are interested in permanent income risk and this specification of the labor income process allows us to filter out transitory shocks of greater duration than the other two estimates do.

Our intention is to estimate parameters measuring income risk and see how changes in these

[17] While it is possible that trade may additionally affect workers (positively or negatively) by affecting the volatility of mean income growth in industries, in our data we do not find evidence of any relationship between the variance of changes in mean industry earnings and import penetration. Besides, estimates of risk obtained by including industry-year effects into the error term, are quantitatively extremely close to estimates obtained without taking these into account (the correlation between the two sets of estimates is around 0.97).

parameters over time (i.e., across panels) are related to international trade. In order to do this, we first estimate the income risk parameters at the industry level separately for each panel (for each of the cases with K=0, K=6 and K=12). Estimation of the income process parameters is discussed next.[18]

II.3. Estimation

Consider the change in the residual of income of individual i between period t and $t+n$ (we drop the subscript *s* for notational convenience; it is understood that the estimation exercises are conducted separately for each panel):

$$\Delta_n u_{ijt} = u_{ijt+n} - u_{ijt} = \varepsilon_{ijt+n} + \cdots + \eta_{ijt+n} - \eta_{ijt} \qquad (4)$$

We have the following expression for the variance of these income changes:

$$var[\Delta_n u_{ijt}] = \sigma^2_{\varepsilon jt+1} + \cdots + \sigma^2_{\varepsilon jt+n} + \sigma^2_{\eta jt} + \sigma^2_{\eta jt+n} \qquad (5)$$

As noted earlier, the parameters $\sigma^2_{\varepsilon j}$ and $\sigma^2_{\eta j}$ are assumed to be constant within the period covered by a single SIPP panel (i.e., within each of the 1993–1995, 1996–1998 and 2001–2003 panels).

Given this constancy, (5) can be written as:

$$var[\Delta_n u_{ijt}] = 2 \cdot \sigma^2_{\eta j} + n \cdot \sigma^2_{\varepsilon j} \qquad (6)$$

Thus, the variance of observed n-period income changes is a linear function of n, where the slope coefficient is equal to $\sigma^2_{\varepsilon j}$. This insight, that the random walk component in income implies a linearly increasing income dispersion over time, is the basis of the estimation method used by several authors. Following Carroll and Samwick (1997), we estimate the parameters in (6) by regressing individual measures of $var\,(\Delta_n u_{ijt})$, the square of the individual deviation from mean income difference over the n periods, on n . Equation (6) is estimated separately for each industry and panel.

[18] We discuss below the estimation of the parameters of (2) and (3). The estimation of income risk parameters when K>0 as in (2') is entirely analogous.

Note that, conditional on this income specification, identification of the magnitude of the shocks is achieved by simply comparing the cross sectional variances of income changes measured over different time periods. Since income changes over longer durations carry with them the cumulative effect of permanent shocks, while the transitory shocks die out, separate identification of the magnitude of the two sets of shocks is possible. Therefore, conditional on the specification, identification of "permanent" income shocks is not problematic, even with panels of limited duration (three years, in our case).

A different question may be raised as to whether income itself is mis-specified, and whether a more elaborate income specification would yield different results. We note that a recent analysis in Hryshko (2012), which provides a comparison of alternate income specifications that include permanent, autoregressive and moving average terms, each estimated using PSID data of significant longitudinal duration (30 years), finds strong evidence for a "permanent" (as opposed to merely persistent) component of income (with estimated autocorrelation coefficients in the range of 0.992 to 1). Moreover, the estimated magnitude of permanent shocks from these more elaborate specifications are similar to those estimated using simply the basic permanent/transitory specification that others (including us) have used. Consistent with this, our own (alternate) analysis allowing for persistent income to be modeled as following an autoregressive AR(1) process yielded estimates of the autoregressive parameter that were insignificantly different from one, implying permanency of shocks.[19] Besides, as we discuss in the next section, our estimates of permanent income shocks are also consistent with a range of earlier estimates in the literature, obtained using data sets of significantly longer duration than the one we have used here.

II.4. Data and Implementation of Estimation Methodology with the SIPP data

Since trade data is only available for the manufacturing sector, we restrict our sample to those workers employed in this sector during the first month of each panel. We assign individuals to those industries in which they were initially observed, and maintain this industry assignment throughout.

The risk estimates from this sample account for both the shocks to workers who experience income changes due to changes in their wage rates or the number of hours in a given job and

[19] Specifically, as in Feigenbaum and Geng (forthcoming), we specify income to be the sum of persistent and transitory components, $u_{t=}\ \omega_t + \eta_t$, with the persistent component given by $\omega_{t+1} = \rho\ \omega_t + \varepsilon_{t+1}$. We obtain estimates of the autoregressive parameter ρ from non-linear least squares estimation of the following moment condition: $Var_{d+1} - Var_d = \rho^d\ (\sigma_\eta^2 + (\rho^2\ - 1))\sigma_\varepsilon^2)$, where $Var_d = Var\ (u_{t+d} - u_t)$. Using this methodology, we obtained estimates of ρ in the range of 0.96 to 0.99.

the shocks to workers who change jobs within or between industries, allowing for intermediate periods of unemployment. Specifically, the sample analogs to $var\ (\Delta_n u_{ijt})$ are formed by estimating (residual) income differences for workers between time periods *t* and *t+n* regardless of their employment status in any intermediate period. While losing a worker from the data set due to unemployment in intermediate periods between *t* and *t+n* will bias the estimate of transitory income shocks, it will not bias our estimate of the magnitude of permanent income risk as long as the individual does not remain unemployed for the remainder of the duration of the panel. In the event that individuals are simply lost from the data set because of unemployment, we would indeed underestimate the magnitude of shocks to income. However, this is not a severe problem here since less that 2% of the individuals in our sample are unemployed as of the last month they were surveyed and the average duration of unemployment for our sample is less than 2 months in all three panels.[20]

II.5. Results

The preceding section provided a detailed description of the general econometric methodology that we use to estimate income risk given longitudinal data on individual incomes. Using this methodology, we estimate the risk parameters, σ_ε^2 and σ_η^2, separately for the three SIPP panels and 18 manufacturing industries in the United States.[21] In this section, we report these risk estimates and note some additional issues that arise in applying this methodology to our data.

Table 2 describes the estimates obtained using our benchmark specification, where transitory shocks are purely transitory and have no persistence at all ($\sigma_{\varepsilon,k=0}^2$) as well as when we allow for transitory shocks of longer duration ($\sigma_{\varepsilon,k=6}^2$ and $\sigma_{\varepsilon,k=12}^2$). As indicated earlier, $\sigma_{\varepsilon,k=12}^2$, obtained after we filter out shocks lasting up to a year, is our preferred estimate.[22]

[20] We also find that the change in attrition rates between panels is not correlated with change in import penetration in our sample. This suggests that attrition due to non-response or to unemployment is not likely to bias our main results on the relationship between income risk and import penetration.

[21] Tobacco Products (SIC 21) and Petroleum Refining (SIC 29) are omitted from our analysis due to an insufficient number of observations on individuals within these industries, resulting in outlier estimates of income risk. Furthermore, not all the right hand side variables used in our empirical specifications linking trade and income risk are available for Industry 21. That said, we should note that our results are not driven by this sample restriction. Conclusions from both our baseline and the more detailed empirical specifications continue to hold (with even larger magnitudes for estimates than reported in this paper) when both of these industries are included, and also if we drop these industries one at a time.

[22] As described in Section II.2, an alternative to specification (1) is to estimate income risk by treating the changes in returns to observable worker characteristics as unpredictable. We explore this alternative by pooling all months, and estimating the Mincer regression for each panel with month fixed effects. We also estimate specification (1) by including individual fixed effects. The risk estimates from these two time invariant Mincer specifications differ very little from those reported in Table 2.

As indicated in Table 2, the mean value of the monthly variance of the persistent shock, $\sigma^2_{\varepsilon,k=0}$, for the 1993 panel is estimated to be 0.0033 (or 0.0396 annualized). For the 1996 and 2001 panels, the corresponding estimates for monthly $\sigma^2_{\varepsilon,k=0}$ are 0.0043 (or 0.0516 annualized) and 0.0052 (or 0.0624 annualized), respectively. The corresponding annualized standard deviations of permanent income growth (calculated as $(12^* \sigma^2_{\varepsilon,k=0})^{1/2}$) are 0.20, 0.23 and 0.25 for the 1993, 1996 and 2001 panels, respectively.[23] Clearly, income risk is rising over time: On average, $\sigma^2_{\varepsilon,k=0}$ rose by 30 percent between the 1993 and 1996 panels and by a further 20 percent between the 1996 and 2001 panels.

Table 2 also reports the summary statistics for the estimates of $\sigma^2_{\varepsilon,k=6}$ and $\sigma^2_{\varepsilon,k=12}$. As expected, allowing for shocks of greater duration, but which are not permanent, lowers our estimates of risk: The mean estimate of the monthly value of $\sigma^2_{\varepsilon,k=12}$ is 0.0014, 0.0025 and 0.0031 for the 1993, the 1996 and the 2001 panels (with corresponding annualized values of 0.0168, 0.03 and 0.0372), respectively. The annualized standard deviations corresponding to these estimates are 0.13, 0.17 and 0.19 for the 1993, 1996 and 2001 panels, respectively.

Since our estimates for $\sigma^2_{\varepsilon,k=6}$ are intermediate in magnitude to the estimates of $\sigma^2_{\varepsilon,k=0}$ and $\sigma^2_{\varepsilon,k=12}$, we simply focus on the estimates corresponding to k=0 and k=12 throughout the rest of the paper. Greater detail on $\sigma^2_{\varepsilon,k=0}$ and $\sigma^2_{\varepsilon,k=12}$ is provided in Table 3, which lists the industry level estimates of these parameters for each of the three SIPP panels. As Table 3 indicates, there is considerable variation in risk estimates across industries and over time. During this period, estimated risk increases significantly for some industries, like Apparel, but remain rather stable for some others like Electronics and Transportation Equipment.

It is informative to compare our estimates of the permanent component of income risk, σ^2_{ε}, with the estimates obtained by the extensive empirical literature on US labor market risk using annual income data drawn from the PSID. Note that our results are estimated using SIPP, a three-year panel for the United States, instead of the PSID data, which has a time dimension of many years. Most of these studies find an average value of around 0.02 for the annual variance σ^2_{ε} (see for instance, Carroll and Samwick (1997), who use PSID data from

[23] We note here that although the very vast majority of our risk estimates are positive, we do obtain two estimates (out of a total of 96 estimates reported here) that are negative and statistically different from zero. In estimating risk, we do not constrain our estimates to be positive, and our interpretation is that the negative estimates presumably reflect a reduction in the cross-sectional variance of residual income taking place in these industries over time for reasons other than those considered in our paper. We note additionally that our results linking trade openness to income risk, as reported in Section III.2 are robust to the exclusion of these industries from our analysis.

1981-1987)) with a value of σ_ε^2 = 0.03 being about the upper bound (see Meghir and Pistaferri, 2004, who use PSID data from 1968-1993). Thus, the average values of our estimates of permanent income risk, especially those that allow for transitory shocks of longer duration, are in line with the estimates that have been obtained by the previous literature on US labor market risk. Furthermore, we have used an additional (fourth) year of data, available for the 1996 panel, to explore the implications of filtering out shocks of even longer duration (18 months and 24 months) from our estimates of income risk. We find that the estimates are relatively stable after 12 months (K=12). Indeed, the point estimates of σ_ε^2 with K=18 months and K=24 months were nearly identical to the estimates of σ_ε^2 obtained with K=12 months, further validating the use of the specification with K=12 as our benchmark estimate of permanent income risk.[24]

We also conduct Monte Carlo analysis to investigate the possibility that using high frequency monthly samples in a context in which the conceptually relevant shocks take place at lower frequencies (say, annually) may result in biased estimates of the magnitude of permanent income shocks.[25] To get to this issue, we generate income data using an autoregressive (AR(1)) process in which innovations to individual income are drawn at annual frequency, while individual income data are observed at monthly frequency. In other words, in the simulated data set, an individual's income changes each year, but is set constant during the 12 months within the year. We generate individual income data on 1000 such individuals. To maintain proximity to the characteristics of our own panel in the frequency dimension, we generate data on individual incomes for 3 years (36 observations with three different values for income per individual). The AR(1) innovations are drawn from a N(0, 0.03) distribution (as the mean value of our estimates of the annualized variance of permanent income is roughly 0.03). Estimating the magnitude of shocks to income using all 36 income observations per individual, while assuming an AR(1) process and averaging the resulting estimates across individuals yields an estimate of risk of 0.0302. Our estimate is statistically

[24] As another check, we use the Panel Study of Income Dynamics (PSID) to evaluate the sensitivity of our estimation methodology to the length of the sample period over which income risk is estimated. Specifically, while the PSID data provides up to 26 years of annual longitudinal data on individuals, we restrict the sample to shorter duration (focusing initially on a 4 year sample, the duration of the longest panel in our SIPP data set) and then compare our estimates from the shorter 4 year sample with those obtained using longer samples of up to 10 years of data over the 1970-1980 and the 1980-1990 period. Although this exercise is being conducted using a different data set and a different time period than the one we use in the paper, the comparison of estimates from the 4 year sample to estimates from the 10 year sample yields interesting, and we believe, relevant, results. On average, estimates of permanent risk using the 10 year sample are only around 10 percent lower than risk estimated using the shorter 4 year sample during both periods. As we have argued in the paper, this again suggests that shocks to income that last for a few years do indeed mostly persist for a much longer time period. Thus, this exercise also reassures us that the quantitative consequence of estimating permanent risk using the limited duration of our data, rather than a longer time series, is likely to be modest.

[25] We are grateful for the editor who suggested this Monte Carlo analysis as an additional robustness check.

indistinguishable and quantitatively very close to the value of the income risk parameter, 0.03, used to generate the simulated data.[26] As an alternative check, we estimate risk using our SIPP data set, but using only data at annual frequency. That is, we only use wage information from months 1, 12, 24 and 36 in estimating risk, and compare those to our monthly estimates from the same dataset. Risk estimated using data at annual frequency is quite close to the estimates with monthly data, but, as expected, has higher associated estimation error.[27]

III. Trade and Income Risk

Our primary motivation in this study is to examine empirically the links between trade and labor income risk. As we have discussed in the introduction, the theoretical literature in international trade has suggested a number of ways in which trade openness can alter labor income risk. For instance, increased import competition, which increases the elasticity of the demand for goods, also raises the elasticity of the derived labor demand, implying a more volatile labor market response to economic shocks. Also, with greater openness, changing patterns of comparative advantage can increase the risk to which domestic workers are exposed. The link between import competition and income risk has intuitive appeal; in the analysis that follows, we will largely focus on imports (specifically, import penetration) as the measure of trade exposure of a sector and study the association between import penetration and income risk.

As we discuss in greater detail later, theory also suggests that labor income risk may be linked to other modes of globalization such as outsourcing and exports, but in a manner that is different from its link to import competition. For instance, Bergin, Feenstra and Hanson (2009) argue that if US offshoring patterns are such that fixed cost activities are retained in the US while marginal cost activities are offshored, domestic volatility may actually fall (as all of the variability in demand, for instance, is absorbed by changes in production activity abroad, while the magnitude of domestic fixed cost activity, by definition, stays fixed). Also while Rodrik (1997) has argued that greater labor market volatility will follow greater import competition, this need not be the case with exports, as greater exports need not result in greater elasticity of demand for goods (and thus production factors). In Section III.4, we

[26] Varying the value of income risk parameter used to generate the income data yields very similar results. For instance, when we draw shocks from a distribution with $\sigma_\varepsilon^2 = 0.09$, we estimate risk to be 0.0907. This estimate is also statistically indistinguishable from the value of the parameter used to generate the data for this exercise, i.e., 0.09.

[27] We also find that the estimated association between trade and risk is qualitatively and quantitatively very similar to the estimates reported in the next section obtained using monthly data.

explore these links empirically as well.

To explore the association between import penetration and income risk, we proceed as follows. The methodology outlined in the previous section provides us with estimates of individual income risk, $\sigma^2_{\varepsilon js}$, for each industry j and SIPP panel s. We now use these time-varying, industry-specific estimates of income risk, in conjunction with observations on import penetration M_{js},[28] to examine the relationship between income risk and import penetration. In Figures 1A and 1B, we plot the changes in estimated permanent income risk, $\Delta\sigma^2_{\varepsilon,k=0}$ and $\Delta\sigma^2_{\varepsilon,k=12}$, against changes in import penetration across panels. More specifically, we plot changes in risk and changes in import penetration between the 1993-95 and 1996-98 panels and between the 1996-98 and 2001-03 panels (with import penetration measured at the beginning of each panel). In each case, for both K=0 and K=12, the relationship appears to be strongly positive, suggesting that an increase in import penetration is associated with an increase in income risk for the workers in that industry.

III.1. Specification

We examine the relationship between income risk, $\sigma^2_{\varepsilon js}$, and import penetration, M_{js}, formally using a linear regression specification that includes industry fixed effects and time fixed effects:[29]

$$\sigma^2_{\varepsilon js} = \alpha_s + \alpha_j + \alpha_M \cdot M_{js} + \upsilon_{js} \qquad (7)$$

In (7), the inclusion of industry dummies, α_j, in the specification allows us to control for any time invariant industry-specific factors that may affect the level of riskiness of income in that industry. Similarly, the time dummy, α_s, controls for any changes in macroeconomic conditions that affect the level of income risk. While this ensures that our estimation results are not driven by changes in macroeconomic conditions (such as business cycle effects and/or long-run structural changes) unrelated to trade, it also means that identification of the relationship between $\sigma^2_{\varepsilon js}$ and M_{js} will have to be based on the differential rate of change

[28] Import penetration is defined as Imports/(Shipments - Exports + Imports).

[29] Our specification examines the association between income risk faced by workers in a sector and import penetration in that same sector. To account for possible general equilibrium effects, one may include additionally on the right hand side the import penetration ratios in the *other* manufacturing sectors. However, controlling for import penetration in other sectors has very little effect, in practice, on the estimates of the relationship between risk and import penetration of the original sector. This is consistent with the pattern of transitions we observe in the data, with the vast majority of workers who leave the manufacturing sector finding employment in the services sector rather than in other manufacturing sectors.

in import penetration across sectors over time. This, however, does not pose problems for our estimation since changes in import penetration over time do in fact exhibit substantial cross-sectional variation. For instance, the change in import penetration between 1993 and 1996 (1996 and 2001) varies between -0.03 and 0.08 (0.004 and 0.09), with a standard deviation of 0.025 (0.0026). Finally, since the dependent variable is estimated rather than measured, we adjust the standard errors for heteroscedasticity using a White correction.[30]

III.2. Estimation Results

The results estimated for our full sample of workers using the specification described above are reported in Table 4. We estimate two separate regressions described by (7), including, separately, import penetration at the beginning of each panel (i.e., for 1993, 1996 and 2001) and import penetration lagged one year (i.e., for 1992, 1995 and 2000). For each specification, the dependent variable is income risk measured either by filtering out purely transitory shocks ($\sigma^2_{\varepsilon,k=0}$) or by filtering out transitory shocks that last up to a year ($\sigma^2_{\varepsilon,k=12}$).

We find that import penetration is significantly and positively associated with income risk in each of the specifications we examine. When only purely transitory shocks are filtered out, the coefficient on import penetration (measured at the beginning of each panel) is estimated to be $\hat{\alpha}_M$ = 0.022. This estimate indicates that an increase in import penetration by 10% of its initial (1993) level would raise $\sigma_{\varepsilon,k=0}$ by a little over 5%. In our preferred specification, when transitory shocks of duration up to a year are filtered out, the coefficient estimate is larger, $\hat{\alpha}_M$ = 0.045. This corresponds to an increase in $\sigma_{\varepsilon,k=12}$ by about 23%. As Table 4 indicates, our estimates change very little when we instead include lagged values of import penetration as the independent variable.[31]

We have also estimated other variants of specification (7) in which we have considered, for instance, the long difference between the 1993-1995 and the 2001-2003 panels and alternately two separate specifications estimated in differences between the 1993-1995 and the 1996-1998 panels and, the 1996-1998 and the 2001-2003 panels. The results are broadly similar to those that we have reported here, affirming a positive association between income

[30] We also use weighted least squares (WLS) to correct for a heteroscedastic error structure, as suggested by Saxonhouse (1976). This correction has little effect on the magnitude or the significance of the coefficients on import penetration reported in the paper.

[31] The coefficient on import penetration remains significant and positive with little change in its magnitude, when the dependent variable in specification (7) is replaced with the risk estimates from the Mincer specifications with time invariant coefficients described earlier.

risk and import penetration.[32] Separately, we have also included in our analysis data from a more recent SIPP panel that covers the years 2004-2007. With the inclusion of this fourth panel, the estimate of coefficient of α_M remains similar to those estimates obtained using just the three earlier panels.[33]

We have also explored whether the source of imports matters for the association between income risk and trade by testing for a differential effect of imports coming from low wage countries,[34] by breaking down imports into imports coming from China and imports coming from the rest of the world and, separately, by breaking down imports into imports coming from a set of low wage countries (as in Bernard, Jensen and Schott (2003), a country is classified as low-wage if its per capita GDP is less than 5% of U.S. per capita GDP) and imports coming from the rest of the world. These decompositions of imports do not appear to matter much for the effect of import penetration on income risk. We find that the coefficient on imports from China (and from low wage countries) is very similar to the coefficient on imports from the rest of the world.

III.3. Selection Bias

One potential concern in the estimation relates to the possibility that workers of different types may self-select into particular industries. Suppose, for example, that industries with high levels of import penetration are also industries with high job destruction rates. Suppose further that there are two types of workers, Type I and Type II, and that Type I workers quickly find a new job in the event of job displacement, but Type II workers do not. Other things being equal, we would expect Type II workers to move to low import penetration industries (or, over time, to industries in which import penetration has increased to a smaller extent relative to other industries).

This type of self-selection, if present, would actually bias our results against finding a positive

[32] Estimates in long differences are comparable in magnitude to those reported in Table 4. When we repeat the difference estimates by breaking down the sample into two periods (1993-95 and 1996-98; 1996-98 and 2001-03) we find the estimated impact of import penetration to be larger in magnitude for the first period compared to the latter period. We note that the standard errors are larger in this case (and the estimated coefficient statistically insignificant for the second period), which is perhaps to be expected as the difference estimates are identified off of only 18 observations.

[33] Specifically, the estimated coefficient on import penetration is always positive and significant; it is virtually identical for the K=0 specification while it declines modestly to 0.024 in the K=12 specification. While data from the 2004 SIPP panel were available, we have nevertheless reported results in the paper using only the three earlier panels, since a number of other data series that we have used in the robustness exercises (for instance, in Table 7) are not readily available for the 2004-2007 time period (in part due to changes in industrial classification systems these data series have been reported during this time period).

[34] We are grateful to a referee for suggesting this exercise.

association between income risk and import penetration, thus strengthening the results we find in the paper. Nevertheless, we consider the possibility of selection in the data and find that our concerns regarding selection bias are mitigated for the following reasons. First, we examine industries over time, so any fixed differences across industries in the composition of the workforce or worker characteristics are taken into account by our fixed effects estimation. Second, we test whether the distribution of workers within an industry is related to change in import penetration in our data. We find that changes in share of each *educational* category, share of each *occupational* category, share of each *race* group, share of each *gender* and, finally, the average *age* within a sector are each uncorrelated with changes in import penetration across the span of the three SIPP panels. In addition, we find that the changes in the *variance* of years of education and age within a sector across panels are also uncorrelated with changes in import penetration. Third, even when we allow income risk to vary with age and education within industries in a specification analogous to (7), we continue to find the coefficient on import penetration to be significant. More specifically, in this part of the analysis, we first estimate risk separately for each age group and education level within an industry for each panel.[35] Then we estimate equation (7) by including dummy variables for each age group in addition to import penetration and, time and industry fixed effects. We then repeat this analysis for workers with different education levels. In both cases, the coefficient on import penetration remains significant and positive, with little change in its magnitude. This suggests that controlling for the (potentially changing) age and educational composition of sectors does not alter our findings regarding the link between trade and income risk.

Finally, we examine the possibility that selection is based on unobserved ability differences across workers. In this case, we would expect selection to be reflected in unexplained wage differentials across industries, as long as high-ability workers are paid higher wages. Our data suggests that any bias due to such unobserved 'ability' differentials (that are uncorrelated with observable characteristics) across industries is likely to be small. Specifically, we find that changes in unexplained portion of industry average wages are uncorrelated with changes in import penetration in our data, further mitigating our concerns regarding selection bias.

III.4. Endogeneity Bias

An important concern with our estimation of equation (7), which relates import penetration

[35] We estimate risk by industry for five age groups (less than 30, between 30 and 40, between 40 and 50, and above 50) and for five education categories (high school drop-outs, high school graduates, college drop-outs, college graduates).

to income risk, is that import penetration may not be fully exogenous to income risk. Specifically, there could be a number of other time-varying industry-specific factors that are correlated with both income risk and import penetration simultaneously, implying that OLS estimation of (7) will yield biased estimates of α_M.

To deal with the endogeneity of import penetration, we use two alternate approaches. First, we use a two stage least squares (2SLS) approach, using the sectoral volume of exports from China to high-income OECD countries other than the US as an instrument for sectoral import penetration in the US. Increased Chinese exports to OECD countries (other than the US) as a result of increased competitiveness of Chinese producers (due to accession to WTO, reduction of trade barriers, technological change and macroeconomic reforms) provides plausibly exogenous variation in sectoral import penetration in the US that is unrelated to idiosyncratic income risk in that sector in the US. This approach is similar to Autor, Dorn and Hanson (2012) who use exogenous variation in industry level Chinese import growth in other high-income countries in their analysis of import competition on US local labor markets.

The estimates from the instrumental variable specification are presented in Table 5. The first stage of the estimation suggests that Chinese exports to the non-US OECD countries are closely related to US import penetration: The estimated coefficient is significant at least at the 5% level with F statistic of 15.96 (and 13.39 for the specification with lagged explanatory variables). Importantly, idiosyncratic income risk continues to be significantly related to import penetration. Indeed, as anticipated by our earlier discussion, the IV estimates are slightly higher than the OLS estimates reported in Table 5 (and comparable to those reported in Table 7 from the specification including a wide array of time-varying industry controls). Thus, our results indicate that accounting for the endogeneity of import penetration using an instrumental variable does not attenuate the magnitude or significance of the estimated coefficient. [36]

Next, we include on the right hand side of equation (7) a substantial number of time-varying industry-specific factors (Z) that could be correlated with both income risk and import

[36] We should also note that we have experimented with a wider set of instrumental variables. For example, in an attempt to exploit variation in protection due to the implementation of NAFTA, we have instrumented industry-level import penetration in the US with US tariffs on Mexico and Canadian tariffs on Mexico. In both cases the coefficient on import penetration in the second stage is significant and positive pointing to a positive association between import penetration and income risk. However, we have chosen not to report these results in the paper since the first stage of the estimation is weak with an F-statistic only slightly above 5. In our view, the argument for exogeneity is strongest in the case of variation in Chinese export supply and given the strong first stage, we only report estimation results using this instrument in the paper.

penetration simultaneously and, whose omission could potentially result in biased estimates:

$$\sigma^2_{\varepsilon js} = \alpha_s + \alpha_j + \alpha_M \cdot M_{js} + \alpha_Z \cdot Z_{js} + \upsilon_{js} \qquad (8)$$

We first include on the right hand side the share of exports in total sales. If the risk faced by individuals employed in the export sector is lower, and exporting industries face lower import competition, then the omission of this variable could lead to an overestimation of the coefficient on import competition. A second concern is that industries with high levels of final good imports tend to engage in higher levels of offshoring. On the one hand, offshoring could insulate domestic workers from output volatility by shifting the non-core activities of an industry abroad and hence decreasing risk for those who remain (Bergin, Feenstra and Hanson, 2009). On the other hand, increased offshoring could result in higher income risk by increasing labor demand elasticity due to increased substitutability between foreign production and domestic workers. To explore this issue, we include the share of imported intermediate inputs as a measure of offshoring. Third, if industries respond to increased import competition by investing in information and communication technologies (ICT) and if such technology increases the risk faced by workers (for example, by increasing their substitutability with machines), this would lead to an upward bias in our coefficient of interest. Fourth, we include labor productivity against the possibility that a negative productivity shock in an industry could simultaneously lead to an increase in both import penetration and in income risk. Fifth, omission of union density could bias our estimates if union density changes in response to increased import competition and if higher unionization rates are associated with lower levels of risk. Finally, sectoral employment growth and employment volatility are included to account for the possibility that the volatility and growth rate of an industry could simultaneously impact both the import penetration in that industry as well as the income risk faced by workers employed in these industries.[37] In Table 6, we report the summary statistics for each of these variables calculated at the beginning of each panel.[38]

We report our estimation results in Table 7. As before, each specification reported includes industry and year fixed effects. All explanatory variables are measured as of the first year of each panel (columns 7-13) and in one-year lags (columns 1-6). For brevity, we report the

[37] We are grateful to a referee for suggesting this exercise.

[38] With the exception of employment growth and employment volatility, all variables are constructed using annual data and enter into the specification as of the beginning of the panel or one year lagged. Annual employment growth and volatility are calculated from monthly data during the panel period. Our results are unchanged if employment growth and volatility are instead entered in the regression as of the beginning of the panel or one year lagged.

results with our preferred income risk estimates (allowing for transitory shocks that last up to a year) as the dependent variable. In columns (2) and (8), we include the share of exports in addition to the share of imports. The coefficient of import penetration remains significant and positive with little change in its magnitude. The coefficient of exports is insignificant suggesting a differential effect of openness on labor income risk across import competing and exporting sectors. As we have mentioned earlier, greater exports may not result in greater income risk, since greater exports, unlike greater imports, are not necessarily associated with an increase in product demand and factor demand elasticities.

The inclusion of offshoring leads to an increase in the coefficient of import penetration. In the specifications reported here, the offshoring variable is itself significant and negative, suggesting that an increase in offshoring in an industry is associated with a decline in income risk in that industry. This is consistent with the argument of Bergin, Hanson and Feenstra (2009) that outsourcing in an industry may well result in a reduction in labor demand volatility in that firm or industry. Specifically, if outsourcing patterns are such that fixed cost activities are retained in the US, while marginal cost activities are outsourced, domestic volatility may actually fall (as the variability in demand is absorbed by changes in production activity abroad, while the domestic fixed cost activity, and thus domestic labor demand, stays stable, as we have previously discussed). The inclusion of ICT, labor productivity, union density, sectoral employment growth and sectoral employment volatility does not affect the coefficient on import penetration.[39,40] The estimated coefficients on none of these additional variables, except employment growth, is statistically significant. The estimated coefficient on employment growth is negative which suggests that a higher level of employment growth is associated with a lower level of income risk in that industry, which conforms with basic intuition.

Note that the "additional" variables discussed above are mostly variables that relate to the "supply side" of the economy. It could be argued that demand shocks may also potentially bias the estimate of the relationship between trade and income risk. Thus, there may be say, positive shocks to product demand in a sector that lead to greater import flows and raise

[39] In specifications not reported here, we also consider the effect of including the share of foreign multinationals (MNE) in total industry employment. Exclusion of this variable could lead to an upward bias in the magnitude of the coefficient of import competition if an increase in MNE share is associated with a decrease in imports in that industry and if employment in such firms is more stable than that of domestic firms. Since the MNE measure comparable across time is available until 1996, we check the robustness of our results to the inclusion of this variable for only the 1993 and 1996 panels. We find that the coefficient on import penetration remains positive and significant, while the coefficient on MNE share is insignificant.

[40] We also estimate Equation (7) by including each additional explanatory variable one-by-one along with import penetration. In each of these specifications, the coefficient on import penetration remains significant with little or no change in its magnitude.

both the mean and the variance of income in that sector. While this is indeed a theoretical possibility (for instance, if innovations to permanent income are related to industry prices), we do not find support for this argument in our data. We find the correlation between mean income changes and income risk to be insignificantly different from zero. Furthermore both the correlations between mean income changes and import penetration and between sectoral prices and income risk are not significantly different from zero, mitigating this as an empirical concern.[41]

Thus, the relationship between income risk and import penetration remains significant when we account for the endogeneity of import penetration by implementing an instrumental variable approach and when we control for a range of time-varying industry specific factors that are potentially correlated with both income risk and import penetration. We believe that these results allow us to credibly assert a causal link between import penetration and income risk.

IV. Trade and Income Risk in Sub-Samples

Our dataset is sufficiently large to estimate risk faced by sub-samples of workers who experience a range of different outcomes in the labor market. Our first sub-sample is constructed by including only the individuals who were employed in the same manufacturing industry in each month that they were employed (and surveyed).[42] This sample (denoted STAY-IND) includes workers who remained in the same job as well as those who switched jobs within the same industry (thereby possibly losing returns to firm or occupation specific human capital). Displaced workers who move to a different manufacturing or non-manufacturing industry are excluded from this sample and are instead grouped together in a different sample (SWITCH-ALL). We restrict the SWITCH_ALL sample to those individuals who switched to the non-manufacturing sector for at least one period in the

[41] Another possible concern relates to the endogenous choice of trade policies. While the large theoretical and empirical literature on the political economy of trade policy has not directly studied income risk as a determinant of cross-sectional variation in trade policy, it is possible that trade policy, which affects import penetration, may itself be endogenously determined by income risk in the sector. Consider an "equity" minded government that uses trade policy to reach its goal of equalizing welfare across individuals in this economy. This government will choose high (low) protection levels for those industries with intrinsically high (low) levels of income risk, in order to say, increase (decrease) the mean level of wages in these industries. Nevertheless, our fixed-effects estimates of α_M, identified by within-industry variation, will not be biased due to such cross-sectional variation in the determinants of trade policy. But it is also plausible that this government could increase (decrease) protection and lower (raise) import penetration in industries that experience an increase (decrease) in income risk. If this is the case, such endogeneity of policy will bias our estimates of the relationship between income risk and import penetration, α_M, downwards (i.e., towards not finding a positive relationship between trade and risk) and therefore strengthen the results presented in this paper.

[42] In constructing these sub-samples, an industry is defined according to the Census of Population Industry Classification System, which includes 235 industry categories, 82 of which are in the manufacturing sector.

panel to construct the SWITCH-NON-MANUF sample. Our last sub-sample includes individuals who stayed in the manufacturing sector throughout, but may have worked in a different industry within manufacturing than their original industry at some point (STAY-MANUF).

Differences in income risk experienced by workers in these four different sub-samples (STAY-IND, STAY-MANUF, SWITCH-ALL and SWITCH-NON-MAN) reflect the costs of switching industries both within and outside manufacturing. However, the interpretation of the differences in risk estimates across different sub-samples is subject to an important qualification. In principle, whether a worker remains in the same manufacturing industry or switches to another industry is an endogenous decision. The allocation may be non-random and may very well reflect differences in worker characteristics such as their level of human capital. Therefore, we cannot infer that a worker who did not switch sectors (i.e., a worker in STAY-IND) would face the level of income risk estimated for the sub-sample of workers who did switch sectors (say SWITCH ALL) if he was exogenously forced to switch. The estimates of income risk for the different groups of workers are conditional estimates, reflecting the differences in worker characteristics and endogenous actions that place workers in these different sub-samples, and should be interpreted with this qualification in mind.[43] Importantly, the risk estimates for the full sample of workers, and for the switcher and non-switcher samples are conceptually different. The risk estimates that are obtained with the full sample reflect the risk faced by a worker who do not have information on whether or not he will be involved in a switch in the current period, to which sector he might switch and what the associated wage change will be. On the other hand, the switcher sample yields an estimate of risk that we interpret as being conditional on the worker's knowledge that he will be involved in a switch, but without knowing *ex-ante* which sector he will be switching to and what the associated income change will be.

Table 8 provides a summary description of our estimates of income risk for each sub-sample and panel.[44] As Table 8 indicates, $\sigma^2_{\varepsilon,k=0}$ continues to be greater than $\sigma^2_{\varepsilon,k=12}$ in each of the

[43] In practice, there is little systematic difference in the observable educational characteristics of switchers relative to non-switchers in our data. The fraction of switchers who fall in different educational categories (high school dropout, high school graduate, some college, college graduate, more than college) is nearly identical across switchers and non-switchers. While, these two samples reflect some differences in other observable characteristics, these differences are rather small (switchers are slightly younger with an average age of 36 years compared to 39, and slightly less likely to be married, 59% versus 64% married compared to non switchers). These empirical facts somewhat mitigate our concern regarding the comparison of income risk estimates across different sub samples. Nevertheless, we cannot rule out the possibility that the workers in these sub-samples are different in terms of unobservable characteristics and therefore, the estimates we discuss below are still subject to the qualification regarding the non-random allocation of workers discussed above.

[44] Due to sample size restrictions, income risk for these sub-samples are estimated at the 2-digit SIC level which is more aggregated than the Census classification used in constructing the sub-samples.

sub-samples. Note that income risk for those who stayed in the same manufacturing industry throughout the sample (STAY-IND) is the lowest, possibly as these workers continue to earn returns on their industry-specific skills, even if they switch jobs within the sector. The mean estimate of the monthly value of $\sigma^2_{\varepsilon,k=12}$ for this sub-sample increases from 0.0008 to 0.0021 between the 1993 and 1996 panels, and then rises to 0.0025 in the 2001 panel. The corresponding annualized standard deviations are 0.098, 0.159 and 0.173 for the 1993, 1996 and 2001 panels, respectively. The risk faced by workers in STAY-MANUF, who stay within manufacturing throughout but may have switched from one industry to a different industry at some point in time, are close to (but in almost all cases higher than) the risk faced by workers in STAY-IND. Workers in SWITCH-ALL who have switched to jobs in either a different industry within the manufacturing sector or to the non-manufacturing sector, face higher levels of risk. As Table 6 indicates, the monthly variances for this group are 0.0029, 0.0030 and 0.0033 (with corresponding annualized standard deviations of 0.19, 0.19 and 0.20) for the 1993, 1996 and 2001 panels, respectively. Table 8 also provides estimates of income risk experienced by workers who switch out of manufacturing (SWITCH-NON-MANUF). The variance in shocks to permanent income experienced by these workers is significantly larger (by at least fifty percent) than those who stayed in the same industry throughout.

In order to evaluate the effects of international trade on workers in different sub-groups, we next estimate specification (7) separately for each sub-sample. As discussed earlier, the interpretation of the differences in risk estimates across different sub-samples is subject to the important qualification that the switching decision itself is an endogenous one and the estimates of income risk for the different groups of workers are conditional estimates, reflecting the actions that place workers in the different sub-samples. Nevertheless, this exercise is potentially useful in understanding the relationship between international trade and income risk as experienced by different segments of the labor market. When we are examining income risk conditional on switching (i.e., workers know that they will switch), we do not have a strong prior that the coefficient on import penetration should be different from zero, even taking as given the result that greater import penetration is associated with higher income risk for the full sample of workers. Namely, conditional on switching, one may argue that risk for switchers should be the same in high import penetration industries and in low import penetration industries. Or alternately, it may be argued that risk should be higher in high import penetration industries, because of labor market congestion for job seekers as a larger number of workers with similar skills leave these industries at the same time and, experience greater variance in outcomes as a result. Similarly, it is possible that greater exposure to imports results in greater instability for those workers who stay within the sector, as there may be within-sector (across firm) displacements (and associated loss of

return to firm-specific human capital) that result in income changes even for these workers.

The results from specifications with $\sigma^2_{\varepsilon,k=0}$ and $\sigma^2_{\varepsilon,k=12}$ as the dependent variable are reported in Table 9A and Table 9B, respectively. The first two columns of Table 9A report the results using income risk estimates $\sigma^2_{\varepsilon,k=0}$ for the sub-sample STAY-IND as the dependent variable. When values of import penetration at the beginning of the panel are used as the explanatory variable, our estimates suggest that for workers who stayed in the same industry throughout, $\sigma_{\varepsilon,k=0}$ would increase by 5% as a result of a 10% increase in import penetration over its initial value. When $\sigma^2_{\varepsilon,k=12}$ is the dependent variable, we find that the same increase in import penetration would result in an increase in $\sigma_{\varepsilon,k=12}$ of about 27% percent. The next two columns report results for workers in the sub-sample STAY-MANUF, which includes workers who stay within the manufacturing sector (in the same industry or moving to another industry within manufacturing). Our estimates suggest that for this group, a 10% increase in import penetration is associated with an increase in $\sigma_{\varepsilon,k=0}$ and $\sigma_{\varepsilon,k=12}$ by about 5% and 22%, respectively.

Next, we focus exclusively on workers who switch industries. For the two sub-samples we consider here (SWITCH-ALL and SWITCH-NON-MANUF), the estimated coefficient on import penetration is positive in each specification but significant only when $\sigma^2_{\varepsilon,k=12}$ is the dependent variable. We find that a 10% increase in import penetration is associated with an increase in $\sigma_{\varepsilon,k=12}$ of 18% for workers who switch sectors (either within or outside the manufacturing sector) and of 22% for workers who switch to the non-manufacturing sector.

Overall, these results indicate that even conditional on switching, greater import penetration is associated with greater income risk.

V. Welfare

The preceding sections have focused on estimating the relationship between trade exposure and income risk. We now turn to the analysis of the link between income risk and welfare using a simple dynamic model with incomplete markets and (exclusively) permanent income shocks, developed by Krebs (2004) and implemented in Krebs, Krishna and Maloney (2010). The model is tractable enough to permit closed-form solutions for equilibrium consumption and welfare, yet rich enough to provide a tight link to the empirical analysis we have outlined. We strongly emphasize that our goal here is not to provide a complete assessment of the effects of international trade on welfare, but rather to obtain indicative estimates of

welfare changes operating exclusively through the income risk channel. [45]

The theoretical set-up (with details provided in the Appendix) features *ex-ante* identical and long-lived workers with CRRA (Constant Relative Risk Aversion) preferences, who make consumption choices in the face of uninsurable permanent income shocks.[46] Workers are allowed to save, but are not able to borrow to smooth consumption in the face of labor income shocks. The economy is a closed exchange economy, so that any of the choices made by agents do not affect aggregate output. In the equilibrium analyzed by Krebs, Krishna and Maloney (2008), the interest rate in the economy adjusts endogenously to ensure that consumption equals income in each period for each worker (i.e., there is no saving and no smoothing of consumption in the face of permanent income shocks, consistent with the intuition of the permanent income hypothesis).[47] Under this structure, closed form solutions may be obtained for expected lifetime utility as a function of the variance of shocks to permanent income (and thus for changes in lifetime utility as a function of changes in the magnitude of the variance of these shocks).

More specifically, the thought experiment that the theoretical structure allows us to address is the following one: Imagine a group of workers facing an income process with variance of permanent income risk σ_s^2, who now experience an increase in permanent income risk measured by Δ_σ, so that $\sigma_{s'}^2 = (1 + \Delta_\sigma)\sigma_s^2$ is now the risk to income that they face forever going forward. What is the welfare effect of this increase in risk, in compensating variation terms?

It can be shown that the percent change in consumption Δ_c, in each period and each state of

[45] While our focus in this paper is on the welfare effects of international trade solely through the income risk channel, we have also explored the relationship between mean growth rates of (raw and residual) income and import penetration (using econometric specifications like (7), with income growth on the left hand side rather than income risk). However, we did not find any consistent relationship between mean growth rates of (raw and residual) income and import penetration.

[46] We should note that, as a practical matter, not allowing insurance against permanent labor income shocks might not be too restrictive as direct insurance against labor income shocks is generally not available to workers. More importantly, our results concerning either the estimates of permanent income risk or its links with trade do not change significantly when total income (including any capital earnings and transfers) instead of labor earnings is used as our income measure. Nevertheless, it should be recognized that in theoretical contexts in which full insurance is available, increased variability in wages might not result in a welfare loss.

[47] While this is an evolving literature, a number of studies report a high marginal propensity to consume out of permanent income shocks. For instance, Kaplan and Violante (2009) report, using simulation results, that for households that face borrowing constraints, the marginal propensity to consume out of permanent income shocks is 0.93. In contexts in which consumers accumulate buffer stocks of savings, Carroll (2001) reports a marginal propensity to consume out o permanent income shocks of about 0.9. We believe that these values of the marginal propensity to consume out of permanent shocks are close enough to 1 to broadly validate our theoretical set up with no saving in equilibrium.

the world, required to compensate the individual for the change in risk Δ_σ is given by:[48]

$$\Delta_c = [\frac{1-\beta(1+\mu)^{1-\gamma}\exp(.5\gamma(\gamma-1)(1+\Delta_\sigma)\sigma_\varepsilon^2)}{1-\beta(1+\mu)^{1-\gamma}\exp(.5\gamma(\gamma-1)\sigma_\varepsilon^2)}]^{\frac{1}{1-\gamma}} - 1, \qquad if \quad \gamma \neq 1 \qquad (9)$$

and

$$\Delta_c = \left(\frac{\beta\Delta_\sigma\,\sigma_\varepsilon^2}{2(1-\beta)^2}\right) - 1, \qquad if \quad \gamma = 1$$

where β is the pure discount factor, γ the coefficient of relative risk aversion, μ the mean growth rate of income and σ_ϵ^2 the estimated variance of the permanent component of labor income shocks.

The welfare expression (9) has standard properties. With $\gamma > 0$, individuals are risk averse and risk is costly. That is, an increase in risk, $\Delta_\sigma > 0$, requires positive compensation, Δ_c for the individual to be just as well off as before. The magnitude of this compensation is increasing in the degree of risk aversion, γ. Using (9) along with estimates of change in risk associated with trade, Δ_c (from Sections III.2), and standard values for the parameters β and γ, we could obtain indicative estimates of the benefits or costs of trade through the income risk channel.

The welfare expression (9) is derived under the assumption that the increase in permanent income risk, Δ_σ associated with the increase in import penetration lasts forever. Similarly, specification (7) is a "long-run" specification associating a higher level of import penetration with a higher level of income risk.[49] However, since our data on income risk and import penetration spans only a 10 year period (between 1993-2003), our estimates, strictly speaking, do not allow us to reject the hypothesis that changes in income risk associated with changes in import penetration do not last longer than 10 years. We therefore conduct the quantitative welfare analysis by allowing for income risk to be higher with higher import penetration for a period of T = 10 years, while also reporting calculations for T = 5 (shorter duration) and 15 years (longer duration).[50]

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[48] The interested reader is referred to Krebs, Krishna and Maloney (2008, Appendix Pages: 39-41) for detailed derivations and discussion.

[49] To explore whether the increases in income risk we estimate in (7) are indeed "long-run" changes, we have also estimated variants of specification (7) by including changes in import penetration in preceding periods on the right hand side. We find that while the coefficient on the level of import penetration remains unchanged, the lagged (1 and 2 year) changes in import penetration, capturing purely "short-run" effects, are not significant.

[50] Note that even when the increase in permanent income risk with greater import penetration lasts only for a temporary period of T years, any shocks to worker incomes ε_{ijt} have permanent effects. Specifically, when

The welfare change corresponding to a change in the variance of the permanent income shocks (income risk) for T years is given by:

$$\Delta_c = [\frac{(1-x)(1-(x')^{T+1})}{(1-x')+x(x')^T}]^{\frac{1}{\gamma-1}} - 1, \qquad if\ \gamma \neq 1 \qquad (10)$$

and

$$\Delta_c = \left(\frac{\beta(1-\beta^T)\Delta_\sigma\ \sigma_\varepsilon^2}{2(1-\beta)^2}\right) - 1, \qquad if\ \ \gamma = 1,$$

where,

$x = \beta(1+\mu)^{1-\gamma} \exp(.5\gamma(\gamma-1)\sigma_\varepsilon^2)$

and

$x' = \beta(1+\mu)^{1-\gamma} \exp(.5\gamma(\gamma-1)(1+\Delta_\sigma)\sigma_\varepsilon^2)$

Table 10 provides welfare calculations using our preferred income risk estimates, $\sigma^2_{\varepsilon,k=12}$ as well as results when income risk is estimated assuming K=0 ($\sigma^2_{\varepsilon,k=0}$). Results are provided separately for parameter values for the coefficient of risk aversion at γ = 1 and γ = 2 and for durations of T = 5, 10 and 15 years. The calculations in the top panel of Table 10 use a discount factor β = 0.98. With γ= 2, for our central set of risk estimates with K=12, the increase in persistent income risk associated with a 10 percent increase in import penetration is certainty equivalent to a reduction in lifetime consumption in the range of 4.2 percent to 11.3 percent. In Table 10, we also report welfare estimates corresponding to a lower level of risk aversion, γ = 1. As expected, welfare costs are smaller when individuals are less risk averse and are in the range of 2 percent to 6 percent. Clearly, in both cases, the welfare costs associated with the income risk channel are economically quite significant.[51]

The welfare analysis we have presented may be qualified along several different dimensions. First, we may examine the implications of a theoretical set up with infinitely long-lived workers, while, in reality, individuals only have finite working lives. In this case, the welfare costs we have just computed may be adjusted downward using a simple (although admittedly very rough) approximation. Specifically, we consider lower values of discount factor β, so that the cost of lower income in later periods is much more significantly discounted, roughly

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permanent income risk rises for duration of T years, workers draw their *permanent* income innovation terms ε_{ijt} in (3) from a bin with greater variance $\sigma^2_{\varepsilon'}$ than before, for duration T, before returning to a bin with the original level of σ^2_ε.

[51] Indeed, even results for T=1 and T=3 years, i.e., for a very short run increases in risk following an increase in import penetration, are economically significant (specifically, for T=3 years, we have a welfare cost of 1.33 percent of lifetime consumption for γ=1 and 2.6 percent for γ =2 in our benchmark (K=12) case. With T=1, the corresponding numbers are 0.45 percent and 0.89 percent respectively.

imitating a "finite life". For instance, under our benchmark value of $\beta = 0.98$, utility in year 30 after a shock is presently discounted by a factor 0.98^30 = 0.545, while with $\beta = 0.85$, the discount factor is 0.85^30 = 0.007. The lower panels of Table 10 present the "adjusted" welfare estimates for a lower range of values of β (from 0.95 to 0.85). As expected, we obtain smaller welfare costs with smaller values of β. For instance, with $\beta = 0.85$, welfare costs are in the range of 1.45 to 2.4 percent with $\gamma = 1$ and in the range of 2.85 to 4.62 percent with $\gamma = 2$. Clearly, the magnitude of the welfare costs remains economically significant in each case.[52]

Second, we may ask about the extent to which the additional risk being borne by workers is risk that they "seek" in exchange for higher mean compensation that they receive. To address this issue, we examine whether mean income changes are positively correlated with changes in import penetration. We find that the two are uncorrelated in our data, a finding that we additionally confirm with regression analysis (with time and industry dummies included on the right hand side). Furthermore, mean wages are uncorrelated with income risk itself. This suggests that the risk we measure is indeed "borne" by workers rather than being sought by them in exchange for greater mean compensation.

Third, we may ask about the plausibility of the quantitative estimates of welfare costs delivered by our welfare-theoretic framework. As discussed above, our welfare estimates are simply measures of the "willingness to pay" to avoid the higher risk associated with greater exposure to trade. Seen in this light, and given our estimates regarding the magnitude of the association between income risk and trade, our estimates seem quantitatively plausible especially when computed with lower magnitudes of parameters γ and T. Furthermore, it should be clear that these estimates only provide an indicative sense of what our risk estimates translate into in welfare terms. Among other things, we have ignored several potential dimensions of "adjustment" which may allow workers to lower the cost of risk, such as the labor-leisure choice or intra-household diversification of sector and occupation.[53] The inclusion of these channels is outside of the scope of the present analysis and is left for

[52] We are grateful to an anonymous referee for suggesting this exercise. This same exercise may also be seen as achieving a very rough evaluation of the costs of higher income risk, if, notwithstanding our estimation framework, shocks to income, estimated as permanent, are nevertheless less than fully permanent. A separate possibility is to scale down all estimates of σ_ε^2 by a common factor, say three quarters or one-half, and ask what the welfare costs would be of an increase in import penetration by ten percent. In this case, our calculations suggest that the welfare costs would be lower by a factor slightly greater than three quarters (in the range 0.69 to 0.75) and slightly greater than one-half (in the range 0.46 to 0.49) respectively, than our original estimates. Clearly, these continue to be economically significant magnitudes.

[53] However, we have considered the possibility that workers may make asset ownership choices that allow them to reduce risk by considering "total" earnings rather than just labor income. As noted earlier, this does not make any significant difference to our results.

future research.

VI. Conclusions

This paper studies the links between international trade and individual income risk using longitudinal earnings data on workers in the United States. Our results suggest that increased import penetration has a statistically and economically significant effect on labor income risk in US manufacturing. We find that within-industry changes in income risk are strongly related to changes in import penetration for the full sample of workers as well as various sub-samples we consider, such as workers who stayed within the same manufacturing industry throughout and those who switched industries within or outside the manufacturing sector. Our welfare analyses suggest that the welfare cost of increased income risk associated with increased trade exposure is economically significant.

We emphasize that our analysis has focused exclusively on the links between trade exposure and income risk. Our results should be considered alongside the findings of a large literature on international trade, which has explored the many ways in which exposure to trade may positively affect the economy. Our finding of economically significant negative effects through the income risk channel does not suggest that the gains from trade are negative overall. It indicates instead that the income risk channel should be considered seriously in exercises evaluating the overall gains from trade. While the theoretical literature offers some suggestive ideas regarding the link between trade and labor income risk and of possible asymmetries in effects through different modes of globalization, such as imports, exports and outsourcing, a unified theoretical framework considering these issues is as-yet lacking. While the analysis presented in this paper is entirely empirical in nature, we modestly hope that our findings will serve to guide the development of theoretical models that consider links between trade and labor income risk.

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Appendix

A. Trade Openness and Factor Demand Elasticities

To explore the question of whether an increase in labor demand elasticity follows trade openness, Krishna, Mitra and Chinoy (2001) consider monopolistically competitive firms facing the following inverse demand functions:

$$P_{ij} = \theta \overline{P}_j Q_{ij}^{-1/\epsilon}$$

where P_{ij} is firm *i*'s own price, $\overline{P}_j$ is the average price at industry *j*, Q_{ij} denotes firm *i*'s output and ϵ denotes the price elasticity of demand. Further, output is given by:

$$Q_{ij} = \prod_{k=1}^{n} V_k^{\alpha_k}$$

where V_k denotes the kth input to production.

In this setting, Krishna, Mitra and Chinoy (2001) show that the derivative of the absolute value of the price elasticity of input demand (δ_l) with respect to product demand elasticity (ϵ) is given by:

$$\frac{d\delta_l}{d\epsilon} = \frac{\alpha_1}{\epsilon^2\left(1-\left(1-\frac{1}{\epsilon}\right)\left(\sum_1^n \alpha_k\right)\right)^2} > 0$$

That is, factor demand elasticities are increasing in product demand elasticities. Since product demand elasticities can be expected to increase with trade openness, this implies that factor demand elasticities will also increase with trade openness.

B. Income Risk and Welfare

Here we provide some details of the welfare theoretic setup from which we obtain the welfare expressions that we have used in our quantitative analysis. We describe below the main assumptions of the analysis and provide some intuition regarding its results and refer the reader to Krebs (2004) and its implementation in Krishna, Krebs and Maloney (2010) for further details.

Krebs, Krishna and Maloney (2005) model long-lived workers who make consumption and

saving choices in the face of uninsurable permanent income shocks. The economy is a closed exchange economy, so that any of the choices made by agents do not affect aggregate output. The assumptions underlying the analysis are as follows:

1. Suppose income of worker *i* employed in industry *j* in period *t* is denoted by $\tilde{y}_{ijt}$. Income is random and defined by an initial level $\tilde{y}_{ij0}$ and evolves as:

$$\tilde{y}_{ijt+1} = (1 + \mu_{jt+1})(1 + \theta_{ijt+1})y_{ijt}$$

where μ_{jt+1} is a mean growth-rate effect common across workers in the sector and θ_{ijt+1} is an individual-specific shock to the growth rate of income. It is assumed that $\log(1 + \theta_{ijt+1})$ is normally distributed with time- and industry-dependent variance σ^2_{jt+1}. Although the distribution of individual-specific shocks may change over time, the shocks are unpredictable in the sense that current and future shocks are uncorrelated. To ensure that workers are *ex-ante* identical, it is assumed that the distribution of shocks is identical across workers.

2. Each worker begins life with no initial financial wealth. Workers have the opportunity to save at the common interest rate r_t, but they cannot borrow. Hence, the sequential budget constraint of worker *i* is:

$$a_{ijt+1} = (1 + r_t)a_{ijt} + \tilde{y}_{ijt} - c_{ijt}, \qquad a_{ijt} \geq 0, \quad a_{ij0} = 0$$

Here c_{ijt} denotes the consumption of worker *i* employed in industry *j* in period *t* and a_{ijt} denotes his asset holdings at the beginning of period *t* (excluding interest payments in this period).

3. Workers have identical preferences that allow for a time-additive expected utility representation:

$$U = E[\sum_{t=0}^{\infty} \beta^t \; u(c_{ijt})]$$

where the one-period utility function, $u(c)$, is given by

$$u(c) = \frac{c^{1-\gamma}}{1-\gamma}, \;\; \gamma \neq 1 \quad or \;\; u(c) = \log(c) \;\; ,$$

that is, preferences exhibit constant degree of relative risk aversion γ.

As Krebs, Krishna and Maloney (2005) show, given these assumptions, one can obtain an equilibrium in which the interest rate is low enough so that no savings are induced from any worker, domestic asset markets clear, and it is optimal for consumption to equal income in each period for each worker. We should emphasize that the result that consumption equals income in each period for each agent follows directly from the maintained assumptions that the country is in financial autarky, so the saving instruments are in zero net supply, that agents cannot be net borrowers, i.e., cannot hold negative net asset positions and that everybody starts life with zero net assets. Nevertheless, the result that consumption moves one to one with income is perhaps not particularly troublesome as the only income shocks that are considered here are permanent income shocks. In this sense, the result conforms to the intuition underlying the permanent income hypothesis.

To get to welfare effects, the theory evaluates expected lifetime utility (for *ex-ante* identical workers) assuming constancy of the parameter describing income variance and then asks by how much expected lifetime utility would fall if there was an increase in the value of this parameter. This yields the welfare expressions that are used in the main part of the text. [54]

[54] Please see Krishna, Krebs and Maloney (2005, Appendix pages: 39-41) for a detailed derivation of these welfare expressions.

Table 1 Summary Statistics

	1993-1995		1996-1998		2001-2003	
Variable	*Mean (All)*	*Mean (Manuf)*	*Mean (All)*	*Mean (Manuf)*	*Mean (All)*	*Mean (Manuf)*
Log (Real Earnings)	7.34	7.63	7.37	7.61	7.46	7.67
Age	35.39	37.49	36.62	37.94	37.40	39.33
Variable	*Percent (All)*	*Percent (Manuf)*	*Percent (All)*	*Percent (Manuf)*	*Percent (All)*	*Percent (Manuf)*
High school drop out	17.53	19.68	11.49	14.82	11.55	13.86
High school graduate	38.1	43.86	36.37	43.58	33.87	41.08
Some college	21.92	19.21	29.76	26.03	30.11	26.96
College graduate	12.73	10.9	15.51	11.75	16.69	13.25
More than college	9.72	6.34	6.87	3.82	7.79	4.85
Female	48.32	32.91	49.04	35.74	48.68	32.86
Married	56.99	64.13	57.75	62.78	56.32	62.48
White	78.37	78.28	73.05	73.32	69.72	70.04
N	24,998	4,430	41,008	7,219	37,579	5,608

Summary statistics calculated for each panel of the SIPP seperately for the full sample of workers and workers in the manufacturing sector.

Table 2 Risk Estimates

	Mean	Median	Std. Dev.
1993-1995			
$\sigma^2_{\varepsilon,k=0}$	0.0033	0.0031	0.0016
$\sigma^2_{\varepsilon,k=6}$	0.0018	0.0015	0.0016
$\sigma^2_{\varepsilon,k=12}$	0.0014	0.0014	0.0019
1996-1998			
$\sigma^2_{\varepsilon,k=0}$	0.0043	0.0042	0.0013
$\sigma^2_{\varepsilon,k=6}$	0.0024	0.0023	0.0014
$\sigma^2_{\varepsilon,k=12}$	0.0025	0.0026	0.0018
2001-2003			
$\sigma^2_{\varepsilon,k=0}$	0.0052	0.0051	0.0016
$\sigma^2_{\varepsilon,k=6}$	0.0033	0.0034	0.0019
$\sigma^2_{\varepsilon,k=12}$	0.0031	0.0032	0.0025

Reported mean, median and standard deviations are calculated across point estimates for eighteen 2-digit SIC industries, in each panel.

Table 3 Monthly Risk Estimates by Industry for each Panel ($\sigma^2_{\varepsilon,k=0}$ and $\sigma^2_{\varepsilon,k=12}$)

SIC (2-digit)	$\sigma^2_{\varepsilon,k=0}$						$\sigma^2_{\varepsilon,k=12}$					
	1993-1995		1996-1998		2001-2003		1993-1995		1996-1998		2001-2003	
20 Food and Kindred Products	0.004***	88,481	0.004***	164,063	0.005***	124,443	0.003***	32,315	0.000	59,324	0.004***	45,940
22 Textile Mill Products	0.006***	40,609	0.003***	61,924	0.004***	31,315	0.005***	15,083	-0.000	21,953	0.004***	11,589
23 Apparel	0.003***	53,716	0.005***	98,627	0.009***	45,322	0.001**	19,336	0.004***	34,762	0.010***	16,128
24 Lumber and Wood Products	0.004***	48,251	0.005***	72,618	0.004***	60,215	0.003***	17,205	0.006***	26,056	0.002***	22,208
25 Furniture and Fixtures	0.003***	31,146	0.003***	54,938	0.002***	43,780	-0.000	11,069	0.001	19,575	0.002***	16,032
26 Paper and Allied Products	0.003***	46,081	0.004***	69,061	0.005***	49,806	0.001**	17,242	0.004***	25,537	-0.000	18,420
27 Printing, Publishing and Allied Industries	0.005***	112,856	0.004***	143,778	0.005***	110,806	0.002***	41,223	0.003***	51,985	0.001	40,783
28 Chemicals and Allied Products	0.003***	90,748	0.004***	116,748	0.006***	93,479	0.001*	33,936	0.001**	42,437	0.003***	35,130
30 Rubber and Misc. Plastic Products.	0.002***	59,445	0.003***	89,049	0.007***	62,880	-0.002***	22,437	0.000	32,779	0.006***	23,142
31 Leather and Leather Products	-0.000	2,925	0.003***	10,038	0.004***	6,915	-0.001	1,019	0.003**	3,558	0.006***	2,476
32 Stone, Clay, Glass, and Concrete Products	0.005***	34,316	0.004***	63,190	0.006***	46,769	0.004***	13,075	0.003***	22,975	0.004***	17,241
33 Primary Metal Industries	0.002***	53,139	0.004***	77,627	0.005***	48,049	-0.001***	19,692	0.001**	28,164	-0.001	17,826
34 Fabricated Metal Products	0.004***	91,221	0.003***	142,675	0.004***	103,207	0.003***	33,224	0.002***	53,227	0.001**	37,893
35 Industrial and Commercial Machinery	0.002***	152,489	0.004***	245,656	0.005***	167,864	0.001***	57,265	0.002***	89,855	0.002***	61,912
36 Electronic and Other Electrical Equipment	0.003***	136,858	0.003***	217,116	0.005***	151,539	0.002***	50,799	0.002***	80,023	0.002***	56,591
37 Transportation Equipment	0.003***	177,427	0.005***	259,820	0.005***	186,520	0.002***	66,055	0.003***	95,492	0.003***	69,297
38 Instruments	0.002***	53,364	0.005***	78,029	0.006***	53,595	0.001**	19,743	0.005***	28,117	0.003***	20,051
39 Miscellaneous Manufacturing Industries	0.006***	27,410	0.008***	50,487	0.007***	39,194	0.001	9,860	0.005***	17,357	0.005***	14,032

Robust standard errors in paranthesis. * significant at 10%; ** significant and 5%; *** significant at 1%.

Table 4 International Trade and Income Risk: Full Sample

	$\sigma^2_{\varepsilon,k=0}$		$\sigma^2_{\varepsilon,k=12}$	
Import penetration	0.023**		0.042***	
(Lagged)	(0.009)		(0.014)	
Import penetration		0.022**		0.045***
		(0.010)		(0.013)
Constant	0.003***	0.003***	0.001	0.001
	(0.000)	(0.000)	(0.001)	(0.001)
R-squared	0.71	0.70	0.58	0.60
N	54	54	54	54

Each specification includes industry and panel fixed effects. Robust standard errors in parantheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5 Robustness: Instrumental Variable Estimates

	$\sigma^2_{\varepsilon,k=0}$		$\sigma^2_{\varepsilon,k=12}$	
Import penetration	0.055***		0.049*	
(Lagged)	(0.015)		(0.027)	
Import penetration		0.067***		0.054*
		(0.020)		(0.033)
Constant	0.003***	0.003***	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
R-squared	0.57	0.44	0.58	0.59
First stage F-stat	13.39	15.96	13.39	15.96
N	54	54	54	54

Each specification includes industry and panel fixed effects. Robust standard errors in parantheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6 Summary Statistics: Explanatory Variables

	Mean	Std. Dev.	Min	Max
1993				
Import Penetration	0.169	0.140	0.014	0.561
Share of Exports	0.101	0.063	0.022	0.235
Offshoring	0.148	0.082	0.039	0.324
Share of ICT	0.080	0.058	0.029	0.225
$(\text{Labor Productivity})_{t-1}$	1.098	0.112	0.981	1.474
Union Density	0.188	0.105	0.072	0.398
Employment Growth	0.007	0.025	-0.051	0.040
Employment Volatility	0.003	0.002	0.001	0.007
1996				
Import Penetration	0.192	0.158	0.015	0.638
Share of Exports	0.122	0.080	0.022	0.282
Offshoring	0.160	0.080	0.047	0.352
Share of ICT	0.082	0.057	0.028	0.219
$(\text{Labor Productivity})_{t-1}$	1.232	0.343	0.963	2.464
Union Density	0.171	0.105	0.036	0.391
Employment Growth	-0.014	0.043	-0.156	0.023
Employment Volatility	0.003	0.003	0.001	0.011
2001				
Import Penetration	0.234	0.178	0.019	0.717
Share of Exports	0.138	0.092	0.023	0.320
Offshoring	0.192	0.097	0.054	0.393
Share of ICT	0.082	0.057	0.024	0.222
$(\text{Labor Productivity})_{t-1}$	1.769	1.519	1.076	7.464
Union Density	0.148	0.085	0.043	0.317
Employment Growth	-0.065	0.041	-0.155	-0.013
Employment Volatility	0.005	0.003	0.002	0.015

These summary statistics are calculated at the beginning of each panel, except labor productivity. Since this variable is not available after 2000, summary statistics for one year lags are reported.
Import Penetration is defined as Imports/Shipments-exports+imports and Share of Exports is defined as Exports/Shipments (Source: Federal Reserve Bank of New York).

$$\text{Offshoring} = \sum_j \left[\frac{\text{purchases of input j by industry i at time t}}{\text{total non-energy inputs used by industry i at time t}} \right] * \left[\frac{\text{imports of input j at time t}}{\text{production}_j + \text{imports}_j - \text{exports}_j \text{ at time t}} \right]$$

Share of ICT is calculated as (Software+Computers and peripheral equipment+Communication equipment + Photocopy and related equipment+Instruments)/K. (Source: BEA, NIPA)
Labor productivity is Output/Hours with 1987 as the base year; aggregated to 2-digit SIC level using employment shares as of 1992 as weights. (Source: BLS)
Union Density is (Union Members)/Employment. (Source: Hirsch and Macpherson (2003))
Annual Employment Volatility is calculated as the volatility of monthly employment growth. (Source: BLS)

Table 7 Robustness ($\sigma^2_{\varepsilon,k=12}$)

Full Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Import Penetration	0.042***	0.044**	0.050**	0.049**	0.050**	0.053***							
(lagged)	(0.014)	(0.020)	(0.019)	(0.020)	(0.019)	(0.019)							
Share of exports		-0.005	-0.002	-0.001	0.004	-0.009							
(lagged)		(0.018)	(0.019)	(0.020)	(0.022)	(0.023)							
Offshoring			-0.023*	-0.022*	-0.024*	-0.021							
(lagged)			(0.011)	(0.012)	(0.013)	(0.013)							
Share of ICT				0.020	-0.014	-0.038							
(lagged)				(0.036)	(0.040)	(0.046)							
Labor Productivity					-0.001	-0.0005							
(lagged)					(0.000)	(0.000)							
Union Membership						0.027							
(lagged)						(0.016)							
Import Penetration							0.045***	0.045**	0.057***	0.056**	0.055**	0.054**	0.050**
							(0.013)	(0.019)	(0.020)	(0.021)	(0.020)	(0.020)	(0.022)
Share of exports								0.000	0.001	0.004	0.001	0.004	0.004
								(0.016)	(0.017)	(0.017)	(0.017)	(0.015)	(0.015)
Offshoring									-0.044**	-0.043**	-0.039*	-0.047**	-0.052**
									(0.018)	(0.019)	(0.021)	(0.020)	(0.021)
Share of ICT										0.031	0.023	0.032	0.028
										(0.034)	(0.038)	(0.041)	(0.043)
Union Membership											0.013	0.011	0.013
											(0.014)	(0.013)	(0.014)
Employment Growth												-0.013*	-0.011*
												(0.007)	(0.007)
Employment Volatility													0.251
													(0.197)
R-squared	0.58	0.58	0.61	0.61	0.62	0.66	0.60	0.60	0.63	0.64	0.65	0.67	0.68
N	54	54	54	54	54	54	54	54	54	54	54	54	54

Each specification includes industry and panel fixed effects. Robust standard errors in parantheses. * significant at 10%; ** significant at 5%; ***significant and 1%. Since comparable data for labor productivity is not available after 2000, the estimates from the specification including productivity as of the beginning of the panel are not included in this table. Lagged variables are one year prior to the beginning of the panel and level variables are as of the first year of the panel.

Table 8 Income Risk in Sub-Samples

	$\sigma^2_{\varepsilon,k=0}$		$\sigma^2_{\varepsilon,k=12}$	
1993-1995	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
SWITCH_NON-MANUF	0.0063	0.0033	0.0026	0.0053
SWITCH_ALL	0.0059	0.0029	0.0029	0.0050
STAY_MANUF	0.0027	0.0014	0.0011	0.0019
STAY_SIC2	0.0024	0.0012	0.0008	0.0016
1996-1998				
SWITCH_NON-MANUF	0.0082	0.0031	0.0036	0.0055
SWITCH_ALL	0.0067	0.0026	0.0030	0.0043
STAY_MANUF	0.0033	0.0010	0.0021	0.0017
STAY_SIC2	0.0031	0.0008	0.0021	0.0015
2001-2003				
SWITCH_NON-MANUF	0.0090	0.0032	0.0039	0.0057
SWITCH_ALL	0.0081	0.0026	0.0033	0.0036
STAY_MANUF	0.0039	0.0017	0.0024	0.0023
STAY_SIC2	0.0037	0.0017	0.0025	0.0025

Reported mean and standard deviations are calculated across point estimates for eighteen 2-digit SIC industries, for each panel and sub-sample.

Table 9A International Trade and Income Risk: Sub-Samples ($\sigma^2_{\varepsilon,k=0}$)

	STAY_SIC2		STAY_MANUF		SWITCH_ALL		SWITCH_NON-MANUF	
Import Penetration	0.017*		0.019*		0.028*		0.023	
(Lagged)	(0.0084)		(0.0097)		(0.0157)		(0.0201)	
Import Penetration		0.015*		0.017		0.027		0.024
		(0.0089)		(0.0104)		(0.0169)		(0.0218)
Constant	0.002***	0.002***	0.002***	0.002***	0.007***	0.007***	0.008***	0.008***
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.001)	(0.0011)	(0.0013)	(0.0014)
R-squared	0.59	0.58	0.62	0.61	0.61	0.61	0.55	0.55
N	54	54	54	54	54	54	54	54

Each specification includes industry and panel fixed effects. Robust standard errors in parantheses. * significant at 10%; ** significant at 5%; ***significant and 1%.

Table 9B International Trade and Income Risk: Sub-Samples ($\sigma^2_{\varepsilon,k=12}$)

	STAY_SIC2		STAY_MANUF		SWITCH_ALL		SWITCH_NON-MANUF	
Import Penetration	0.028*		0.031*		0.070***		0.081**	
(Lagged)	(0.0158)		(0.0159)		(0.0240)		(0.0330)	
Import Penetration		0.031*		0.034**		0.070***		0.081**
		(0.0157)		(0.0157)		(0.0251)		(0.0344)
Constant	0.000	0.000	0.001	0.000	0.003	0.003	0.002	0.002
	(0.0008)	(0.0009)	(0.0008)	(0.0009)	(0.0027)	(0.0028)	(0.0033)	(0.0034)
R-squared	0.50	0.51	0.51	0.53	0.50	0.49	0.42	0.42
N	54	54	54	54	54	54	54	54

Each specification includes industry and panel fixed effects. Robust standard errors in parantheses. * significant at 10%; ** significant at 5%; ***significant and 1%.

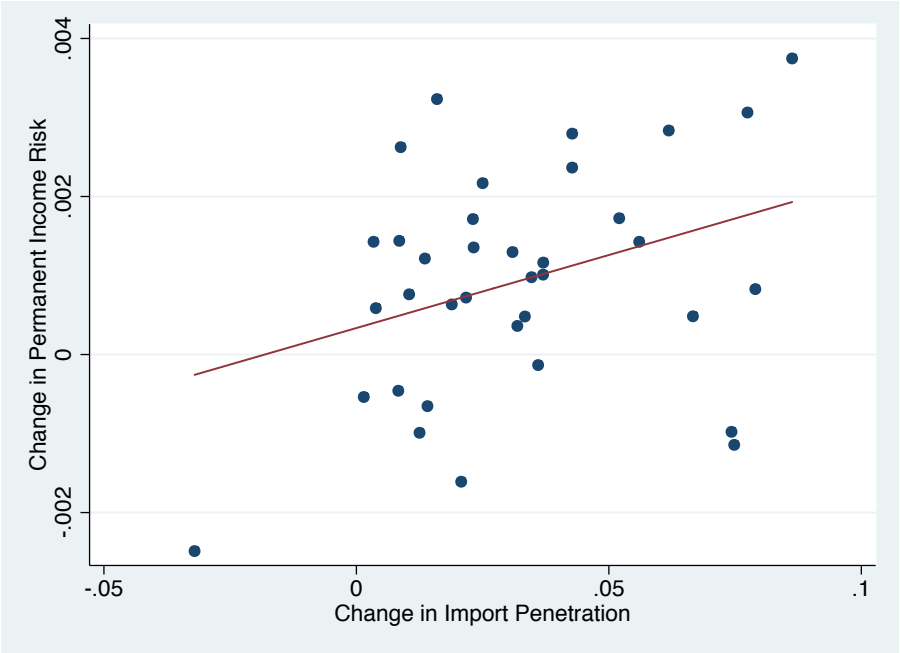
Table 10 Welfare Effects (Percent of Lifetime Consumption)

		T=5	T=10	T=15
$\beta = 0.98$				
K=0	$\gamma = 1$	1.06	2.02	2.90
	$\gamma = 2$	2.19	4.32	6.39
K=12	$\gamma = 1$	2.18	4.18	6.03
	$\gamma = 2$	4.24	8.00	11.34
$\beta = 0.95$				
K=0	$\gamma = 1$	0.97	1.72	2.31
	$\gamma = 2$	2.00	3.66	5.05
K=12	$\gamma = 1$	1.98	3.55	4.77
	$\gamma = 2$	3.87	6.81	9.05
$\beta = 0.90$				
K=0	$\gamma = 1$	0.83	1.32	1.61
	$\gamma = 2$	1.71	2.80	3.49
K=12	$\gamma = 1$	1.70	2.72	3.32
	$\gamma = 2$	3.32	5.25	6.36
$\beta = 0.85$				
K=0	$\gamma = 1$	0.71	1.02	1.16
	$\gamma = 2$	1.46	2.16	2.49
K=12	$\gamma = 1$	1.45	2.10	2.39
	$\gamma = 2$	2.84	4.08	4.62

Results are provided separately for parameter values for the coefficient of risk aversion (γ) at 1 and 2, for durations of higher import penetration for T = 5, 10 and 15 years and for different values for the discount factor (β). All of the calculations are based on a 10% increase in import penetration.

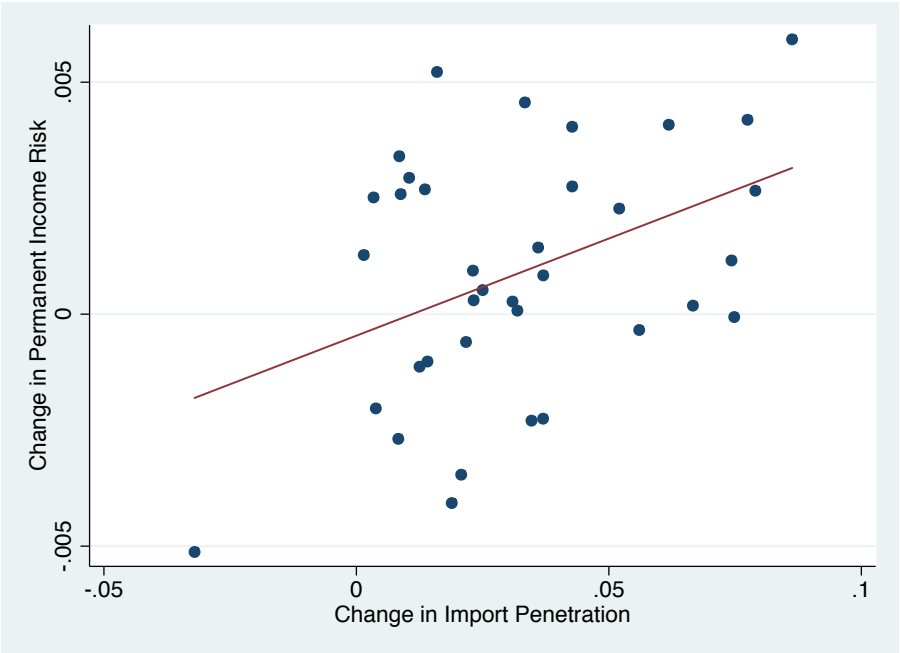
Figure 1 Changes in Permanent Income Risk and Changes in Import Penetration

A. $\sigma^2_{\varepsilon,k=0}$



Changes in import penetration between the 1993-95 and 1996-98 panels and between the 1996-98 and 2001-03 panels are calculated using import penetration measured at the beginning of each panel.

B. $\sigma^2_{\varepsilon,k=12}$



Changes in import penetration between the 1993-95 and 1996-98 panels and between the 1996-98 and 2001-03 panels are calculated using import penetration measured at the beginning of each panel.