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AN EVALUATION OF THE UK FOOD STANDARDS AGENCY'S SALT CAMPAIGN

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ABSTRACT

Excessive salt intake is linked to cardiovascular disease and several other health problems around the world. The UK Food Standards Agency initiated a campaign at the end of 2004 to reduce salt intake in the population. There is disagreement over whether the campaign was effective in curbing salt intake or not. We provide fresh evidence on the impact of the campaign, by using data on spot urinary sodium readings and socio-demographic variables from the Health Survey for England over 2003–2007 and combining it with food price information from the Expenditure and Food Survey. Aggregating the data into a pseudo-panel, we estimate fixed effects models to examine the trend in salt intake over the period and to deduce the heterogeneous effects of the policy on the intake of socio-demographic groups. Our results are consistent with a previous hypothesis that the campaign reduced salt intakes by approximately 10%. The impact is shown to be stronger among women than among men. Older cohorts of men show a larger response to the salt campaign compared to younger cohorts, while among women, younger cohorts respond more strongly than older cohorts.

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KEY WORDS: salt intake; salt campaign; cardiovascular disease; pseudo-panels; diet and nutrition

1. INTRODUCTION

Cardiovascular disease (CVD) is the leading cause of deaths globally, and elevated blood pressure (BP) is the most important risk factor for CVD. There is substantial evidence that high salt intake, spurred in recent decades by increasing consumption of processed foods, is a major contributing factor to elevated BP and, therefore, CVD (He and MacGregor, 2009). Additionally, excessive salt intake has been linked to progression of renal disease and stomach cancer, among other deleterious health effects.

In 1994 in the UK, the Committee on Medical Aspects of Food and Nutrition Policy (COMA), having reviewed the evidence on salt and BP, recommended a population intake target of no more than 6 g of salt a day. In 2003, the Scientific Advisory Committee on Nutrition (SACN) reviewed fresh evidence and endorsed the COMA recommendation. Against this backdrop, the Food Standards Agency (FSA) launched its salt intake reduction campaign in the last quarter of 2004, with a two-pronged strategy. The first was an awareness campaign, the most prominent aspect of which was a series of advertisements featuring 'Sid the Slug', a giant gastropod character warning the public of the dangers of too much salt. In following years, the salt message was also communicated in the agency's 'talking foods' and 'full of it' campaigns, in addition to a series of

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partnerships programmes with institutions working towards similar goals, involving peer education, social cooking projects, etc. (Levy, 2009) The second part of the strategy was to work with the food industry to encourage product reformulation.

The campaign is reported to have been quite successful at raising awareness, as well as encouraging reformulation. Within a year, the proportion of people expressing awareness of the need to not exceed 6 g a day had risen from 3% to 34%. Within 3 years from the launch, salt content of processed food in supermarkets had dropped by 20% to 30% (He and MacGregor, 2009). However, the effect on actual intake has proven harder to pin down. The main available evidence on changes in salt intake during the period is from a comparison of 24-hour urinary sodium analyses from 2000–2001 and 2008 (FSA, 2008). The 2000–2001 National Diet and Nutrition Survey (NDNS) in the UK estimated the average salt intake at 9.5 g per day. The FSA conducted a urinary sodium survey among 692 adults in the UK in 2008 and computed the average intake to be 8.6 g per day. This suggests an approximately 10% reduction during a period that covers the campaign. However, this has been called into question by McCarron *et al.* (2009) who examined data from the entire series of urinary sodium surveys collected across the UK since 1984 and argued that there has been minimal variation over the period, concluding that the evidence does not support the ability of public policy to influence salt intake.

A comparison of average intake at relatively distant points in time does not provide adequate appreciation of the actual impact of a flagship programme with potentially large health consequences. Changes to socio-demographic determinants of salt intake, changes in relative prices of salty foods and policy-independent time-specific shocks could all have contributed to observed changes. This short paper aims to reduce the gap in knowledge about the true effects of this important policy intervention[1] by controlling for price changes, income and education level changes, and demographic group-specific effects (region, age cohorts and sex) while examining the trend in urinary sodium in the initial one-way fixed effects model estimated, and subsequently adding control for year-specific shocks in the two-way fixed effects model estimated. It also contributes to addressing a broader gap identified by Traill, *et al.* (2010), who noted a surprising paucity in a rigorous evaluation of health eating policies, in Europe, on behaviour and intakes. We use spot urinary sodium sample data from the Health Survey for England before and after policy introduction, construct pseudo-panels and match food price data from another dataset to overcome data difficulties, and estimate panel data models to provide insight into policy effects.

2. DATA AND METHODS

Datasets with dietary information available in the UK with some continuity through the decade, such as the Expenditure and Food Survey (EFS), report food consumption from which salt intake may be inferred based upon conversion tables. However, indications show that industry reformulation, with regard to salt, occurred rapidly during this time. Available conversion tables are thus likely to be out of date. Additionally, under-reporting is frequently a problem in such surveys, and the presence of any trends in under-reporting would affect the reliability of estimates.

Data from a series of 24-hour urinary sodium surveys conducted by the FSA are available. However, these are not ideal from the point of view of the econometric policy evaluation that we wish to undertake. Firstly, they are scattered irregularly through the 2000–2010 decade; whereas, ideally, we would want samples from multiple years before and after policy implementation. Secondly, samples are relatively small in some of these surveys (e.g. the England survey of 2005–2006 is from 530 individuals). Thirdly, changes in the values of socio-economic and demographic variables, such as income and education, may drive some of the observed variation in these samples, but information on these variables is not available from these datasets. However,

[1]Other relevant work to note in this regard is an unpublished report to the Food Standards Agency by DiFalco (2008) that includes a structural break analysis of the effect of salt campaign publicity on weekly sales of selected TESCO products identified as high in salt content. This report found some evidence that rounds of the publicity campaign were followed by reduced sales volumes.

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an entry point for evaluation is available through spot urinary sodium measurement provided at the individual level by the Health Survey of England (HSE). Spot urinary sodium levels have been measured by the HSE since 2003 for a subsample of the overall HSE sample. There is one key issue in using these: it is well known that spot urinary sodium measurement is prone to error (compared to 24-hour samples) because of variation in urinary sodium levels of individuals within a day. However, separate studies by the HSE (HSE, 2003) and by the Joint Health Surveys Unit in Scotland (JHSU, 2007) investigating this issue both found that spot urinary sodium levels do correlate adequately with 24-hour measurements and that 'the results support the ability of a single spot urine sample to differentiate between subgroups of the population in the same way as the 24-hour sample' (HSE, 2003, p. 189).

Given the measurement error issue and the indication from previous studies that spot samples are most useful in differentiating between subgroups rather than individuals within the population, we are naturally led to the consideration of analysis based upon group-level averages. Pseudo-panel methods (Deaton, 1985; Verbeek, 1995) aggregate individual or household-level data into group-level averages, e.g. on the basis of age cohort, region of residence, etc. Where only repeated cross-sectional data are available, as in the case of the HSE, rather than panel data where the same individuals/households are followed over time, this form of aggregation allows the same units to be followed over time, albeit at a group rather than the individual/household level. Thus, if the researcher is willing to aggregate the data to group level (or, as in our case, such aggregation is considered advisable) for pseudo-panel analysis, it is possible to gain the advantages afforded by panel data, e.g. control for cross-sectional and time-specific heterogeneity, aspects likely to be important in any food policy evaluation.

On the basis of the subsamples from the HSE (cross-sectional waves 2003–2007) with urinary sodium readings, we constructed a pseudo-panel dataset. The subsample sizes from the HSE that were used in forming the pseudo-panel groups in each year are as follows: 2003:1668; 2004:2840; 2005:4643; 2006:8844; and 2007:4269.Pseudo-panel groups were defined on the basis of birth cohort, gender and area of residence. A key consideration was to ensure a reasonable number of individuals within each group. On this basis, we used the following specifications: birth cohorts, 1909–1940, 1940–1950, 1951–1960, 1961–1970, 1971; and later, gender, male and female; area of residence, north, south and midlands. Thus, we have 30 pseudo-panel observations per year over 5 years, 2 years of which (2003 and 2004) are prepolicy[2] and 3 years (2005–2007) are post-policy introduction.[3] On average, there are 121 individuals per group.

The HSE does not collect food price information and, therefore, we supplemented HSE data with food price information derived from the EFS. Two price indices were estimated, one for food in general and one for a 'salty food' category. These estimates are based on EFS unit values, which are computed as the ratio between expenditure and purchased quantities.[4] The price estimation procedure is based on the rationale that unit values embody both an actual price component and a quality choice component[5] (see e.g. Deaton, 1997, p. 284). On the basis of the works by Deaton (1988, 1990) and Crawford *et al.* (2003), our procedure for inferring prices from unit values assumes that variation in actual prices stems from spatial and time differences only. Further details regarding the multistep procedure are available from the authors upon request. These derived prices that vary by month and region were averaged to match the structure of the pseudo-panel data set.

Ideally, policy analysis would be able to define a treatment and control group so that estimation strategies, such as difference-in-differences methods, could be applied. However, alternative approaches are required in the frequent circumstance when the entire population is exposed to the policy, and there is no treatment group that can be specified (Capacci and Mazzocchi, 2011), as is the case with the salt campaign. We, therefore,

[2]Because the campaign started with advertising spots towards the end of 2004, our maintained assumption is that 2004 is prepolicy and 2005 is post-policy.

[3]Although it would have been desirable to have more years of data for analysis, urinary sodium data collection in the HSE was limited to these years.

[4]The definition of the 'relatively salty foods' category is based on the nutrient content table that accompanies the EFS data set. Foods exceeding the threshold of 3 mg of sodium per 100 g of product were classified as 'salty foods'.

[5]In a given period of time, the same food product is generally available at different quality and price levels (e.g. different points of purchases, special deals related to quantity, etc.). Besides, EFS unit values for food categories reflect an aggregation effect, as the 'salty food' basket for different households reflects a different composition of relatively more expensive or cheaper products.

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follow the strategy of Lee and Jones (2004) who analysed the introduction of global budgets of healthcare in Taiwan on dentist activity with the use of panel data. We first estimate a one-way fixed effects model with all 5 years of pseudo-panel data to examine the time profile of urinary sodium before and after policy introduction, having controlled for price changes, socio-demographics and group-specific effects (i.e. fixed effects specific to each birth cohort, region and gender combination). This is described in (1).

$$Y_{it} = \alpha + X_{it}\beta + \gamma D_t + v_i + u_{it} \tag{1}$$

Here, Y_{it} is group i's mean urinary sodium excretion in year t. X_{it} is a set of socio-demographic and price covariates. D_{t} is a dummy variable taking on a value of 0 for the prepolicy years (2003–2004) and a value of 1 for the post-policy years (2005–2007). The v_{i} is the group-specific effect, and u_{it} is the residual error term.

Then, we estimate separate prepolicy and post-policy two-way fixed effects models and compute the differences in the overall group-specific effects to gauge how the policy effect has varied across groups. Thus, each cross-sectional unit is treated as its own control group (Jones, 2009). This is written as follows:

$$\begin{aligned} Y_{it}^0 &= \alpha^0 + X_{it}^0\beta^0 + v_i^0 + \tau_t^0 + u_{it}^0 \\ Y_{it}^1 &= \alpha^1 + X_{it}^1\beta^1 + v_i^1 + \tau_t^1 + u_{it}^1 \end{aligned} \tag{2}$$

Here, the superscripts 0 and 1 indicate prepolicy and post-policy situations, respectively. The τ_{t} is a set of time-specific (year) effects, and the other notation is as under (1). We estimate the full set of the cross-sectional (group) and time-specific effects in (2) for the prepolicy and post-policy models in (2) by using dummy variable representations. On the basis of the work by Lee and Jones (2004), 'full group-specific effects' are computed for each model as the estimated group-specific effects, plus the mean of time-specific effects for the model. The group-specific change induced by policy is then the difference between the prepolicy and post-policy full group-specific effects.

Table I shows the summary of statistics.[6]

3. RESULTS

Results from the one-way fixed effects model are shown in Table II. High education appears to have no discernible influence on urinary sodium levels because the estimated coefficient is statistically insignificant. The income dummy variables, however, are both significant at least at the 10% level, and the positive signs for the low income and medium income categories indicate that urinary sodium excretion is higher in these categories compared to the omitted high income group. For example, with the use of the expression given by Halvorsen and Plamquist (1983) for the interpretation of dummy variable in models with logarithmic-dependent variables, the coefficient of 0.33 for low income indicates that those in the low income category (less than £23 400) have about 39% higher salt intake than those in the omitted category, the highest income group (more than £60 000); all else held fixed. The (log) relative price of salt variable is insignificant, possibly because of the lack of adequate variation in relative prices, but has an intuitive sign indicating that an increase in the price of salty food relative to all foods acts to lower urinary sodium. Critically, the salt campaign variable is highly significant and suggests that, even after control for group heterogeneity, socio-demographics and price changes, salt intake in the years following the FSA campaign was lower than in prepolicy years. On the basis of the expression by Halvorsen and Palmquist (1980) for dummy variables in log-transformed dependent variable models, the salt campaign is computed to have reduced salt intake by approximately 10%, consistent with FSA claims.[7]

[6]It is worth reiterating that the average sodium levels from spot samples reported in Table I are not meant to be taken as accurate readings of sodium intakes, as discussed before. The analysis that follows is correspondingly couched in terms of changes over time and across groups.

[7]Given the small sample size, we found that parsimony was an overriding concern in the choice of variables for inclusion in our models. However, several different models with slightly differing sets of independent variables were estimated, and the policy/campaign effect remained robust.

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Table I. Means and standard deviations of variables used in estimation*

Year	Sodium	High education	Low income	Medium income	Relative price of salty food
2003	109.03	0.34	0.53	0.37	0.75
	(20.41)	(0.15)	(0.20)	(0.15)	(0.008)
2004	111.28	0.46	0.65	0.25	0.74
	(19.82)	(0.15)	(0.16)	(0.11)	(0.0004)
2005	99.94	0.39	0.44	0.42	0.75
	(22.17)	(0.14)	(0.20)	(0.13)	(0.003)
2006	97.49	0.41	0.45	0.41	0.75
	(20.00)	(0.14)	(0.20)	(0.14)	(0.002)
2007	94.16	0.42	0.43	0.39	0.75
	(19.39)	(0.15)	(0.20)	(0.13)	(0.003)
Whole sample	102.38	0.40	0.50	0.37	0.75
	(21.18)	(0.15)	(0.21)	(0.14)	(0.005)

*Variables are measured as follows: urinary sodium units, mmol/l from spot samples; and high education, low income and medium income are all originally dummy variables that became proportions when aggregated to form pseudo-panel data. High education, originally a dummy variable where age at which left full time education > 16 is coded as 1 and others (omitted category) as 0. Income was coded originally as low income, 1 if income less than £23 400; 0, if otherwise. Medium income, 1 if income between £23 400 and £60 000 (omitted dummy: high income > £60 000). Relative price of salty food, price of salty food relative to all food.

Table II. One-way fixed effects model estimates†

Variable	Coefficient	Standard error
High education	0.03	0.11
Low income	0.33**	0.16
Medium income	0.40*	0.20
Ln(relative price of salty food)	−0.56	1.48
Salt campaign	−0.11***	0.02

†Dependent variable is the log of urinary sodium excretion.
*Denotes significance at 10%, **denotes significance at 5%, ***denotes significance at the 1% level.

Note that the difference between average sodium excretion in prepolicy and post-policy years (as can be computed from Table II) is not substantially different from the 10% effect estimated by the model. Thus, adjustment for prices and socio-demographic covariates do not alter indications available from a simple examination of the time trend. This is perhaps not surprising given that the key covariate that might have displayed substantial variation within the period, the relative price of salty food, did not, in fact. vary much, as noted previously. Although the effects of changes in relative prices have not clouded policy impacts in this particular case, it was important to check for this in the analysis on the basis of the findings of Capacci and Mazzocchi (2011) that changes in the relative prices of fruits and vegetables masked the true effect of the five-a-day campaign in the UK.

By estimating separate two-way fixed effect models for the prepolicy and post-policy years as described in (2), all parameters, including group-specific intercepts, are allowed to be affected by the introduction of the salt campaign. The ‘overall’ group-specific effect within a period is then a combination of the group-specific intercept and the average time-specific effects in that period, and the group-specific policy effect is the difference between post-policy and prepolicy overall group-specific effects (Lee and Jones, 2004). Table III presents groupwise percentage change in urinary sodium implied by the two-way fixed effects models.

Apart from one small positive number, all other entries in Table III are negative, revealing that the salt campaign has lowered urinary sodium levels in almost all groups defined by combinations of birth cohort, gender and region in our pseudo-panel. The impact on women has generally been larger than on men, especially considering that women in the sample had lower average sodium excretion than men in prepolicy years. This is consistent with previous research showing that women are more likely to report salt-limiting consumption behaviour than men (Wardle, *et al*., 2004). Older cohorts of men show a larger response to the salt campaign compared to younger cohorts, with cohorts born before 1960 showing a 13% to 15% reduction

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Table III. Group-specific campaign effects (percentage change in urinary sodium level) from the two-way fixed effects precampaign and post-campaign models

Age cohort	Midlands	North	South	Average
Men				
1909–1940	−18.3	−12.3	−16.3	−15.7
1941–1950	−0.2	−16.2	−22.8	−13.1
1951–1960	−8.9	−16.0	−17.6	−14.2
1961–1970	−7.9	−1.8	−12.5	−7.4
1971 and after	−4.7	0.6	−11.0	−5.0
Average	−8.0	−9.2	−16.0	
Women				
1909–1940	−14.6	−17.6	−11.1	−14.4
1941–1950	−11.1	−11.6	−9.8	−10.8
1951–1960	−20.8	−9.1	−19.8	−16.6
1961–1970	−14.9	−20.0	−15.8	−16.9
1971 and after	−24.5	−28.1	−16.4	−23.0
Average	−17.2	−17.3	−14.6	

in comparison to a 5% to 7% reduction for cohorts born after 1960. However, among women, it is the reverse, with the younger cohorts (born post-1950) demonstrating larger policy impacts (16% to 23%) than older cohorts (10% to 14%). Although there are no major geographical differences in campaign effects on women, men from the south appear to have been much more responsive to the salt campaign than men in the rest of the country. The computed policy effect for men in the south is a reduction of 16%, compared to the 9% and 8% reductions observed in the north and the midlands, respectively.

4. DISCUSSION AND CONCLUSION

In this study, we have attempted to improve the evidence base of the impact of UK FSA's salt campaign by econometrically modelling spot urinary sodium readings from the HSE over the period of 2002–2007, controlling for socio-economic characteristics and relative prices. Our results provide support for the FSA'S claim for an approximately 10% reduction in salt intake as a result of the campaign. There are several caveats and limitations to be noted. Spot urinary sodium samples do not provide accurate direct measurement of sodium intake. However, previous research has shown that they correlate well across time and groups with the 24-hour samples that do indicate intake. Hence, our analysis is couched exclusively in terms of changes over time and across groups. Also, following pseudo-panel formation, our samples are small. As an economy-wide campaign, there is no control group definable for evaluating campaign effects. The number of years for which spot urinary sodium data are available from the HSE is limited, particularly for the prepolicy period. Consequently, we stress that our results are indicative, rather than conclusive, in nature.

A vibrant debate is ongoing in health research and action communities concerning the potential efficacy of public policy in regulating salt intake. Subject to the qualifications noted above, our results lend support to the argument that policy can indeed contribute to salt intake reduction. This raises the question as to how these results match with the conclusion reached by McCarron *et al*. (2009) that statistical analysis of all the 24-hour urinary sodium surveys conducted over two decades does not support pronouncements about FSA campaign success in securing a significant reduction in salt intake. McCarron *et al*. compute the overall mean and standard deviation (SD) of urinary sodium from the mean levels drawn from 13 different surveys spanning over two decades. These surveys range from city-level to regional-level to national-level samples. They then plot the overall mean $\pm$ 2SD, along with each of the 13 data points. Their conclusion that no support is found for FSA's campaign success pronouncements is (presumably) based on the 2001 and 2008 data points, on the basis of which the FSA claim is made, being within the $\pm$ 2SD range plotted. Our approach is however based upon the notion that it is important to control for potential variation in the socio-demographic profiles of the samples, as well as relative prices, in examining changes in the target variable and in isolating campaign effects.[8] Our results, as well as those of DiFalco (2008), provide additional, although not conclusive, support in the ability of such campaigns to achieve a significant reduction.[9] A notable recent development in the UK is the food industry's commitment, under the 2012 'responsibility deal', to further reformulate a range of food products to reduce salt content beyond the 2010 levels.

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CONFLICT OF INTEREST

The authors have declared that there is no conflict of interest. The article only uses publicly available secondary data. Thus, there are no particular ethical issues for consideration.

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[8]It is also worth noting that a statistically significant reduction of 10% is indeed possible within the mean $\pm$ 2SD range for urinary sodium plotted by McCarron *et al*.

[9]Note, however, that our investigation does not purport to shed any light on whether policy can induce further lowering of population intake, particularly below the 120 mmol/l (approximately 7 g) 'floor' identified by McCarron *et al*. (2009).

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