

The price effects of a permanent reduction in VAT on food: what the European evidence can and cannot establish

A memorandum on the Ministry of Finance's reply to Question No. 16 of 4 August
2026

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Summary

The Ministry's reply to Question No. 16 uses the empirical literature on whether cutting VAT rates reaches consumer prices. It cites our 2020 study of all VAT changes in the European Union and sets it aside, on the ground that it reports an average across goods rather than a result for food. I have taken that objection seriously and re-estimated our results for food alone and for the kind of rate change Denmark proposes.

The proposed VAT cut faces four main challenges.

First, **the price effect is small**. Disaggregating does not rescue the assumption the proposal is being scored on. Restricting our estimates to food raises the pass-through of a VAT cut from about 6 percent to about 24 percent. The Ministry is right that food differs from the average. However, pass-through is still nowhere near complete. And Denmark would not be lowering a rate that applies across the economy; it would be moving a group of goods onto a reduced rate. Split that way, the data put pass-through close to zero.

Second, **early pass-through does not last**. The three recent studies that do find complete pass-through all examine temporary reductions, two of them crisis measures, and not one of them follows prices far past the point at which the rate went back up. Salience is the explanation the Portuguese authors themselves reach for. Where monitored and unmonitored sellers can be compared inside a single reform, pass-through is near-complete where prices are watched and about a third where they are not. In two European restaurant reforms the initial full pass-through was undone six months to a year, by the same firms that had delivered it. The Danish reduction would be permanent, and it would be salient and closely watched only for its first months. Those first months are what the table predominantly measures. The initial price response is what can be verified. Whether it stays is a question the design of a permanent reform makes difficult to answer.

Third, **the long run is assumed, not measured**. The Ministry concedes that its table is "pre-

dominantly short-run” and that long-run evidence is limited, and then assumes that pass-through rises over time. The evidence it cites for the long run is ours, and it runs the other way.

Fourth, **most of the VAT cut benefits sellers.** In previous estimates of the effect of VAT cuts, firm owners had retained around 56 percent of the cut thirty months later, and consumers benefited least of the four groups examined. On that evidence a Danish reduction would be, in substantial part, a transfer to food retailers. Those retailers are campaigning for it: the Danish grocers’ association has published its own analysis in support.

Each of those four difficulties rests on a reading of the price evidence, and the Ministry may prefer its own. The last point does not. Sweden reduced VAT on food in 1996 and has kept it reduced for thirty years. In 2018 its national audit office examined the policy. It found that the cut had reached prices in full in the short run — the finding the Ministry’s table reproduces — and then asked what the table does not: whether the money was well spent. Its answer was that reduced VAT on food “is not a cost-effective way to enhance the purchasing power of families with children and low-income households,” because the same households could be given the same gain for about half the money by other means. The Danish Ministry cites the Swedish report but does not mention its main conclusion: even with full pass-through of the VAT cut, there are better policies available.

1. Disaggregating to food raises the estimate fourfold and leaves it nowhere near full

The Ministry's objection to our work is that it reports "the average degree of pass-through for a broad range of goods and services" rather than for food. That is a fair objection, and it has a straightforward answer: disaggregate and look. We did so in the paper itself, and I have since done it again in the specification the Ministry cites.

Our 2020 paper reports the disaggregation as Online Appendix Figures E.20, E.21 and E.22, which plot the price response separately for each two-digit commodity group. The text summarizing them states that "this asymmetry is not driven by any subset of commodities," and that all commodities exhibit it apart from two, Communication and Household Furnishings. Food and non-alcoholic beverages is Figure E.20a, reproduced here as Figure 1. It shows prices rising by 65 percent of a VAT increase and falling by 23 percent of a VAT decrease.

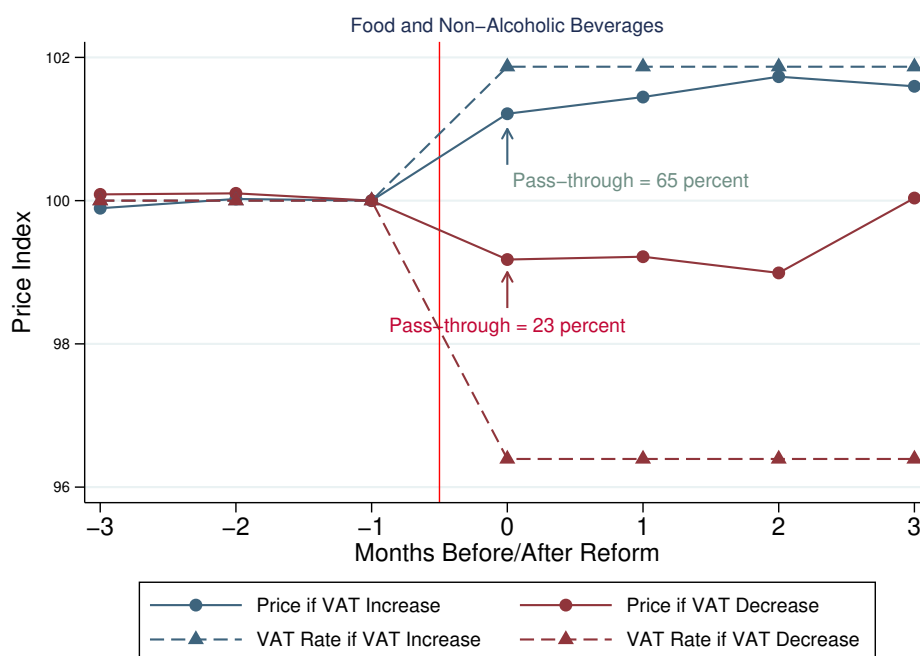


Figure 1: Food and Non-Alcoholic Beverages: Price and VAT Responses

Notes: Reproduced from Benzarti, Carloni, Harju and Kosonen (2020), Online Appendix Figure E.20a. The figure plots the average price index and average VAT rate for food and non-alcoholic beverages in the three months before and after a VAT change, normalized to 100 in the month before the reform, separately for VAT increases and VAT decreases.

Running the paper's estimating equation on food alone gives the same picture: pass-through of 44 percent for increases and 24 percent for decreases. This is new analysis rather than a published result. But both approaches, one published and one not, put the pass-through of a food VAT cut at roughly a quarter.

The Ministry is therefore right that food differs from the average, and the difference is substantial. Across all commodities the pass-through of a VAT decrease is about 6 percent. For food it is about four times that. It is also nothing like the figure the proposal is being scored on. Using the food estimate, I can rule out pass-through of 75 percent or more.

I should be equally clear about what the food estimate cannot do. It rests on 28 food VAT decreases — eleven distinct reforms — across nine countries between 1997 and 2012. That is more reforms than any of the country studies in the Ministry's table, and it is still not enough to pin down a precise figure. Saying that the price effect of a food VAT cut is known with precision is overstating what the evidence can support, and that applies to my estimate for food and non-alcoholic beverages as much as to theirs. It does not apply in the same way to the all-commodity estimate in our 2020 paper, which rests on 366 VAT decreases rather than 28 and is correspondingly firmer.

There is, however, a sharper way to cut the data, and it is closer to what Denmark is actually proposing. The relevant distinction is not really food against other commodities. It is between changing a rate that applies across the economy and changing a rate that applies to a selected group of goods. Denmark is not proposing to lower its VAT rate. It is proposing to move food onto a new, lower rate. European VAT reforms can be separated the same way, and the two behave differently.

When countries change the standard rate that applies broadly, the pass-through of a decrease is about 12 percent. When they change a reduced rate applied to selected goods, it is essentially zero. The difference between the two is statistically significant, and for the targeted kind I can rule out pass-through of 10 percent or more. This is again new analysis and not a published result. But it is drawn from the same data and the same specification as the estimate the Ministry cites, and it points the same way as everything else in this memo.

Two conclusions follow. Disaggregating the average, as the Ministry asks, does not rescue the assumption of full pass-through; it moves the number from about 6 percent to about 24 percent when the cut is defined by commodity, and to approximately zero when it is defined the way the Danish reform actually defines it.

The second is that the objection cuts the other way. If an average across many reforms conceals important variation, then a single reform in a single country conceals more, and single reforms in single countries are what the Ministry's own estimate rests on. Its eight food entries cover seven reforms in seven countries. Two of them examine the same Spanish measure of January 2023, and one measures a VAT increase rather than a cut. The studies in the table that pool reforms across many countries and years are precisely the ones set aside as averages. Averaging across many reforms is not a weakness of the estimate. It is what allows it to say anything about a reform that has not happened yet.

2. Most of the table records a verdict rather than a result

If an average across many reforms is the wrong evidence, the question is what the table offers instead. I have read the studies it lists. These are careful papers, several of them in leading journals, and my disagreement is not with their execution. It is with the inference from a temporary, closely watched reform in one country to a permanent change in the Danish rate structure. What follows is a description of what the table contains, not an argument about the quality of the work in it.

Most of the table does not report a result. Eight of the rows concern food or fruit and vegetables, and six of them record a verdict rather than a figure: “full pass-through,” or “substantial pass-through.”

I have looked at the papers cited by the Ministry in more detail. David’s study of the Czech Republic measures a VAT *increase*, not a cut. It appears in a table assembled to inform a reduction, and is recorded there as showing full pass-through; the paper itself compares food price growth of 7.94 percent with a rate increase of 4 percent, and its author notes that other factors also determine food prices. The European Commission entry describes itself as “a preliminary assessment”; it compares average prices in late December 2022 with prices in the first days of January 2023, uses data from one supermarket in each of two countries, and states that its sample “should therefore be considered neither exhaustive nor representative.” The table records it as “substantial pass-through”; its own text reports 30 to 70 percent for bread, milk and cooking oil. The Latvian study is a paired comparison of first-year prices against Estonia and Lithuania rather than a regression, and its authors report that significance varies from month to month. It is also the only entry in the table that concerns fruit and vegetables, so the zero-rating half of the Danish proposal rests on it alone.

The Swedish entry deserves a further word, because its authors raised the difficulty themselves. Their stated reason for conducting the audit was that “it has been just over 20 years since the VAT on food was reduced, and yet no evaluation of the price effects of the reduction have been published. There is therefore a risk that the price impact has been overstated.” That sentence is in the document the table cites for full pass-through in Sweden.

3. The reforms that passed through fully were temporary, salient and closely watched

Three of the recent studies in the Ministry's table find high pass-through in food retail, and two of them find an asymmetry running opposite to ours. Fuest and co-authors put it plainly: the German price decrease after the cut was about twice the price increase after the reinstatement, and "this result is the opposite of what Benzarti et al. (2020) find." Forteza and co-authors report Spanish pass-through of roughly 90 percent on the cut against 75 and then 50 percent on the two reversal phases. Bernardino and co-authors find Portuguese pass-through complete and symmetric, and say so against our work by name. I do not think these results can be waved away, and I will not try.

What they have in common is more interesting than the disagreement. Every one of them studies a rate cut announced as temporary: Germany for six months in 2020, Spain from January 2023, Portugal for less than nine months from April 2023. Every one uses daily prices scraped from supermarket websites. And not one of them follows prices far past the point at which the rate went back up — three months in Germany, under two in Portugal, three weeks after the final Spanish step, with the Spanish pass-through figures themselves measured a week after each change. Spain and Portugal cut in response to an inflation crisis. The German cut was a pandemic stimulus, announced, in its authors' words, "to the surprise of the general public."

The Portuguese authors go further, and name a mechanism. They describe media saturation, a consumer association running a public price tracker, "IVA 0" stickers on the shelves, and Google search intensity for the policy comparable to that for a Taylor Swift concert announcement. They also concede that producer prices were falling at the same time, and that this "may account for the full pass-through observed at the time of policy implementation." The German authors are candid about what their own window can show: they write that "it cannot be ruled out that the price reduction in German supermarket retail following the temporary VAT rate cut will be fully revoked with a delay."

Salience is the explanation on offer, and it is the Portuguese authors' own. But none of these studies can test it, because each observes a single regime. In work with Santiago Garriga and Dario Tortarolo I have been able to. Argentina zero-rated thirteen staple foods in 2019 and reinstated the tax later, and we observe barcode-level prices in more than three thousand supermarkets. In the chain supermarkets, where the government monitored prices, pass-through of the cut was near-complete and statistically indistinguishable from full. In the unmonitored independent stores, pass-through was about 35 percent, the reinstatement passed through roughly twice as strongly as the cut, and prices settled above where they had started. Same country, same reform, same months. What differed was whether anyone was watching.

Sweden has just supplied a live example of the same pattern. It halved food VAT from 12 to 6 percent on 1 April 2026, and an independent price-monitoring firm reported one month later that the cut had reached the shelves in full. That finding is now the Danish Government's central piece of evidence: announcing a meeting with the retail sector in June, the Ministry of Taxation and Economic Growth said that in Sweden "the most recent VAT reduction from this year has apparently passed fully through to prices." It is one month of data from a monitored, heavily publicised reform. It is not evidence about what Swedish prices will look like in 2030.

The best-identified study in the Ministry's table is not one of the three I began with, and none of the foregoing explains it away. Ingvil Gaarder examines Norway's decision, effective 1 July 2001, to cut VAT on all food from 24 to 12 percent while leaving other goods at 24. Using a regression discontinuity at the reform date, the paper finds the tax completely shifted to consumer prices, with little effect on the prices of other goods. This is a food-specific move from the standard rate to a reduced rate, which is structurally what Denmark is proposing, and I have no criticism to make of the design. It is, however, an estimate of the immediate response by construction. Its two specifications use bandwidths of two months and of one month either side of the reform date; in the second, the estimate is the difference between average food prices in June 2001 and in July 2001. It establishes what happened to Norwegian food prices in the month the reform took effect. It was not built to establish what they were three years later, and it does not.

There is direct evidence on what a one-month reading is worth. Harju, Kosonen and Nordström Skans studied two restaurant VAT cuts, in Finland in July 2010 and in Sweden in January 2012, collecting prices shortly after each reform, again at three to six months, and again at fifteen to eighteen months. The immediate response was sharply divided. Nearly all independent restaurants left their prices unchanged, while a substantial share of chain restaurants passed the cut through in full. By the final collection the two groups had converged and the difference between them was no longer statistically significant. The convergence did not come about because the independents lowered their prices. It came about because the chains raised theirs. In the authors' words, it "is primarily achieved through additional price increases by those restaurants (belonging to chains) that initially responded with a full pass-through."

The firms that pass a cut through immediately are therefore not necessarily the firms that leave it in place. A price collection taken one month after a reform will record those firms as having delivered the reduction in full. A collection taken a year later, in this case, would not.

This bears directly on the Danish proposal. A reform of this size will be salient when it arrives. It has been debated for a year, it will be announced, and the Government has said it will watch prices. If the Swedish and Portuguese experiences are any guide, the initial adjustment will be visible and probably large. The difficulty is that this is the phase everyone measures, and it is also the phase that ends. Attention moves on, the watching stops, and the rate stays where it is. Argentina is the only setting in which both phases have been observed, and in the unmonitored one pass-through was about a third and prices drifted back above where they had started.

There is a further problem, and it is the reason the Swedish auditors could not answer this question after twenty-two years. A permanent rate change produces a single event. Prices can be compared with the day before, and the initial cut can be confirmed. After that there is no further tax change to measure against, and any later increase arrives with an explanation of its own: energy, wages, commodity prices, a weaker krone. Monitoring can establish that a cut reached the shelves. It cannot establish that it stayed there.

4. The Ministry concedes its evidence is short-run, and then assumes the long run goes its way

The Ministry's reply turns to the time horizon in a single paragraph, and it is worth reading in the order it is written. It begins with theory: supply "may be expected to be more price-sensitive in the long run than in the short run," and "this points towards the degree of pass-through increasing over time." It then concedes the position of the evidence: "the degrees of pass-through shown in Table 1 are predominantly short-run effects. There is limited empirical evidence on the long-run effects of VAT changes."

Having said that long-run evidence is scarce, the reply then describes the long-run evidence that exists. It is ours, and the Ministry summarises it accurately. Pass-through, it notes, "declined slightly during the first 20 months after the VAT change." The Finnish asymmetry "remained present 3.5 years after the VAT increase." And in the 2025 follow-up, "this asymmetry still exists ten years later, although it has diminished." Every one of those findings points away from the assumption made two sentences earlier. The proposition that pass-through rises over time is offered as theory. The evidence cited immediately afterwards runs the other way, and it is the only evidence there is.

What that evidence consists of is worth stating plainly, because it is not the full sample. In Finland the VAT on hairdressing services was cut by 14 percentage points in 2007 and raised by the same 14 points in 2012; 3.5 years after the restoration, prices remained higher than for the control group even though both groups faced the same rate. Figure 2 shows the eleven-year series.

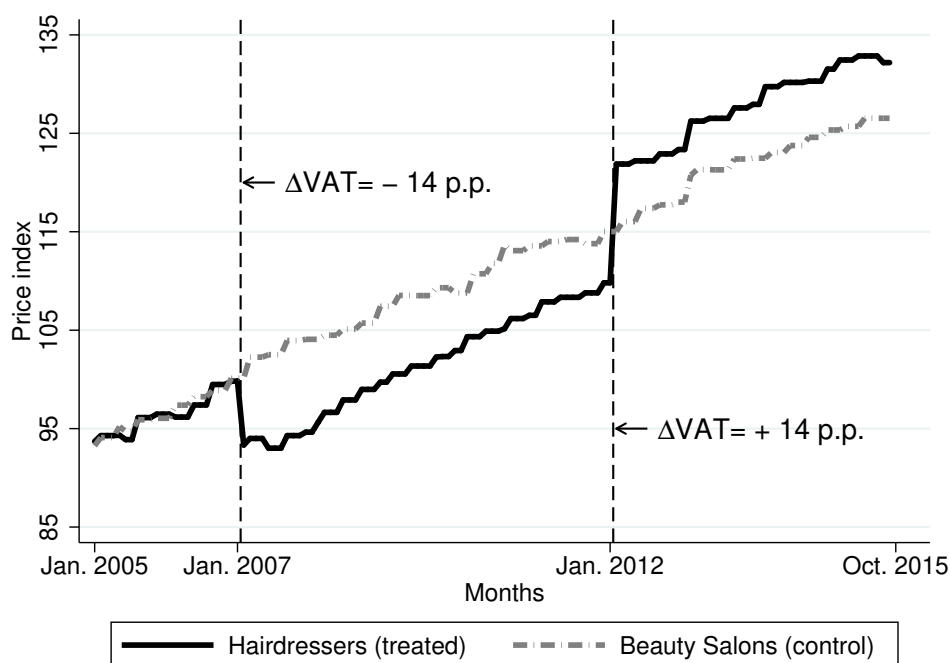


Figure 2: Finnish Hairdressing Sector VAT Reforms

Notes: Reproduced from Benzarti, Carloni, Harju and Kosonen (2020), Figure 1. Price index for hairdressing services (treated) and beauty salons (control), January 2005 to October 2015. The VAT on hairdressing services was cut by 14 percentage points in January 2007 and raised by 14 percentage points in January 2012. Beauty salons were unaffected throughout.

In Hungary the standard rate was cut from 25 to 20 percent in January 2006 and raised from 20 back to 25 percent in July 2009. In each case the change is the same size in both directions, which is what makes these tests of asymmetry rather than of the level of the rate. What Hungary adds is breadth: the standard rate covers a wide range of goods, so the persistence it shows is not confined to a single sector.

These are case studies, and deliberately so. Detecting a price effect years after a reform requires large tax changes in sectors where prices are otherwise stable; absent those conditions the effect is masked by ordinary price variation and inflation. Our full-sample estimates cannot carry this weight, and I would not claim they do: beyond about eighteen months their precision falls away, and we cannot reject that the cumulative pass-through of increases and decreases is equal at that horizon. The long-run claim rests on the cases, not on the average.

There is a further implication for a permanent reform, and I put it as a risk to be priced rather than a prediction. A reduction that is legislated as permanent can still be reversed, and Denmark's fiscal position may one day make reversal attractive. If that happens, our evidence says prices will not simply return to where they were. In the Finnish case, profits and markups settled at a higher level after the cut was repealed and had not converged three years later. A cut followed by a restoration does not return the price level to its starting point; it leaves it higher. That is the specific risk this policy carries, and nothing in the Ministry's table speaks to it.

5. When a comparable cut was last decomposed, firm owners kept more than half

The debate has been conducted as though the question were whether prices fall. The more useful question for a finance committee is where the money goes, and on that there is direct evidence rather than inference. In 2009 France cut the VAT rate for sit-down restaurants from 19.6 percent to 5.5 percent. That reform is the closest precedent available to what Denmark is considering: it moved a large category of consumption from the standard rate to the reduced rate, and it was permanent.

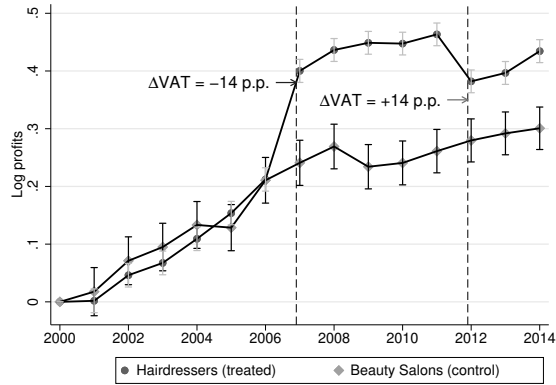
With a co-author, we estimated the effects of that cut on four groups rather than on prices alone: workers, firm owners, consumers and suppliers of material goods. Thirty months after the reform, firm owners had kept around 55.7 percent of the VAT cut. Consumers, suppliers and employees shared what remained, “with consumers benefitting the least,” and the employment effects were limited. Prices fell by about 1.3 percent in the first month against a tax cut of 14.1 percent, a pass-through of 9.7 percent. I would note that this figure sits inside the range I report in the previous sections for targeted rate cuts, from a different country, a different decade and a different method.

The obvious reply is that firms may have done something useful with the money. Administrative corporate tax data on the full population of Finnish hairdressers and beauty salons allows that reply to be tested, and it does not survive. Profits rose by 0.2 log points after the VAT cut and fell by only 0.1 after the subsequent increase. Markups rose twice as much after the cut as they fell after the increase, and neither had converged three years after the rate returned to its original level. Variable and fixed costs did not change, there was no increase in entry or exit, and investment did not respond. The money did not go into expansion, employment or absorbed costs. It went into profits and stayed there. Figure 3 shows the four series together.

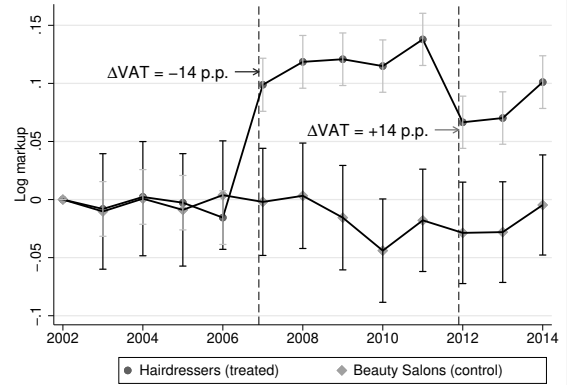
Both of these are service sectors rather than grocery retail, and a reader is entitled to ask whether food retailers would behave differently. That is a fair question and I cannot settle it with these data. What I can say is that the mechanism does not depend on the sector: firms retain what they are not compelled by competition to pass on, and the price evidence across all commodities in the European sample points the same way.

There is a further consideration specific to Denmark, and it does not require any econometrics. The firms that would retain this money are campaigning for the policy. De Samvirkende Købmænd, the Danish grocers’ association, has published a fourteen-page analysis arguing that lower food VAT works, and it rests on Gaarder, on Forteza, Prades and Roca, and on Bernardino and co-authors, three of the eight studies the Ministry later assembled in Table 1. Our own work does not appear in it. Neither does the word asymmetry.

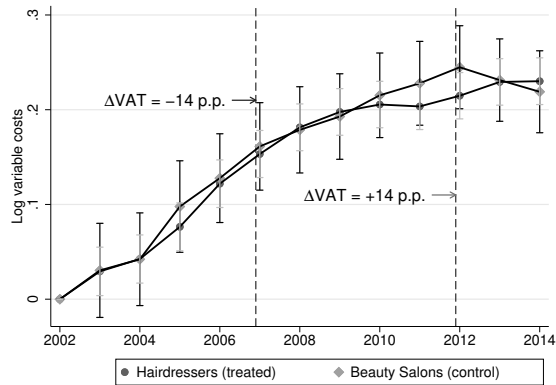
The Government’s assurance to consumers rests on those same firms. On 10 June 2026 the Minister of Taxation and Economic Growth, Jakob Engel-Schmidt, summoned the retail sector to a meeting, saying that he had “stor tillid” — great confidence — that the industry wanted what the Government wanted. The announcement holds up the Swedish example, where, it says, “the most recent VAT reduction from this year has apparently passed fully through to prices.” That Swedish reduction had taken effect ten weeks earlier. As Section 3 sets out, pass-through does appear high where a reform is salient and prices are watched. Both of those conditions are temporary. A permanent VAT reduction is not.



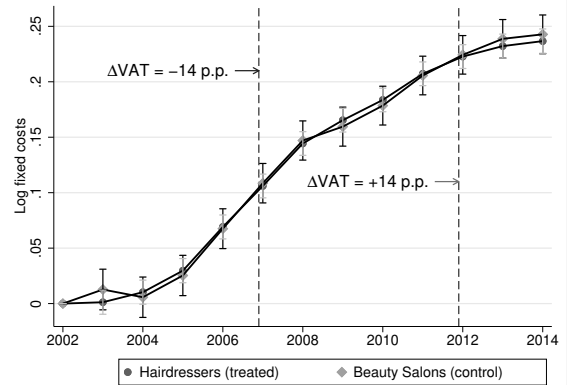
(a) Log profits



(b) Log markups



(c) Log variable costs



(d) Log fixed costs

Figure 3: Profits, Markups, Variable and Fixed Costs, Finnish Hairdressers

Notes: Reproduced from Benzarti, Carloni, Harju and Kosonen (2020), Figure 5. Coefficients from regressions of log profits, log markups, log variable costs and log fixed costs on year dummies, for Finnish hairdressers (treated) and beauty salons (control). The VAT on hairdressing services was cut in January 2007 and raised in January 2012.

6. Sweden's own auditor concluded the money would do more good spent another way

Sweden has run this policy permanently for thirty years, and has examined whether it was worth the money. It reduced the VAT rate on food from 21 percent to 12 percent on 1 January 1996, for reasons that will be familiar: the Government was cutting benefit levels, and the food VAT cut was meant to offset the distributional consequences for families with children and low-income households, “because these use a relatively high proportion of their household budget on food purchases.” The rate stayed at 12 percent for three decades, and Sweden halved it again, to 6 percent, in April 2026. In 2018, after the policy had been in force for twenty-two years, the Swedish National Audit Office examined it and concluded that “the reduced VAT is not a cost-effective way to enhance the purchasing power of families with children and low-income households.”

The auditors reached that conclusion by pricing the alternative. The tax expenditure was nearly 30 billion SEK for the 2018 income year. They simulated a package of ordinary spending measures — child allowance, the large family supplement, the study allowance, the national standard for financial assistance and the housing supplement — designed to deliver the same purchasing-power gain to the same households. That package would cost 13.4 billion SEK, “which is about half of our estimate of the tax expenditure (27.3 billion SEK) that the reduced VAT on food entails.” The same benefit for the intended group, for roughly half the money.

The reason is that a food VAT cut cannot be aimed. The report does not find that the policy fails its target group; it finds the opposite, that reduced VAT on food “has a positive distribution profile” and does strengthen their purchasing power relative to other households. The difficulty is accuracy. “The reduced VAT on food is a boon to everyone who consumes food, regardless of whether or not they have children and whether they are low or high-income earners.” Most of the money reaches households the policy was never intended for, and the cost of helping the intended ones rises accordingly.

The auditors therefore recommend that the Government “take the initiative to investigate the reduced VAT on food,” establish whether its purpose is still to support families with children and low-income households, and “in such case, to consider other ways to support these households.”

This conclusion does not depend on anything in the preceding sections, and that is why I place it last. The Swedish auditors found full short-run pass-through. Theirs is the report the Ministry cites in Table 1 for exactly that. They then assumed full pass-through when assessing the distributional effects, and reached their conclusion anyway. The case against a permanent food VAT cut does not require the reader to accept the estimates in our 2020 paper, or to prefer them to any study in the Ministry's table, or to reach any view at all on whether prices fall. It requires only the observation that Sweden audited this policy after twenty-two years and found that the same families could be helped for half as much.

Appendix

A. The estimates used in this memo

A.1 The specification

Sections 1 and 4 use the estimating equation of Benzarti, Carloni, Harju and Kosonen (2020) without modification. It is an event study in first differences: the monthly change in the log price of a commodity is regressed on the change in log of one plus its VAT rate, with ten monthly leads and ten lags, time fixed effects, and controls for the unemployment rate, the interest rate and GDP per capita. Standard errors are clustered on time. Pass-through is the cumulative sum of the coefficients after the reform, so a value of one is exactly full pass-through and a value of zero is none.

Increases and decreases are estimated separately, each against the same set of never-treated commodities. The panel covers 27 European Union member states, 44 COICOP commodity codes falling into ten two-digit groups, and the period 1996 to 2015, or 460,318 monthly observations.

Two things follow from that design and are worth stating before any number is read. Pass-through is measured against commodities that were not treated, so it is a relative statement and not a claim about the absolute path of prices. And the estimate is an average over every reform in the window, which is precisely the property Section 1 defends and the Ministry objects to.

A.2 Published results, reproduced

Before running anything new I reproduced the published estimates from the replication package. This is not a formality: it establishes that the new results below differ from the published ones only in the sample restriction, and not in the code.

Source	Sample	Increase	Decrease
Table 1	Full sample	0.338 (0.063)	0.064 (0.030)
Online Appendix E.8	No controls, time fixed effects only	0.330 (0.061)	0.071 (0.029)
Online Appendix E.9	Temporary reforms	0.341 (0.084)	0.079 (0.043)
Online Appendix E.10	Permanent reforms	0.260 (0.073)	0.028 (0.023)

All four match the published tables to the reported precision. Standard errors in parentheses.

The published disaggregation by commodity is Online Appendix Figures E.20 to E.22. Food and non-alcoholic beverages is Figure E.20a, reproduced as Figure 1 of this memo; recomputing its annotation from the underlying series gives pass-through of 0.649 for increases and 0.228 for decreases, matching the published figure.

A.3 New estimates: food alone

These are new analysis. They are not a published result and have not been refereed. They apply the equation in A.1 to COICOP 011 (food) and 0122 (non-alcoholic beverages) only.

Specification	Increase	Decrease
Food, with controls	0.441 (0.140)	0.242 (0.188)
Food, no controls	0.433 (0.115)	0.238 (0.189)

The decrease estimate of roughly a quarter is the figure Section 1 relies on. Dropping the controls moves it by four thousandths, which is the reason Section 1 treats the two approaches as one result.

Two cautions belong with these numbers rather than in the body of the memo. The food decrease estimate is not statistically distinguishable from zero, so nothing in this memo should be read as establishing that food prices fall at all; what it establishes is a ceiling, which is set out in A.5. And restricting to permanent reforms leaves six events and an estimate of 0.565 with a standard error of 0.369 — too imprecise to carry any weight, and I do not use it.

A.4 New estimates: economy-wide against targeted rate changes

Also new analysis, and also unrefereed. This split is the one that maps onto what Denmark is proposing, because Denmark is not lowering a rate that applies across the economy but moving a group of goods onto a reduced rate. The paper's data distinguishes changes to a generally applied standard rate from changes to a reduced rate applied to selected goods.

Rate category	Increase	Decrease	Reform observations
Generally applied (economy-wide)	0.394 (0.063)	0.120 (0.038)	2,003
Reduced rate (targeted)	0.275 (0.069)	0.021 (0.036)	976

Both arms are well powered. The pass-through of a targeted rate cut is 2.1 percent and is not distinguishable from zero; the pass-through of an economy-wide cut is 12.0 percent. The difference between them is statistically significant, at $p = 0.010$.

This result matters for a second reason, which Section 1 does not have room for. Eight of the eleven food VAT decreases in the sample were economy-wide standard-rate cuts that happened to cover food, rather than food-targeted reforms — the United Kingdom's December 2008 cut is the clearest case. A reader could fairly object that the food estimate in A.3 is therefore not identified off the kind of policy Denmark is considering. The category split answers that objection and reverses its direction: broad cuts are the *generous* case, and the targeted kind Denmark proposes passes through by less, not more.

I record one analysis that I ran and discarded. Splitting reforms by breadth directly — by how many commodity groups changed at once — leaves nine events in the targeted arm and an

estimate of 0.142 with a standard error of 0.141. It is uninformative, and it points the other way. The category split is the correct construction and the breadth split should not be quoted.

A.5 What can be ruled out

Section 1 states ceilings rather than confidence intervals, and this is the arithmetic behind them. Each cell tests the hypothesis that pass-through equals the stated value.

Hypothesised pass-through	Food decrease (0.242)	All commodities (0.064)
Zero	not rejected ($p = 0.200$)	rejected ($p = 0.032$)
25 percent	not rejected ($p = 0.965$)	rejected ($p < 0.0001$)
40 percent	not rejected ($p = 0.401$)	rejected
50 percent	not rejected ($p = 0.171$)	rejected
60 percent	not rejected ($p = 0.058$)	rejected
75 percent	rejected ($p = 0.0074$)	rejected
100 percent	rejected ($p = 0.0001$)	rejected

For targeted reduced-rate cuts, pass-through of 10 percent or more is rejected at $p = 0.027$.

The honest reading of the first column is that the food data cannot tell this committee what food pass-through *is*. It can tell the committee what it is *not*, and full pass-through is not it.

B. What the estimation sample covers, and what it does not

The panel is 27 European Union member states from 1996 to 2015. Within it there are 2,979 commodity-level VAT changes, of which 366 are decreases; restricting to food and non-alcoholic beverages leaves 167 changes, of which 28 are decreases. Those 28 commodity-level decreases correspond to eleven distinct reforms in nine countries.

Country	Date	Commodity groups cut simultaneously
Italy	October 1997	3
France	April 2000	10
Ireland	January 2001	8
Ireland	January 2003	2
Hungary	January 2006	9
Portugal	July 2008	9
United Kingdom	December 2008	9
Finland	October 2009	6
Ireland	January 2010	9
Poland	January 2011	1
Latvia	July 2012	9

Five of the eleven are classified as temporary, which means the pooled food estimate in A.3 is carried substantially by temporary reforms. That is the same objection this memo raises against the studies in the Ministry's table, and it applies to my food estimate too. It does not

apply to the all-commodity estimate, which rests on 366 decreases, or to the category split in A.4.

Neither of the two reforms the Ministry treats as showing complete pass-through is in this data. Norway is not a member of the European Union, so Gaarder’s 2001 reform is excluded by construction. Sweden contributes 19,548 observations but no reform observations at all: its food VAT cut took effect on 1 January 1996, and the earliest reform this panel can identify is 1997, because the event study requires a pre-period. Sweden is a pure control country throughout.

These two bodies of evidence are therefore disjoint rather than contradictory. On the two reforms most like Denmark’s, my data is silent — and it is worth saying plainly that Sweden’s own auditor is not.

C. The Ministry’s Table 1, entry by entry

The eight rows concerning food or fruit and vegetables, as printed in the reply, with what the underlying study reports.

Study	Country	Direction	As recorded in Table 1
Bernardino et al. (2025)	Portugal	Reduction	“Full pass-through” — no figure
David (2012)	Czech Republic	Increase	“Full pass-through” — no figure
Forteza et al. (2025)	Spain	Increase and reduction	“Substantial / full” — no figure
Fuest et al. (2025)	Germany	Reduction	70 percent
Gaarder (2019)	Norway	Reduction	“Full pass-through” — no figure
Nipers et al. (2019)	Latvia	Reduction (fruit and vegetables)	88 percent
European Commission (2023)	Spain	Reduction	“Substantial” — no figure
Swedish National Audit Office (2018)	Sweden	Reduction	“Full pass-through” — no figure

Three observations follow, and they are descriptive rather than critical. Two of the eight rows carry a number, and those two numbers are 70 and 88 percent, neither of which is complete pass-through. One row records a VAT *increase* in a table assembled to inform a reduction. And the eight rows cover seven reforms in seven countries, because Forteza and the European Commission both examine the Spanish measure of January 2023.

The Ministry’s own footnote concedes that the German, Spanish and Portuguese studies concern temporary changes.

D. References

Studies in the Ministry's Table 1

Benedek, D., De Mooij, R. A., Keen, M., and Wingender, P. (2015). *Estimating VAT Pass Through*. IMF Working Paper WP/15/214.

Benzarti, Y., Carloni, D., Harju, J., and Kosonen, T. (2020). What Goes Up May Not Come Down: Asymmetric Incidence of Value-Added Taxes. *Journal of Political Economy* 128(12), 4438–4474.

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Bernardino, T., Duque Gabriel, R., Quelhas, J., and Silva-Pereira, M. (2025). The Full, Persistent, and Symmetric Pass-Through of a Temporary VAT Cut. *Journal of Public Economics* 248, 105416.

David, P. (2012). Distribution of the Increased Tax Burden for Agricultural Products and Food in the Czech Republic. *Agricultural Economics (Zemědělská ekonomika)* 58(5), 239–248.

Forteza, N., Prades, E., and Roca, M. (2025). Analysing the VAT Cut Pass-Through in Spain Using Web-Scraped Supermarket Data and Machine Learning. *SERIEs* 16(1), 137–189.

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Nipers, A., Upite, I., Pilvere, I., Stalgiene, A., and Viira, A.-H. (2019). Effect of VAT Rate Reduction for Fruits and Vegetables on Prices in Latvia: Ex-Post Analysis. *Agraarteadus* 30(1), 25–31.

Amores, A. F., Barrios, S., Speitmann, R., and Stoehlker, D. (2023). *Price Effects of Temporary VAT Rate Cuts: Evidence from Spanish Supermarkets*. European Commission, Joint Research Centre, JRC132542.

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Also relied on in this memo

Benzarti, Y., and Carloni, D. (2019). Who Really Benefits from Consumption Tax Cuts? Evidence from a Large VAT Reform in France. *American Economic Journal: Economic Policy* 11(1), 38–63.

Benzarti, Y., Garriga, S., and Tortarolo, D. (under review). Can VAT Cuts and Anti-Profitteering Measures Dampen the Effects of Food Price Inflation? Evidence from Argentina. **Working paper**.

Harju, J., Kosonen, T., and Nordström Skans, O. (2018). Firm Types, Price-Setting Strategies, and Consumption-Tax Incidence. *Journal of Public Economics* 165, 48–72.

De Samvirkende Købmænd (2026). *Sverige viser vejen*. Copenhagen.

Skatte- og Vækstministeriet (2026). Press release of 10 June 2026 on the meeting with the retail sector. `cited_papers/SKM_2026-06-10_pressemeddelelse.txt`

E. What I have not been able to establish

This section exists because a memo that states ceilings should also state its own.

The new estimates in A.3 and A.4 are not published and have not been refereed. They use the published data and the published code, and A.2 shows the code reproduces the published results, but they carry none of the assurance that peer review provides. They should be read as what they are.

The food estimate cannot establish that food prices fall. Zero is not rejected. Everything Section 1 draws from it is a ceiling.

The Swedish price-monitoring finding of April 2026 reaches me second-hand. Section 3 reports that an independent monitoring firm found the cut had reached the shelves in full one month later. That finding comes through the Danish grocers' association's citation of it, and I have not seen the underlying report. Section 3 is written so that nothing turns on it: the point being made there is about horizon, and it would survive if the finding were confirmed in full.

I have not verified what Danish retailers undertook. An earlier draft attributed a pass-through undertaking to named retailers. The source traced back through a television summary of a paywalled interview, so I removed it. The Ministry's press release of 10 June names no retailer and records no undertaking and no monitoring commitment, and that release is all Section 5 now relies on.

The Swedish National Audit Office report was read in the audit office's own English summary, not in the full Swedish original. Every passage quoted in Section 6 appears in that summary.

Gaarder was read in the 2016 working paper, not the 2019 *Economic Journal* article. I quote only findings that are unlikely to have changed, but no page or figure reference should be cited to the published version without checking it.

Norway's rate history after 2001 — that food remained on a reduced rate, now 15 percent against a standard rate of 25 — is taken from secondary VAT guides rather than an official Norwegian source. Nothing in the memo turns on it.

The Argentine study is under review, not published, and Section 3 describes it without a citation for that reason.

On the long run I am relying on two case studies, not on the average, and Section 4 says so. Our full-sample estimates lose precision beyond about eighteen months, and at that horizon we cannot reject that the cumulative pass-through of increases and decreases is equal. The claim that asymmetry persists rests on Finnish hairdressing and on the Hungarian standard rate, both chosen because they are large changes in settings where prices are otherwise stable.