Microdata Evidence on the Empirical Importance of Selection Effects in Menu-Cost Models*

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Abstract

We use microdata on product prices linked to information on the producing firms that set them to study to what extent the timing of price changes reacts to changes in marginal cost. This self-selection of price-changes is a key feature in the canonical Menu-Cost model a la Golosov and Lucas (2007), which may generate near monetary neutrality (Golosov and Lucas, 2007, Karadi and Reiff, 2016), but is absent in the Calvo (1983) model. We find that the microdata strongly favors the Calvo (1983) model. Thus, upstream in the supply chain, price setting is best characterized by a very low degree of self-selection into price changes.

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1 Introduction

In the canonical workhorse model of applied macroeconomics, the New Keynesian model, nominal frictions are the keystone for generating monetary non-neutrality and a role for monetary policy.[1] A key simplifying assumption in this model, relying on the Calvo (1983) price-setting model, is that the pricing decision faced by the firm is only about the magnitude of the price change and not the timing of the price change.[2] However, treating the timing (as well as the magnitude) of price changes as a regular profit-maximizing choice subject to a fixed cost of price changes as in the Menu-Cost model of pricing, can have a dramatic effect on the degree of monetary non-neutrality; see Caplin and Spulber (1987), Dotsey, King, and Wolman (1999), Golosov and Lucas (2007), Midrigan (2011) and Karadi and Reiff (2016). The main driver behind this result is the self-selection mechanism that mitigates the real effects of money. That is, firms that change price in the Menu-Cost model of pricing are those that have the most to gain from it. This increases the effect on the price level from a monetary shock relative to a model relying on Calvo (1983) pricing and reduces the degree of monetary non-neutrality. Moreover, this modeling choice also affects other properties of the model, such as determinacy under a specific policy rule; see Dotsey and King (2005) for a discussion. Thus, whether self-selection by firms into the price-changing group is a feature of observed firm behavior or not is an important question for macroeconomic analysis and the policy advice derived from it.

The key empirical challenge when assessing the empirical relevance of selection effects is that we generally do not observe the price the firm would set if it had the opportunity to re-optimize it's price (i.e. the "reset price"). Due to the unobservability of reset prices some authors have sought to infer the degree of selectivity in price changes with methods that does not require a direct measure of the reset price.[3] In contrast, a number of studies relate individual pricing decisions to some measure of a firms reset price such as the deviation from the price set by local competitors (e.g. Campbell and Eden, 2014) or

[1] See Smets and Wouters (2003) and Christiano, Eichenbaum, and Evans (2005).

[2] The tractability gain from making the firm's pricing decision only about the magnitude of the price change comes from the reduced dimensionality needed when describing the evolution of the aggregate price level.

[3] See e.g. Caballero and Engel (1993a), Caballero and Engel (1993b), Bils, Klenow, and Malin (2012), Gopinath and Itskhoki (2010), Gopinath and Itskhoki (2011), Gagnon, Mandel, and Vigfusson (2014), Gagnon, López-Salido, and Vincent (2013), Klenow and Kryvtsov (2008). These are merely examples from a vast literature and a more complete reference list is given in Klenow and Malin (2010).

a replacement cost measure of marginal cost (e.g. Eichenbaum, Jaimovich, and Rebelo, 2011). The latter strand of the literature focuses on data downstream in the supply chain, i.e. the retail sector. In this paper, and as in Carlsson and Nordström Skans (2012), the focus is instead on price-setting behavior upstream in the supply chain and draws on very detailed annual Swedish data on product producer prices matched to a rich data set containing information on the activity of the firms that set these prices. To our knowledge, this is the first data set where such detailed quantitative price data have been merged with detailed information on firm-level activity for a broad sample (702) of industrial firms. Using the firm-level data, we construct a measure of marginal cost (i.e. unit labor cost, ULC henceforth) consistent with the vast majority of DSGE models in the literature, as well as, the price-setting models outlined below. In contrast to retail replacement costs, unit labor cost is not affected by different marketing considerations in consumer markets that may affect the inference.[4]

Relying on the same data set and measurement as employed here, Carlsson and Nordström Skans (2012) established that the marginal cost measure (unit labor cost) is an important driver of the magnitude of price changes and report empirical evidence in support of a nominal frictions interpretation of the data. Carlsson and Nordström Skans (2012) is focused on studying the drivers of the magnitude of price changes, but does not address the question of whether or not the timing of price changes is endogenous. To handle this issue, they instead restrict the regression sample to observations where prices change. In contrast, this paper explores to what extent price setting features important selection effects or not. Importantly, the focus here is directly on firm behavior and whether or not we observe economically significant selection effects on the micro level. This is a necessary condition for this mechanism to play an important role in the degree of monetary non-neutrality. Note, however, that the overall importance of selection effects for monetary non-neutrality is driven by the interaction of the measure of marginal firms lying close to the adjustment threshold and the size of the adjustment needs; see Karadi and Reiff (2016) for a discussion.

To impose discipline on the empirical exercise at hand, we first outline and calibrate a

[4]For example, contracts between retailers and their suppliers that cause simultaneous changes in retail prices and replacement costs measures of marginal cost are common and may lead researchers to find counterfactually strong selection effects (see e.g. the discussion in Anderson, Nakamura, Simester and Steinsson, 2014). Also, the smaller product offerings of producers, relative to retailers, brings the empirical analysis on closer to the assumptions of standard pricing models.

baseline Menu-Cost model to match key moments in the data (apart from the empirical size of the selection effects). The Menu-Cost model we rely on is along the lines of Golosov and Lucas (2007), but allows for fat-tailed idiosyncratic shocks to marginal cost (akin to Midrigan, 2011) in order to better match the micro-data.[5] Moreover, the model is calibrated to a monthly frequency, which allows us to gauge the effect of time aggregation in the annual data, both in terms of observed moments, as well as, in terms of the power of the inference.

Aggregating the simulated data in the same way as the actual data is aggregated, we find that time aggregation fills out the gap of very small price changes that is otherwise a hallmark of the price-change distribution in menu-cost models. Actually, this type of data filtering takes the Menu-Cost model a long way in replicating the observed annual price change distribution. Thus, time aggregation is a complementary mechanism for generating small price changes in the Menu-Cost model to the economies of scope suggested by Lach and Tsiddon (2007), Midrigan (2011) and Alvarez and Lippi (2014) or stochastic menu costs as in Caballero and Engel (1999) and Dotsey, King, and Wolman (1999). Intuitively, pricing patterns where e.g. large positive and negative monthly changes within a year nearly cancel one another out generates small overall price movements in the time-aggregated data. Though, arguably the strength of the mechanism should increase with the time span of aggregation, but the potential for this mechanism is shared with many other data sets employed in the literature where the price is calculated from reported values and volumes over a time period as in this paper; see Eichenbaum, Jaimovich, Rebelo, and Smith (2014) for a discussion. Evaluating the importance of this mechanism in time aggregated data with higher frequency is left for future research, but we note that, any rebuttal of the Menu-Cost model using the data set at hand will not be due to its inability to capture the frequency of small price changes in the observed data.

Next, we analyze the strength of the selection mechanism by running probability models along the routes of what Cecchetti (1986), Buckle and Carlson (2000), Loupias and Sevestre (2013) and others have done previously relying on aggregate/sectoral or qualitative data to measure drivers of the reset price. Specifically, we investigate if the absolute value of the accumulated change in the firm's marginal cost, as well as the non-

[5] The Menu-Cost model of Golosov and Lucas (2007) builds in turn on work by Barro (1972) and Sheshinski and Weiss (1977) amongst others.

accumulated version of the same, affects the probability of a price change. A test of the Menu-Cost model is then provided by comparing the empirical results with the results from synthetic time-aggregated data generated by the Menu-Cost model matched to key moments in the data. We find an order of magnitude smaller effect on the probability of a price change than expected if the Menu-Cost model was generating the data. Moreover, when considering measurement issues pertaining to the classification of small price changes in the data, the (small) positive estimates we find seems to be the result of upward bias. Importantly, the sharp difference between the model prediction and the data also confirms that time aggregation is not an issues for the inference in this exercise.

Thus, the data reject that the selection effects are anywhere near the size that is expected from the Menu-Cost model. The question then is wether or not they are so small that a Calvo (1983) model would fit the data or if the data is better described by hybrid model including elements of both. To structurally quantify the regression results we then fit a price-setting model that nests both the Calvo (1983) and the Menu-Cost model to the data (i.e. a fat-tailed shocks version of the CalvoPlus model outlined in Nakamura and Steinsson, 2010), which can generate an arbitrary degree of selection effects in the simulated micro data from the model. Importantly, the procedure to fit the model parameters can be constructed to be unaffected by the measurement issues that may bias the regression results. When choosing parameters so that the model matches key empirical moments as closely as possible, including the size of the empirical selection effects from the probability regression from the observed data, the parameters are driven very close to a standard Calvo (1983) model. This again implies that the selection effects are not an important feature of the data. It is also interesting to see that the calibrated CalvoPlus model does a good job in replicating the price change distributions, especially in terms of the kurtosis and the overall shape.

Thus, overall, timing adjustments of price changes to marginal-cost developments do not seem to be an important feature of observed price-setting behavior of goods-producing firms and the timing behavior with respect to price changes of these firms does not imply any potential for reducing monetary non-neutrality as in Golosov and Lucas (2007) or Karadi and Reiff (2016). Moreover, we also find that a Calvo (1983) model seems to provide a reasonable description of the price-setting behavior in our data. Note though that it is not argued that the Calvo (1983) model is the true underlying model of micro-level price setting, but rather that in order to be aligned with the data, any successful model of price setting in firms upstream in the supply chain needs to predict very small selection effects with respect to marginal cost shocks.

Interestingly, Eichenbaum, Jaimovich, and Rebelo (2011) also links a measure of marginal cost, i.e. the replacement cost of the vended product, to the price set in data drawn from a large US food and drug retailer and documents a high degree of selection effects in pricing downstream in the supply chain.[6] This indicates that there seems to be considerable differences in pricing behavior along the supply chain.[7] This is perhaps not surprising given differences in conditions between consumer and business-to-business markets, but this observation may provide important hints for future research on the microfoundations of pricing behavior.

An additional intriguing question is how upstreams marginal-cost shocks feeds through the pricing decision along the supply chain (see e.g. Nakamura, 2008). Although this issue is not addressed here, it is worth noting that in a simple supply chain model, where the price set by the upstreams firm represent marginal cost for the downstream firm, does not need price-setting frictions on all levels in order to generate significant monetary non-neutrality.[8] In fact, frictions found in the downstream sector can only add to monetary non-neutrality and given the results presented here, they are not instrumental for the existence of sizable monetary non-neutrality.

This paper is organized as follows: Section 2 presents the data, Section 3 outlines the Menu-Cost model used as a benchmark, Section 4 presents our results and, finally, Section 5 concludes the paper.

2 Data

The data set consists of quantitative price data on the product level that have been merged with information on the producing firm's production level, inputs and costs for

[6]Especially when considering reference prices (and costs) - i.e. when abstracting from high frequency variation such as sales commonly observed in consumer prices. As noted by Nakamura and Steinsson (2008), sales seem to be uncommon in producer price data.

[7]With the caveat that the close relation between prices and replacement cost could be a feature of how contracts are written between retailers and their suppliers as opposed to prima facie evidence of state dependence in retail.

[8]See e.g. the canonical New Keynesian model (Smets and Wouters, 2003, or Christiano, Eichenbaum and Evans, 2005).

a broad sample of manufacturing firms. This data set combines information on detailed product-prices drawn from the Swedish IVP ("Industrins Varuproduktion") survey with information on plant-level activity from the IS ("Industristatistiken") survey.

The IVP micro data provides annual information on prices and quantities of products for all Swedish industrial plants with at least 10 (20) employees for the years $1990-1996$ $(1997-2002)$ and a sample of smaller plants. The product classification is at the 8/9-digit level of the Harmonized System (HS) for the years $1990-1995$ and the Combined Nomenclature (CN) for the years $1996-2002$. The data allow us to follow the same product (or at least a very closely defined group of products) over time. The codes are fairly exact; an example of a product code is 84181010 for "*A combined freezer and cooler with separate exterior doors with a volume exceeding 340 liters intended for use in civilian aircrafts*". The (unit) price for each product code is calculated by dividing the firms' yearly reported value for the product code with the accompanying volume (in terms of the relevant measure, e.g. the number of products, cubic meters, metric tons, etc.). The data are thus based on actual transaction prices and not list prices.

A key novelty is that the price data can be matched to data on activity for the individual plant. The IS survey contains annual information on inputs and output for all Swedish industrial plants with 10 employees or more and a sample of smaller plants. We only use plants that are also a firm since pricing essentially is a firm-level and not a plant-level decision and since there is some scope for transactions between plants within a firm for tax reasons. In addition, we limit the analysis to firms that are in operation throughout the sample period since we want to identify normal behavior.

Following Rotemberg and Woodford (1999), Carlsson and Nordström Skans (2012) and others, we rely on unit labor cost as a measure of marginal cost.[9] To construct unit labor cost we use the IS survey data on the firms' wage bill divided by real output, where the latter variable is obtained by deflating nominal output from the IS survey (the value of total sales) using a firm-specific producer price index.[10]

Since the raw price data involve a few very large swings we apply a cleaning procedure in which we split the individual price series and give them a new unique plant-price

[9]See Section 3.1 for a theoretical justification for this measure.

[10]The price index is constructed as a chained index with Paasche links combining the plant-specific unit prices described above and the most detailed product/producer-price indices available. The product/producer-price indices are used if the 8/9-digit unit value data are not available due to missing data, changes in the firm's product portfolio, or when there are large swings (over the 1.5/98.5 centiles).

identifier whenever a large change in the growth rate appears in the raw data. The cut-off levels are given by the 1.5 and 98.5 centiles of the full raw data distribution. We also remove firms that are subject to large swings in the observed marginal cost. As with prices, we use the full distribution of log changes in unit labor cost across all firms for which this variable can be computed and remove firms with growth rates outside the $[1.5, 98.5]$ centiles in any one year of the sample period.

When merging data sets, we are left with $17,282$ price observations (where we at least observe the price of a product for two consecutive periods) across $1,610$ unique product codes, $3,510$ unique product/firm identities and 702 firms (as in Carlsson and Nordström Skans, 2012). These industrial firms are mainly medium to small firms with an average of 65 employees. See also Appendix A for more details on the data construction. There we also present evidence on the robustness of the results to more generous cut-off levels.

In Figure 1, we plot the final data distribution of log price changes (for the 8/9-digit unit price data). All in all, this comprises $13,772$ price-change observations. Each bin in the histogram represents a log difference of 0.01. Note that since these prices are calculated from reported values and volumes of sold products, there might be small rounding errors in the data, motivating a focus on a zero bin rather than exactly zero price changes.For example, survey respondents are asked to state the value of sold products in thousands of SEK, which will lead to rounding errors in calculated prices and thus small erroneous price changes in the data.[11,12] As can be seen in Figure 1 there is a substantial spike for the bin centered around zero. In fact, 13.6 percent of the price-change observations are confined within the ± 0.5 percent interval.

The observation that a substantial fraction of price spells remain fixed across years is well in line with existing survey evidence. When surveying 626 Swedish firms in 2002, Apel, Friberg, and Hallsten (2005) found that about 70 percent of the firms adjust their price once a year or less. Moreover, for the approximately $15,000$ European firms surveyed

[11] Note that the median value of sold products across product codes for the firms in our sample is SEK 6.1 million. Thus, at the median, the implied maximum potential span of rounding error in the price stemming from rounding values to nearest thousand of SEK is 0.0164 ($= (1000/6100000) * 100$) percentage units.

[12] Changes in the composition of buyers who pay different prices are another reason for small measurement errors when computing prices by dividing value with volume. Although common in retail prices, see Eichenbaum, Jaimovich, Rebelo, and Smith (2014), some of the price-setting practices in that sector, like discount coupons, two for one offers, and so on, are less likely to be prevalent in producer price setting. Also, Nakamura and Steinsson (2008) notes that sales seem to be uncommon in producer price data.

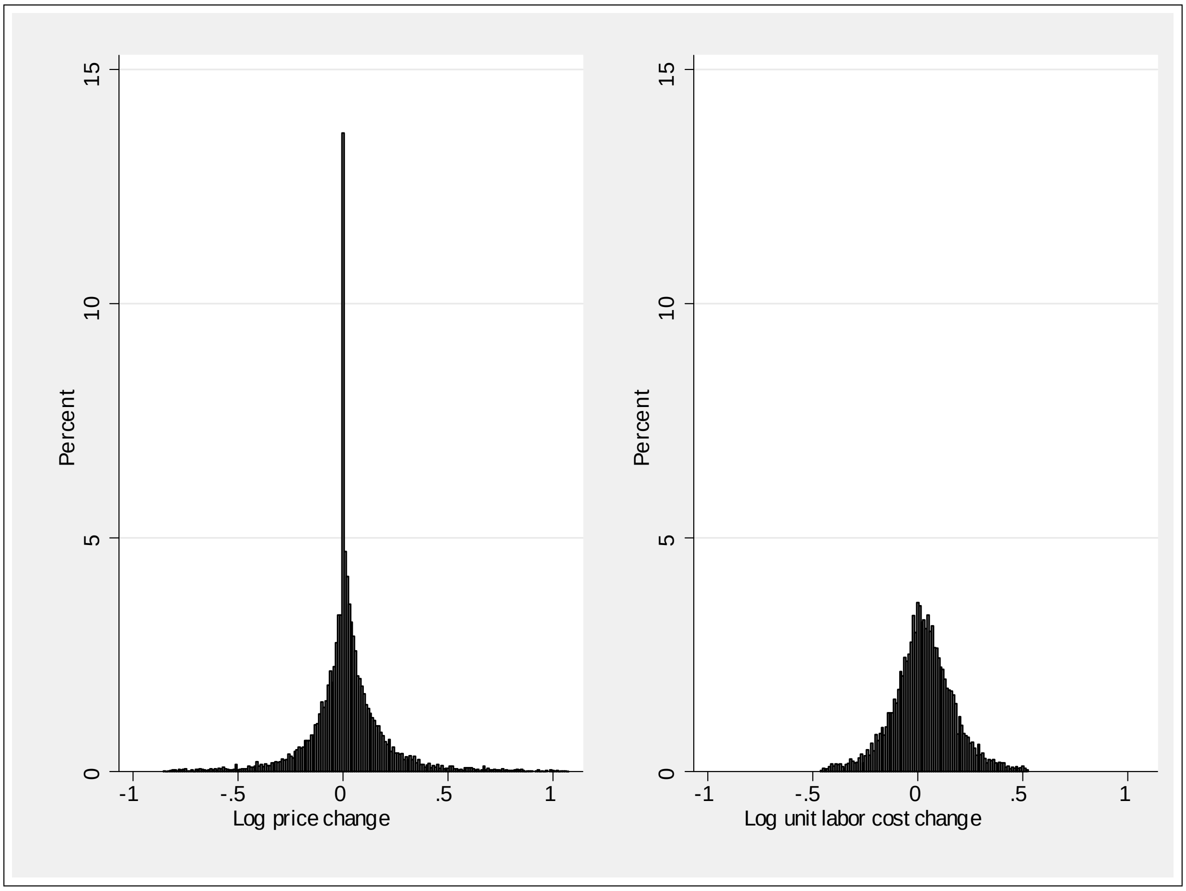

Figure 1: Histograms of data. The left-hand panel describes the distribution of log price changes across 13,772 observations (for 1,610 different products across 702 firms). The right-hand panel describes the distribution of log unit labor cost changes across 8,424 observations (for 702 firms). Bin size 0.01.

in the Eurosystem Wage Dynamics Network, Druant *et. al.* (2012) reports that about half of the firms on average change their price once a year or less. Using the spike to calculate a crude measure of the price-change probability we arrive at a monthly probability of 0.08. This is lower than the median monthly frequency of price changes of 0.108 reported by Nakamura and Steinsson (2008) for US micro producer-price data.[13] However, it is important to note that Nakamura and Steinsson (2008) reports substantial heterogeneity across sectors in the monthly frequency of price changes (from 0.013 to 0.875) and, in light of this dispersion, the finding here does not stand out as very low.[14]

In the right-hand panel of Figure 1, we plot the distribution of log changes in unit labor cost for the 702 firms (all in all 8,424 observations). As can be seen in the figure, there is no corresponding spike at the zero unit labor cost change bin.[15] The shapes of the two distributions is thus indicative of nominal price rigidities in the sense that the spike in the price change distribution is not matched with a spike in the marginal-cost change distribution.

3 A Baseline Menu-Cost Model

To obtain a benchmark for what micro-level selection effects to expect in the empirical work if the data where generated from a Menu-Cost model, we rely on a standard partial equilibrium Menu-Cost model along the lines of Golosov and Lucas (2007), which in turn builds on work by Barro (1972) and Sheshinski and Weiss (1977). The notation and presentation here is, however, closest to the Menu-Cost model outlined in Nakamura and Steinsson (2008).

As documented by Carlsson and Nordström Skans (2012), idiosyncratic variation strongly dominates any common variation in the data we use and there are no signs of bunching, or spikes, in the price-change distribution apart from the zero spike. Moreover, time dummies makes no difference for the results when estimating the probability models

[13] To our knowledge, no similar study has been performed on the Swedish micro data underlying producer-price index calculations.

[14] Vermeulen *et. al.* (2007) also report a substantial degree of heterogeneity across sectors when studying price-setting behavior on monthly micro data on producer prices for six European countries. Interestingly, the mode of the monthly price-change distribution reported by Vermeulen *et. al.* (2007) (0.09) is close to what is reported above for the average monthly price-change frequency (0.08) although, once more, the distribution shows outliers with a very high frequency of price changes.

[15] In fact, there are only three observations with exactly zero growth in marginal cost, whereas the corresponding number for price changes is 529.

discussed below. All, in all, this makes us focus on only idiosyncratic factors when trying to explain the firm-level price-change distribution. Moreover, motivated by the finding in Carlsson, Messina, and Nordström Skans (2014) for Swedish manufacturing plants that they face a downward sloping demand function, when relying on idiosyncratic variation for identification and employing the IV methodology of Foster, Haltiwanger, and Syverson (2008), we assume that the (single plant) firm operate in monopolistically competitive environment. Also, the model outlined below focuses on idiosyncratic marginal cost (or equivalently, as the model is formulated, technology) shocks as the driver of reset prices. In general, the reset price of a firm depends on factors that influences marginal cost, as well as, the optimal markup. Thus, opening up for any idiosyncratic shock that changes the scale of operations to have an effect via these two channels. Here we shut down these two channels by assuming a constant elastic demand function (implying a constant desired flex-price markup), as well as, a constant returns to scale technology (implying a flat marginal-cost schedule). These two assumptions is motivated by two sets of results. First, results from probability regressions on qualitative data (see e.g. Loupias and Sevestre, 2013), as well as surveys (see e.g. Fabiani et. all., 2006) indicate that variations in demand has a limited impact on the likelihood of changing prices. Secondly, Carlsson and Nordström Skans (2012) finds an essentially flat firm-level marginal-cost schedule in the same data as used in this paper. Similar results is also reported by Gagnon and López-Salido (2014) for U.S. data. Assuming that firm-level marginal cost is independent from any decisions taken by the firm that affects the scale of production also motivates modeling marginal cost as an exogenous process. All, in all, the assumptions made here implies that we stay close to the vast majority of the theoretical literature and, in turn, makes the model affine to the Menu-Cost model of Nakamura and Steinsson (2008).

Finally, we explicitly consider the effects of the time aggregation of our data by calibrating and simulating an underlying monthly Menu-Cost model from which we generate synthetic annual data by time aggregating the synthetic monthly data in the same way as our annual data are constructed.

3.1 The Menu-Cost Model

Let firm j produce according to the following technology

$$y_{jt} = A_{jt} L_{jt}, \tag{1}$$

where y_{jt} denotes the output of the firm in period t and A_{jt} is the labor force productivity of the firm in period t. Let firm j's product demand at time t, c_{jt}, be given by

$$c_{jt} = p_{jt}^{-\theta}, \tag{2}$$

where $p_{jt} = P_{jt}/P_t$ is the relative price of firm j and $\theta(> 1)$ is the (negative) of the price elasticity of demand. To change the nominal price, P_{jt}, κ units of labor is needed. To derive the real wage of the economy, we make a flexible price approximation as in Nakamura and Steinsson (2008). Given (2), the optimal price for the individual firm is given as a markup over nominal marginal cost

$$P_{jt} = \frac{\theta}{\theta - 1} MC_{jt}. \tag{3}$$

Marginal cost is given by

$$MC_{jt} = \frac{\partial Cost_{jt}}{\partial L_{jt}} \frac{\partial L_{jt}}{\partial y_{jt}} = \frac{\partial (W_t L_{jt})_{jt}}{\partial L_{jt}} \frac{1}{A_{jt}} = \frac{W_t}{A_{jt}}, \tag{4}$$

where W_t denotes the nominal market wage in time t. Notice thus that the last equality above together with equation (1) implies that nominal marginal cost can be measured by unit labor cost, i.e. $W_t L_{jt}/y_{jt}$. Focusing on a symmetric equilibrium, normalizing average productivity to unity, and using (3) and (4), the constant real wage in the economy is given by

$$W_t/P_t = \frac{\theta - 1}{\theta}. \tag{5}$$

Using (1), (2) and that markets clear, the firm's real profit can then be written as

$$\Pi_{jt} = p_{jt}^{-\theta} \left(p_{jt} - mc_{jt}\right) - \kappa \left(\frac{\theta - 1}{\theta}\right) I_{jt}, \tag{6}$$

where mc_{jt} is the real marginal cost of firm j, and I_{jt} is an indicator that takes the value one if the nominal price is changed, i.e. $P_{jt} \neq P_{jt-1}$, and zero otherwise. The log of real marginal cost follows an AR(1) process

$$\ln mc_{jt} = \lambda + \rho \ln mc_{jt-1} + \epsilon_{jt}, \tag{7}$$

where $\lambda = (1-\rho)\log((\theta-1)/\theta)$ so that the expectation of long-run real marginal cost converges to the real wage. Moreover, $\epsilon_{jt} \sim \mathit{Laplace}(0, \sigma_\epsilon/\sqrt{2})$, implying a standard deviation of ϵ_{jt} equal to σ_ϵ. The assumption of a Laplace distribution (with fatter tails than a normal distribution) is motivated by the non-normal shape of the observed annual marginal cost change distribution (when controlling for time dummies the kurtosis (skewness) coefficient equals 3.95 (0.01) and a standard test (D'Agostino, Belanger and D'Agostino, 1990) rejects the null of normality on the one-percent level due to the relatively high kurtosis). This assumption is also in line with the fat-tails assumption of Midrigan (2011). The log of the price level drifts with the rate μ[16]

$$\log P_t = \mu + \log P_{t-1}. \tag{8}$$

Assuming that the firm discounts profit streams at a constant rate β and denoting the relative price the firm enters the period with as $p_{jt}^{-} = P_{jt-1}/P_t$, the value function of firm j can be written as

$$V(p_{jt}^{-}, mc_{jt}) = \max_{P_{jt}} [\Pi_{jt} + \beta E_t V(p_{jt+1}^{-}, mc_{jt+1})], \tag{9}$$

where E_t is the expectations operator. Following Nakamura and Steinsson (2008) we solve this problem by value-function iterations on a grid, using the method of Tauchen (1986) to approximate the mc_{jt} process.[17]

The optimal decision rule, solving this problem, implies a region of inaction where the accumulated change in marginal cost is too small to warrant any adjustment in

[16] Nakamura and Steinsson (2008) models the log of the price level to follow a random walk with drift. Adding an i.i.d. normally distributed shock to (8) calibrated to match the monthly PPI series does not change the results to any noticeable degree and we leave it out of the exercise presented here.

[17] Since the model presented here is just a slightly rewritten version of the model in Nakamura and Steinsson (2008) we rely heavily on their MATLAB code available at http://www.columbia.edu/~js3204/papers/MenuCostModelCode.zip.

the price given the size of the fix adjustment cost. However, when the accumulated change in marginal cost is large enough to cross the adjustment threshold, the firm will adjust the price. Thus, running a probability model with I_{jt} as the dependent variable and the accumulated (log) change in marginal cost will yield a positive slope parameter, reflecting that firms self-select into price changes. There are however two degenerate cases, were the menu cost is either zero (implying a completely flexible pricing-model without selection effects) or the menu cost is high enough to rule out any price adjustments for a given marginal cost process (implying a completely rigid model with no selection effects), which would yield a zero slope parameter. To rule out these two uninteresting cases it is important to condition the model on matching other key moments in the data, as done below.

In contrast, the Calvo (1983) model implies a zero slope in the probability model outlined above since the firms that are allowed to costlessly adjust their price are drawn independently of their marginal cost developments.

The prediction of a positive slope parameter in the probability model outlined above for the canonical Menu-Cost model is shared with many other recent advances of price-setting models and it thus serves as natural benchmark model for the analysis in this paper. In fact, any price-setting models featuring a region of inaction in price setting, generated by a menu-cost or some other mechanism such as e.g. trembling-hand price setters as in Costain and Nakov (2015), will share this prediction. Moreover, the multi-product extension of Midrigan (2011) and Karadi and Reiff (2016) also predicts a positive slope parameter.[18] In this sense, the empirical analysis estimating the slope parameter in the probability model also encompasses these models.

Also, testing the prediction of a positive slope parameter in the probability model also has bearing on the recent debate wether or not self-selection can generate near non-neutrality to nominal shocks as in Golosov and Lucas (2007) and Karadi and Reiff (2016) or not as in Midrigan (2011). The overall importance of selection effects for monetary non-neutrality is driven by the interaction of the measure of marginal firms lying close

[18] The multi-product extension of Midrigan (2011) and Karadi and Reiff (2016) with separate, but correlated marginal-cost process, implies that price-changes are correlated with high menu-cost changes, which is what is needed to predict a positive slope parameter in the probability model. Aggregating the marginal-cost measure to a firm-wide measure, as in the empirical analysis below, does not change this conclusion. As a robustness exercise, we also present empirical results for single-product firms in Appendix D.

Table 1: Menu-Cost Model Calibration

	Parameter	Value
μ	Inflation Drift	0.00138
β	Discounting	$0.96^{1/12}$
θ	Price elasticity of demand	3
ρ	Real marginal cost persistence	0.921
σ_ϵ	S.D. real marginal cost shock	0.0676
$\frac{\kappa(\theta-1)}{\theta}$	Menu Cost	0.0791

to the adjustment threshold and the size of the adjustment needs; see Karadi and Reiff (2016) for a discussion. Importantly, though, a prerequisite for any such mechanism to be able to generate near non-neutrality, as in Golosov and Lucas (2007), Karadi and Reiff (2016), is that firms' self-select into price changes in the first place, which is the focus of the analysis below.

3.2 Monthly Calibration

To calibrate the model, we first estimate the drift parameter of the inflation process to (μ) to 0.00138 using monthly data on the Swedish industrial producer-price index for the period 1990:1 to 2002:12. This implies an annualized average inflation rate of 1.7 percent, which is very close to the annual mean price change in the data (1.8 percent). We set $\beta = 0.96^{1/12}$ to generate an annualized real interest rate of about 4 percent. We set $\theta = 3$ which is in line with the firm-level estimate for the Swedish manufacturing sector reported in Carlsson, Messina, and Nordström Skans (2014).

To calibrate the remaining parameters, we set ρ, σ_ϵ and κ so as to match the annual data in terms of (i) the persistence of log real marginal cost estimated in Carlsson and Nordström Skans (2012) (0.542), (ii) the standard deviation of the log real marginal cost change distribution (0.145) and (iii) the size of the zero bin in the log price change distribution (0.136). The statistics for real marginal cost variables derived from the unit labor cost data controls for time fixed effects.[19] This procedure removes any aggregate or common factors (including deflating the nominal data).

As noted above, the prices are calculated from reported values and volumes of sold

[19]The estimate of the annual persistence of log real marginal cost in Carlsson and Nordström Skans (2012) actually controls for time interacted by two-digit sector code (NACE). Using this procedure for the standard deviation of the log real marginal cost change distribution yields a very similar estimate to what is used here (0.142 vs.145).

products, which will lead to small rounding errors. In contrast, there are no measurement errors in the synthetic data from the model. This difference motivates calibrating the model to match the zero bin rather than to the share of observation that are exactly zero in the data. That is, as long as any measurement error is small enough to be confined within the zero bin, misclassification should not matter for the moment-matching exercise. Also, judging from the continuous shape of the log price change distribution on both sides surrounding the zero bin, there is no reason to believe that a wider band than the zero bin should be warranted.

3.3 Time Aggregation

To match annual statistics, we time-aggregate the monthly data using monthly output weights consistently with the annual data we observe. The annual unit price of firm j is constructed as

$$\begin{aligned} P_{jt} &= \frac{Annual\ Sales_{jt}}{Annual\ Volume_{jt}} = \frac{\sum_m P_{jt}^m Y_{jt}^m}{\sum_m Y_{jt}^m} = \\ &= P_{jt}^1 \frac{Y_{jt}^1}{\sum_m Y_{jt}^m} + ... + P_{jt}^{12} \frac{Y_{jt}^{12}}{\sum_m Y_{jt}^m}, \end{aligned} \tag{10}$$

where m denotes month. Similarly we can write

$$\begin{aligned} ULC_{jt} &= \frac{Annual\ Wage\ Bill_{jt}}{Annual\ Volume_{jt}} = \frac{\sum_m W_{jt}^m L_{jt}^m}{\sum_m Y_{jt}^m} = \\ &= \frac{W_{jt}^1 L_{jt}^1}{Y_{jt}^1} \frac{Y_{jt}^1}{\sum_m Y_{jt}^m} + ... + \frac{W_{jt}^{12} L_{jt}^{12}}{Y_{jt}^{12}} \frac{Y_{jt}^{12}}{\sum_m Y_{jt}^m} = \\ &= ULC_t^1 \frac{Y_t^1}{\sum_m Y_t^m} + ... + ULC_t^{12} \frac{Y_t^{12}}{\sum_m Y_t^m}, \end{aligned} \tag{11}$$

which motivates the use of monthly output weights.

The full calibration is presented in Table 1 and implies that the model needs a sizable menu cost, about 23 percent of the average monthly real gross profits, in order to match annual moments.[20]

[20] That is the ratio of $\kappa(\theta - 1)/\theta$ and the average of $p_{jt}^{-\theta}\,(p_{jt} - mc_{jt})$ in the simulated monthly data.

4 Results

In this section we first present the simulation results for the Menu-Cost model calibrated to key data moments. We then compare the empirical strength of the selection effects in the micro data to what is expected from the Menu-Cost model, outlined above, using probability regression methods. We also discuss whether these results can be interpreted as true selection effects. In a final step, we structurally quantify the regression results in a model that can generate an arbitrary degree of selection effects in the simulated data (i.e. the CalvoPlus model of Nakamura and Steinsson, 2010).

4.1 Simulation Results

In Figure 2 we plot the monthly log price/marginal cost change distributions for $100,000$ simulated monthly observations. For clarity we have omitted the spike at zero which contains 92 percent of the observations. Here we see that the high menu cost generates the usual price change distribution with no mass in a region around zero price adjustment.

In Figure 3 we plot the observed and the simulated annual data from the model, focusing on the interval $[-0.5, 0.5]$ log points. A first observation is that the log marginal cost change distribution is well replicated from the simulation. In terms of the similarity of the dispersion of the distributions this is no big victory since the standard deviation of the log real marginal cost change distribution is a target moment when fitting the model combined with a constant inflation rate in the model. Importantly, however, the kurtosis of the actual data (3.82) is not far from that of the simulated distribution (3.24). Turning to the log price change distribution, a key observation is that we find no regions of inaction in the time aggregated synthetic data, although we do see some difference in the observed log price change data and the time-aggregated synthetic data in that there is a lack of mass around the spike at the zero bin. Moreover, the simulated distribution is not dispersed enough, the observed/simulated standard deviations are 0.19 vs. 0.13 and the kurtosis of the actual data (8.62) is much higher than that of the simulated distribution (3.39). However, time aggregation gives a lot of mileage in replicating the observed log price change distribution with a stylized Menu-Cost model and provides a complementary mechanism for generating small price changes in the Menu-Cost model to the economies of scope suggested by Lach and Tsiddon (2007), Midrigan (2011) and

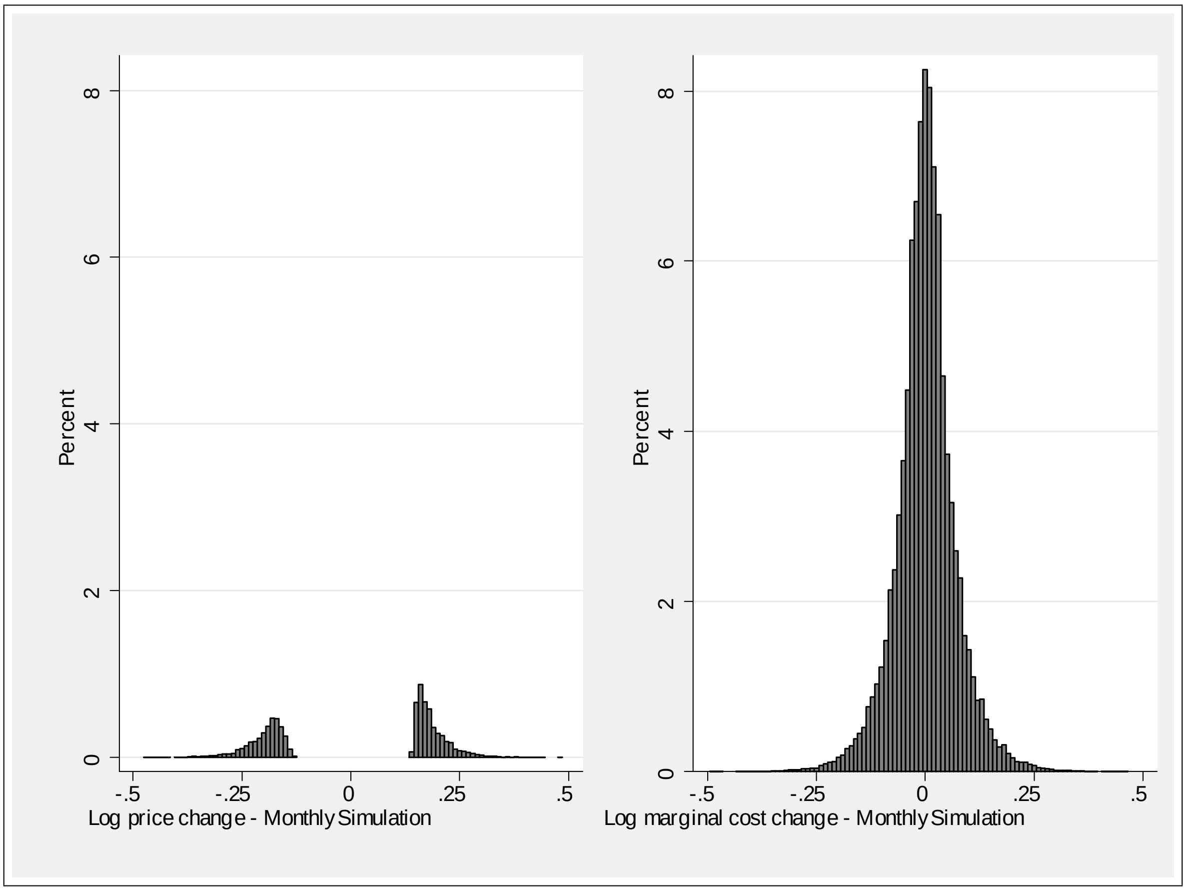

Figure 2: Histograms of simulated monthly data from the Menu-Cost model. The log price change distribution (left panel) omits the zero bin.

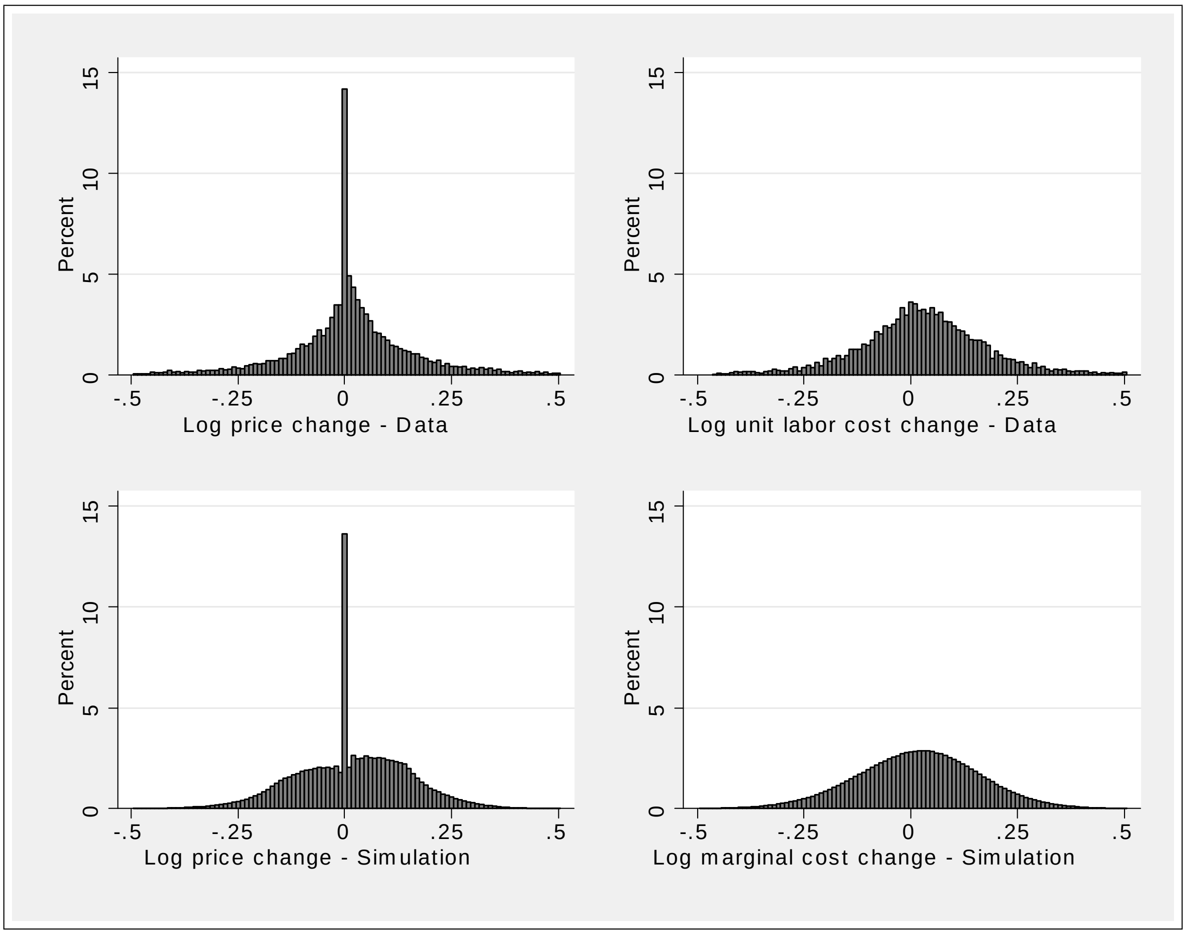

Figure 3: Histograms of actual annual data (top panel) and simulated data from the Menu-Cost model time aggregated to the annual frequency (bottom panel). Bin size 0.01.

Alvarez and Lippi (2014) or stochastic menu costs as in Caballero and Engel (1999) and Dotsey, King, and Wolman (1999). Intuitively, pricing patterns where e.g. large positive and negative monthly changes within a year nearly cancel one another out generates small overall price movements in the time-aggregated data.[21] Though, arguably the strength of the mechanism should increase with the time span of aggregation, but the potential for this mechanism is shared with many other data sets employed in the literature. In scanner data e.g., the price is seldom recorded, but instead calculated from reported values and volumes of sold products, as pointed out by Eichenbaum, Jaimovich, Rebelo, and Smith (2014). One example of this is the data used in Eichenbaum, Jaimovich, and Rebelo (2011). This is the same method as used in this paper to measure the price, although in scanner data it is done over a shorter time span like a week or a month. Evaluating the importance of this mechanism in data time aggregated data with higher frequency is left for future research, but we note that, any rebuttal of the Menu-Cost model using the data set at hand will not be due to its inability to capture the frequency of small price changes in the observed data.

4.2 Probability Regression Results

To compare the relative strength of the selection effects in the Menu-Cost model vs. the data, we run price-change probability regressions as outlined in Section 3.1, and as previously done by Cecchetti (1986), and later by e.g. Buckle and Carlson (2000), Loupias and Sevestre (2013) and others. Due to data limitations these papers have to rely on aggregate/sectoral or qualitative data to measure drivers of the reset price. Here, instead we can compute a quantitative firm-specific measure of marginal cost change.

We first define an indicator for price changes outside the zero bin as

$$I_{gt}^{OZB} = \begin{cases} 1 \text{ if } (|d \ln P_{g,t}| > 0.005) \\ 0 \text{ otherwise} \end{cases} , \tag{12}$$

where $P_{g,t}$ denote the price of good g (produced by firm j) at time t. Next, we regress the absolute value of the accumulated change in (log) marginal cost change ($|d^s \ln MC_{j,t}|$), where d^s denotes the accumulated change since the last price change, on this indicator,

[21] Note also that other price-change patterns can give rise to small price changes in time-aggregated data. For example, a price change early in the first period followed by a constant price gives rise to a small time-aggregated price change.

i.e.

$$I_{gt}^{OZB} = \gamma_0 + \gamma_1 \left| d^s \ln MC_{j,t} \right| + \eta_{gt}, \tag{13}$$

where γ_0 and γ_1 are coefficients to be estimated and η_{gt} is a goods-specific error term. That is we run a linear probability model to try to determine whether or not movements in the forcing variable of the reset price (i.e. the accumulated marginal costs change since the last price change) have an impact on the price-change probability, or in other words, the timing of the price change. To account for the fact that $|d^s \ln MC_{j,t}|$ varies on the firm level and not the goods level we correct the standard errors by clustering on the firm level, which handles any type of error-term dependence within the firm over time. Also, in Appendix D we show that the findings are robust to only relying on single-product firms. This result does not only speak to the issue of classical measurement errors, but also implies that any differences in observed pricing patterns between single- and multi-product producers, emphasized in empirical work by Bhattarai and Schoenle (2014), and implied by the multi-product extension of the menu-cost model by Midrigan (2011) and Karadi and Reiff (2016) are not important for the overall conclusions of this paper.

Looking at a small band around zero (instead of the zero point) in the price change distribution is very useful when relying on annual data since it increases the variation in the dependent variable and also renders potential misclassification of small price changes a non-issue for the results when comparing the model to the data. Note, however, that this estimate is likely to be an upward-biased estimate of the true selection effects, since absent any such effects we are still likely to obtain a positive estimate. This is because even in the Calvo (1983) model small price changes (within the band) are associated with small accumulated marginal cost changes.[22] Here, the main focus is to evaluate the structural model with respect to fitting data moments and for this purpose this bias does not matter since it should also be captured by the model. Below, however, we will try to evaluate the size of this potential bias in the regression model.

In Table 2, we present summary statistics of the data used in the probability regressions. In the top panel of Table 2 we see that the mean of I_{gt}^{OZB} (0.884) in the regression sample indicate that we have 11.6 percent of the observations in the zero bin and that

[22] Or, in other words, if we erroneously redefine observations in the dependent (dummy) variable to zero that at the same time have values on the independent variable that are below its mean, the estimate of the slope parameter from the probability model will be upward-biased.

Table 2: Summary Statistics of Regression Data

Variable	Obs	Mean	Std. Dev.	Min	Max
I_{gt}^{OZB}	$9,694$	0.884	0.320	0	1
$\lvert d^s \ln MC_{jt}\rvert$	$9,694$	0.104	0.138	0	0.694
I_{gt}^{OZB}	$13,772$	0.864	0.343	0	1
$\lvert d \ln MC_{jt}\rvert$	$13,772$	0.105	0.091	0	0.521

Note: $|d^s \ln MC_{j,t}|$ and $|d \ln MC_{jt}|$ are weighted as in the regressions.

there is a sizable variation in $|d^s \ln MC_{jt}|$ (s.d. of 0.104). However, since we cannot start computing the accumulated change since the last price change until we actually observe a price change in the previous period, we lose $4,078$ observations relative to the full sample of price and marginal-cost changes. This is also a reason for running regressions on the absolute value of marginal cost change, $|d \ln MC_{jt}|$, (i.e. without any accumulation) where we can use the full sample of $13,772$ price changes. Although less directly interpretable from theory, the Menu-Cost model also has comparable predictions in this dimension of the data. In the bottom panel of Table 2 we present the summary statistics for this version of the regression model. As can be seen in the table, there is a slightly higher share of the observations in the zero bin (13.6 percent - as in the price-change distribution in Figure 1), but a slightly lower, but still sizable, variation in the explanatory variable $|d \ln MC_{jt}|$ (s.d. of 0.091) as also reflected in the log unit labor cost change distribution of Figure 1.

In the first column of the top panel of Table 3 we present the results from running the linear probability model as outlined in (13). The estimated marginal effect is 0.071 (s.e. 0.05) and statistically insignificant on the five-percent level. Also, the point estimate indicate a very small effect, a standard deviation change in $|d^s \ln MC_{jt}|$ implies only a 1 percentage point higher probability of the firm changing price. This should be compared to the results from doing the same exercise on simulated and time-aggregated data from the Menu-Cost model presented in the first column in the bottom panel of Table 3. Here, we use the monthly Menu-Cost model to generate panels of simulated, time-aggregated annual data on price and marginal-cost changes consisting of $3,510$ price identities (as in the data) observed for five years (the average number of observations per price identity is 4.92 years in the data). The average estimate of the linear probability model across 200

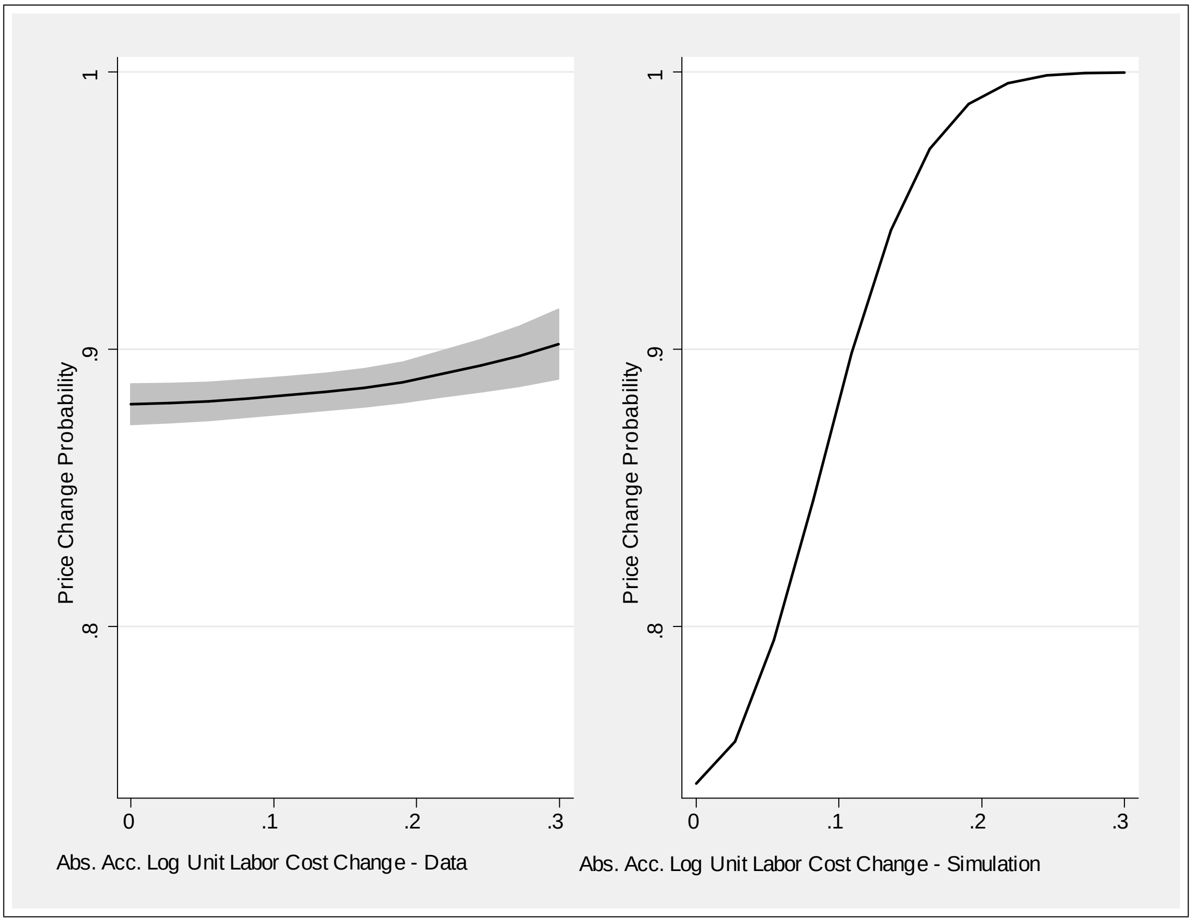

Figure 4: Kernel regressions of price-change dummy on the absolute accumulated change in log marginal cost. The left-hand panel present results from data. Gray area depicts the 95-percent confidence band. The right-hand panel presents results from simulated data from the Menu-Cost model.

Table 3: Estimation and Simulation Results

	(1)	(2)	(3)
		Data	
$\lvert d^s \ln MC_{jt}\rvert$	0.071		
	(0.050)		
$\lvert d \ln MC_{jt}\rvert$		0.129^{*}	0.114^{*}
		(0.053)	(0.053)
$\lvert d \ln MC_{jt-1}\rvert$			-0.014
			(0.072)
	Simulation - Menu-Cost Model		
$\lvert d^s \ln MC_{jt}\rvert$	0.959^{**}		
	[0.032]		
$\lvert d \ln MC_{jt}\rvert$		1.076^{**}	1.067^{**}
		[0.031]	[0.033]
$\lvert d \ln MC_{jt-1}\rvert$			0.308^{**}
			[0.035]

Notes: Dependent variable takes on a value of one if the price change is outside the zero bin and zero otherwise. Data panel: Superscript **, * and + denote estimates significantly different from zero at the one-, five- and ten-percent level, respectively. Robust standard error clustered on the firm level is inside the parenthesis. The number of observations (by columns) is 9,694, 13,772 and 12,292, respectively. Simulation panel: The coefficent denotes the average across 200 panel simulations. The standard deviation of the point estimate across 200 panels is inside the square bracket.

simulated panels is presented in the first column in the bottom panel of Table 3 together with the standard deviation of the point estimate across all repetitions. As can be seen from the table the point estimate does not move much across simulations and the mean, 0.96, is more than 13 times larger than found in actual data, implying that a standard deviation increase in $|d^s \ln MC_{f,t}|$ should increase the probability of price adjustment by 13.2 percentage points points. Another way to see the sharp difference between the data and the model predictions is depicted in Figure 4, where kernel regressions are used to illustrate the dramatic discrepancy between the relationship in the data (left hand panel) and the predicted relationship from the Menu-Cost model (right-hand panel).[23]

In the second column of the top panel of Table 3, the results from using the non-accumulated absolute change of log marginal cost as the driver of price-changes are presented. The estimated marginal effect in this case is 0.13 (s.e. 0.05) and statistically significant on the five-percent level. Thus, taking the estimate at face value and disregarding any biases, this result indicate the presence of selection effects in the sense that

[23]In Appendix C we also present the results of running a kernel-regression on the accumulated log marginal cost change distribution, but without taking the absolute value. This gives rise to a slightly U-shaped relationship where both ends of the kernel behaves as expected.

the timing of the pricing decision is state-dependent. However, in an economic sense, the estimate is still very small and comparable to when using absolute accumulated changes; a standard deviation change in $|d \ln MC_{jt}|$ implies only a 1.2 percentage points higher probability of the firm changing price. Moreover, as compared to the bottom panel, the Menu-Cost model predicts an eight times higher effect.

In column 3 of Table 3 we also include lagged changes in marginal cost, i.e. $|d \ln MC_{jt-1}|$. In a Menu-Cost model we would also expect lagged changes to matter due to pent-up adjustment incentives (otherwise captured in the accumulation of changes). As can be seen in the second column of the bottom panel of Table 3, this prediction is confirmed in the simulated and time-aggregated data with a mean point estimate of 0.31 (s.d. of 0.03) on $|d \ln MC_{jt-1}|$. However, we do not see this effect in the observed data. The point estimate is very close to zero -0.01 (s.e. 0.07) and naturally statistically and economically insignificant.

Importantly, the sharp difference between the (time aggregated) predictions of the Menu Cost model and the empirical results presented in Table 3 and Figure 4 dispels any concerns that time aggregation and the use of annual data may drive the inference in this exercise.

Appendix B present evidence of that the conclusions are robust to using Probit and Logit estimators instead of the linear probability model and also to controlling for a variety of real-world features not included in the model such as time dummies, which control for any common variation and firm-fixed effects, which control for any heterogeneity in average price-change probabilities across firms, as well as the combination of the latter two. Thus, across models, we get the same message that the timing adjustments of price changes in response to marginal-cost developments do not seem to be an important feature of observed price-setting behavior of goods-producing firms. Moreover, in Appendix C we present evidence of that even the small positive point-estimates found here is likely to be due to the upward bias discussed above. Next, however we turn to a structural evaluation of these regression results, which can be done regardless of the presence of any bias in the regression results.

4.3 Structural Evaluation Results - The CalvoPlus Model

As noted above, the Menu-Cost model generates selection effects that are much too strong. The question then is whether or not they are so small that a Calvo (1983) model would fit the data or if the data is better described by hybrid model including elements of both. In order to structurally quantify the selection effects implied by the regression results above, we fit a price-setting model that nests both the Calvo (1983) and the Menu-Cost model and thus can generate an arbitrary degree of selection effects. To this end we use the CalvoPlus model outlined in Nakamura and Steinsson (2010). As compared with the Menu-Cost model outlined in section 3, the firms now get an opportunity with probability $(1-\alpha)$ to change price at a low cost κ_L, and to a high cost κ_H otherwise. Thus, this model nests the standard Calvo (1983) model with $\kappa_L = 0$ and $\kappa_H \to \infty$, as well as the baseline Menu-Cost model presented above with $\alpha = 1$ (or 0) or $\kappa_L = \kappa_H$.

The firm's real profit in the CalvoPlus economy can be written as

$$\Pi_{jt}^{CP} = p_{jt}^{-\theta}\left(p_{jt} - mc_{jt}\right) - \left(\kappa^L\left(1 - I_{jt}^H\right) + \kappa^H I_{jt}^H\right)\left(\frac{\theta-1}{\theta}\right) I_{jt}, \tag{14}$$

where I_{jt}^{High} is an indicator that takes on the value one if the the firm faces the high menu cost and zero otherwise. The value function can be written as,

$$V^{CP}(p_{jt}^{-}, mc_{jt}, I_{jt}^H) = \max_{P_{jt}}[\Pi_{jt}^{CP} + \beta E_t V^{CP}(p_{jt+1}^{-}, mc_{jt+1}, I_{jt+1}^H)], \tag{15}$$

where

$$I_{jt+1}^H \sim Bernoulli(\alpha), \tag{16}$$

and subject to the processes (8) and (7) above. We solve the model by value function iterations on a grid, using the method of Tauchen (1986) to approximate the mc_{jt} process.[24]

To fit this model, we again set $\mu = 0.00138$, $\beta = 0.96^{1/12}$ and $\theta = 3$. To keep computations feasible we set ρ and σ_ϵ to the same values as for the Menu-Cost model. The remaining parameters, κ_H, κ_L and α are set so as to minimize the criterion function $\mathbf{M'M}$ where

[24] Again, we rely heavily on the MATLAB code written by Emi Nakamura and Jon Steinsson available at http://www.columbia.edu/~js3204/papers/MenuCostModelCode.zip.

Table 4: CalvoPlus Model Calibration

	Monthly Calibration		
	Parameter	Value	
μ	Inflation drift	0.00138	
β	Discounting	$0.96^{1/12}$	
θ	Price elasticity of demand	3	
ρ	Real marginal cost persistence	0.921	
σ_ϵ	S.D. real marginal cost shock	0.0676	
$\frac{\kappa_H(\theta-1)}{\theta}$	Menu cost (High State)	4.733	
$\frac{\kappa_L(\theta-1)}{\theta}$	Menu cost (Low State)	0.000153	
α	Calvo probability	0.892	
	Annual Moments Match		
	Moment	Model	Data (S.E.)
	Persistence of log real marginal cost	0.544	0.542 (0.042)
	S.D. log real marginal cost change distribution	0.143	0.145 (0.002)
	Price spike $\bar{I}^{IZB}$	0.135	0.136 (0.008)
	Parameter $\lvert d\ln MC_{jt}\rvert$	0.173	0.114 (0.053)
	Parameter $\lvert d\ln MC_{jt-1}\rvert$	0.122	-0.014 (0.072)

Note: Robust standard error clustered on the firm-level within parenthesis in the moments-match panel.

$$\mathbf{M} = \begin{bmatrix} (\bar{I}^{IZB}_{Model} - \bar{\bar{I}}^{IZB}_{Data})/\sigma(\bar{\bar{I}}^{IZB}_{Data}) \\ (\gamma_{1,Model} - \gamma_{1,Data})/\sigma(\gamma_{1,Data}) \\ (\gamma_{2,Model} - \gamma_{2,Data})/\sigma(\gamma_{2,Data}) \end{bmatrix}, \tag{17}$$

and $\bar{I}^{IZB}$ is the average of $1 - I^{OZB}_{gt}$ and $\gamma_{1,Data}$ and $\gamma_{2,Data}$ denote the coefficients on contemporaneous and lagged $|d\ln MC_{jt}|$, respectively, presented in column 3 of the top panel of Table 3, which is used since we need two additional moments to match the model to.[25] Finally, σ denotes the standard errors of the observed data moments (clustered on the firm level).[26] The resulting parameter values, as well as observed and synthetic data moments, for the CalvoPlus model are presented in Table 4. The data wants a menu-cost setup that is in line with the standard Calvo (1983) model with a very high menu cost in the high cost state (about 14 months of average monthly real gross profits) and a very low menu cost in the low cost state (about 22 minutes of average real gross profits for a continuously operating firm). In fact, setting $\kappa_L = 0$ and $\kappa_H = 150$ in the CalvoPlus

[25]Note that the Menu-Cost model could be calibrated to exactly match the data moments used for that model. Thus, any sensible weighting of the moments would return the same parameters.

[26]To find the minimum of the weighted squared deviations we use a combination of a global minimization method (the ga algorithm in MatLab), to rule out local minimums, and a simplex method (fminsearch in MatLab). To make computations feasible, the number of grid points for the state space as well as the number of simulated panels of firms is gradually increased in this process.

model gives rise to nearly identical results for the model to those presented in the bottom panel of Table 4. Thus, this exercise speaks against any important selection effects in the data or that the timing behavior with respect to price changes of these firms imply any potential for reducing monetary non-neutrality as in Golosov and Lucas (2007) or Karadi and Reiff (2016). Moreover, the data wants a Calvo parameter, $\alpha = 0.89$, that is not too far from estimates from macro-data studies. Adolfson, Laséen, Lindé, and Villani (2008) present a quarterly estimate of α of 0.84 using Swedish data, which translates into a monthly Calvo parameter of 0.94. Moreover, Carlsson and Nordström Skans (2012) presents estimates of 0.562 (s.e. of 0.165) on current marginal cost and 0.364 (s.e. of 0.154) on expected future marginal cost when estimating the first-order condition for pricing in the standard Calvo (1983) model on the same data as used in this paper. Interestingly, solving for these coefficients using the first-order condition from the Calvo (1983) model and setting $\alpha = 0.89$ and $\beta = 0.96^{1/12}$ yields expected coefficients of 0.763 on current marginal cost and 0.181 on expected future marginal cost, which is well within the 95-percent confidence interval of the reduced form estimates.[27]

In the bottom panel of Table 4 the model moments are compared to their targets in the annual observed data (with standard errors clustered on the firm level). Although the model is not able to exactly match the targets, it does a good job when considering the confidence bands for the observed moments and notably so when it comes to replicating the regression estimates as compared to the coefficients obtained from the canonical Menu-Cost model. Next, in Figure 5, we plot the implied annual log price/marginal change distributions and compare them to both the observed data and the simulated data from the Menu-Cost model. As compared to the dispersion generated by the Menu-Cost model (s.d. of 0.13), the dispersion of the simulated log price-change distribution (s.d. of 0.08) is actually further away from the observed dispersion (s.d. of 0.19). However, what is clear from the figure is that the CalvoPlus model is better at capturing the high kurtosis observed in the data (8.62) and the overall shape of the log price change distribution The kurtosis of the log price change distribution of the CalvoPlus model is 4.71 as compared to 3.39 from the Menu-Cost model. Importantly, the results presented here support the view that the CalvoPlus model provides a sensible basis for a structural

[27] These coefficients are given by $(1-\alpha\beta) \cdot \sum_{m=0}^{11} (\alpha\beta)^m$ and $(1-\alpha\beta) \cdot \sum_{m=12}^{23} (\alpha\beta)^m$, respectively (see, e.g., equation (8) in Carlsson and Nordström Skans, 2012).

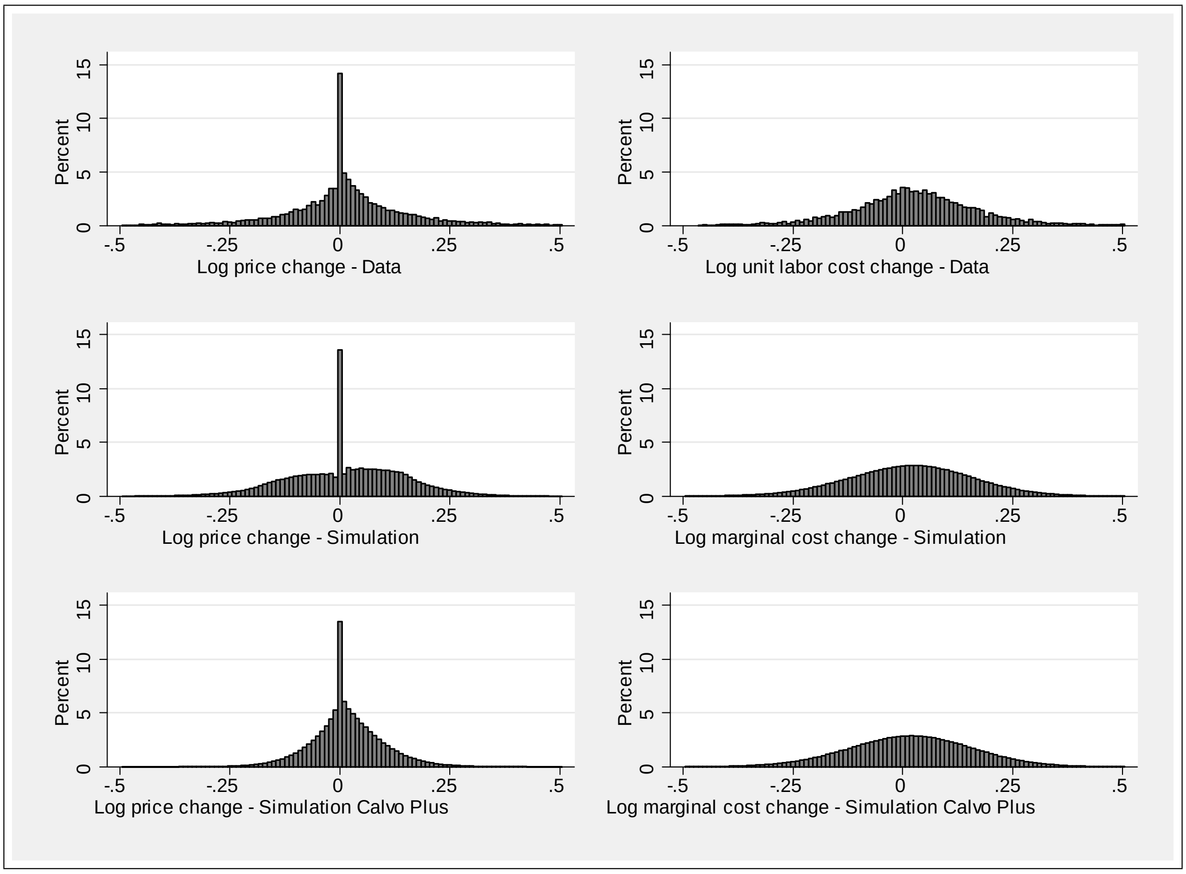

Figure 5: Histograms of actual data (top panel), simulated data from the Menu-Cost model (middle panel) and simulated data from the CalvoPlus model (bottom panel). Bin size 0.01.

investigation of the data. Note, however, that by this is not meant that the matched CalvoPlus model, relying on enormous costs of price change in 89 percent of the months, is literary a good model of the microfoundations of price setting. But as a short-hand for some more realistic model featuring very small selection effects in response to marginal cost shocks it does a good job in replicating the observed price-change distribution in upstream firm-level data.

5 Concluding Discussion

We use detailed Swedish micro data on product producer prices linked to a detailed data set containing information on the firms that set these prices to test the empirical relevance of selection effects in micro-level producer pricing. To impose discipline on the empirical exercise at hand, we first outline and calibrate a baseline Menu-Cost model to match key moments in the data. The Menu-Cost model we rely on is along the lines of Golosov and Lucas (2007), but allows for fat-tailed idiosyncratic shocks to marginal cost (akin to Midrigan, 2011) in order to better match the micro-data. Moreover, the model is calibrated to a monthly frequency, which allows us to gauge the effect of time aggregation in the annual data, both in terms of observed moments, as well as, in terms of the power of the inference. Aggregating the data the same way as actual data is aggregated, we find that time aggregation gives a lot of mileage in replicating the observed price change distribution with a stylized Menu-Cost model. This is because the time aggregation filter fills out the gap of small price changes otherwise expected in the price-change distribution from an Menu-Cost model. Thus, time aggregation is a complementary mechanism for generating small price changes in the Menu-Cost model to the other mechanisms proposed in the literature. Intuitively, price patterns where e.g. large positive and negative monthly changes within a year nearly cancel one another generates small price movements in the time-aggregated data. Though, arguably the strength of the mechanism should increase with the time span of aggregation, but the potential for this mechanism is shared with many other data sets employed in the literature where the price is calculated from reported values and volumes over a time period as in this paper; see Eichenbaum, Jaimovich, Rebelo, and Smith (2014) for a discussion. Evaluating the importance of this mechanism in data time aggregated data with higher frequency is left for future research, but we note that, any rebuttal of the Menu-Cost model using the data set at hand will not be due to its inability to capture the frequency of small price changes in the observed data.

To analyze the strength of the selection effects we investigate if the absolute accumulated value of the change in the firm's marginal cost, as well as a non-accumulated version of the same, affects the probability of a price change. A test of the Menu-Cost model is then provided by comparing the empirical results with the results from synthetic time-aggregated data generated by the Menu-Cost model matched to key moments in the data. We find much smaller effects on the probability of a price change than we would expect in the Menu-Cost model. Moreover, when considering measurement issues pertaining to the classification of small price changes in the data, the (small) positive estimates we find seems to be the result of upward bias. Importantly, the sharp difference in predictions and results also confirms that time aggregation is not an issues for the inference in this exercise.

To structurally quantify the regression results we also fit a price-setting model that nests both the Calvo (1983) and Menu-Cost model to the data (i.e. a fat-tailed shocks version of the CalvoPlus model outlined in Nakamura and Steinsson, 2010), which can generate an arbitrary degree of selection effects in the simulated micro data from the model. Importantly, the procedure to fit the model parameters can be constructed to be unaffected by the measurement issues that may bias the regression results. When choosing parameters so that the model matches empirical moments as closely as possible, the parameters are driven very close to a Calvo (1983) model. This suggests, in agreement with the previous results, that selection effects are not being an important feature of the data.

Thus, overall, timing adjustments of price changes in response to marginal-cost developments do not seem to be an important feature of observed price-setting behavior of goods-producing firms, which, in turn, speaks against the mechanism reducing monetary non-neutrality emphasized in Golosov and Lucas (2007) or Karadi and Reiff (2016). Note though that it is not argued that the Calvo (1983) model is the true underlying model of micro-level price setting, but rather that in order to be aligned with the data, any successful model of price setting in firms upstream in the supply chain needs to predict low selection effects.

Interestingly, Eichenbaum, Jaimovich, and Rebelo (2011) also link a measure of marginal cost, i.e. the replacement cost of the vended product, to the price set in data drawn from a large US food and drug retailer (downstream in the supply chain) and documents a high degree of selection effects in pricing. This indicates considerable differences in pricing behavior along the supply chain.[28] This is perhaps not surprising given differences in conditions between consumer and business-to-business markets, but it may provide important leads for future research on the microfoundations of pricing behavior.

A related question of interest is how upstreams marginal-cost shocks feeds through the pricing decision along the supply chain (see e.g. Nakamura, 2008). Although this issue is not addressed here, it is worth noting that in a simple supply chain model, where the price set by the upstreams firm represent marginal cost for the downstream firm, does not need price-setting frictions on all levels in order to generate significant monetary non-neutrality.[29] In fact, frictions downstream can only add to monetary non-neutrality and given the results presented here, they are not instrumental for the existence of sizable monetary non-neutrality.

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[28] With the caveat that the close relation between prices and replacement cost could be a feature of how contracts are written between retailers and their suppliers as opposed to prima facie evidence of state dependence in retail.

[29] See e.g. the canonical New Keynesian model (Smets and Wouters, 2003, or Christiano, Eichenbaum and Evans, 2005).

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Appendices

A Data

The data we use are drawn from the Industristatistiken (IS) survey for plant-level data and the Industrins Varuproduktion (IVP) survey for the 8/9-digit price data, which can be linked to the producing plant.

The IVP survey provides plant-level information on prices and quantities for the years $1990-2002$ at the finest (i.e. 8/9 digit) level of the Harmonized System (HS) for the years $1990-1995$ and according to the Combined Nomenclature (CN) for the years $1996-2002$. Although these two coding systems are identical only down to the 6-digit level, the change means that we have no overlap in the raw data at the most detailed level between 1995 and 1996. To avoid throwing away too much information, we need to merge spells across these two coding systems while minimizing the risk of creating spells of price observations for non-identical products. Thus, we take a very cautions approach by only merging price spells for products produced by firms that only produce a single product in 1995 and 1996 and whose product code is identical between 1995 and 1996 at the 6-digit level.

In the left-hand panel of figure 6, we plot the raw data distributions of log price changes (for 8/9-digit unit value data) for all price changes that we can match to the firms in the IS data (including the merged price spells in 1995/1996). All in all, this comprises $18,878$ observations for $2,059$ unique product codes and $4,385$ unique product/firm identities across 934 firms. Each bin represents a log difference of 0.01. As can be seen in the figure, there is a substantial spike for the bin centered around zero. About 13.2 percent of the price-change observations are confined within the ± 0.5 percent interval (with 714 observations identically equal to zero, i.e. 3.8 percent).

Since the raw price data involve quite a few large swings (Max/Min. in the log price change distribution is $7.08/-7.65$) we apply a cleaning procedure for the data used in the analysis. We are concerned with two types of errors in the price data. First, there may be measurement errors (of some magnitude) which show up as a zigzag pattern in the growth rate of the price and, second, there may be significant changes in, say, the quality of a product within a 8/9-digit product group, which will show up as a large

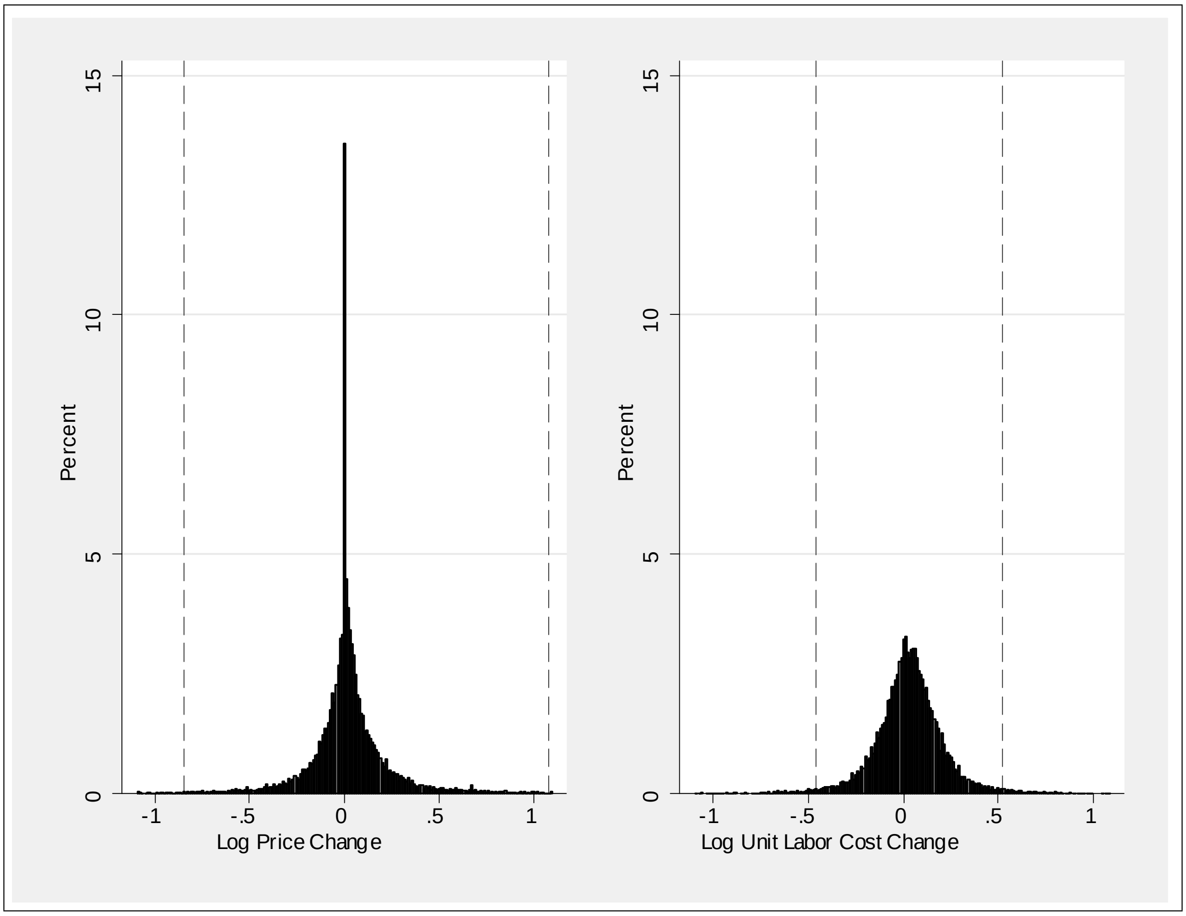

Figure 6: Histograms of raw data of log changes truncated at ± 1.1. The left-hand panel describes the distribution of log price changes across $18,878$ observations (for $2,463$ different products across 943 firms). The right-hand panel describes the distribution of log unit labor cost changes across $17,760$ observations (for $1,480$ firms). Dashed lines indicate truncation limits. Bin size 0.01.

one-period increase in the difference. To remove the impact of this type of observations on the results, we split the individual price series and give them a new unique plant-price identifier whenever a large change in the growth rate appears in the data. We use the full distribution of log price change and determine the cut-off level as given by the 1.5 and 98.5 centiles of this distribution, depicted in the left-hand panel of figure 6. We also correct the firm-specific producer price index used to compute real output in unit labor cost by not using unit-value data in them for these observations. Moreover, price spells with holes in them are given separate unique plant-price identifiers for each separate continuous spell.

For the data from the IS database we start out with standard data quality checking, removing obviously erroneous observations like negative sales or a zero wage bill. Moreover, after constructing the firm-level variables needed, we remove firms which are subject to large swings in unit labor cost, since we aim at capturing normal behavior and not firms in extreme circumstances. In the right-hand panel of figure 6, we plot the log changes in firm-level unit labor cost for all firms $(1,480)$ for which we can compute this measure in the IS data, in sum, $17,760$ observations. The distribution is much less spread out as compared to the price change distribution with the Max/Min at $3.52/-3.79$. Similarly, as with prices, we only keep firms that have unit labor cost changes that are inside the 1.5 and the 98.5 percentile of this distribution in all years (the limits are depicted by dashed lines in the right-hand panel of figure 6).

All in all, this then leaves us with 702 firms with at least one price spell that is longer than one period. The sample of industrial firms is dominated by small to medium sized firms with an average of 65 employees. The firms are distributed across 22 two-digit sectors (NACE). The four industries with most firms represented are industry 28 (Fabricated metal products, except machinery and equipment), industry 20 (Wood and products of wood and cork), industry 15 (Food products and beverages) and industry 29 (Machinery and equipment) with altogether 422 firms (out of the 702). The four smallest sectors, industry 14 (Other mining and quarrying products), industry 23 (Coke, refined petroleum products and nuclear fuels), industry 32 (Radio, television and communication equipment and apparatus) and industry 37 (Secondary raw materials), only have one firm.

When experimenting with more generous cut-off rules for prices and unit labor cost, we find the regression results presented in the top panel of Table 3 in the main text to

Table 5: Robustness Estimation Results

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator	OLS	Probit	Logit	OLS	OLS	OLS
Time Dummies	No	No	No	Yes	No	Yes
Firm-Fixed Effects	No	No	No	No	Yes	Yes
$\lvert d^s \ln MC_{jt} \rvert$	0.071	0.073	0.073	0.075	0.153^{**}	0.158^{**}
	(0.050)	(0.053)	(0.054)	(0.049)	(0.052)	(0.050)
$\lvert d \ln MC_{jt} \rvert$	0.114^{*}	0.118^{*}	0.120^{*}	0.100^{*}	0.167^{**}	0.158^{**}
	(0.053)	(0.057)	(0.058)	(0.051)	(0.056)	(0.052)
$\lvert d \ln MC_{jt-1} \rvert$	-0.014	-0.014	-0.014	-0.032	-0.012	-0.018
	(0.072)	(0.072)	(0.072)	(0.071)	(0.061)	(0.059)

Notes: Dependent variable takes on a value of one if the price change is outside the zero bin and zero otherwise. Superscript **, * and + denote estimates significantly different from zero at the one-, five- and ten-percent level, respectively. In the Probit and Logit columns (3 and 4) marginal effects at the mean are presented. Robust standard error clustered on the firm level is inside the parenthesis. The number of observations is 9,694 (top panel) and 12,292 (bottom panel), respectively.

be very similar. More specifically, we tried using the 1 and 99 centiles instead, leaving us with an estimation sample of 767 firms and $14,990$ price-change observations in the final sample (751 firms and $13,368$ price-change observations when also including a lag in the regression).

B Specification and Estimator Variations

In Table 5 we first present various variations on the baseline regressions presented in the main text. Column (1) replicate the baseline results from Table 3. Columns (2)-(6) show that the baseline results are robust to using a Probit or a Logit estimator instead of a linear probability model, the inclusion of time dummies, firm-fixed effects and the combination of the latter two. Although the statistical significance varies across variations, in an economic sense, the estimated effects are still very small across all variations. Thus, non-linearities or common factors over time or firm-specific factors constant over time do not seem to be important drivers of the results.[30]

[30] Finding that including time dummies does not change the results is of special interest since the data covers 1990-2002 and Sweden faced macroeconomic turbulence in the beginning of that period with a financial crisis starting in 1990 in commercial real estate, culminated in 1992, followed by a recovery and then normal business cycle fluctuations from 1995 and onwards. Also starting the sample in 1995 yields very similar estimates as compared to the baseline regression (p.e. of 0.084 [s.e. 0.065]).

C Selection Effects and Estimation Bias

As discussed in the main text, the small positive point estimates we find in the regression exercise may be due to the way we define the zero band. Note that shrinking the I^{IZB} band in the analysis will have two consequences in that it reclassifies true price changes as price changes in the data and potentially reclassifies true non-changing observations as price changes in the data. First, reclassifying small true price changes as price changes in the data would reduce the positive bias discussed above and drive down the point estimate in the probability regression. Second, to the extent there are small rounding errors in the price data, shrinking the I^{IZB} band creates misclassified price changes in the data. In a Calvo (1983) model this will not bias the point estimate in the probability model since the probability of being stuck with the old price and the measurement error in prices are independent of marginal cost. However, in a Menu-Cost model, firms that do not change the price do so because they typically had small changes in marginal costs. Thus, reclassifying true non-changing observations as price changes will bias the point estimate downwards if the data is generated by a Menu-Cost model. For this reason, comparing the baseline regression results with those obtained when shrinking the band towards only including exactly zero price changes yields an interval within which the true selection effect lies.

Comparing column (1) and (2) in the top-left panel of Table 6, we see that narrowing the band lowers the point estimate from 0.071 to -0.020 as expected. In this formulation the $I_{gt}^{IZB} = 1$ observations constitute 2.6 percent of the sample (as compared to 11.6 percent in the baseline formulation in column (1)). But note that the standard error actually shrinks in the latter case (0.050 vs. 0.025), thus not indicating any precision problems when shrinking the band (also using a Probit or Logit estimator yields very similar results quantitatively). Also, in columns (1) and (2) in the bottom-left panel we present a very similar effect of shrinking the band when using the absolute value of the non-accumulated changes.

In columns (3) and (4) of Table 6 we redo the experiment above on synthetic data from the Menu-Cost model. Here, we still expect a positive estimate when using only exactly zero price-change observations since in the Menu-Cost model firms choose not to change price due to small changes in marginal cost and vice versa. Comparing the results in columns (4) and (5), we see that the point estimate falls slightly with 0.128 when

Table 6: Estimation and Simulation Results - Band Size

	(1)	(2)	(3)	(4)	(5)	(6)
Band Size:	Base	Zero	Base	Zero	Base	Zero
	Data		Menu Cost		CalvoPlus	
$\lvert d^s \ln MC_{jt} \rvert$	0.071	-0.020	0.959**	0.831**	0.143**	-0.031
	(0.050)	(0.025)	[0.032]	[0.031]	[0.034]	[0.030]
$\lvert d \ln MC_{jt} \rvert$	0.114*	-0.001	1.067**	0.948**	0.173**	0.009
	(0.053)	(0.035)	[0.033]	[0.031]	[0.033]	[0.026]
$\lvert d \ln MC_{jt-1} \rvert$	-0.014	-0.060	0.308**	0.334**	0.122**	0.017
	(0.072)	(0.071)	[0.035]	[0.031]	[0.036]	[0.030]

Notes: The dependent variable takes on a value of one if the price change is outside the zero band defined in the first row above and zero otherwise. Superscript **, * and + denote estimates significantly different from zero at the one-, five- and ten-percent level, respectively. The number of observations in the data panel is 9,694/10,071 (top) and 12,292/12,292 (bottom), respectively. Robust standard error clustered on the firm level insiden the parenthesis. In the simulation panel the coefficent denotes the average across 200 panel simulations. Standard deviation of the point estimate across 200 panels is inside the square bracket.

going from using the zero bin to exactly zero price change observations in the probability regression (average point estimates across simulations are 0.959 vs. 0.831). Since there are no measurement errors and thus no associated cases of misclassified price changers in the synthetic data, this result gives a measure of the size of the positive bias from misclassifying small true price changes when relying on the baseline definition of I^{IZB}.

In columns (5) and (6) we do the same experiment in the calibrated CalvoPlus model. The point estimate drops from 0.143 to -0.031 when shifting the dependent variable from the baseline zero bin to only looking at exactly zero price changes. The intuition is that since the data want a calibration of the CalvoPlus model that is, for all relevant aspects, a standard Calvo model, there are no selection effects. This exercise thus confirms that the time aggregation does not affect the basic intuition for the mechanisms at work. Moreover, the difference between the estimates, 0.174, gives a slightly larger estimate of the positive bias from including small positive price changes in the I^{IZB} definition as compared to the Menu-Cost model. In Figure 7 we present a kernel regression exercise, which graphically illustrates the results discussed above. Comparing the top-left panel with the bottom-left panel of Figure 7 we see that the positive slope disappears when changing the zero-bin definition to only include exactly zero price-change observations. Comparing the top-right panel with the bottom-right panel, we see that not using the absolute value (of the accumulated log marginal cost change) leads to an expected U-

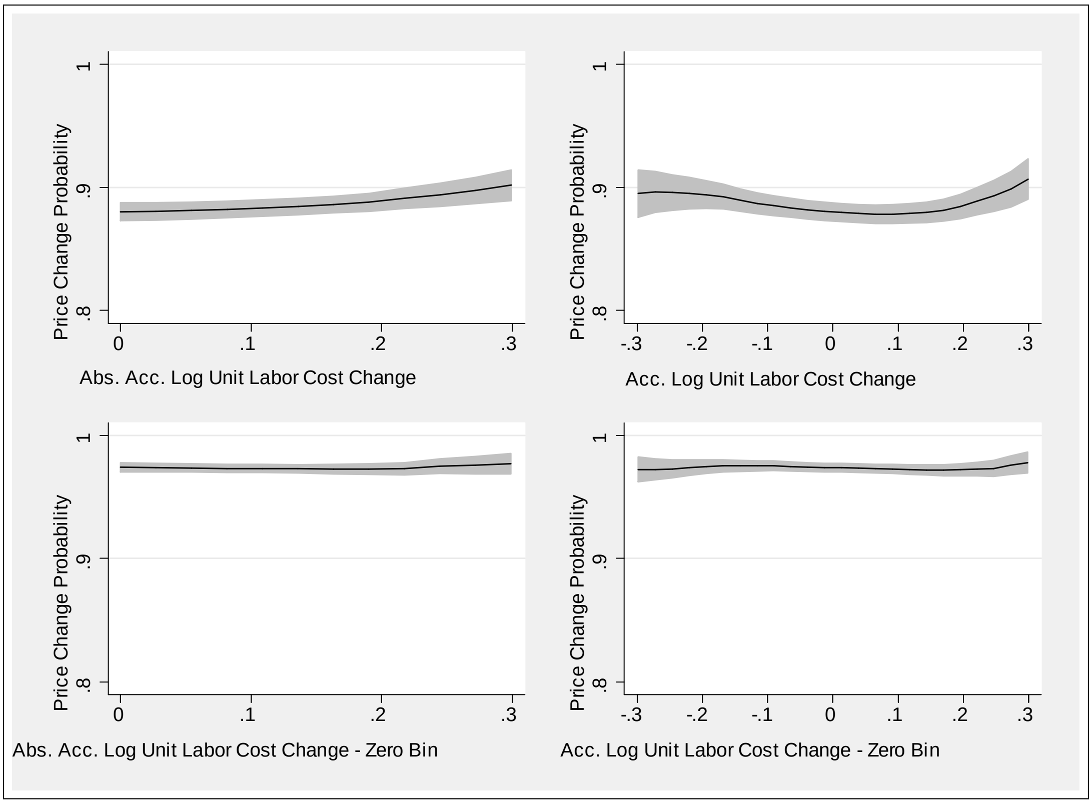

Figure 7: Kernel regressions of the baseline (top panels) and the exactly zero (bottom panels) price-change dummy on the absolute (left-hand panels) and regular (right-hand panels) accumulated change in log marginal cost. Gray area depicts the 95-percent confidence band.

shaped relationship that disappears once only relying on exactly zero price changes in the zero bin.

In the bottom panels of Table 6 we redo the exercises outlined above using the absolute value of the non-accumulated change.[31] As can be seen in the two bottom rows of Table 6, results are qualitatively unchanged from this extension. Also, comparing the results in columns (3) and (4) we see that the lagged effect in the Menu-Cost model is qualitatively unchanged from using the baseline zero bin or only the observations that are exactly zero. Moreover, Figure 8 repeats the exercise the exercise of Figure 7, but using the non-accumulated change, with very similar results.

The results suggest that the difference between estimated selection effects in the data when comparing the baseline with the results from relying on only the exactly zero observations is well in line with the bias estimates from the simulated data. In fact the point

[31]The small differences between the results in the bottom panel of column (5) and the bottom panel of Table 4 stems from that here we average point estimates from each panel, whereas in the bottom panel of Table 4 we first stack all data and then run the regression.

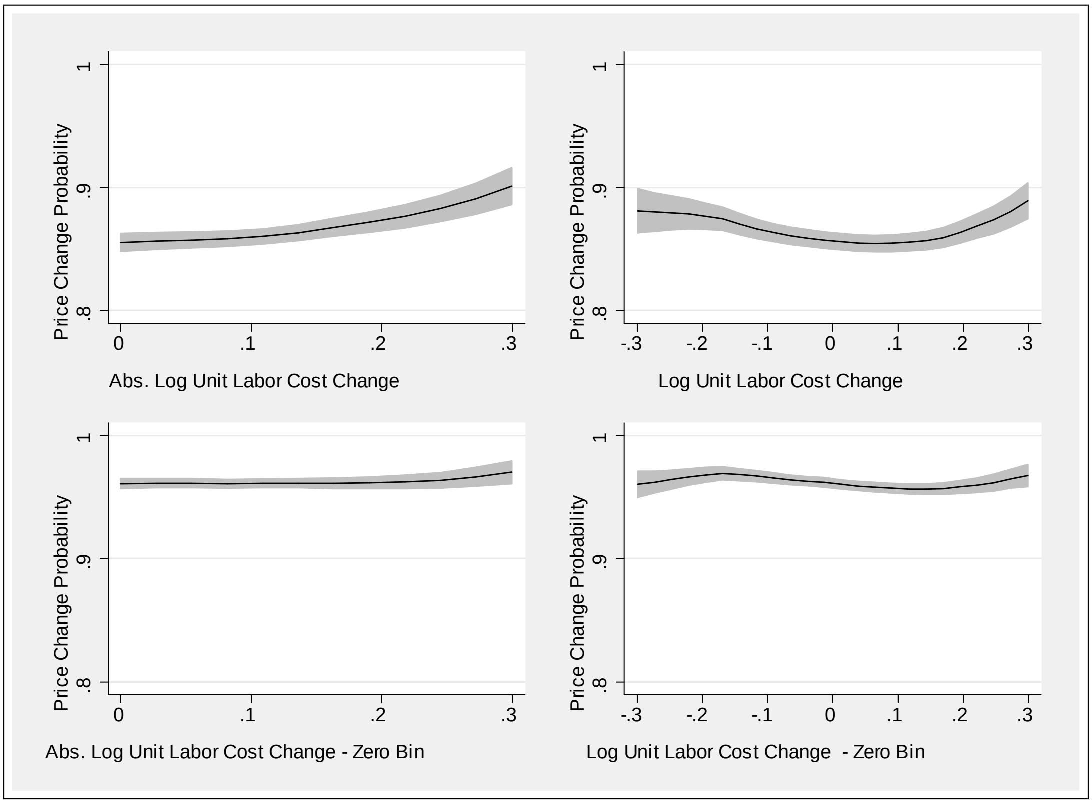

Figure 8: Kernel regressions of the baseline (top panels) and the exactly zero (bottom panels) price-change dummy on the absolute (left-hand panels) and regular (right-hand panels) change in log marginal cost. Gray area depicts the 95-percent confidence band.

Table 7: Robustness Single-Product Firms / Band Size

	(1)	(2)	(3)	(4)
Band Size:	Base	Zero	Base	Zero
Sample	Baseline		Single-Product Firms	
$\lvert d^s \ln MC_{jt} \rvert$	0.071	-0.020	0.172	0.028^{+}
	(0.050)	(0.025)	(0.113)	(0.016)
$\lvert d \ln MC_{jt} \rvert$	0.114^{*}	-0.001	0.297^{**}	0.045^{+}
	(0.053)	(0.035)	(0.095)	(0.025)
$\lvert d \ln MC_{jt-1} \rvert$	-0.014	-0.060	0.104	-0.023
	(0.072)	(0.071)	(0.094)	(0.033)

Notes: The dependent variable takes on a value of one if the price change is outside the zero band defined in the first row above and zero otherwise. Superscript **, * and + denote estimates significantly different from zero at the one-, five- and ten-percent level, respectively. The number of observations are, by column (top/bottom panel) 9,694 (12,292), 10,071 (12,292), 898 (1,144), 955, (1,144). Robust standard error clustered on the firm level inside the parenthesis.

estimate of the drop (0.091) when shrinking the band is actually smaller than in the models, thus pointing away from the hypothesis that the estimate when only relying on exactly zero observation in the data is downward-biased due to misclassification of price changes in combination with state dependence in price-setting. Moreover, the results from fitting the CalvoPlus model, which indicate very little state dependence, suggests that the estimates in column (2) of Table 6 are more or less an unbiased estimates of the true selection effects. Thus, taken together, the results presented here lend support to the Calvo (1983) interpretation of the data and the view that the (small) positive point estimates reported in Table 3 is the result of upward bias from including small price changes in the zero bin.

D Single-Product Firms

In Table 7 we present the results from only relying on the 264 firms in the sample identified as single-product firms from the IVP survey (in accordance with the 8/9 digits HS/CN codes). First, column (1) reproduces the results from the baseline sample. Compared to these results, we see that when relying only on single-plant firms, column (3) of Table 7 leads to somewhat higher point estimates. Confidence intervals overlap on regular significance levels, however, and, even taken at face value, the findings remain consistent with a much weaker selection effects than implied by a standard menu-cost models.

Higher point estimates are to be expected if there are measurement errors that attenuates the estimates from the full sample. However, when looking at the results from only relying on exactly zero price change observations in the definition of the price-change dummy, column (2), we see quantitatively similar results as compared to the multi-product results, column (4). Thus, the likely explanation for the somewhat higher point estimates in the single-product sample is that the upward bias in the point estimates from using an interval definition of the price-change dummy (see the discussion in Appendix C) is stronger in the single-product sample than in the full sample. All, in all, we conclude that we do not find any important differences in observed pricing patterns between single- and multi-product producers in the dimensions studied in this paper.