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Passthrough of Firm Performance to Income and Employment Stability

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DISCUSSION PAPER SERIES

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ABSTRACT

Passthrough of Firm Performance to Income and Employment Stability*

To what extent do firms pass through idiosyncratic shocks to their workers? In this paper, we investigate this question focusing on passthrough to income for workers that stay in the firm and passthrough to employment stability. We take an empirical approach and use matched employer-employee data from Denmark, three different measures of firm performance (sales, value added, and value added per worker), and two measures of income (earnings and hourly wages). We distinguish between unemployment and job-to-job transitions. We find that passthrough to income is much higher for permanent (5-9 percent) than transitory (1 percent) shocks. Income passthrough is higher for blue collar workers and workers in small firms. On the employment margin, we find that worse firm performance increases both unemployment and job-to-job transitions. The unemployment risk is especially pronounced for blue collar, low-educated, low tenure workers, while the effect on job-to-job transitions is larger for managers and high-educated workers. We also find clear evidence of non-linearities with negative shocks driving both unemployment and job-to-job transitions.

JEL Classification: C33, D22, J31, J33

Keywords: firm shocks, passthrough, income, employment stability

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1 Introduction

Variation in performance is substantial both across firms and within firms over time, see e.g. Abowd et al. (1999) and Song et al. (2019). In a fully competitive labor market, idiosyncratic firm performance shocks should be unrelated to worker income, since worker income is determined by the market wage, and workers can move to other firms without costs. However, any number of imperfections in the labor market could dilute this extreme prediction. Imperfections could be e.g. due to search frictions, which introduce rents to the current match, contractual imperfections, or incentive problems as in a classical principal-agent problem or an efficiency wage model. These imperfections lead to changes in worker income in response to a firm shock, but may also directly affect the probability that the worker is separated from his current job. This paper seeks to understand to what extent idiosyncratic shocks to firm performance are passed through to their workers via income and employment stability.

We use two well-known and complementary estimation methods for estimating rates of pass-through to income, and we extend the analysis to study employment stability. We use Danish matched employer-employee data covering 25-years and leverage that we can measure firm performance, income, and labor market transitions in different ways. In particular, we study how firm performance measures (value added, sales and value added per worker) affect income measures (hourly wages and yearly earnings) and employment stability in the form of exits from the firm into unemployment or into other jobs, i.e. job-to-job transitions.¹ We make four main contributions. First, we move beyond most previous empirical work and study how firm shocks affect both the employment and the income margin. Thereby, we can characterize the importance of passthrough for both stayers and non-stayers. Second, we quantify the extent to which firm adjustments to shocks differ depending on the type of workers and we allow for non-linearities in passthrough. Third, since we use two complementary estimation methods simultaneously, we can compare results across methods and implicitly assess the role of different identifying assumptions. Fourth, having access to many different firm performance and worker income measures allows us to examine whether the differences in results in the current literature, focused on passthrough to income, arise because of differences in which measures are used. By studying different methods and measurements simultaneously, we therefore to some extent bridge previous work.

We distinguish between shocks to firm performance which are more permanent in type, representing long lasting changes in firm performance, and transitory shocks, which are temporary and mean reverting.² Our two estimation approaches use different degrees of econometric structure,

¹For expositional reasons we use income (or worker income) as a collective term covering hourly wages and yearly earnings. We realize that income is normally used differently, but we could not find another word which fitted better and was not overly cumbersome to use. Similarly, we refer to employment stability (or the employment margin) as a collective term covering the probability of making an employment-to-unemployment or a employment-to-employment transition for worker who start the period inside a specific firm.

²Throughout the paper we follow the convention in the literature (see e.g. Guiso et al. (2005); Juhn et al. (2018)) and refer to residual variation in firm performance as a firm shock. We acknowledge that this is at best an approximation of true firm shocks and likely mixes up several different types of unexpected changes to firm performance, which requires the firm to re-optimize. We discuss this issue further in Section 3.1.

and we refer to these methods as a non-structural and a structural approach. The non-structural approach builds on Juhn et al. (2018) and Card et al. (2016). This approach directly correlates changes in firm performance with changes in income varying the time window over which the differences are taken. Further, in an IV setup, we instrument e.g. one period differences with longer differences to avoid concerns about reverse causality and measurement error. The approach is rather informal in how permanent and transitory factors are quantified. Longer differences of firm performance should contain more permanent shocks, and by comparing estimates with increasing difference length the importance of permanent shocks is thus quantified. On the contrary, the structural approach explicitly models variations in residual firm performance as resulting from either permanent or transitory processes. As a result, we get separate estimates on the rate of passthrough of permanent and transitory firm shocks. The structural approach follows the estimation procedure developed by Guiso et al. (2005).

Previous literature on the role of passthrough has primarily focused on passthrough of firm performance to income and ignored the employment margin with a few exceptions that we mention below, see a review in Card et al. (2018). One of our main contributions is to show that passthrough of firm performance also materializes on the employment margin directly. The employment margin may be particularly important when the firm prefer to respond to a negative shock by firing some workers and only reduce income for stayers marginally to keep work incentives for remaining employees high. Such situations could arise due to e.g. contractual impediments or efficiency wage concerns. The implication is that we need to address both the employment and income margins in order to understand the full extent of passthrough and overall risk-sharing. To quantify passthrough on the employment margin, we adapt the estimation methods used to study passthrough to income. We focus on the relationship between same period changes in firm performance and the probability of leaving the firm for workers who are employed in the beginning of the period. To address concerns about reverse causality and other confounders, we instrument changes in firm performance with similar type instruments as those used in the analysis of passthrough to income. We distinguish exits from the firm by whether workers transition into unemployment or make a job-to-job transition. This distinction is important. While it is perhaps reasonable to expect job-to-job mobility to serve as a channel through which workers can reduce the importance of idiosyncratic firm shocks on their life time earnings, i.e. avoid decreases in income by making a transition into other firms, a larger share of transitions into unemployment may actually be involuntary from the point of view of the worker.

We find that there is significant passthrough of firm performance shocks to income no matter which measures and methods we use. Both the structural and non-structural approach suggest that permanent shocks have much higher passthrough than transitory shocks. Using the structural approach, we find a passthrough (elasticity) of permanent shocks between 5-9 percent. I.e. a one percent permanent shock to firm performance changes worker income by 0.05-0.09 percent. Transitory shocks only have a passthrough of 1 percent. The non-structural estimates using different difference lengths in both OLS and IV typically lie between the estimate of the transitory

and permanent shock from the structural approach. However, even using long differences lengths the non-structural estimates of passthrough are well below the structural estimate of passthrough for a permanent shock. The reason, which the structural approach reveals, is that even transitory shocks are fairly persistent. The non-structural approach mistakes these long-lasting effects as permanent shocks. Looking at differences across workers, we find that passthrough is especially pronounced for blue collar workers and workers in small firms. Passthrough to income is higher for positive performance shocks than negative ones, which is consistent with downward rigid wages. By contrasting passthrough rates on hourly wages and earnings and across firm performance measures, we also find that firms make adjustments via both hourly wages as well as in the number of hours, even for stayers in the firm.

We also find significant passthrough of firm performance to the probability of becoming unemployed or making a job-to-job transition. In fact, we show that changes on the employment margin are actually more important quantitatively for firm's total labor costs than adjustments on hours or hourly wages. Our results suggest that workers employed at a firm who experience a negative permanent shock of one percent have an increased probability of being fired of around 0.13 to 0.61 percentage point along with a similar sized increase in the probability of making a job-to-job transition. The unemployment risk is especially pronounced for blue collar, low educated, low tenure workers, while the effect on the probability of making a job-to-job transition is larger for managers and high-educated workers. Overall, our results suggest that the consequences of fluctuations in firm performance vary across workers. Higher skilled workers may have more easily available alternatives and can thus "escape" negative firm shocks by changing employers, while lower skilled workers on the other hand are more likely to enter unemployment and thus are relatively more affected. Lastly, we also present evidence in favor of some non-linearities in passthrough. Positive performance shocks have much lower effect on both extensive adjustment margins than negative shocks.

Our paper is related to a large and growing literature focused on passthrough to worker income. A key reference is the seminal paper by Guiso et al. (2005) which has also inspired our structural approach. Guiso et al. (2005) find insignificant effects of transitory shocks, but significant effects of permanent shocks. A series of papers following Guiso et al. (2005) have presented estimates using the same approach, but from different countries and also using different measures of income and firm performance.³ One of the contributions of this paper is that we are able to bridge some of the different results, since we use several firm performance measures and worker income measures within the same sample. For instance, we generally find that sales and value added lead to higher passthrough rates than value added per worker. Further, we find that passthrough is higher for earnings compared to hourly wages suggesting that adjustments on hours are important even for those workers that stay in the firm following a shock. Compar-

³For example, Rute Cardoso and Portela (2009) look at monthly wages and sales using Portuguese data, Fagereng et al. (2018) look at worker total income and value added using Norwegian data, and Guertzgen (2014) use data from Germany and look at daily earnings and value added per worker. For a more complete review, see Card et al. (2018). When we present our results, we will go into more detail comparing these previous findings with our findings.

ing our estimates to the literature, previous differences across countries seems partly to be driven by differences in measures of firm performance and worker income. Some recent papers have analyzed the importance of passthrough to worker income using alternative methods to Guiso et al. (2005). Juhn et al. (2018), who have inspired our non-structural approach, investigate passthrough by regressing differences in wages on differences of firm sales for various period lengths. They formally show that the correlation between longer differences should reflect the impact of permanent shocks. Other recent work has focused on using quasi-experimental variation to identify passthrough associated with specific types of shocks. For example, Kline et al. (2019) take an event study approach and focus on passthrough to worker income from the arrival of a firm patent, which is a specific type of firm shock.⁴

Our paper is also related to a much sparser literature looking at the role of employment adjustments. First, some papers focus on the life-cycle profiles of earnings and the role of employment mobility as a channel through which workers can reduce the impact of firm specific shocks on worker income. For example, Friedrich et al. (2019) construct a statistical model, which explicitly models the workers (endogenous) employment responses to productivity shocks, thereby controlling for selection into continued employment. One of the key points in Friedrich et al. (2019) is that an analysis focused on wage changes for firm stayers focus on a selected sample since workers experiencing very large shocks may (choose to) become unemployed or transition into other jobs. Second, recent work have also looked at the role of employment adjustments as a direct adjustment channel to firm shocks. Roys (2016) studies firm-level employment and wage adjustments in response to shocks to firm performance using data from France 1994-2000. He develops a model of firm demand for labor, where labor adjustment costs are a key element, and shows that the persistence of the shocks matter for how the firm responds. In particular, the employment margin is much more responsive when shocks are more permanent in type. The results in Roys (2016) thereby underline the importance of the employment margin as a separate adjustment channel through which the firm may respond, i.e. employment separations may arise as a direct choice made by the firm and not only as an endogenous response to future lower earnings. Balke and Lamadon (2020) develop a structural search model with worker and firm specific productivity shocks as well as optimal contracts, where firms insure risk-averse workers. The model is estimated on Swedish data. Balke and Lamadon (2020) find that a significant fraction of earnings risk is related to unemployment and job mobility, but at the same time firms absorb around 90 percent of persistent firm productivity shocks. Both of the above papers quantify the role of employment adjustments through the lenses of a structural model. While this has several advantages in terms of e.g. decomposing the role of different mechanisms and quantifying overall implications of passthrough, it also requires taking an explicit stand on the driver of employment separations. I.e. are firms laying off workers because the match is no longer generating a surplus, which is typically assumed? Or does the layoff arise because of e.g. contractual impediments, or

⁴Some examples of other recent papers are Chan et al. (2020) using data from Denmark, Carlsson et al. (2016) using Swedish data, and Lamadon et al. (2019) using US data. The methods differ in these papers, but in general they find passthrough rates ranging from 0.05-0.20.

other frictions forcing the firm to terminate an otherwise productive match. Obviously, what we assume about the nature of employment separations also affects the eventual assessment of the importance of risk through this channel. In this paper we complement this literature by studying passthrough of firm shocks to employment separations in a reduced-form setting.

Two other recent papers analyze the relationship between firms shocks and the level of employment at the firm level. Bagger et al. (2020b,a) use Danish data to study the relationship between firm level employment (hiring, firing) and value added in a statistical model. They focus on the firm level and analyze changes in hiring and separation patterns at the monthly and quarterly level for different types of shocks. Finally, Carlsson et al. (2020) use Swedish data. They estimate structural vector auto regressions to filter out permanent idiosyncratic demand and technology shocks. They find that permanent demand shocks drive employment fluctuations, while technology shocks do not have large effects. They also find asymmetric responses. Most of the response to permanent positive shocks is through increased hiring, while most of the adjustment to permanent negative shocks is through increased separations. Compared to these papers, we study employment separations at the level of the individual allowing us to control for individual heterogeneity and consider effects for different sub-groups of workers such as e.g. low tenure workers or blue versus white collar workers. In addition we simultaneously consider passthrough to income as well. As we show below, both the variability of income for stayers and the type of exits (unemployment or job-to-job transitions) vary across these worker groups suggesting that the passthrough of firm shocks to these groups differ.

The remaining paper is structured as follows. In Section 2, we briefly introduce the institutional setting and describe the data we will use in our empirical analysis. In Section 3, we set up our empirical approach. In Section 4, we present our estimates (non-structural as well as structural) of passthrough to worker income. In Section 5, we present our estimates for passthrough to employment stability. In Section 6, we illustrate non-linearities in the rate of passthrough. Lastly, we end the paper with a conclusion in Section 7.

2 Data and Institutional Setting

In this section we first highlight important characteristics of the Danish labor market and then proceed with a presentation of the data. Our data is organized in two different samples: 1) a firm sample with observations at the firm-year level and 2) a worker sample with observations at the worker-year level. We go through the details of the construction of each sample, and lastly we show descriptive statistics.

2.1 Institutional Setting

The Danish labor market is characterized by the Flexicurity system, which is a combination of a flexible labor market, generous social security, and active labor market policies, see Andersen and Svarer (2007). This system enables the firms to fire workers rather easily compared to other coun-

tries, while still providing a generous social insurance for unemployed. The wage setting system in Denmark has since the 1980's been more and more decentralized (Andersen et al. (2012)). In our sample period, general items such as vacation, on-the-job training, etc. are determined at the national level in negotiations between the trade unions and employer confederations. In these negotiations a minimum wage is set. However, on top of this, there are local negotiations at the firm level, where wage increases are determined as well as overtime pay and advance notice rules. These two features, the flexibility of hiring/firing decisions and the gradually more decentralized wage setting, enables a keen opportunity to investigate the risk sharing in the Danish labor market. In terms of labor market transition rates, Denmark is comparable to the US, see Jolivet et al. (2006).

2.2 Firm Sample

Our starting point is the FIRM database from Statistics Denmark. This database contains firm level accounting data and value added tax (VAT) records. We select firms with non-missing, non-zero, and non-imputed value added, sales, and wage bill information from the industries Manufacturing, Construction, Trade & Transportation, Information & Communication, and Business Services.⁵ The industries are selected based on the degree of coverage in the FIRM database, which varies across industries for historical reasons. For the chosen industries, our sample covers more than 70 % of the workers employed in these industries.

We only make two additional sample restrictions besides the selection of specific industries. First, since our empirical strategy relies on being able to observe the firm over several consecutive years to focus on within-firm changes in performance, we require firms to be present in at least 5 consecutive years. Second, we choose to focus on firms with more than 10 full-time workers on average through the panel. Focusing on larger firms is standard in the literature, and the restriction serves several (related) purposes. First, it is reasonable to expect the rate of passthrough to vary with the size of the firm, and we are primarily interested in passthrough of larger firms to link to existing literature. Second, smaller firms are more likely to have imprecisely measured firm performance measures, and fluctuations in firm performance over the years may not necessarily reflect real differences in the economic environment (e.g. for smaller firms sales is obviously more sensitive to the specific timing of payments in the contract). The firm performance measures are also easier to manipulate in smaller firms, where the owner typically work as an employee too.⁶ Further details on the sample selection can be found in Table 13 in the Appendix A.

⁵We only include firms who enter the panel prior to 2010 to ensure a sufficiently long panel to study the evolution of firm performance in. The industry classification is based on NACE Rev. 2. See also Appendix A.3. Note, that our analysis excludes public sector firms, where information on value added is missing/incomplete.

⁶Note that Danish firms are generally small with a median firm size of around 6-8. Focusing on firms with more than 10 full-time workers thus drops a substantial share of firms, but the implications for the number of workers included in the analysis is much smaller, see Table 13. Obviously selecting firms based on the size of their workforce has the risk of confounding/biasing our analysis on the employment margin. However, we show in Appendix C.2 that our non-structural estimates are basically unchanged when we instead consider firms with more than 5 workers on average throughout the panel suggesting that our results are not driven by this specific sample selection.

2.3 Worker

The unit of observation in our worker sample is a worker-year. Our sample includes all employment records associated with firms present in our firm sample. The employment status is determined as the status of the worker in the last week of November each year. For each worker we have information on income measures, tenure, occupation, age, education levels, region of residence, labor market experience, and labor market transitions. All this information comes from the IDA database maintained by Statistics Denmark. We focus on workers with high quality and non-missing information on these variables and who are in the age range 19 to 59 and are not under education.⁷

2.4 Key Variables

In our analysis we measure firms performance (in logs) in three different ways in the data: Value added, sales (revenue), and value added per worker, where the latter is measured in full-time equivalents.⁸ Each measure has its own advantages and disadvantages, which we will describe more in detail later.

To analyze passthrough to worker income, we use two different measures: hourly wages and yearly earnings. Yearly earnings include all earnings from all employers accumulated through the year including potential overtime work etc. Passthrough to earnings may thus reflect changes in both the number of hours worked as well as the payment per hour. Hourly wages are calculated by Statistics Denmark by dividing yearly earnings from the employer where the worker is working in the last week of November with an estimate of hours from that employment relationship. Note that the hours measure is not continuous, but primarily captures discrete changes in the degree of part-time and full-time work as it is indirectly determined from mandatory pension contributions, see Lund and Vejlin (2016) for details. Notice that we generally do not capture if the worker works more than the contractual number of hours.⁹ These features have implications for our estimates of passthrough to hourly wages. If a firm is hit by a positive shock, it may in many cases ask its workers to work more. The same flexibility is typically not present for negative hours adjustments, where contractual arrangements make downward hours adjustments harder. Therefore, since we do not capture all hours adjustments and in particular not the positive adjustments, our estimates on passthrough rates to hourly wages should generally represent an upper bound.

⁷We provide additional details on these different sample selection steps in Appendix A. Occupations are classified by the Danish version of the standard international ISCO classifications. We group occupations into 5 categories: Managers, Knowledge (Professionals and Process Control Technicians), Clerical (Service/Sales/Secretary work) and Production (including crafts and other manual labor) and one group for missing information on occupations. See the distribution in Table 12.

⁸We chose not to use profit, since we want to avoid problems with the possible discretionary power the firms have over the reported profit. Value added is defined as the difference between the value of the production and the value of intermediate inputs in Danish Kroner. Value added is essentially what is left to pay workers and capital. The measure is constructed from annual firm statistics by Statistics Denmark and take changes in inventory, depreciation of capital, etc into account.

⁹This is a common problem in general in almost all register data sets that have an hours measure. From 2008 we can measure overtime work if it is paid out and extra hours are registered, but in many jobs this does not occur.

To analyze passthrough on the employment margin, we leverage that we can link our data to data covering all labor market transitions and thus for example differentiate between transitions into temporary unemployment or directly into other jobs (see Appendix A.4 for additional details). We can therefore distinguish worker exits from the firm by three different destinations: unemployment, direct entry into other firms (job-to-job transition) or other exits (e.g. retirement etc). In our analysis we focus on the first two types of exit. We categorize an exit from a firm as an unemployment exit if the worker has been unemployed during the year following the last employment observation with the firm. We categorize an exit from a firm as a job-to-job transition if the worker has another employer one year after the last observed time with the firm and that she/he has not been unemployed during that year.

2.5 Descriptive Statistics

Tables 11 and 12 in Appendix A provide some descriptive statistics based on the firm (and thus firm-year weighted) and worker (and thus worker-year weighted) samples. Recall that we only include firms that are present in the data for at least 5 consecutive years, and only include workers who work in firms included in the firm sample.¹⁰ Note that we winsorize our key variables value added, value added per worker, sales, hourly wages, and earnings at the 1th and 99th percentile.¹¹ Most firms are in the Manufacturing and Trade & Transportation sectors. The average firm size in the firm sample is 66 and the average yearly change is around one worker. In relative terms this corresponds to a eight percent change in firm size on average underlining that turnover is especially pronounced at smaller firms. Overall these numbers also illustrates the potential importance of the employment margin in adjusting to fluctuations in firm performance.

In the worker sample we see that the average worker is 40 years old, has 7 years of tenure and around 15-years of formal education (i.e. has a vocational education or something similar). The average job spell is 8 years, and each worker on average has around 2 jobs while being in the sample.

3 Framework and Empirical Model

In this section, we describe the empirical models that we use. First, we sketch a conceptual framework and second, we present our estimating equations for the analysis of passthrough to income and employment stability.

¹⁰As is clear from Table 12, our worker sample therefore includes job spells of different durations (including 1-year job spells). As we explain later, our different econometric models differ in the requirements to how long jobs spells we can use in the analysis, and the number of observations consequentially varies across some of the specifications. For example, in studying income passthrough for stayers, we focus on jobs with a duration of at least 5 years.

¹¹Since our empirical strategy requires long panels, we prefer not to trim the data.

3.1 Conceptual Framework

Before we move into the empirical models we briefly sketch a conceptual framework in order to try to fix ideas.¹² Our analysis below is focused on how, and to what extent, firms pass performance shocks through to their employees. We consider two key objects in the firm's objective function: The first object is a measure of the firm's revenue fp_{jt} . Empirically, this is sales or value added, which is basically sales net of costs of intermediate goods, changes in inventory, etc. Second, the labor costs of the firm. We are generally interested in how unpredictable/idiosyncratic changes in fp_{jt} leads to changes in labor costs. We can represent the costs in firm j in period t as a function of

$$C(w_{jt}, e_{jt}..)$$

where $w_{jt} = \{w_{1jt}, \dots, w_{Ijt}\}$ is a vector with each individual worker's hourly wage if he works in the firm during period t , $e_{jt} = \{e_{1jt}, \dots, e_{Ijt}\}$ is an indicator for the number of hours for each worker (where $i = 1, I$ index workers in the firm in the beginning of the period). The specification above suggests two channels of passthrough: wages may change for some or all workers in the firm, and the employment status may change through changes in the number of hours worked. The later could consist of changes in the number of hours worked in the firm, but it could also reflect workers leaving the firm. Our empirical approach aims to quantify changes along these margins without explicitly modeling the firm's choices. First, we focus on the intensive margin and on workers who stay in the firm and analyze changes in hourly wages and earnings. Comparing estimates on these margins quantifies the importance of hourly wage and hours adjustments for stayers. Second, we look at the extensive margin and analyze how employment status change for workers who start the period working in the firm.

Note that we do not consider firm hiring since our framework is not well suited to look at this due to the explicit focus on worker present in the firm prior to the shock and the focus on within job-spell variation. Modeling firm entry would require a different sample and a lot of additional choices regarding e.g. the relevant pool at risk (e.g. entries from unemployment only) and outside options (competing firm offers and remaining UI benefits) and overall empirical strategy. For a quantification of this margin see e.g. Carlsson et al. (2020); Bagger et al. (2020a).

Extensive and Intensive Margin Adjustments We analyze the intensive and extensive margin separately and independently of each other since they may represent separate margins of adjustment from the perspective of the firm and because their "interaction" may be non-standard. For instance efficiency-wage considerations, binding minimum wages, or downward rigidities in wages may favor extensive employment adjustments over intensive adjustments and thus lead to larger employment adjustments. Oppositely, contractual regulations such as clauses, warnings periods, or fixed work hours may favor wage adjustments over employment adjustments. In

¹²Since the analysis of our paper is purely statistical we abstain from putting up too much economic structure. Further note that the passthrough rates we quantify below may not only be results of firms behavior in response to shocks, but are generally also a result of worker actions.

addition, inter-temporal investment issues or firm specific human capital may influence current period decision making favoring adjustments on one margin over another. On top of this, workers in the firm may also make decisions and choose to become unemployed or leave the job in favor of another job in response to a negative firm shock. The relative importance of all these different elements would likely also differ depending on the type of the match, for instance, depending on the workers level of firm specific human capital and more generally his outside options. Our analysis focus on the worker-year level and therefore enables us to quantify whether the relative importance of different adjustment channels (extensive versus intensive) change with the characteristics of the worker.

As explained in the introduction, we consider exits to unemployment and employment-to-employment exits separately when we analyze passthrough to the extensive margin. These two types of exit would likely differ in the degree to which they are “initialized” by the firm or the worker. Employment-to-employment transitions should primarily reflect worker driven adjustments to firm shocks. On the other hand transitions into unemployment could arise from mutual consent or primarily be firm driven. The latter could arise if the firm faces e.g. constraints which makes adjustments on the employment margin more attractive than adjustments on other margins, see the above discussion.¹³

Measuring Firm Performance A separate issue is how to identify idiosyncratic changes in fp_{jt} (or firm shocks) empirically. Throughout our analysis we use three different measures of fp_{jt} : Value added per worker, Sales, and Total value added. We measure all variables in logs and focus on changes in firm performance. Therefore we can easily compare estimates across firm performance measures for the same relative change. Our measure of shocks is data driven and indirectly obtained via a specific econometric model. This also means that our measure of firm shocks is likely a composite of different type of shocks such as demand shocks, production shocks (e.g. broken machinery or changes in production technology), and other types of shocks. In this sense, we take the broadest view possible on what constitutes a shock. This can of course be viewed as an advantage and a disadvantage. Obviously, it makes the link to theory less explicit and perhaps also harder to interpret compared to e.g. TFP measures of firm shocks, which has been used by e.g. Chan et al. (2020). On the other hand it can be argued that by focusing on a composite of shocks we are also quantifying a larger set of passthrough rates and hence get closer to a full quantification of e.g. risk sharing between workers and firms.

Although our three measures of fp_{jt} are obviously related, we can also think of these measures as somewhat different types of firm shocks. Total value added is essentially sales subtracted the cost of intermediate inputs accounting for changes in inventories etc. Thus we may expect a

¹³Closely linked to this issue is the nature of employment separations and whether they result from “mutual consent”, since the job match is no longer generating (enough) surplus or whether one party would actually prefer maintaining the job match. For a recent discussion of this see Jäger et al. (2019). From the workers point of view this also speaks to the role of employment mobility as a magnifier or demagnifier of the role of idiosyncratic firm shocks in lifetime earnings. If job separations are always with workers consent employment mobility should generally serve as a channel through which the worker can reduce the role of firms shocks, but as argued in the main text this may not always hold.

positive demand shock to affect sales more than total value added since firms are likely to have increased their use of intermediate inputs to keep up with demand. It is also clear from the discussion above that a potential channel through which the firm can adjust to e.g. a demand shock is to make adjustments on the labor input margin and thereby potentially mitigating a part of the shock. In that sense value added per worker, which corrects for the amount of labor input used within the period (in full time equivalents), may be affected by the endogenous response of the firm to an initial shock to e.g. total sales. We could therefore cautiously interpret fluctuations in value added per worker as representing the part of the shock which remains after the firm has made adjustments on labor inputs.¹⁴ Of course when we directly study the employment margin, value added per worker becomes less meaningful as we are precisely interested in these adjustments on employment which are filtered out when focusing on the per worker measure. For this part of the analysis we therefore leave out this later measure. We discuss this choice further in Section 3.3.1.

Broadly speaking, our empirical analysis focus on year to year changes in fp_{it} , which are idiosyncratic and specific to the individual firm. Confounding changes are generally “controlled for” by focusing on changes in firm performance. Hence we net out permanent firm differences and further include a set of control variables. Control variables generally remove the effect of e.g. aggregate shocks, shocks on the industry level, or predictable dynamics associated with e.g. worker experience accumulation. We follow the convention in the literature (see e.g. Guiso et al. (2005); Juhn et al. (2018)) and refer to the remaining residual variation in firm performance as a firm shock. Further, since it is reasonable to expect the degree of passthrough to also depend on the nature of the firm shock, we try to distinguish between short term transitory fluctuations in firm performance and more persistent ones, which we will refer to as more permanent.

Overall implications In summary, the discussion above have highlighted the two main margins which may change in response to firm shocks: an intensive margin with changes in income and hours and an extensive margin where a worker may simply exit the firm. Note that extensive margin adjustments may affect two margins. First, it affects the firm performance measure value added per worker directly, and second it affects the probability of exiting the firm. Our empirical analysis contrast passthrough rates across different measures. E.g. by comparing passthrough rates on hourly wages to earnings we learn something about the relative importance of hours adjustments in the job matches in our estimating sample. Further by comparing differences in passthrough for value added to value added per worker we learning something about the importance of changes in employment/hours at the firm level, i.e. not necessarily driven by job matches in our estimating sample (which may not always contain all workers employed at a given firm in a given year).

¹⁴An alternative interpretation is that value added per worker addresses a potential reverse causality concern (or attenuation bias), where a firm is always facing unlimited demand and succeeds in increasing its labor input and thus increases total sales, but keeps value added per worker constant. In this case it could be argued that the fluctuations in sales do not constitute an idiosyncratic firm shock.

3.2 Income Passthrough Regressions

We estimate the rate of passthrough to income with two complementary approaches, which differ in the degree of statistical structure they impose, but both use within job-spell variation in income and thus focus on passthrough to workers present before and after the change in firm performance. We refer to them as the non-structural and the structural approach..

The non-structural approach is inspired by Juhn et al. (2018) and Card et al. (2016) and correlates changes in worker income with changes in firm performance. Varying the time length over which these differences are formed allows us to investigate passthrough of permanent and transitory shocks in a more informal way. Longer differences capture shocks that are more persistent in nature, while shorter differences capture all shocks. In addition, we also estimate a series of IV models using differences from e.g. longer periods as instruments for shorter period differences, thereby isolating the part of the change in firm performance which is more permanent in nature.

In contrast to this, the structural approach explicitly models variations in (residual) firm performance as resulting from either permanent or transitory processes. This approach adds more parametric structure on the firm performance and worker income processes in the spirit of Guiso et al. (2005). The added structure results in estimates which are easier to interpret and we can explicitly distinguish between passthrough of permanent versus transitory shocks.

3.2.1 Non-structural Approach

Our non-structural approach consists of a series of separate OLS and IV regressions. For the OLS regressions, we estimate the following relationship

$$\Delta_x w_{ijt} = \gamma \Delta_x f p_{jt} + X_{ijt} \delta + \epsilon_{ijt}, \quad (1)$$

where $\Delta_x w_{ijt}$ measures changes in worker income for worker i in firm j and time t . Δ_x is the difference over x years taken in a window around t , for example $\Delta_3 w_{ijt} = w_{ijt+1} - w_{ijt-2}$ is the difference over 3 years. Similarly $\Delta_5 w_{ijt} = w_{ijt+2} - w_{ijt-3}$ and $\Delta_1 w_{ijt} = w_{ijt} - w_{ijt-1}$. A key point for our empirical strategy below is that these differences do not have common end points. $\Delta f p_{jt}$ measures changes in firm performance in firm j at time t . X_{ijt} is a set of control variables¹⁵ and ϵ_{ijt} is an error term.

The idea of the non-structural approach is to investigate how γ in Equation (1) changes as we alter the difference length x in both firm performance and worker income. By varying the difference length, we can get an indication of how worker income responds to shocks with different

¹⁵Our baseline set of control variables include: year dummies, linear, squared and cubic labor market experience, occupation group dummies (in total 4 dummies and one omitted group, see footnote 7), and dummies for industry (4 dummies and one omitted group, see footnote 47). The choice of including occupation/industry dummies as control variables allows us to control for variation potentially created by e.g. collective bargaining, while experience allows us to take general productivity increases due to human capital into account. We prefer this (parsimonious) set of controls as our baseline measure in order to also include the same set of controls in the structural approach, where increasing the set of control variables makes estimation slower and more demanding. Further, as discussed in the results section, our estimates are not very sensitive to the set of controls included.

degrees of persistence. If, for instance, the difference is taken over a long period, and γ is positive and larger than γ based on shorter periods, we interpret it as evidence of a higher degree of pass-through for longer lasting shocks. Juhn et al. (2018) write up a more formal econometric model with both permanent and transitory elements and show that γ is a weighted average of differential rates of passthrough for transitory and permanent shocks with the weight depending on the length of the period which the difference is taken over. Of course, if shocks are longer lasting but still transitory they may still materialize in longer-term differences. Therefore, we should generally be careful in interpreting estimates based on longer term differences as the effect of permanent shocks only.

For the IV regressions, we instrument $\Delta_x f p_{ijt}$ with differences taken over a different length than x . The IV approach has several advantages. First, instrumenting with overlapping periods without common end points filters out period specific measurement error in the firm's performance, which would result in attenuation bias. Note that this holds both when the instruments are longer and shorter period differences since endpoints are never overlapping. Second, as the difference length for the instrument increases, it also filters out transitory (but still somewhat longer lasting) shocks. Instruments based on longer periods should only contain transitory shocks which are present in the period endpoints. Hence, when differences are taken over long enough periods, they should not correlate with the part of shorter period changes which arise from temporary shocks. Similarly, when using shorter period differences as instruments for longer period differences, we are also focusing on the part of the variation in $\Delta_x f p$ which is present in both differences. Hence, longer lasting shocks which materialize around period t and remains to materialize in longer differences as well. Lastly, the IV regressions can also be thought of as addressing any remaining concerns about whether short term dynamics are driven by reverse causality, i.e. where changes in worker productivity directly affect firm performance. Note first, that we already mitigate some of these concerns by focusing on larger firms, which are not as affected by individual level worker shocks. We also control for industry, occupation, and year effects, so more aggregate effects such as industry or occupation wage floors are taken out. However, some concerns about individual level worker shocks might still persist and the IV approach is partly designed to alleviate some of these concerns. Imagine that a worker has a short-term negative productivity shock (due to e.g. temporary illness, a sick child, etc.) between year $t - 1$ and t . This results in two things. First, the worker gets a lower wage, $\Delta_1 w_{ijt} < 0$, and second, the firm's performance is now adversely affected, $\Delta_1 f p_{jt} < 0$. Estimating Equation (1) will produce an upward bias estimate of γ due to reverse causality. Instrumenting $\Delta_1 f p_{jt}$ with $\Delta_3 f p_{jt}$ would remove the upward bias if the worker shock was only present in one year.¹⁶

¹⁶In the case of reverse causality, where long-term firm performance is affected directly by long-term changes in worker productivity, the problem of course still remains. However, given our statistical (data driven) definition of what constitutes a firm shock it can also be argued that such long term changes in firm productivity resulting from longer lasting decreases in worker productivity may be considered a firm shock (see also the discussion in Section 3.1). Consider for example the case of a very influential sales/marketing employee leaving the firm and affecting total firm sales in many years ahead, this shock may trigger pass through to remaining employees similar to response from declines in sales driven by other factors, and our estimates would quantify how the firm on average responds such

3.2.2 Structural Approach

The structural approach consists of three steps. First, we estimate a dynamic panel data model describing the evolution of firm performance. Second, we perform a similar exercise for worker income. Third, we analyze the covariance between the residuals obtained in connection with the first two steps. Since the structural approach is similar to Guiso et al. (2005), we keep the presentation of each of these steps short and refer to Appendix B and the original paper for additional details.

Firm Performance As a first step, we estimate a dynamic panel data model of firm performance. The overall aim is to net out predictable dynamics and aggregate or industry-level shocks such that the estimated error terms are capturing firm-specific shocks and residual idiosyncratic noise only. The model is given by

$$fp_{jt} = \rho fp_{jt-1} + Z'_{jt}\gamma + h_j + \epsilon_{jt}, \quad (2)$$

where $j = 1, 2, \dots, J$ is a firm index and $t = 1, 2, \dots, T$ is the time period index. fp_{jt} is the firm performance measure (in logs), h_j is the time-invariant firm fixed effect, and ϵ_{jt} is the (compound) error term which contains both permanent and transitory components, see Appendix A.2 for further details. ρ influences the persistence of performance and captures “predictable” dynamics, such as precommitted sales or expenditures. Z_{jt} contains strictly exogenous observable covariates and consists of firm age and firm age squared, in addition to year and industry dummies.¹⁷

We take first differences of Equation (2) and use the IV estimation procedure (and tests) developed by Arrelano and Bond (1991) using properly lagged dependent variables as instruments. To select the appropriate instruments, i.e. fp_{jt-x} , we analyze the error term and in particular the auto-covariances.¹⁸ In addition, we also test for the presence of both transitory and permanent parts of the error process.¹⁹

Income Regressions As a second step of the structural approach, we model worker income as the sum of deterministic components (observable covariates and a worker fixed effect), permanent and transitory firm shocks, and finally an error term:

shocks for remaining workers.

¹⁷Note that we always include dummies directly in our first-differences specifications. As we explain in the results section, we also try more elaborate versions with additional controls such as region dummies, controls for assets, year by industry fixed effects. Changing the set of controls only marginally affects the characteristics of the estimated error term and hence the subsequent analysis.

¹⁸ fp_{t-x} is only a valid instrument when it is uncorrelated with $\Delta\epsilon_{jt}$, else it does not satisfy the exclusion restriction. This requires the absence of auto-correlation of an order greater than or equal to x . To validate the choice of instruments we use the AB test as developed in Arrelano and Bond (1991). Note further that one cannot use the System GMM estimation approach due to the assumed structure of the error terms, see the discussion in Rute Cardoso and Portela (2009).

¹⁹Following Guiso et al. (2005), we specify a test for presence of the permanent part of the error term (a random walk component) where the null hypothesis is $H_0 : E(\Delta\epsilon(\sum_{\tau=-2}^2 \Delta\epsilon_{jt-\tau})) = 0$, i.e. no random walk, and with an alternative hypothesis of $H_1 : E(\Delta\epsilon(\sum_{\tau=-2}^2 \Delta\epsilon_{jt-\tau})) = \sigma_a^2$. The idea of the test is to average out the transitory parts of the error term (modeled as an moving average component) from Equation (8) (see Appendix A.2 and Guiso et al. (2005) for further details).

$$w_{ijt} = X'_{ijt}\delta + h_i + \alpha P_{jt} + \beta T_{jt} + \psi_{ijt}, \quad (3)$$

where $i = 1, 2, \dots, N$ index individuals, $j = 1, 2, \dots, J$ index firms, and $t = 1, 2, \dots, T$ index time. w_{ijt} is the measure of worker income of worker i at firm j at time t . h_i is the worker fixed effect. X_{ijt} is a collection of the observable covariates which are strictly exogenous. This contains experience, experience squared and cubed, industry, occupation, and year dummies (see footnote 15). Again these covariates are included to control for predictable dynamics and aggregate effects. The error term, ψ_{ijt} , captures worker-specific shocks and may be auto-correlated.

Shocks to firm performance enters in two ways: As a transitory term, T_{jt} , and a permanent term, P_{jt} . The key parameters are thus α and β , which govern passthrough of permanent and transitory shocks. Note that the variables T_{jt} and P_{jt} are unobserved and not directly recovered from the estimation of the firm performance process above. Instead estimation of α and β proceeds by constructing a compound residual from the worker equation, which removes the role of the other components from above. Again, we take first differences of Equation (3) and use an IV estimation procedure with properly lagged dependent variables and the AB test developed in Arrelano and Bond (1991) to choose appropriate instruments. In Appendix B we write up the approach in more detail.

Final Regressions As a last step in the structural approach we analyze the correlation between the residuals $\Delta\omega_{ijt}$ ²⁰ and $\Delta\epsilon_{jt}$. Guiso et al. (2005) show that a set of moment conditions can be used to filter out the separate importance of transitory and permanent shocks to firm performance, T_{jt} and P_{jt} , and thus estimate α and β . The derivation of the specific instruments leverages that T_{jt} and P_{jt} are assumed to follow specific statistical processes/functional forms.

The moment conditions can be represented as two separate IV regressions. First, to separate the role of transitory shocks we use $\Delta\epsilon_{jt+1}$ as an instrument for $\Delta\epsilon_{jt}$. This instrument exploits the mean reverting behavior of transitory shocks. Second, to separate the role of permanent shocks we use $\sum_{\tau=-2}^2 \Delta\epsilon_{jt+\tau}$ as an instrument for $\Delta\epsilon_{jt}$. By summing the firm residual over 5-years, we are essentially removing the transitory part of $\Delta\epsilon_{jt}$ such that what is left is the permanent component. See Appendix B for the formal moments conditions and additional details.

3.2.3 Comparison of the Two Approaches

We view the non-structural and structural approaches as complementary as they have different advantages and disadvantages. While the structural approach delivers separate estimates on the rate of passthrough for permanent and transitory shocks, α and β , this approach also relies on very specific functional form assumptions for the innovations to firm performance which may not be innocuous. On the contrary, estimates from the non-structural approach avoid such assumptions

²⁰ $\Delta\omega_{ijt} = \alpha(1 - \rho L)u_{jt} + \beta\Delta v_{jt} + (1 - \rho L)\Delta\psi_{ijt}$ is the composite error term from the worker equation in Equation (3) containing both permanent (u_{jt}) and transitory (v_{jt}) innovations from firm performance. See Appendix B for more details.

and should hence be more robust under alternative firm performance processes and/or measurement errors, but the estimates are also harder to interpret and summarize.²¹

The two approaches also differ in their sample requirements. The structural approach focuses on a panel of workers present in a firm for 5-years or more.²² While the non-structural estimates based on 5-year differences imply similar sample requirements, some of the shorter-run estimates from the non-structural approach can also be used in shorter job spells and thus include less attached workers. For instance, the simple first-difference model only requires observing a worker in a firm for 2 periods. Extending the analysis to less attached workers allows us to inspect the external validity of the estimates of passthrough obtained for the 5-year stayers. If the estimates based on shorter run differences are higher in the unrestricted samples, it suggests that long-term stayers experience lower rates of passthrough on income compared to the full sample of workers.

Overall, the two approaches share the same fundamental idea on how to separate the importance of permanent and transitory shocks. In both approaches long-term changes in firm performance are mostly informative about the role of permanent shocks, while short-term changes may be a result of both transitory and permanent changes as well as measurement error. In fact, as shown in Juhn et al. (2018) the instruments used in the final step of the structural approach to separate transitory and permanent effects are kind of similar to the 1-year and 5-year differences used in the non-structural approach except that the “structural approach” forms these differences after initial filtering as in e.g. Equation (2), whereas the non-structural approach works directly with e.g. the firm performance measures.²³ I.e. a key difference is that the structural approach uses residuals from an initial estimation of fp_{jt} , where predictable dynamics are taken out. In particular, the structural approach allows for an AR(1) in fp_{jt} and thus allows for persistence of transitory shocks. As we show below this distinction is not just expositional, see Section 4.2.

By using both approaches on the same data sample we can informally assess the robustness of our results. Below, we show that both approaches suggest that passthrough of permanent shocks is more pronounced than transitory shocks. Since this finding also holds across all three firm performance measures, and the two income measures, it suggests that the result is generally very robust although the exact rate of passthrough may be debated.

3.3 Employment Passthrough Regressions

To quantify passthrough on the employment margin, we set up the following equation

²¹As argued above, the nonstructural IV estimates filters out measurement error which results in classical attenuation bias in the OLS estimates. Further, the IV estimates alleviates concerns about reverse causality, where shocks to workers productivity affect general firm performance. Juhn et al. (2018) have a nice discussion and illustration of these points. Note that our structural framework interprets short run dynamics due to measurement error as transitory shocks.

²²Two periods are lost due to the lagged dependent variable and the transformation to first differences. Further estimating equation 3 requires using instruments (lagged dependent variables) starting from $t - 3$ or $t - 4$.

²³Note that the structural approach (see also Appendix B) use the moment condition $E(\sum_{\tau=-2}^2 \Delta \epsilon_{jt+\tau} (\Delta \omega_{ijt} - \alpha \Delta \epsilon_{jt})) = 0$ to identify α . $\sum_{\tau=-2}^2 \Delta \epsilon_{jt+\tau} = \epsilon_{jt+2} - \epsilon_{jt-3}$ is thus similar to the 5-year difference $fp_{jt+2} - fp_{jt-3}$ used in the non-structural approach.

$$e_{ijt} = \gamma \Delta f p_{jt} + \delta X_{ijt-1} + \epsilon_{ijt}, \quad (4)$$

where e_{ijt} is 1 if worker i exits from firm j and enters unemployment at some point between measurements $t - 1$ and t , and e_{ijt} is 0 if the worker is still present in the firm at time t . We also run a separate analysis, where e_{ijt} is 1 if the worker exits to other firms (i.e. job-to-job transitions) and 0 if the worker is still present. e_{ijt} is set to missing if the worker exits the firm and enters some other state than the state of interest (e.g. early retirement) in a specific year t . X_{ijt-1} are exogenous covariates measured in year $t - 1$. They are included to control for factors which may correlate with both changes in firm performance and employment separations. In our benchmark specification these variables include a full set of year and age dummies, as well as broad occupation and industry dummies.²⁴

$\Delta f p_{jt}$ measures the change in firm performance between years $t - 1$ and t . γ therefore measures the covariance between changes in firm performance and the risk of becoming unemployed (making a job-to-job transition) for workers working in the firm at the beginning of the period. Note that a key difference to modeling the effect of firm performance on worker income is that the dependent variable, e_{ijt} , is binary and 0 throughout the job spell except for the last period. As a result there is no within job spell variation to exploit, which is what we used when focusing on passthrough to worker income for stayers. Also, we do not consider longer differences of the dependent variable around a given point in time, which no longer makes sense since the worker has left the job (we do however still use longer differences of firm performance as we explain below). Instead, γ is now determined from across and within firm variation in the timing of employment exits controlling for individual level heterogeneity such as e.g. occupation and age.

3.3.1 Reverse Causality Concerns

Studying the interaction between changes in firm performance and employment adjustments may trigger concerns about reverse causality, since the worker who leaves the firm may likely have a direct impact on the firms performance if he/she is not immediately replaced. By construction it is hard to filter out whether a difference in firm performance across two years arises due to a shock to firm performance only, or whether the change in firm performance is a result of the worker exiting. Similar to our identification strategy when studying passthrough to income using the non-structural IV approach, we address the reverse causality issue by constructing longer differences of firm performance and use these as instruments for the annual changes in firm performance. Longer differences should reduce most reverse causality concerns, since variation from

²⁴We use a larger (and more detailed) set of control variables compared to the analysis of worker income primarily because the dependent variable is now binary and does not vary within job spells. These additional control variables are included to control for potentially confounding employment changes. E.g. age dummies are included to control for aggregate life-cycle employment patterns (see Andersen et al. (2017)). Generally, we find that adding additional controls (such as controls for tenure, education or even adding worker fixed effects) change our results very little, which may not be surprising since we focus on changes in $f p_{jt}$ and larger firms where reverse causality issues may be less prevalent (see also the discussion in the next subsection).

shorter run dynamics, such as e.g. finding a replacement worker or restructuring production, is not used as identifying variation.²⁵ Specifically, we use instruments $\Delta_3 f p_{jt} = f p_{jt+1} - f p_{jt-2}$ and $\Delta_5 f p_{jt} = f p_{jt+2} - f p_{jt-3}$. These instruments are similar to the instruments used in the non-structural analysis of passthrough to income. We also use the set of instruments used in the final step of the structural approach (see Section 3.2.2). In particular, we take the residuals from the firm side estimation of the structural approach and instrument $\Delta f p_{jt}$ with $\Delta \epsilon_{jt+1}$ and $\sum_{\tau=-2}^2 \Delta \epsilon_{jt+\tau}$ separately (with ϵ_{jt} defined as in Equation 2). We use these two instruments for $\Delta f p_{jt}$ and interpret the resulting estimates as related to passthrough to employment of transitory and permanent shocks respectively.²⁶

Further, the issue of reverse causality likely varies across firm performance measures. With total sales or value added, it is of course clear that a worker's exit may directly change the level of production and reverse causality may obviously become a problem. However, we attempt to handle this using the IV approach described above. As an additional check, we add controls for firm size (and changes in firm size) measured in different periods prior to period t in Equation (4) to control for changes in labor input. Obviously, controlling for changes in firm size between $t - 1$ and t changes the interpretation of γ as it now measures the relative response of a firm shock conditional on changes in firm size. We discuss this further when we look at the resulting estimates.

We do not use value added per worker in the analysis of passthrough to employment. First, per worker measures such as value added per worker is not well-suited to study passthrough to employment as firms can eliminate the impact of a firm shock by firing or hiring workers. I.e. changes in the number of workers are a part of the endogenous response to a firm shock which we are trying to measure.²⁷ As an example, assume that firms simply adjust their workforce to a large negative sales shock and succeeds in keeping value added per worker constant. In the data we would then see workers exiting the firm while value added per worker is unchanged and this would tend weaken the empirical relationship between value added per worker and worker exits. Second, if there is measurement error in when the worker exits from a firm, then this will positively

²⁵The concern about reverse causality also varies depending on which particular outcome we study. It may, for example, be reasonable to expect that the firm can offer compensation or otherwise prevent transitions into unemployment, which are likely to lead to adverse effects on long term performance of the firm. This is less likely when we consider worker exits such as job-to-job transitions. However, also in this case, the firm should be willing to compensate the worker if he/she is instrumental to performance.

²⁶In the analysis of passthrough to income the formal (structural) link to α and β was directly specified, and hence estimates are interpretable as the effect of transitory and permanent shocks. For the analysis on the employment margin this link is of course less formal and left unspecified, so results should be interpreted with more caution. We prefer to leave it unspecified because of particular structure of the binary outcome variable as mentioned above.

²⁷It may be tempting to simply work with value added per "lagged worker", i.e. where we use the measure of full-time workers from the previous period. This is however not as straightforward in our setup since we are not working with a one-time firm shock (as in a standard event type setup) but rather a whole timeseries of firm shocks. This has the implication that using measures based on previous periods we would risk confounding the analysis of current passthrough rates with previous periods shocks and thus violating orthogonality conditions (this is especially important in the non-structural approach where we use longer differences as instruments and construct differences such that end points do not overlap). A separate issue is that employment turnover is important in our sample, therefore previous period levels of employment would only be an imperfect proxy.

bias the results when we use a “per worker” measure. The basic problem is that mismeasuring an exiting worker will automatically give a positive correlation between exiting (which will increase from 0 to 1) and the “per worker” measure, which will increase, since we now divide by less workers. Thus, measurement error will not only cause attenuation bias as it would in the income passthrough analysis, but it will positively bias γ . We therefore focus the analysis on the two firm performance measures, which directly measure levels: Sales and value added.

4 Passthrough to Income

In this section, we present our results on passthrough to worker income. We first focus on the estimates from the non-structural approach and then proceed with the structural approach. For the non-structural approach, we discuss results separately for a restricted and unrestricted sample. The restricted sample is the sample where we include all observations with a 5-year window around them in order to produce 5-year differences and thus all estimates are based on the same sample. The unrestricted sample is the sample where we use as many observations as possible for each single estimator, and thus the sample size changes across specifications. For our structural approach we compare our results to existing estimates in the literature and analyze heterogeneity across subgroups.²⁸ We end the section by comparing our non-structural and structural results.

4.1 Non-structural Approach

In Table 1, we present the results for worker earnings using the non-structural approach. The table contains results for all the three firm performance measures and both the OLS and IV models resulting in 6 different panels. We first focus on the results from OLS which are reported in the left panels. Here we regress differences in earnings on similar differences of firm performance and add our baseline controls presented in Section 3.2. The results are for the models using the first, third, and fifth differences. Notice that the estimates are elasticities, so the interpretation is that a one percent shock to firm performance correlates with an x percent change in earnings. Across all firm performance measures, we see that the estimates based on 5-year differences are larger than those from 1-year differences. This suggests that there are higher passthrough rates of long term changes in firm performance compared to short term changes consistent with a higher passthrough rate of longer lasting or even permanent firm shocks and/or potentially measurement error in shorter term dynamics.

In the right panels in Table 1, we present results from the IV approach. Like for OLS we regress same period differences (first, third, and fifth) of earnings on firm performance, but we instrument the differences in firm performance with differences based on other period lengths. The instrument is in the left column. Comparing the IV and OLS estimates in Column (1), which

²⁸The structural approach quantifies passthrough with two parameters, α and β . This makes it easier to look at differences in passthrough rates across subgroups of workers compared to the non-structural approach, where there were many more estimates.

Table 1: Non-Structural Results: Earnings

Value Added (OLS)				Value Added (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.028** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.037** (0.001)	0.065** (0.002)
$\Delta_3 f p_{ijt}$	x	0.036** (0.001)	x	$\Delta_3 f p_{ijt}$	0.038** (0.001)	x	0.063** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.063** (0.001)	$\Delta_5 f p_{ijt}$	0.042** (0.001)	0.048** (0.001)	x
Value Added per worker (OLS)				Value Added per worker (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.018** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.018** (0.001)	0.035** (0.003)
$\Delta_3 f p_{ijt}$	x	0.021** (0.001)	x	$\Delta_3 f p_{ijt}$	0.022** (0.001)	x	0.027** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.023** (0.001)	$\Delta_5 f p_{ijt}$	0.028** (0.002)	0.025** (0.001)	x
Sales (OLS)				Sales (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.040** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.052** (0.001)	0.062** (0.001)
$\Delta_3 f p_{ijt}$	x	0.046** (0.001)	x	$\Delta_3 f p_{ijt}$	0.050** (0.001)	x	0.067** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.075** (0.001)	$\Delta_5 f p_{ijt}$	0.040** (0.001)	0.052** (0.001)	x
Obs	3,500,106	3,500,106	3,500,106	Obs	3,500,106	3,500,106	3,500,106

Note: This table shows OLS (left panels) and IV (right panels) estimates for the non-structural approach (see Section 3.2.1) for earnings as the worker income measure. For the IV models (right panels) the columns define the differences used in the main specification, and the rows defines the instruments used. Standard errors are clustered at the worker level and reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels. Control variables in the regression are year dummies, linear, squared and cubed experience measures, occupation group dummies, and dummies for industry. In Table 18 in Appendix C.3 we show that results change very little when additional controls are included.

both focus on one-period changes (Δ_1), across all firm performance measures, we see that the IV estimates are generally larger across all firm performance measures as expected, since the IV regression isolates the part of the variation in firm performance, which is more closely linked to permanent or at least longer lasting shocks and should also decrease any attenuation bias from measurement error in firm performance.²⁹ Comparing OLS and IV estimates in Columns (2) and (3), we see similar changes, but the change is smallest when we look at 5-year differences where IV and OLS estimates are more similar across all the firm performance measures. In fact the (5-year, 5-year) difference regression using smaller periods as instruments for sales decreases the estimate of passthrough. Further note that the estimates of passthrough do not change that much when we change the instrument period used for a given difference, i.e. go down the columns inside a particular IV panel.³⁰

In Table 2, we show the results based on hourly wages instead of earnings.³¹ Broadly speaking most of the elasticities are similar in magnitude to the elasticities based on earnings, and our conclusion about the relative importance of longer lasting shocks is the same. Nevertheless comparing each point estimate separately, we see that passthrough is almost always smaller for hourly wages than for earnings when we look at value added and sales. The difference is largest when we look at our IV models based on longer differences (Column (3)). Overall, this suggests that shocks to sales and value added trigger changes in both the payment per hour as well as changes in the number of hours worked for stayers³², whereas shorter lasting shocks primarily change our (estimated) hourly wages. We do not see the same patterns when we consider value added per worker as the firm performance measure, i.e. compare Column (3) for value added per worker in Table 2 to Table 1. Rather the estimates based on hourly wages versus earnings are pretty comparable in size and perhaps even slightly larger for hourly wages. This makes sense since ideally value added per worker should actually be net of fluctuations in value added due to simply changing the number of hours.

If we now change the comparison and focus on comparing the estimates based on value added to the estimates based on value added per worker, we see that passthrough rates are generally

²⁹The fact that IV estimates are generally larger than OLS estimates in Column (1) could generally be driven by at least two things. First, the IV regressions filter out any period specific measurement error in firm performance. Second, instruments based on longer period differences isolate the part of the variation in firm performance which is longer lasting and longer lasting shocks may have higher rates of passthrough (see also the discussion in Section 3.2.1).

³⁰Comparing our results to Juhn et al. (2018) where they use measures of earnings and sales, we see that our corresponding OLS estimates are generally around 2-3 times as large, and that the increasing passthrough rates for longer differences are much more pronounced in our sample. Our IV estimates are more similar to Juhn et al. (2018) but generally more stable, a possible explanation is that measurement error is less pronounced in the Danish register data. Still the results may suggest that the passthrough of permanent shocks is higher in the Danish labor market.

³¹Keep in mind that we are most likely reporting an upper bound on the passthrough to hourly wages because overtime work etc. in smaller portions will increase earnings, but not the estimate on worked hours. As a result Statistics Denmark estimate on hourly wages may display too much variation (see Section 2).

³²It may be alluring to conclude that the longer lasting shocks entail larger adjustments on hours since the largest differences between earnings and wages in the IV models are those based on longer differences (Column (3)). But, in the section with our structural results we show that the change in hours is actually not driven by permanent shocks per se. Rather the behavior is driven by a fairly high degree of persistence of transitory shocks. Instead permanent type shocks primarily affect the wage and employment margin.

Table 2: Non-Structural Results: Hourly Wages

Value Added (OLS)				Value Added (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.016** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.035** (0.001)	0.033** (0.002)
$\Delta_3 f p_{ijt}$	x	0.027** (0.001)	x	$\Delta_3 f p_{ijt}$	0.035** (0.001)	x	0.038** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.028** (0.001)	$\Delta_5 f p_{ijt}$	0.040** (0.001)	0.039** (0.001)	x
Value Added per worker (OLS)				Value Added per worker (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.015** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.031** (0.001)	0.028** (0.001)
$\Delta_3 f p_{ijt}$	x	0.023** (0.001)	x	$\Delta_3 f p_{ijt}$	0.033** (0.001)	x	0.036** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.027** (0.001)	$\Delta_5 f p_{ijt}$	0.038** (0.001)	0.036** (0.001)	x
Sales (OLS)				Sales (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.027** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.045** (0.001)	0.040** (0.001)
$\Delta_3 f p_{ijt}$	x	0.038** (0.001)	x	$\Delta_3 f p_{ijt}$	0.045** (0.001)	x	0.043** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.037** (0.001)	$\Delta_5 f p_{ijt}$	0.046** (0.001)	0.046** (0.001)	x
Obs	3,500,106	3,500,106	3,500,106	Obs	3,500,106	3,500,106	3,500,106

Note: This table shows OLS (left panels) and IV (right panels) estimates for the non-structural approach (see Section 3.2.1) for hourly wages as the worker income measure. For the IV models (right panels) columns define the differences used in the main specification, and the rows define the instruments used. Standard errors are clustered at the worker level and reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels. Control variables in the regression are year dummies, linear, squared and cubed experience measures, occupation group dummies, and dummies for industry.

higher for value added than value added per worker both for earnings and hourly wages (Tables 2 and 1). The difference however is largest for earnings. The only difference in these two measures is the conversion into per worker units. This is an indication that the firms also make hours or employment adjustments in response to changes in value added. Such adjustments change value added per worker and thereby indirectly affect the estimated passthrough.³³ Imagine the simple example with a linear production technology in labor input. If a firm is hit by a positive demand shock and it might endogenously responds by increasing the number of hours worked for existing workers to keep up with demand. But the firm keeps the hourly wage constant and does not hire new workers. Sales and total value added goes up due to the shock. Earnings goes up due to the increase in hours, but value added per worker measured in full-time equivalents does not change at all. In this case, it is clear that value added per worker is simply less correlated with earnings than total value added or sales simply because value added per worker is net of the firms adjustment on the labor input margin.³⁴ Overall these differences across firm performance measures is one of the reasons why studies using different outcomes may have different findings. In Section 4.2.2, we explicitly compare our estimates based on different measures to those in the literature.

Tables 1 and 2 focus on the *same* sample of workers across all specifications to ease comparison across different instruments. Obviously, this also implies focusing on workers who work in the firm for at least 5-years. Thus, our sample is a particular type of workers.³⁵ It is of course interesting to see how the estimates change when we include workers with shorter durations in the firm when possible. Tables 14 and 15 in Appendix C.1 present results comparable to Tables 1 and 2, but now using the maximum number of observations possible in each regression. Comparing the

³³Keep in mind that in our setup hours adjustments may affect both the measure of firm performance (and in particular value added per worker) as well as the measure of worker income, especially when we consider earnings for stayers. At the same time hours adjustments in the firm, and thus changes in value added per worker compared to value added, may not necessarily imply that stayers work more (or less) if the firm instead hire new workers or increase hours for more marginally attached workers.

³⁴To further illustrate this, we can do the approximate decomposition since all variables are in logs:

$$\text{cov}(\Delta \log(w), \Delta \log(VA/N)) + \text{cov}(\Delta \log(w), \Delta \log(N)) = \text{cov}(\Delta \log(w), \Delta \log(VA)),$$

where VA is total value added, w is worker income, and N is the size of the firm (e.g. total hours), which includes both stayers and non-stayers in a given year. The decomposition illustrates that results based on value added and value added per worker can differ if changes in worker income also correlates with changes in total firm hours. Since we find that $\text{cov}(\Delta \log(w), \Delta \log(VA/N)) < \text{cov}(\Delta \log(w), \Delta \log(VA))$, this suggests that $\text{cov}(\Delta \log(w), \Delta \log(N)) > 0$ holds in our sample. I.e. as the firm changes its total hours (by either hiring more workers or demanding more hours per worker), this also changes workers income on average in our sample. An obvious interpretation is therefore that the lower passthrough rates based on value added per worker reflects that the firm actively responds to shocks by increasing total hours alongside with adjustments on w (either mechanically with more hours and hence more earnings, or with smaller changes in hourly wages, e.g. overtime work). The fact that passthrough rates are similar for value added and value added per worker when we look at wages also makes sense since we would expect $\text{cov}(\Delta \log(w), \Delta \log(N))$ to be smaller when w is wages contrary to earnings.

³⁵Naturally there is some selection into the group of workers (here called stayers) who stay in the same firm for at least 5 years. Therefore our estimates may not be externally valid for all workers. At the same time the selection process into stayers is as such not interesting if the object of interest is quantifying passthrough for stayers only. In other words, our estimates for stayers are not biased because there is selection into who stays in the firm but it is most likely not externally valid for all workers or across all settings.

1-year and 3-year differences for hourly wages in the unrestricted sample (Table 15) to the same differences for the restricted sample (Table 2), estimates are quite similar regardless of the measure of firm performance. This suggests that passthrough to hourly wages is similar across samples and thus the same for 5-year stayers and those that are more marginally attached to the firm. We do not see the same when we consider earnings. Here the estimates based on the unrestricted sample (Table 14 compared to Table 1) are larger for sales and value added. We take the fact that we see similar results for hourly wages in the unrestricted and restricted samples as suggestive evidence that although the samples are different, our differential results on earnings (i.e. comparing the results based on the restricted and the unrestricted samples) are not just driven by differences in the sample composition. Rather, Table 14 suggests that adjustments on hours are more pronounced in the unrestricted sample, i.e. firms adjust to shocks by decreasing hours worked by marginal workers to a larger extent than they adjust hours for stayers.

Lastly, in Appendix C we provide some additional results. First, we show that our results are not very sensitive to the set of controls included. We have tried using both no controls and including more controls (age dummies, gender, years of education dummies, and education type dummies). See Appendix Section C.3 for these robustness checks. Second, in Appendix C.2 in Tables 16 and 17 we reproduce our results on an extended sample where we also allow smaller firms to enter the sample (see Section 2.2 for a discussion of our restriction on firm size). The results are generally very similar to the results in the main text and suggests that our overall conclusions from above extend to this larger sample. In terms of magnitude we perhaps get slightly larger estimates suggesting that passthrough may vary with the size of the firm.

In summary, our initial analysis of passthrough rates using our non-structural approach shows some interesting patterns. First, passthrough of more permanent or at least longer lasting shocks is larger than shorter term shocks. Second, how we measure income and firm performance matters. The rate of passthrough is generally larger when we use earnings and smaller when we look at value added per worker. Third, we see larger effects on earnings when we relax the sample to include workers that are not present for 5 consecutive years. This seems to be driven by an hours response of those workers that are stayers in the short-term (1-3 years), but not stayers in the long-term (5-years). These findings motivate the next two set of results. First, we analyze the passthrough rates using our structural approach allowing us more formally to separate the effect of permanent and transitory shocks. And second, in the next section we look closer at the employment dynamics.

4.2 Passthrough to Income: Structural Approach

In this section, we present the results from the structural approach. We then compare the obtained estimates to the literature and finally, we show how the passthrough rates vary with observable characteristics.

4.2.1 Firm Performance and Worker Income

We now present the results from the structural approach. As described in Section 3 this approach consists of three steps. First, we estimate the firm performance process and generate residuals. Second, we perform a similar exercise for worker income, and third, we estimate the parameters α and β using two IV regressions (Equations (11) and (14)).

In Table 3, we present the estimates for the firm performance process. Across all three measures of firm performance, we find a fairly high estimate of persistence, ρ . It is highest for sales and value added. This is expected as total value added and number of employees are highly correlated, and therefore a part of the persistence of the process is likely removed when removing the number of employees, i.e. constructing value added per worker. We end up using different lags as instruments for the different measures. For value added per worker and sales we use lags 4 and 5, while for value added in total we use lags 3 and 4. Our choice of instruments is based on the Arrelano and Bond (1991) test statistics, which are reported in the table. Given the chosen instruments, we cannot reject the null of no auto-correlation of the same order as our instruments suggesting that our instruments are valid.³⁶ For each model, we also present the test statistic of the random walk component in $\Delta\epsilon_{jt}$, and in all cases we reject the null hypothesis of no random walk, i.e. our firm performance residuals have both permanent and transitory components supporting our theoretical formulation of the error term above.

In the next part of the table, we report the respective contributions to the overall variance in the error term, i.e. we show the variance of both the transitory and permanent parts of the firm shock as well as their ratio. The variance of the transitory part is for all models much higher than the variance for the permanent part. This is comparable to earlier results, where Guertzgen (2014) and Guiso et al. (2005) finds ratios of about 14 and 3, respectively. Finally, we report the correlation between the residuals from the different models of firm performance. All are clearly positively related, with sales and value added per worker being the pair with the lowest correlation of 0.36. In the Appendix Section C.4.1, we provide extensive robustness checks of our estimates. Table 19 in the Appendix shows how the results for the different firm performance measures change when we make alternative choices of the sample, estimator, control variables, and lag structure. Based on this, our main conclusions seem to be robust, i.e. there is a high degree of persistence and the transitory part of the error term is more important in terms of contributing to the overall variance than the permanent part.

We now move to the estimates of the income process, i.e. Equation (3). The estimates are reported in Table 20 in Appendix C.4.2. Again, we estimate a dynamic panel data model using the Arrelano-Bond estimator. We end up using different lags as instruments across the two specifications: For hourly wages we find the most appropriate specification when using lags 3 to 5 as instruments. Earnings display much more serial correlation and there we end up using instruments based on lags 4 to 5. Again the Arrelano-Bond statistic supports these choices.

³⁶For value added there is a small spike in the auto-covariance function in period 4, but changing the set of instruments does not change our estimates of passthrough, see Appendix Section C.4.3.

Table 3: Firm Performance Estimates

	VA	VA per worker	Sales
ρ	0.431** (0.015)	0.292** (0.041)	0.507** (0.015)
Observations	196,909	196,909	196,909
Instruments	fp_{jt-3}, fp_{jt-4}	fp_{jt-4}, fp_{jt-5}	fp_{jt-4}, fp_{jt-5}

Specification tests	VA	VA per worker	Sales
AB(2) test statistic	4.98**	2.24**	3.37**
AB(3) test statistic	1.35	-2.84**	2.94**
AB(4) test statistic	2.17**	0.35	1.05
AB(5) test statistic	-0.57	-1.89	-0.23
RW test	19.13**	17.66**	14.76**

Variance components	VA	VA per worker	Sales
Variance transitory part, σ_v^2	0.06	0.04	0.04
Variance permanent part, σ_u^2	0.01	0.01	0.01
Ratio (transitory/permanent)	4.97	7.96	5.18

Correlation between residuals	VA	VA per worker	Sales
Value added	1	0.77	0.62
Value added per worker	0.77	x	0.36
Sales	0.62	0.36	x

Note: This table reports the estimates of the firm performance process as specified in section 3.2.2. VA is total value added. Control variables are firm age, firm age squared and year and industry dummies. Note that the number of observations represents the number of firm-years used in the estimation. The AB test and RW test are explained in footnotes 18 and 19 respectively. The variance components are determined by the equations $E(\Delta\epsilon_{jt}\Delta\epsilon_{jt-1}) = -E\tilde{v}_{jt}^2 = -\sigma_v^2$ and $E(\Delta\epsilon_{jt}(\Delta\epsilon_{jt-1} + \Delta\epsilon_{jt} + \Delta\epsilon_{jt+1})) = \sigma_u^2$. Standard errors are reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

Finally, we apply the third step of the estimation strategy outlined in Section 3.2.2, which enables us to estimate the two key components regarding passthrough, namely the importance of the permanent, α , and transitory, β , firm performance shocks on workers income. Table 4 provides the results using all the three firm performance measures for earnings and hourly wages in the top and bottom panel, respectively. This gives us a total of six different specifications.

In Table 4 we report the estimates of α and β . Focusing first on the effect of transitory shocks, β , we find relatively small effects of around 1-2 percent. As opposed to some of the literature, however, the estimates are statistically significant which might not be surprising, since compared to e.g. Guiso et al. (2005) we have many more observations and thus smaller confidence intervals. Our estimates are actually only marginally outside the confidence intervals of Guiso et al. (2005). The passthrough of transitory shocks is economically small though. It is worth noting that the passthrough is slightly higher for earnings than hourly wages for all the three firm performance measures. This suggests that hours worked is a primary channel through which a firm respond to a transitory shock. If, for example, a firm experiences a temporary positive demand shock, then it makes sense that the firm adjusts partly by also increasing hours for stayers (and likely also all the other workers) in the firm.

Turning to the passthrough of permanent shocks, α , we find larger effects. This means that when a firm is hit by a permanent shock, we estimate that changes in worker income are around 5-9 percent of the same relative changes in firm performance within the same period. Contrary to the estimates on β , we do not see large differences when we compare estimates across our worker income measures hourly wages and earnings.³⁷ This suggests that changes in hours worked for stayers do not drive the passthrough estimates for earnings. However, as we will show in the next section the extensive employment margin is clearly affected by permanent shocks.

To facilitate comparison to our non-structural estimates in the previous sub-section, the last row of each panel in Table 4, reports the estimate from an OLS regression, where we regress firm residuals, $\Delta\epsilon_{jt}$, on first differenced worker residuals, $\Delta\omega_{ijt}$. These regressions are thus similar to the (1-difference, 1-difference) OLS regressions in e.g. Table 1, and the only difference is that we now work with residuals obtained after running the dynamic panel data models for firm performance and worker income. Our estimates in Table 4 show that while the residuals are positively correlated the estimates are smaller than what we found in the non-structural approach. This is an indication that it is not an innocuous choice whether to estimate the dynamic panel data models and use the residuals from this exercise, or instead focus directly on the “raw” variables and include control variables in the regression. Taking differences as in our non-structural approach corresponds to assuming that $\rho = 0$ (see Equation (2)), where ρ measures the persistence of shocks in the structural approach. However, we estimate ρ to be between 0.3 – 0.5 in the firm performance process above. This implies that transitory shocks have real effects even after a number of years. Thus, a part of the 5-year difference in the non-structural approach (which we partly interpreted

³⁷As shown in Table 21 the estimate on α based on sales (and earnings) increases when we include higher powers of the instrument. The estimate in Table 4 is therefore likely a lower bound.

Table 4: Structural Approach: Passthrough Estimates

Earnings	VA	VA per worker	Sales
Transitory, β	0.016** (0.001)	0.026** (0.001)	0.011** (0.001)
Obs.	5,688,055	5,688,055	5,688,055
Permanent, α	0.079** (0.006)	0.046** (0.005)	0.053** (0.009)
Obs.	2,752,431	2,752,431	2,752,431
OLS	0.016** (0.003)	0.015** (0.004)	0.020** (0.004)
Obs	3,500,106	3,500,106	3,500,106

Hourly Wage	VA	VA per worker	Sales
Transitory, β	0.007** (0.001)	0.015** (0.001)	0.006** (0.001)
Obs.	5,688,055	5,688,055	5,688,055
Permanent, α	0.080** (0.004)	0.047** (0.003)	0.088** (0.001)
Obs.	2,752,431	2,752,431	2,752,431
OLS	0.009** (0.001)	0.013** (0.002)	0.015** (0.002)
Obs	3500106	3500106	3500106

Note: This table reports the estimates of α and β (see equations 11 and 10) for our different measures of firm performance and worker income. The last row (OLS) reports the estimates from (1-difference, 1-difference) regression of residuals from the firm and worker equations for the sample which were used for the non-structural approach. These estimates can therefore be directly compared to corresponding OLS estimates in Tables 1 and 2 to assess the importance of the pre-stage filtering and specification in equations 2 and 3 for the structural approach. Standard errors are reported in parenthesis and clustered at the level of the firm. ** and * indicate statistical significance at the 1% and 5% levels, respectively. Note that the estimate on α based on sales (and earnings) increases when we include higher powers of the instrument. The estimate in Table 4 is therefore likely a lower bound (see also Table 21).

as identifying the role of permanent type shocks) actually contains lingering transitory shocks, and the estimates should therefore not be interpreted as resulting from permanent shocks only. Further, as we generally expect passthrough to be lower for transitory shocks, we should also expect smaller estimates of passthrough in the non-structural approach compared to the structural approach, where the persistent transitory shocks are taken care of through the econometric structure.³⁸ This is exactly what we find comparing the structural and non-structural estimates. We see that the non-structural approach generally estimates lower rates of passthrough than the structural approach. Comparing the IV version estimates, which eliminate the role of measurement error in Column (1) of Tables 1 and 2, i.e. for the (1-year, 1-year) specification with instruments based on 5-year differences, to the estimates on α , we see that passthrough rates are generally 1.5-3 times lower for the non-structural approach. For example, for earnings and value added, the structural approach finds $\alpha = 0.079$ while the non-structural estimates are 0.038 – 0.042.

Lastly, comparing estimates across firm performance measures in Table 4, we note that the ordering in terms of the size of elasticities remains when we focus on permanent type shocks. In particular, shocks to value added and sales have higher passthrough rates than value added per worker. As argued in Section 3.2.1, the fact that value added per worker leads to relatively lower rates of passthrough is suggesting that firms are responding to shocks by changing their labor input and potentially increasing hours (or employment generally) relatively more for more marginal workers (non-stayers). The fact that passthrough rates for transitory shocks are actually larger for value added per worker compared to e.g. value added could on the contrary reflect that the firm primarily focus hours adjustments among stayers (and thus avoid a costly hiring process) when the shock is shorter lasting.

In Appendix C.4.3, we analyze the robustness of our estimates (α and β) by comparing them to specifications with additional control variables in the firm performance equation, changes in the lag structures used for instruments, and higher power instruments. Our results are generally similar across these different specifications, and our qualitative conclusions are not challenged. For additional details see Appendix C.4.3.

4.2.2 Comparison to Literature

As argued earlier, we have chosen to use three measures of firm performance and two measures of worker income for two main reasons. First, it helps us to show that the main results are not driven by a particular measure. Second, it helps to bridge a gap in the literature where different measures have generally been used. This naturally prompts the question whether differences in reported estimates could stem from differences in firm performance or worker income measures. Guiso et al. (2005) estimate an α of 0.069 using earnings and value added from Italy. Fagereng et al. (2018) find an estimate of 0.071 using earnings and value added from Norway. Kerndler

³⁸This suggests that results based on even longer differences in the non-structural approach may increase, but at the same time this would also decrease the sample size substantially. Note that in Juhn et al. (2018), they show that the estimates based on 5-year differences should recover the impact of permanent type shocks, but only when $\rho = 0$ (see also footnote 23).

(2019) estimate α to be 0.05 using daily wages and sales per worker from Germany. Guertzgen (2014) also uses German data, but instead of sales per worker she uses value added per head. She reports insignificant, but negative estimates of α . Both estimates from Germany are significantly lower than the two estimates from Italy and Norway. However, our results suggest that this might partly be driven by the fact that these studies use per worker measures. At least we find that using value added per worker instead of total value added decreases our estimate of α by around 40 percent from 0.080 to 0.047. Rute Cardoso and Portela (2009) use a measure of monthly wages, which does not take hours worked into account, and sales from Portugal and find an estimate of 0.092. The estimate is higher than those mentioned above. However, we would also expect a fairly high estimate given that they use monthly wages and sales. Kátay (2016) uses data from Hungary and uses earnings and value added as measures. He finds an estimate of α of 0.107. Again, this is on the high end of the literature, but using value added is also what gives us one of the highest estimates.

Generally, we find that the ranking of the estimates in the literature is in line with what our results would have predicted. This suggests that some of the differences found across countries might also be due to differences in how income and firm performance are measured.

4.2.3 Heterogeneity

Next, we estimate α and β separately across different subgroups using the residuals based on the estimation of the firm performance and income equation on the full sample. For expositional purposes, we only perform the analysis for worker earnings and value added.³⁹ The results are shown in Table 5.

In Column (1), we report the baseline estimates from Table 4. In Columns (2) and (3), we report results for individuals in managerial positions and blue collar workers, respectively. The estimates reveal higher passthrough rates of permanent shocks for these two groups of workers compared to the baseline estimates. It is likely that the higher passthrough has very different explanations such as e.g. more surplus splitting for managers and larger hours responses for blue collar workers. In Columns (4) and (5), we split based on education. We find no difference for low educated workers. There is a tendency that high educated workers are less affected by permanent shocks, but the standard error on α for high educated workers is large.

In Columns (6) and (7), we focus on characteristics related to the particular firm. In Column (6), we report the estimates for the Business Service industry, where we expect the degree of surplus splitting and other incentive contracts to may be quite intensively used. In Column (7), we focus on smaller firms only. We find much higher degrees of passthrough for both small firms and in the Business Service industry compared to our baseline estimates especially for permanent shocks. We also see larger passthrough for transitory shocks in smaller firms suggesting that small firms have a harder time to effectively smooth out this type of shocks. The higher estimates of passthrough

³⁹Reassuringly, we find the same patterns across the remaining firm performance measures (sales and value added per head). These results are repeated in the Appendix C.4.4, Tables 24 and 23.

Table 5: Heterogeneity: Earnings, Value Added

	Baseline	Managers	Blue Collar	High Educ	Low Educ	Business	Small firms	Year \leq 2001	Year > 2001
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Transitory, β	0.016** (0.001)	0.015** (0.003)	0.019** (0.001)	0.018** (0.002)	0.012** (0.001)	0.019** (0.002)	0.029** (0.002)	0.007** (0.001)	0.022** (0.001)
N	5,688,055	352,926	1,849,250	649,503	752,425	494,942	227,876	1,866,152	3,821,903
Permanent, α	0.079** (0.006)	0.114** (0.035)	0.199** (0.015)	0.025 (0.069)	0.081** (0.014)	0.157** (0.016)	0.199** (0.021)	0.042** (0.005)	0.096** (0.001)
N	2,752,431	183,908	910,630	301,744	359,039	225,202	95,030	784,910	1,967,521

Note: This table reports the estimates of α and β (see equations 11 and 10) across different sub-samples. Column (1) repeats the estimates from Table (4). Column (2) and (3) focus on occupations involving leadership or blue collar work (manual labor) as identified by the Danish occupation classification (DISCO), see section 2. Column (4) and (5) focus on individuals with 10 or less (above 16) years of education. Column (6) focuses on workers in the Business Service Industry. Column (7) focuses on workers in firms with less than 15 employees (in full time equivalents) in a given year. Column (8) [(9)] present estimates based on years up until [after] 2001. Standard errors are reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

could also reflect a greater degree of rent-sharing for smaller firms, since there is a more clear connection between firm performance and the individual worker's effort. Finally, in Columns (8) and (9), we split the sample around 2001. We see that prior to 2001 the effect of permanent shocks is significantly smaller. Also, after 2001 passthrough rates increase for both transitory and permanent shocks. This could be driven by changes in wage bargaining. Wage bargaining has historically been centralized in Denmark, but it got more decentralized during the 1980s and '90s moving away from collective agreements to firm and individual level bargaining, see Andersen et al. (2012) and Dahl et al. (2013). It seems likely that increases in decentralization would imply a higher passthrough rate.

5 Passthrough to Employment

In this section we present our estimates of passthrough on the employment margin. We separately consider exits to unemployment and job-to-job transitions. We then consider potential heterogeneity in passthrough across different subgroups in the data. Finally, we compare the magnitude of our results on the employment margin to the income margin and briefly discuss implications of our results.

5.1 Result

An important part of our paper is to extend the analysis of passthrough to investigate how shocks to firm performance affect employment stability. This is something that most of the literature has largely ignored. As argued in Section 3.1 we focus our analysis of passthrough to the employment margin on two outcomes. First, the probability of exiting to unemployment. Second, we look at how the job-to-job transition probability is affected by firm performance. This later outcome could potentially be more affected by reverse causality, since workers make job-to-job transitions based on e.g. prospects in other firms. If a worker moves to another firm based on the new firm's characteristics, then it will cause reverse causality problems if e.g. value added in the old firm goes down as a result hereof.

For both outcomes, the estimates are based on Equation (4) changing either the set of control variables included in the regression or using different types of instruments as explained in Section 3.3. As argued in Section 3.3.1, we focus on the two firm performance measures of value added and sales. We focus on the results based on value added below. In Appendix D.1 we show the results based on sales which are perhaps slightly larger but generally very similar.

In Table 6, we present the estimates of passthrough to the probability of becoming unemployed. Recall, that we categorize a worker as becoming unemployed if the worker is observed in the firm at year t and not year $t + 1$, but is observed in unemployment between t and $t + 1$. The first two columns present the effect of a change in firm performance with and without control variables. Adding controls only changes the estimated coefficient slightly. A one percent increase in value added decreases the probability of making a job-to-unemployment transition with 0.038

percentage point. Note that for comparison we also report the sample average of the dependent variable in Column (7)

In Columns (3) and (4) in Table 6, we instrument the change in the firm performance measure with either 3- or 5-year differences inspired by the non-structural approach above. The estimates become larger (more negative) suggesting that longer lasting shocks affect the probability of becoming unemployed more than shorter term shocks.⁴⁰ This is also echoed in the results in Columns (5) and (6), where we use the residuals from the structural approach to instrument for the difference in firm performance. We see a large difference between the effect of transitory and permanent shocks. The transitory shocks have a significant and positive effect, but the size of the estimate is very small. Contrary to this, the effects of permanent shocks are larger and negative. Workers employed at a firm who experience a negative permanent shock of one percent have an increased probability of leaving to unemployment of 0.613 percentage point. Taken literally this suggests that in a firm with 1000 workers, 6 workers will on average leave for unemployment following a negative shock of one percent. This is compared to a baseline mean of 0.049, which means that there is about a 5 percent risk that a worker will become unemployed in any given year. Thus, the effect of a one percent negative shock to value added implies that the firm fires approximately 13 percent more workers than usual. As was the case for income passthrough, we tend to find that there is a higher degree of passthrough of permanent shocks estimated by the structural approach compared to the IV estimates. Recall that this is probably due to the fact that transitory firm shocks may still be fairly persistent, and parts of them are therefore still left even using a 5-year change as an instrument.⁴¹

In Table 7, we repeat the same analysis, but use an indicator for job-to-job transitions instead of an indicator for job-to-unemployment transitions. In general, the results mirror those of unemployment and are quite similar in magnitude. Instrumenting with longer lags as in Columns (3) and (4) increases (in absolute terms) the estimate and so does using the permanent shocks from the structural approach. Looking at e.g. Column (6), a one percent decrease in value added increases the probability of making a job-to-job transition by 0.538 percentage point. Column (7) reports the baseline probability of making a job-to-job transition in our sample, which is around 6.6 percent. Thus, a one percent negative shock to value added implies an increase in the probability of making a job-to-job transition of about 8 percent.

Finally, we investigate the role of firm size. As argued in Section 3.3.1, any per worker firm performance measures are particularly problematic in an analysis of the employment margin as the firm may respond to a shock by changing its workforce. At the same time, we may of course want

⁴⁰For completeness, Table 28 in Appendix D.2 shows the estimates of Columns 1-4 on a sample where we only focus on observations where a 5-year window of firm performance measures exist, thus securing the same number of observations across models. The results are very similar to those reported in Table 6. Keep in mind that we do include workers with job durations smaller than e.g. 5 years, so in this part of the analysis we are not focusing on stayers only. As a further robustness check we have also tried to include worker fixed effects in the estimating equations, and the results do not change. If anything they tend to be larger. Notice however, that using fixed effects in a linear probability model tends to produce predicted values outside the domain.

⁴¹Interestingly, the estimate of -0.613 is in the same range as what Friedrich (2020) finds using the Danish Cartoon Crisis as a very specific exogenous demand shock to exporting firms.

Table 6: Employment-to-Unemployment Transitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	IV3	IV5	Transitory (guiso)	Permanent (guiso)	Baseline mean
U exit	-0.040** (0.001)	-0.038** (0.001)	-0.081** (0.001)	-0.130** (0.001)	0.007** (0.001)	-0.613** (0.022)	0.049
N	10,050,182	10,050,159	8,710,002	7,201,872	8,524,766	6,230,141	10,050,182
Controls	No	Yes	Yes	Yes	Yes	Yes	

Note: This table presents estimates of γ in equation 4 based on value added as the measure of firm performance. Column (2) controls for year and age dummies, as well as broad occupation and industry dummies. Columns (3) and (4) instrument Δfp_{jt} with $\Delta_3 fp_{jt} = fp_{jt+1} - fp_{jt-2}$ and $\Delta_5 fp_{jt} = fp_{jt+2} - fp_{jt-3}$ respectively. Columns (5) and (6) instrument Δfp_{jt} with $\Delta \epsilon_{jt+1}$ and $\sum_{\tau=-2}^2 \Delta \epsilon_{jt+\tau}$ where residuals are the same as those used in Section 3.2. For each regression we use all the largest possible samples, in Table 28 in Appendix D.2 we show the estimates of Columns (1)-(4) on a sample where we only focus on observations where a 5-year window of firm performance measures exists. Standard errors are clustered at the worker level and reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

Table 7: Job-to-Job Transitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	IV3	IV5	Transitory (guiso)	Permanent (guiso)	Baseline mean
J-t-J exit	-0.044** (0.001)	-0.049** (0.001)	-0.101** (0.001)	-0.147** (0.001)	0.000 (0.001)	-0.538** (0.014)	0.066
N	10,152,525	10,152,502	8,812,385	7,250,307	8,577,190	6,250,299	10,152,525
Controls	No	Yes	Yes	Yes	Yes	Yes	

Note: These tables present estimates of γ in equation 4 for firm performance measures Value Added. Column (2) controls for year and age dummies, as well as broad occupation and industry dummies. Columns (3) and (4) instrument Δfp_{jt} with $\Delta_3 fp_{jt} = fp_{jt+1} - fp_{jt-2}$ and $\Delta_5 fp_{jt} = fp_{jt+2} - fp_{jt-3}$ respectively. Columns (5) and (6) instrument Δfp_{jt} with $\Delta \epsilon_{jt+1}$ and $\sum_{\tau=-2}^2 \Delta \epsilon_{jt+\tau}$ where residuals are the same as those used in Section 3.2. For each regression we use all the largest possible samples. Standard errors are clustered at the worker level and reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

to control for “exogenous” changes in firm size and firm growth cycles and differential turnover levels. In Table 8, we show how our estimates of passthrough to the probability of becoming unemployed change as we add controls for firm size. In Column (1), we first reproduce the baseline OLS estimate from Table 7. In Column (2), we add controls for (log)firm size in year $t - 2$ or $t - 1$, i.e. in the years prior to the period in which we measure transitions. The OLS estimate does not really change. In Column (3), we additionally control for the change in firm size from period $t - 1$ to t , i.e. the same period in which transitions to unemployment are measured. Doing so reduces the estimates significantly, but this is not really surprising, since we now control for some of the endogenous responses of the firm. The interpretation of estimate is that conditional on a given change in firm size a negative shock to value added makes it more likely that unemployment transitions drive the decline in firm size. Lastly, in Columns (4)-(6) and (7)-(9), we show how our IV estimates using 3- and 5-year differences, respectively, change as we control for firm size. We see the same pattern as with the OLS regressions. Overall, we therefore conclude that our results above are not driven by e.g. smaller firms with higher turnover, firm growth cycles, or similarly. In Table 29 in Appendix D.3, we repeat the same steps and show how our estimates of passthrough to job-to-job transitions change as we include controls for firm size. We find similar results as for the unemployment margin.

5.2 Heterogeneity

In this section, we report heterogeneous effects on the transition probabilities. We split on the same dimensions as we did in Section 4.2.3. In addition we consider different tenure groups since our analysis above suggests that low-tenured workers experience higher passthrough rates to worker income. Table 9 reports the estimates for both employment-to-unemployment and job-to-job transitions. We only report results for the OLS regression and the IV regression using 5-years differences as an instrument and only for value added. See Appendix D.1 for similar results for sales. Keep in mind that instrumenting with 5-year differences implies that we are really evaluating passthrough for more longer lasting shock.

Column (1) in each panel reports the baseline estimates from Tables 6 and 7. Looking first at the result for employment-to-unemployment transitions in the upper panel of Table 9, we find that blue collar workers, workers with low education, and workers with low tenure are more affected by a shock to value added, whereas managers, highly educated workers, and workers with high tenure are not affected as much. These conclusions are true for both the OLS and IV specifications, but as expected the effects are larger in the IV specifications. These results confirm our prior expectation that when hit by a negative performance shock, firms fire workers with low firm specific human capital (low educated, blue collar, low tenure).

We see different results when looking at the effect on job-to-job transitions in the lower panel of Table 9. In the OLS specification those that are more affected by a shock are low-educated and those in the Business Service Industry. In the IV specification, which to a higher degree capture permanent type shocks, the most affected sub-groups are managers and high educated

Table 8: Employment-to-Unemployment Transitions: Firm Size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	OLS	OLS	IV3	IV3	IV3	IV5	IV5	IV5
U exit	-0.038** (0.001)	-0.039** (0.001)	-0.006** (0.001)	-0.081** (0.001)	-0.065** (0.001)	-0.010** (0.001)	-0.130** (0.001)	-0.109** (0.001)	-0.036** (0.002)
N	10,050,159	9,371,629	9,371,629	8,710,002	8,634,968	8,634,968	7,201,872	7,128,958	7,128,958
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
size _{t-2}	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
size _{t-1}	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
$\Delta \text{size}_{t-1,t}$	No	No	Yes	No	No	Yes	No	No	Yes

Notes: In this table we show how our estimates of passthrough from value added on the probability of making an employment-to-unemployment transition change as we include different controls for the size of the firm. See Table 29 for similar results based on job-to-job transitions. size_{t-2} measures the log of the size of the firm (in full-time equivalents) in year $t - 2$. $\Delta \text{size}_{t-1,t}$ measures the change in (log) firm size from year $t - 1$ to t . Standard errors are clustered at the worker level and reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

workers. Overall, these results suggest that the consequences of fluctuations in firm performance vary across workers depending on their type and outside options. Higher skilled workers may have more easily available alternatives and can thus “escape” firm shocks by changing employers, lower skilled workers on the other hand are more likely to enter unemployment and thus are relatively more affected.

5.3 Combined Passthrough and Implications

A natural question that arises is which of the two margins of adjustment, income and employment, is the most important from the perspective of the firm. In this subsection, we therefore relate the two parts of our analysis above to each other.

The estimates for passthrough to employment stability and worker income are actually comparable from the firm’s perspective in terms of the saved labor cost. Recall that the employment stability regressions estimate the change in probability of ending the employment relationship following a percentage change in firm performance, while the income passthrough regressions estimate the percentage income change for stayers following a percentage change in firm performance. Take the example of a firm which experience a one percent permanent decline in e.g. value added. If the estimated passthrough rates to employment stability and worker income were the same, say 1 percent, then the result suggests that both margins matter equally for the decrease in labor costs. I.e. the firm reduced its wage bill for stayers with 1 percent, and it reduced the wage bill 1 percent due to workers leaving the firm. In this sense the estimated passthrough rates for both outcomes represent the same change in overall labor costs.

The estimated passthrough of permanent shocks from the structural approach suggests that earnings for stayers will decrease by around 0.08 percent following a one percent decrease in value added, see Table 4. This reduces the wage bill with around 0.08 for stayers. Likewise, it will result in an estimated increased probability of becoming unemployed of around 0.13 – 0.61 (IV5 and permanent shocks in Table 6) and an almost similar increase in probability of 0.15 – 0.54 of worker making a job-to-job transition (Table 7). Thus, from the firm’s perspective, the saved labor cost is mostly driven by workers exiting the firm either through job-to-job transitions or through employment-to-unemployment transitions.⁴² This result is similar to the findings in Roys (2016), which suggests that the employment margin is much more responsive when shocks are more permanent in type.

A similar assessment of the relative importance of the two margins of adjustment from the point of the view of the worker is beyond the scope of this paper. It would generally require adding more structure, since the current framework does not model the outside options of unemployment and other jobs. Nevertheless, we believe our results underline the importance of separating employment exists by whether they are voluntary (job-to-job transitions) or potentially

⁴²This of course assumes that the firm does not increase its hiring in response to the shock. For example it may be reasonable to expect the firm to hire replacement workers for those lost via job-to-job transitions. See Bagger et al. (2020b) for an analysis of adjustments on the hiring margin. Further, note that we do not take the subgroup differences (nor non-linearities) documented above (below) into account in this back-on-the-envelope calculation.

Table 9: Transitions: Heterogeneity

Employment-to-Unemployment	Baseline	Managers	Blue Collar	High Educ	Low Educ	Business	Small firms	Year \leq 2001	Year > 2001	Low Tenure	High Tenure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
OLS with controls	-0.038** (0.001)	-0.021** (0.001)	-0.047** (0.001)	-0.021** (0.001)	-0.052** (0.001)	-0.035** (0.001)	-0.040** (0.001)	-0.039** (0.001)	-0.037** (0.001)	-0.044** (0.001)	-0.029** (0.001)
	10,050,159	531,407	3,254,329	1,149,004	1,344,762	917,474	459,849	6,254,423	3,795,736	3,689,844	3,999,220
	-0.130** (0.001)	-0.072** (0.005)	-0.164** (0.002)	-0.082** (0.004)	-0.162** (0.004)	-0.080** (0.004)	-0.105** (0.005)	-0.122** (0.002)	-0.122** (0.002)	-0.153** (0.002)	-0.122** (0.002)
	7,201,872	389,809	2,373,215	859,618	918,621	653,489	264,438	4,950,303	2,251,569	2,252,903	3,116,828
Job-to-Job	Baseline	Managers	Blue Collar	High educ	Low educ	Business	Small firms	Year \leq 2001	Year > 2001	Low Tenure	High Tenure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
OLS with controls	-0.049** (0.001)	-0.044** (0.001)	-0.047** (0.001)	-0.047** (0.001)	-0.053** (0.001)	-0.056** (0.001)	-0.045** (0.001)	-0.043** (0.001)	-0.056** (0.001)	-0.045** (0.001)	-0.049** (0.001)
	10,152,502	2,304,824	3,233,304	1,199,536	1,317,630	947,571	461,844	6,342,931	3,809,571	3,695,978	4,052,372
	-0.147** (0.001)	-0.170** (0.003)	-0.138** (0.002)	-0.192** (0.005)	-0.110** (0.003)	-0.145** (0.004)	-0.096** (0.005)	-0.155** (0.002)	-0.139** (0.002)	-0.146** (0.003)	-0.156** (0.002)
	7,250,307	1,666,866	2,345,463	894,895	895,498	672,828	264,334	4,992,189	2,258,118	2,246,122	3,147,764

Note: This table presents estimates of γ in equation 4 for firm performance measure value added across different subsamples (see Table 27 for the same table based on sales). The row with "IV5" instruments $\Delta f p_{it}$ with $\Delta_5 f p_{it} = f p_{it+2} - f p_{it-3}$. Column (1) repeats the estimates from Table (6). Columns (2) and (3) focus on occupations involving leadership or blue collar work (manual labor) as identified by the Danish occupation classification (DISCO), see section 2. Columns (4) and (5) focus on individuals with 10 or less (above 16) years of education. Column (6) focuses on workers in the Business Service Industry. Column (7) focuses on workers in firms with less than 15 employees (in full time equivalents) in a given year. Column (8) [9] present estimates based on years up until [after] 2001. Column (10) [11] focus on workers with no more than 4 [more than 8] years of tenure in the firm. Standard errors are reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

involuntary (employment-to-unemployment), see also the discussion in Section 3.1. Furthermore, our results imply that the role of firm shocks likely varies across different types of workers due to differences in e.g. outside options. We leave this part of the analysis to future work.

6 Non-linearities

In this final section of our analysis we look for potential non-linearities in the relationship between changes in firm performance and changes in income or employment status.

6.1 Non-linearities in Earnings

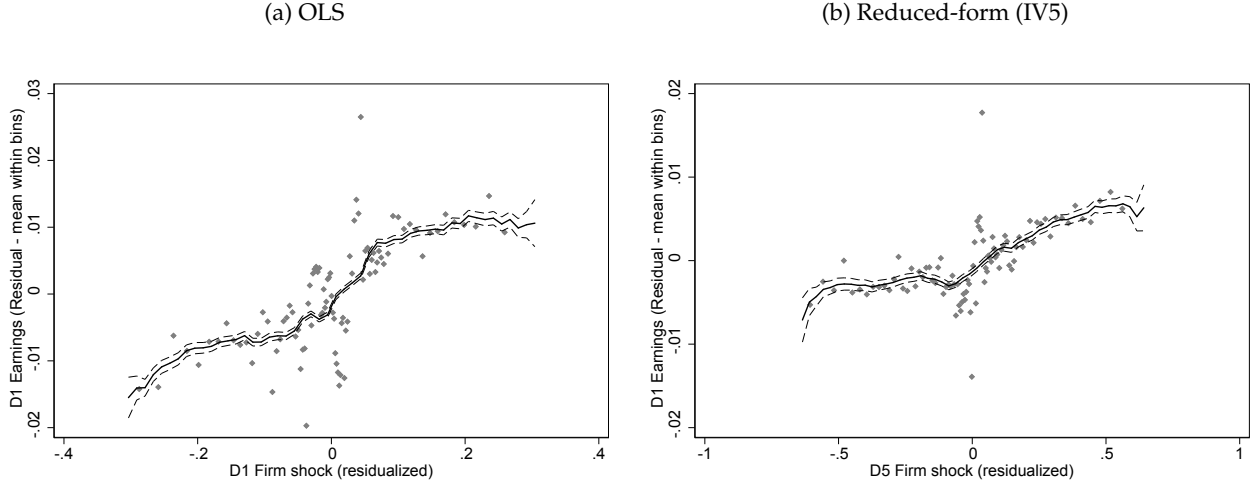
To investigate possible non-linearities, we plot $\Delta_z w_{ijt}$ against $\Delta_z f p_{ijt}$ (see Equation (1)) after controlling for observables. In order not to have too many results, we focus on earnings and value added like we did in the heterogeneity section. In Figure 1a, we plot the one period differences, i.e. $\Delta_1 w_{ijt}$ against $\Delta_1 f p_{ijt}$, where both of them are residualized. Thus, changes in value added can be both transitory and permanent. Each dot in the figure is a percentile bin. The marginal changes in earnings are larger in the middle of the domain. It seems that the steeper part of the relationship is concentrated at small positive firm shocks mostly. The relationship is slightly flatter in the tails, which is not surprising given that there is probably also some measurement error in firm performance. In Figure 1b, we plot the reduced-form of the IV approach, i.e. $\Delta_1 w_{ijt}$ against $\Delta_5 f p_{ijt}$. Since we plot a 5-period difference, this picks up more persistent shocks and filters out contemporary measurement error. The figure has more of a hockey-stick shape with the marginal effect on earnings being zero for negative value added changes, while positive value added changes have a positive correlation with earnings changes. This suggests that positive permanent changes to firm performance are transmitted to earnings, while negative permanent shocks are not to the same extent. This is consistent with downward wage rigidities, where firms cannot adjust wages to negative shocks and as a response might have to lay off workers instead.⁴³

6.2 Non-linearities in Employment

It seems natural to think that the effects on e.g. unemployment are driven by negative shocks to firm performance, whereas positive ones might not matter as much. This relates to a larger literature on labor adjustment costs, see e.g. Caballero et al. (1997) and more recently Cooper and Willis (2009). In Figure 2a, we therefore plot the relationship between e_{ijt} and $\Delta f p_{jt}$, and in Figure 2b we plot the reduced form relationship again using $\Delta_5 f p_{jt}$ as an instrument. From the figures it is easy to see a non-linear relationship. Negative long-run differences in firm performance are associated with a higher probability of separating to unemployment. This is also found in Carlsson et al. (2020). Using a very different methodology, they find that adjustment to permanent negative

⁴³Note that the structural specification, which was used above, does not allow for non-linearities. This is a general challenge with the specification in Guiso et al. (2005).

Figure 1: Plot of Value Added and Earnings



Note: This figure is constructed as follows: first we regress Δfp_{jt} (and subsequently Δw_{ijt}) on the explanatory variables used in equation (1) and obtain the residuals. We then divide Δfp_{jt} into 100 equally sized bins and calculate the mean of Δfp_{jt} and Δw_{ijt} within each bin. These data-points are the gray diamonds in Figure 1a. The solid line is the result of local linear regressions of the two residuals using a rectangular kernel. The plot discards observations above the 5th and 95th percentile. Figure 2b presents results from the same procedure using 5-year differences (Δfp_{jt}) around time t instead of Δfp_{jt} .

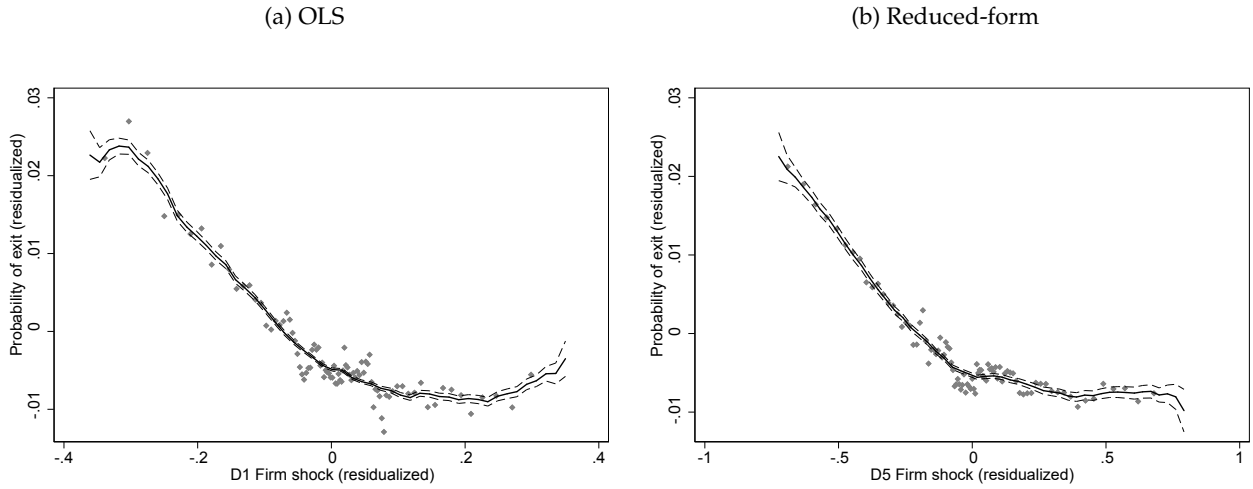
shocks happens through increased separations and not decreases in hiring. These results highlight the role of employment separations (and exits into unemployment in particular) as a potential magnifier of the role of passthrough of negative firm shocks.

Figure 3 in Appendix D.4 repeats the same analysis for job-to-job transitions instead of unemployment transitions as outcome. Again, we find non-linear effects with negative shocks driving the estimates found in Table 7. To quantify this further, Table 10 presents estimates of an extended model of equation 4 where we have two different γ 's, one for positive and one for negative shocks. The table reports OLS estimates in Column (1) and our IV estimates in Columns (2) and (3), where positive and negative 3-year and 5-year differences are used as instruments for positive and negative firm shocks, respectively. The estimates clearly illustrate the asymmetric effect of shocks to firm performance. In the OLS specifications, we find that the effect of a positive shock is very close to zero, whereas the effect of a negative shock is -0.076 for transitions to unemployment, i.e. roughly twice as large as the estimates from the linear model in Table 6. In the IV specification, we find negative estimates for positive shocks, but again these are significantly smaller than the effects of negative shocks.

7 Conclusion

In this paper we estimate the passthrough of firm performance to income and employment stability. For the analysis on income we use two different empirical approaches. A non-structural ap-

Figure 2: Plot of Value added and Unemployment:



Note: This figure is constructed as follows: first we regress $\Delta f p_{jt}$ (and subsequently e_{ijt}) on the explanatory variables used in equation (4) and obtain the residuals. We then divide $\Delta f p_{jt}$ into 100 equally sized bins and calculate the mean of $\Delta f p_{jt}$ and e_{ijt} within each bin. These data-points are the gray diamonds in Figure (2a). The solid line is the result of local linear regressions of the two residuals using a rectangular kernel. The plot discards observations above the 5th and 95th percentile. Figure (2b) presents results from the same procedure using 5-year differences ($\Delta_5 f p_{jt}$) around time t instead of $\Delta f p_{jt}$.

Table 10: Non-linear Estimation: Employment-to-Unemployment

	Employment-to-Unemployment				Job-to-Job		
	OLS	IV3	IV5		OLS	IV3	IV5
	(1)	(2)	(3)		(2)	(3)	(4)
$\gamma_{\Delta f p_{jt} \geq 0}$	0.002** (0.001)	-0.038** (0.001)	-0.027** (0.002)	$\gamma_{\Delta f p_{jt} \geq 0}$	0.005** (0.001)	-0.046** (0.001)	-0.050** (0.002)
$\gamma_{\Delta f p_{jt} < 0}$	-0.076** (0.001)	-0.105** (0.001)	-0.215** (0.002)	$\gamma_{\Delta f p_{jt} < 0}$	-0.098** (0.001)	-0.129** (0.001)	-0.228** (0.002)
With controls	yes	yes	yes	With controls	yes	yes	yes
N	10,050,159	8,710,002	7,201,872	N	10,152,502	8,812,385	7,250,307

Note: Table 10 presents estimates of an extended model of Equation (4) where we have two different γ 's, one for positive and one for negative shocks. Results are reported for transitions into unemployment and job-to-job transitions separately. The measure of firm performance is value added. The control variables are the same as those used in Table (6). Columns (2) and (3) use positive and negative 3-year and 5-year differences as instruments for positive and negative firm shocks, respectively. Standard errors are reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

proach, which builds on Juhn et al. (2018) and Card et al. (2016), and a structural approach, which builds on Guiso et al. (2005). For the analysis on the employment margin we use some of the same insights and instruments which are used in the non-structural and structural approach. Our analysis uses a 25-year long panel of Danish register data, which allows us to use several measures of firm performance (value added, sales, and value added per worker), worker income (earnings and hourly wages) and to differentiate between different types of exit from employment as we separately consider exits to unemployment and job-to-job transitions. We study average rates of passthrough, but we also look into differences across sub-groups and potential non-linearities in the effect of shocks.

The non-structural approach correlates changes in firm performance with changes in income varying in the time window over which the differences are taken. The approach puts no formal structure on how permanent and transitory factors are quantified. Longer differences of firm performance proxy more permanent shocks. Comparing estimates with increasing difference length, we assess the importance of permanent shocks. On the contrary, the structural approach explicitly models firm performance and splits the residual performance into permanent or transitory processes. As a result, we get direct estimates on the separate passthrough of permanent and transitory firm shocks.

For the analysis on worker income, we find slightly lower degrees of passthrough using the non-structural approach than for the structural approach where we find that income passthrough is higher for permanent (5-9 percent) than transitory (1 percent) shocks. The estimated process for firm performance in the structural approach suggests that differences between the structural and non-structural approaches are largely due to a high degree of persistence of shocks, even transitory ones. Thus, the longer differences (5-year) in the non-structural approach, which was supposed to proxy more permanent shocks, actually still contain a fair degree of transitory shocks, which gives a lower passthrough estimate. Using both approaches, we find lower passthrough estimates for hourly wages than earnings. This indicates that firms adjust by changing hours for staying workers. This is a clear indication that shocks affect not only wages, but also hours worked. We find higher degrees of passthrough for managers, blue collar workers, and workers in small firms. We also show that passthrough from firm performance to income is driven by positive firm performance shocks, whereas negative shocks do not affect income to the same extent. The result that hours, even for staying workers, seem to be affected suggests that passthrough to employment stability might be important.

For the analysis on the employment margin, we explicitly look at the probabilities of becoming unemployed and making a job-to-job transition following changes in firm performance. We find that worse firm performance increases both unemployment and job-to-job transitions. The passthrough rate is larger for more permanent shocks. A one percent negative permanent shock to value added increases the probabilities of moving to unemployment and other jobs by 0.613 and 0.538 percent, respectively. We show that the passthrough on both margins is caused by negative firm performance shocks, while positive ones do not affect employment stability to the same ex-

tent. The effect of a negative firm performance shock on the probability of becoming unemployed is higher for blue collar workers, workers with low education, and workers with low tenure. The is contrary to the results on job-to-job transitions, where those most affected by a negative firm performance shock are high-educated workers and managers.

A back of the envelope calculation suggests that labor cost adjustments from changes on the employment margin are quantitatively more important than adjustments in wage payments in response to permanent type shocks. Overall, our results illustrate the importance of employment adjustments as a potential risk propagation mechanism when we want to assess the importance of passthrough of risk from firms to their workers.

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A Appendix: Data and Sample Selection

A.1 Descriptive Statistics on Final Sample

In Table 11, we show descriptives based on our firm sample as defined in Section 2. In Table 12 we show descriptives based on our worker sample. These descriptives were discussed in Section 2. Note that we winsorize key variables value added, value added per worker, sales, hourly wages, earnings at the 1th and 99th percentile. Since our empirical strategy requires long panels we prefer not to trim the data.

Table 11: Descriptive Statistics: Firm Sample

Statistics	Mean	Std	Industries (shares)	
Firm age	17.64	14.98	Manufacturing	0.28
Spell length	16.32	6.49	Construction	0.17
log(VA)	9.39	1.24	Trade & Transportation	0.39
log(VA per worker)	6.09	0.48	Information & Communication	0.04
log(Sales)	17.29	1.34	Business Service	0.11
Firm size (workers)	66.66	310.12		
Change in firm size	1.04	43.47	Number of firms	18,488
Percent change in firm size	0.08	1.57	Min number of years	5
Number of firm-years	244,362		Max number of years	25

Notes: This table provides descriptives (averages/shares and standard deviations) for our firm sample (see Section A.3). VA is value added, Change in firm size is the average per period change in firm size.

A.2 Firm Performance

Starting from Equation (2) we take first differences to eliminate the firm fixed effect:

$$(1 - \rho L)\Delta f p_{jt} = \Delta Z'_{jt}\gamma + \Delta \epsilon_{jt}. \quad (5)$$

where L is the lag operator. Note that Equation (5) cannot be estimated by OLS due to endogeneity problems due to having $\Delta f p_{jt-1}$ as an explanatory variable, which is correlated with $\Delta \epsilon_{jt}$ (ϵ_{it-1} enters both variables). Instead we use the estimation procedure developed by Arrelano and Bond (1991). The idea is to solve the endogeneity problem by instrumenting $\Delta f p_{jt-1}$ with properly lagged dependent variables. That is, to obtain consistent estimates we can use $f p_{jt-x}$ as an instrument for $\Delta f p_{jt-1}$, where the choice of x depends on the length of the serial correlation of ϵ_{jt} . To select the appropriate instruments i.e. $f p_{jt-x}$, we analyze the error term and in particular the autocovariance patterns as specified by the AB test developed in Arrelano and Bond (1991).

Table 12: Descriptive Statistics: Worker Sample

Industries, Education and Job Type		Statistics	Mean	Std
Manufacturing	0.37	Worker age	40.27	10.24
Trade & Transportation	0.36	Tenure in job	7.29	5.77
Information & Communication	0.66	Experience	17.55	9.91
Business Service	0.10	Job spell length (years)	8.22	5.98
<11 years education	0.13	log(hourly wage)	5.23	0.36
11-13 years of education	0.19	log(earnings)	12.48	0.63
14-16 years of education	0.54			
>16 years of education	0.11	Number of worker-years	13,068,556	
Managers	0.05	Number of workers	1,929,357	
High level knowledge	0.28	Number of jobs	3,942,858	
Clerical	0.19	Min length of job	1	
Production	0.42	Max length of job	25	

Notes: This table provides descriptives (averages/shares and standard deviations) for our worker sample (see Section A.4).

Guided by the analysis of autocovariance patterns and the presence of both transitory and permanent parts (see Section 3.2.2), we end up following the standard assumption in the literature and work with error terms which allow for both permanent and transitory elements in the error term. In particular, we adopt the following structure:

$$\epsilon_{jt} = \zeta_{jt} + (1 - \theta L)\tilde{v}_{jt}, \quad (6)$$

where

$$\zeta_{jt} = \zeta_{jt-1} + \tilde{u}_{jt}, \quad (7)$$

such that the error term consists of two components: A permanent part that is represented by the random walk, ζ_{jt} , and a moving average of order 1, $(1 - \theta L)\tilde{v}_{jt}$. We assume covariance stationarity of the disturbance in Equation (6), such that $E(\tilde{u}_{jt}^2) = \sigma_u^2$, $E(\tilde{v}_{jt}^2) = \sigma_v^2$ for all t , no serial correlation $E(\tilde{u}_{jt}\tilde{u}_{jt-s}) = E(\tilde{v}_{jt}\tilde{v}_{jt-s}) = 0$ for $t \neq s$, and no cross correlation $E(\tilde{u}_{jt}\tilde{v}_{js}) = 0$ for all s, t .

By taking the first difference of Equation (6) and inserting Equation (7), we obtain

$$\Delta\epsilon_{jt} = \Delta\tilde{u}_{jt} + (1 - \theta L)\Delta\tilde{v}_{jt}, \quad (8)$$

which has an MA(2) structure. Note further that this formulation of a MA process does not necessitate a random walk component in the error term, therefore it is of interest to test whether the error term displays dynamics consistent with permanent and transitory type shocks. Following

Guiso et al. (2005), we therefore specify a test for a random walk component (the permanent part of the error term) of the error term, see footnote 19.

Notice that we can rewrite (2) such that value added is a function of a deterministic term, D_{jt} , a transitory term, T_{jt} , and a permanent term, P_{jt} .

$$y_{jt} = D_{jt} + T_{jt} + P_{jt},$$

where

$$D_{jt} = (1 - \rho L)^{-1} (Z'_{jt} \gamma + f_j)$$

$$T_{jt} + P_{jt} = (1 - \rho L)^{-1} \epsilon_{jt}.$$

Inserting the expression of ϵ_{jt} from (6) we get⁴⁴

$$T_{jt} = (1 - \rho L)^{-1} \left[(1 - \theta L) \tilde{v}_{jt} - (1 - \rho)^{-1} \rho \tilde{u}_{jt} \right]$$

$$P_{jt} = (1 - \rho)^{-1} \zeta_{jt}.$$

The point of the above is that our structural decomposition of the error term from the firm performance equation into Equation (6) implies that firm performance has both a transitory term, T_{jt} , and a permanent term, P_{jt} . Note that the variables T_{jt} and P_{jt} are unobserved and are not directly recovered from the estimation of the firm performance equation. Estimation of α and β proceeds by first constructing a compound residual from the worker income process, and subsequently a set of moment conditions is used to filter out the importance of D_{jt} and P_{jt} between the two residuals and thus estimate α and β .

A.3 Firm Sample

In Table 13, we show the effects of our different sample selection steps used in creating the firm sample as described in Section 2.2. Our starting point is the FIRM database which contains firm level accounting data and value added tax (VAT) records.⁴⁵ We select firms with non-missing, non-zero and non-imputed value added, sales, and wage bill information from this dataset.⁴⁶ The link between firms in the FIRM data base and their workers is obtained from the FIDA database, we therefore focus on the firms we can link to workers in FIDA. Rows 3 to 5 further focus on firms which are present for at least 5-years and are from one of the selected industries Manufacturing,

⁴⁴See Guiso et al. (2005) for additional details.

⁴⁵The FIRM database we use is a collection of the registers: FIRM (Generel Firmastatistik), FIGE, FIGT, and FIRE from Statistics Denmark. As explained in the main text our measure of value added is calculated by Statistics Denmark, see <http://www.dst.dk/extranet/staticsites/TIMES3/html/63c1f70e-7933-40fd-89de-90f8ab191b0c.htm>

⁴⁶We identify observations with imputed information using the variable JKOD and the value "R" indicating that the information is imputed by Statistics Denmark. Note that we keep observations where some of the information was collected from the Tax Registers, and remaining information has been imputed by Statistics Denmark from firms in the same industry, ownership structure, and reported information (value "S").

Table 13: Sample selection steps: Firm side

	Firm Sample	Firm-years
1	FIRM obs with non-missing information, 1992-2016	1,697,018
2	Observe Firm ID (CVRNR) in FIDA	1,657,267
3	Select firms entering panel up until 2012	1,566,678
4	Focus on selected industries	1,506,642
5	Require 5-years of consecutive observations	918,431 [15,030,341]
6	Require average of 5 workers	427,056 [14,079,358]
7	Require average of 10 workers	244,369 [13,068,556]

Note: This table reports the change in the number of firm-years response to our different steps in constructing our firm sample. In square brackets we report the corresponding “worker-year observations”.

Construction, Trade & Transportation, Information & Communication, and Business Services.⁴⁷

Finally, to study passthrough from firms to worker income or employment, we want to focus on “larger” firms. We therefore focus on firms with an average of at least 10 workers in the sample. Row 7 shows that this reduces our sample of firms from 1,506,642 firms to 244,369 firms. Since we are only excluding smaller firms, the consequences for the worker sample are much smaller and lead to a drop from 15,030,341 to 13,068,556 worker-years. In Section C.2, we show that including slightly smaller firms does not challenge our results in the main text, in fact on the earnings margin rates are slightly higher suggesting more adjustments on hours, but we leave it to future work to explicitly investigate how the rate of passthrough generally varies with the distribution of firm size.

A.4 Worker Sample

The unit of observation in our worker sample is a worker-year we include all workers with positive earnings and hourly wages that work for firms present in the firm sample.⁴⁸ The links between firms and workers are obtained from the FIDA database, which is available from 1995 and onward. As we require firms being present for more at least 5-years, we simply use the links to extrapolate back to 1992. We focus on workers below 60 to reduce concerns about early retirement and we require that workers are above 18 years of age and they are not included in the sample if they are enrolled into the education system. The employment status is determined as the status of the worker in November each year. If the workers hold multiple jobs, then we select the primary

⁴⁷The industry classification is based on NACE Rev. 2 and is grouped in 10 respective industries from which we sample the 5 industries: Manufacturing, Construction, Trade & Transportation, Information & Communication, and Business Services. Information on the aggregation of the NACE Rev. 2 nomenclature for industries can be found here: <https://www.dst.dk/klaskifikationsbilag/8cf95f88-8153-43b5-a82a-fa89adf6f214> (pp. 463-477)

⁴⁸We further drop observations with low quality information on hourly wages as accessed by Statistics Denmark, see <https://www.dst.dk/da/Statistik/dokumentation/Times/ida-databasen/ida-ansattelser/tlonkval>

job. This is either based on hours or earnings depending on the year. The reason to use the last week of November as the cross-sectional time is that hourly wages calculated by Statistics Denmark is only calculated for this job. Lastly, we delete workers if they enter the labor market after 2010, since we only have educational information up until 2012. This is to be able to control for gap years. I.e. a person finishes high school in 2010, has a gap year in 2011, and starts college in 2012. Our final worker sample has 13,068,556 worker-years, see Table 12 for some descriptives.

To each job spell we add information on how the job ends, i.e. if it ends in a job-to-job transition, unemployment, or something else. Here exploit two different data sources: SPELL data which contains the universe of employment spells and labor market transitions (see Bertheau et al. (2020) for an introduction to this data source) and variables from Statistics Denmark, which mark whether a given worker has experienced weeks in unemployment during the last year.

B Appendix: Details of Econometric Model

In this appendix we present the structural approach (see Section 3.2.2) used to study passthrough to worker income with more details. The structural approach consists of three steps: first, we estimate a dynamic panel data model describing the evolution of firm performance and determine the residuals, second, we perform a similar exercise for worker income, and finally, we analyze the covariance between these different residuals.

B.1 Income Regressions

As a first step rewrite Equation (3) as:

$$(1 - \rho L)\Delta w_{ijt} = (1 - \rho L)\Delta X'_{ijt}\delta + (1 - \rho L)\alpha\Delta P_{jt} + (1 - \rho L)\beta\Delta T_{jt} + (1 - \rho L)\Delta\psi_{ijt}.$$

Note here that we use the ρ from the firm's performance Equation (2), such that by inserting the expression of P_{jt} and T_{jt} derived in the last subsection, we obtain

$$\begin{aligned} (1 - \rho L)\Delta w_{ijt} &= (1 - \rho L)\Delta X'_{ijt}\delta + (1 - \rho L)\alpha\Delta(1 - \rho)^{-1}\zeta_{jt} + \beta\Delta v_{jt} + (1 - \rho L)\Delta\psi_{ijt} \\ &= (1 - \rho L)\Delta X_{ijt}\delta + \Delta\omega_{ijt}, \end{aligned} \quad (9)$$

where $\Delta\omega_{ijt} = \alpha(1 - \rho L)u_{jt} + \beta\Delta v_{jt} + (1 - \rho L)\Delta\psi_{ijt}$. Following the exact same argument as for Equation (5), we again have an inherent endogeneity problem of estimating equation (9).⁴⁹ We can again use the Arellano-Bond estimator to estimate the dynamic panel data model, see Guiso et al. (2005).

B.2 Income Passthrough Estimates

We can predict the residuals from the estimation of firm performance, $\Delta\epsilon_{jt}$, and from the estimation of worker income, $\Delta\omega_{ijt}$. One can then write up two moment conditions to identify the key parameters α and β (see Guiso et al. (2005) for further details).

The first moment condition is

$$E(\Delta\epsilon_{jt+1}(\Delta\omega_{ijt} - \beta\Delta\epsilon_{jt})) = 0. \quad (10)$$

Intuitively, once we subtract $\beta\Delta\epsilon_{jt}$ from $\Delta\omega_{ijt}$, what is left is no longer affected by the transitory component. $(\Delta\omega_{ijt} - \beta\Delta\epsilon_{jt})$ is orthogonal to the firm shock in the next period. This means that the moment condition exploits the orthogonality between the future firm shock, which has a transitory component and the residual from the workers earnings cleaned from the transitory shock component. In practice we estimate β by regressing $\Delta\omega_{ijt}$ on $\Delta\epsilon_{jt}$ but instrument $\Delta\epsilon_{jt}$ with

⁴⁹Note that the derivations to identify α and β are based on ρ estimated in (5) and (9) are the same. This is not necessarily true in our data or across all our measures.

$\Delta\epsilon_{jt+1}$. The consistency condition of this IV estimation is exactly similar to Equation (10), thus, by running this estimation we will be able to consistently estimate β .⁵⁰

The second moment for identification of α is very similar to the one for β , namely

$$E\left(\sum_{\tau=-2}^2 \Delta\epsilon_{jt+\tau}(\Delta\omega_{ijt} - \alpha\Delta\epsilon_{jt})\right) = 0. \quad (11)$$

The intuition of orthogonality of the moment condition in Equation (11) is based on $\sum_{\tau=-2}^2 \Delta\epsilon_{jt+\tau}$, which averages the moving average part of $\Delta\epsilon_{jt}$ out such that what is left is the permanent component. In expectation this permanent component is orthogonal to $(\Delta\omega_{ijt} - \alpha\Delta\epsilon_{jt})$, because it has been cleansed of the permanent component. Estimation of α from Equation (11) therefore consists of running the regression of $\Delta\omega_{ijt}$ on $\Delta\epsilon_{jt}$ and instrumenting $\Delta\epsilon_{jt}$ with $\sum_{\tau=-2}^2 \Delta\epsilon_{jt+\tau}$.

⁵⁰In principle higher powers of $\Delta\epsilon_{jt+1}$ can also be included as instruments. In Section (C.4.3) we show that higher powers generally will not change our estimates substantially.

C Appendix: Supplementary Results for Analysis on Income

C.1 Results for the Non-structural Approach in the Unrestricted Sample

In Section 4.1 (Tables 1 and 2) we present the results from our non-structural approach for a “restricted” sample where we select observations with a 5-year window around them in order to produce 5-year differences and thus base all estimates on the same sample. In Tables 14 and 15 we present results using the maximum number of observations possible in each regression. This means that we include workers with shorter durations in the firm and thus include workers who are potentially less attached to the firm. Comparing estimates across cells should however be done with more caution as the samples differ. We indicate the sample size of each regression in []. The results were commented on in Section 4.1.

Note that by construction, the estimates based on 5-year differences (i.e. cells in either row 3 or column 3 in each panel) should be very similar to the results in the main text (i.e. Table 1 and 2), since the main difference to the restricted sample is the few cases where 5-year differences can be constructed but e.g. 3 years cannot (due to e.g. missing firm data in a given year) and hence these observations are dropped in the restricted sample.

C.2 Selection on Firm Size

As explained in Section 2.2 we only consider firms with an average of firm size of at least 10 workers over the sample period. In Tables 16 (17) we reproduce our results from the non-structural approach (Table 1 and 2) when we extend the sample by including firms (and workers) with at least 5 workers over the sample period. The results are generally very similar to the results in the main text, but slightly larger suggesting that passthrough may vary with the size of the firm. We leave further exploration on this margin for future work.

C.3 Robustness of the Non-structural Earnings Passthrough Estimates

In Table 18 we show how our OLS estimates on passthrough (as reported in Table 1) change as we change the set of control variables we include in the analysis. In row (1) we report the estimates when we do not include any control variables. In row (2) we add year dummies to the specification. In row (3) we add linear, squared, and cubic terms of labor market experience, occupation group dummies, and dummies for industry (these estimates are the same as those reported in the main text in Table 1). Finally in row (4), we additionally control for tenure in the firm, age dummies, gender, and education levels. Looking across the different rows in the table it is clear that the estimates generally change little, although there is some tendency that the (5-year, 5-year) differences fall slightly, but nothing really challenges our main findings.

Table 14: Non-Structural Results: Earnings, Large Sample

Value Added (OLS)				Value Added (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.062** (0.001) [9,553,665]	x	x	$\Delta_1 f p_{ijt}$	x	0.059** (0.001) [5,688,056]	0.063** (0.001) [3,552,213]
$\Delta_3 f p_{ijt}$	x	0.061** (0.001) [5,688,056]	x	$\Delta_3 f p_{ijt}$	0.043** (0.001) [5,688,056]	x	0.063** (0.001) [3,500,106]
$\Delta_5 f p_{ijt}$	x	x	0.062** (0.001) [3,552,213]	$\Delta_5 f p_{ijt}$	0.042** (0.001) [3,552,213]	0.048** (0.001) [3,500,106]	x

Value Added per worker (OLS)				Value Added per worker (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.007** (0.001) [9,553,665]	x	x	$\Delta_1 f p_{ijt}$	x	0.025** (0.001) [5,688,056]	0.037** (0.003) [3,552,213]
$\Delta_3 f p_{ijt}$	x	0.016** (0.001) [5,688,056]	x	$\Delta_3 f p_{ijt}$	0.016** (0.001) [5,688,056]	x	0.027** (0.001) [3,500,106]
$\Delta_5 f p_{ijt}$	x	x	0.023** (0.001) [3,552,213]	$\Delta_5 f p_{ijt}$	0.028** (0.001) [3,552,213]	0.025** (0.001) [3,500,106]	x

Sales (OLS)				Sales (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.099** (0.001) [9,553,665]	x	x	$\Delta_1 f p_{ijt}$	x	0.072** (0.001) [5,688,056]	0.063** (0.001) [3,552,213]
$\Delta_3 f p_{ijt}$	x	0.081** (0.001) [5,688,056]	x	$\Delta_3 f p_{ijt}$	0.055** (0.001) [5,688,056]	x	0.067** (0.001) [3,500,106]
$\Delta_5 f p_{ijt}$	x	x	0.075** (0.001) [3,552,213]	$\Delta_5 f p_{ijt}$	0.041** (0.001) [3,552,213]	0.052** (0.001) [3,500,106]	x

Note: In this table we repeat the results from Table 1, now using the maximum number of observations possible in each regression. The number of observations are reported in []. See the notes to Table 1 for further details.

Table 15: Non-Structural Results: Hourly Wages, Large Sample

Value Added (OLS)				Value Added (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.010** (0.001) [9,553,665]	x	x	$\Delta_1 f p_{ijt}$	x	0.035** (0.001) [5,688,056]	0.032** (0.001) [3,552,213]
$\Delta_3 f p_{ijt}$	x	0.023** (0.001) [5,688,056]	x	$\Delta_3 f p_{ijt}$	0.037** (0.001) [5,688,056]	x	0.038** (0.001) [3,500,106]
$\Delta_5 f p_{ijt}$	x	x	0.028** (0.001) [3,552,213]	$\Delta_5 f p_{ijt}$	0.039** (0.001) [3,552,213]	0.039** (0.001) [3,500,106]	x
Value Added per worker (OLS)				Value Added per worker (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.013** (0.001) [9,553,665]	x	x	$\Delta_1 f p_{ijt}$	x	0.034** (0.001) [5,688,056]	0.028** (0.003) [3,552,213]
$\Delta_3 f p_{ijt}$	x	0.021** (0.001) [5,688,056]	x	$\Delta_3 f p_{ijt}$	0.032** (0.001) [5,688,056]	x	0.036** (0.001) [3,500,106]
$\Delta_5 f p_{ijt}$	x	x	0.026** (0.001) [3,552,213]	$\Delta_5 f p_{ijt}$	0.038** (0.001) [3,552,213]	0.036** (0.001) [3,500,106]	x
Sales (OLS)				Sales (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.017** (0.001) [9,553,665]	x	x	$\Delta_1 f p_{ijt}$	x	0.043** (0.001) [5,688,056]	0.041** (0.001) [3,552,213]
$\Delta_3 f p_{ijt}$	x	0.033** (0.001) [5,688,056]	x	$\Delta_3 f p_{ijt}$	0.046** (0.001) [5,688,056]	x	0.043** (0.001) [3,500,106]
$\Delta_5 f p_{ijt}$	x	x	0.037** (0.001) [3,552,213]	$\Delta_5 f p_{ijt}$	0.046** (0.001) [3,552,213]	0.046** (0.001) [3,500,106]	x

Note: In this table we repeat the results from Table 2, now using the maximum number of observations possible in each regression. The number of observations are reported in []. See the notes to Table 2 for further details.

Table 16: Non-Structural Results: Earnings (including smaller firms)

Value Added (OLS)				Value Added (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.030** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.044** (0.001)	0.071** (0.002)
$\Delta_3 f p_{ijt}$	x	0.040** (0.001)	x	$\Delta_3 f p_{ijt}$	0.045** (0.001)	x	0.070** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.069** (0.001)	$\Delta_5 f p_{ijt}$	0.049** (0.001)	0.056** (0.001)	x
Value Added per worker (OLS)				Value Added per worker (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.017** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.023** (0.001)	0.036** (0.003)
$\Delta_3 f p_{ijt}$	x	0.022** (0.001)	x	$\Delta_3 f p_{ijt}$	0.026** (0.001)	x	0.030** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.024** (0.001)	$\Delta_5 f p_{ijt}$	0.030** (0.002)	0.029** (0.001)	x
Sales (OLS)				Sales (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.043** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.059** (0.001)	0.076** (0.001)
$\Delta_3 f p_{ijt}$	x	0.052** (0.001)	x	$\Delta_3 f p_{ijt}$	0.057** (0.001)	x	0.078** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.085** (0.001)	$\Delta_5 f p_{ijt}$	0.054** (0.001)	0.061** (0.001)	x
Obs	3,706,470	3,706,470	3,706,470	Obs	3,706,470	3,706,470	3,706,470

Note: This table replicates the results in Table 1, but now for an extended sample where we also include smaller firms, see Section C.2 for further details. See Table 1 for further details on measurements and definitions etc.

Table 17: Non-Structural Results: Wages (including smaller firms)

Value Added (OLS)				Value Added (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.018** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.039** (0.001)	0.037** (0.002)
$\Delta_3 f p_{ijt}$	x	0.030** (0.001)	x	$\Delta_3 f p_{ijt}$	0.039** (0.001)	x	0.043** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.032** (0.001)	$\Delta_5 f p_{ijt}$	0.045** (0.001)	0.045** (0.001)	x
Value Added per worker (OLS)				Value Added per worker (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.017** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.033** (0.001)	0.030** (0.003)
$\Delta_3 f p_{ijt}$	x	0.025** (0.001)	x	$\Delta_3 f p_{ijt}$	0.035** (0.001)	x	0.038** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.028** (0.001)	$\Delta_5 f p_{ijt}$	0.040** (0.002)	0.038** (0.001)	x
Sales (OLS)				Sales (IV)			
OLS	(1) $\Delta_1 w_{ijt}$	(2) $\Delta_3 w_{ijt}$	(3) $\Delta_5 w_{ijt}$	IV	(1) $\Delta_1 w_{ijt}, \Delta_1 f p_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 f p_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 f p_{ijt}$
$\Delta_1 f p_{ijt}$	0.029** (0.001)	x	x	$\Delta_1 f p_{ijt}$	x	0.050** (0.001)	0.047** (0.001)
$\Delta_3 f p_{ijt}$	x	0.042** (0.001)	x	$\Delta_3 f p_{ijt}$	0.050** (0.001)	x	0.049** (0.001)
$\Delta_5 f p_{ijt}$	x	x	0.041** (0.001)	$\Delta_5 f p_{ijt}$	0.052** (0.001)	0.052** (0.001)	x
Obs	3,706,470	3,706,470	3,706,470	Obs	3,706,470	3,706,470	3,706,470

Note: This table replicates the results in Table 2, but now for an extended sample where we also include smaller firms, see Section C.2 for further details. See Table 2 for further details on measurements and definitions etc.

Table 18: Non-structural results: Earnings

Value Added				
OLS		(1) $\Delta_1 w_{ijt}, \Delta_1 y_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 y_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 y_{ijt}$
1	No controls	0.030** (0.001)	0.040** (0.001)	0.066** (0.001)
2	Add year dummies	0.029** (0.001)	0.039** (0.001)	0.070** (0.001)
3	Baseline controls	0.028** (0.001)	0.036** (0.001)	0.063** (0.001)
4	Add further age, tenure, education controls	0.028** (0.001)	0.035** (0.001)	0.053** (0.001)

VA pr worker				
OLS		(1) $\Delta_1 w_{ijt}, \Delta_1 y_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 y_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 y_{ijt}$
1	No controls	0.014** (0.001)	0.011** (0.001)	0.006** (0.001)
2	Add year dummies	0.002 (0.017)	0.020** (0.001)	0.021** (0.001)
3	Baseline controls	0.018** (0.001)	0.021** (0.001)	0.023** (0.001)
4	Add further age, tenure, education controls	0.018** (0.001)	0.021** (0.001)	0.022** (0.001)

Sales				
OLS		(1) $\Delta_1 w_{ijt}, \Delta_1 y_{ijt}$	(2) $\Delta_3 w_{ijt}, \Delta_3 y_{ijt}$	(3) $\Delta_5 w_{ijt}, \Delta_5 y_{ijt}$
1	No controls	0.050** (0.001)	0.062** (0.001)	0.090** (0.001)
2	Add year dummies	0.041** (0.001)	0.049** (0.001)	0.084** (0.001)
3	Baseline controls	0.040** (0.001)	0.046** (0.001)	0.075** (0.001)
4	Add further age, tenure, education controls	0.040** (0.001)	0.045** (0.001)	0.065** (0.001)

Obs. 3,500,106 3,500,106 3,500,106

Note: In this table we show how our OLS estimates on passthrough (as reported in Table 1) change as we change the set of control variables we include in the analysis. In row (1) we report the estimates when we do not include any control variables. In row (2) we add year dummies to the specification. In row 3 control variables in the regression are year dummies, linear, squared, and a cubic experience measures, occupation group dummies, and dummies for industry. In row 4 we add age, gender and education dummies, in addition we add a linear tenure term. Standard errors are clustered at the worker level and reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

C.4 Robustness of the Structural Income Passthrough Estimates

C.4.1 Alternative Specifications of the Firm Performance Equation

In Table 19, we show how our estimates of the firm performance process as presented in Section 4.2 change as we make changes to the overall estimation setup. In Column (1), we repeat our main results as reported in Table 3 in the main text. In Column (2) we add controls for firm (log)assets and a dummy variable indicating whether information on assets is missing (if assets are missing, we set $\log(\text{assets})$ to 0). In Column (3) we additionally control for investments (in levels), and in Column (4) we include a variable for firm age cubic (we already include linear and quadratic firm age in our set of baseline controls). Unsurprisingly, the estimate of the persistence in the firm process (ρ) decreases when we include these additional controls, and while the importance of transitory shocks in the error term decreases, none of these specifications challenge the existence of a permanent part in the error-term. In the case of value added, the relative importance of transitory part declines from around a factor of 5 to around 3.

In Column (5) we control for industry specific year effects and generally see very limited changes in estimates. In Columns (6)-(9) we change the set of instruments we use to estimate the dynamic panel data model. We note that estimates should be interpreted cautiously as only the estimates reported in the main text “survives” the standard specification tests (e.g. the AB test and RW test, see Table 3). In Column (6) we use values of the lagged dependent variable from period $t - 4$ and as long back as possible. In Column (7) we use values of the lagged dependent variable from period $t - 3$ only. Finally, in Column (8) we use values of the lagged dependent variable from period $t - 4$ only. In Columns (6) and (8) the estimates for value added change a lot, and the estimates suggest that there is no permanent part in the error term (or that the error term is generally not well behaved), the most likely driver is that the instruments are simply too weak (too far back in time). This illustrates the importance of specification tests and careful analysis of the error term. This is further reinforced since the estimates for value added in Column (7) are more similar to the baseline results (as reported in Table 3 we use instruments $t - 3$ and $t - 4$ for value added to create our main results). In contrast the estimates for value added per worker and sales change very little in Columns (6) and (8), this is expected as the main results are using instruments from period $t - 4$ and $t - 5$. On the contrary especially the estimates for sales change when we use earlier periods as instruments as in Column (7).

C.4.2 Worker Income Process Estimates

In Table 20 we present the results from the estimation of the worker earnings and hourly wage process, respectively. As outlined in Section 3.2.2 we estimate a dynamic panel data model using the Arrelano-Bond estimator. Again, our choice of instruments is guided by the autocovariance function of the subsequent error term and the Arrelano and Bond (1991) test statistics. We use different lags as instruments across the two specifications: For hourly wages we find the most appropriate specification when using lags 3 to 5 as instruments. For this specification the Arrelano-

Table 19: Robustness of firm performance estimates

Value added	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Benchmark	Add Assets	Add Investment	Add cubic firmage	Industry*Year	Instrument $f_{p_{t-4}, \dots}$	Instrument $f_{p_{t-3}}$	Instrument $f_{p_{t-4}}$
ρ	0.431** (0.015)	0.279** (0.012)	0.278** (0.012)	0.270** (0.012)	0.421** (0.015)	0.584** (0.015)	0.365** (0.015)	0.515** (0.017)
Number of firm-years used in estimation	196,909	196,909	196,909	196,909	196,909	196,909	196,909	196,909
Variance transitory part, σ_e^2	0.06	0.04	0.04	0.04	0.06	0.07	0.05	0.07
Variance permanent part, σ_u^2	0.01	0.01	0.01	0.01	0.01	-0.00	0.02	0.00
Ratio (trans/perm)	4.97	3.07	3.05	2.86	4.64	-39.39	2.72	17.40

Value added per worker	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Benchmark	Add Assets	Add Investment	Add cubic firmage	Industry*Year	Instrument $f_{p_{t-4}, \dots}$	Instrument $f_{p_{t-3}}$	Instrument $f_{p_{t-4}}$
ρ	0.292** (0.041)	0.157** (0.036)	0.156** (0.036)	0.188** (0.037)	0.292** (0.042)	0.282** (0.037)	0.258** (0.030)	0.261** (0.041)
Number of firm-years used in estimation	196,909	196,909	196,909	196,909	196,909	196,909	196,909	196,909
Variance transitory part, σ_e^2	0.04	0.03	0.03	0.03	0.04	0.04	0.04	0.04
Variance permanent part, σ_u^2	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Ratio (trans/perm)	7.96	1.93	1.92	2.43	8.32	6.84	5.05	5.22

Sales	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Benchmark	Add Assets	Add Investment	Add cubic firmage	Industry*Year	Instrument $f_{p_{t-4}, \dots}$	Instrument $f_{p_{t-3}}$	Instrument $f_{p_{t-4}}$
ρ	0.507** (0.015)	0.318** (0.012)	0.317** (0.012)	0.320** (0.014)	0.500** (0.020)	0.561** (0.015)	0.321** (0.014)	0.486** (0.017)
Number of firm-years used in estimation	196,909	196,909	196,909	196,909	196,909	196,909	196,909	196,909
Variance transitory part, σ_e^2	0.04	0.02	0.02	0.02	0.04	0.04	0.03	0.04
Variance permanent part, σ_u^2	0.01	0.01	0.01	0.01	0.01	0.01	0.02	0.01
Ratio (trans/perm)	5.18	3.01	3.00	2.96	5.19	9.93	1.25	4.28

Notes: In this table we analyze the robustness of our estimates of the firm performance process as we change the set of control variables or instruments used in estimation, see the text in Section C.4.1 for a detailed explanation of the differences in models across the different columns of the table. Standard errors are clustered at the firm level and reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

Table 20: Worker Income Process Estimates

	Earnings	Hourly Wage
ρ_W	0.036** (0.001)	0.153** (0.010)
Observations	9,553,665	9,553,665
Instruments	w_{it-4}, w_{it-5}	w_{it-3}, w_{it-4}
Specification tests	Earnings	Hourly Wage
AB(2) test statistic	16.44**	5.06**
AB(3) test statistic	-12.16**	1.54
AB(4) test statistic	0.87	-0.28
AB(5)	-2.22**	-1.03
Correlation between residuals	Earnings	Hourly Wage
Hourly Wage	0.41	1
Earnings	1	x
$Var(\Delta\omega_{ijt})$	0.049	0.016

Note: In this table we present the estimates of ρ_W in Equation (3), along with values of the AB test and the correlation across the obtained residuals (see also Section C.4.2). Control variables are experience, experience squared and cubed, industry, occupation, and year dummies (see footnote 15). Standard errors are reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

Bond tests reject auto-correlation in the differenced error terms above the order of 2. Earnings display much more serial correlation and there we end up using instruments based on lags 4 to 5.

Table 20 presents the estimates of ρ_W in Equation (3), along with values of the AB test and the correlation across the obtained residuals. Note that although there is a small spike in the autocovariance function for earnings of order 5, this does not drive our results, see also our robustness checks in the next subsection.

C.4.3 Robustness of Income Passthrough

In this section we illustrate the robustness of our wage passthrough estimates reported in Table 4. Across the different columns we show how the baseline results change as we focus on specific subsamples, change the included controls in the firm performance estimation (and thus potentially change the firm shock process), or change which lags in the dependent variable we use for estimation on either the worker or firm side, and finally as we include additional instruments in the final estimation of α and β . We note that these models (Columns 2 to 5) are not necessarily internally consistent in the sense that the serial correlation as suggested by the AB-test suggests that the chosen instruments are invalid (or that earlier lags should also be used) and results should

therefore be interpreted with caution. Still we think the estimates are useful in illustrating how our main findings change as we make small changes in the setup of the estimation.

In Column (2) we exclude observations where there are cases of part-time, leave, or other spells of non-employment throughout the year. Across the different worker and firm performance measures we see that the general result of higher passthrough rates for permanent shocks remains. Columns (3)-(5) report the income passthrough estimates when we change the instruments we use for identification. Again the estimates suggest that α is significantly and quantitatively larger than β across the different worker and firm performance measures. Generally the role of permanent shocks increases in Columns (3) and (4) suggesting that our estimate may be conservative. Also note that the ordering in terms of the size of elasticities across firm performance measures remains. Fluctuations in value added and sales have higher passthrough rates than value added per head.

C.4.4 Income Passthrough: Heterogeneity

Below we present our estimates of α and β for different sub-samples using different firm performance measures Value added per worker and Sales to complement the results reported in Section 4.2.3, see also the discussion there.

Table 21: Robustness Structural Approach: Earnings

(a) Value Added (levels)

	Baseline	Core workers	Instruments Worker	Instruments Firm	Instrument powers
	(1)	(2)	(3)	(4)	(5)
Transitory β	0.016** (0.001)	0.012** (0.001)	0.016** (0.001)	0.016** (0.001)	0.015** (0.004)
N	5,688,055	5,041,373	5,688,055	5,688,055	5,688,055
Permanent α	0.079** (0.006)	0.064** (0.006)	0.145** (0.013)	0.139** (0.013)	0.079** (0.006)
N	2,752,431	2,391,612	2,752,431	2,752,431	2,752,431

(b) Value Added per head

	Baseline	Core workers	Instruments Worker	Instruments Firm	Instrument powers
	(1)	(2)	(3)	(4)	(5)
Transitory β	0.026** (0.001)	0.026** (0.001)	0.027** (0.001)	0.027** (0.001)	0.026** (0.001)
N	5,688,055	5,041,373	5,688,055	5,688,055	5,688,055
Permanent α	0.046** (0.005)	0.045** (0.006)	0.044** (0.004)	0.043** (0.004)	0.048 (0.030)
N	2,752,431	2,391,612	2,752,431	2,752,431	2,752,431

(c) Sales

	Baseline	Core workers	Instruments Worker	Instruments Firm	Instrument powers
	(1)	(2)	(3)	(4)	(5)
Transitory β	0.011** (0.001)	0.090** (0.001)	0.011** (0.001)	0.011** (0.001)	0.011* (0.005)
N	5,688,055	5,041,373	5,688,055	5,688,055	5,688,055
Permanent α	0.053** (0.009)	0.065** (0.009)	0.053** (0.009)	0.053** (0.009)	0.077** (0.009)
N	2,752,431	2,391,612	2,752,431	2,752,431	2,752,431

Note: These tables analyze the robustness of our estimates of earnings passthrough. In Column (1) we repeat the baseline estimates from Table 4. In column (2) we exclude part-time workers or workers with leave or other spells of non-employment throughout the year. In Column (3) we report the passthrough estimates when we change the instruments (lags on the dependent variable) used to estimate Equation (3). In particular we only use lags 3 to 4 compared to lags 4 to 5 in the baseline. In column (4) we report the passthrough estimates when we change the instruments (lags on the dependent variable) used to estimate Equation (5). In particular we use lags 4 to 5 for value added and sales in contrast to 3 to 4 in the baseline, and we use lags 3 to 4 for value added per head in contrast to lags 3 to 4 in baseline. Finally, in Column (5) we report the passthrough estimates when we include squared and cubic versions of the instruments in Equations (11) and (10). Standard errors are reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

Table 22: Robustness Structural Approach: Wages

(a) Value Added

	Baseline	Core workers	Instruments Worker	Instruments Firm	Instrument powers
	(1)	(2)	(3)	(4)	(5)
Transitory β	0.007** (0.001)	0.005** (0.001)	0.008** (0.001)	0.008** (0.001)	0.005** (0.002)
N	5,688,055	5,041,373	5,688,055	5,688,055	5,688,055
Permanent α	0.080** (0.004)	0.069** (0.004)	0.141** (0.009)	0.129** (0.009)	0.068** (0.013)
N	2,752,431	2,391,612	2,752,431	2,752,431	2,752,431

(b) Value Added per head

	Baseline	Core workers	Instruments Worker	Instruments Firm	Instrument powers
	(1)	(2)	(3)	(4)	(5)
Transitory β	0.015** (0.001)	0.012** (0.001)	0.015** (0.001)	0.015** (0.001)	0.013** (0.003)
N	5,688,055	5,041,373	5,688,055	5,688,055	5,688,055
Permanent α	0.047** (0.003)	0.029** (0.004)	0.048** (0.003)	0.043** (0.003)	0.038* (0.017)
N	2,752,431	2,391,612	2,752,431	2,752,431	2,752,431

(c) Sales

	Baseline	Core workers	Instruments Worker	Instruments Firm	Instrument powers
	(1)	(2)	(3)	(4)	(5)
Transitory β	0.006** (0.001)	0.003** (0.001)	0.006** (0.001)	0.006** (0.001)	0.005 (0.003)
N	5,688,055	5,041,373	5,688,055	5,688,055	5,688,055
Permanent α	0.089** (0.001)	0.087** (0.006)	0.088** (0.006)	0.088** (0.006)	0.092** (0.024)
N	2,752,431	2,391,612	2,752,431	2,752,431	2,752,431

Note: These tables analyze the robustness of our estimates of wage passthrough. In Column (1) we repeat the baseline estimates from Table 4. In column (2) we exclude part-time workers or workers with leave or other spells of non-employment throughout the year. In Column (3) we report the passthrough estimates when we change the instruments (lags on the dependent variable) used to estimate Equation (3). In particular we only use lags 3 to 4 compared to lags 3 to 5 in the baseline. In column (4) we report the passthrough estimates when we change the instruments (lags on the dependent variable) used to estimate Equation (5). In particular we use lags 4 to 5 for value added and sales in contrast to 3 to 4 in the baseline, and we use lags 3 to 4 for value added per head in contrast to lags 3 to 4 in baseline. Finally, in Column (5) we report the passthrough estimates when we include squared and cubic versions of the instruments in Equations (11) and (10). Standard errors are reported in parenthesis. ** and * indicate statistical significance (two-sided) at the 1% and 5% levels, respectively.

Table 23: Heterogeneity: Earnings, VA per worker

	Earnings, VA pr worker	Managers	Production	High educ	Low education	Business Service	low firm size	year<=2001	year>2001
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Transitory	0.026** (0.001)	0.029** (0.003)	0.022** (0.001)	0.037** (0.002)	0.017** (0.002)	0.028** (0.002)	0.030** (0.003)	0.014** (0.001)	0.035** (0.001)
N	5,688,055	352,926	1,849,250	649,503	752,425	494,942	227,876	1,866,152	3,821,903
Permanent	0.046** (0.005)	0.065** (0.023)	0.070** (0.007)	-0.015 (0.019)	0.050** (0.011)	0.102** (0.012)	0.120** (0.027)	0.013** (0.005)	0.076** (0.008)
N	2,752,431	183,908	910,630	301,744	359,039	225,202	95,030	784,910	1,967,521

Note: This table reports the estimates of α and β (see Equations (11) and (10)) across different subsamples using value added per worker (these results complement the results in Table 5). Column (1) repeats the estimates from Table (4). Columns (2) and (3) focus on occupations involving leadership or blue collar work (manual labor) as identified by the Danish occupation classification (DISCO), see section 2. Columns (4) and (5) focus on individuals with 10 or less (above 16) years of education. Column (6) focuses on workers in the Business Service Industry. Column (7) focuses on workers in firms with less than 15 employees (in full time equivalents) in a given year. Column (8) [(9)] present estimates based on years up until [after] 2001. Standard errors are reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

Table 24: Heterogeneity: Earnings, Sales

	Earnings, Sales	Managers	Production	High educ	Low education	BusinessService	low firm size	year<=2001	year>2001
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Transitory	0.011** (0.001)	0.014** (0.003)	0.023** (0.001)	0.001 (0.003)	0.012** (0.002)	-0.001 (0.002)	0.036** (0.003)	0.011** (0.001)	0.011** (0.001)
N	5,688,055	352,926	1,849,250	649,503	752,425	494,942	227,876	1,866,152	3,821,903
Permanent	0.053** (0.009)	0.268** (0.037)	0.117** (0.019)	0.028 (0.030)	0.031 (0.020)	0.195** (0.056)	0.209** (0.024)	0.076** (0.008)	0.038** (0.014)
N	2,752,431	183,908	910,630	301,744	359,039	225,202	95,030	784,910	1,967,521

Note: This table reports the estimates of α and β (see Equations (11) and (10)) across different subsamples using sales (these results complement the results in Table 5). Column (1) repeats the estimates from Table (4). Columns (2) and (3) focus on occupations involving leadership or blue collar work (manual labor) as identified by the Danish occupation classification (DISCO), see section 2. Columns (4) and (5) focus on individuals with 10 or less (above 16) years of education. Column (6) focuses on workers in the Business Service Industry. Column (7) focuses on workers in firms with less than 15 employees (in full time equivalents) in a given year. Column (8) [(9)] present estimates based on years up until [after] 2001. Standard errors are reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

Table 25: Employment-to-Unemployment transitions for Sales

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	IV3	IV5	Transitory (guiso)	Permanent (guiso)
U exit	−0.061** (0.001)	−0.059** (0.001)	−0.112** (0.001)	−0.130** (0.001)	0.001 (0.001)	−0.589** (0.019)
N	10,050,182	10,050,159	8,710,002	7,201,872	8,524,766	6,230,141
Controls	No	Yes	Yes	Yes	Yes	Yes

Note: This table presents estimates of γ in Equation (4) based on sales as the measure of firm performance. Column (2) controls for year and age dummies, as well as broad occupation and industry dummies. Column (3) and (4) instruments $\Delta f p_{jt}$ with $\Delta_3 f p_{jt} = f p_{jt+1} - f p_{jt-2}$ and $\Delta_5 f p_{jt} = f p_{jt+2} - f p_{jt-3}$ respectively. Column (5) and (6) instruments $\Delta f p_{jt}$ with $\Delta \epsilon_{jt+1}$ and $\sum_{\tau=-2}^2 \Delta \epsilon_{jt+\tau}$ where residuals are the same as those used in Section 3.2. For each regression we use all the largest possible sample. Standard errors are clustered at the worker level and reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

D Appendix: Supplementary Results for Analysis on Employment Margin

D.1 Employment passthrough: Sales

Below we repeat the analysis of Section 5 now using sales as the measure of firm performance instead of value added. In Table 25 we report similar type estimates to Table 6. Our results from Column 1-4 are slightly larger than the results based on value added but follow the same overall pattern. For instance the estimates in Column 2 suggest that a one percent increase in value added decreases the probability of making a job-to-unemployment transition with 0.059 percentage point which is larger than the 0.038 effect for value added. In Table 26 we report similar type estimates to Table 7.

Finally, in table 27 we repeat the subgroup analysis from Table 9 and Section 5.2 now using Sales as the measure of firm performance. Again the results discussed in the main text appear robust, i.e. considering exits to unemployment, blue collar workers, workers with low education and workers with low tenure are more affected by a shock to sales whereas managers, highly educated workers, and workers with high tenure are not affected as much. On the contrary, when we consider job-to-job transitions, the most affected sub-groups are managers and high educated workers.

D.2 Same Number of Observations

In Section 5, Tables 6 and 7, we show how our estimates for passthrough change as we use different control variables and industries. However, the sample also changes across the columns in the tables because the different models imply different sample requirements. For completeness, in Table 28 we therefore display the results for the different IV models (Columns 1-4) on a sample

Table 26: Job-to-Job Transitions for Sales

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	IV3	IV5	Transitory (guiso)	Permanent (guiso)
J-t-J exit	−0.070** (0.001)	−0.078** (0.001)	−0.138** (0.001)	−0.143** (0.001)	−0.009** (0.001)	−0.668** (0.018)
N	10,152,525	10,152,502	8,812,385	7,250,307	8,577,190	6,250,299
Controls	No	Yes	Yes	Yes	Yes	Yes

Note: This table presents estimates of γ in Equation (4) based on sales as the measure of firm performance. Column (2) controls for year and age dummies, as well as broad occupation and industry dummies. Columns (3) and (4) instrument $\Delta f p_{jt}$ with $\Delta_3 f p_{jt} = f p_{jt+1} - f p_{jt-2}$ and $\Delta_5 f p_{jt} = f p_{jt+2} - f p_{jt-3}$ respectively. Columns (5) and (6) instrument $\Delta f p_{jt}$ with $\Delta \epsilon_{jt+1}$ and $\sum_{\tau=-2}^2 \Delta \epsilon_{jt+\tau}$ where residuals are the same as those used in Section 3.2. For each regression we use all the largest possible samples. Standard errors are clustered at the worker level and reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

where we only focus on observations where a 5-year window of firm performance measures exists, thus securing the same number of observations across models. The results are very similar to those reported in Table 6. Keep in mind that focusing the sample this way does not imply selecting workers who stay in the firm for 5-periods, so in this sense we are not focusing on stayers as such.

D.3 The Role of Firm Size

In Table 29 we show how our baseline estimates of passthrough of firm shocks to value added to the probability of making a job-to-job transition change as we include various controls for firm size. See also the discussion in the main text in relation to Table 8.

D.4 Non-Linear Plots

In Figure 1, we plot the relationship between e_{ijt} and $\Delta f p_{jt}$ and the reduced form relationship using $\Delta_5 f p_{jt}$ as an instrument when e_{ijt} is an indicator for making a job-to-job transition. The results were discussed in Section 6.

Table 27: Transitions, Sales: Heterogeneity

Employment-to-Unemployment	Baseline	Managers	Blue Collar	High Educ	Low Educ	Business	Small firms	Year \leq 2001	Year > 2001	Low Tenure	High Tenure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
OLS with controls	-0.059** (0.001)	-0.034** (0.002)	-0.070** (0.001)	-0.037** (0.001)	-0.075** (0.002)	-0.039** (0.001)	-0.047** (0.001)	-0.070** (0.001)	-0.049** (0.001)	-0.065** (0.001)	-0.048** (0.001)
	10,050,159	531,407	3,254,329	1,149,004	1,344,762	917,474	459,849	6,254,423	3,795,736	3,689,844	3,999,220
	-0.130** (0.001)	-0.071** (0.005)	-0.155** (0.002)	-0.082** (0.003)	-0.166** (0.004)	-0.080** (0.004)	-0.110** (0.005)	-0.124** (0.002)	-0.140** (0.002)	-0.168** (0.003)	-0.110** (0.002)
	7,201,872	389,809	2,373,215	859,618	918,621	653,489	264,438	4,950,303	2,251,569	2,252,903	3,116,828
Job-to-Job	Baseline	Managers	Blue Collar	High educ	Low educ	Business	Small firms	Year \leq 2001	Year > 2001	Low Tenure	High Tenure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
OLS with controls	-0.078** (0.001)	-0.079** (0.001)	-0.068** (0.001)	-0.083** (0.001)	-0.076** (0.001)	-0.063** (0.001)	-0.054** (0.001)	-0.081** (0.001)	-0.075** (0.001)	-0.064** (0.001)	-0.082** (0.001)
	10,152,502	2,304,824	3,233,304	1,199,536	1,317,630	947,571	461,844	6,342,931	3,809,571	3,695,978	4,052,372
	-0.143** (0.001)	-0.171** (0.003)	-0.127** (0.002)	-0.189** (0.004)	-0.109** (0.004)	-0.140** (0.004)	-0.101** (0.005)	-0.147** (0.002)	-0.140** (0.002)	-0.147** (0.003)	-0.147** (0.002)
	7,250,307	1,666,866	2,345,463	894,895	895,498	672,828	264,334	4,992,189	2,258,118	2,246,122	3,147,764

Note: This table presents estimates of γ in Equation (4) for firm performance measures Sales across different subsamples (see Table 9 for the same table based on value added). The row with "IV5" instruments $\Delta f p_{it}$ with $\Delta_5 p_{it} = f p_{it+2} - f p_{it-3}$. Column (1) repeats the estimates from Table (6). Columns (2) and (3) focus on occupations involving leadership or blue collar work (manual labor) as identified by the Danish occupation classification (DISCO), see section 2. Columns (4) and (5) focus on individuals with 10 or less (above 16) years of education. Column (6) focuses on workers in the Business Service Industry. Column (7) focuses on workers in firms with less than 15 employees (in full time equivalents) in a given year. Column (8) [9] present estimates based on years up until [after] 2001. Column (10) [11] focus on workers with no more than 4 [more than 8] years of tenure in the firm. Standard errors are reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.

Table 28: Employment-to-Unemployment Transitions - restricted sample

(a) Value Added

	OLS - no controls	OLS - w controls	IV - instrument 3	IV - instrument 5
UE exit	−0.037** (0.001)	−0.033** (0.001)	−0.065** (0.001)	−0.130** (0.002)
N	5,887,372	5,887,360	5,887,360	5,887,360

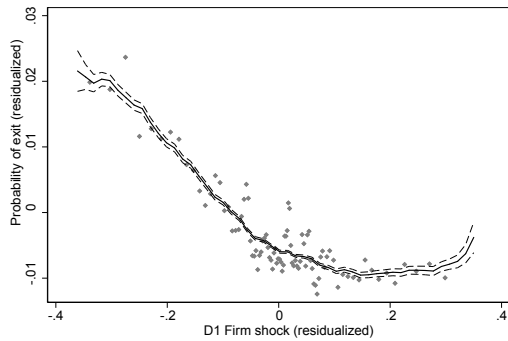
(b) Sales

	OLS - no controls	OLS - w controls	IV - instrument 3	IV - instrument 5
UE exit	−0.064** (0.001)	−0.059** (0.001)	−0.084** (0.001)	−0.131** (0.002)
	5,887,372	5,887,360	5,887,360	5,887,360

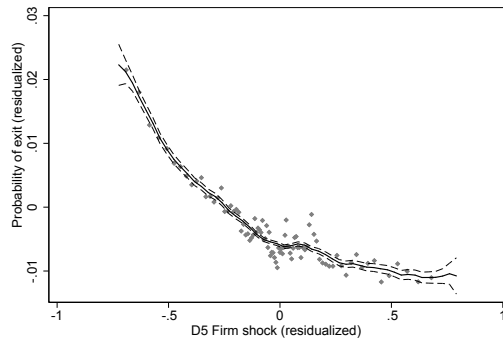
Note: This table repeats the results reported in Table 6 on a sample where we only focus on observations where a 5-year window of firm performance measures exists, thus securing the same number of observations across models. For further information see the notes to Table 6.

Figure 3: Plot of Value added and Job-to-Job Transition

(a) OLS



(b) Reduced-form



Note: This figure is constructed as follows: first we regress $\Delta f p_{jt}$ (and subsequently e_{ijt}) on the explanatory variables used in Equation (4) and obtain the residuals. We then divide $\Delta f p_{jt}$ into 100 equally sized bins and calculate the mean of $\Delta f p_{jt}$ and e_{ijt} within each bin. These data-points are the gray diamonds in Figure (3a). The solid line is the result of local linear regressions of the two residuals using a rectangular kernel. The plot discards observations above the 5th and 95th percentile. Figure (3b) presents results from the same procedure using 5-year differences ($\Delta_5 f p_{jt}$) around time t instead of $\Delta f p_{jt}$.

Table 29: Job-to-Job Transitions: Firm Size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	OLS	OLS	IV3	IV3	IV3	IV5	IV5	IV5
U exit	-0.049** (0.001)	-0.057** (0.001)	-0.007** (0.001)	-0.101** (0.001)	-0.093** (0.001)	-0.017** (0.001)	-0.147** (0.001)	-0.138** (0.002)	-0.060** (0.002)
N	10,152,502	9,549,102	9,549,102	8,812,385	8,807,308	8,807,308	7,250,307	7,229,071	7,229,071
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
size _{t-2}	No	yes	yes	No	yes	yes	No	yes	yes
size _{t-1}	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
$\Delta \text{size}_{t-1,t}$	No	No	Yes	No	No	Yes	No	No	Yes

Notes: In this table we show how our estimates of passthrough on the probability of making an job-to-job transition change as we include different controls for the size of the firm. The firm performance measure is value added. See Table 8 for similar results for employment-to-unemployment transitions. size_{t-2} measures the log of the size of the firm (in fulltime equivalents) in year $t - 2$. $\Delta \text{size}_{t-1,t+1}$ measures the change in (log) firm size from year $t - 1$ to t . Standard errors are clustered at the worker level and reported in parenthesis. ** and * indicate statistical significance at the 1% and 5% levels, respectively.