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US Consumer Inflation Expectations: Evidence Regarding Learning, Accuracy and Demographics

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1. Introduction and Rationale

US monetary policy post-1979, according to Clarida *et al* (1998), has shifted to proactively respond to changes in expected inflation. As stated by the current Governor of the Federal Reserve:

> "…wages and prices that are set for some period in the future will of necessity embody the inflation expectations of the parties to the negotiation; increases in expected inflation will thus tend to promote greater actual inflation. […] If expectations are not well tied down, inflationary impulses that are in themselves transitory may become embedded in expectations and hence affect inflation in the longer term. Therefore, an essential prerequisite for controlling inflation is controlling inflation expectations." Bernanke (2004)

Accordingly, there is an increasing body of research which attempts to understand the underlying determinants of these inflation expectations. It is a mistake to assume that only the expectations of professional forecasters influence the economy. As suggested above, and further articulated by the President of the Federal Reserve Bank of St. Louis (FRBSL), of similar importance to policy makers are the inflation expectations formed by individual consumers:

> "…the unfolding inflationary experience is most strongly anchored by how the public and financial market participants expect inflation to evolve. Well designed and articulated policy under such conditions can produce great outcomes. However, badly designed policies under the same conditions can produce disasters!" Poole (2004)

Monthly US consumer inflation expectations have been recorded since 1978 as part of the 'Survey of Consumer Attitudes and Behavior', managed by the Survey Research Centre (SRC) at the University of Michigan[1]. The aggregate, all-agent, year-ahead inflation expectation index published by the SRC has received considerable attention in literature which has generally sought to examine forecast rationality. Although this index is broadly representative of all US consumers, the implicit averaging of responses could lead to published results providing a misleading generalisation of the

[1] Reuters now have exclusive distribution rights on the release of new index-information.

forecast accuracy of specific groups of consumers. This is especially likely since, as shown by Souleles (2004), interviewee demographic characteristics are correlated with the accuracy of inflation expectations.

Although question wording and probes can and are used by the SRC to elicit considered responses, the variation of month-to-month responses is still significant, with some extreme responses (50% inflation and above, for example) suggesting a total misunderstanding of the concept of inflation by some individuals. Such temporal variation in forecast accuracy has received no attention in the related literature. However, as this survey contains a short-panel aspect, with potential reinterview six-months following initial contact, there is scope to examine whether agents can learn about inflation, and so improve their forecast accuracy from the first to second interview.

Learning is particularly relevant to central banks. If agents can and do learn about inflation, and learning results in forecasts becoming more accurate, central banks which pursue policies which stimulate learning will cause the implicit expectations built into wage and purchasing decisions to be more realistic. Corresponding cost-push inflationary pressures would therefore reduce and the potency of central bank inflation rate policy would improve.

In summary, this paper uses individual survey response data from the SRC to analyse forecast accuracy and learning jointly, conditional on demographic characteristics. Results will also show whether learning takes place within certain groups, potentially suggesting ineffectiveness in current non-targeted inflation news dissemination practice[2]. Quantifying and improving the effectiveness of monetary and particularly inflation policy outcomes in such cases would require consideration of demographic characteristics of the targeted population.

No related literature has considered jointly the issue of learning and demographic heterogeneity. Most studies involving this data use aggregate monthly data, which

[2] The author was unable to find any documents discussing dissemination practice from either the Bureau of Labor Statistics or the US Federal Reserve. As such, it is assumed that no procedure exists to manage the dissemination of information by the media.

completely ignores demographic heterogeneity and the differences between first and second interviews responses. Souleles (2004), Bryan & Venkatu (2001a, 2001b) are the only papers to use the individual survey response data to control for demographics, but none consider learning when investigating forecast accuracy.

As a brief preview, results suggest there is significant demographic and temporal heterogeneity in inflation forecasts. Having accounted for the affects of attrition, outliers and unobserved heterogeneity, gender, race, age, household income, and adult child residential composition are all found to be statistically significant characteristics to initial forecast accuracy. Across all groups, second interviews are significantly more efficient than initial interviews. This learning effect is enhanced by gender, age, and children in the household, but reduced, if the respondent is in a top income household.

The paper is set out as follows. The SRC survey data is discussed in Section 2 in relation to the analysis of the affect of demographics on inflation expectations and learning. Section 3 presents the model hypothesised to explain learning and forecast accuracy. Issues related to estimating this model are described in Section 4, while Section 5 presents the results. Finally, conclusions are drawn in Section 6. An appendix is included containing additional graphs and tabulations. A detailed discussion of the SRC survey, and the methods used to form build from the individual survey response data the dataset used in this article, is available from the author on request.

2. Data

Individual consumer year-ahead inflation expectations are recorded for around 500 US consumers each month by the SRC survey. Respondents are asked:

> "During the next 12 months, do you think that prices in general will go up, or go down, or stay where they are now?"
>
> "By about what percent do you expect prices to go (up/down) on the average, during the next 12 months?"

A "stay where they are now" response is probed to clarify whether the respondent means that prices will not increase, or that prices will continue to increase at the same rate. In quantifying future inflation, respondents are given the option of responding as cents on the dollar, rather than as a percentage. Combined, these measures improve response quality, and reduces inflation expectation (rate) question non-response below 10% (for the raw data). No correction is made for this question non-response propensity: it is implicitly assumed a random subset of the sample.

The distinctive aspect of this survey is its short rotating panel design: 40% of the monthly sample are re-contacts from six-months previously, the remaining 60% being initial interviews from a random sub-sample of the telephone-owning mainland US population. Although data is available on a monthly basis since January 1978 until December 1996, this is truncated to start at January 1983. Data anomalies in the pre-1983 period, including non-standard reinterview horizons and inconsistencies in reinterview demographic characteristics for 14 non-consecutive months, are the primary reason for this truncation.

Responses from the inflation questions are combined to form a (directional) quantification of year-ahead expected inflation, $E_{i,s}^{t}\pi^{t+12}$, where $E_{i,s}^{t}$ is the expectation of an individual survey respondent $i = 1,\ldots,N$, formed either in the first or second interview $s = 1,2$, during the survey dated $t = 8301,...,9612$, for the inflation rate twelve-months hence, π^{t+12}. Individuals without a first interview inflation rate response, or those who have not attritted but fail to report a second interview inflation rate response, are dropped from the sample[3].

Expected future inflation ($E_{i,s}^{t}\pi^{t+12}$) responses are sometimes extreme, the size of which, in relation to the majority of responses, may unduly affect estimated models. It is decided, following the reasoning of Curtain (1996), to censor all inflation expectation responses at +50% and -10%, which affects less than 1% of all responses in each tail.

The survey records a wide range of individual demographic and interview characteristics. Selecting the characteristics which are routinely recorded, we group the responses into the categories shown in Table 1. This table also contains further information regarding base categories and category codes, used in subsequent analysis. Detailed information about these variables, the transformation we apply to the raw-survey data, and further insight into the survey itself, is available in an associated survey appendix, on request from the author.

Initial interview demographic characteristics are assumed to govern behaviour[4]. As seen from the data-tabulation in the appendix, “Don’t Know” and “Not Available” missing response codes for these variables form a small proportion of the total monthly sample. Where possible, a missing first interview demographic characteristic is substituted by a non-missing second interview demographic characteristic. If this is not possible, the individual is dropped from the sample.

Second interview attrition is generally around 30% for most of the sample period. Clearly, this may cause a non-random sub-sample in the second wave, resulting in

[3] Following this rule, around 10% of the complete sample is purged. Note, this does not rule is not applied to the January to June 1983 reinterviews, as the initial interviews in this period are dropped and so are irrelevant, nor to the July to December 1996 period first interviews, where reinterviews do not exist. See the appendix for further information.

[4] As such, people who move, for example, are treated as if resident in the region recorded for their first interview. Therefore, a change in demographic group between interviews is implicitly assumed to have no effect of forecasting behaviour.

potentially biased and inefficient model parameter estimates. Existence of and correction for attrition will be dealt with in the next section[5].

Table 1: Available Interview and Demographic Characteristics

Characteristic	Categories	Base	Remaining Category Abbreviations
Age	18-34, 35-54, 55-97	35-54	Age1, Age3
Income	Lower 20%, middle, upper 20% of US (Census) Income Distribution	Middle	Inc1, Inc3
Race	White, non-white	White	NonWhi
Gender	Male, female	Male	Female
Adults in household	1 (survey respondent), 2+	1 (respondent)	Adults
Children in household	0, 1+	0	Children
Region of residence	North Central (Mid-West), North East, South, West	North Central	NE, S, W
Education	No high school diploma, high school diploma, some college, college degree	High school diploma	NoHSDeg, SomeCol, CollDeg
Marital status	Married, separated, divorced, widowed, never married	Never married	Married, separated, divorced, widowed
Household head status	Household head, non-household head	Household head	Non-head
Interview length	Over 60 minutes, under 60 minutes	Under 60 mins	Interview length >60
Interview interruption[A]	Interview interrupted and required one or more call-backs, not interrupted	Not interrupted	IntIntrpt
Interview break-off[B]	Incomplete interview (break-off), complete interview (no break-off)	No break-off	IntBrk
Number of calls from coversheet	1, 2+, 5+, 10+	1	Call2, Call5, Call10
Initial refusal on coversheet	No, yes	No	CovSR
Interviewer	Experience 1 (<0.05% interviews) Experience 2 (0.05 to 0.2%) Experience 3 (0.2 to 1%) Interviewers (>1%) #1-16	Most experienced interviewer #16	Exp1 Exp2 Exp3 #1-15

Table notes:
A: Interview interruptions have only been recorded since the February 1984 survey. Prior to this date, this variable is set to no-interruption.
B: Interview break-offs were not recorded in the March 1983, June 1983, December 1983, or March 1984 surveys. For these surveys, this variable is set to no-break-off.

[5] The small proportion of individuals who provide a response in the first interview for a year-ahead inflation expectation, but do not in the second interview are dropped from the sample, and are not treated as having attrited. As the factors which determine complete attrition from the survey as opposed to question attrition may be different, these cases are not used for the estimation of attrition probability.

Macroeconomic condition variables are considered for the subsequent analysis of inflation expectations. Using NBER business cycle dates[6], a business cycle contraction indictor variable is generated, which in our sample, is only activated for the August 1990 to March 1991 period. Using the percentage change of the annual CPI-U price index, actual inflation at the time of the formation of the expectation is calculated. Lagging this series by one month is used to proxy inflation news[7]. Other macroeconomic variables considered individually include the Federal Funds rate and unemployment rate, but these are discounted since they are found to capture less temporal variation.

[6] Using NBER business-cycle news release dates versus actual NBER business-cycle dates has little effect on estimated results. Actual NBER business-cycle dates are used as these are thought more likely to capture shifts in general economic condition sentiment.

[7] The release of CPI-U data by the BLS occurs approximately two-weeks following the month to which it refers (see BLS Fact Sheet 94-1). As surveys are conducted throughout the survey month, this will cause some respondents in any particular survey month to have access to more recent inflation news than others. This issue is assumed unimportant.

3. Modelling Learning

Forecast bias (or rationality) is explored by examining the average forecast error, the difference between actual inflation less the forecast made 12 months previously of that inflation, $\pi^{t} - E_{i,s}^{t-12}\pi^{t}$. Forecast errors, however, cannot be used to quantify forecast accuracy changes from first to second interview, as this implicitly involves a differencing of forecast errors. As an example, consider an initial overprediction followed on reinterview by an underprediction of identical size. This would result in a non-zero forecast error difference despite no actual improvement in forecast accuracy, in terms of the new forecast error being closer to zero. Using the absolute value of each individual's forecast error, accuracy improvement would be correctly quantified as zero.

The absolute value of the difference between actual inflation less the forecast made 12 months previously of that inflation, $\left|\pi^{t} - E_{i,s}^{t-12}\pi^{t}\right|$, henceforth referred to as the absolute forecast error, is used to quantify learning. Properties of this variable are considered in greater detail in section 4.4. Using the absolute forecast error as the dependent variable, and assuming that agents forecast a common, national inflation rate[8], learning and forecast accuracy can be investigated by using a range of macroeconomic, demographic characteristic and survey specific explanatory variables.

General macroeconomic sentiment shifts are captured in two ways. The business-cycle contraction indicator variable, interacted with all other regressors, is used to allow for differing responses in the period of economic contraction. The inflation news variable is used to capture whether forecast errors are dependent on the size of confirmed inflation experience.

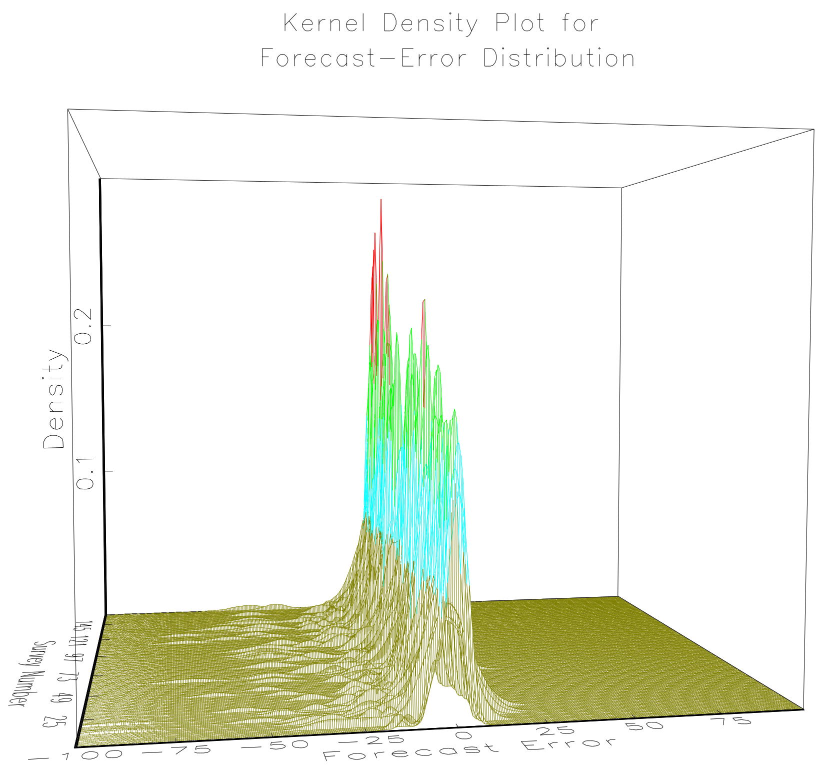
The affects of demographic characteristics are allowed to vary over time through interaction of the main groups with the survey date dummies[9]. All other, non-

[8] This is implicitly assuming that the CPI-U is representative for all demographic groups.
[9] It is not feasible to interact all combinations, only the main groups, of demographic characteristics with the survey date dummies, as each interaction involves 166 extra regressors.

demographic time-specific sentiment is assumed captured by the (non-interacted) survey date dummies.

Learning effects are measured using a second interview indicator variable, interacted with the demographic and business cycle state indicator variables. This variable is not interacted with survey month dummies, as it is assumed the learning-effect is time-invariant. The assumption of time invariance of learning effects can be partially justified by looking at the distribution of quantified "stay where they are now: same-rate" first-interview responses[10]. Such responses implicitly indicate what the agent assumes to be the current, prevailing inflation rate, since $E_{i,s}^{t}\pi^{t+12} = E_{i,s}^{t}\pi^{t}$. The distribution of the bias in such responses, $\pi^{t} - E_{i,s}^{t}\pi^{t}$, as shown in Figure 1, is largely time-invariant: the level of learning is not driven by agents becoming more aware (or learning) at certain times, of the prevailing rate of inflation when first interviewed.

Figure 1: Same Rate Response Distribution[11]



[10] This response category accounts for between 5% and 25% of all quantified responses and assumes time-invariant non-demographic selectivity propensity to give a 'same-rate' response. It is also possible to examine bias in responses to a past-year inflation experience question, to quantify underlying inflation understanding. As this question is only asked between September 1980 and August 1985, the "same rate" response distribution is preferred due to its longer availability.

[11] The non-symmetric distribution is due to the lack of a same-rate down response category.

It is hypothesised that forecast accuracy is related to demographic characteristics, learning, macroeconomic and unobserved sentiment through equation (1).

$$\begin{aligned}\left|\pi^{t+12}-E_{i,s}^{t}\pi^{t+12}\right| = \gamma_0 &+ \boldsymbol{\gamma}_1\mathbf{demog}_i + \gamma_2\mathrm{BCC}_t + \gamma_3\pi^{t-1} + \gamma_4\mathrm{survey2}_s \\ &+\boldsymbol{\gamma}_5\left(\mathbf{demog}_i \times \mathrm{BCC}_t \times \mathrm{survey2}_s\right) \\ &+\boldsymbol{\gamma}_6\mathbf{time}_t \\ &+\boldsymbol{\gamma}_7\left(\mathbf{demog}_i \times \mathbf{time}_t\right) \\ &+a_i+\varepsilon_{is} \\ \text{for } s=1,2 \;\; i=1,\ldots,N,\;\; & t=8301,\ldots,9612\end{aligned} \tag{1}$$

In summary, $\mathbf{demog}_i$ contains individual demographic characteristics indicator variables. Macroeconomic effects are captured through, BCC_t, a business cycle contraction indictor variable[12], π^{t-1}, the actual inflation rate one-month previously, and hence the inflation news available at the time of the forecast. The remaining temporal sentiment is captured through the survey month dummies, $\mathbf{time}_t$, which contains indicator variables for each survey date. $\mathrm{survey2}_s$ is an indicator variable for whether the observation refers to a reinterview. a_i is an unobserved individual (fixed) effect, and ε_{is} is an assumed white-noise individual error term.

Since a constant, γ_0, is used in (1), there will be exact multi-colinearity if a complete set of dummy indicator variables are used for all groups. Base categories are defined, as shown in Table 1, and are chosen on the basis of the category from which differences have most interest. A base survey date is chosen for periods of business cycle expansion and contraction. The October 1995 survey is treated as the base in periods of business-cycle expansion, and is chosen on the basis that the inflation rate at this date is close to the average inflation rate for periods of expansion in order to minimise temporal variability in forecast accuracy[13]. The December 1990 survey month is the base for periods of business-cycle contraction, and is chosen on the basis of being the mid-point for the contraction period[14]. To avoid exact multi-colinearity

[12] Business cycle state can be both interview and individual dependent, as the first interview of an individual could be in a period of expansion, while the second is in a period of contraction.
[13] That is, that as macroeconomic variables are not adrift from average, individual demographic characteristics should dominate.
[14] The inflation rate peaks at this date, so business cycle effects should be strongest.

with the (survey constant) inflation rate, the indicator survey date dummy indicator variable for January 1993 is also dropped with this date chosen as inflation is close to its average over the entire sample.

Non-time specific demographic differences in forecast accuracy are captured by the estimated coefficients in $\boldsymbol{\gamma}_1$. Non-time specific forecast accuracy can be ranked relative to the base category; absolute forecast accuracy would require consideration of joint significance with survey date dummies, $\boldsymbol{\gamma}_6$ and associated demographic interactions, $\boldsymbol{\gamma}_7$.

The estimated coefficient on γ_4 quantifies the improvement or reduction in forecast accuracy for the base category for reinterviewed individuals. A significant negative coefficient would indicate learning. The differing effects of demographic characteristics on learning is quantified through estimated coefficients in $\boldsymbol{\gamma}_5$, for the coefficient relating to interactions involving an activated reinterview dummy. A significantly positive (negative) coefficient would require additional independent significance testing if a significantly negative (positive) coefficient on γ_4, base group learning, was estimated in order to identify the presence of overall, not just relative learning. Likewise for the interactions of demographic characteristics with the business cycle contraction indicator.

Base categories, and so regression constants, should be interpreted with care, as this represents the base category, in a specific base month, with **no past inflation**. As such, the regression constant should not be interpreted as the average forecast accuracy of the base group without also adding the temporal effects of past inflation (γ_3) and, if relevant, survey dates ($\boldsymbol{\gamma}_6$).

4. Estimation Issues

Four issues arise in the estimation of equation (1), which will be dealt with in turn in this sections:

i. Biased inefficient estimates due to second period attrition (subsection 4.1)
ii. Two-period individual level error correlation due to the (unknown) individual unobserved effect a_i in (1) (subsection 4.2)
iii. Possible heteroskedasticity in composite model disturbances (subsection 4.3)
iii. Distribution of absolute forecast errors is truncated non-normal (subsection 4.4)

4.1 Attrition

Although the SRC tries to ensure first interviews are a random sub-sample of the population, as mentioned in the previous section, because people can opt not to be reinterviewed, then the second wave sample may be non-random.

Methods to deal with attrition, or what Wooldridge (2002b) describes as *incidental truncation*, primarily depend on what the researcher is willing to assume about the drop-out mechanism. If the subset of observations which drop-out is a completely random sub-sample of a random sample, what is now commonly referred to as data *missing completely at random* (MCAR), then attrition can be ignored with no cost (see Rubin 1976).

If data are *missing at random* (MAR) in the sense that known recorded characteristics determine drop-out propensity, then Robins *et al* (1994) suggest correcting for the associated bias using inverse-probability weighting (IPW)[15]. Carpenter *et al* (2006) provide a straightforward exposition of the rationale behind IPW. As discussed by Tsiatis (2006) and Little (1995), if drop-out depends on unobserved current or future characteristics, what is sometimes referred to as *non-missing at random* (NMAR) or *observed at random*, correction is problematic and can be unreliable. As suggested by Little (1995), the sample selection model of Heckman (1979) can potentially deal

[15] The Heckman (1979) selection model (see footnote 16) can also be used in this situation, as suggested by Wooldridge (2002b). Carpenter *et al* (2006) compares IPW with a further method attrition correction method, multiple-imputation.

with such data[16]. As commented by Tsiatis (2006), since there is no way to test, using the observed data, whether the data are NMAR or MAR, and given the complexities and problems associated with modelling and correcting NMAR attrition, it is assumed drop-out is MAR. As such, attrition is corrected using IPW.

IPW weights are calculated as the inverse of the probability of an individual being observed, conditional on a set of auxiliary variables, which may or may not contain the set of regressors used in (1). A recent review of the survey data literature by Vandecasteele *et al* (2006) identified commonly recognized factors related to attrition as: age, gender, region, race, education, marital status, household size, income, interviewer, length of interview, labour force status, home ownership status. Table 1 shows that all except the final two indicators are available in the SRC dataset. Reinterview propensity is hypothesised as conditional on the available (first interview) factors suggested by Vandecasteele *et al* (2006), survey date indicator variables, a continuous inflation rate (macroeconomic sentiment) variable, and extra interview characteristics, as detailed in Table 1. All characteristics except interviewers and the survey month dummies are interacted with non-white (race) and female (gender) indicator variables, as this improves the explanatory power of the regression.

In summary, the Probit regression model (2) is estimated using maximum-likelihood[17]:

$$\begin{aligned}\text{reinterviewed}_i = \alpha_0 + \alpha_1 \mathbf{characteristics}_i + \alpha_2 \text{interviewer}_i + \alpha_3 \pi^{t-1} \\ \alpha_4 \left(\pi^{t-1} \times \text{nonwhite}_i \times \text{female}_i \right) + \alpha_5 \mathbf{time}_t\end{aligned} \tag{2}$$

where reinterviewed_i is an indicator variable for whether the individual is reinterviewed, $\mathbf{characteristics}_i$ is a matrix of demographic and survey characteristics indicator variables as in Table 1, all interacted with race and gender. interviewer_i is a matrix of interviewer indicator variables (not interacted with race or gender). For the

[16] The Heckman sample-selection model involves treating the bias caused by the attrition as a variable to estimate, a variable Heckman (1979) describes as the inverse-Mills ratio (IMR). Adding the IMR interacted with a reinterview dummy to (1) and correcting for the induced heteroskedasticity would remove coefficient bias in this situation. This correction is highly sensitive to model misspecification, however, and researchers can encounter problems of convergence using the maximum-likelihood version, or boundary-convergence, using the two-step, Heckit, procedure.

[17] Reinterview propensity is only calculated for periods with initial and possible matched reinterviews: the January to June 1983 reinterviews and July to December 1996 initial interviews are excluded.

reasons discussed in the previous section, the October 1995 and January 1993 survey date indicator variables are dropped to avoid exact multicolinearity. Estimation results obtained using Intercooled Stata8.2 (excluding the individual gender and race interaction terms) are shown in Table 2.

Results suggest attrition is related to demographic characteristics. Individuals who are more highly educated than average, those who are not heads of household, or those who are married have a higher propensity for reinterview. Households in the South or West regions, those with below average income, individuals who are aged 18-34 or who are below average educated, are non-white or divorced, all have a reduced propensity for reinterview. All the main demographic characteristics are jointly significant (test 1).

Interview characteristics are also important. Initial interviews containing interruptions or breakoffs are less likely to be re-observed. Initial coversheet refusal and five or more calls from the coversheet, reduce the propensity for reinterview. The reduction in reinterview propensity is further increased if there are more than 10 calls from the coversheet. These variables are jointly significant (test 2).

Interviewer effects are generally individually insignificant with only marginal significant effects for two of the more experienced interviewers compared with the most experienced interviewer, though jointly, interviewer effects are significant (test 3).

Characteristics which individually seem to have no affect on reinterview probability are inflation news, length of the initial interview, gender, and children or other adults within the household. Although not shown, interactions between gender, race and other demographic characteristics are generally individually insignificant[18]. Jointly these interactions are significant (test 5), and also when combined with the other demographic effects (test 6).

[18] The is marginal (5%-level) significance for the following interactions only: CovSR x Female (-ve); Separated x NonWhi x Female (+ve); Separated x NonWhi (-ve); Divorced x Female (+ve); Non-head x NonWhi x Female (+ve). There is stronger (1%) significance for IntBrk x Female (+ve) interaction.

Table 2: Probit Response Model Estimation Results

	Coef.	Std. Err		Joint Test	Test Stat.		Ref. No.
Inflation News	0.004	0.3362					
Inc1	-0.170	0.0389	***	$\chi^2_{(20)}$	224.46	***	(1)
Inc3	0.020	0.0279					
Adults	-0.033	0.0453					
Children	0.122	0.0764					
Children x Adults	-0.092	0.0802					
North East	-0.027	0.0317					
South	-0.056	0.0279	*				
West	-0.102	0.0316	**				
Age1	-0.109	0.0276	***				
Age3	-0.014	0.0335					
NoHSDeg	-0.133	0.0385	**				
SomeCol	0.101	0.0302	**				
CollDeg	0.175	0.0279	***				
Female	0.051	0.0878					
Non-head	0.109	0.0543	*				
Non-white	-0.421	0.1626	*				
Separated	-0.075	0.0730					
Married	0.136	0.0467	**				
Widowed	0.009	0.0710					
Divorced	-0.095	0.0460	*				
Interview Breakoff	-0.629	0.0977	***	$\chi^2_{(7)}$	264.98	***	(2)
Interview Interrupt	-0.210	0.0429	***				
Interview length >60	0.089	0.0833					
Calls 2+	-0.025	0.0326					
Calls 5+	-0.153	0.0272	***				
Calls 10+	-0.123	0.0326	***				
Initial Coversheet Refusal	-0.196	0.0348	***				
Interviewer Exp1	0.004	0.0473		$\chi^2_{(18)}$	32.14	*	(3)
Interviewer Exp2	0.014	0.0386					
Interviewer Exp3	0.017	0.0358					
Interviewer #1	0.148	0.0779					
Interviewer #2	0.046	0.0763					
Interviewer #3	0.038	0.0685					
Interviewer #4	-0.002	0.0685					
Interviewer #5	-0.016	0.0690					
Interviewer #6	0.002	0.0698					
Interviewer #7	-0.020	0.0630					
Interviewer #8	-0.027	0.0627					
Interviewer #9	-0.041	0.0610					
Interviewer #10	-0.110	0.0610					
Interviewer #11	0.145	0.0642	*				
Interviewer #12	0.027	0.0576					
Interviewer #13	0.057	0.0587					
Interviewer #14	0.087	0.0552					
Interviewer #15	-0.101	0.0495	*				
Constant	0.651	0.9180					

Additional Hypothesis Tests:				
Survey month dummies joint significance	$\chi^2_{(160)}$	703.73	***	(4)
Demographic interaction coefs. joint significance	$\chi^2_{(79)}$	123.42	**	(5)
All demographic & svy coefs. joint significance	$\chi^2_{(125)}$	1638.2	***	(6)
All coefs. (excl. constant) joint significance	$\chi^2_{(285)}$	2306.23	***	(7)

Notes: The equation estimated is given by (2) where $\mathbf{characteristics}_i$ is a matrix of demographic and survey characteristics indicator variables as in Table 1, all interacted with race and gender. Joint tests refer to coefficient (sets) significance (null is joint equality with zero). * denotes significance at the 5% level, ** at the 1% level, *** at the 0.1% level. A blank in this column indicates significance only at levels above 5%. Jointly, survey month dummies (not shown) and interviewer dummies are significant at the 0.1% and 1% level, respectively. A summary of category abbreviations used and respective base categories is shown in Table 1.

Although detailed results are not shown, attrition rates are also affected by the survey date, most particularly in the pre-May 1989 period. Jointly, these time-effects are significant (test 4).

The explanatory power of the model is relatively low, with a pseudo-R^2 of 4.92%[19]. Jointly, however, the model is explaining significant reinterview propensity effects (test 7). Although attrition is not completely described by available demographic or interview characteristics[20] this is to be expected, as some (perhaps large) proportion of attrition will be random (at least with respect to observable characteristics).

Estimation of equation (1) can therefore be corrected for attrition by weighting (second-interview) observations by the inverse of the probability of re-interview[21], according to the demographic and initial interview characteristics of that individual, as show in Table 2[22].

[19] The application of IPW by Inkmann (2005) does not recognise pseudo-R^2 as a measure of model adequacy.

[20] Other models were tested including those containing different sets of interactions, all of which had inferior explanatory power. Although slightly higher explanatory power is achieved if survey month dummies are also interacted with race, the model estimated is preferred on ground of parsimony.

[21] Although not used to calculate the IPW, second interview observations in the January 1983 to June 1983 period are weighted using the reobservation propensities calculated above.

[22] All characteristics, not just significant characteristics, are used to construct the weights.

4.2 Individual Level Error Correlation

Assuming that the individual unobservable effect, a_i in (1), is uncorrelated with any of the explanatory variables[23], treating this as a component of a composite error term for estimation purposes[24] will cause two-period (individual level) error correlation.

Applying GLS with such an estimated random-effects structured error covariance matrix will correct standard errors for the error correlation caused by the unobservable effect. Such a transformation implicitly assumes homoskedasticity of the both the error term in (1) and the unobservable effect.

4.3 Error Heteroskedasticity

Instead of using the more restrictive covariance structure implicit in the standard random-effects GLS approach, by using a correction similar to that proposed by White (1980), it is possible to accommodate the individual level error correlations and heteroskedasticity (of unknown form) for inference purposes, after ordinary least squares (OLS) estimation. An extension of a (White 1980) heteroskedasticity robust covariance matrix to the situation of individual error level correlation and heteroskedasticity has been proposed by Rogers (1991), and is employed here[25]. Such within sample (in this case individual-level) correlation structures can synonymously be referred to as clusters or primary sampling units (PSUs).

More specifically, the methodology requires obtaining full sample residuals, where OLS is used on the entire sample to estimate (unbiased) coefficients. These residuals, split into clusters, are interacted with the explanatory variables applicable to that cluster, to form a covariance matrix for each cluster. The full-sample

[23] This assumption would usually be tested by comparing a random-effects model with a fixed-effects model, using a Hausman test. However, since there are only two-periods the fixed-effects model is identical to OLS on a first-differenced equation, and since most explanatory variables are time invariant, such a regression model would difference out the very effects we are wishing to examine. Further, a fixed-effects model would effectively reduce the analysis to available-case analysis and would implicitly involve weighting responses from both interviews. This would reduce efficiency relative to using available first interviews with weighted second interview responses.

[24] Since the unobserved effect is not estimated directly.

[25] Such a correction can be implemented in Stata 8.0 using the "cluster(#)" option.

heteroskedasticity and cluster correlation robust covariance matrix is formed by summing these covariance matrices, as shown in (3).

$$\hat{V} = \sum_{i=1}^{N} \left(X_i' \hat{u}_i \right) \left(X_i' \hat{u}_i \right)' \tag{3}$$

where there are $i = 1,\ldots,N$ clusters (individuals) and $s = 1,\ldots,S$ observations (interviews) on each individual and where $\hat{u}_i$ is the $(S \times 1)$ vector of residuals applicable to that cluster, and X_i is the $(S \times k_i)$ matrix of the k_i explanatory variables applicable to that cluster[26].

Rogers (1991) provides simulation evidence to prove the general effectiveness of this correction except in situations where the size of any cluster is more than 5% of the total observations. This condition is not violated by the SRC dataset, since all clusters contain a maximum of two observations (an initial and possible re-interview).

4.4 Distribution Non-normality

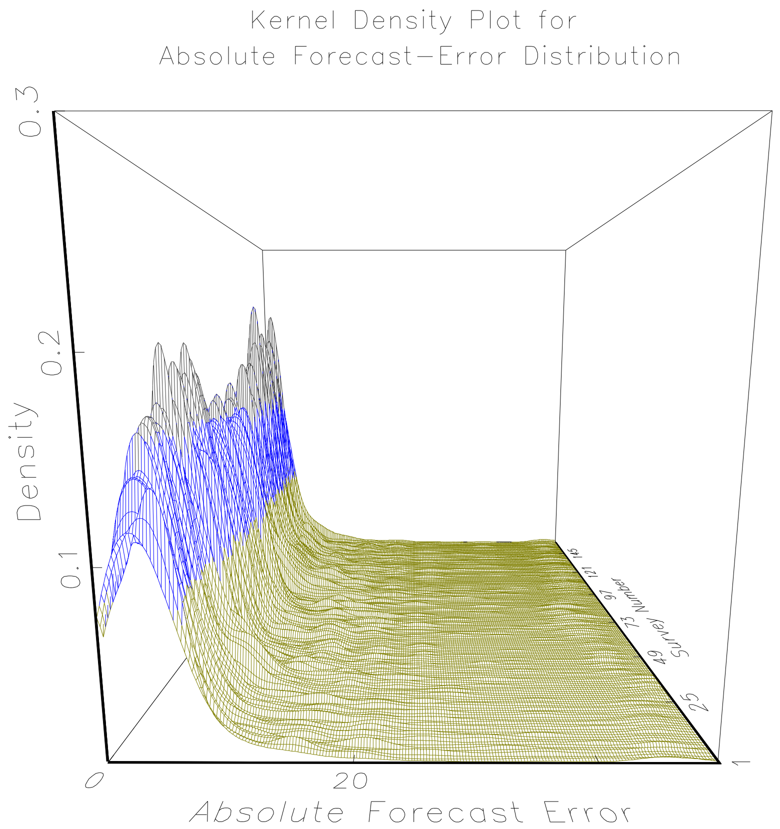
Treating year-ahead forecasts as a continuous variable[27], the distribution of first and second interview (non-censored) forecasts over time is not normal, being both positively skewed and having excess kurtosis (leptokurtic). Figure 7 in the appendix shows a kernel density plot of this distribution. Censoring this distribution at –10 and +50 will reduce, but not eliminate the excess kurtosis (21.04), and maintain a positive skew (3.57).

The forecast error is a horizontal shift of the year-ahead forecast distribution. The variance, and kurtosis for each survey month will remain unchanged, with skew of opposite sign but identical magnitude. As the proportion of under and over predictions each month remains relatively constant, the variance (37.42) and excess kurtosis (20.22) of the overall distribution remains virtually unchanged, with an overall negative skew (-3.46).

[26] This is a modified version of the Rogers (1991) formula. When S=1, $\hat{u}_i$ will be a scalar, and (3) will collapse to the usual White heteroskedasticity robust covariance matrix, equivalent to $X'\hat{\Omega}X$ where $\hat{\Omega}$ is a diagonal matrix containing $\hat{u}_i^2$ on the main diagonal.

[27] This is a simplification, since forecasts are somewhat discrete in nature, taking only integer values.

Figure 2: Absolute Forecast Error Response Distribution



The distribution of the absolute forecast errors, as shown in Figure 2, is a zero left-limit truncated version of the forecast errors distribution. Essentially the left tail of the distribution of forecast errors is flipped onto the right tail, resulting in no probability mass below zero.

It should be noted that this distribution is not due to negative responses being censored at zero due to unobservability or due to zero being a corner (optimal) solution for a large-proportion of the sample (Wooldridge 2002a). As such, zero left censored regression modelling techniques, such as a zero left censored Tobit, despite guaranteeing non-negative outcomes for all coefficient-regressor combinations, are not relevant in this situation[28].

[28] Following Greene (2003), in this context, such models would assume that the forecast error is affected by both an estimated probability of being above the censoring point and the full set of demographic variables. In this situation, however, the probability of being non-negative is one. There should be no distribution scaling due to the implicitly assumed negative forecast error probability.

Essentially, therefore, there are two sources of non-normality. The first is due to the non-normality of the underlying forecast error distribution. The second (compounding) factor comes from using a truncated version of this underlying distribution.

Truncated normal distribution maximum-likelihood modelling techniques critically rely on the underlying (non-truncated, in this case the forecast errors) distribution being normal. As already discussed, this is not the case, with the underlying distribution having excess kurtosis and negative skew. The failure of the normality assumption, particularly due to the excess kurtosis, invalidates standard truncated-normal regression techniques[29].

Non-normality in terms of excess kurtosis and skew will reduce the efficiency of least-squares inference relative to methods more robust to outlier effects, such as quantile (least-absolute deviation - LAD) regression techniques (Koenker and Bassett 1978). Relative to limited-dependent variable models, not explicitly accounting for the truncated distributional moments will also reduce efficiency and induce coefficient bias[30].

As already mentioned, modelling a truncated underlying non-normal distribution is not straightforward, and especially problematic since there are further issues of attrition, individual-level correlations and heteroskedasticity. Partial correction for outlier effects has been attempted by censoring extreme values, as previously discussed. Attrition, individual-correlations and heteroskedasticity are corrected as outlined above. Further correction for the truncated nature of the resulting distribution, however, is not undertaken, as we do not know of a technique which can account for all the issues encountered in this dataset.

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[29] Mixing draws from two normal distributions, one standard normal and one with a different variance, can induce kurtosis of a specific amount to the combined (mixed) distribution, by setting the mixing parameter or the alternative variance appropriately. Simulating such data for mixed distributions containing varying degrees of excess kurtosis, a mixing parameter of 0.5 with a variance in the second distribution of 15 will produce a distribution with excess kurtosis of 2.3, which is found to be large enough, in this context, for the truncated regression algorithm to fail.

[30] Greene (2003) discusses these points more fully, and shows how truncation from below (as in this case) will increase the estimated mean but reduce variance. Accordingly, such bias may also be corrected by treating the bias due to truncation as a variable to estimate, using the Heckman procedure.

In summary, equation (1) coefficients are obtained for the various demographic groups using inverse probability weighted OLS, so correcting for potential coefficient bias caused by attrition. (Asymptotically) efficient inference is achieved by replacing the standard OLS covariance matrix with a less restrictive version, robust to individual-level correlations and potential heteroskedasticity of unknown form. The usual random-effects assumption regarding the unobserved heterogeneity term being uncorrelated with all explanatory variables is assumed to hold. Henceforth, we refer to this estimation methodology as 'inverse probability weighted robust random effects'.

5. Results

Before estimating demographic learning effects, it is useful to quantify whether learning is prevalent regardless of respondent demographic characteristics. A simple regression model of the forecast error on the reinterview indicator variable and a complete set of survey month dummies (excluding a constant[31]) is used for this purpose, as shown by equation (4).

$$\begin{gathered}\left|\pi^{t+12} - E_{i,s}^{t}\pi^{t+12}\right| = \beta_1 \text{survey2}_s + \beta_2 \mathbf{time}_t + a_i + \varepsilon_{is} \\ \text{for } s = 1,2 \;\; i = 1,\ldots,N, \;\; t = 8301,\ldots,9612\end{gathered} \tag{4}$$

Estimating equation (4) using the robust IPW weighted random-effects methodology outlined in the previous section produces the result shown in Table 3 for the survey2 coefficient[32]. All results are obtained using Intercooled Stata8.2.

Table 3: Inverse Probability Weighted Robust Random Effects

	Coefficient	Std. Err.	
Survey2	-0.447	0.0320	***

Notes: *** indicates significance at the 0.1% level

On average, responses are closer to the actual year-ahead inflation rate by approximately 0.4 percentage points when an individual is reinterviewed, than when initially interviewed. This suggests that learning, *per se*, is not purely dependent on demographic characteristics, since, on average, all respondents improve their forecast accuracy from the first to second interview. However, the amount of improvement in forecast accuracy may be dependent on individual characteristics.

Returning to the more general specification of equation (1), it would be inefficient to test all possible combinations of *ceteris paribus* demographic characteristics in one regression model. Although incidence of each characteristic is not required in each survey month[33], interaction with a business cycle state indicator variable greatly reduces sample size in periods of business cycle contraction. As the sample consists

[31] The general level of forecast accuracy, therefore, is spread across all survey month dummies rather than a date-specific constant with temporal variations. This parameterisation has no effect on the survey indicator variable coefficient or its significance. This sample is identical to that used for all subsequent analysis.
[32] Sample of 46,920 individuals, interviewed over 168 months. Around 70% of this sample are interviewed twice yielding a total of 80,159 observations.
[33] Since survey months are not interacted with demographic characteristics.

mainly of white individuals from multiple occupancy households residing in the South, investigating forecasting differences for non-white single occupancy households in the West, for example, would be inefficient, as, particularly in periods of business cycle contraction, there may be few available cases.

The groupings investigated are: gender and race; age and income; children and adults[34], with results presented for each of these groups. All (non-base category) within group interactions are included. Interactions involving the remaining survey date coefficients and their corresponding significance will be presented graphically due to the number of coefficients involved. The section concludes with a brief analysis of the information contained in the non-interacted survey date coefficient estimates.

[34] Education effects are not investigated as these are likely to be highly correlated with income.

5.1 Gender and Race

For the results here, equation (1) is estimated where $\mathbf{demog}_i$ contains gender and race individual indicator variables. The base category is white males. This model is estimated, as discussed above, using inverse probability weighted robust random effects estimation. Gender temporal effects are found to be insignificant[35], so the model is re-estimated without these interactions. Table 4 reports these results (not listing individual survey month dummies).

Inflation news, used to partly capture non-survey specific macroeconomic sentiment, is not a statistically significant component of forecast inaccuracy. This may be explained by agents having thresholds of perception set above the average rate over this period, especially since inflation is relatively low compared to pre-1983 levels (see Figure 8 in the appendix). Furthermore, the only significant increase in inflation is experienced in a period of business cycle contraction, a period in which the business cycle contraction indicator variable and not the inflation news variable, is used to capture macroeconomic sentiment.

Initial forecasts by white females, on average, are statistically significantly less accurate by 1.1 percentage points than those of the white male base. Non-white male forecasts are marginally statistically significantly different from those of white males by 0.8 percentage points. The inaccuracy of white female initial forecasts is compounded if the female is also non-white, with an extra inaccuracy of a further 1.1 percentage points, giving a total (significant – test 6) forecast inaccuracy compared with white males of 3.1 percentage points. These demographic effects are jointly significant (test 1).

The Survey2 indicator variable coefficient shows the average improvement or reduction in second interview forecast accuracy for the white male base group. White males improve their forecast accuracy in the second interview by 0.4 percentage points, which is a statistically significant learning effect.

[35] Jointly, non-white temporal interactions are insignificant at the 5% level, with a joint test statistic of $F_{(165, 46755)} = 1.15$.

Relative learning is investigated by examining estimated coefficients on demographic indicator and Survey2 indicator interaction terms. White female learning is greater than the white male base group by a statistically significant 0.2 percentage points. Total learning by white females is a statistically significant 0.6 percentage points (test 5). The forecasts of white females in the second interview are only adversely adrift from those of second interview white males by 0.9 percentage points (test 4).

Table 4: Inverse Probability Weighted Robust Random Effects Race/Gender Regression Results

	Coefficient	Std. Err.		Joint Test	Test Stat.		Ref. No.
Inflation News	1.113	0.6351					
Non-white	0.846	0.4212	*	$F_{(3, 46917)}$	212.72	***	(1)
Female	1.062	0.0476	***				
Non-white x Female	1.149	0.1972	***				
Survey2 x Inflation News	0.040	0.0359		$F_{(4, 46916)}$	6.15	***	(2)
Survey2 x Non-white	-0.154	0.1486					
Survey2 x Female	-0.176	0.0597	**				
Survey2 x NonWhi x Female	-0.437	0.2510					
BCC	-2.176	2.2839		$F_{(8, 46911)}$	0.62		(3)
BCC x Non-white	0.669	1.1848					
BCC x Female	-0.168	0.2633					
BCC x NonWhi x Female	0.523	0.9744					
BCC x Reint	-0.364	0.2544					
BCC x Svy2 x Non-white	0.245	0.9827					
BCC x Svy2 x Female	0.074	0.3706					
BCC x Svy2 x NonWhi xFem	-0.097	1.5166					
Survey2	-0.416	0.1260	**				
Constant	-1.098	1.7410					
<u>Additional Hypothesis Tests</u>							
(Survey2 x Female) + Female = 0				$F_{(1, 46919)}$	367.43	***	(4)
(Survey2 x Female) + Survey2 = 0				$F_{(1, 46919)}$	20.42	***	(5)
Nonwhite + Female + (Nonwhite x Female) = 0				$F_{(1, 46919)}$	49.35	***	(6)
(Svy2xNonWhi)+(Svy2xFemale)+ (Svy2xNonWhix Female) = 0				$F_{(1, 46919)}$	15.15	***	(7)
(Svy2xNonWhi)+(Svy2xFemale)+ (Svy2xNonWhix Female)+Svy2 = 0				$F_{(1, 46919)}$	29.17	***	(8)
Test (8) coefficients + Test coefficient (9) = 0				$F_{(1, 46919)}$	18.18	***	(9)
Survey month dummies: Non-Interacted				$F_{(165, 46755)}$	5.27	***	(10)
Survey month dummies: Interacted with Non-White				$F_{(165, 46755)}$	1.25	*	(11)

Notes: The equation estimated is given by (1) where $\mathbf{demog}_i$ contains female, non-white, and non-white female dummy indicator variables. Joint tests refer to survey adjusted (robust) Wald tests of coefficient (sets) significance (null is joint equality with zero). * denotes significance at the 5% level, ** at the 1% level, *** at the 0.1% level. A blank in this column indicates significance only at levels above 5%. The constant term includes the effect of both the base group and the base month, despite having attempted to minimise survey month effects, as discussed above. The magnitude and significance of this coefficient, therefore, will depend on the survey month chosen and should not be taken as a measure of forecast accuracy for the base group. A summary of category abbreviations used and respective base categories is shown in Table 1.

Non-white females have a significant improvement in second interview forecast accuracy of 0.8 percentage points (test 7) compared to second interview white males, resulting in a total forecast improvement for this group of 1.2 percentage points (test 8). Non-white female second interview forecasts are still statistically different from white males by 2.2 percentage points (test 9). Jointly, all learning effects are significant (test 2).

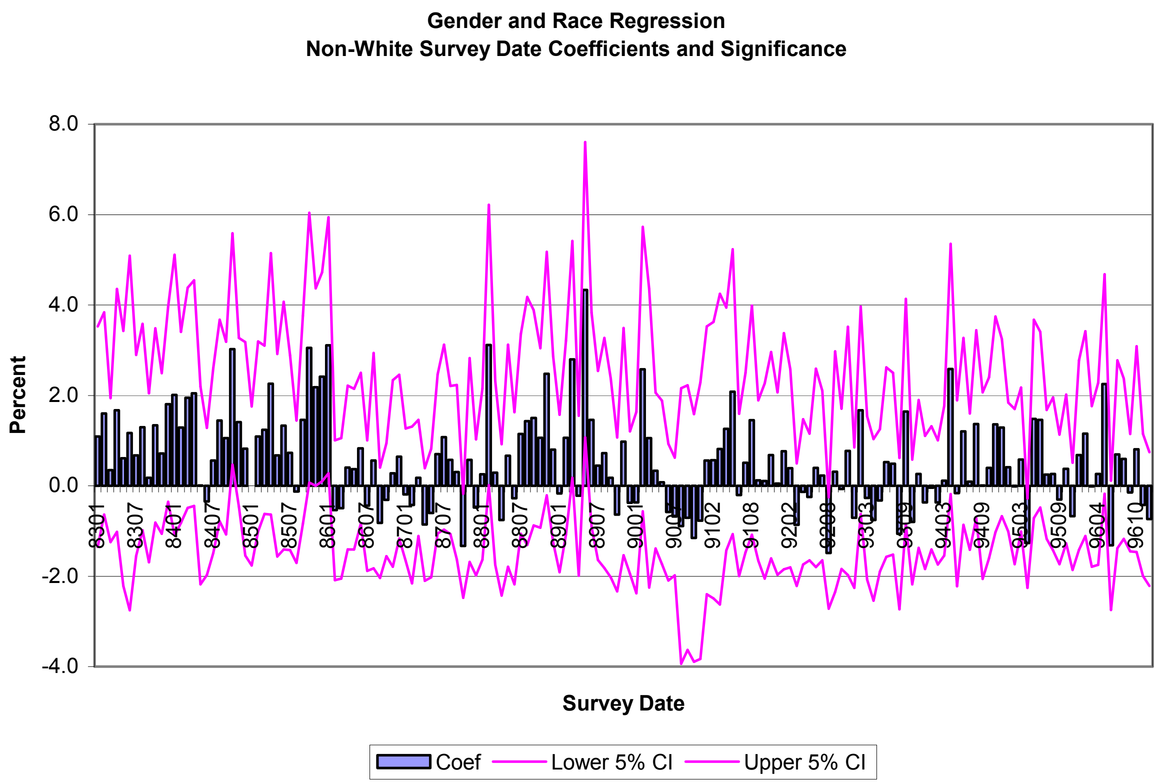
The period of business cycle contraction has no additional statistically significant influence either individually or jointly (test 3) on first interview forecast accuracy or second interview learning. Controlling for race and gender effects, economic contraction, and the sentiment this entails, appears irrelevant to agents forecasting and learning processes. This result might be a consequence of the short length of this contraction period (10 months), which may not be long enough for differences in forecasting processes to become entrenched.

The above discussion implicitly excludes temporal effects. These temporal effects are measured by survey date indicator variables, both singly, and interacted with the non-white indicator variables.

The relative ranking of forecast accuracy is unaffected by the jointly significant (test 10) temporal effects estimated by the survey date indicator variables. These refer only to the base group and would only affect the constant term. These coefficients, of a similar pattern of significance for all the demographic regressions, are examined in more detail at the end of this section.

The survey date indicator variables, interacted with the non-white indicator variable, are relevant for forecast accuracy rankings (but not the ranking of learning). The coefficients for the non-white interaction terms, and associated confidence intervals, are shown in Figure 3.

Figure 3: Race x Survey Date Interaction Coefficients



Despite being predominately insignificant individually, temporal effects are marginally statistically significant (test 11). As joint and individual significance of these effects is marginal, these temporal effects are not considered further.

In conclusion, on average, ignoring temporal effects, gender and race are significant constituents to both initial forecast accuracy and learning. Macroeconomic sentiment effects captured through inflation news are insignificant to initial forecast accuracy, as are those effects captured through the business cycle contraction indicator. Neither macroeconomic sentiment proxy contributes additionally to learning.

5.2 Age and Income

Selecting a base group of respondents in the 35-54 age-group in average income households, equation (1) is estimated (using IPW robust random-effects) where $\mathbf{demog}_i$ contains individual age and household income level indicator variables. Temporal variations in initial forecast accuracy for different age groups are found to be jointly insignificant[36]. The results presented in Table 5 drop these insignificant interactions, retaining only the jointly significant low and high household income temporal interactions (not shown individually).

Household income group and respondent age influence initial interview forecast accuracy. Households with incomes in the bottom 20% of the national income distribution, are significantly less accurate by 1.2 percentage points compared to the base of households in the middle of the income distribution with middle aged respondents. Households in the top 20% are conversely more accurate by 0.7 percentage points.

Respondents in the younger (18-34) age group are significantly more accurate initial forecasters than the base group, by 0.3 percentage points. Those respondents in the older (55-97) age group do not forecast significantly differently compared to the base group.

The effects of age and income individually tend to be nullified when both age and income jointly differ from the base (average) category. Households in the bottom income group and with the respondent in the bottom age group, or those in the top age group and bottom income group, jointly forecast with no difference compared to the base (tests 4 and 6 respectively). Despite having no significant age and income interaction effect, this is also the case for respondents in the 18-34 age group who are also in high income households. Jointly there are no income and age effects for this group (test 5). For high income households with respondents in the 55-97 age group,

[36] Due to variable matrix size software limitations it is necessary to estimate the models containing age temporal interactions separately from the model containing income temporal interactions. Age temporal interactions are jointly insignificant at the 5% level for both (non-base) age groups ($F_{(330, 46590)} = 1.11$).

age and income effects are not nullified (test 7) and jointly, such individuals forecast with improved accuracy compared to the base of 0.7 percentage points. Jointly, all demographic effects are significant (test 1).

Table 5: Inverse Probability Weighted Robust Random Effects Income/Age Results

	Coefficient	Std. Err.		Joint Test	Test Stat.		Ref. No.
Inflation News	1.093	0.6374					
Inc1 (Bottom)	1.248	0.4522	**				
Inc3 (Upper)	-0.683	0.2244	**				
Age1 (18-34)	0.343	0.0723	***				
Age3 (55-97)	-0.013	0.0790		$F_{(8, 46912)}$	11.50	***	(1)
Age1 x Inc1	-1.076	0.3200	**				
Age3 x Inc1	-1.145	0.3077	***				
Age1 x Inc3	0.167	0.1229					
Age3 x Inc3	-0.044	0.1171					
Survey2 x Inf. News	0.056	0.0362					
Survey2 x Inc1	-0.250	0.3379					
Survey2 x Inc3	0.213	0.0846	*				
Survey2 x Age1	-0.178	0.0876	*				
Survey2 x Age3	0.008	0.0970		$F_{(9, 46911)}$	4.38	***	(2)
Survey2 x Age1 x Inc1	0.139	0.4106					
Survey2 x Age3 x Inc1	0.140	0.3915					
Survey2 x Age1 x Inc3	-0.157	0.1484					
Survey2 x Age3 x Inc3	0.151	0.1534					
BCC	-2.042	2.3067					
BCC x Inc1	-0.302	1.8516					
BCC x Inc3	0.595	0.7025					
BCC x Age1	0.423	0.3700					
BCC x Age3	-0.311	0.4064					
BCC x Age1 x Inc1	-1.847	1.8227					
BCC x Age1 x Inc3	-0.895	0.6933					
BCC x Age3 x Inc1	-0.288	1.6209					
BCC x Age3 x Inc3	-0.665	0.6013		$F_{(18, 46902)}$	1.28		(3)
BCC x Survey2	-0.316	0.3468					
BCC x Survey2 x Inc1	-0.819	1.9678					
BCC x Survey2 x Inc3	-0.298	0.5677					
BCC x Survey2 x Age1	-0.322	0.5236					
BCC x Survey2 x Age3	0.188	0.5668					
BCC x Survey2 x Age1 x Inc1	3.166	2.5169					
BCC x Survey2 x Age3 x Inc1	1.341	2.1709					
BCC x Survey2 x Age1 x Inc3	-0.095	0.8858					
BCC x Survey2 x Age3 x Inc3	0.814	0.9821					
Survey2	-0.585	0.1348	***				
Constant	-0.241	1.7627					
<u>**Additional Hypothesis Tests**</u>							
Age1 + Inc1 + (Age1 x Inc1) = 0				$F_{(1, 46919)}$	1.56		(4)
Age1 + Inc3 + (Age1 x Inc3) = 0				$F_{(1, 46919)}$	0.51		(5)
Age3 + Inc1 + (Age3 x Inc1) = 0				$F_{(1, 46919)}$	0.05		(6)
Age3 + Inc3 + (Age3 x Inc3) = 0				$F_{(1, 46919)}$	10.08	**	(7)

Survey2 + (Survey2 x Inc3) = 0	$F_{(1, 46919)}$	7.85	**	(8)
Inc3 + (Survey2 x Inc3) = 0	$F_{(1, 46919)}$	4.40	*	(9)
Survey2 + (Survey2 x Age1) = 0	$F_{(1, 46919)}$	31.60	***	(10)
Age1 + (Survey2 x Age1) = 0	$F_{(1, 46919)}$	5.32	*	(11)
(Survey2xAge1)+(Survey2xInc1)+(Survey2xAge1xInc1) = 0	$F_{(1, 46919)}$	1.50		(12)
(Survey2xAge3)+(Survey2xInc1)+(Survey2xAge3xInc1) = 0	$F_{(1, 46919)}$	0.29		(13)
(Survey2xAge1)+(Survey2xInc3)+(Survey2xAge1xInc3) = 0	$F_{(1, 46919)}$	1.01		(14)
(Survey2xAge3)+(Survey2xInc3)+(Survey2xAge3xInc3) = 0	$F_{(1, 46919)}$	9.39	**	(15)
Test (17) coefficients + Svy2 = 0	$F_{(1, 46919)}$	1.72		(16)
Test (17) coefficients + Test (11) coefficients = 0	$F_{(1, 46919)}$	2.39		(17)
Survey month dummies: Non-Interacted	$F_{(165, 46755)}$	3.96	***	(18)
Survey month dummies: Interacted with Income	$F_{(330, 46590)}$	1.39	***	(19)
Survey month dummies: Interacted with Low Income	$F_{(165, 46755)}$	1.50	***	(20)
Survey month dummies: Interacted with High Income	$F_{(165, 46755)}$	1.21	*	(21

Table notes: The equation estimated is given by (1) where $\mathbf{demog}_i$ contains age and income dummy indicator variables, with interactions. Joint tests refer to survey adjusted (robust) Wald tests of coefficient (sets) significance (null is joint equality with zero). * denotes significance at the 5% level, ** at the 1% level, *** at the 0.1% level. A blank in this column indicates significance only at levels above 5%. The notes below Table 4 should be considered when interpreting the estimated constant term. A summary of category abbreviations used and respective base categories is shown in Table 1.

The base group improve forecast accuracy between the first and second interview by 0.6 percentage points. Compared with this learning, respondents in above average income households learn by 0.2 percentage points less. The reduced total statistically significant learning effect (test 8) for this group is 0.4 percentage points. Compared to the base, respondents in above average income households are still forecasting 0.5 percentage points better than the base group in the reinterview (test 9).

Respondents in the 18-34 age group, however, improve forecast by 0.2 percentage points more than the base group, giving a total learning effect of 0.8 percentage points (test 10). Including the initial interview effect, compared to base group, respondents in this group are still marginally forecasting with 0.2 percentage points less overall accuracy than the base group (test 13).

Despite individual marginal differences in forecast accuracy due to combined age income effects being insignificant, jointly, respondents aged 55-97 in above average income households improve forecast accuracy by 0.4 percentage points less than the base group, which is a statistically significant amount (test 15). This difference acts to nullify the learning achieved by this group (test 16). Further, this affect acts to removes the initial forecasting difference for this group compared to the base (test 17). Jointly, learning by all other age income group combinations is not different from base group learning (tests 12, 13 and 14). All demographic learning effects are jointly significant (test 2).

Macroeconomic sentiment as captured by inflation news is, as previously, not a significant determinant of initial forecast accuracy, nor does it contribute any further effects to learning. Jointly, business cycle sentiment has no influence on first or second interview forecast accuracy (test 3).

Temporal sentiment for the base group is significant to initial forecasting accuracy of the base group (test 18). Below average and above average household income temporal effects are jointly significant (tests 19, 20 and 21), though individually, are predominantly insignificant. Figure 4 and Figure 5 plot these coefficients and associated confidence intervals for low and high income households respectively. These effect may change the ranking of initial forecast accuracy rankings (for the group) but will have no impact on the learning rankings discussed.

To summarise, respondent age and household income level are contributory factors to initial and reinterview forecast accuracy. Macroeconomic sentiment, as captured by the inflation rate, does not influence initial or reinterview forecast accuracy, nor does the business cycle state.

Figure 4: Low Income Temporal Variation Coefficient and Significance

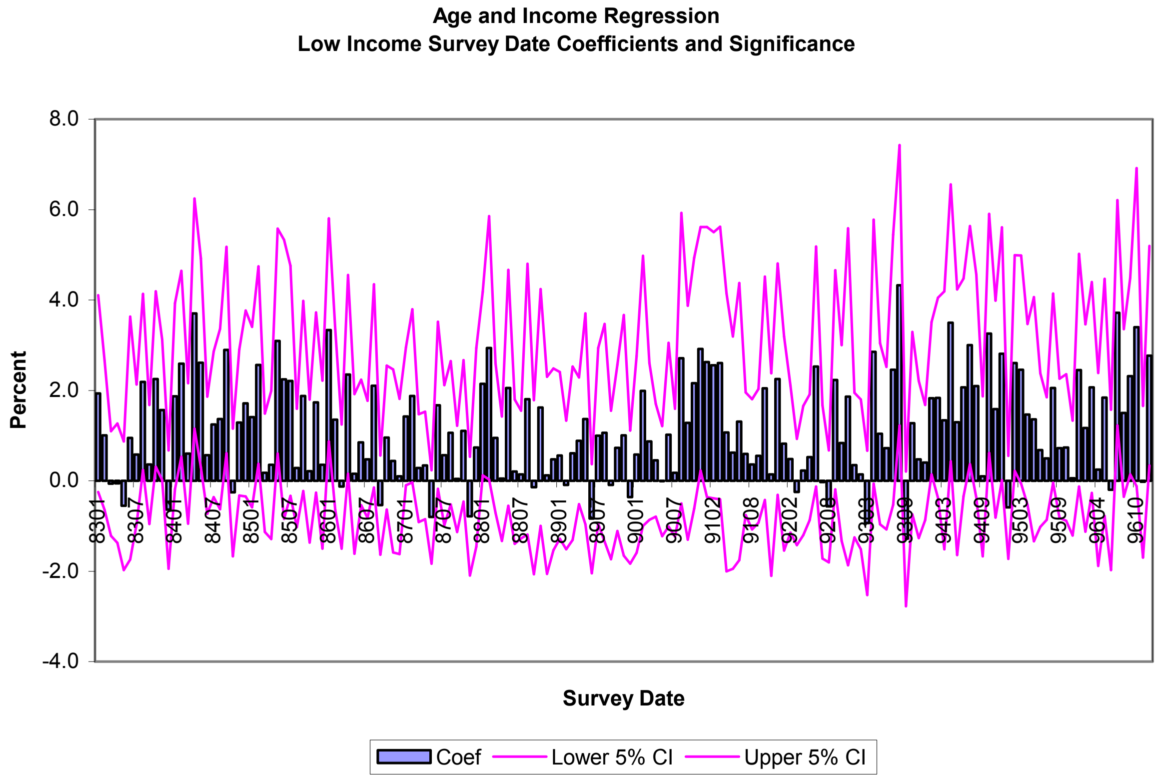
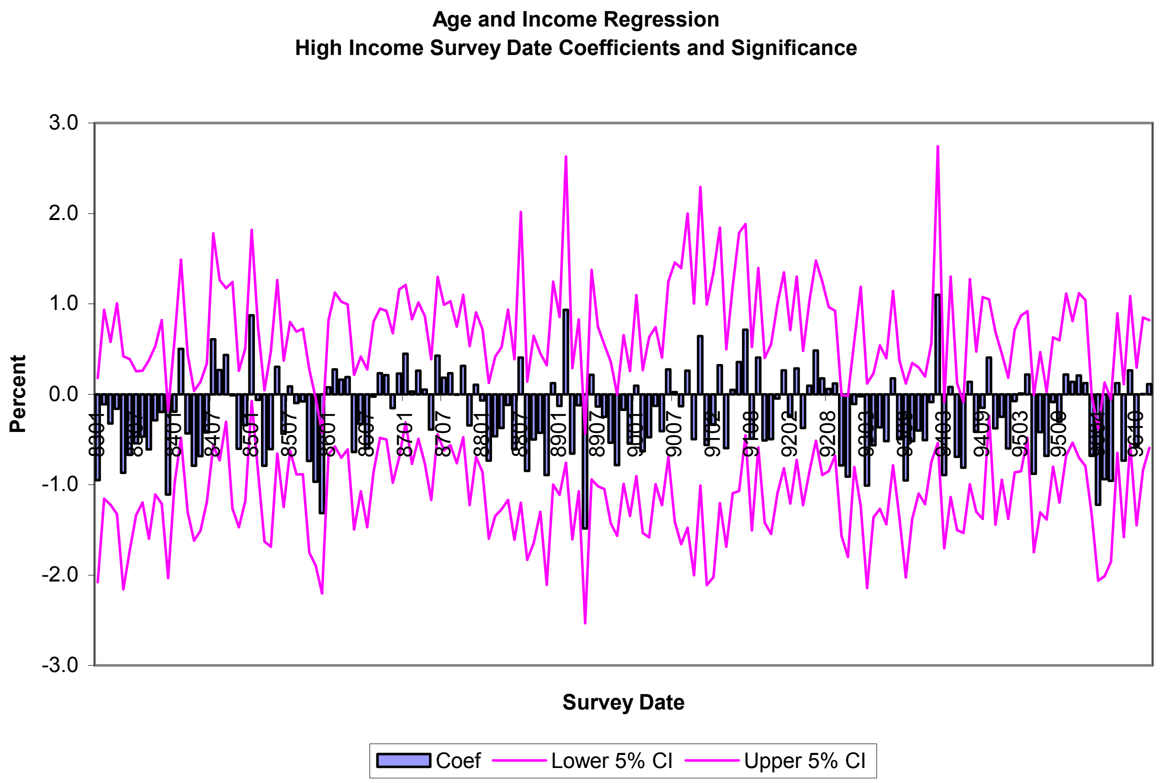


Figure 5: High Income Temporal Variation Coefficient and Significance



5.3 Children/Adults

Compared to a base group of single occupancy adult households containing only the survey respondent and no children, the affect on forecast accuracy and learning of extra adults and children in the household is investigated. Equation (1), where $\mathbf{demog}_i$ contains children and adults in the household indicator variables, is estimated (using IPW robust random-effects). Temporal interactions between the children and adults indictor variables are found to be insignificant[37]. The model is re-estimated without these interactions, with the results shown in Table 6.

Compared with the base category of single adult occupancy households containing no children, the initial forecasts of respondents in households with children are less accurate by a statistically significant 1.2 percentage points. If the household contains no children, but other adults (other than the respondent), forecasts compared to the base are more accurate by 0.2 percentage points. Jointly, the forecasting inefficiency effect of children is nullified (test 4) if there multiple adults in the household. All initial interview demographic effects are jointly significant (test 1).

A general improvement in second interview forecast accuracy of 0.5 percentage points is achieved by the base group. Respondents in households with children improve second interview forecast accuracy by 0.7 percentage points more than the base group (total learning of 1.1 percentage points - significant test 10). Compared to the base, second interview respondents in households with children forecast 0.5 percentage points higher (test 11). If the household contains extra adults and children, this learning difference compared to the base group learning is reduced to 0.2 percentage points more learning (test 7). Overall learning by respondents in this group of 0.7 percentage points is statistically significant (test 8). Second interview forecasts of this group are no longer statistically significantly different from the base group (test 9). Jointly, all demographic learning effects are significant (test 2).

[37] Temporal interactions in involving the 'household containing children' indicator variable are jointly insignificant at the 5% level ($F_{(165, 46755)} = 1.06$). Temporal interactions in involving the 'household containing adults other than the respondent' indicator variable are jointly insignificant at the 5% level ($F_{(165, 46755)} = 1.18$). Jointly, both sets of interactions are insignificant at the 5% level ($F_{(330, 46590)} = 1.09$).

Table 6: Inverse Probability Weighted Robust Random Effects Children/Adults Results

	Coefficient	Std. Err.		Joint Test	Test Stat.		Ref. No.
Inflation News	1.029	0.6466					
Children	1.200	0.1597	***	$F_{(3, 46917)}$	27.09	***	(1)
Adults	-0.158	0.0684	*				
Adults x Children	-0.986	0.1705	***				
Survey2 x Inf. News	0.053	0.0363		$F_{(4, 46916)}$	4.24	**	(2)
Survey2 x Children	-0.740	0.1995	***				
Survey2 x Adults	-0.109	0.0892					
Survey2 x Adults x Child	0.665	0.2124	**				
BCC	-1.228	2.3244		$F_{(8, 46912)}$	1.25		(3)
BCC x Children	-0.537	0.8309					
BCC x Adults	-0.944	0.3989	*				
BCC x Adults x Children	0.883	0.8830					
BCC x Survey2	-0.646	0.4799					
BCC x Survey2 x Children	0.388	1.1921					
BCC x Survey2 x Adults	0.717	0.5589					
BCC x Svy2 x Adults x Child	-1.002	1.2649					
Survey2	-0.469	0.1402	**				
Constant	-0.097	1.7905					
<u>Additional Hypothesis Tests</u>							
Children + Adults + (Adults x Children) = 0				$F_{(1, 46919)}$	0.62		(4)
(BCC x Adults) + Adults = 0				$F_{(1, 46919)}$	7.85	**	(5)
(BCC x Adults) + BCC = 0				$F_{(1, 46919)}$	0.88		(6)
(Svy2xChildren)+(Svy2xAdults)+(Svy2xAdultsxChildren)=0				$F_{(1, 46919)}$	3.94	*	(7)
Svy2+(Svy2xChildren)+(Svy2xAdults)+(Svy2xAdultsxChildren)=0				$F_{(1, 46919)}$	24.43	***	(8)
(Svy2xChildren)+(Svy2xAdults)+(Svy2xAdultsxChildren) +Adults+Child+(AdultsxChild)=0				$F_{(1, 46919)}$	2.77		(9)
Svy2+(Svy2xChildren)				$F_{(1, 46919)}$	29.53	***	(10)
Children+(Svy2xChildren)				$F_{(1, 46919)}$	8.38	**	(11)
Svy2+(Svy2xAdults)+(BCCxSvy2)+(BCCxSvy2xAdults)=0				$F_{(1, 46919)}$	1.82		(12)
Svy2+(Svy2xAdults)+(BCCxSvy2)+(BCCxSvy2xAdults) +(BCCxAdults)+BCC=0				$F_{(1, 46919)}$	1.33		(13)
(BCC x Survey2) + (BCC x Survey2 x Adults) = 0				$F_{(1, 46919)}$	0.05		(14)
Survey month dummies				$F_{(165, 46755)}$	4.64	***	(15)

Notes: The equation estimated is given by (1) where $\mathbf{demog}_i$ contains all demographic indicator variables for whether there are adults and or children in the household. Joint tests refer to survey adjusted (robust) Wald tests of coefficient (sets) significance (null is joint equality with zero). * denotes significance at the 5% level, ** at the 1% level, *** at the 0.1% level. A blank in this column indicates significance only at levels above 5%. The notes below Table 4 should be considered when interpreting the estimated constant term. A summary of category abbreviations used and respective base categories is shown in Table 1.

There is possible erroneous significance for a business cycle contraction sentiment effect on households containing adults compared with the base category. Overall, however, business cycle sentiment effects for this group (test 6) and all other groups jointly (test 3) are insignificant. Other macroeconomic sentiment captured by inflation news is insignificant for initial forecast accuracy, and for learning.

Temporal variations in base group initial forecast accuracy are significant (test 15). As mentioned previously, this will have no effect on either the relative forecast accuracy rankings or on the learning identified.

In conclusion, the forecasts of respondents in single adult occupancy households containing children are significantly less accurate than those of single adult occupancy no children households. If it is correct to reason that single parent families would have more limited resources[38], and so are more price-sensitive, this finding seems somewhat surprising if price sensitivity equates to a clearer understanding of inflation. This might suggest that the general (CPI-U) inflation rate is not a good proxy for the inflation experience of this group. This would, however, create a paradox as the realignment of forecasts in the second interview to more accurately predict inflation would suggest an understanding of national (CPI-U) inflation. Perhaps this suggests such agents become aware of the difference between actual inflation and inflation experience.

Finally, households containing adults tend to be more efficient first interview forecasters compared to the base. Jointly multiple occupancy adult households nullify the forecasting inaccuracy effects of households containing children, and reduce second interview learning effects.

[38] More specifically in terms of money, and not time. This might not be the case, however, caused by child benefit systems which may introduce recklessness to purchasing decisions, and so distort the experience of actual price changes.

5.4 Combined Analysis

The analysis thus far presented implicitly assumes demographic characteristics are uncorrelated between groups[39]. Such analysis allowed temporal effects and base groups (and so constant term coefficients) to differ between models, and yet maintain reasonable within group sample size and a parsimonious model specification.

For robustness, a final model is estimated which includes all demographic characteristics used in the above models, without interactions. This model is more restrictive in constraining the temporal effects measured by the survey date dummies to be identical regardless of demography. It also implies a less generally representative base category from which differences are measured of middle income households, containing no further adults and no children, where the respondent is in the 35-54 age group, is white and male, with zero past inflation. The inflation news effect is also constrained to be equal amongst all groups. As the business cycle was found to have no strong effects in the previous analysis, all terms involving the business cycle contraction indicator are dropped.

In summary, a modified model based on equation (1) is estimated:

$$\begin{aligned} \left|\pi^{t+12} - E_{i,s}^{t}\pi^{t+12}\right| = \delta_0 + \delta_1 &\mathbf{demog}_i + \delta_2\pi^{t-1} + \delta_3 \text{survey2}_s \\ &+\delta_4\left(\mathbf{demog}_i \times \text{survey2}_s\right) \\ &+\delta_5 \mathbf{time}_t \\ &+a_i + \varepsilon_{is} \\ \text{for } s = 1,2 \quad i &= 1,\dots,N, \;\; t = 8301,\dots,9612 \end{aligned} \tag{5}$$

As previously discussed, this model is estimated using the inverse probability weighted random effects methodology, outlined above. The results of this are presented in Table 7.

[39] This was the reason for not examining income and education separately, as these are likely to be highly correlated.

Table 7: Inverse Probability Weighted Robust Random Effects General Model Results

	Coef.	Std. Err		Joint Test	Test Stat.		Ref. No.
Inflation News	0.52525	0.0957	***				
Inc1	1.12517	0.1032	***				
Inc3	-0.65707	0.0495	***				
Age1	0.26671	0.0606	***				
Age3	0.10867	0.0716					
Non-white	1.86965	0.0995	***	$F_{(8, 46912)}$	157.22	***	(1)
Female	1.10819	0.0488	***				
Children	0.21390	0.0592	***				
Adults	0.09009	0.0650					
Survey2 x Inf. News	0.00884	0.0321					
Survey2 x Inc1	-0.00244	0.1346					
Survey2 x Inc3	0.12552	0.0608	*				
Survey2 x Age1	-0.19335	0.0747	*				
Survey2 x Age3	-0.05929	0.0899		$F_{(9, 46911)}$	4.37	***	(2)
Survey2 x Non-white	-0.35260	0.1249	**				
Survey2 x Female	-0.23573	0.0604	***				
Survey2 x Children	-0.16699	0.0728	*				
Survey2 x Adults	0.03949	0.0820					
Survey2	-0.19302	0.1374					
Constant	-0.02221	0.3728					
Additional Hypothesis Tests							
Survey Month Dummies				$F_{(165, 46755)}$	4.89	***	(3)

Notes: The equation estimated is given by (5) where $\mathbf{demog}_i$ contains all demographic indicator variables, but no interactions. Joint tests refer to survey adjusted (robust) Wald tests of coefficient (sets) significance (null is joint equality with zero). * denotes significance at the 5% level, ** at the 1% level, *** at the 0.1% level. A blank in this column indicates significance only at levels above 5%. The notes below Table 4 should be considered when interpreting the estimated constant term. A summary of category abbreviations used and respective base categories is shown in Table 1.

The general pattern of significant initial interview demographic and household characteristics is virtually unchanged from that uncovered by the grouped analysis. As previously noted, gender, race, income and below average age individual demographic characteristics are all significant. The smaller variance for the non-white term in the above regression compared to the age/gender grouped regression model is probably due to the lack of non-white female interaction term in this regression. Conversely, without the household interaction term for both adults and children in the household, the previous marginal significance of the extra adult indicator variable becomes insignificant in the above model. There is, however, a corresponding reduction in the variability of the children in household indicator variable coefficient.

Race, gender and income are comparatively strong effects compared to age and household adult child composition. The direction of all effects is identical to that of the grouped analysis. All initial interview characteristic effects are jointly significant (test 1).

Regarding learning, again a similar pattern of significance prevails. The significance of the children in the household reduces to being only marginal at the 5 percentage points level. The primary difference between the grouped analysis demographic terms is the non-white learning effect, which in this model is highly significant (compared to insignificant in the grouped analysis). This coefficient is also the largest compared to the other demographic characteristics. Compared to initial interview forecast accuracy demographic differences, there is more similarity in the magnitude of all significant coefficients, which all tend to be relatively small. The direction of all effects is the same as that uncovered using the grouped analysis. All learning characteristics are jointly significant (test 2).

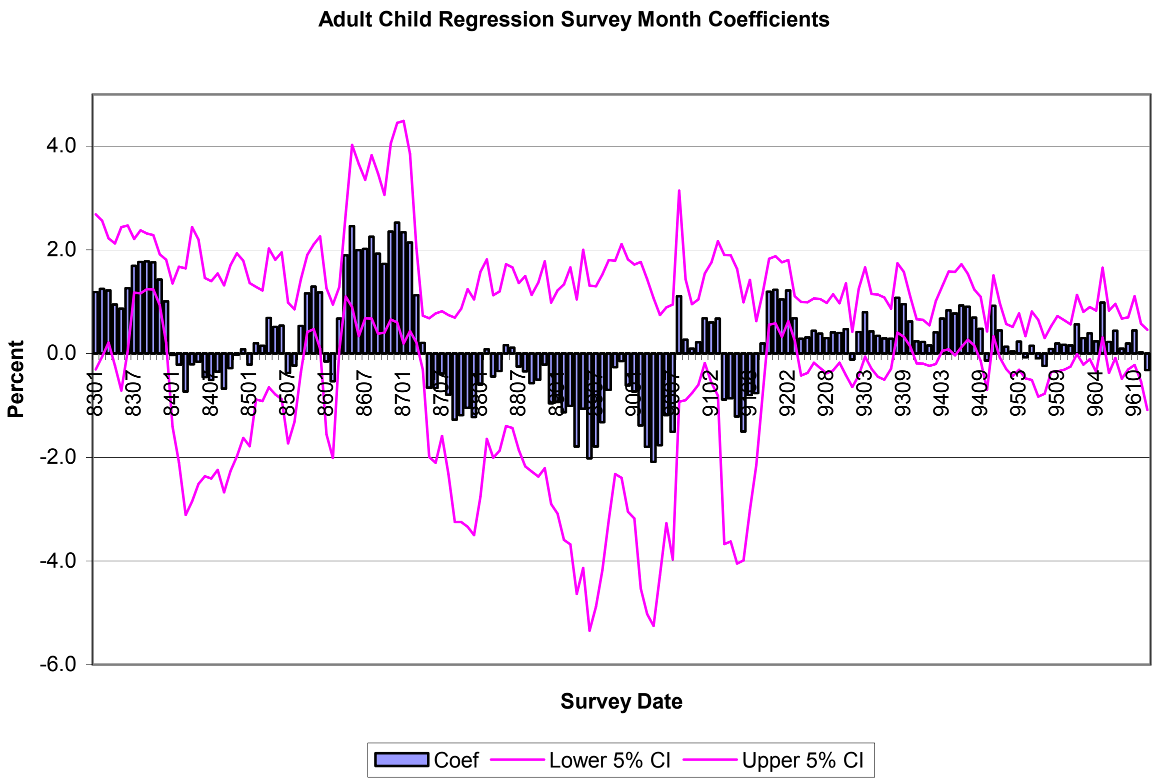
The primary exception to the above is the inflation news variable, which having dropped all terms involving the business cycle indicator variable, is now highly significant. Inflation news is useful in explaining temporal variability otherwise captured by the survey date indicator variables.

5.5 Survey Month Dummies

Survey month dummies follow a similar month-to-month pattern in terms of coefficient and significance for all models. Sizable positive (negative) significant coefficients in a particular month would reduce (increase) overall forecast accuracy in that period, though the pattern of learning outlined above would still apply.

The pattern of coefficient significance and magnitude for these survey month dummies is shown in Figure 6 (using the adult and child regression estimates). Most of these coefficients are individually insignificant. Modelling forecast accuracy and learning through equation (1), it appears that this model captures sentiment best around 1995 (small and generally insignificant coefficients). From the 1990s onwards, the magnitude of all coefficients tends to be smaller. Overall, however, since the maximum monthly shift is only 2 percentage points, the demographic effects identified above, generally in excess of 0.5 percentage points, are a sizable proportion of inflation forecast accuracy.

Figure 6: Survey Month Dummies Coefficients and Significance



6. Conclusions and Summary

The SRC dataset offers a unique opportunity to examine demographic and temporal heterogeneity in consumer year-ahead inflation forecast accuracy. Such analysis is particularly pertinent since central bank monetary policy has shifted towards targeting inflation, and, as discussed in the introduction, controlling inflation embodies controlling inflation expectations. This paper has sought to highlight whether differences in forecasting processes are attributable to (or correlated with) a range of demographic characteristics. The short-panel aspect of the dataset allowed quantification of average differences in responses between interviews, and so demographic specific learning has been investigated.

Estimation issues are discussed including relaxing some of the usual random-effects methodology assumptions to allow for possible heteroskedasticity of unknown form. Attrition is modelled, and assuming it only related to observed characteristics, is corrected using inverse probability weighting. The affect of outliers was reduced, mindful of the need to permit changes in large responses to quantify learning, by censoring extreme observations. The necessity to use an absolute value of a non-normal dependent variable to quantify changes in forecast accuracy was discussed, but further correction was deemed beyond the scope of this study, as no commonplace methods exist to deal with such a combination of dataset anomalies.

Results suggest initial forecast accuracy, compared to an average male, is strongly reduced if the respondent is non-white, female, or in a below average income household. Other, less strong characteristics which reduce initial forecast accuracy are the respondent being in the 18-34 age group, or if there are children in the household. The magnitude of inflation news is a model dependent feature which may further reduce initial forecast accuracy. Conversely, individuals resident in high income households forecast with more initial forecasts accuracy. Some of these effects are nullified if characteristics are combined. The business cycle is found to have no effect on initial forecast accuracy.

Learning is prevalent regardless of the demographic makeup of the respondent: second interview forecasts are more accurate than first interview responses. The magnitude of this improvement is found to depend on demographic characteristics, with females and respondents in the 18-34 age group, and those in households containing children improving forecasts more than the male base group. There is evidence to suggest further learning effects may depend on race though this conclusion depends on the model estimated. Conversely, high income households tend not to improve forecasts as much as the base group. Learning is unaffected by the business cycle. Generally learning is found to be greatest for those individuals who forecast least accurately in the first interview. The improvement in the forecasts of such individuals might suggest that a known reinterview acts as an incentive for these individuals to notice inflation, an incentive which wasn't present on the initial 'cold-call' interview.

In conclusion, central banks wishing to anchor inflation expectation to actual inflation in order to control inflation should consider initiatives which stimulate agents to learn about or simply notice inflation. The results presented suggest that when agents learn about inflation, forecasts of future inflation become more realistic. The magnitude of learning and the level of initial forecast accuracy does depend on demographic characteristics. Accordingly, exploiting such differences in the dissemination of information may further improve the effectives of procedures designed to meet monetary policy objectives.

Further research would be useful in understanding the causes of temporal variations in forecast accuracy. The SRC survey offers scope for the analysis of such problems in also containing sentiment and news related questions. Finally, it would be useful to explore and implement estimation methodologies which are robust to all the dataset issues encountered.

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8. Appendix 1: Population Tabulations

Table 8: Possible Matched Observation Survey Constituent Group Populations

		Raw Data		No Inflation Blanks		No Other Blanks	
		Freq.	Percent	Freq.	Percent	Freq.	Percent
Region	NC	14,209	27.3%	12,991	27.4%	12,108	27.6%
	NE	10,213	19.6%	9,281	19.6%	8,553	19.5%
	S	17,069	32.8%	15,448	32.6%	14,325	32.6%
	W	10,451	20.1%	9,588	20.2%	8,913	20.3%
	NA/DK	95	0.2%	87	0.2%	0	0.0%
Kids	No	31,276	60.1%	27,826	58.7%	25,551	58.2%
	Yes	20,673	39.7%	19,495	41.2%	18,348	41.8%
	NA/DK	88	0.2%	74	0.2%	0	0.0%
Extra Adults	No	14,534	27.9%	12,599	26.6%	11,672	26.6%
	Yes	37,437	71.9%	34,739	73.3%	32,227	73.4%
	NA/DK	66	0.1%	57	0.1%	0	0.0%
HH Head	Yes	32,322	62.1%	29,448	62.1%	27,416	62.5%
	No	19,650	37.8%	17,891	37.8%	16,483	37.6%
	NA/DK	65	0.1%	56	0.1%	0	0.0%
Education	NOHS	6,903	13.3%	5,497	11.6%	5,115	11.7%
	HS	17,157	33.0%	15,749	33.2%	14,754	33.6%
	Some College	11,602	22.3%	10,847	22.9%	10,239	23.3%
	College	15,487	29.8%	14,605	30.8%	13,791	31.4%
	NA/DK	888	1.7%	697	1.5%	0	0.0%
Income	Bottom	7,734	14.9%	6,393	13.5%	6,125	14.0%
	Average	32,213	61.9%	30,047	63.4%	29,282	66.7%
	Top	9,129	17.5%	8,689	18.3%	8,492	19.3%
	NA/DK	2,961	5.7%	2,266	4.8%	0	0.0%
Sex	Female	28,771	55.3%	25,348	53.5%	23,400	53.3%
	Male	23,230	44.6%	22,014	46.5%	20,499	46.7%
	NA/DK	36	0.1%	33	0.1%	0	0.0%
Race	Non-white	8,118	15.6%	7,282	15.4%	6,874	15.7%
	White	42,829	82.3%	39,213	82.7%	37,025	84.3%
	NA/DK	1,090	2.1%	900	1.9%	0	0.0%
Age	18-34	18,565	35.7%	17,751	37.5%	16,790	38.3%
	35-54	18,725	36.0%	17,468	36.9%	16,411	37.4%
	55-97	14,453	27.8%	11,950	25.2%	10,698	24.4%
	NA/DK	294	0.6%	226	0.5%	0	0.0%
Marital Status	Divorced	5,546	10.7%	5,041	10.6%	4,797	10.9%
	Married	29,831	57.3%	27,701	58.5%	25,883	59.0%
	Never married	9,316	17.9%	8,727	18.4%	8,240	18.8%
	Separated/MA	1,754	3.4%	1,608	3.4%	1,526	3.5%
	Widowed	4,928	9.5%	3,783	8.0%	3,453	7.9%
	NA/DK	662	1.3%	535	1.1%	0	0.0%
	Total	52,037	100.0%	47,395	100.0%	43,899	100.0%

Table 9: January 1993 to June 1983 Reinterview Survey Constituent Group Populations

		Raw Data		No Inflation Blanks		No Other Blanks	
		Freq.	Percent	Freq.	Percent	Freq.	Percent
Region	NC	1,165	26.0%	486	28.9%	471	29.2%
	NE	1,015	22.6%	356	21.2%	337	20.9%
	S	1,522	33.9%	536	31.9%	516	32.0%
	W	784	17.5%	302	18.0%	290	18.0%
	NA/DK	1	0.0%	0	0.0%	0	0.0%
Kids	No	2,658	59.2%	932	55.5%	891	55.2%
	Yes	1,829	40.8%	748	44.5%	723	44.8%
	NA/DK	0	0.0%	0	0.0%	0	0.0%
Extra Adults	No	1,246	27.8%	419	24.9%	405	25.1%
	Yes	3,202	71.4%	1,260	75.0%	1,209	74.9%
	NA/DK	39	0.9%	1	0.1%	0	0.0%
HH Head	Yes	2,641	58.9%	995	59.2%	957	59.3%
	No	1,795	40.0%	684	40.7%	657	40.7%
	NA/DK	51	1.1%	1	0.1%	0	0.0%
Education	NOHS	857	19.1%	246	14.7%	239	14.8%
	HS	1,553	34.6%	559	33.3%	546	33.8%
	Some College	834	18.6%	383	22.8%	371	23.0%
	College	931	20.8%	475	28.3%	458	28.4%
	NA/DK	312	7.0%	17	1.0%	0	0.0%
Income	Bottom	1,014	22.6%	277	16.5%	265	16.4%
	Average	2,475	55.2%	1,064	63.3%	1,041	64.5%
	Top	635	14.2%	312	18.6%	308	19.1%
	NA/DK	363	8.1%	27	1.6%	0	0.0%
Sex	Female	2,556	57.0%	919	54.7%	881	54.6%
	Male	1,894	42.2%	760	45.2%	733	45.4%
	NA/DK	37	0.8%	1	0.1%	0	0.0%
Race	Non-white	684	15.3%	173	10.3%	169	10.5%
	White	3,505	78.1%	1,486	88.5%	1,445	89.5%
	NA/DK	298	6.6%	21	1.3%	0	0.0%
Age	18-34	1,857	41.4%	703	41.8%	684	42.4%
	35-54	1,294	28.8%	583	34.7%	561	34.8%
	55-97	1,225	27.3%	388	23.1%	369	22.9%
	NA/DK	111	2.5%	6	0.4%	0	0.0%
Marital Status	Divorced	435	9.7%	166	9.9%	164	10.2%
	Married	2,307	51.4%	1,003	59.7%	965	59.8%
	Never married	859	19.1%	315	18.8%	305	18.9%
	Separated/MA	158	3.5%	45	2.7%	43	2.7%
	Widowed	464	10.3%	143	8.5%	137	8.5%
	NA/DK	264	5.9%	8	0.5%	0	0.0%
	Total	4,487	100.0%	1,680	100.0%	1,614	100.0%

Table 10: July 1996 to December 1996 Initial Interview Survey Constituent Group Populations

		Raw Data		No Inflation Blanks		No Other Blanks	
		Freq.	Percent	Freq.	Percent	Freq.	Percent
Region	NC	457	26.3%	422	26.4%	378	26.9%
	NE	348	20.1%	327	20.5%	289	20.5%
	S	578	33.3%	525	32.9%	452	32.1%
	W	352	20.3%	324	20.3%	288	20.5%
	NA/DK	0	0.0%	0	0.0%	0	0.0%
Kids	No	999	57.6%	909	56.9%	785	55.8%
	Yes	736	42.4%	689	43.1%	622	44.2%
	NA/DK	0	0.0%	0	0.0%	0	0.0%
Extra Adults	No	496	28.6%	450	28.2%	393	27.9%
	Yes	1,239	71.4%	1,148	71.8%	1,014	72.1%
	NA/DK	0	0.0%	0	0.0%	0	0.0%
HH Head	Yes	1,105	63.7%	1,025	64.1%	906	64.4%
	No	630	36.3%	573	35.9%	501	35.6%
	NA/DK	0	0.0%	0	0.0%	0	0.0%
Education	NOHS	177	10.2%	137	8.6%	119	8.5%
	HS	574	33.1%	534	33.4%	474	33.7%
	Some College	380	21.9%	354	22.2%	315	22.4%
	College	581	33.5%	552	34.5%	499	35.5%
	NA/DK	23	1.3%	21	1.3%	0	0.0%
Income	Bottom	219	12.6%	186	11.6%	182	12.9%
	Average	1,043	60.1%	981	61.4%	954	67.8%
	Top	288	16.6%	279	17.5%	271	19.3%
	NA/DK	185	10.7%	152	9.5%	0	0.0%
Sex	Female	943	54.4%	845	52.9%	742	52.7%
	Male	792	45.7%	753	47.1%	665	47.3%
	NA/DK	0	0.0%	0	0.0%	0	0.0%
Race	Non-white	364	21.0%	330	20.7%	293	20.8%
	White	1,325	76.4%	1,227	76.8%	1,114	79.2%
	NA/DK	46	2.7%	41	2.6%	0	0.0%
Age	18-34	532	30.7%	494	30.9%	443	31.5%
	35-54	746	43.0%	707	44.2%	642	45.6%
	55-97	449	25.9%	389	24.3%	322	22.9%
	NA/DK	8	0.5%	8	0.5%	0	0.0%
Marital Status	Divorced	199	11.5%	183	11.5%	168	11.9%
	Married	1,014	58.4%	944	59.1%	841	59.8%
	Never married	300	17.3%	278	17.4%	243	17.3%
	Separated/MA	67	3.9%	63	3.9%	57	4.1%
	Widowed	141	8.1%	117	7.3%	98	7.0%
	NA/DK	14	0.8%	13	0.8%	0	0.0%
	Total	1,735	100.0%	1,598	100.0%	1,407	100.0%

9. Appendix 2: Additional Graphs

Figure 7: Year Ahead Forecast Distribution

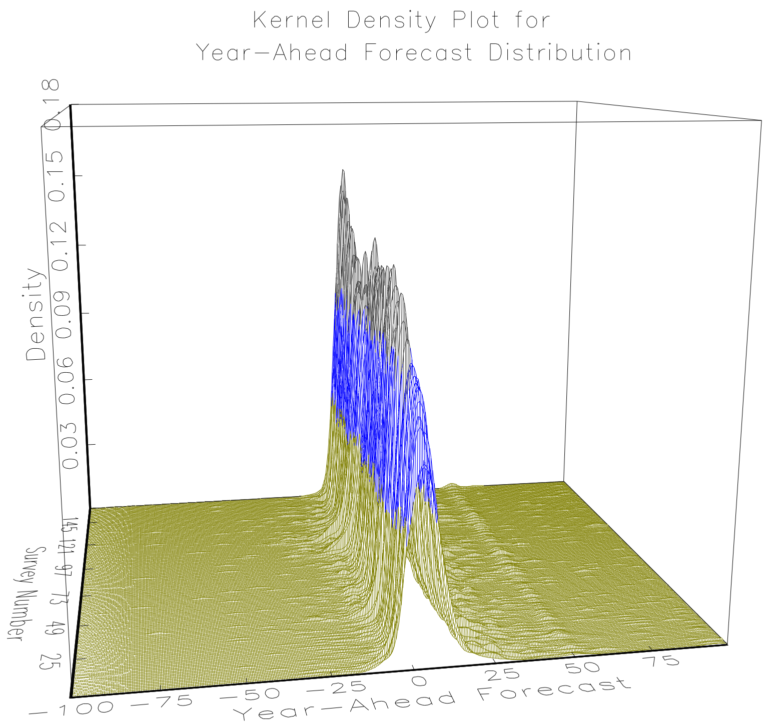

Figure 8: CPI-U Inflation