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To change or not to change

The evolution of forecasting models at the Bank of England

Aurélien Goutsmedt* Francesco Sergi[†] Béatrice Cherrier[‡]

Juan Acosta[§] Clément Fontan[¶] François Claveau^{||}

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1 Introduction

In the fall of 2011, the Bank of England’s Monetary Policy Committee (MPC) started to rely on a new forecasting platform to inform its monetary policy choices. This platform introduced COMPASS, a “new Keynesian” Dynamic Stochastic General Equilibrium (DSGE) model, a type of macroeconomic model that had become widespread in policymaking institutions over the previous decade. The implementation of COMPASS may seem surprising, especially considering that it came only four years after the onset of the Global Financial Crisis. Indeed, the crisis led many to criticise DSGE models for failing to include financial and banking components, thus mischaracterising the economic system and, ultimately, being blind to the signals of an impending crisis.¹ Why then was COMPASS adopted in this context? More broadly, why do agents within a policymaking organisation decide to throw away

*UCLouvain, ISPOLE; F.R.S.-FNRS.

[†]Université Paris-Est Créteil, LIPHA.

[‡]CNRS; Ecole Polytechnique, CREST.

[§]Universidad de los Andes, Facultad de Economía.

[¶]UCLouvain, ISPOLE; USL-B, IEE.

^{||}Université de Sherbrooke; CIRST.

¹See e.g. Stiglitz (2011), Romer (2016), or Vines and Wills (2018), for post-crisis debates on the relevance of DSGE models. Despite these criticisms and despite the advocacy for new approaches (e.g. Haldane 2012; Mehrling 2010), the case of the Bank of England is not an exception: DSGE models (even if amended and extended) remained widely used in academia and in policymaking institutions, notably in central banks, for forecasting and policy analysis (e.g. Gürkaynak and Tille 2017).

one model and develop a new one? Alternatively, why do they decide to stick with what they have? In this paper, we provide answers to these questions by studying the evolution of modelling practices at the Bank of England between 1974 and 2014.

The literature on economic models is vast, but most of it does not attempt to explain the persistence or change of modelling practices.² The focus of authors with a philosophical leaning—including influential contributors such as Morgan and Morrison (1999); Sugden (2000); Mäki (2005); Boumans (2005); Morgan (2012); Rodrik (2015)—is typically on the nature and functions of models (Morgan and Knuuttila 2012). The same focus persists in recent philosophical contributions about macroeconomic models (e.g., Kuorikoski and Lehtinen 2018). Within social studies of finance and economics, most research efforts are directed toward documenting the performativity of economic modelling—i.e., the sociopolitical effects of modelling practices (MacKenzie, Muniesa, and Siu 2007)—with significant attention recently placed on macroeconomic models (Henriksen 2013; Braun 2014; Ban 2015; Heimberger, Huber, and Kapeller 2020).

In the smaller subset of literature aiming to explain model persistence and change, various explanatory factors are emphasised. Standard approaches in the history of economics tend to prioritise the agency of academic economists. Studies on macroeconomics discuss at length the influence of John Maynard Keynes, Robert Lucas, James Tobin, and other major academic economists (e.g., Snowdon and Vane 2005; De Vroey 2016; Ingraio and Sardoni 2019). Although these scholars are essential references, centring the history of macroeconomic modelling on them generates various blind spots. In fact, recent scholarship demonstrates “how much of the history of macroeconomic modelling took place outside universities” (Boumans and Duarte 2019). Consequently, we should pay attention to the *agency of modellers* outside academia, by drawing for instance on the sociology of the economics profession (Fourcade 2009; Coman 2019). Such agency is however not what most of the existing literature on model changes has focused on. Sociologists and political scientists single out *institutional factors* that shape the evolution of models, that is, the opportunities and constraints faced by modellers. Helgadóttir (2021a); Helgadóttir (2021b) argues that

²Our adoption of the concept of “practices” builds on a decade-long impetus in this direction by scholars in the history of economics (Maas, Mata, and Davis 2011; Stapleford 2017).

the “publish or perish” characteristic of the academic environment is an important explanatory factor of the rise of “portable models” such as Real Business Cycle models in the 1980s. Helgadóttir and Ban (2021) also explain model persistence in academia with institutional factors: the control of gatekeeping positions (e.g., editorial board membership of ‘top’ journals) by mainstream economists would fuel the persistence of the rational expectations assumption in academic macroeconomic modelling despite vigorous criticisms in the aftermath of the Global Financial Crisis.

Institutional factors also play a role within policymaking organisations, which bring their own set of constraints and opportunities for modelling practices. The fact that models act as ‘bridges’ between researchers and policymakers in these organisations (Butter and Morgan 2003) implies that the beliefs, methodological preferences and priorities of senior policymakers affect in-house modelling practices. These practices also respond to institutional logics beyond the organisation. For instance, Mudge and Vauchez (2018, 268) explain the persistence of the main macroeconomic model of the European Central Bank (ECB), despite its predictive limitations, by the embeddedness of this organisation in “three transnational systems of relations: professional economics, transnationalized finance, and EU institutions.” According to these authors, this position of the ECB makes it “too embedded to fail.”

Finally, historians of modelling practices need to be attentive to another type of opportunities and constraints that come from the fact that models are artefacts (Halsmayer 2017, 2018)—a point already underlined in the historical and philosophical work of Morgan (2012) among others. These tools are powerful, but also unwieldy. Implementing new macroeconomic models requires large amounts of funds, infrastructure, and technical know-how, and maintaining them is almost equally challenging. The *material* factor is important to account for the persistence or change of modelling practices.

This article is a contribution to the history of macroeconomic modelling in policymaking organisations. In carefully studying four episodes in the evolution of modelling practices at the Bank of England over five decades, we highlight the hitherto understudied *agency of modellers*—how their background, beliefs and methodological

preferences can partly explain persistence and change. Second, we underline two kinds of *institutional* factors: direct and indirect. Direct institutional factors refer to power struggles between the Bank and other UK public authorities, changes of mandate and function as well as to preferences of the Bank of England policymakers on modelling functions. Indirect institutional factors refer to the intellectual context about modelling, which is shaped by academia and modelling practices within other central banks. Third, we explain how *material* factors constrain modelling routines and practices, and thus might be conducive to model change. The main lesson of our research is one of diversity: an explanation of persistence or change must be sought for each episode in the rather unique configuration of agency, institutional forces and material reality. Our article can thus be read as a plea to be attentive to these three general explanatory factors.³

This article also advocates for a careful analysis of the identity of each model in a study of the evolution of modelling practices. Indeed, to explain *why* practices change, we must first identify *how* they change. The bird’s eye view adopted by most of the literature on model persistence or change—e.g., lumping big families of models under labels such as “DSGE”—obfuscate model characteristics which might either weigh on or be influenced by intellectual, institutional or material constraints. To describe models properly, we draw on the aforementioned historical and philosophical work on the nature of models, e.g., Boumans (1999)’s idea that models are “recipes.” Modellers first choose a theoretical framework and some key mechanisms. Modellers must also make choices regarding the *size* of the model (such as the number of variables and behavioural equations included in the model), its *centrality* (should policymakers rely on a single model or on a suite of models?) and the empirical strategy for running the model. The choice to build a smaller model, for instance, may be related among other things to computational limitations, policymakers’ preferences, or modellers’ training. It is therefore necessary to disaggregate the different modelling choices: these choices make visible the interplay between the agency of modellers and the institutional and material factors they face.

³For recent historical research adopting the same perspective to study the emergence of macroeconomic modelling in the 1960s and early 1970s in the US Federal Reserve, see Cherrier and Backhouse (2019) and Acosta and Cherrier (2021).

We provide the first detailed history of macroeconomic modelling at the Bank of England (hereafter, BoE or the Bank),⁴ based on a large set of documents collected at the BoE Archive or on the BoE website, as well as on 30 semi-structured interviews conducted between October 2019 and May 2021 with former and current BoE executives and staff.⁵ Each of the next four sections focuses on one episode with its specific configuration of factors. In 1979 Margaret Thatcher won the general election and formed a government strongly influenced by monetarist ideas. It was a period where monetarist ideas shattered both the academic and political consensus. And yet, this period of intellectual turmoil left the BoE model unchanged (Section 2). In 1992, the UK exited the European Monetary System; this led the UK government to rethink its monetary regime and to give more responsibilities to the BoE, which eventually became independent in 1997. In Section 3, we explain how this new mandate led BoE economists to develop a new model, one that however did not reflect the recent transformations in macroeconomic scholarship. Conversely, it was the growing gap between BoE practices and the rise of DSGE models in academia that largely drove the next model change in 2003 (Section 4). The final model replacement in 2011 was the result of preexisting dissatisfaction with the previous model, more than a consequence of the Great Financial Crisis. BoE modellers settled on a textbook DSGE model. This change seems unrelated both to the challenges that DSGE models faced and to the new policy context (e.g., the implementation of quantitative easing after the financial crisis). It rather reflected a shift in the practices and epistemological status of macroeconomic models used for forecasting. The new model indeed came embedded in a suite of diverse models (Section 5).

⁴Current historical scholarship on the BoE has focused on the institutional context and the history of policy decisions (Elgie and Thompson 1998; Needham 2014; Kynaston 2017; James 2020). The history of macroeconomic modelling in the UK has focused on other institutions than the Bank (Ball and Holly 1991; Barrell, Blake, and Young 2018; Hall and Henry 2018). Hendry and Muellbauer (2018) documents the genealogy of BoE macroeconomic models viewed through the lenses of methodological and technical debates.

⁵This article is also informed by previous research on the Bank (Acosta et al. 2023) and, even if not used directly in this article, the quantitative analyses undertaken previously.

2 Resisting Thatcher and monetarism

The Bank started using an in-house macroeconomic model in 1972, after acquiring a copy of the one developed by the London Business School (LBS).⁶ The model was handled by the economic staff of the Economics Division.⁷ The staff saw the model forecasts as serving two purposes: “First, they enable the Bank to influence [H.M. Treasury’s] forecasts; secondly, they are a source of information within the Bank.”⁸ The first purpose was related to the UK’s monetary framework in the 1970s (one that remained until the Bank’s independence in 1997): the government (through the Treasury) had the final say on monetary policy. The Bank simply suggested policy changes ahead of decisions, and then operationalised the Treasury’s decisions (Elgie and Thompson 1998, 58–60). The Treasury also maintained indirect control over many activities of the Bank, for instance by orienting what would or would not be published in the Bank’s *Quarterly Bulletin* (James 2020, 27). This also determined the restricted circulation of the Bank’s model forecasts, which could not be disclosed publicly (Staff economist 14, interview). However, even within these limitations, acquiring a macroeconomic model gave the Bank some intellectual autonomy *vis-à-vis* the Treasury as it allowed them to produce their own scenarios and forecasts—even if they could not be published. In other words, it was the power struggle between the BoE and the Treasury that spearheaded macroeconomic modelling practices at the Bank.

The first description of the Bank’s forecasting model was published in Latter (1979). The model consisted of several hundred variables and equations, disaggregating the functioning of the UK economy in dozens of distinct sectors and types of goods (distinguishing, for instance, between “beer” and “wine & spirits” as parts

⁶This occurred in an era where macroeconomic modelling was still not seen as the “norm” in policymaking in the UK (Ball and Holly 1991, 199–201). In fact, its advocates in the early 1970s felt they were facing a “crusade against quantification in economics” (Peacock 1975, v).

⁷The BoE organisational chart has evolved numerous times across the decades. In the 1970s, the “Economic Section” (then “Economics Division”), was tasked with conducting “studies which have a bearing on the choice of official policies and operational strategy and to undertake longer-term research on the working of the monetary system and other topics of concern to the Bank.” (“Economic Intelligence Department and Economic Section”, 01/1974, EID8/7, BoE Archives.) For a discussion of these internal changes see Acosta et al. (2023).

⁸Ash to Dow *et al.*, “Draft minutes of the Model Development Group, meeting of Dec 11 1975”, 29/12/1975, 10A216/5, BoE Archives.

of the consumers' spending on non-durable goods). The level of disaggregation was continuously adjusted, following the approach to macroeconomic modelling initiated by Lawrence Klein (Hoover 2012; Pinzón-Fuchs 2017; Goutsmedt 2019). The underlying assumption of this intellectual agenda was that modelling the dynamics of national economies required describing sectors at a fairly granular level, then aggregating the resulting sets of equations used to describe these sectors. The model's dynamics was driven by changes in income and aggregate demand. Nonetheless, Latter (1979, 9) warned the reader against regarding the model as a simple "Keynesian" apparatus, because additional features "set the model at some distance from what the word 'Keynesian' usually means".

The mechanisms of the Bank model were maybe "at some distance" from standard "Keynesianism", but they were nevertheless at an even greater distance from alternative "anti-Keynesian" frameworks—i.e. the contributions of new classical economists like Thomas Sargent and Robert Lucas, or of monetarists like Milton Friedman. First, prices in the Bank's model were determined by a set of cost-push factors (productivity, wages, world prices, the exchange rate, and indirect taxes) rather than by monetary forces.⁹ The effect of "financial variables" was "not in aggregate very great" and was indirect, through the effect of monetary base growth on exchange rates (Latter 1979, 8).¹⁰ The model was thus far from Friedman's adage "inflation is always and everywhere a monetary phenomenon" (Friedman 1970, 24). Furthermore, the model did not include any explicit equation describing the determinants of the demand for money, and the supply of money simply accommodated the demand (Latter 1979, 8). Second, wages were determined by an exogenous markup on past prices. The level of activity, thus the level of unemployment, only influenced average earnings (6). Hence, the model did not adopt the "expectation-augmented Phillips curve" that had spread during the 1970s (Goutsmedt and Rubin 2018).¹¹ Finally, the

⁹The Bank modellers' formalisation of inflation was relatively consistent with UK policymakers' understanding of inflation in the 1970s. For instance, DiCecio and Nelson (2013) have documented the dominance, before 1979, of "nonmonetary approaches to inflation analysis and control" (394).

¹⁰This mechanism could be seen as consistent with the monetary approach of the balance of payment (Frenkel and Johnson 1976), although the model did not follow the "law of one price" (Latter 1979, 9).

¹¹Prices are mainly determined by costs, but the equation is not homogeneous in costs (Latter 1979, 45). In other words, even if past prices were taken into account, this formalisation is not

paper did not even mention the Lucas critique (Lucas 1976), rational expectations, or even microfoundations.¹²

These features were the direct result of the agency of the BoE modellers. Their approach to modelling was consistent with the methodological preferences of many other modelling teams, in the UK and abroad—like, for instance, those of the Fed’s MPS model (Rancan 2020), or of the Treasury’s and the National Institute of Economic and Social Research’s models (Barrell, Blake, and Young 2018). However, the persistence of stagflation in developed countries stimulated criticisms against ‘Keynesianism’ (and of the tools that appeared associated with it) in political debates. In the UK, monetarism, disseminated by newspapers and editorialists, gained wider popularity and captivated many Conservatives (D. Smith 1987, chap. 4; Needham 2014, chap. 4). The arrival of Margaret Thatcher at 10 Downing Street further consolidated the popularity of Friedman’s ideas. The relationship between the government and the Treasury on one side, and the Bank on the other, was deeply altered. For the first time since inflation had soared in the late 1960s, the Bank was confronted with politicians who endorsed a strong stance against inflation and who pushed for a new doctrine regarding monetary policy (James 2020, 71).

In March 1980 the government launched the Medium Term Financial Strategy (MTFS). The strategy specified money supply targets for monetary policy. In doing so, they explicitly relied on key monetarist assumptions (Best 2020, 603–4) and on the idea that such “control of the money supply will over a period of years reduce the rate of inflation” (James 2020, 70).¹³

Contrary to the government, BoE officials were reluctant to endorse a strict targetting of £M3. Many of them, for instance monetarist economist Charles Goodhart, who was the Bank’s “special advisor” on monetary policy, had concluded from attempts

compatible with the natural rate (or equilibrium rate) of unemployment (Friedman 1968) and the verticality of the long-run Phillips curve. It thus seems that the Bank’s model displayed the same resistance against Friedman’s and New Classical economists’ contributions on inflation determination as the Fed’s MPS forecasting model (Rancan 2020).

¹²For a presentation of new classical contributions in the 1970s, see De Vroey (2016); see Goutsmedt et al. (2019) for a more in-depth analysis of the context of the criticisms addressed to macroeconomic models.

¹³The target was £M3, an aggregate that had grown by 16% a year in 1977-1979. The government wanted to shrink its expansion from 7% to 11% a year.

to quantify the demand of money that the relation between the demand for money, income, and interest rates as unstable (James 2020, 76) During a meeting with the Chancellor, the Bank’s Governor Gordon Richardson apparently criticised the MTFs for being “undesirably dogmatic, mechanical and rigid” (Needham 2014, 149). The Bank’s Deputy Governor Kit MacMahon invoked “Goodhart’s law” to defend the necessity to target a larger set of variables, and not just £M3 (*ibid.*).¹⁴

Even before Thatcher’s election and the launch of MTFs, monetarism and the targetting of money aggregates had been regularly discussed at the Bank. For instance, the first meeting of the BoE’s “Panel of Economic Consultants” (1977) reached the conclusion that “the adoption of monetary targets by the Bank does not imply wholesale conversion to monetarism [...] it is possible to believe in the importance of money & not be a convinced monetarist”.¹⁵ Regarding the Bank’s model, the economic situation and the resulting policy debates pushed the modellers, for instance, to pay more attention to the Bank’s monetary sector.¹⁶ However, Thatcher’s government’s intransigence later forced the Bank into a defensive stance.

This power struggle explains why the Bank’s executives did not press modellers at the Economics Division to transform their model fundamentally.¹⁷ First, the Head of the Division, Dow, remained a faithful Keynesian. Second, the Bank’s staff decided to further its analysis of the links between monetary aggregates and inflation in a separate “small monetary model,” rather than by revising the main model. A first version was presented by Richard Coghlan (1979), who promoted his model as “an alternative [...] which places much greater emphasis on the supply of money”, than the “essentially ‘Keynesian’ ” model of the Bank (5). He acknowledged that “certain

¹⁴“Goodhart’s law” is the idea that “any observed statistical regularity will tend to collapse once pressure is placed upon it for control purposes” (Goodhart 1984).

¹⁵“Panel of Academic Consultants, First meeting 5th October 1977: Monetary targets”, 31/10/1977, EID19/4, BoE Archives. Later on, Christopher Dow, the chief economist of the Bank at the time, or John Fforde, an executive director, imagined different arguments to make the Bank appear relatively monetarist and thus counter the attacks of monetarists (James 2020, 80–81).

¹⁶The lack of an adequate monetary sector was not exclusive to the Bank’s model (Dicks-Mireaux 1975, 125–26), and all UK modelling teams worked on this issue throughout the 1970s.

¹⁷The subsequent failures of monetary targetting due to financial market deregulation and the release of exchange controls led the adoption of a more “pragmatic” stance towards monetarism by the government (Best 2020; Clift 2019). This released the intellectual pressure off the Bank, and thus off modellers as well.

monetary influences” had been introduced recently in large-scale macroeconomic models, like the one of the Bank, but highlighted the merits of studying monetary issues separately. He explained that this additional model was not a “monetarist” one, “since the money stock [was] not regarded as an exogenous variable directly under the control of the authorities” (8) but as endogenously determined by demand. Likewise, Goodhart cautioned against interpreting the small monetary model as “a specifically monetarist framework”. Rather, “its main advantage is that it explores [...] the disequilibrium approach to monetary determination” and, as claimed by Coghlan, “this could be considered as consistent with Keynesian analysis as with monetarist analysis”.¹⁸ Other similar projects were under consideration in the early 1980s. Goodhart suggested that the Bank should fund the development and the estimation of a monetary model by James Davidson and David Hendry at the London School of Economics (LSE).¹⁹ For Peter Brierley, from the Bank’s staff, this model would “facilitate an evaluation of policy issues within a specifically monetarist framework” and the results could be “compared with those produced by the Bank and Treasury large structural models, which basically reflected a more Keynesian approach”.²⁰ Overall, Goodhart and the staff took seriously the challenges raised by monetarism, but they did not consider the alternative modelling strategies that they developed as a rejection of the “Keynesian” approach.

In sum, the absence of a model change is mainly explained by the agency of Bank modellers in the context of a power struggle between the Bank and the Treasury. Policymakers provided a buffer against Thatcher’s Monetarism, whereas the modellers’ own beliefs regarding the working of the UK economy determined the model’s specification. Beyond the power struggle between the Bank and the Treasury, a

¹⁸Goodhart, “Small Monetary Model - Collaboration with Davidson”, 5/10/1981, 6A160/3, BoE Archives.

¹⁹Goodhart, “Davidson and the small monetary model”, 5/11/1981, 6A160/3, BoE Archives.

²⁰Brierley, “Small Monetary Model - Collaboration with Davidson”, 2/10/1981, 6A160/3, BoE Archives. In the research project they transmitted to the Bank in October 1981, Davidson and Hendry made clear that their approach “provide[d] a new framework for the testing of established theories such as the monetary approach to the balance of payments . . . and the ‘monetarist’ view of inflation” (Davidson and Hendry, “A Proposal for Further Research on Econometric Modelling of the UK Monetary Sector”, 26/10/1981, 6A160/3, BoE Archives). Hendry regularly opposed monetarists in the 1980s, notably Friedman and Schwartz (1963) results (see Hendry and Ericsson 1991).

second direct institutional factor was also at play: the function and place of the model in the Bank’s organisational structure during this period. At the time, modellers and economists, in general, did not have the role and importance they would get in the 1990s (James 2020, 28–29; Acosta et al. 2023). Crucially, although the model had been acquired and developed to strengthen the Bank’s expertise against the Treasury, the implementation stage of monetary policy was not discussed within the Economics Division. Finally, as the modelling work played only a minor role at the Bank, which resulted in a larger agency for modellers, it also placed strong material constraints on them—mainly in the form of a lack of personnel, which resulted in a growing backlog of pending modifications to the model.²¹

3 Small is beautiful: The development of MTMM in the early 1990s

In September 1992, growing deficits and speculation against the pound forced the UK government to exit the European Exchange Rate Mechanism. The move was largely considered as the result of a failure in monetary policy. The next month, Chancellor Norman Lamont announced that the government would set an inflation target and that the Bank “would be responsible for monitoring the Government’s progress” towards the target through the publication of a quarterly *Inflation Report* (Lamont in Elgie and Thompson 1998, 77). Monthly BoE-Treasury meetings would be planned in advance and would lead to the publication of the meeting minutes. They would serve as a forum to discuss the evolution of the UK economy. This reform opened the way to the actual operational independence of the BoE, which came in 1997, with the instauration of a Monetary Policy Committee (MPC). The BoE modelling team was put in charge of providing inflation forecasts and scenarios on the consequences of alternative policies. This new monetary framework thus made the Bank’s central forecasting model a cornerstone of the monetary policy process.

²¹Midgley to Dick-Mireaux, “Future forecast effort,” 19/03/2019, 1A6/1, BoE archives. Modellers at the Treasury, on the contrary, did not have this autonomy and were under constant political pressure—notably by Nigel Lawson, who was Financial Secretary (1979-1983) and then Chancellor [1983-1989; R. Smith (2003) and Non-BoE economist 2, interview]

As soon as 1994, the BoE's staff was also tasked with developing a new forecasting model.

The main novelty of this new model was its size: the first version was built around a core of “only” 12 behavioural equations (Staff economist 15, interview) and the second version had “twenty behavioural variables” (Britton et al. 1996, 3). This broke with Klein's modelling tradition and constituted a sharp departure from the 134 variables “described by behavioural equations” in the previous BoE model (in its version published by Harnett et al. 1987, 399). The Bank's new model also ranked as the “smallest” model in use at UK policy institutions, especially compared to the Treasury's or the NIESR's (Whitley 1997, 166). John Whitley, who oversaw the construction of the new model, explained the downsizing by the desire to clarify “the underlying economic analysis so that the numerical conclusions can be understood and used with confidence” (Whitley 1997, 163). This reduction of the model's size went hand in hand with a clarification of the goals of the model: key mechanisms were now focused on price determination, and less attention was devoted to the output level (compared to the previous model). Moreover, the overall goal was to “incorporate the key elements in the transmission of monetary policy to inflation” (*ibid.*).

The new model, called the Medium-Term Macro Model (MTMM), consisted of an estimated form of Aggregate Supply/Aggregate Demand (AS/AD) model (Britton et al. 1996, 4–5). The Phillips curve, which was not formalised as such in MTMM, was negatively sloped in the short run, but vertical in the long run (3). Modellers thus assumed an equilibrium rate that they called the “Non-accelerating inflation rate of unemployment” (NAIRU).²² The staff also communicated on how they had modelled inflation inertia by using nominal and real rigidities, such as a bargaining framework determining earnings (12-13). The adoption of a long-run vertical Phillips curve associated to rigidities and a bargaining framework gave MTMM a “new Keynesian”

²²In the last version of the previous, large-scale model, in use until 1994 (Harnett et al. 1987), there was no such discussion of the Phillips curve or of an equilibrium rate of unemployment. What determined domestic prices was the large disaggregation of the model, with a manufactured sector, a public sector and a third sector with remaining activities. The former model also had separate equations for the price of different commodities.

flavour. Although rational expectations were still absent, two modellers, Paul Fisher and John Whitley, explicitly argued relying on “new Keynesian theory” (78) to formalise the determination of inflation by expectations (measured through surveys) and the “output gap”.

Slimming the model down was not merely driven by theoretical considerations. It also reflected a wider strategy. The new model could be entirely managed by a small team, without relying on a large division of labour across different teams within the Economics Department (then Monetary Analysis Directorate). One member of the MTMM modelling team recalls: “for the first time we took total control of the equations in the forecasting group. We didn’t bother having the wage experts do the wage equation, for example. We just did it all ourselves.” (Staff economist 15, interview)

The period 1993-1994 thus initiated a major shift in the building and management of the central forecasting model at the Bank. Which factors could account for this change? A recurring answer in our interviews with economists working at the Bank in the 1990s has been: “The short answer to this question [“why changing the model?”] is: Mervyn King” (Staff economist 7, interview).

When King arrived at the Bank in 1990, becoming chief economist the next year, he had a well-established academic career.²³ As a taxation economist, he had published in (and edited) several of the most prestigious academic journals in economics. Though not a macroeconomist, he nevertheless held specific views on what kind of macroeconomic models should be developed.²⁴

King’s vision was that a central forecasting model had to provide policymakers with a ‘story’ about the economic mechanisms at play.²⁵ To achieve this, a model had to be simple and based on a clear and consistent theoretical framework—his definition

²³After graduating from Cambridge (BA Economics, 1969), King held several positions as Fellow and Lecturer in Cambridge, before becoming Professor at the LSE (1984). When he joined the Bank, he had just returned from a visit to Harvard and the NBER. King would become Deputy Governor in 1998, and finally Governor from 2003 to 2013.

²⁴King had developed a certain interest in new classical economics (the works by Lucas, Kydland, Prescott, . . .) and rational expectations (Executive 3 and Staff economists 7 and 12, interviews).

²⁵King’s vision at the time was reconstructed based on concurring depiction from interviews and archives; see Acosta et al. (2023, sec. 4) for more details.

of such a framework corresponded to his personal preference for macroeconomics inspired by the U.S. dominant academic standards of the time. As a result, King fiercely opposed large-scale macroeconometric models as used previously at the Bank. First, because they were very large and henceforth not ‘transparent’; second, King saw them as lacking theoretical foundations, because they assembled different theoretical insights (some of which were ‘outdated’, ‘Keynesian’ ones) and because they were driven by econometric or empirical considerations. This vision of the chief economist was very influential in guiding the building of the new macroeconometric model from 1994 onwards.

King’s uncompromising modelling vision, sustained by an unyielding management style, resulted in the resignation of those modellers who disagreed with him.²⁶ For instance, one of the economists in charge of modelling before King recalls that:

There was no justification for this shift at the time, it just happened. The boss (King) says “this is the way to do things”, and for newcomers, this was the way to progress. [...] I left the Bank because of this change of model. [...] I was delighted to do that, considering the new restrictions at the BoE. (Staff economist 7, interview).²⁷

For instance, Brian Henry, who had been instrumental in reorganising the work on the old model after 1984, left the Bank for the International Monetary Fund (IMF). King then asked Fisher and Shamik Dhar to build a new smaller model for forecasting.²⁸ Whitley arrived at the Bank in 1995 and took over the management of

²⁶A BoE executive describes King’s attitude at the time:

Mervyn turns up, with one qualification: he thinks, what is inherited is useless. Useless. He thinks it is so useless, and he is so aggressive about changing things, that there was a discussion with the Governor about whether to get rid of him. . . . I suspect Mervyn was horribly patronizing to them [the staff], because they were not doing rational expectations, they were not doing modern finance theory, they probably didn’t know the search models for the labour markets. (Executive 3, interview)

²⁷Another modeller explains that:

Mervyn did not believe in empirical work very much at all. Although he came from the LSE in the UK he really came from the American DSGE type of tradition of modelling, which doesn’t really require very much data, and so he started to move the whole thing towards that sort of direction, which is when a number of us decided that we didn’t need to be at the Bank anymore. (Staff economist 6, interview)

²⁸After an MSc. at Warwick in 1983, Fisher worked for Kenneth Wallis and the Macroeconomic

the model from Dhar. These new appointments facilitated the change in modelling choices.²⁹

However, King's views alone were not sufficient to convince his staff to develop and operate a new, smaller forecasting model. Several of those staffers, trained in the 1970s and 1980s, actually shared some of King's views—but they were not all inclined to push macroeconomic modelling at the Bank towards the new classical approach. Whitley and Fisher had been members of the Warwick Macroeconomic Modelling Bureau before joining the Bank. In 1983, the Social Sciences Research Council—the future Economic and Social Research Council (ESRC)—had established the Bureau “with the remit to improve accessibility to macroeconomic models, to promote general understanding of the properties of the United Kingdom models, and to allow comparisons between models to be made” (Ball and Holly 1991, 212–13). The Bureau operated out of the University of Warwick, under the direction of Kenneth Wallis. In the following decade, the Bureau published several comparative analyses of UK models, including those of LBS, NIESR, Treasury, the model of the Cambridge Growth Project, the City University Business School model, the Liverpool model and the Bank's model (Wallis 1984). Some of these analyses were notably co-authored by Fisher and Whitley (e.g. Wallis et al. 1986). Like many others at Warwick (and notably Wallis), the two modellers did not oppose structural econometric models, nor did they adhere to new classical macroeconomics. As described above, however, the model they developed at the BoE reflected neither of these standards—MTMM was “just” a AS-AD model, with new Keynesian rigidities but without rational expectations. The modelling choices of the Bank's staff reflected the lack of trust in the large-scale macroeconometric model that grew in the 1990s rather than an active endorsement of the new classical macroeconomics that was becoming dominant in the academic environment.³⁰ Indeed, between 1979 and 1999, the publications

Modelling Bureau (see below) and was granted a PhD in 1990 before joining the BoE. Dhar joined the BoE in 1992 after having worked for the Treasury (1987-1990) and for a private forecasting company, Oxford Economics (1990-1992).

²⁹Whitley worked as a research fellow at the NIESR during the 1970s before joining the university of Warwick and working for the Modelling Bureau after its creation in 1983. He was granted a PhD in 1989.

³⁰Another anecdote is typical of the interplay between policymakers' and modellers' agencies: on a later version of MTMM, to convince King of the modelling of monetary policy, and knowing that

by BoE staff exhibited significantly fewer references to key contributions generally associated with the New Classical approach, by Robert Lucas or Finn Kydland and Edward Prescott for instance (Acosta et al. 2023, Online Appendix, 3.8.1) — After the mid-1990s, the ESRC decided to stop financing the work of the Warwick Bureau (Staff economist 16, interview). The Bank’s modellers reflected this changing intellectual context when they wrote: “We stress that the days of relying on a large macroeconomic model as a “pictorial” representation of the economy which expected to be able to answer almost any economic question thrown at it, are gone” (Britton et al. 1996, 2).

Finally, BoE modellers were also driven by the new function of MTMM: to produce the inflation forecasts and policy scenarios (for any change in the Bank’s rate) that formed the basis of the *Inflation Report*. In this perspective, MTMM offered several advantages in comparison to the large-scale model of the 1970s and 1980s. First, its more focused theoretical structure allowed modellers and policymakers to draw narratives more easily from its use (one of King’s wishes). Second, on the material aspect, its small size made it easier to modify some equations at the margin and reduced the computational burden of running new forecasts. Rational expectations, although theoretically appealing for some modellers and policymakers, like King, remained too heavy a computational burden (Cherrier, Saïdi, and Sergi 2023).

Both King and the staff were aware that a model producing public forecasts would be exposed to more intensive scrutiny. A small model would be particularly vulnerable to criticisms if it was to produce a simple point estimation of inflation. A first line of defence against this was to publicly expose, and thus manage their own “ignorance” (Best 2021): they published their forecasts as a probability distribution, displaying a (non-symmetric) margin of error around the central projection for the next quarters to come.³¹ These “fan charts” were launched in February 1996 and presented as an innovative form of forecasting by King and the BoE’s staff

he considered inflation as “a monetary phenomenon”, they added a supply of money equation in addition to the standard policy reaction function, even if this was not required in the model (Staff economist 11, interview).

³¹Moreover, King was sensitive to the idea that the economic context was characterised by some radical uncertainty (Kay and King 2020).

(King 1997; Britton, Fisher, and Whitley 1998). Secondly, King and the BoE’s staff insisted repeatedly and publicly that the forecasts were not derived from a single model, but from a “suite of models”. Whitley (1997) advocated for this “eclectic approach” for modelling in policymaking institutions, arguing that “structural vector-autoregressions (VARs), theory-based optimising approaches and macroeconomic models are natural allies rather than competitors” (163). However, according to BoE executives and economists, this “suite” was “a complete scam”, a “rhetorical device” (Executive 1, interview) or “an ambition, but never the truth about how the inflation forecast was really done” (Staff economist 12, interview). Even if the “suite” was never used for forecasting at the time, the idea of a suite illuminates both the loss of confidence in forecasting models and the wish to avoid the central model becoming an easy target for the Bank’s critics.

The new policy framework that emerged between 1992 and 1997 was an important direct institutional factor that encouraged the change of model. One of its main effects, by reinforcing the importance of forecasting, was to highlight the material constraints underpinning the operation of the old large-scale model. The development of a smaller model also came from another direct institutional constraint: King, the chief economist, favoured such small models. Nonetheless, the theoretical content of the model was essentially in line with the past experience of the modellers in charge of building MTMM.

4 Keeping up with academic standards and forecasting performance: The BEQM model

Because they formed the basis of monetary policy discussions within the Monetary Policy Committee (MPC), the forecasts produced with the MTMM, and their underlying mechanisms, were much debated. In particular, the external MPC members (i.e. those who were not BoE Executives), constantly challenged forecasts (James 2020, 436).³² Additional criticisms came from outside the Bank and the MPC

³²As a former executive explains, “you had [external members] who were never holding back what they thought. We never knew if they would sign the Bank forecast” (Executive 2, interview). On

(including academic criticisms, like Arestis and Sawyer 2002). In 2001, the House of Lords Select Committee on Economic Affairs recommended an independent audit of the BoE forecasting model.³³ The Bank itself also commissioned Adrian Pagan, an Australian econometrician and former Board member of the Reserve Bank of Australia, to assess “whether [BoE’s economic modelling and forecasting work] is ‘state of the art’” (Terms of references, cited in Pagan 2003, 62).

At the beginning of the 2000s, within this challenging context, the Bank decided to replace MTMM and build a new forecasting model, which was later named BEQM (Harrison et al. 2005). The change, however, was not prompted by institutional factors such as MPC requests or outside political pressure. Rather, it reflected the agency of modellers who were dissatisfied with MTMM (Staff economist 3, interview). In fact, critical outsiders did not really ask for a new model to substitute MTMM and its forecasts. Timing also confirms that the decision to change the model was not primarily driven by external pressures. The Bank staff was already at work on BEQM by September 2000, before Pagan’s review even began. The model started being used in forecasting rounds in 2003. A former BoE executive recalls: “the pressure for changing the model came rather from staff than from the MPC” (Executive 1, interview). The same interviewee adds: “Basically, the MPC was not uninterested [in modelling], but they would not be involved in the process (‘you should do it this way, or that way’).” A former BoE modeller agreed that the model per se was not at the centre of the concerns and demands of MPC members:

Day-to-day, during the process of producing the forecasts, most Committee members very rarely mention the word “model” [...] occasionally, they ask “how does this work in the model”. [...] It makes it less likely, in the way we interact with the [MPC members], to have a discussion like “Hang on, this model is not working for us”. [...] Changing models is very costly and painful for us to go through. If we want to change, the change has to come from the bottom-up, from the staff’s desire to

‘dissent’ within the MPC, see e.g. Harris, Levine, and Spencer (2011).

³³House of Lords, 2001, “Response of the Government to the Report of the House of Lords Select Committee on the Monetary Policy Committee of the Bank of England”, House of Lords Paper 34, Session 2000-01, February 2001.

do it. I think this was the case in this case [BEQM]. (Staff economist 2, interview)

The staff saw MTMM’s theoretical structure as not fully stock-flow consistent, something that the Bank executives also complained about (Staff economist 2, interview).³⁴ The staff was also adamant to address recurring questions from the MPC on which MTMM was silent. A BoE modeller recalls: “For example, there was a debate [in 1996] about anticipated productivity growth, potentially driving the huge appreciation in the exchange rate, and that could also impact expectations on future wealth: what was the theory beyond that? That was something that was not straightforward to deal with in MTMM.” (Staff economist 3, interview) The staff understood that solving such problems rather required building a new model ‘from the ground up’, taking for instance stock-flow consistency as a starting principle and making the necessary consistency arrangements for introducing rational expectations. Moreover, the staff ambioned to introduce substantial theoretical changes, moving the BoE forecasting model closer to the standards of academia and of other central banks—in short, closer to a DSGE model.³⁵ A former BoE executive recalls:

MTMM did not really have microfoundations. Academia was moving on: you had the DSGE approach developing as the standard framework for a lot of academic work. [...] So the staff started pushing in that direction. (Executive 1, interview)

A BoE economist involved with the BEQM project confirmed the overall motivation, although emphasising that policymakers were also open to the development of a new model closer to these academic standards:

The key motivation was to have at least the core of the model to be

³⁴For instance, generated profits were not feeding back into any agent resources.

³⁵Obviously, the benchmark DSGE model did not stabilise until Christiano, Eichenbaum, and Evans (2005) and Smets and Wouters (2003). Even the label ‘DSGE’ was not widely used before the mid-2000s. However, one can already identify, in some mid-1990s contributions (Cooley 1995; Clarida, Galí, and Gertler 1998) some distinctive characteristics of what became the benchmark DSGE model of the mid-2000s. That is: a representation of the economy as the equilibrium outcome of optimising individual behaviour (in short: ‘microfoundations’); exogenous shocks generating the dynamics; sluggish adjustment to shocks, due to real and/or nominal frictions.

better microfounded. We were influenced not only by what other central banks were doing, but also by having a chief economist [*The interviewee is referring here to King, although King was not anymore the chief economist when the BEQM project had started.*] who was very keen on developing this kind of things, and external MPC members who wanted more theoretical rigour in the model. (Staff economist 2, interview)

With respect to this ambition, the impulsion for BEQM, though coming “from the bottom” and modellers’ agency, was not disconnected from the institutional changes implemented “from the top” during the 1990s. As discussed in the previous section, King advocated his vision throughout the 1990s, making it an intellectual compass both for the central forecasting model development and for other decisions within the Bank, including hiring and promotion (Acosta et al. 2023). This had two notable influences on the staff building BEQM.

King laboured to move the BoE modelling culture closer to the new generation of models (DSGE), which was developing in US academia and in a few other central banks (like the Bank of Canada). In 1994, he hired Andrew Scott (who had just completed his PhD in Oxford) as an external consultant to the Bank. Scott was to conduct, in collaboration with BoE staff, the “Small Analytical Model Project” (Holland and Scott 1998, 1). The outcome of the project was a UK-calibrated model mostly consisting of the neoclassical growth model *a la* Kydland and Prescott (1982) and Long and Plosser (1983), with a stochastic trend and fluctuations mainly driven by productivity shocks. Such a model was allegedly part of the suite (Whitley 1997, 166), but it was not intended to produce forecasts (hence, it was not intended to replace MTMM); rather, its role was to “identify the causes of the business cycle and outline the propagation mechanisms” (Holland and Scott 1998, 7). The actual use of this model turned out to be quite limited, as a former BoE economist recalls: “A lot of money was spent on [the small analytical model project], but it didn’t really go anywhere, except it was the first infusion of the US modern macro into the Bank.” (Staff economist 12, interview)

Adherence to DGSE macroeconomics within the staff was further reinforced by a

generational shift. As previously explained, King’s vision and management style resulted in a significant turnover in the early 1990s. The new recruits were much younger and had been trained in a more international environment (see Acosta et al. 2023, sec. 4). As a consequence, they had grown sceptical of the old macroeconomic modelling approach and, in line with King’s vision, exhibited an interest in furthering DSGE-style modelling at the Bank. A former staff member, who entered the Bank in 1992, recalls:

I was probably closer to Mervyn King intellectually. [...] I was slightly disillusioned with the UK macroeconomic modelling tradition at the time [...] I felt that Mervyn’s approach was much cleaner, much more in tune with modern macroeconomics. I think that a lot of UK economists who were working at that time had a strong resistance to that approach. They felt it was too stark, too simplistic, and frankly they also hold on to the techniques and systems they grew up with. [...] I was increasingly impressed by DSGE-type modelling [...] That I felt was what you had to provide to Mervyn. (Staff economist 5, interview)

As a result, a few macroeconomists within the Bank staff began to engage with this line of work. Interaction with academia was encouraged although this led only to a few visible outputs, such as working papers and publications in peer-reviewed journals (e.g. Millard, Scott, and Sensier 1997; Dhar, Pain, and Thomas 2000; Batini, Harrison, and Millard 2003; Nelson and Nikolov 2004).

The development of BEQM aimed to pursue the same theoretical line “infused” by these projects developed in the 1990s. However, in lieu of individual initiatives, BEQM involved the whole modelling unit, consisting of around 20 economists. Of the six modellers who more specifically authored the model and its documentation (Harrison et al. 2005), all had joined the Bank in the mid-1990s, straight after graduating with masters in economics in the UK (Ryland Thomas in 1994; Meghan Quinn in 1996; Richard Harrison in 1998; Kalin Nikolov and Gareth Ramsay in 1999) or abroad (Alasdair Scott in 2001). They all had begun their careers at the Bank in Monetary Analysis (MA), often starting in the Monetary Strategy and Assessment

Division, “[the Bank’s] biggest interface with the academic works ... attracting new staff that was more research-oriented” (Executive 1, interview), before joining the Conjunctural Assessment and Projections Division, in charge of the forecasting model.

The BEQM modelling team was led by Harrison and Alasdair Scott (Staff economists 1 and 12, interview). After an MSc at the LSE, Harrison joined the Bank in 1998 and started working on monetary policy rules in open-economy models (e.g. Batini, Harrison, and Millard 2003). When Harrison boarded the BEQM project in September 2001, he was a relatively new (and young) member of the modelling unit, having spent only 3 years at the Bank. Although also young, Scott arrived at the BoE already carrying significant experience in macroeconomic modelling. He had worked previously (from 1998) at the Reserve Bank of New Zealand, where he participated in the development of a “core model” for forecasting purposes (Hunt, Rose, and Scott 2000). The “core model” consisted of a small calibrated general equilibrium model, based on optimising, forward-looking behaviour of economic agents, and frictions ensuring a sticky adjustment back to steady-state after any exogenous shocks. It embodied the essential features of the DSGE approach, although some formulations were different from what later became the benchmark DSGE model.³⁶

The ambitions of the BEQM modellers encountered two obstacles along the way. First, BEQM was developed ‘too early’, ahead of the emergence of a stabilised, benchmark DSGE model in academia and central banks. As mentioned above, DSGE-like models had been designed throughout the 1990s, but it was not until the 2000s that larger versions were developed within policymaking institutions for policy purposes. In 2002-2003, the IMF and the Fed’s Board of Governors built two similar multi-country DSGE-type models to be used in policy routines, scenario analysis, and forecasting—respectively, the Global Economy Model (GEM, Tchakarov et al. 2004) and the SIGMA (Erceg, Guerrieri, and Gust 2005). Meanwhile, the European Central Bank developed its new model, based on Smets and Wouters’s (2003) work.

³⁶These differences included for instance: a Blanchard-Yaari specification for household behaviour, instead of a “Ricardian/non-Ricardian” household assumption; Rotemberg “shadow” menu costs instead of Calvo-pricing for firms; a Calvo-bargaining mechanism on the labour market instead of assuming monopolistic competition.

These three projects rapidly set the theoretical and empirical benchmark for DSGE models, either in academia or in central banks.

The BEQM project was hence one or two years ahead of these other institutions' projects. As a result, many of BEQM's modelling choices were significantly different from those adopted by GEM, SIGMA, and Smets-Wouters. Although these differences did not seem significant at that time, they have been retrospectively seen as a "missed opportunity". One of the BoE staff involved with the BEQM project argues:

I think there is a lot in terms of the timing. [...] Between the specific time we have chosen to start and the time it took to get to the end, this meant that we missed some of the opportunities that were coming up. [...] we were talking to people like Frank Smets about their effort; and our project was going on alongside what they were doing, while we had already kind of pre-set our path really. I think, if we had done the project three or four years later, things would have looked quite different (Staff economist 3, interview).

Furthermore, the modellers' agency faced the direct institutional constraint related to the preferences of BoE executives: they wanted the new model (BEQM) to resemble the old one (MTMM) in many significant ways: "we were trying to make the forecast process from the perspective of MPC members unchanged. [...] they should not notice the difference, from the structure of the meeting and the things that they discuss", a BEQM modeller remembers (Staff economist 3, interview). As a consequence, the modellers decided to keep the key economic mechanisms driving inflation identical to those in MTMM, so that the baseline narrative about inflation relied on the output gap.

The modellers also understood that several topics that the MPC constantly discussed had to be included in the new model, which expanded the number of sectors and agents: BEQM "ended up being really complex [...] Understandably, because there were a lot of important questions to ask. [...] [For instance, MPC members] wanted a supply-side with an input sector with not just domestic inputs but also imported inputs" (Staff economist 5, interview).

Finally, the BEQM modellers worried that by bringing the new model closer to academic standards, they would lessen its forecasting performance, which remained its main function within the Bank. The then chief economist shared this concern: “I was cautious about [changing the model], because I was sceptical about how useful a DSGE model would be upstairs [in MPC meetings]. They are good for academic exercises, but not so obviously for forecasting,” (Charles Bean, interview). Therefore, he encouraged the staff to proceed with developing the new model “providing it doesn’t materially damage the fit of the model to the data. So we converge to this idea of having the theory determining the long run and the data determining the short-run” (*ibid.*). This statement illustrates the modelling strategy finally retained by the staff: BEQM combined a “core” and a “non-core” subset of equations. The “core” constituted the “organising framework for analysing the economy” (Harrison et al. 2005, 23), while the “non-core” was a collection of single equations describing the evolution of additional variables (not included in the core) or adding “*ad hoc* dynamics” (9). In short, BEQM relied on a form of Vector-Error-Correction mechanism, which had been very popular earlier in the 1990s. Its implementation with a core/non-core structure was quite specific to BEQM. Though conceived to accommodate the various institutional constraints that came with modelling for forecasting purposes within a central bank, the core-non-core empirical strategy nevertheless attracted significant internal and external criticism, leading, a few years later, to different modelling choices.

In a nutshell, while the impulse for launching BEQM came from staff’s agency, the project was nurtured by a new modelling culture at the Bank, itself fueled by King and the recruitment of a new generation of modellers, influenced by a new intellectual context: the early emergence (1990s) and later gradual stabilisation (early 2000s) of the DSGE approach both within academia and other central banks and international organisations. Since this influence had not fully materialised into MTMM, the ambition of the BEQM modelling staff was to devise a new model that would be closer to the new standards of this approach.³⁷ When they began building a new

³⁷In academia and even more in public and international organisation relying on very large models, methods to numerically solve these models and estimate them were crucial. In the 1990s, economists have proposed various relaxation and perturbation algorithms, some that became embodied in

model, however, neither the theoretical characteristics nor the computational and estimation strategies for this new class of DSGE models were stabilised. BEQM ended up being bigger, more complex and less legible empirically than other new models from this era, not just because of such timing, but also because policy-makers' various demands required a 'compromise' between the builders' methodological preferences and users' need for narratives.

5 Discontent with BEQM and the long road to COMPASS and its suite

BEQM operated between 2003 and 2011, when it was formally replaced by a new model, COMPASS, i.e. "Central Organising Model for Projection Analysis and Scenario Simulation" (Burgess et al. 2013, ii).³⁸ From 2003 onwards, the reception of BEQM inside and outside the Bank had pointed out the need for a new model. BEQM had been regularly presented by Harrison and his colleagues at the Central Bank Macromodelling Workshop (2003, 2004), before central banks' economists. In December 2005, shortly after the publication of the official documentation, BEQM was presented at an international conference, "DSGE Modeling and Policymaking Institutions: Progress and Prospects". During this conference, Christopher Sims offered a very harsh discussion of the BEQM core-non-core approach, which he deemed particularly problematic in terms of consistency and transparency (later published in Sims 2008). These criticisms hit hard part of the BoE staff and of the executives. One former executive recalls: "I read Sims's paper and I thought he was right: '[BEQM] is bollocks, what have we done?! It's awful!" (Staff economist 12, interview). Misgivings about how BEQM had been conceived only grew during the 2000s, after the DSGE approach had stabilised both in academia and in other policymaking institutions: BEQM increasingly appeared "out of tune," though part

software such as DYNARE that allowed a faster and more standardised resolution of DSGE models. In the early 2000s, macroeconomists such as Smets, Wouters and others implemented Bayesian estimation methods which were an alternative to calibration. But not only was this coincidental to the development of BEQM: software allowing for a reliable, fast and flexible use of Bayesian estimation were also just being developed (Cherrier, Saïdi, and Sergi 2023).

³⁸COMPASS is still used today (2022).

of the BEQM team continued to defend their approach, especially the core-non-core strategy (e.g. Alvarez-Lois et al. 2008).

Other Bank executives (and policymakers) were less affected by criticisms and by the widening gap with the new standards: “most people at the Bank thought: ‘These guys [who criticize BEQM] are academics, what do they know?!’” (Executive 1, interview) In his review of BEQM, Pagan (2005, 191) likewise explained that, though some of BEQM’s theoretical specifications “may not appeal to outside observers,” modelling in academia and central banks obeyed different rules and standards: “the decisions taken by the builders of BEQM have to be rooted in what is tractable and what is familiar to policymakers, and one needs to recognise these constraints when making criticism”, he pointed. All things considered, then, Pagan thought BEQM was an overall “success” and a “state-of-the-art” model (*ibid.*). But a then staff member paints a different picture of the in-house reception: “BEQM ended up pleasing none of the sides”, he recalls, “It did not satisfy the DSGE-purists, because it ended up being much *ad hoc* and larger than people felt would be useful; and it did not please the macroeconometric traditionalists, because they felt that a traditional macro-model would do exactly the same job.” (Staff economist 5, interview) Even more concerning, the core-non-core empirical strategy was increasingly deemed unsuitable to the forecasting function of BEQM. “Because we had this core-non-core structure, it was more difficult to provide narratives to the MPC. When we tried to decompose forecasts, it was difficult to say what was driving them: Was it the core model? Was it the non-core?”, a staff member explains (Staff economist 2, interview). Materiality also mattered: the calibration of the core model proved time-consuming, mainly because of the number of parameters (*ibid.*).

Discontent against BEQM only grew with the onset of the Great Financial Crisis. In widening the missions and instruments of the Bank, the Great Financial Crisis both highlighted and increased the feeling that BEQM had become irrelevant to fulfilling the Bank’s mission. The first consequence of the crisis was that the Bank rate hit zero a few months after the collapse of Lehman Brothers. In March 2009, the BoE thus added the asset-purchase facility to its monetary policy toolkit and

began to perform “quantitative easing” (QE). The transmission channels whereby QE operated—portfolio adjustments and stabilisation of banks’ balance sheets—were not modelled by BEQM. The model could not help decide whether to pursue QE nor how to implement it. It was also useless to understand which mechanisms created financial instability, at a time the BoE was set to recover the macro- and microprudential oversight mandate that it had lost just as it was granted independence in 1997.³⁹ BEQM’s helplessness was later encapsulated by a modeller:

Especially during the financial crisis, when all the mechanisms we were trying to understand were not in the core [model], the marginal economic benefit of the core was less and less apparent, and the model was not adding much to the thinking about the economic mechanisms (Staff economist 4, interview).

Plans to replace BEQM were thus rolled out beginning in the Fall of 2009. The staff decided to start with building a smaller model, nicknamed “baby BEQM” (Staff economists 2 and 3, interview). It was completed in a few months by Harrison and Oomen (2010). While this initial downsizing might be reminiscent of the dynamics that occurred in the 1990s with MTMM (*cf.* Section__3), the underlying rationale was significantly different. First, this time the key economic mechanisms (in particular of what drove inflation) were preserved. Second, while the 1990s downsizing was largely imposed by King, the 2010s one was largely driven by modellers’ agency.

After the development of “baby BEQM”, a larger team worked full time to develop COMPASS, following the same lines; the final version was completed in about 18 months. Except for Harrison, the team leader, the main contributors were ‘newcomers’, i.e. economists who had not been previously involved with building or operating BEQM (Burgess et al. 2013, i).⁴⁰ Unlike the BEQM team, the new

³⁹Its financial stability mandate and associated supervision duties were transferred to the Financial Services Authority, which began its operation in 2001. See Acosta et al. (2023). The 2012 Bank Act transferred it back to the BoE and led to the creation of a Financial Stability Committee.

⁴⁰This resulted from the arrangement for new resources for building a new model: the team developing COMPASS was built by taking in new staff, while the existing modelling unit was in charge of operating the forecasting routine. Of course, the whole modelling team was still associated with the discussions about the development of COMPASS.

modellers had almost all completed a PhD, and several had been trained abroad.⁴¹ This standardised international training partly explains the theoretical and empirical structure of COMPASS.

COMPASS was estimated using Bayesian techniques, formally very different from the BEQM ‘core-non-core’ strategy. Its theoretical underpinnings were much closer than BEQM’s to what had become, by the late 2000s, the standard for DSGE models in academia and central banks. However, the key economic mechanisms embodied in COMPASS remained the same as in the BEQM core model (output-gap-driven inflation, arising from real and nominal adjustment frictions to exogenous shocks). Like BEQM, COMPASS does not include any sophisticated representation of the functioning of financial markets, the banking system, households’ and firms’ portfolio choices, etc.

Theoretical modelling choices made for COMPASS may thus seem paradoxical. The model grew out of BoE executives’, academic macroeconomists’ and the staff’s discontent with BEQM, as well as from its inability to help the BoE understand and properly respond to the Great Financial Crisis. And yet, COMPASS did not embody a financial sector or the monetary policy transmission mechanisms at work in the implementation of Quantitative Easing. But here laid the major break from BEQM: COMPASS was not designed as a standalone model. As the official model documentation clearly highlighted, COMPASS was conceived as embedded in a whole *suite* of models. As previously explained, the “suite of models” had previously been advertised by King’s modellers, but some considered that it had largely remained a rhetorical deceit. Conversely, the suite presented by (Burgess et al. 2013, 39) consisted of at least 50 different models, with models being “added or removed” depending on the context (ii). Some of them provided forecasts for economic variables that are not included in COMPASS, and other “alternative forecasts” for included variables.

The first class of models provided insights into key economic mechanisms left out

⁴¹Charlotta Groth held a PhD from Stockholm (2002), Francesca Monti from Université Libre de Bruxelles (2011); Emilio Fernandez-Corugedo was trained at University of Bristol (PhD 1999) and Konstantinos Theodoridis at Cardiff (PhD 2006). Matt Waldron and Stephen Burgess had no PhD training.

from COMPASS, for instance, financial frictions and unconventional monetary policies (Burgess et al. 2013, 40). The second class of models in the suite provided forecasts for financial variables that were not included in COMPASS.⁴² The main model from this class, nicknamed the Balance-Sheet Model (BSM), is a large (140-variables) statistical model. It is based on modelling developed in the early 2000s within the Financial Stability Directorate to produce the Financial Stability Review (Benito, Whitley, and Young 2001). Finally, the third class of suite models consists of alternative specifications of inflation and other variables to forecast. These alternative specifications are either driven by alternative theories (e.g. “Keynesian consumption functions,” “financial accelerator,” “Tobin-Q”) or by purely statistical (VAR-type) considerations. These alternative forecasts are combined through model averaging techniques, which were already developed in the early 2000s under the leadership of Simon Price (Kapetanios, Labhard, and Price 2006, 2008).

The 2011 published presentation of COMPASS exemplified how the financial crisis spurred another shift in the modelling culture of the BoE. The COMPASS team emphasised that “no canonical model in the academic literature [...] articulates all of the effects of the financial sector on the wider economy [...] this remains an active area of academic research” (Burgess et al. 2013, 40). Different frameworks were needed to address these particular questions. The suite would include, for instance, extended versions of the core COMPASS DSGE model (introducing “credit spread,” i.e. a wedge between the policy interest rate and the cost of borrowing for economic agents), as well as VAR statistical models (e.g. Barnett and Thomas 2014). The BoE modellers’ stance is consistent with the larger post-crisis intellectual shifts in central banking macroeconomic modelling, which also constitutes a rhetorical strategy to protect the macroeconomic models against criticisms. As explained by Smets (2021, 3), the author of the benchmark DSGE model and a former director general economics at the European Central Bank, central banking economists have long oscillated between a “core model” and a “suite of models” approach. The former allows policymakers to become more familiar with the model and to achieve a higher

⁴²As well as, again, other types of variables, including labour market conditions, fiscal variables, etc. These are notably studied using the “Post-Transformation Model” (PTM), a large (150-equations) backwards-looking model, following a similar error-correction mechanism than BEQM.

degree of consistency but creates a risk of “seeing everything through one lens.” The latter helps tailor models to specific objectives and “improves the degree of policy robustness” but is more discretionary. After a period of dominance of core models, the pendulum may have swung back to suites.⁴³

The Great Financial Crisis and the resulting institutional transformations in the Bank’s functions and organisation may have played a role of catalyst. However, the intellectual context of modelling in academia and central banks constituted a significant indirect institutional constraint on modellers’ choices. In daily modelling practices within the Bank, dissatisfaction and material constraints regarding BEQM gave modellers incentive to promote the building of a new model.

6 Concluding remarks

The history of modelling at the BoE, as recounted in this paper, makes a case for considering the multiple forces at play in model building (and model change) in policymaking institutions. Our case study documented how there was no unique or recurrent pattern in the factors that resulted in the decision to change (or not) a model at the Bank. Each time, model replacement resulted from a different combination of factors: sometimes modellers’ agency drove most of the modelling choices (as in the case of the monetarist episode); sometimes institutional factors play a decisive role: policymakers, academics and other central banks shaped (directly for the former, indirectly for the two others) modelling choices (as in the MTMM and in the BEQM/COMPASS episodes); sometimes the materiality of models, tied to the resources needed to develop and run these models, is a particularly binding constraint (as in the monetarist episode also).

Moreover, our case study documented how modelling within policymaking institutions

⁴³See also Blanchard (2018) and Rodrik (2015)’s rules. The rise of suites of models in economic organisations required the development of a whole new information technology infrastructure. The BoE was no exception. The operation of COMPASS was underpinned by the launch of a new user interface called EASE (Economic Analysis and Simulation Environment), of a software that allowed for BoE staff to solve and estimate the various models under MATLAB, one called MAPS (Burgess et al. 2013). It paralleled the launch of new DYNARE versions (Cherrier, Saïdi, and Sergi 2023) and of YADA at the European Central Bank.

involves more dimensions than “just” discussions about economic theory or empirical performance. In particular, none of our sources suggest that poor performance in terms of forecasting was ever the impetus for a change of model. However, material factors, raising the issues of the model size and its tractability (Cherrier 2022) often took a considerable place in the discussions between BoE modellers and policymakers; likewise, empirical strategies and complementarity across models (e.g. central models vs. satellite models) are important drivers in determining modelling choices. For instance, in the BEQM/COMPASS episodes, we have highlighted how discussions and dissatisfaction with the empirical strategy (BEQM’s “core-non-core” approach) took at least equal, if not greater, importance than its adequacy with theoretical DSGE standards practised in academia and other central banks.

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