

# To change or not to change

The evolution of forecasting models at the Bank of England

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## 1 Introduction

Why do policymakers and economists within a policymaking institution decide to throw away a model and to adopt or to develop an alternative model? Why do they decide to stick with an existing one? This recurring question surfaced once again after the Great Financial Crisis (GFC), which spurred discussion on maintaining, altering (reforming) or replacing Dynamic Stochastic General Equilibrium (DSGE) models that had spread throughout policymaking institutions in the previous decade.<sup>1</sup> Explanations about why models change (or do not change) have hitherto focused on the evolution of academic debates (e.g., De Vroey 2016; Wren-Lewis 2018; Inghrao and Sardoni 2019) or on the political and institutional transfor-

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<sup>1</sup>See e.g. Stiglitz (2011), Romer (2016), or Vines and Wills (2018), for post-crisis debates on the relevance of DSGE. Despite these criticisms and despite the advocacy for new approaches (e.g. Haldane 2012; Mehrling 2010), 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). Scholars have thus analysed this persistence of the DSGE approach in academia (e.g. Helgadóttir and Ban 2021) and in policymaking institutions (e.g. Mudge and Vauchez 2018).

mations brought by crises and their political and institutional consequences.<sup>2</sup> It is only recently that historians of macroeconomics have begun documenting how these transformations drive actual changes in forecasting models used by policymaking institutions (Acosta and Rubin 2019; Cherrier and Backhouse 2019; Acosta and Cherrier 2021). These works explain as well how macroeconomic models gained a preeminent place within policymaking processes in the 1960s and the 1970s in the US.

However, the focus on either academic or political/institutional transformations does not exhaust the possible explanations for the changes (or the absence of change) in policymaking institutions models. For instance, it does not properly explain why large scale Keynesian macroeconomic models persisted in most central banks long after the 1970s crisis, the resulting change in policy regime, and the ‘academic dismissal’ of such models by new classical economists (e.g. Boumans and Duarte 2019); similarly, it does not explain why DSGE models encountered a diverging fate within central banks in the past decade.

In early contributions to the broader literature on models in economics, the emphasis has been put on the functions of models: for instance, their role as ‘mediators’ between theoretical and empirical endeavours (Morgan and Morrison 1999), or their role as ‘bridges’ between economists and policymakers (Butter and Morgan 2003). The resulting contribution of these works has been that macroeconomics, as a field of knowledge, has been shaped by the agency of models (or their ‘epistemic power,’ Morgan 2012). The agency of *modellers* (especially those working in policymaking institutions) is thus very much hidden behind the agency of models. Thus, to fully understand why policy institutions change (or not) their models, we need to adopt a different focus, moving from the models per se to the underlying choices made by the modellers in charge of developing, adapting, and using them.

Modellers from policy institutions face a large set of choices: first, choosing a the-

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<sup>2</sup>Contributions to international political economy and sociology of expertise have focused on the consequences of the political and institutional transformations on change (and absence of change) in “policy devices” (Hirschman and Berman 2014; Hirschman 2015; Best 2020) rather than just models. Changes in models are explained by incentives operating within a policymaking institution and the institution’s mission and mandate (e.g. Helgadóttir 2021). In this literature, models are “embedded” within the institutions that use them (Mudge and Vauchez 2018).

oretical framework, i.e. some key economic mechanisms; second, determining how detailed the economy in the model should be, i.e. how precisely it should reflect the complexity of the real economy (number of sectors, heterogeneity of economic agents, *etc.*), which determines the model’s size (number of variables and behavioural equations); third, relying on a single model *vs.* building a suite of models; fourth, selecting an empirical strategy, which includes, among others, choosing the appropriate dataset(s), opting between calibration and econometric estimation (and which type), setting a procedure for determining the values of exogenous variables or calibrated parameters.

In this article, we document the way modellers navigate these choices, based on: (1) the modellers’ own dispositions, which depend on their training, their academic standards, and their integration in national or international professional networks;<sup>3</sup> (2) the visions of policymakers and executives at different levels of the institution’s hierarchy; and (3) the model’s function, which is shaped by the institution’s organisation and mandates—any model has to be (at least partly) successful in the accomplishment of this function.<sup>4</sup>

As a case study, this paper documents how modellers at the Bank of England (hereafter BoE or the Bank) have developed, reformed and replaced their core macroeconomic model since the early 1970s. We focus on four important moments in the five-decade-long history of modelling at the Bank.<sup>5</sup> What we show is that, each time, a change (or no-change) in models was a response to different types of pressure. In 1979 Margaret Thatcher won the general election and formed a gov-

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<sup>3</sup>By focusing on the modellers’ choices rather than on the models *per se*, this article looks also at modellers with a sociology of professions perspective (Abbott 1988; Fourcade 2009; Ban, Seabrooke, and Freitas 2016; Coman 2019). See also R. Smith (2003) on the “market” for macroeconomic modelling in the UK.

<sup>4</sup>If the institution’s mandate is, for instance, to support economic planning, the model function is to provide a quantitative sectoral assessment of the economic situation and alternative scenarios for planning decisions (see Halsmayer 2017 for such an example).

<sup>5</sup>Current 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. Downward and Mearman (2008) rely on those successive models to theorize about the role of methodological triangulation in monetary decision-making at the BoE.

ernment 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 1). In 1992, the UK exited from 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 2, we explain how this new mandate led BoE economists to develop a new model, but this change did not reflect the major changes in macroeconomic ideas that had occurred since the 1970s. The next model change, in 2003, was largely driven by the growing gap between BoE and academic modelling practices, the latter embodying the rise of a new generation of DSGE models (Section 3). The final model replacement that occurred in 2011 resulted from a growing dissatisfaction with the theoretical and empirical strategy of the previous model. BoE modellers settled on a standard DSGE model. This change hence seems unrelated both with the challenges to the DSGE academic consensus and with the new policy context (like the implementation of Quantitative Easing during the GFC). The choice actually reflected a shift in the epistemological status of macroeconomic models used for forecasting. The new model indeed came embedded in a large suite of diverse models aimed at improving inflation forecasts (Section 4).

## 2 Resisting Thatcher and Monetarism

The Bank started using an in-house macroeconometric model for forecasting in 1972, after acquiring a model from the London Business School (LBS).<sup>6</sup> The model was handled by the economic staff within the Economics Division.<sup>7</sup> The staff saw the model forecasts as serving two purposes: “First, they enable the Bank to influence

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<sup>6</sup>This occurred in an era where macroeconometric modelling was still not seen as the “norm” in policymaking (Ball and Holly 1991, 199–201).

<sup>7</sup>The 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,” January 1974, EID8/7, BoE Archives.) For a discussion of these internal changes, see Acosta et al. (2021).

[H.M. Treasury’s] forecasts; secondly, they are a source of information within the Bank.”<sup>8</sup> The first purpose was related to the UK monetary framework in the 1970s (and until independence in 1997): the government (through the Treasury) had the final say on monetary policy—while the Bank simply suggested policy changes ahead of decisions, and then operationalized 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 divulged (Staff economist 14, interview). The acquisition of a macroeconomic model constituted a way for the Bank to gain some autonomy *vis-à-vis* the Treasury.

The first description of the Bank’s forecasting model was published in Latter (1979). The model consisted of several hundreds of variables and equations, disaggregating the functioning of the UK economy in dozens of distinct sectors (including, for instance, a “Beer” sector, distinct from the “Wine & Spirits” sector). The level of disaggregation was continuously adjusted and furthered, following the approach to macroeconomic modelling initiated by Lawrence Klein (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.<sup>9</sup>

Theoretically, the model 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.”

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

<sup>9</sup>Hoover (2012) describes this approach as the “aggregation program”:

[W]e start with the available data and a feasible macroeconomic model consisting of “a small number of equations” and constantly work to disaggregate it and to elaborate it in the direction of a complete Walrasian general-equilibrium model. This is the strategy behind the program that developed through various simpler models . . . and ultimately into the famous Brookings macroeconomic model of the US economy with its more than 150 equations (Duesenberry et al. 1965). (41)

The mechanisms of the Bank model were maybe “at some distance” from standard “Keynesianism”; 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, exchange rate, and indirect taxes) rather than by monetary forces.<sup>10</sup> 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).<sup>11</sup> The model thus was 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, and thus the level of unemployment, had an effect only on average earnings (6). Hence, the model did not adopt the “expectation-augmented Phillips curve” that had spread during the 1970s (Goutsmedt and Rubin 2018).<sup>12</sup> Finally, the paper did not even mention the Lucas critique (Lucas 1976), rational expectations or even microfoundations.<sup>13</sup>

These three features should not come as a surprise. They were consistent with the modelling choices made by many other forecasting 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 (Barrell,

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<sup>10</sup>The 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).

<sup>11</sup>This 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).

<sup>12</sup>Prices 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 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).

<sup>13</sup>For a presentation of new classical contributions in the 1970s, see Hoover (1988) and De Vroey (2016); see Goutsmedt (2019) and Goutsmedt et al. (2019) for more in-depth analysis of the context of the criticisms addressed to macroeconomic models.

Blake, and Young 2018). However, the persistence of stagflation in developed countries stimulated criticisms of “Keynesianism” (and of the tools that appeared associated with it) in political debates. In the UK, monetarism, relayed 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 Milton 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 facing politicians with a strong stance against inflation and willing to push for a new doctrine regarding monetary policy (James 2020, 71).

In March 1980, the government launched the Medium Term Financial Strategy (MTFS). Regarding monetary policy, MTFS specified money supply targets, relying on the idea that such “control of the money supply will over a period of years reduce the rate of inflation” (James 2020, 70).<sup>14</sup> With this strategy of targetting monetary aggregates, the government was explicitly relying on key monetarist assumptions (see Best 2020, 603–4).

Contrary to the government, BoE’s officials were reluctant to endorse a strict targetting of £M3. 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). For many officials at the Bank, the relation between the demand for money, income, and interest rates was unstable (James 2020, 76). The Bank Deputy Governor Kit MacMahon invoked “Goodhart’s law” to promote the necessity for the Bank to target a larger set of variables, and not just £M3 (*ibid.*).<sup>15</sup> The monetary economist Charles Goodhart himself, who worked at the Bank at the time as a “special advisor” on monetary policy and had tried to quantify the demand for money, argued in favour of different targets since there was too much doubt about the stability of the relationship between targets

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<sup>14</sup>The target was £M3, an aggregate that had grown by 16% a year in 1977-1979. The government wanted to shrink its expansion to 7% to 11% a year.

<sup>15</sup>“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)

and inflation (*ibid.*).

Even before Thatcher’s election and the launch of MTFS, monetarism and the targeting 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.”<sup>16</sup> Regarding the Bank’s model, the economic situation and the resulting policy debates pushed the modellers, for instance, to add a monetary sector to the Bank’s model. However, Thatcher’s government’s intransigence later forced the Bank into a defensive stance.<sup>17</sup>

These policy battles explain why Bank’s executives did not press modellers at the Economics Division to transform their model fundamentally.<sup>18</sup> 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.<sup>19</sup> 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 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 un-

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<sup>16</sup>“Panel of Academic Consultants, First meeting 5th October 1977: Monetary targets,” 31 October 1977, Box XXX, 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).

<sup>17</sup>Christopher Dow, the chief economist of the Bank at the time, explained in his memoirs that “The Bank has had the experience, strange to any central bank, of having a government more monetarist than itself, and has felt uncomfortable about it, and fearful of the future” (Dow in Taylor and Hache 2013, 139).

<sup>18</sup>The subsequent failures of monetary targeting due to financial market deregulation and the release of exchange controls followed 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.

<sup>19</sup>The persistence of large-scale structural macroeconomic models in the UK, coupled with the executives’ resistance to monetarist policies could explain this choice.



der 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.”<sup>20</sup> 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 LSE’s James Davidson and David Hendry.<sup>21</sup> 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 reflect a more Keynesian approach.”<sup>22</sup> 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 addition to policymakers’ pressures and modellers’ dispositions, a third element could explain the absence of a model change: the function and place of the model in the general organisation of the Bank. At the time, economists did not have the role and importance they would get in the 1990s (James 2020, 28–29; Acosta et al. 2021). Even if the model had been developed to strengthen the Bank’s expertise against the Treasury, policy implementation was not discussed within the Economics Division. Ironically, their marginal role gave modellers relative autonomy, since their work was not engaging the Bank. However, this also placed strong material constraints on modellers, since they had limited access to manpower, computational capacities,

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<sup>20</sup>Goodhart, “Small Monetary Model - Collaboration with Davidson,” 5/10/1981, 6A160/3, BoE archives.

<sup>21</sup>Goodhart, “Davidson and the small monetary model,” 5/11/1981, 6A160/3, BoE archives.

<sup>22</sup>Brierley, “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).

and working time. Consequently, it was likely that most executives did not care much about the different specifications of the model and there were fewer chances that the pressure imposed by Thatcher’s government and monetarism could be reflected on the modelling choices.<sup>23</sup>

### **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 central forecasting model a cornerstone of the monetary policy process. As soon as 1994, the BoE staff was also tasked to develop 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 former BoE model (in

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<sup>23</sup>Conversely, Treasury’s modellers did not have this autonomy as they were constantly under political pressure, notably by Nigel Lawson, who was Financial Secretary (1979-1983) then Chancellor [1983-1989; R. Smith (2003) and Non-BoE economist 2, interview].

its version published by Harnett et al. 1987, 399). The new Bank's model ranked also as the "smallest" model in use at UK policy institutions, especially compared to the Treasury or the NIESR (Whitley 1997, 166). The new model's building was overseen by John Whitley, who 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 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 model (Britton et al. 1996, 4-5).<sup>24</sup> 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).<sup>25</sup> 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). Together, the long-run vertical Phillips curve, the rigidities, and the bargaining framework gave MTMM a "New Keynesian" flavour. Two of the model builders, Paul Fisher and Whitley, also presented a reduced-form model that was used to "cross-check" the forecasts of MTMM (Fisher, Whitley, and Mahadeva 1997, 1). Although rational expectations were still absent, Fisher and 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

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<sup>24</sup>For a history of AS-AD models, see Dutt (2002).

<sup>25</sup>In 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.

also reflected an institutional 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 is: Mervyn King” (staff economist 7, interview).

When he arrived at the Bank in 1990, becoming chief economist the next year, King had a well-established academic career.<sup>26</sup> As a taxation economist, he had published in (and edited) several of the most prestigious academic journals in economics. His theoretical and econometric work covered income and capital taxation, and their effects on the wealth distribution, family welfare, housing, and financial markets. Though not a macroeconomist, he nevertheless held specific views on what kind of macroeconomic models should be developed (see Acosta et al. 2021, sec. 4).<sup>27</sup>

King’s vision was that a central forecasting model had to provide policymakers with a “story” about the economic mechanisms at play.<sup>28</sup> To achieve this, a model had to be simple and based on a clear and consistent theoretical framework—his definition of such a framework corresponding to his personal preference for macroeconomics inspired from the U.S. dominant academic standards of the time. As a result, King fiercely opposed large-scale macroeconomic models as used previously at the

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<sup>26</sup>After 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.

<sup>27</sup>King 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, interview).

<sup>28</sup>King’s vision at the time was reconstructed based on concurring depiction from interviews and archives; see Acosta et al. (2021, sec. 4) for more details.

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 macroeconomic model from 1994.

King’s uncompromising modelling vision, sustained by an unyielding management style, resulted in the resignation of those modellers who disagreed with him.<sup>29</sup> 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).<sup>30</sup>

For instance, Brian Henry, who had been instrumental in reorganising the work on the old model after 1984, decided to leave the Bank for the International Monetary Fund (IMF). King then asked Fisher and Shamik Dhar to build a new smaller model for forecasting.<sup>31</sup> Whitley, Fisher’s former boss at Warwick modelling Bureau,

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<sup>29</sup>A 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)

<sup>30</sup>Another 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)

<sup>31</sup>After an MSc. at Warwick in 1983, Fisher worked for Kenneth Wallis and the Modelling Bureau and was granted a PhD in 1990 before joining the BoE. Dhar joined the BoE in 1992

arrived at the Bank in 1995 and took over the management of the model from Dhar. These new appointments facilitated a change in modelling choices.

It would however be simplistic to assume that King alone *convinced* 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 view—although they were not all inclined to push macroeconomic modelling at the Bank towards a form of new classical approach. The 1980s represented indeed a dynamic period for research in macroeconometric modelling in the UK. In 1983, the Social Sciences Research Council—the future Economic and Social Research Council (ESRC)—had established the Macroeconomic Modelling 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 from 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 new classical macroeconomics. 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 intellectual context when they wrote: “We stress that the days of relying on a large macroeconometric model as a”pictorial” representation of the economy

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after having worked for the Treasury (1987-1990) and for a private forecasting company, Oxford Economics (1990-1992).

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 driven by the new function of MTMM: that is, to produce inflation forecasts and policy scenarios (for any change in the Bank’s rate), which 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, its small size made it more flexible whether for changing some equations at the margin or running new forecasts. At the same time, although theoretically appealing for some modellers and policymakers (like King), rational expectations appeared as too heavy a computational burden.

Both King and the staff were however 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 defense against this was to publish the forecasts as a probability distribution, displaying a (non-symmetric) margin of error around the central projection for the next quarters to come.<sup>32</sup> This was launched in February 1996 and the “fan charts” were presented as an innovative form of forecasting by King and the staff (King 1997; Britton, Fisher, and Whitley 1998). Secondly, King and the staff insisted repeatedly and publicly that the forecasts were not derived from a single model, but from a “suite of models.” Whitley (1997) advocated 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

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<sup>32</sup>Moreover, King was sensitive to the idea that the economic context was characterized by some radical uncertainty (Kay and King 2020).

in forecasting models and the wish to avoid the central model becoming an easy target for the Bank’s critics.

## 4 Keeping up with academic standards and forecast 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).<sup>33</sup> Additional criticisms came from outside the Bank and the MPC (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.<sup>34</sup> The Bank itself also commissioned Adrian Pagan 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 to build a new forecasting model, which was later named BEQM (Harrison et al. 2005). Those criticising MTMM and its forecasts did not really ask for a “change the model. 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.<sup>35</sup> 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

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<sup>33</sup>As 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 ‘dissent’ within the MPC, see e.g. Harris, Levine, and Spencer (2011).

<sup>34</sup>House 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.

<sup>35</sup>The development of BEQM was well underway in 2001. A first presentation of the model, open to BoE staff, took place in September 2001 (Staff economist 9, interview), after one year of work (Staff economist 3, interview).



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

While the BEQM project was spurred by the Bank modellers, modelling choices also reflected an attempt to deal with executives’ dissatisfaction with MTMM (Staff economist 3, interview). For instance, 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).<sup>36</sup> 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 actually 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

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<sup>36</sup>For instance, generated profits were not feeding back into any agent resources.

banks—in short, closer to a ‘DSGE’ model.<sup>37</sup> 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 emphasizing that Executives and policymakers were actually also willing to develop the new model towards these academic standards:

“The key motivation was to have at least the core of the model to be better microfounded. We were influenced not only by what other central banks were doing, but also by having a chief economist<sup>38</sup> 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,” was indeed not disconnected from the 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. 2021). This had notably two influences on the staff building BEQM.

King laboured to move the BoE modelling culture closer to the new generation of ‘DSGE’ models, which was developing in US academia and in a few other central

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<sup>37</sup>Obviously, the ‘benchmark’ DSGE model did not stabilize 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 optimizing individual behaviour (in short: ‘microfoundations’); exogenous shocks generating the dynamics; sluggish adjustment to shocks, due to real and/or nominal frictions.

<sup>38</sup>The interviewee is referring here to King, although King was not anymore the chief economist when the BEQM project had started.

banks (like the Bank of Canada, cf. *infra*). 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 recruits were much younger, academically trained in a more international environment (see Acosta et al. 2021, sec. 4). As a consequence, they had grown sceptical of the old macroeconomic modelling approach and exhibited an interest in furthering US-style modelling researching more in line with King’s vision. 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 and that kind of shock/propagation

mechanism, the fact that once linearized they can be written down as VAR. 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 (see notably Millard, Scott, and Sensier 1997; Dhar and Millard 2000; 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.<sup>39</sup> Forging a new model became their priority. 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 from 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 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 then a relatively new (and young) member of the modelling

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<sup>39</sup>Moreover, as customary since the 1970s, the modelling team sought advice from the “technical advisor group” (or “technical review committee”), consisting of “other modellers in the Bank, Heads of Division” (Staff economist 3, interview), as well as external advice from the UK modelling community, from other central banks, and from academics.

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 (RBNZ), 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 optimizing, 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.<sup>40</sup>

The ambitions of the BEQM modellers encountered two obstacles along the way. First, BEQM was developed ‘too early,’ ahead of the emergence of a stabilized, 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. 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 operating one or two years ahead of these other institutions’ projects. As a result, most of BEQM’s modelling choices were significantly different from those adopted by GEM, SIGMA, and Smets-Wouters.<sup>41</sup> Although these differences did not seem significant at that time, they have been retrospec-

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<sup>40</sup>Moreover, to build this new model, the RBNZ had hired three modellers (Benjamin Hunt, David Rose, and Douglas Laxton) who had previously been involved with developing a similar model for the Bank of Canada (see Coletti et al. 1996).

<sup>41</sup>These 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.

tively 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, BoE executives wanted the new model (BEQM) to resemble the old one (MTMM) in many significant ways, which also constrained modellers’ choices: “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: they combined a “core” and a “non-core” model. The former constituted the “organising framework for analysing the economy” (Harrison et al. 2005, 23), while the latter was a collection of single equations describing the evolution of additional variables (not included in the core model) or designed to add “*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 constraints that came with modelling for forecasting purposes within a central bank, the core-non-core empirical strategy attracted nevertheless significant internal and external criticism, leading, a few years later, to different modelling choices (see *infra*).

In a nutshell, if the impulse for launching BEQM came from the staff, the project was nurtured by a new modelling culture at the Bank, one itself fueled by King and the recruitment of a new generation of modellers. These were influenced by the early (1990s) emergence and later (early 2000s) gradual stabilization of the DSGE approach both within academia and other central banks and international institutions. Since this influence had not fully materialized 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. However, various demands from policy-makers, as well as the timing of the BEQM project resulted in a sort of “compromise model” that was bigger, more complex and less legible empirically than initially anticipated.

## 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).<sup>42</sup> Actually, from 2003, the reception of BEQM inside and outside the Bank had already 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 stabilized both in academia and in other policymaking institutions: BEQM increasingly appeared “out of tune,” though part 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

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<sup>42</sup>COMPASS is still used today (2021).



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). The calibration of the core model proved costly to perform, mainly because of the number of parameters (*ibid.*).

Hitting after years of mounting discontent, the Great Financial Crisis acted as a catalyst for a change of model. In widening the missions and instruments of the BoE, the GFC both highlighted and increased the feeling that BEQM had become irrelevant to fulfilling the Bank’s mission. The first consequence of the crisis was 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. The transmission channels whereby QE operated—portfolio adjustments and stabilization 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.<sup>43</sup> BEQM’s helplessness was later encapsulated by a modeller: “Especially during the financial crisis, when all the mechanisms we were trying to

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<sup>43</sup>Its financial stability mandate and associated supervision duties were transferred to the Financial Services Authority, which began its operation in 2001. See Acosta et al. (2021). The 2012 Bank Act transferred it back to the BoE and led to the creation of a Financial Stability Committee.

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 2), 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 the modellers’ own vision.

After the development of ‘baby BEQM,’ a larger team exclusively worked to develop COMPASS, following the same lines; the final version was completed in about 18 months.<sup>44</sup> 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).<sup>45</sup> Unlike the BEQM team, the new modellers had almost all completed a PhD, and several had been trained overseas.<sup>46</sup> This standardized 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 beginning of the 2010s, the standard for DSGE models in academia and central banks. However, the key economic mech-

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<sup>44</sup>This contrasted with the development of BEQM, where the same team was working simultaneously on building the new model and keeping up with the routine of the forecasting rounds using the old model (Staff economist 3, interview).

<sup>45</sup>This 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.

<sup>46</sup>Charlotta 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.

anisms 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 the financial markets, of the banking system, of 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, in fact, 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" has long been advertised by King's modellers, but some considered that this suite 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, other "alternative forecasts" for included variables.

The first class of models provided insights into key economic mechanisms left out from COMPASS, for instance, financial frictions and unconventional monetary policies.<sup>47</sup> The second 'class' of models in the suite provided forecasts for financial variables that were not included in COMPASS.<sup>48</sup> The main model from this class, nicknamed "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

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<sup>47</sup>Additional transmission channels modelled within the suite are the role of energy prices in driving inflation, the role of fiscal policy in determining output, the importance of labour hoarding in creating persistent unemployment, and tax-wedges between output price and consumption prices (Burgess et al. 2013, 40).

<sup>48</sup>As 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.

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 pure 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 had spurred another shift in the modelling culture of the BoE. For instance, the COMPASS team emphasized 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 epistemological 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 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.<sup>49</sup>

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<sup>49</sup>See also Blanchard (2018) and Rodrik’s (2015) rules.

## 6 Concluding remarks

The history of modelling at the BoE, as recounted in this paper, makes a case about the multiple forces at play for model building (and model change) in policymaking institutions.

Our case study documented how there was no single or recurrent pattern for the factors resulting in the decision of changing (or not changing) a model. Each time, model replacement resulted from a different combination of pressures: sometimes modellers' own dispositions drove most of the modelling choices (as in the case of the monetarist episode); sometimes policymakers and executives imposed intellectual constraints (indirectly or directly) on modelling choices (as in the MTMM episode); sometimes, peers pressure (in academia and other central banks) played a crucial role (as in the BEQM/COMPASS episode). Henceforth, the history of macroeconomic modelling in policymaking institutions should not focus either on the evolution of academic debates or on the political and institutional transformations brought by crises and their political and institutional consequences. Understanding why policymaking institutions change (or not) their models, requires a historical, contextual perspective taking into due account three factors (modellers' own dispositions, policymakers' visions, and the model function within the institutions) and their interplays.

Moreover, our case study documented how modelling within policymaking institutions involves more dimensions than "just" discussions about theory. The issue of the model size (i.e. the complexity of the representation of the economy) often took a considerable place in the discussions between modellers, policymakers and BoE Executives; 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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