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The impact of climate information on milk demand: Evidence from a field experiment

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ABSTRACT

It has been suggested that carbon labelling of food, on voluntary or non-voluntary basis, could reduce emissions of greenhouse gases. However, there is limited empirical evidence on the influence of such labels on consumer purchases. The purpose of this study was to investigate whether voluntary carbon labelling affects the demand for milk. A randomized field experiment was conducted in 17 retail stores in Sweden, where a sign provided consumers with qualitative information about the carbon impact of climate-certified milk. The results suggest that the sign increased the demand for the climate-certified milk by approximately 6–8%, and the result is robust to alternative model specifications. The effect is entirely driven by large stores, such as supermarkets. We find no statistically significant impact on total milk sales, and the dataset is too small to verify the consequences for other milk brands. The effect on the demand for the labelled milk is short-lived.

Introduction

Food consumption accounts for a large proportion of the global greenhouse gas (GHG) emissions (Steinfeld et al., 2006). In Europe, nearly one-third of the consumption-based GHG emissions originate from the food sector (Tukker et al., 2006). Current trends point towards increased demand for food with large environmental impacts, but changed consumption patterns can lower GHG emissions substantially (Carlsson-Kanyama and González, 2009; Carlsson-Kanyama and Lindén, 2001; Duchin, 2005; Weber and Matthews, 2008).

Carbon labelling of consumer products could potentially reduce GHG emissions (Dietz et al., 2009; Vandenbergh et al., 2011). Successful voluntary labelling schemes might relieve the pressure on governments to introduce more stringent policy instruments, such as environmental regulation and carbon taxation, which could meet substantial resistance from different interest groups due to the associated private costs. Private and public actors seem highly aware of the potential gains of carbon labelling. In a survey of 544 eco-labelling schemes across the world, Gruère (2015) finds that the number of carbon labelling schemes has grown rapidly since 1990, compared to labelling schemes with other environmental focus, and currently constitute more than one-tenth of all eco-labelling schemes.

The ability of carbon labels to significantly reduce carbon emissions ultimately depends on consumer response to labelling. The central question is whether labelling leads to increased consumption of goods with small carbon emissions at the expense of goods with large emissions. Studies show that the impact of eco-labelling is determined by several factors such as label design, the type of product labelled, and how and where the product is marketed (Cohen and Vandenbergh, 2012; Hainmueller and Hiscox, 2012; Hallstein and Villas-Boas, 2013; Onozaka et al., 2010; Vlaeminck et al., 2014). These factors are typically affected by whether the labelling system is voluntary or mandatory, and private or public (Cohen and Vandenbergh, 2012; Sedjo and Swallow, 2002).

The purpose of this paper is to investigate if an in-store information sign on a voluntary carbon labelling scheme has the potential to alter consumer demand for climate-friendly milk in the short run. A randomized controlled field trial (RCT) is carried out in 17 grocery stores around the city of Uppsala in Sweden. The trial allows for isolation of the effect of carbon *information* on consumption (rather than the effect of *advertising* low-carbon foods) and thereby provides an estimate of the impact of carbon labelling on consumer demand for milk. We investigate the direct effect on climate-certified milk, substitution effects on other milk products, and the dynamic effects across time. Causal parameter estimates suggest that in-store information increased sales of the

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climate-certified milk by 6–8%. The treatment effect is stronger for larger stores, which means that the overall market impact is closer to the upper bound. We find no lagged effects of labelling. The increase in the sales of climate-certified milk is likely to be compensated for by reductions in the sales of either other milk brands or other products, but the dataset is too small to verify the substitution patterns.

A growing number of studies use field experiments to identify the impact of eco-labelling on demand for consumer products. Some of these studies carry out RCTs across multiple stores. For example, Hainmueller et al. (2015) derive the impact of a Fair Trade label on demand for coffee, showing that sales rose by 10% when coffee was labelled as Fair Trade, and Hainmueller and Hiscox (2012) investigate the response to an environmental label on jeans, showing that purchases by female shoppers in retail stores increased by 8%. Other studies carry out experiments in a single grocery store. For example, Vanclay et al. (2011) labelled 37 different products to indicate embodied GHG emission using a traffic light colour system and showed that sales of the least carbon intensive products increased by 4%, whereas sales of the most carbon intensive products fell. A few studies apply quasi-experimental approaches using data from multiple stores. Hallstein and Villas-Boas (2013) analyse demand response to the introduction of a traffic-colour sustainable sea-food advisory label, which was first introduced in two randomly chosen stores and then fully implemented in all ten stores belonging to a supermarket retail chain. The results show that consumers reduced their overall purchases of seafood through a reduction in demand for the yellow-labelled category. Kortelainen et al. (2013) apply a quasi-experimental approach to market data with an aim of quantifying the price-premium on carbon-labelled detergents but find no evidence of such a premium. Other in-store experiments involve recruitment of participants among store customers, such as Vlaeminck et al. (2014), where it is shown that a colour scale label, which indicates multiple sustainability attributes, significantly raised purchases of the most sustainable products. Finally, Koppel and Schulze (2013) recruited participants in cafés at German universities and asked them to choose between buying Fair Trade coffee and giving a direct donation to the aims of the Fair Trade mark. Their results indicate that the product's public good attributes were more important for the willingness to pay a premium for Fair Trade coffee compared to its private good attributes.

A small number of studies have investigated consumers' stated preferences for carbon-labelled food products. The results in Echeverría et al. (2014) show that consumers are willing to pay 29% more for fluid milk and 10% more for bread that is provided with a carbon footprint label. Comparing willingness-to-pay for different labels, Caputo et al. (2013) conclude that consumers' prefer a CO_2-label to a food-miles-label on tomatoes, and Van Loo et al. (2014) show that consumers are willing to pay more for a free range or animal welfare label on chicken compared to a carbon footprint or organic label.

Several studies investigate revealed preferences for eco-labels on food using hedonic price models and either scanner data or data collected in food stores. There appears to be no applications to carbon-labelled food, but the most relevant articles analyse organic labelling, showing a substantial price premium on milk, fresh fruit, vegetables, and jarred baby food (Kiesel and Villas-Boas, 2007; Lin et al., 2008; Maguire et al., 2004). Real market data are also used to identify the impact of the dolphin-safe tuna label (Teisl et al., 2002) and the Nordic Swan label (Bjørner et al., 2004), where the latter is used on non-food everyday commodities.

Given our interest in actual consumer behaviour, we follow Hainmueller et al. (2015) and Hainmueller and Hiscox (2012) by carrying out an RCT on a sample of multiple stores to identify the impact of a food carbon label. When compared to studies that make use of a single time series of scanner data (e.g., Bjørner et al., 2004; Teisl et al., 2002) or use a quasi-experimental approach (Hallstein and Villas-Boas, 2013; Kortelainen et al., 2013), the randomized assignment of treatments across both weeks and stores allows us to estimate causal effects of labelling without control variables. Similar to Hainmueller et al. (2015), Hainmueller and Hiscox (2012), and most of the stated and revealed preference studies mentioned above, we analyse the substitution between products that are provided with an eco-label and close substitutes that are not. Limitations to our study include lack of knowledge about consumers' pre-experimental perceptions about the labelled product's climate characteristics and lack of data on consumer characteristics and behaviour in different stores and store types.

The remainder of the paper is structured as follows: Section 'Research design' presents the experimental design, Section 'Estimation and data' explains the econometric approach and the data, Section 'Results' presents the results, and Section 'Robustness' describes the robustness of our findings. Finally, concluding remarks are made in Section 'Concluding remarks'.

Research design

Experimental setting

The RCT was conducted in 17 Coop grocery stores that sell climate-certified milk. Coop is a grocery retail group accounting for approximately 20% of the Swedish grocery retail market (KF, 2012). The stores included in the trial vary significantly in size and turnover because supermarkets, mid-size shops, and also convenience stores are included. The stores are spread out over a relatively large region around the city of Uppsala, including rural, suburban and metropolitan areas throughout Uppsala and Stockholm Counties. Market shares are skewed: the descriptive statistics in Table 1 show that the largest 20% of the stores in our sample are supermarkets, and milk sales in these supermarkets amount to 80% of all milk sales.

Our study focuses on the demand for unflavoured milk of the brand Sju Gårdar ("Seven Farms"), which is a local economic association for milk producers. Fluid, unflavoured milk is a suitable choice of product when studying the demand effects of environmental labelling because it is a relatively standardised commodity with no significant differences in flavour or quality between various brands (Kiesel and Villas-Boas, 2007).

Since 2010, the milk from Sju Gårdar has been certified according to the Swedish standards for Climate Certification of Food (CCF). The CCF is a voluntary labelling scheme that requires certified food producers to strive towards a significant reduction of GHG emissions by focusing on the production choices with the largest climate impact (CCF, 2010). A requirement for accreditation to a carbon label is that the producer already has another quality certification.[3] The milk from Sju Gårdar fulfils this requirement because it is organically certified as well.

Most existing carbon labelling schemes provide consumers with quantitative information about the amount of GHGs emitted during the product's life-cycle (see, e.g., Vandenbergh et al., 2011), such as the British Carbon Reduction Label. Some schemes, however, only provide consumers with a logo that states that the product is certified, thereby indicating that producers are committed to make a particular effort to reduce GHG emissions from production (Czarnezki, 2011). The Swedish CCF label applies the latter approach. The treatment design builds on this specific labelling

[3] From one or both of the Swedish third-party monitored labelling organizations Svenskt Sigill (Swedish Seal) and KRAV.

Table 1
Descriptive statistics of store sample.

	Share of stores (%)	Sales of all milk[a] (litres)	Sales of climate milk[a] (litres)	Sales of organic milk[a] (litres)	Sales of convent. milk[a] (litres)
Region					
Rural	35.3	1795	161	52	1583
		(770.6)	(67.7)	(39.2)	(774.3)
Sub-urban	29.4	6026	794	277	4955
		(4374.5)	(649.3)	(205.6)	(3825.3)
Metropolitan	35.3	2475	341	146	1988
		(1050.1)	(154.3)	(109.5)	(1819.8)
Store size					
Convenience store	23.5	1196	308	84	804
		(194.6)	(108.3)	(56.4)	(87.0)
Mid-size store	53.0	1906	256	90	1560
		(704.9)	(174.3)	(66.6)	(666.7)
Supermarket	23.5	8453	861	357	7235
		(2597.5)	(716.7)	(196.3)	(2046.2)

[a] The unit of the sales of milk presented is the average per week during the 4-week experimental period. Standard deviations in parentheses.

scheme because the aim is to investigate the potential impact of a voluntary labelling scheme.

Arguably, using a voluntary labelling scheme for a specific product as a basis for the treatment, rather than a general carbon labelling scheme, strengthens the external validity of the study. The reason is that a general scheme, such as the one studied by Vanclay et al. (2011), is not likely to be introduced on a wider scale due to the large transaction costs associated with verification of the products' climate impact. Furthermore, Kortelainen et al. (2013) highlight the importance of investigating carbon labels that only convey qualitative information, due to the cognitive difficulties for consumers to process complex quantitative information.

Treatment and reference groups

The study uses a simple intervention to estimate how the demand for climate-certified milk is influenced by the presence of climate information. The intervention consists of two different signs, in the format of 18×13 cm, attached to a shelf in close proximity to Sju Gårdar's medium-fat milk. The treatment sign explains that Sju Gårdar's milk is climate certified. The control sign is identical, but without the climate-related information. This procedure implies that the treatment effect captures the response to environmental information *per se* rather than the reaction to marketing signs (Carpenter et al., 1994). Hence, the treatment impact measured in this study pertains to the addition of climate information to a label. Throughout the remainder of this article, we denote this as a carbon-labelling effect.[4]

The factorial structure of the two treatments, T0 (control) and T1 (treatment), is displayed in Table 2. The treatment of interest, T1, builds on the design of T0 but adds the information that the milk from Sju Gårdar is climate certified. Images of the signs used in the experiment can be found in Appendix (Fig. A1). Notably, the information signs include a URL to a webpage that informs about the CCF standards and validates the claim of certification.

The researchers instructed the stores' personnel to alternate the signs weekly, following a fully randomized scheme over a four-week period. The random assignment of treatment weeks was stratified to make sure that the treatments were balanced across both weeks and stores, to achieve maximum precision in a regression with store and week fixed effects. The randomization scheme is displayed in Appendix (Table A1). As observed, the treatment assignments exhibit a bell-shaped distribution across stores (one

[4] The concept of a labelling effect differs somewhat between studies; sometimes, the term refers to the effect of using a label compared to having no label at all.

Table 2
Factorial structure of treatment signs.

	T0	T1
Logo-type	*Seven Farms*	*Seven Farms*
Product	*Milk*	*Climate-certified milk*
Implication of CCF	–	*"We have committed to decrease our climate impact"*
Validity of CCF	–	*URL to webpage with information about the CCF standards*

store was assigned four treatment weeks; four stores were assigned three treatment weeks; seven stores had two treatment weeks; four stores had one treatment week; and one store had zero treatment weeks). By contrast, the treatments are uniformly distributed across weeks. This implies that there is enough variation across space and time to permit time and fixed effects, while at the same time making sure that the total number of treatment weeks varies across stores.

Estimation and data

The main aim of the study is to estimate an immediate treatment effect on purchases of the climate-certified milk. We hypothesise that this treatment effect is positive. There are two supplementary hypotheses: the first supplementary hypothesis regards the substitution effects, that is, how the treatment will affect the demand for other milk brands, and the second supplementary hypothesis regards dynamic effects, that is, the persistence of the treatment effects. Given the nature of the studied product, we expect to find a negative impact on demand for other milk brands. Earlier studies indicate that the impact of information on food demand can be relatively short-lived (Piggott and Marsh, 2004). However, our four-week panel can only be used to test for dynamic effects in the very short-run. In spite of this limitation, we hypothesise that the effect of information about climate certification will be short-lived even within the studied time frame.

The collected data represent a balanced panel consisting of 28 days (four weeks) across 17 stores, implying 476 day-store cells. Our main hypothesis is tested using the following baseline specification:

$$\log(sales_{it}) = \beta \cdot treatment_{\mathrm{it}} + \alpha \cdot \log(turnover_{it}) + \boldsymbol{\theta} \cdot \boldsymbol{x_{it}} + \delta_i + \gamma_t + u_{it},$$

where $sales_{it}$ is the sales of climate-certified milk in store i at time t in SEK. The variable $treatment_{\mathrm{it}}$ is a dummy that is equal to one if the climate information was displayed in store i at time t and equal to zero otherwise. The coefficients δ_i and γ_t capture store and time fixed effects, respectively, and $turnover_{it}$ is total store turnover at time t and store i. The vector $\boldsymbol{x}_{it}$ is a vector of exogenous controls: the (logarithms of the) prices of the climate-certified milk item (Sju Gårdar's fat milk) and the closest substitute (Arla's fat milk),[5] and the number of customers; these controls will be used in complementary regressions but not in our preferred specification (see below for a discussion of their potential endogeneity). The primary purpose of the controls, including the fixed effects and the control for turnover, is to reduce the unexplained variation in the dependent variable and thereby improve precision. Finally, β captures the impact on demand of being exposed to climate information. The regressions are estimated with Ordinary Least Squares (OLS).

Daily scanner data are used for all sales. Our primary interest is in the total sales of climate-certified milk, that is, the aggregate sales of low-, medium- and high-fat milk. Altogether, the final dataset includes 23 different fluid unflavoured milk products (either low-, medium- or high-fat contents) with associated purchases and prices. To ensure comparison of relatively homogeneous products, extra low- and extra high-fat milk, non-lactose milk, non-dairy alternatives (e.g., soy and rice milk) and flavoured milk were excluded from the final dataset.

Although treatments were alternated weekly, the baseline regressions use daily data to exploit exogenous variation in turnover and the number of customers at the store-day-level, as well as day-fixed effects. Standard errors take into account serial correlation across days and are thus cluster-adjusted at the treatment level (the store-week-level). The results from both unweighted and weighted regressions are presented. The weighted regressions adjust the estimates to cell population, that is, the number of customers per day and store. Adjusting the estimates to be representative to the customer population provides a sense of the overall market impact of the experiment, which is further discussed in Section 'Results'.

Results

Baseline results: the impact of climate information on climate-certified milk

The descriptive statistics of the key variables are displayed in Appendix (Table A2). The baseline results are presented in Panel A–C in Table 3. Our most parsimonious estimation, Model 1 in Panel A, uses only store and week fixed effects and produces a point estimate of 5% (p-value 0.125). This effect is statistically insignificant at conventional levels. However, including a control for daily turnover in the store increases the explained variation from 0.77 to 0.83 and reduces the standard error of the treatment effect. With this model, the impact of treatment is 6.33% and statistically significant at the 5% level. Including day fixed effects and other controls has only a limited impact on the point estimates and the standard errors. The interpretation of the estimates is as follows: stores treated with a climate information campaign increased average daily sales of climate-certified milk by approximately 6%.

In Panel B in Table 3, we test for differential effects across store size. In these regressions, the sample has been divided in half: "large stores" indicates that the store had more customers than the median-sized store during the experiment. As seen, there is considerable treatment heterogeneity: the treatment effects appear to be completely driven by the larger stores. The treatment effect in larger stores is equal to 11–12%, which is statistically significant irrespective of whether turnover is used as a control or not. The point estimate for the smaller stores is −0.01 and is statistically insignificant.

The presence of treatment heterogeneity raises the issue of whether the average treatment effects should be estimated using population weights. As shown in Appendix (Table A1), the stores differ in outreach: the largest fifth of the stores in our data represent approximately 80% of the total milk sales. The 12% effect among the larger stores is thus representative for most of the market for milk, which the estimates in Panel A in Table 3 do not capture. When population weights are not used, β captures the effect of carbon labelling on the average store's milk sales, thus treating small convenience stores as equally important as supermarkets. By contrast, if population weights are used, β captures the effect of carbon labelling on the average customer's demand for milk. The estimates from the population-weighted regressions are arguably the more relevant for environmental policy because they capture the overall population effect on climate-certified milk demand (see DuMouchel and Duncan, 1983 for a general discussion of weights in the presence of treatment heterogeneity).

In the third panel, Panel C in Table 3, we therefore consider weighted regressions in which the data are weighted by the number of customers. The number of customers can be seen as a proxy for the population exposed to the information treatment. In this

Table 3
The impact of information on sales (log) of climate-certified milk.[a]

	(1)	(2)	(3)	(4)
	Panel A: Baseline OLS			
Treatment	0.0508	0.0633**	0.0652**	0.0674**
	(0.0328)	(0.0290)	(0.0298)	(0.0319)
R^2	0.7746	0.8318	0.8464	0.8471
Autocorr[b] (*p-value*)	0.7846	0.1577	0.1183	0.2211
	Panel B: Differential effects			
Treatment × Small store[c]	−0.0308	−0.0154	−0.0131	−0.0138
	(0.0293)	(0.0267)	(0.0284)	(0.0290)
Treatment × Large store[c]	0.112**	0.122***	0.124***	0.129***
	(0.0521)	(0.0442)	(0.0447)	(0.0473)
R^2	0.7758	0.8329	0.8475	0.8483
Autocorr[b] (*p-value*)	0.7587	0.1719	0.1366	0.2420
	Panel C: Population-weighted regression [d]			
Treatment	0.0672**	0.0791***	0.0808***	0.0817***
	(0.0322)	(0.0261)	(0.0268)	(0.0286)
R^2	0.8133	0.8643	0.8825	0.8831
Store effects	Yes	Yes	Yes	Yes
Week effects	Yes	Yes	Yes	Yes
Ctrl for turnover (log)	No	Yes	Yes	Yes
Day effects	No	No	Yes	Yes
Other controls[e]	No	No	No	Yes
Obs.	476	476	476	476

[a] Each panel-column represents a separate regression, based on daily data (17 stores; 28 days) observed from April 6 to May 3, 2013. Cluster-adjusted standard errors in parenthesis, clustered at the treatment level (week-store). Dependent variable is the log of the sales of climate-certified milk in SEK.
[b] The test for autocorrelation of the residual is based on Wooldridge (2002, pp. 282–283); xtserial with Stata (Drukker, 2003). The test is not defined for population-weighted regressions.
[c] "Large store" is equal to one if the total number of customers during the experiment is equal to or above the median; "small store" equals one if the total number of customers is below the median.
[d] The population-weighted regression uses the current number of customers (receipts) for each store and day as analytical weights.
[e] Other controls include log of the number of receipts and log of prices ("Sju Gårdar" and "Arla" fat milks).
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

[5] Prices were not observed directly but were calculated by dividing sales by quantity sold for each milk product, store and day. This means that we do not observe the price on days when the store had zero sales of the product, which is why we only include the prices of fat milk, which is the product where we have the most observations with positive sales.

specification, the estimates are more precise (typically significant at the 1% level) and somewhat larger. It is intuitive that the weighted estimates are larger than the baseline estimates in Panel A because they give more weight to larger stores in which the treatment effect is larger. The weighted estimates tell us that for the average customer, the carbon labelling campaign increased sales of climate-certified milk sold by approximately 6.8–8.0%.

Substitution effects

Because carbon labelling has a statistically significant effect on sales of climate-certified milk, consumers must compensate by reducing purchases of other goods, either other milk products, other foods, or other consumption goods. This section studies the impact of carbon labelling on the demand for other milk products within the studied time period. The model specification is the same as in Model 2 in Table 3 (Panels A and C), with week and store fixed effects and the logarithm of turnover as controls, but the outcome is now replaced with the logarithm of the sales of other milk products.

As observed in the last column of Table 4, the overall impact of the intervention on sales of all milk products is negligible. The weighted regressions (the second panel in Table 4) are precisely estimated, and we can reject quantitatively low effects (approximately 1% effects; the point estimate is equal to 0.00265, and the 95% confidence interval, not reported in the table, is between −0.01 and 0.015). It is thus possible that consumers substitute across milk products in such a way that the increase in the consumption of climate-certified milk is largely offset by reductions in other milk products. However, the sample is unfortunately too small to estimate differential effects with high statistical power for organic milk and conventional milk. Moreover, the baseline estimate of 6–8% increase in purchases of the climate-certified milk, presented in Table 3, is small enough to be compatible with a less than 1% increase in total milk sales. It is thus not possible to draw any conclusions on whether a carbon labelling campaign promoting climate-certified milk primarily will work through a reduction in other milk brands or through a reduction in other products than milk.

Dynamic impacts

The results in the previous sections capture short-run, immediate responses to carbon labelling. This section addresses dynamic impacts, that is, impacts of past treatments and their interaction with current treatments. Both positive and negative intertemporal effects are theoretically possible. Customers' preferences might be permanently affected by past treatments, but consumers might also switch back to their past consumption behaviour once the treatment week ends. Furthermore, customers could even react in a compensatory manner, buying less carbon-labelled milk after a treatment period than they would otherwise have done. Such negative intertemporal effects might arise due to pure income effects because carbon-labelled milk is more expensive but also due to psychological mechanisms such as "moral licensing" behaviour (Sachdeva et al., 2009).

Because treatment varied not only across stores but also across time, it is possible to evaluate dynamic effects in a straightforward manner. A first issue is the impact of lagged treatments on purchases of climate-certified milk. The question is if the effect of a treatment wears out rapidly over time or if it is more long-lasting. A second issue concerns the impact of accumulated treatments – that is, whether a prolonged exposure to treatment has an additional effect over the current treatment.

The dynamic analysis uses the baseline, population-weighted specifications with week and store fixed effects (i.e., the same specification as in Model 2, Panel C in Table 3). As observed in Table 5, there is limited evidence of significant dynamic effects. The lagged treatments do not have an economically meaningful impact given the current treatment (although the first lag is negative and statistically significant at the 10% level in one of the models). This indicates that the effects of climate information are not very persistent, even in a short time perspective.

The interaction term included in Model 4 in Table 5 measures the cumulative effect of treatment, that is, the effect of being treated with an information campaign in the current week, given that treatment was also carried out the week before. Although the coefficient is positive, in keeping with an increasing effect of exposure, the interaction term is statistically insignificant.

Overall, the results in this subsection indicate that the effects of carbon labelling are short-lived. However, given that the sample size is gradually reduced when using lags, the lack of statistical

Table 4
Impact on sales of other milk products.[a]

	Climate-certified milk	Other organic milk	Other conventional milk	All milk products
	Baseline OLS			
Treatment	0.0633**	0.0105	−0.0304	0.00785
	(0.0290)	(0.00975)	(0.0581)	(0.00689)
R^2	0.832	0.982	0.722	0.987
	Population-weighted regressions[b]			
Treatment	0.0791***	−0.00148	−0.0513	0.00157
	(0.0261)	(0.00831)	(0.0481)	(0.00654)
R^2	0.864	0.988	0.749	0.991
Obs.	476	426	476	476

[a] Each cell in the table represents a separate regression, based on daily data (17 stores; 28 days) observed from April 6 to May 3, 2013. Cluster-adjusted standard errors in parentheses, clustered at the treatment level (week-store). Dependent variable is the log of the sales of climate-certified milk and other varieties in SEK. All regressions includes store and week fixed effects and a control for (the log of) turnover. The regression models used in this table are identical to those used in Table 3, Column 2, Panel A and C, except for the choice of dependent variable.
[b] The population-weighted regression uses the current number of customers (receipts) for each store and day as analytical weights.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table 5
Dynamic impacts of information on sales (log) of climate-certified milk.[a]

	(1)	(2)	(3)	(4)
Treatment	0.0791***	0.102***	0.115***	0.0835**
	(0.0261)	(0.0339)	(0.0365)	(0.0384)
Treatment ($t-1$)		−0.0223	−0.0405*	−0.0348
		(0.0244)	(0.0218)	(0.0369)
Treatment ($t-2$)			0.00767	
			(0.0267)	
Treatment × Treatment ($t-1$)				0.0408
				(0.0567)
Obs.	476	357	238	357

[a] Standard errors in parenthesis, cluster-adjusted at the treatment level (week-store). Dependent variable is sales of climate-certified milk in SEK (log). All regressions include week fixed effects, store fixed effects, and a control for turnover (log). Regressions are population-weighted by the number of customers (receipts). All lags correspond to weeks; treatment ($t-1$) thus represents an indicator variable equal to one if the store had a treatment sign on display in the previous week, zero otherwise. Model (1) includes no lags and is thus identical to the benchmark estimate also found in, e.g., Table 3, Panel C, Column 2. In Model (2), the treatment variable lagged one week is added; Model (3) adds a two-week lag of the treatment variable; Model (4) considers an interaction effect between the current week's treatment status and that of the previous week.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

power becomes an issue, and we are not able to draw strong conclusions regarding the magnitude of these effects.

Robustness

Apart from using a fixed-effects specification, no other statistical commitments were made prior to the experiment. The choice of unit of analysis, the choice of dependent variable, and the choice of controls were made by the authors *ex post*. In this section, we therefore address the *ex post* research design choices made. To assure research transparency, a variety of specifications and robustness tests are presented together with the main results.

Using turnover as a control reduces the unexplained variation in the dependent variable. However, a source of concern is whether turnover is itself endogenous to treatment. It is theoretically possible that an information campaign attracts new customers to the stores, which would make interpretation of the causal mechanism more difficult. A similar story applies to the use of prices as controls: it is possible that stores change the price of milk in response to demand effects, which implies that prices are unsuitable as controls. Fortunately, we find no effect of treatment on (the logarithm of) total store turnover, the (logarithm of) the total number of receipts or the (logarithm of) price of milk (see Table A3, Models 1–4). The causal effect thus appears to run chiefly through in-store exposure to information. That prices remain invariant to treatment is expected given that the experimental effect does not seem to be large enough to affect the supply (there is no indication that stores run out of climate-certified milk on treatment days; see Table A3, Model 5). However, it must be emphasised that an assumption of constant prices is not necessarily warranted if the intervention is scaled up to a nation-wide campaign or policy, which might have general equilibrium effects.

In all regressions, we have taken logarithms of sales as the dependent variable. This eases interpretation and makes the regression less vulnerable to extreme values in the dependent variable. In Table A4, we re-estimate the baseline results but without taking logs of the dependent variable. The table is identical to Table 3, except the dependent variable and turnover are not logged. The treatment effect on sales when using the weighted regression is typically significant. Relating the size of the point estimates to the average sales of 741.8 (Table A2), the weighted estimate (Panel C of Table A4) is approximately 10%.

Table A5 considers the use of a lagged dependent variable in the regressions. The structure in Table A5 is identical to the structure in Table 3; the only difference is the inclusion of the lagged dependent variable. Because daily data are used but the treatment is changed on a weekly basis, the lagged dependent variable is defined as the log of sales of climate-certified milk seven days prior to the current observation. Notably, the inclusion of a lagged dependent variable increases the point estimates of the treatment effect compared to the baseline specification in Table 3, but the overall directions of the estimates are the same: large and significant for the larger stores and insignificant for the smaller stores.

Finally, we consider estimating the effects on the week-store level, rather than the day-store level. So far, we have consistently used daily data, allowing us to maximise precision by using time and store-varying controls, and all standard errors have been cluster-adjusted to take into account serial correlation of the error term within weeks. A more conservative approach is to aggregate the data and perform the estimation at the week-store level and either cluster-adjust at the store level with fixed effects (see also Hallstein and Villas-Boas, 2013) or to use first-differencing at the week level. Because 17 clusters is at the lower end of the number of groups necessary for cluster-adjusted standard errors to be consistent (see Bertrand et al., 2004), first-differencing is an alternative to fixed effects when the error term is serially correlated across weeks (if the error term follows a random walk, first-differencing with heteroscedasticity-robust standard errors yields consistent standard errors). As indicated in Table A6, the effects are statistically significant at the 5% level when using weighted estimation and significant at the 1% level when using first-differencing.

Concluding remarks

The purpose of this study is to investigate whether information about a voluntary carbon labelling scheme affects the demand for milk. By conducting a randomized controlled field trial in which consumers are in a close-to-natural purchase situation, we measure the average demand response to the introduction of a carbon label on climate-certified milk. The product information is manipulated experimentally through an in-store information sign placed on the shelf in close proximity to the climate friendlier milk. The trial is carried out across a variety of geographical locations, hence capturing a wide consumer group, which strengthens the general validity of the results. The findings suggest that the information sign has the potential to increase the demand for climate-certified milk by approximately 6–8%. We find no dynamic effects of labelling across time, which suggests that consumers' response to carbon labelling is short-lived.

Studies investigating the impact of eco-labelling on consumer demand use a variety of approaches including different types of product attributes considered in the labelling and differently detailed information about these attributes. Applications are made to products that vary with regard to price, price elasticity and the availability of close substitutes. Different store types are included, and experiments differ with regard to the degree of randomization and the similarity of experimental and everyday shopping conditions. To observe how this study adds to the understanding of the impact of eco-labelling, these aspects need to be considered.

In our experiment, the label provides qualitative information on the sustainability attribute of the product. We obtain a lower impact on demand from labelling compared to most earlier experimental studies that study a qualitative label (Hainmueller et al., 2015; Hainmueller and Hiscox, 2012; Hallstein and Villas-Boas, 2013), except for the study by Vanclay et al. (2011). Compared to studies that provide quantitative information on the label, we obtain a higher impact than Kortelainen et al. (2013), who use market data, but smaller than Vlaeminck et al. (2014), where consumers are aware of the fact that they are participating in an experiment.

The environmental attribute that we consider, climate impact, is a pure public good type. In that regard, our study differs from, for example, Hainmueller et al. (2015), where attributes with private good characteristics, such as the taste of different coffee brands, can be associated with the labelling. Economic theory predicts that due to the free-riding effect, consumers are more likely to take into account private good attributes, compared to public good attributes, and consequently, demand responses can be expected to differ. In practice, this can be complicated by the fact that consumers tend to have subjective perceptions about the attributes associated with a label (Janssen and Hamm, 2012; Wier et al., 2008). Care should therefore be taken when re-interpreting results from experimental studies such as ours in terms of the revealed willingness to pay for reductions in GHG emissions.

With regard to food category, our study is close to Echeverría et al. (2014) in which the stated preferences for carbon footprint labelled milk are investigated. Whereas Echeverría et al. (2014) obtain a high willingness-to-pay for carbon footprint-labelled milk, we obtain a modest response in purchased quantity. On an intuitive level, this difference can be expected given earlier experience from survey studies in which the stated support to a public good is costless (Gadema and Oglethorpe, 2011; List and Gallet, 2001; Murphy et al., 2005; Vermeir and Verbeke, 2006). However, the difference in impact of carbon labelling on willingness-to-pay and quantity demanded could also be related to the elasticities of supply and demand.

In principle, the consumers' budget constraint implies that increased purchases of an eco-labelled product are likely to be followed by reduced demand for other products. This matters for the net environmental impact as well as for retailer profits. However, in spite of the fact that we find no significant change in total milk demand, we are not able to quantify substitution patterns with certainty. A larger dataset would have probably facilitated the identification of such patterns. It appears that other studies had corresponding difficulties to measure substitution effects (see, for example, Hallstein and Villas-Boas (2013) and Vanclay et al. (2011)), but further research on the topic is likely to be valuable given the importance of substitution effects for the overall environmental impact of eco-labelling.

The treatment effect in our study is strongly related to store type. We obtain a larger impact in large stores, which usually have lower prices than small stores. This result contrasts with Hainmueller and Hiscox (2012) and Tanner and Kast (2003), where it is shown that environmentally friendly products are less demanded in supermarkets and stores belonging to the lower price segment. To better understand why the impact differs across store types, further data on store and consumer characteristics would be necessary. In addition, earlier studies indicate that the impact of eco-labelling can differ between regions (Yu et al., 2014) and that the pricing of eco-labels varies across retailer chains (Asche et al., 2015). Hence, the impact of adding climate information could potentially vary across regions and retailer chains, although we are not able to analyse this given that our dataset is limited to a single region and a single retailer chain.

Consumer trust in the overall eco-labelling scheme and consumer beliefs about the environmental impact of food purchases could also affect the demand impact of eco-labels (Cason and Gangadharan, 2002; Janssen and Hamm, 2012; Röös and Tjärnemo, 2011; Upham et al., 2011). Issues of trust and perceived environmental impact might affect the outcome of our experiment given the limited use of, and hence limited knowledge about, the Swedish carbon label. Moreover, producer-driven labelling schemes such as the one in this study usually convey the positive properties of the more environmentally friendly product, whereas the properties of the substitutes remain hidden. If consumers infer that the non-labelled substitutes are less environmentally friendly than the labelled goods, they are likely to choose the labelled product, *ceteris paribus*. If, instead, they believe that there is a risk that private labelling schemes overstate the environmental friendliness of the products, then they will be more reluctant to change their purchase behaviour. Broader labelling schemes, which inform about both good and bad products in a product group, such as the schemes investigated by Hallstein and Villas-Boas (2013), Vanclay et al. (2011) and Vlaeminck et al. (2014), are more likely to be provided by governments, retail chains, or non-governmental organizations. Such schemes are likely to be associated with higher costs for their development and for measuring labelling criteria. Also, mandatory eco-labelling schemes are not necessarily seen as more trustworthy than private schemes. For example, Janssen and Hamm (2014) show that the EU label for organic food is less trusted among German consumers that alternative private labels.

Finally, it is only efficient for producers, environmental organizations and governments to spend efforts on improving environmental performance and to develop environmental certification schemes if eco-labelling actually changes consumer demand. Further knowledge on the impact of carbon labelling on demand and how it is related to label type, product, and location is therefore likely to be valuable to all of these actors in food markets.

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Appendix A

See Fig. A1.
See Tables A1–A6.

Fig. A1. Control, T0 (to the left), and treatment, T1 (to the right).

Table A1
Distribution of treatment assignments.

Store	Week 1	2	3	4	Total
CE GRÄNBY	1	0	1	0	2
CF BOLÄNDERNA	1	1	0	0	2
CF NORRTÄLJE	1	0	0	0	1
CF STENHAGEN GEM	1	0	1	1	3
CK HALLSTAVIK	1	0	0	1	2
CK RIMBO	0	1	0	0	1
CK RINGGATAN FTG	0	0	1	0	1
CK STORVRETA FTG	0	1	1	1	3
CK TORBJÖRNSGAT	0	1	1	1	3
CK UPPSALA	0	0	1	0	1
CK VÄSTERTORG	1	0	0	1	2
CK ÖREGRUND FTG	0	1	1	0	2
CK ÖSTHAMMAR	1	1	1	1	4
CN ALMUNGE FTG	0	1	1	1	3
CN EKEBY FTG	0	0	0	0	0
CN LILJEGATAN	1	0	0	1	2
CN SUNNERSTA FTG	1	0	1	0	2
Total	9	7	10	8	34

Table A2
Sales and market shares in the sample.

	Small stores	Large stores	Full sample
	Average sales per store (SEK)		
Sju Gårdar	481.6	973.0	741.8
	(244.3)	(1114.9)	(863.2)
All milk	1712.7	5831.5	3893.2
	(409.1)	(4172.1)	(3675.9)
Organic milk	116.6	298.7	213.0
	(109.9)	(286.1)	(239.2)
Conventional Milk	1114.4	4559.8	2938.4
	(309.3)	(3058.7)	(2819.9)
	Market shares in sample		
Total sales, Sju Gårdar (SEK)	451,480	1,026,677	755,996
Total sales, all milk products (SEK)	1,661,651	6,325,180	4,130,578
Sales share, all milk products (%)	0.208	0.792	1
Sales share, Sju Gårdar (%)	0.305	0.695	1
N	8	9	17

Table A4
Baseline result, with sales in SEK as the dependent variable.[a]

	(1)	(2)	(3)	(4)
	Panel A: Baseline OLS			
Treatment	15.48	30.68*	32.34*	31.61*
	(18.64)	(15.91)	(16.41)	(17.71)
R^2	0.8349	0.9155	0.9283	0.9284
Autocorr[b] (*p-value*)	0.0653	0.1645	0.1642	0.1513
	Panel B: Differential effects			
Treatment × Small store[c]	−5.288	2.078	2.882	0.802
	(15.87)	(15.57)	(16.19)	(16.45)
Treatment × Large store[c]	31.03	52.11**	54.42**	54.99**
	(29.41)	(24.12)	(24.81)	(26.50)
R^2	0.8350	0.9157	0.9284	0.9285
Autocorr[b] (*p-value*)	0.0650	0.1622	0.1607	0.1493
	Panel C: Population-weighted regression[d]			
Treatment	48.72	74.26**	75.99**	76.46**
	(36.14)	(33.49)	(32.72)	(34.90)
R^2	0.8356	0.9193	0.9429	0.9430
Store effects	Yes	Yes	Yes	Yes
Week effects	Yes	Yes	Yes	Yes
Ctrl for turnover	No	Yes	Yes	Yes
Day effects	No	No	Yes	Yes
Other controls[e]	No	No	No	Yes
Obs.	476	476	476	476

[a] This table has an identical structure to Table 3; the difference is that the dependent variable is measured in sales in SEK rather than the logarithm of sales and that turnover, rather than log(turnover), is used as control variable. Each panel-column represents a separate regression, based on daily data (17 stores; 28 days) observed from April 6 to May 3, 2013. Cluster-adjusted standard errors in parenthesis, clustered at the treatment level (week-store).
[b] The test of autocorrelation of the residual is based on Wooldridge (2002, pp. 282–283); xtserial with Stata (Drukker, 2003). The test is not defined for population-weighted regressions.
[c] "Large store" is equal to one if the total number of customers during the experiment is equal to or above the median; "small store" equals one if the total number of customers is below the median.
[d] The population-weighted regression uses the current number of customers (receipts) for each store and day as analytical weights.
[e] Controls include log of receipts and log of prices ("Sju Gårdar" and "Arla" fat milks).
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table A3
Impact of treatment on exogenous variables.[a]

	(1) Turnover (log)	(2) Customers (log)	(3) Price of Sju Gårdar's fat milk (log)	(4) Price of Arla's fat milk (log)	(5) Climate-certified milk products sold out[b]
	Regular OLS				
Treatment	−0.0114	−0.00885	−0.0321	0.000786	−0.0407
	(0.0110)	(0.00640)	(0.0249)	(0.00296)	(0.0406)
	Population-weighted regression				
Treatment	−0.0116	−0.00911	−0.0407	0.00346*	−0.0340
	(0.0105)	(0.00782)	(0.0420)	(0.00205)	(0.0368)
Obs.	476	476	476	476	476

[a] Cluster-adjusted standard errors in parenthesis (at the week-store level). All regressions include week fixed effects and store fixed effects. Dependent variable in column header.
[b] "Climate-certified milk products sold out", ranges from 0 to 3; 0 if all three products (low, medium-fat and fat milk) from Sju Gårdar were in stock; 3 if all products were missing.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5
Baseline result (Table 3) when including a lagged dependent variable in all regressions.[a]

	(1)	(2)	(3)	(4)
	Panel A: Baseline OLS			
Treatment	0.0559	0.0677**	0.0695**	0.0723**
	(0.0347)	(0.0290)	(0.0299)	(0.0318)
R^2	0.7787	0.8329	0.8478	0.8484
Autocorr[b] (*p-value*)	0.9715	0.1679	0.1331	0.2498
	Panel B: Differential effects			
Treatment × Small store[c]	−0.00674	−0.00674	−0.00398	−0.00307
	(0.0260)	(0.0260)	(0.0281)	(0.0289)
Treatment × Large store[c]	0.119***	0.119***	0.121***	0.125***
	(0.0431)	(0.0431)	(0.0433)	(0.0456)
R^2	0.8338	0.8338	0.8487	0.8493
Autocorr[b] (*p-value*)	0.9408	0.1820	0.1523	0.2714
	Panel C: Population-weighted regression[d]			
Treatment	0.0733**	0.0811***	0.0822***	0.0833***
	(0.0347)	(0.0260)	(0.0265)	(0.0280)
R^2	0.8186	0.8643	0.8829	0.8834
Store effects	Yes	Yes	Yes	Yes
Week effects	Yes	Yes	Yes	Yes
Ctrl for turnover (log)	No	Yes	Yes	Yes
Day effects	No	No	Yes	Yes
Other controls[e]	No	No	No	Yes
Store effects	Yes	Yes	Yes	Yes

[a] This table has an identical structure to Table 3; the difference is that the models presented here include a lagged dependent variable in all regression models. Each panel-column represents a separate regression, based on daily data (17 stores; 28 days) observed from April 6 to May 3, 2013. Cluster-adjusted standard errors in parenthesis, clustered at the treatment level (week-store). The dependent variable is sales of climate-certified milk in SEK (log); the lagged dependent variable is the same variable observed one week before the actual treatment day.
[b] The test of autocorrelation of the residual is based on Wooldridge (2002, pp. 282–283); xtserial with Stata (Drukker, 2003). The test is not defined for population-weighted regressions.
[c] "Large store" is equal to one if the total number of customers during the experiment is equal to or above the median; "small store" equals one if the total number of customers is below the median.
[d] The population-weighted regression uses the current number of customers (receipts) for each store and day as analytical weights.
[e] Controls include log of receipts and log of prices ("Sju Gårdar" and "Arla" fat milks).
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table A6
Estimates for weekly aggregated data[a].

	(1)	(2)	(3)	(4)
	Fixed effects		First-differencing	
Treatment	0.0623*	0.0812**	0.0726**	0.0944***
	(0.0365)	(0.0365)	(0.0343)	(0.0342)
Weighted regression	No	Yes	No	Yes
Obs.	68	68	51	51

[a] Cluster-adjusted standard errors in parenthesis (at the store level) for the fixed effects regression; robust to heteroscedasticity when using first-differencing. All regressions include week fixed effects and store fixed effects. Dependent variable in column header. The specification used is the same as in Table 3, Model 1.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

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