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FORECASTING QUARTERLY AGGREGATE CRIME SERIES*

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In this paper we assess the forecasting performance of quarterly economic models of aggregate property and personal crime. We show that models that include long-run relationships between crime and its economic determinants tend to generate inaccurate forecasts, and attribute this to structural change. The forecast performance of the economic models is compared with that of time-series models, and forecast encompassing tests are reported.

1 Introduction

Until recently, most econometric studies of crime, especially in the UK, have focused on the determinants of crime, such as opportunity cost of time, social disorder, returns to crime and enforcement. However, recent events, not least the renewed interest in potential effective crime prevention programmes, have led to an increased interest in forecasting crime. Indeed, a recent Home Office seminar[1] and an issue of the *International Journal of Forecasting*[2] have been devoted to forecasting crime. The main contribution of our paper is to provide a longer historical perspective on the forecast performance of economic models of crime. The papers by Deadman (2003) and Harries (2003) assess the accuracy of annual predictions for the period 1999–2001. This is a valuable exercise in that the projections were made prior to the event and are therefore truly *ex ante*. That said, the three-year period is perhaps too short to infer much about the usefulness of the models at other periods, and as the projections rely on assumptions about the future values of the explanatory variables, their accuracy will depend on these assumptions as much as the implied relationships or correlations between crime and the explanatory variables.

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†The computations recorded in this paper were carried out using PcGive version 10; see Hendry and Doornik (2001). We are grateful to the Home Office for making the crime data available, and acknowledge the helpful comments of a referee on an earlier draft.
[1]Modelling and Forecasting Crime Seminar, organized by the Home Office, London, April 2003.
[2]See the *International Journal of Forecasting*, 19 (4), 2003, and especially the papers by Deadman (2003) and Harries (2003). The paper by Harries discusses the modelling of crime at the Home Office and the research that culminated in three-year projections of property crime for 1999–2001 in Dhiri *et al*. (1999).

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In this paper we assess the forecasting performance of quarterly models of aggregate crime against rival time-series models. The two main data series we use are total property crime (burglary, theft and handling stolen goods, and criminal damage) and total personal crime (violence against the person and sexual offences). These series are available quarterly on a seasonally unadjusted basis from 1975Q2 to 2003Q1. These series were modelled by Osborn (2000) using total real consumption and the number of unemployed as economic variables for the period ending in 1993. Our aim is to assess the forecast performance of models, such as those of Osborn, that seek to relate the crime series to economic variables. In recent years there has been a great deal of interest in modelling long-run relationships between crime series and economic variables using annual data (e.g. Dhiri *et al.*, 1999; Deadman, 2003), although Clements and Hendry (1999) warn that error correction models (ECMs) may not be very good forecasting models when there are structural breaks. Nevertheless, there have been few attempts to model crime using economic variables on quarterly data, and fewer attempts still to investigate the forecast performance of those models. We estimate economic models for the crime aggregates on data up to 1993, and then forecast the subsequent quarters. The model specifications are kept fixed over the forecast period with the parameter estimates held at the values determined on the sample to 1993. Sequences of one-step ahead forecasts are generated, based on replacing the models' explanatory variables by their true values. The fixed specification and no-updating of parameter estimates[3] undoubtedly worsen the performance of the economic models, but serve to highlight the impact of imposing long-run solutions when these appear to be non-constant.

Second, we consider the forecast performance of simple time-series models, noting that in other areas (see, for example, Fildes and Makridakis (1995), and the 'M Competitions' reviewed in Makridakis and Hibon (2000)) such models have fared well relative to more sophisticated models, or more 'structural' models. Of particular interest is not solely whether the models using economic variables are better on traditional forecast accuracy criteria, such as (root) mean-squared forecast errors, but whether combinations of the two models may be better still. Put differently, do the time-series models contain useful information that is not already present in the economic model forecasts? The use of quarterly data provides a large enough sample of forecasts to enable statistical inference on whether one model 'encompasses' another. Univariate time-series models serve to sharpen the distinction between modelling and forecasting. They simply extrapolate past patterns in the single time series and are agnostic about the causes or determinants of crime. A good performance of such models relative to the economic models

[3]See Swanson and White (1997) for a detailed study of the use of flexible model specification.

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has traditionally been viewed as symptomatic of dynamic mis-specification of the latter, and more recently to the greater robustness of some time-series models to structural change (see Clements and Hendry, 1999).

The plan of the remainder of the paper is as follows. In Section 2 we briefly discuss the economic models, and the time-series models that will be used as competitors. Section 3 sets out the form of the forecast encompassing tests, and discusses why this is a natural method of assessing the forecasts from the economic and time-series models. Section 4 presents the estimated models and the forecasting comparisons. Section 5 concludes.

2 Models of Crime

The economic models are based on those of Osborn, and relate the crime aggregate to consumers' expenditure and the unemployment rate. The models are ECMs, and model the dynamic relationships between the changes in the crime aggregates and the economic variables simultaneously with the long-run relationship (where this is warranted by the presence of cointegration). Although no attempt is made to use exactly the same variables and models as Osborn, our variables are sufficiently similar that there is no need to redo the unit root tests, and we model similar transforms of the variables.[4] We follow Osborn in taking the first difference of the log of property crime as the quantity to be modelled, and as the transformation of property crime to be used as an explanatory variable. For personal crime, suspicions regarding unit roots at the seasonal frequencies lead to use of the annual (fourth difference) of this variable. Given that a focus of our paper is the impact of modelling the long run on forecast performance, we carry out a cointegration analysis.

The time-series models we use are in the tradition of Box and Jenkins (1970), based on the transformation of the variable noted above. We experimented with the popular Box and Jenkins 'airline model' for seasonal quarterly data

$$(1-L)(1-L^4)x_t = \mu + (1-\theta_1 L)(1-\theta_4 L^4)\varepsilon_t \tag{1}$$

as this model has been used with some success. In this model, $\varepsilon_t \sim \mathrm{IN}(0, \sigma_\varepsilon^2)$, and $|\theta_1| < 1$, $|\theta_4| < 1$. The lag operator L is such that $L^k x_t = x_{t-k}$. This model imposes annual and one-period (zero-frequency) differencing filters in the autoregressive polynomial. The forecasts from the airline model were poor, and instead we focus on autoregressive moving average models of the first difference of property crime and of the fourth difference of personal crime.

[4]One difference is the use of seasonally adjusted total real consumption, so obviating taking the annual sum to estimate the long-run relationship, and we use the first rather than the annual difference of the log in the dynamic model.

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3 Forecast Combination and Encompassing

There is an extensive literature in economic and management science[5] attesting to the usefulness of forecast combination, and it is therefore of interest to see whether combination proves useful for forecasting crime. The candidate models for combination are the 'economic models' of Osborn, and the traditional time-series models for seasonal data, such as the airline model. The basic idea behind the combination (or 'pooling') of forecasts is that, although one forecast may be superior to another (e.g. using tests of equal forecast accuracy, such as those of Diebold and Mariano (1995)), a combined forecast of the two may be better still. Studies such as Newbold and Granger (1974), Stock and Watson (1999) and Fildes and Ord (2002), for example, suggest that simple forms of combination (that do not take into account the relative past performances of the forecasts) often work just about as well as more elaborate schemes.

Following Nelson (1972) and Granger and Newbold (1973), a forecast is said to be conditionally efficient if the variance of the forecast error from a combination of that forecast and a rival forecast is not significantly less than that of the original forecast alone. In the terminology of Chong and Hendry (1986), a conditionally efficient forecast is said to forecast encompass another. Thus, a set of forecasts $\{f_{1t}\}_{t=1}^{n}$ is said to forecast encompass another $\{f_{2t}\}_{t=1}^{n}$ if the latter contains no useful information not already present in $\{f_{1t}\}$, in the mean squared error sense that a linear combination of f_{1t} and f_{2t} (with non-zero weight accorded to f_{2t}) has a mean-squared forecast error no smaller than that of f_{1t}. We define the forecast errors as $e_{it} = y_t - f_{it}$, $i = 1, 2$, where $\{y_t\}_{t=1}^{n}$ is the sequence of outcomes and f_{it} is the forecast made of period t at period $t - 1$, so the forecasts are one-step ahead. The forecast error of the combined forecast $f_{ct} = (1 - \lambda) f_{1t} + \lambda f_{2t}$ is given by ε_t, so that rearranging implies that the null hypothesis that forecast f_{2t} contains no useful information that is not already present in f_{1t} is given by $\lambda = 0$ in the ordinary least squares (OLS) regression:

$$e_{1t} = \lambda(e_{1t} - e_{2t}) + \varepsilon_t \tag{2}$$

That is, the expected squared error of the combined forecast is minimized by $\lambda = 0$, so that no weight is accorded to f_{2t}. The form of this regression implies that the individual forecasts are unbiased ($E(e_{it}) = 0$, $i = 1, 2$). We include an intercept in (2) to allow for biases in the forecasts. The weights on the individual forecasts are restricted to sum to unity, and we restrict λ to $0 \leq \lambda \leq 1$ to rule out forecasts being given negative weights (and weights in excess of one). The alternative hypothesis is therefore one-sided: $\lambda > 0$. The null of $\lambda = 0$ corresponds to $E(e_{1t}, e_{1t} - e_{2t}) = 0$ against the one-sided alternative $E(e_{1t}, e_{1t} - e_{2t}) > 0$.

[5]See *inter alia* Diebold and Lopez (1996) and Newbold and Harvey (2002) for recent surveys, and Clemen (1989) for an annotated bibliography.

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Under standard assumptions about forecast errors it follows from Diebold and Mariano (1995) that the t statistic that $\lambda = 0$ in (2) has an asymptotic standard normal distribution. However, Harvey *et al*. (1998) show that when the forecast errors are conditionally heteroscedastic ($E(e_{1t}^2|e_{1t} - e_{2t}) = g(e_{1t} - e_{2t})$) the standard test will be incorrectly sized, and they propose the use of heteroscedasticity-robust methods as well as a number of modifications to improve the small-sample performance, which we implement.

4 Forecasting Comparisons

The period up to end 1993 is taken as the 'in-sample' period, and the remainder of the data is the forecast period. The data are displayed in Fig. 1. There is a break in the personal crime series in 1998 due to definitional changes, so that we restrict the forecast period for this variable to 1998Q1. Personal crime is found to be an important explanatory variable for property crime, so we will consider models for property crime that include this variable with a relatively short forecast period (1994Q1–1998Q1) and models that 'marginalize' with respect to this, permitting an additional five years of observations in the forecast period.

The analysis of forecast accuracy uses one-step-ahead forecasts. We could calculate sequences of multi-step-ahead forecasts, but this would require modelling the explanatory variables to obtain true multi-step forecasts, or using the actual values of the explanatory variables. Neither strat-

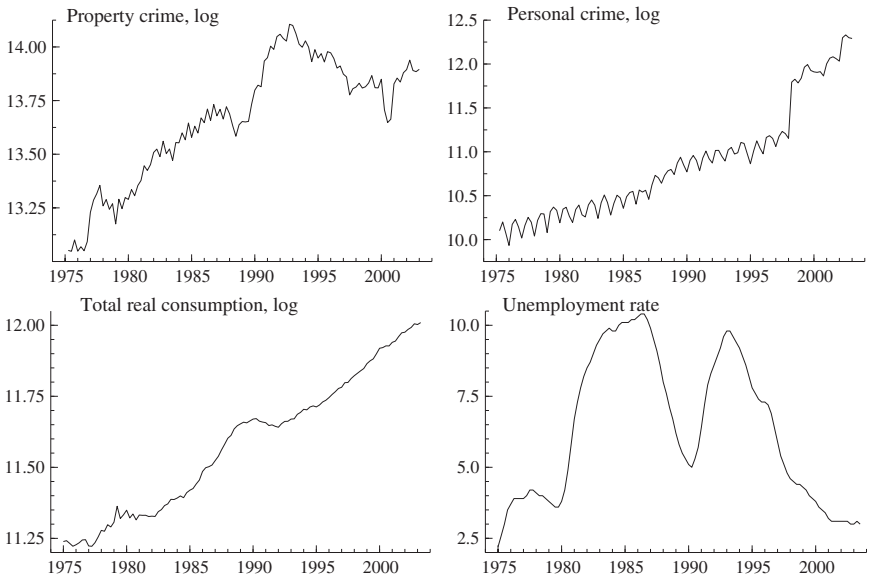

Fig. 1 Crime Series and Economic Variables

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egy appears very satisfactory: the results of the former will be influenced by the quality of the models for consumption and unemployment, whereas our focus is on modelling crime, and the latter is open to critiques of the sort levelled by Chong and Hendry (1986). As it is, we use the actual values of contemporaneous explanatory variables to derive the one-step-ahead forecasts. So these are not true *ex ante* forecasts, and operating in this way favours the economic models' forecasts relative to those of the time-series models. We also adopt a 'fixed forecasting' scheme, whereby the models' parameter estimates are not updated as the forecast horizon is moved through the data. Updating would probably improve the forecast accuracy of the models, but if the models change dramatically over the sample then it is not clear that a good forecast performance provides much support for the initial specification (see Clements and Hendry (2005) for a discussion of this and related issues).

4.1 Property Crime

Table 1 reports the results of estimating static regressions[6] for the log of property crime, where the explanatory variables are the log of total consumption and that variable plus the unemployment rate. When consumption alone is included we narrowly fail to reject the null of no cointegration at the 10 per cent level. Given the low power of such tests, these results point towards a possible cointegrating relationship between property crime and consumption. Including the unemployment rate results in less evidence against the null based on the augmented Dickey–Fuller (ADF) statistic, and in the following we assume that unemployment is not a determinant of long-run property crime.

Turning to the dynamic modelling of property crime, we began with a general specification that regressed the first difference of the log of property crime on four lags of itself, lags 1 to 4 of the fourth difference of the log of personal crime, lags 0 to 4 of the first differences of log consumption and of the first differences of the unemployment rate, and the ECM lagged one period. In addition, we included seasonal dummies. When the long-run relationship is given by the error correction (EC) term omitting the unemployment rate, joint F tests suggested that the changes in consumption could all be excluded, as could all the lag 3 and lag 4 terms. Further simplifications are possible, but were not enforced. The coefficient estimates and model diagnostics for the general specification of the ECM, and of the simplified version used in forecasting, are recorded in Table 2.

The one-step forecasts for the period 1994Q1–1998Q1 are shown in Fig. 2. It is apparent that the forecasts tend to overstate the actual changes over the period. The mean error is –0.02, indicating that the growth rate of crime

[6]See Engle and Granger (1987).

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TABLE 1
PROPERTY CRIME

Variable	*Coefficient*	*Standard error*	*t value*	*t probability*	*Partial R^2*
Regressing property crime on consumption by OLS, 1975Q2–1993Q4					
Constant	−5.117	1.032	4.96	0.00	0.252
Consumption	1.631	0.090	18.1	0.00	0.818
RSS = 1.158	$\hat{\sigma} = 0.126$	$R^2 = 0.818$	$\overline{R}^2 = 0.815$	DW = 0.227	t(ADF) = −2.928[a]
Regressing property crime on consumption and unemployment					
Constant	−2.535	0.854	2.97	0.00	0.109
Consumption	1.384	0.076	18.2	0.00	0.822
Unemployment	0.036	0.005	7.45	0.00	0.435
RSS = 0.654	$\hat{\sigma} = 0.095$	$R^2 = 0.897$	$\overline{R}^2 = 0.894$	DW = 0.352	t(ADF) = −2.617[a]

RSS, residual sum of squares; DW, Durbin–Watson.
[a]Lag augmentation = 4 and includes seasonal dummies. Approximate critical values (Davidson and MacKinnon, 1993, p. 722, Table 20.2) are −3.04 and −3.34 at the 10 and 5 per cent level, respectively, for the upper part, and −3.45 and −3.74, for the lower part.

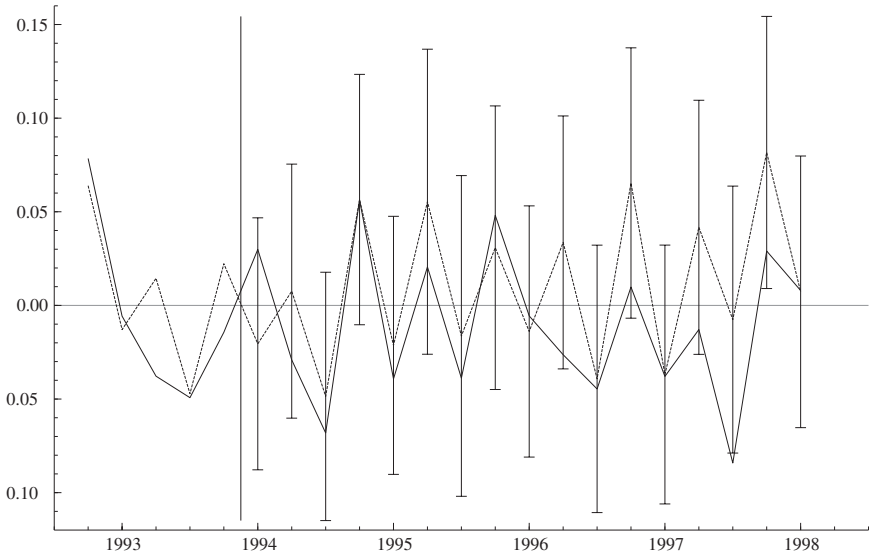

FIG. 2 Economic Model One-step Forecasts 1994Q1–1998Q1 (Dotted Line) and Actual Quarterly Changes in (Log) Property Crime (Solid Line). The Economic Model EC Term Excludes the Unemployment Rate. The Vertical Lines Denote 95 Per Cent Error Bands

was 2 per cent points lower than predicted on average for these 17 quarters. Similar results are obtained if the EC term includes the unemployment rate (in the same restricted dynamic specification) or if the EC term is excluded—crime is overpredicted although by around 1.5 per cent points on average in both instances.

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TABLE 2
(a) GENERAL UNRESTRICTED SPECIFICATION OF ECM MODEL AND (b) SIMPLIFIED ECM MODEL FOR PROPERTY CRIME

Variable	*Coefficient*	*Standard error*	*t value*	*t probability*	*Partial R^2*
(a) Modelling the first difference of property crime by OLS, 1977Q2–1993Q4					
EC term					
Lag 1	–0.206	0.065	3.17	0.003	0.186
Property crime difference					
Lag 1	0.200	0.139	1.43	0.159	0.046
Lag 2	0.092	0.133	0.69	0.492	0.011
Lag 3	–0.095	0.122	0.77	0.443	0.013
Lag 4	0.180	0.119	1.51	0.138	0.049
Personal crime difference					
Lag 1	–0.315	0.116	2.72	0.009	0.144
Lag 2	0.256	0.118	2.17	0.035	0.097
Lag 3	–0.147	0.117	1.26	0.215	0.035
Lag 4	–0.107	0.116	0.93	0.360	0.019
Consumption difference					
Lag 0	0.650	0.422	1.54	0.130	0.051
Lag 1	–0.000	0.395	0.00	1.000	0.000
Lag 2	–0.422	0.401	1.05	0.299	0.025
Lag 3	–0.342	0.403	0.85	0.400	0.016
Lag 4	0.223	0.391	0.57	0.571	0.007
Unemployment difference					
Lag 0	–0.015	0.036	0.41	0.685	0.004
Lag 1	0.125	0.052	2.39	0.021	0.115
Lag 2	–0.134	0.054	2.46	0.018	0.121
Lag 3	0.033	0.060	0.55	0.588	0.007
Lag 4	0.032	0.041	0.77	0.445	0.013
Constant	0.057	0.018	3.11	0.003	0.180
S1	–0.057	0.020	2.77	0.008	0.148
S2	–0.023	0.014	1.64	0.109	0.058
S3	–0.067	0.019	3.63	0.001	0.231
RSS = 0.042, $\hat{\sigma} = 0.031$, $R^2 = 0.753$, $\overline{R}^2 = 0.629$, DW = 1.97					
Model diagnostics:	F_{ar} (4, 40) = 2.256 [0.08]		F_{arch} (4, 36) = 0.625 [0.65]		
F_{het} (2, 41) = 0.083 [0.99]	F_{reset} (1, 43) = 0.604 [0.44]		χ^2_{norm} (2) = 1.864 [0.39]		
(b) Modelling the first difference of property crime by OLS, 1976Q3–1993Q4					
EC term	–0.119	0.048	2.49	0.016	0.089
Lagged one period					
First difference log of unemployment lagged one period	0.105	0.033	3.20	0.002	0.140
First difference log of unemployment lagged two periods	–0.063	0.036	1.74	0.087	0.046
Constant	0.050	0.009	5.65	0.000	0.336
S1	–0.055	0.012	4.43	0.000	0.238
S2	–0.019	0.012	1.54	0.128	0.036
S3	–0.080	0.012	6.55	0.000	0.406
RSS = 0.082, $\hat{\sigma} = 0.036$, $R^2 = 0.563$, $\overline{R}^2 = 0.521$, DW = 2.03					
One-step (*ex post*) forecast analysis 1994Q1–2003Q1. Parameter constancy forecast tests:					
Forecast $\chi^2(37) = 130.93$ [0.00]**		Chow $F(37,63) = 1.805$ [0.02]*			
Model diagnostics:	F_{ar} (4, 59) = 0.343 [0.85]		F_{arch} (4, 55) = 2.240 [0.08]		
F_{het} (21, 41) = 1.185 [0.31]	F_{reset} (1, 62) = 0.751 [0.39]		χ^2_{norm} (2) = 9.512 [0.01]		

The regressions include three seasonal dummy variables: S1 takes the value 1 in the first quarter of the year, etc.
*Significant at the 5 per cent level.
**Significant at the 1 per cent level.

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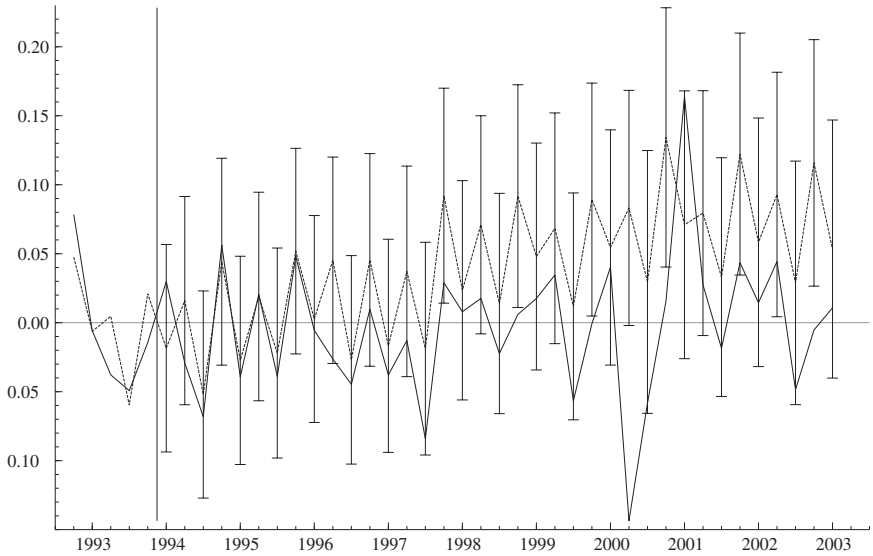

FIG. 3 Economic Model One-step Forecasts 1994Q1–2003Q1 (Dotted Line) and Actual Quarterly Changes in (Log) Property Crime (Solid Line). The Economic Model Excludes the Personal Crime Explanatory Variables, and the EC Term Excludes the Unemployment Rate

In order to exploit the last five years of the sample, we then estimate the same general model for property crime for the period up to 1993Q4, but omitting the personal crime terms. The own lag terms and changes in consumption were omitted on the basis of joint F tests, as were the lag 3 and 4 unemployment rate variables. The results are recorded in Fig. 3. We now focus on this longer forecast period, and the results for the various models we estimate are collected in Table 3. The first point to note is that the tendency to overpredict is exacerbated over the 1998–2002 period. The average bias is now nearly 5 per cent points a quarter (–0.044) and the root mean-squared forecast error (RMSE) at 0.068 is approximately double the in-sample estimated standard error, indicating a marked deterioration in the fit of the model to the data over the forecast period. Using the ECM with the unemployment rate (UR) in the above restricted model reduces the bias (with a commensurate reduction in RMSE), while omitting the ECM cuts the average overprediction dramatically to around 1.25 per cent. The forecasts from the model without an ECM are given in Fig. 4.

Why do the EC terms have such a bad effect on forecast performance? Figure 5 plots the EC terms over the whole sample. Towards the end of the 1990s the 'equilibrium error' becomes large and negative—crime is lower than would be expected based on the past long-run relationship—and the model predicts higher changes in crime, other things being equal, to restore the long-

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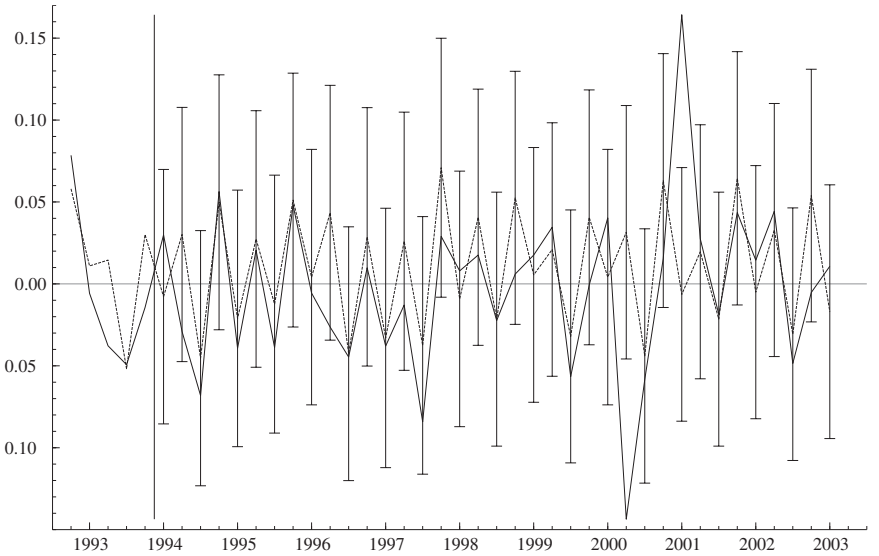

Fig. 4 Economic Model One-step Forecasts 1994Q1–2003Q1 (Dotted Line) and Actual Quarterly Changes in (Log) Property Crime (Solid Line). The Economic Model Has No Long-run Solution

Table 3
Comparison of Forecast Performance for Property Crime, One-step Ahead Forecasts

Model	*Bias*	*RMSE*
Property crime, 1994Q1–2003Q1		
(1) Economic model, EC omits UR	–0.044	0.068
(2) Economic model, EC includes UR	–0.030	0.056
(3) Economic model, no EC term	–0.013	0.050
(4) Time-series model	–0.015	0.053
Forecast combination, (1) + (4)	—	0.046
Forecast combination, (3) + (4)	—	0.046
Personal crime, 1994Q1–1998Q1		
Economic model, symmetric	–0.016	0.064
Economic model, asymmetric	–0.026	0.076
Time-series model	0.001	0.057

Note: The bias is the average forecast error, and RMSE the root mean-squared forecast error. The bias of the two forecast combinations is zero by construction.

run equilibrium. The problem appears to be that the long-run relationship between crime and the economic indicators has shifted. The reason for the relatively better performance of the model with UR in the EC term is also apparent.

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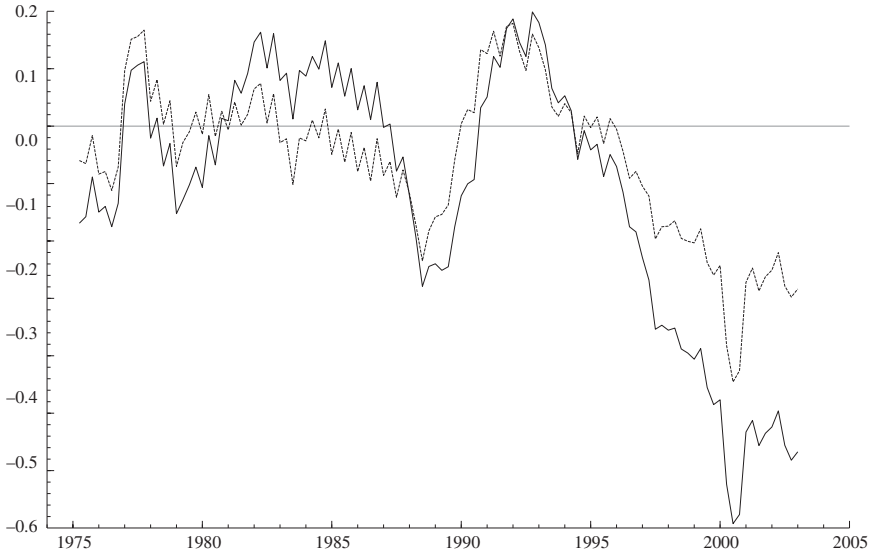

FIG. 5 Property Crime ECM Terms Based on Parameters Estimated up to 1993Q4. ECM Includes the Unemployment Rate (Broken Line) and Without the Unemployment Rate (Solid Line)

If we denote the log of property crime in period t by pc_t, then estimation of the time-series model for the first difference of the log of property crime ($\Delta\mathrm{pc}_t$, where $\Delta = 1 - L$) up to 1993Q4 yielded

$$(1-0.344L^4)\Delta\mathrm{pc}_t = 0.013 + (1-0.267L)(1+0.717L^4)\varepsilon_t$$

with an average forecast error of –0.015 and a RMSE of 0.053. Including seasonal dummies made the lag 1 moving average term insignificant, but the forecast performance was little changed, with a similar bias and a RMSE of 0.051. In the following we use the model without seasonals.

As is evident from the RMSEs recorded in Table 3, the time-series models are better than the 'error-correction' economic models, but are bettered by the model with no long-run solution. Recall that the latter model relates $\Delta\mathrm{pc}_t$ to the first difference in the unemployment rate (ΔUR) lagged one and two periods, $\Delta\mathrm{UR}_{t-1}$ and $\Delta\mathrm{UR}_{t-2}$ (as well as seasonal dummy terms). The coefficients on the unemployment rate difference terms are approximately of equal and opposite sign, so that the model implies that $\Delta\mathrm{pc}_t = 0.11 \times \Delta^2\mathrm{UR}_{t-1}$—property crime growth is only affected by an acceleration or deceleration in the changes in the unemployment rate. The impact of a continuous increase in the unemployment rate of 1 percentage point each quarter (e.g. 4 per cent to 5 per cent, then 5 per cent to 6 per cent) will be a 10 per cent increase in crime each quarter.

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Following the suggestion in Dickinson (1993) that crime may grow at a faster rate when unemployment increases than it declines when there are falls in unemployment, we experimented with a number of dummy variables of the sort used by Osborn to allow for possible asymmetries. Specifically, we defined two dummy variables $D_t^+ = 1(\Delta \mathrm{UR}_t > 0)$ and $\Delta \mathrm{UR}_t^+ = \Delta \mathrm{UR}_t \times D_t^+$, where $1(A)$ is the indicator variable, i.e. $1(A) = 1$ when the event A occurs and zero otherwise. So D_t^+ is unity when unemployment is increasing and zero otherwise, and $\Delta \mathrm{UR}_t^+$ is $\Delta \mathrm{UR}_t$ when $\Delta \mathrm{UR}_t > 0$ and zero otherwise. We entered the dummy variables with up to four lags in general specifications, as well as in the restricted specification described above, involving only lagged changes of the unemployment rate (and deterministic terms). The dummy variables were not significant at the 5 per cent level, and consequently had little influence on the forecasts.[7]

Notwithstanding the rankings of the models on RMSE, of interest is to see whether the time-series model (without seasonal dummies) contains any useful information not already incorporated in the economic models, and vice versa. The first comparison is between the model with an EC term (that excludes the unemployment rate) and the time-series model. We run a forecast-encompassing regression of the form of (2), where e_{1t} is taken to be the error from the economic model and e_{2t} the error from the time-series model. Given that both individual forecasts appear to be biased, a constant was included in the regression equation. We estimated λ at 0.50, suggesting that a linear combination that attached equal weights of one half to each forecast would have a smaller RMSE than the economic model. Moreover, the estimate of λ is significantly greater than zero, whether we use the OLS standard error for $\hat{\lambda}$ or a heteroscedastic-consistent standard error. This outcome is perhaps not surprising given the poor performance of the ECM forecasts. We can also reject the hypothesis of forecast encompassing in the reverse direction; that is, we find that the ECM forecasts lower the RMSE of the time-series model when used in combination.

Finally, we assess whether the forecasts from the model of $\Delta \mathrm{pc}_t$ on $\Delta^2 \mathrm{UR}_{t-1}$ and seasonals are conditionally efficient. We obtain a lower estimate of λ of 0.39, with a standard t value of 1.75, and a t value of 2.4 using a heteroscedastic-consistent standard error. Whichever we use we reject the null against the one-sided alternative that $\lambda > 0$ (approximately 5 per cent critical value of 1.69) and conclude that neither economic model forecast encompasses the time-series model's forecasts. The RMSEs of the combined forecasts are also recorded in Table 3 for ease of comparison. They are some 8 per cent smaller than those of the best individual forecasts (the economic model with no EC term and the time-series model). This comparison is somewhat artificial, as the combinations have been chosen as those that minimize

[7]Experimenting with similar asymmetry terms for the growth in consumers' expenditure also led to negative findings.

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the sums of squared forecast errors over the forecast period (and set the bias to zero). In practice the combination weights would need to be chosen on a 'training set' of forecast errors—our relatively small data sample precludes this possibility. However, the prescription to take a simple average would yield weights very similar to those in the first combination, and presumably gains of a similar magnitude, so the reductions in RMSE recorded in the Table 3 may well be achievable.

4.2 Personal Crime

Table 4 reports the cointegrating regressions for personal crime. The ADF tests for cointegration suggest that personal crime is not cointegrated with consumption or with consumption and unemployment, consistent with the findings of Osborn. For the dynamic model, we began with a general specification that regressed the fourth difference of the log of personal crime on four lags of itself, lags 1 to 4 of the difference of the log of property crime, and lags 0 to 4 of the first differences of log consumption and of the first differences of the unemployment rate. After simplifying we obtained the model set out in Table 5. Since we do not find any cointegrating relationship, we exclude ECM terms. The results are striking. The coefficients on both the consumption difference and its lag are statistically significant at the 10 per cent significance level and of the same signs as those found by Osborn. The independent effect of consumption suggests that personal crime increases as consumption increases—matching the prediction of Field (1990). However, the coefficient on the unemployment difference term is statistically significant at the 5 per cent significance level and of *opposite* sign to Field for the violence model. To the extent that low unemployment and high consumption are linked, this relationship suggests that personal crime increases in booms

Table 4
Personal Crime

Variable	*Coefficient*	*Standard error*	*t value*	*t probability*	*Partial* R^2
Regressing personal crime on consumption by OLS, 1975Q2–1993Q4					
Constant	–9.970	0.721	13.8	0.00	0.724
Consumption	1.789	0.063	28.4	0.00	0.917
RSS = 0.566	$\hat{\sigma} = 0.088$	$R^2 = 0.917$	$\bar{R}^2 = 0.916$	DW = 1.62	t(ADF) = –1.49[a]
Regressing personal crime on consumption and unemployment					
Constant	–9.776	0.793	12.3	0.00	0.679
Consumption	1.770	0.070	25.1	0.00	0.898
Unemployment	0.003	0.004	0.6	0.55	0.005
RSS = 0.563	$\hat{\sigma} = 0.088$	$R^2 = 0.917$	$\bar{R}^2 = 0.915$	DW = 1.62	t(ADF) = –1.44[a]

[a]Lag augmentation = 4 and includes seasonal dummies. Approximate critical values (Davidson and MacKinnon, 1993, p. 722, Table 20.2) are –3.04 and –3.34 at the 10 and 5 per cent level, respectively, for the upper part, and –3.45 and –3.74 for the lower part.

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Table 5
Econometric Forecasting Model for Personal Crime

Variable	*Coefficient*	*Standard error*	*t value*	*t probability*	*Partial R^2*
Fourth difference log of personal crime lagged one period	0.235	0.123	1.91	0.061	0.056
First difference of unemployment lagged one period	–0.041	0.018	2.22	0.030	0.074
First difference log of consumption	0.738	0.370	2.00	0.050	0.060
First difference log of consumption lagged one period	0.654	0.361	1.81	0.075	0.050
Constant	0.032	0.010	3.27	0.002	0.147

RSS = 0.088, $\hat{\sigma} = 0.038$, $R^2 = 0.402$, $\overline{R}^2 = 0.364$, DW = 1.87

One-step (*ex post*) forecast analysis 1994Q1–1998Q1. Parameter constancy forecast tests:
Forecast $\chi^2(17) = 48.418$ [0.00]** Chow $F(17, 62) = 2.281$ [0.00]**

Model diagnostics: $F_{ar}(4, 58) = 1.897$ [0.12] $F_{arch}(4, 54) = 0.392$ [0.81]
$F_{het}(14, 47) = 0.517$ [0.91] $F_{reset}(1, 61) = 3.745$ [0.06] $\chi^2_{norm}(2) = 1.60$ [0.45]

Modelling the fourth difference of personal crime by OLS, 1977Q2–1998Q1 less 17 forecasts. Forecast period 1994Q1–1998Q1.
**Significant at the 1 per cent level.

and falls in recessions. This result conflicts with the US evidence reported in Fajnzylber *et al*. (2002a, 2002b) where homicide rates are found to be counter-cyclical. The in-sample diagnostic tests suggest no clear signs of mis-specification, although the RESET (regression error specification test) test (of possible functional form mis-specification) is close to significance at the 5 per cent level. The values of the coefficient estimates were reasonably robust to various re-specifications. For example, entering the unemployment term contemporaneously instead of lagged one period yielded essentially the same empirical model. Figure 6 suggests that the model tracks actual annual changes reasonably well in-sample, although misses the main out-of-sample dip. The mean error and RMSE are recorded in Table 3.

We also considered the evidence for an asymmetric relationship between personal crime and the economic variables. A number of models were estimated allowing increases and decreases in the economic variables to have different effects, but in line with the results for property crime, in no cases were the dummy variables interacted with consumption significant at the 5 per cent level. So here we focus on models with asymmetric unemployment terms. Our preferred parsimonious specification is given in Table 6, where the dummy variable referred to there is positive when unemployment increases (and is zero otherwise), and multiplies the unemployment rate. This corresponds to Table 5 augmented by the inclusion of the unemployment variable interacted with the dummy variable. This specification was obtained from a general

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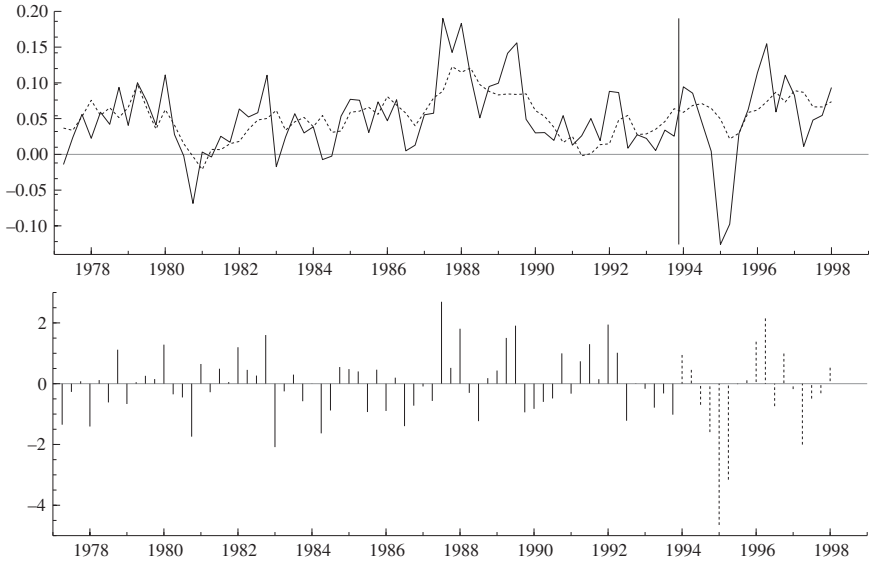

FIG. 6 Economic Model for Personal Crime (Symmetric Unemployment Rate Effects). The Top Panel Displays the Annual Change in Personal Crime (Solid Line), and the Fitted Value (Up to 1993Q4) and Thereafter the One-step Forecasts (Dotted Line). The Bottom Panel Displays the (Scaled) In-sample Errors and the One-step Forecast Errors

dynamic specification. There is some evidence of autocorrelated errors, but this was also present in the more general formulations. The model estimates, reported in Table 6, provide evidence of an asymmetric response to changes in unemployment. Personal crime is seen to exhibit a larger response to falling than to rising unemployment. A 1 per cent point fall in the unemployment rate leads to a 12 per cent rise in crime over the year while a corresponding rise in the unemployment rate leads to a small 0.5 per cent fall in crime. The response to a rise in unemployment is not in fact significant, as is evident from the failure to reject the null of equal and opposite signs on the two unemployment terms (not reported). Imposing this restriction makes little difference to the model estimates or forecasts.

Comparing the estimates and summary statistics given in Tables 5 and 6, and the forecast statistics in Table 3, there are several salient points. First, allowing for asymmetry provides a better fit within the sample period. Second, although the fit is improved, the overprediction is exacerbated by a full 1 percentage point on average over the 17 quarters. In this instance, allowing for the asymmetry found by Osborn and others is not helpful from a forecasting perspective. Third, the coefficient on the lagged personal crime variable becomes insignificant in the model with asymmetries and, for the same equation, there is some evidence of residual serial correlation.

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Table 6
Econometric Forecasting Model for Personal Crime
(with an Asymmetric Term)

Variable	*Coefficient*	*Standard error*	*t value*	*t probability*	*Partial R^2*
Fourth difference log of personal crime lagged one period	0.144	0.123	1.18	0.242	0.022
First difference of unemployment lagged one period × dummy	0.119	0.045	2.68	0.010	0.105
First difference of unemployment lagged one period	–0.124	0.036	3.47	0.001	0.165
First difference log of consumption	0.755	0.353	2.14	0.036	0.070
First difference log of consumption lagged one period	0.681	0.344	1.98	0.053	0.060
Constant	0.022	0.010	2.20	0.031	0.074

RSS = 0.079, $\hat{\sigma} = 0.036$, $R^2 = 0.465$, $\overline{R}^2 = 0.421$, DW = 1.89

One-step (*ex post*) forecast analysis 1994Q1–1998Q1. Parameter constancy forecast tests:
Forecast $\chi^2(17) = 76.719$ [0.00]** Chow $F(17, 62) = 2.909$ [0.00]**

Model diagnostics: $F_{ar}(4, 58) = 2.799$ [0.03]* $F_{arch}(4, 53) = 0.479$ [0.75]
$F_{het}(19, 41) = 0.627$ [0.86] $F_{reset}(1, 60) = 0.539$ [0.47] $\chi^2_{norm}(2) = 0.433$ [0.81]

Modelling the fourth difference of personal crime by OLS, 1977Q2–1998Q1 less 17 forecasts. Forecast period 1994Q1–1998Q1.
*Significant at the 5 per cent level.
**Significant at the 1 per cent level.

Finally, estimating a simple first-order autoregressive model for the fourth difference of the log of personal crime up to 1993Q4 yielded a mean error of virtually zero, faring better at tracking actual changes over the forecast period. The RMSE was also lower than for the economic models.

We then calculated forecast-encompassing tests of whether the economic model forecasts can improve upon the AR(1) model forecasts, when used in combination. Unconstrained estimates of λ based on (2) indicated a weight in excess of one for the time-series model, and consequently a negative weight for the economic model. Without the requirement that the weights on the two forecasts sum to one, we again found a weight in excess of one on the time-series model forecasts and a very small positive weight attached to the economic model forecasts. Due to the small sample of forecasts, we were not able to formally reject the null that the coefficient on the time-series model forecasts in this latter case was significantly different from zero. Nevertheless, the magnitudes of the estimated weights on the competing forecasts point to

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the superiority of the time-series model, and suggest little is to be gained from combination with the economic model.

5 Conclusion

There are a number of implications of our results on forecasting crime. The first is that the current preoccupation with modelling the long-run relationships between crime and economic indicators may be misplaced if the aim is to forecast crime. Models that incorporate long-run relationships will generate poor forecasts if—as appears to be the case here for property crime—the long-run relationships are non-constant over the forecast period. That the long-run relationship between property crime and the unemployment rate is non-constant over the last 10 years is a catalyst for seeking a better specification of the long run from a modelling perspective. But from a forecasting perspective models that eschew long-run relationships exhibit greater adaptability and more accurate forecasts in the face of certain types of structural change.

Second, forecasts from economic models might be competitive with those of time-series models, but it might be overoptimistic to suppose that they will exploit all the information in the latter. This suggests routine testing for forecast encompassing, and using combinations of forecasts when encompassing is rejected. In the case of forecasting property crime, we found that an approximate simple average of two models' forecasts had a smaller mean squared forecast error than either alone.

Third, in the case of personal crime, we were unable to find a cointegrating relationship, and instead report a more substantive analysis of asymmetry effects in models specified solely in differences. Again, a good in-sample fit is no guarantee of a good forecast performance. A model with statistically significant asymmetric unemployment rate terms generated less accurate forecasts than a model without such terms, and neither model had a smaller mean squared forecast error than a univariate autoregression.

Finally, better models (both 'economic' and 'time-series') no doubt exist than those whose forecast performance we have commented upon in this paper. Our goal has been to shed some light on the forecast performance of models that incorporate economic indicators relative to time-series models, and to do so based on reasonable runs of forecasts.

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