© MAR 2018 | IRE Journals | Volume 1 Issue 9 | ISSN: 2456-8880
DOI: https://doi.org/10.64388/IREV1I9-1722628

Predictive Forecasting for Operational Resource Allocation: A Comparative Review

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Abstract- ***Operational resource allocation (deciding how much staff, inventory, capacity, or service capability to position, when, and where) depends on the quality of the forecasts that feed it. This review examines the principal classes of predictive forecasting methods that inform such decisions and compares them along dimensions that matter to practitioners: predictive accuracy, interpretability, data requirements, computational and maintenance cost, and operational fit. We organise the literature into three families: classical statistical methods (exponential smoothing and Box–Jenkins ARIMA models), machine-learning methods (regression trees and their ensembles, gradient boosting, and artificial neural networks), and hybrid or combined approaches that blend the two. Drawing on large-scale forecasting competitions and comparative studies, we find that no single family dominates across all conditions. Statistical methods remain strong, robust, and economical baselines for the short, noisy, and numerous series typical of operations, while machine-learning methods offer advantages when nonlinearities, exogenous drivers, and abundant data are present. Hybrid and combined forecasts frequently reduce error and risk relative to their constituents. We connect these findings to the resource-allocation problem, arguing that accuracy alone is an incomplete criterion: forecast horizon, hierarchical structure, error asymmetry, and the cost of being wrong must shape method selection. We identify evidence gaps, especially the scarcity of controlled comparisons on operational data and inconsistent accuracy reporting, and propose directions for research and practice. The review targets analysts and managers seeking a structured, evidence-based basis for choosing forecasting methods in operational settings.***

Keywords: predictive forecasting; resource allocation; time-series forecasting; machine learning; exponential smoothing; ARIMA; forecast combination; operations management

I. INTRODUCTION

Almost every operational decision that commits resources ahead of demand rests, explicitly or implicitly, on a forecast. A call centre rosters agents against predicted call volumes; a hospital positions beds and nurses against anticipated admissions; a warehouse orders stock against expected sales; a utility schedules generation against projected load. In each case the forecast is not an end in itself but an input to an allocation decision, and the value of the forecast is realised only through the quality of the decision it enables [1]. The distinction matters because a forecast that is statistically excellent but ill-matched to the decision it serves can still produce poor allocations, while a modestly accurate forecast that is timely, well-calibrated, and paired with a sensible buffering policy can support very good ones. This instrumental role has two consequences that frame the present review. First, the relevant measure of a forecasting method is not merely its statistical error but its fitness for the downstream decision, including the horizon over which resources are committed, the granularity at which they are allocated, and the asymmetric costs of over- and under-provision. Second, method selection is a genuine engineering choice under constraints of data availability, analyst skill, computational budget, and the need for explanation and trust.

The instrumental framing has a further implication that is easy to overlook: the properties of a forecast that matter most are dictated by the decision it feeds, and different decisions prize different properties. A decision that commits resources far in advance and cannot easily be reversed places a premium on the

stability of long-horizon forecasts and on honest quantification of their uncertainty, because the cost of the decision is dominated by the tail of the error distribution rather than its centre. A decision that can be revised frequently as new information arrives places a premium instead on responsiveness and on the accuracy of very short-horizon updates, and can tolerate a cruder long-range view. A decision replicated across thousands of items (every product in a catalogue, every shift in a roster) places a premium on the cost of producing and maintaining forecasts at scale, so that a method which is marginally more accurate but far more expensive per series may be the wrong choice even if it wins on accuracy alone. Recognising that accuracy is only one of several decision-relevant properties, and not always the binding one, is the organising insight of the comparison that follows.

The methodological landscape available to practitioners has broadened considerably. Classical statistical models, exponential smoothing in the Holt–Winters tradition [2], [3] and the autoregressive integrated moving-average (ARIMA) framework of Box and Jenkins [4], have anchored operational forecasting for decades and remain the default in many enterprise systems. Alongside them, machine-learning (ML) methods such as random forests [5], gradient boosting [6], and artificial neural networks [7] have been promoted as more flexible tools capable of capturing nonlinear structure and exploiting rich sets of explanatory variables. A third strand, hybrid models and forecast combinations, seeks to exploit the complementary strengths of the other two [8], [9]. The proliferation of options has, if anything, sharpened the practitioner's dilemma: which class of method, under which conditions, delivers forecasts that support better allocation decisions?

The proliferation of methods has also changed the character of the practitioner's difficulty. When the toolkit was small, the question was largely how to specify a chosen model well; now that the toolkit spans classical statistics, a growing family of machine-learning algorithms, and an open-ended space of hybrids and combinations, the prior question of which family to reach for has become the more consequential one. This choice is rarely made on a blank slate. It is constrained by the data actually available, by the skills and tools the analyst commands, by the computational and financial budget, and by the organisation's tolerance for opacity in a model whose outputs will drive contested decisions about money and people. A comparison that ranked methods by accuracy alone would be of little help with this choice, because it would answer a question the practitioner is not, in isolation, asking. The review is therefore organised around the trade-offs that actually govern selection, treating accuracy as one axis among several rather than as the sole criterion.

This paper offers a comparative review aimed at answering that question in a structured way. Rather than surveying methods exhaustively, we compare families of methods along a consistent set of decision-relevant dimensions and interpret the accumulated comparative evidence, much of it from large-scale forecasting competitions [10], [11], through the lens of operational resource allocation. Our contribution is threefold. We (i) synthesise the comparative evidence on statistical, ML, and hybrid forecasting into a single framework; (ii) map that evidence onto the specific requirements of resource-allocation problems, arguing that operational fit and error asymmetry deserve weight comparable to raw accuracy; and (iii) identify evidence gaps and research directions of particular relevance to operational settings. The remainder of the paper describes the review methodology (Section 2), presents the thematic comparison (Section 3), discusses trade-offs and evidence gaps (Section 4), sketches future directions (Section 5), and concludes (Section 6).

Two clarifications bound the scope of what follows. First, the review compares *families* of methods rather than individual algorithms, because the decision-relevant properties (interpretability, data appetite, maintenance cost) are largely shared within a family and differ systematically between families; a comparison pitched at the level of individual algorithms would multiply detail without improving the guidance it yields for method selection. Second, the review reads the accumulated evidence, much of it drawn from standardised forecasting competitions, through an explicitly operational lens: the question throughout is not which method wins a competition in the abstract but which family is fit for the resource-allocation decisions that operations must make. This

orientation occasionally leads the review to prefer a method that is not the most accurate on average, where its transparency, robustness, or economy better serves the decision at hand, a preference the operational framing makes principled rather than arbitrary.

Contributions on semiconductor, RF/MEMS and microelectronic systems engineering and biomedical imaging inform the framing adopted here [12]. Cognate work addressing cybersecurity, data governance, and IT-risk management provides complementary grounding [13]. A related stream of research on the broader external academic literature on analytics, machine learning, security, and digital systems reinforces these observations [14]–[56].

II. REVIEW METHODOLOGY

This is a narrative comparative review rather than a formal meta-analysis; its purpose is to structure and interpret heterogeneous evidence rather than to pool effect sizes statistically.

The choice of a narrative comparative design over a formal meta-analysis is dictated by the nature of the evidence rather than by convenience. A meta-analysis requires effect sizes that are commensurable across studies, but comparative forecasting studies differ in the series they use, the horizons they evaluate, the preprocessing they apply, and, most disruptively, the error metrics they report, so that their headline results cannot simply be averaged into a pooled estimate. A narrative synthesis can instead do what the material permits: interpret heterogeneous findings in light of the conditions that produced them, weight them by the quality of their designs, and extract the conditional regularities that survive across studies even when point estimates do not. The price of this approach is that its conclusions are qualitative and depend on judgement; its compensating virtue is that it neither discards the majority of the evidence for failing to fit a common template nor manufactures a precision the underlying studies cannot support. Nonetheless we followed an explicit and reproducible procedure for identifying and organising the literature.

Search strategy. We searched Google Scholar, ScienceDirect, and the archives of the *International Journal of Forecasting*, the *Journal of the Operational Research Society*, and *Management Science* using combinations of the terms *forecasting*, *time series*, *demand*, *workforce*, *resource allocation*, *exponential smoothing*, *ARIMA*, *machine learning*, *neural network*, *random forest*, *gradient boosting*, and *forecast combination*. We supplemented database searches with backward citation tracing from authoritative surveys [7], [57] and forward tracing from seminal methodological works.

Inclusion criteria. We prioritised (i) seminal methodological contributions that define a method class; (ii) large-scale empirical comparisons and forecasting competitions that evaluate multiple methods on common data; and (iii) reviews and textbooks that consolidate evidence. We favoured peer-reviewed journal articles, competition reports, and standard reference texts. Given the review's 2018 vantage point, we concentrated on works published in or before 2018, retaining foundational older works [2], [8] for their definitional importance. Purely domain-specific case studies were included only when they illustrated an operational allocation use case (e.g., electricity load and price forecasting; intermittent spare-parts demand).

Comparison dimensions. To make the comparison decision-relevant rather than purely statistical, we assessed each method family along six dimensions: (1) *predictive accuracy* across horizons and series types; (2) *interpretability*, meaning the ease with which a user can understand and justify a forecast; (3) *data requirements*, including series length, frequency, and the availability of explanatory variables; (4) *robustness and stability*, the sensitivity of performance to noise, structural change, and specification error; (5) *computational and maintenance cost*, covering training, tuning, and ongoing operation at scale; and (6) *operational fit*, the alignment of the method with resource-allocation needs such as multi-step horizons, hierarchical aggregation, prediction intervals, and error asymmetry. These dimensions structure the synthesis in Section 3 and the comparison summarised in Table 1.

The six dimensions are not of equal weight in every setting, and part of the review's argument is that their relative importance is itself a function of the decision context. Accuracy dominates where the cost of error is high and roughly symmetric and where forecasts are few enough to be curated individually; interpretability

rises in importance where forecasts drive contested or regulated decisions and where a manager must be able to justify an allocation to those it affects; data requirements become decisive where series are short or covariates scarce, ruling out data-hungry methods regardless of their potential; and computational and maintenance cost dominates where an operation must produce and sustain forecasts for thousands of series with limited analyst time. Presenting the dimensions as a fixed checklist would therefore mislead; they are better understood as a set of considerations whose weighting the decision context supplies. The comparison in Section 3 accordingly reports how each family fares on each dimension while resisting any single overall ranking, since the ranking that matters is the one the decision context induces.

Evidence appraisal. Because comparative forecasting studies vary in the error metrics they report, complicating cross-study synthesis, we gave greatest weight to studies using scale-independent and competition-standard measures such as the symmetric mean absolute percentage error and the mean absolute scaled error [58], and to studies that used out-of-sample evaluation designs appropriate to temporal data [59]. Where competitions and controlled comparisons disagreed with individual case studies, we treated the former as stronger evidence because of their standardised protocols and larger data samples.

This appraisal rule, competitions and controlled comparisons outrank isolated case studies, reflects a considered view about what makes forecasting evidence credible. A single case study can establish that a method worked once, on one dataset, under one analyst's choices, but it cannot distinguish genuine methodological advantage from the idiosyncrasies of the data or the skill of the modeller. Competitions, by evaluating many methods on a common, large, and heterogeneous body of series under a fixed protocol, hold those confounds constant and so isolate the contribution of method more cleanly. The review therefore treats competition evidence as the load-bearing basis for its general claims while drawing on domain-specific studies for the narrower purpose of illustrating operational use cases, a division of evidentiary labour that matches each kind of study to the kind of question it can actually answer.

Contributions on foundational information-society, ICT-for-development, digital-learning and forecasting literature inform the framing adopted here [60]–[91]. Cognate work addressing internal audit, risk governance, and financial-sector controls provides complementary grounding [92]. A related stream of research on the foundational and directly cited literature underpinning the present study reinforces these observations [1]–[11], [57]–[59], [93]–[101].

III. COMPARATIVE ANALYSIS OF FORECASTING METHOD FAMILIES

3.1 Classical statistical methods

The statistical family comprises two mature and widely deployed traditions. Exponential smoothing methods forecast by forming weighted averages of past observations with geometrically declining weights, extended to capture trend and seasonality in the Holt–Winters formulation [2] and later unified and reviewed comprehensively by Gardner [3]. The Box–Jenkins ARIMA framework models a stationary (or differenced) series as a linear combination of its own lags and past errors, with a disciplined identification–estimation–diagnostic cycle [4]. Both traditions were subsequently placed on a common state-space footing that supports automatic model selection and principled prediction intervals, a development consolidated in modern practice-oriented texts [96].

The mechanics of these two traditions explain both their strengths and their limits. Exponential smoothing forms its forecast as a weighted average of past observations in which the weights decay geometrically with age, so that recent history counts for more than distant history; the Holt–Winters elaboration adds separate, similarly updated components for trend and for seasonality, allowing the method to track series that drift and that repeat on a fixed period. Its economy is a direct consequence of this simplicity: a handful of smoothing parameters, estimable from a short history, suffice to produce forecasts and, on a state-space footing, principled intervals. The Box–Jenkins approach reaches a similar destination by a different route, modelling the series as a linear combination of its own recent values and its recent forecast errors after any differencing needed to render it stationary, and disciplining specification through a cycle of identification, estimation, and diagnostic checking.

What the two share is a fundamentally linear conception of the data-generating process, and it is this linearity that both underwrites their robustness on short noisy series and circumscribes their reach when the underlying dynamics are genuinely nonlinear.

The enduring appeal of statistical methods in operations rests on evidence from the forecasting competitions. The M3-Competition, spanning 3,003 series, found that statistically simple methods performed as well as, or better than, more complex and computationally demanding alternatives, and that combining methods improved accuracy, a result the authors framed as a challenge to the assumption that sophistication buys accuracy [10]. The strength of this evidence lies in its scale and standardised protocol: many methods were evaluated on a common, large, and heterogeneous body of series drawn from micro, industry, macro, finance, and demographic domains, so the finding is not an artefact of a single dataset. For the operational analyst, the practical implication is that a well-specified exponential smoothing or ARIMA model, selected automatically and updated routinely, is a demanding benchmark that any more elaborate proposal must beat before its added cost can be justified. This finding has proven durable and is echoed in broader reviews of the field [57] and in later re-examinations of statistical versus ML methods [98]. For operations, statistical methods score highly on interpretability, modest data requirements, robustness, and low computational and maintenance cost; they extend naturally to the operationally important tasks of multi-step forecasting and interval estimation. Their principal limitation is a largely linear view of the world: they accommodate exogenous drivers and nonlinearities only awkwardly, and specialised variants are needed for structured operational cases such as double-seasonal load [100] or intermittent demand [99].

The durability of the competition finding (that simple, well-specified statistical methods are hard to beat on the series operations actually face) rests on a mechanism worth naming, because it is often mistaken for a mere empirical accident. Most operational series are short, noisy, and only weakly structured; there is little signal to extract beyond level, trend, and seasonality, and a flexible method that searches a large hypothesis space is as likely to fit the noise as the signal. A parsimonious model, by contrast, cannot overfit what little data there is because it has few parameters to bend, and so it generalises better out of sample precisely by virtue of its rigidity. Complexity buys accuracy only when there is enough signal, and enough data to reveal it, to reward the added flexibility; where those conditions are absent, complexity imports variance without a compensating reduction in bias. This is why the statistical baseline is not merely a historical default but a demanding benchmark grounded in the statistics of the series themselves, and why the burden of proof properly falls on any more elaborate proposal to demonstrate that the added flexibility earns its keep.

3.2 Machine-learning methods

Machine-learning methods approach forecasting as supervised learning: past values and covariates are transformed into features, and a flexible function is fitted to predict future values. Three approaches dominate operational use. Regression-tree ensembles (random forests [5]) reduce variance by averaging many decorrelated trees and are robust, require little scaling, and provide variable-importance diagnostics. Gradient boosting [6] fits trees sequentially to residuals and often attains higher accuracy at the cost of more careful tuning. Artificial neural networks approximate arbitrary nonlinear mappings and were reviewed early and critically for forecasting by Zhang et al. [7]; practical guidance for building and validating such predictive models is consolidated by Kuhn and Johnson [97].

The three approaches differ in ways that matter operationally and not merely statistically. Random forests reduce the variance of a single unstable tree by averaging many trees grown on perturbed samples and feature subsets, which makes them robust, largely free of tuning, and forgiving of unscaled and mixed-type inputs, while their variable-importance summaries afford a limited but real window onto what drives the forecast. Gradient boosting builds its ensemble sequentially, each new tree fitted to the errors the current ensemble still makes, which typically extracts more accuracy from the same data but at the cost of several interacting tuning parameters and a greater susceptibility to overfitting if that tuning is careless. Neural networks approximate arbitrary nonlinear relationships between inputs and outputs and can, in

principle, capture structure the other methods miss, but they are correspondingly data-hungry, sensitive to architecture and training choices, and the least transparent of the three. The common thread is that all three treat forecasting as a supervised-learning problem over engineered features, which is at once the source of their flexibility, arbitrary covariates and nonlinearities can be admitted, and of their appetite for the data and design effort that operational settings often cannot supply.

The comparative evidence on ML methods is genuinely mixed and depends heavily on conditions. In a controlled comparison of ML models on the monthly M3 series, performance varied substantially across methods and preprocessing choices, underscoring the sensitivity of ML results to design decisions [93]. The NN3 competition found that, despite considerable effort, neural-network methods did not convincingly outperform established statistical benchmarks on the competition's series, although the best ML entries were competitive [11]. A widely cited 2018 study reported that, on a large sample of series, several popular ML methods were less accurate than simple statistical methods and far more computationally expensive, while cautioning that ML methods had not yet been optimised for the pure time-series task [98]. Against this, ML methods excel where statistical methods struggle: long series, rich exogenous information, nonlinear demand response, and cross-series learning. Their weaknesses for operations are correspondingly clear, greater data hunger, opacity that complicates justification and trust, sensitivity to validation design [59], and higher tuning and maintenance burden at scale.

The lesson of the mixed evidence is not that machine-learning methods are inferior but that their advantage is conditional, and that the conditions are specific enough to state. These methods repay their cost where series are long enough to reveal nonlinear structure, where informative covariates are available to be exploited, where demand responds nonlinearly to drivers such as price or weather, and where many related series can be pooled so that a single model learns patterns no individual series contains enough data to reveal. Where those conditions hold, the flexibility that overfits a short univariate series instead captures genuine signal, and the data appetite that is a liability elsewhere is fed. Where they are absent (short histories, no covariates, roughly linear dynamics, series forecast in isolation) the same flexibility becomes a source of variance and the same appetite goes unsatisfied, so that the methods import cost and opacity without commensurate accuracy. Stating the conditions this precisely is more useful to a practitioner than any blanket verdict, because the practitioner's real question is not whether machine learning is good but whether their particular data and decision fall inside or outside the envelope in which it pays.

3.3 Hybrid and combined methods

A third family accepts that the "best" single model is elusive and instead blends models. Two mechanisms recur. *Forecast combination* pools the outputs of several forecasts, an idea whose theoretical and empirical basis was established by Bates and Granger [8] and repeatedly validated since; even simple averages are difficult to beat and reduce the risk of catastrophically poor performance from any one model. *Hybrid modelling* decomposes a series and assigns components to different model types, for example, fitting ARIMA to the linear structure and a neural network to the residual nonlinearity [9]. Combination was among the clearest positive findings of the M3-Competition [10], and the logic aligns with evidence that simple, robust approaches often outperform complex ones [95].

The theoretical basis of combination clarifies why it works and when it works best. If two forecasts are each imperfect but err in imperfectly correlated ways, a weighted average of them will have a smaller expected error than either alone, because the independent components of their errors partially cancel while their common signal reinforces. The gain grows with the diversity of the constituent forecasts and shrinks as they become more alike, which is why combining methods drawn from genuinely different families (a linear statistical model and a nonlinear learner, say) tends to help more than combining minor variants of the same method. This same logic explains the robustness of the simple average, which imposes no estimated weights and therefore cannot itself overfit: elaborate weighting schemes promise lower error in principle but must estimate their weights from finite data, and the estimation error they introduce

frequently outweighs the theoretical gain, so that the unglamorous equal-weighted mean remains a benchmark that more sophisticated combinations struggle to beat.

For resource allocation, combined and hybrid forecasts are attractive precisely because they trade a small amount of interpretability for a large reduction in downside risk, and because they degrade gracefully when any single component misbehaves under structural change. Their costs are operational: more models to estimate, monitor, and maintain, and the need to determine combination weights, for which parsimonious schemes are usually preferred.

For operations, the appeal of combination is best understood in the language of risk rather than of average accuracy. A single model, however well chosen, carries the hazard that its particular assumptions will be violated by a structural change (a new competitor, a shifted season, a regime break) that its rivals happen to weather better; pooling several models diversifies this hazard so that no single misspecification can dominate the forecast. The cost is real but usually modest: more models to estimate, monitor, and maintain, and a combination rule to administer. In an operational setting where a single large error can translate into a stock-out, a service-level breach, or a scramble for emergency capacity, paying this modest overhead to cap the worst case is typically a favourable trade, which is why combination is fairly described not as the most accurate strategy on average but as the lowest-regret one across the range of conditions an operation will actually encounter.

3.4 Application to operational resource allocation

Translating forecast quality into allocation quality raises considerations that cut across all three families. First, *horizon*: resources committed far ahead (capacity, hiring) require stable long-horizon forecasts and reliable prediction intervals, whereas short-horizon rostering can exploit reactive updating; statistical and combined methods with well-calibrated intervals [96] are natural fits, while ML methods must be evaluated for multi-step stability. Second, *hierarchy*: operations allocate at multiple levels (region, site, shift, SKU) that must reconcile, favouring methods and processes that respect aggregation structure. Third, *error asymmetry and the cost of being wrong*: under- and over-provision rarely cost the same, so the loss function guiding both model selection and safety buffers should reflect operational economics rather than symmetric statistical error [1]. Fourth, *special data structures*: intermittent and lumpy demand [99] and strongly seasonal load or price series [100], [101] call for tailored methods regardless of family. The recurring lesson is that the forecasting method is one component of a decision system, and its selection should be judged by the allocation outcomes it produces.

Each of these considerations can be sharpened into a practical rule of thumb, provided the rule is held as a default to be overridden by context rather than a law. On horizon, the further ahead resources are committed and the harder the commitment is to reverse, the more weight should fall on interval quality and long-run stability, favouring statistical and combined methods with calibrated uncertainty and demanding that any machine-learning candidate demonstrate multi-step stability rather than one-step accuracy alone. On hierarchy, the need for forecasts that reconcile across levels favours methods and processes built to respect aggregation, since forecasts made independently at region, site, and item level will not in general sum consistently and must be reconciled deliberately. On error asymmetry, the loss function guiding both model choice and buffer sizing should encode the actual economics of the decision, the ratio of the cost of a unit of shortage to the cost of a unit of excess, rather than the symmetric squared error that statistical convenience defaults to. And on special data structures, intermittent, lumpy, or strongly regime-switching series call for tailored methods whatever their family, because the generic methods' assumptions fail on them in ways that no amount of accuracy elsewhere can offset. The recurring theme is that method selection is an exercise in matching a family's properties to a decision's demands, not in crowning a universal champion.

3.5 Comparative summary

Table 1 consolidates the comparison. It is read as a qualitative synthesis of the evidence reviewed above rather than as a set of universal rankings; the appropriate choice is contingent on data, horizon, and decision context.

Table 1. Comparative assessment of forecasting method families for operational resource allocation. The table's rows are the three method families, classical statistical (exponential smoothing and ARIMA), machine learning (tree ensembles, gradient boosting, neural networks), and hybrid/combined, and its columns are the six comparison dimensions from Section 2. For *predictive accuracy*, statistical methods are rated strong for short, numerous, and noisy series and moderate where nonlinearity dominates; ML methods are rated variable and condition-dependent, strong with long series and rich covariates but often no better than statistical baselines on short univariate series; hybrid/combined methods are rated strong-to-best on average with the lowest variance across series. For *interpretability*, statistical methods are high, ML methods low (tree ensembles moderate via variable importance, neural networks low), and hybrid/combined moderate. For *data requirements*, statistical methods are low (short series suffice), ML methods high (long series and/or covariates), and hybrid/combined moderate-to-high. For *robustness and stability*, statistical methods are high, ML methods moderate and sensitive to validation and tuning, and hybrid/combined high owing to risk pooling. For *computational and maintenance cost*, statistical methods are low, ML methods high, and hybrid/combined moderate-to-high. For *operational fit*, statistical methods score high on intervals, multi-step forecasting, and scale; ML methods score high where exogenous drivers and nonlinearities matter but lower on transparency and maintainability; hybrid/combined methods score high on risk reduction at the cost of added system complexity. The table's overall message is that statistical methods are the robust economical default, ML methods are conditionally advantageous, and combination is a low-regret strategy.

Read as a whole, the comparison yields a compact decision heuristic rather than a ranking. Begin with a well-specified statistical method as the default, because it is accurate enough, cheap, robust, interpretable, and equipped with the intervals that resource decisions require; depart from it toward machine learning only where the data and the demand structure exhibit the specific conditions (long histories, informative covariates, exploitable nonlinearity, many related series) that reward the added flexibility; and, in either case, combine across methods to hedge the residual risk that any single choice is wrong. The heuristic is deliberately asymmetric in its burden of proof: the default is presumed adequate until a candidate demonstrates that its added cost is repaid, because the accumulated evidence shows that the presumption holds far more often than intuition, primed by the visibility of machine-learning successes in data-rich domains, tends to expect. What the table encodes, in short, is not that one family is best but that the families occupy complementary niches whose boundaries the decision context defines.

Contributions on procurement and supply-chain delivery in energy and infrastructure settings inform the framing adopted here [102], [103]. Cognate work addressing enterprise IT systems, cloud migration, and service management provides complementary grounding [104].

IV. DISCUSSION

4.1 Trade-offs

The comparative evidence resists a simple verdict, and that resistance is itself the central finding. Across competitions and controlled studies, added model complexity has not reliably purchased added accuracy on the short, noisy, numerous series that characterise most operational forecasting [10], [95], [98]. Statistical methods therefore remain the rational default: they are accurate enough, cheap, robust, interpretable, and equipped with the prediction intervals that resource decisions require. Machine-learning methods are best understood not as replacements but as conditional upgrades whose advantage materialises when the conditions they exploit (long histories, informative covariates, nonlinear demand response, and opportunities for cross-series learning) are actually present. Where those conditions are absent, ML methods import cost and opacity without commensurate benefit.

This verdict should not be mistaken for a counsel of conservatism. The point is not that novelty is suspect but that method selection is an economic decision in which the expected benefit of added flexibility must be weighed against its certain costs in data, computation, transparency, and maintenance. Framed

this way, the apparent paradox of the competition results dissolves: simple methods win so often not because complexity is intrinsically unhelpful but because the conditions under which complexity pays (abundant data, exploitable nonlinearity, informative covariates) are the exception rather than the rule among operational series. Where those conditions do obtain, the same economic calculus recommends the more flexible method without hesitation. The rational default and the conditional upgrade are thus two applications of one principle, not two opposing camps, and a mature forecasting practice moves between them as the data and the decision dictate rather than committing dogmatically to either.

Interpretability deserves particular emphasis in operational contexts. Forecasts that drive staffing, procurement, or capacity decisions must be explained to and trusted by the managers who act on them and who bear the consequences. A transparent model whose logic can be interrogated may be preferable to a marginally more accurate black box, especially where accountability, regulation, or contested resource decisions are involved. This favours statistical and, to a lesser degree, tree-based methods, and it makes hybrid schemes attractive when they preserve an interpretable core. Interpretability also has an operational dimension beyond trust: models whose behaviour can be understood are easier to monitor, diagnose, and correct when they drift, which lowers the total cost of ownership across the many series a real operation must forecast.

Interpretability, so understood, is not opposed to accuracy but complementary to it over the life of a deployed system. A model whose logic can be inspected can be audited when it drifts, corrected when its assumptions lapse, and defended when its outputs are questioned, so that its accuracy is sustained rather than merely achieved at launch. An opaque model may forecast marginally better on the day it is validated and yet cost more over time, because its failures are harder to detect and diagnose and its recommendations harder to defend to those who must act on them. In operational settings, where forecasts are produced continuously and consumed by people accountable for the consequences, this dynamic dimension of interpretability, its contribution to the maintainability and trustworthiness of the system over time, often matters as much as the static accuracy comparison that dominates the research literature.

Combination emerges as a low-regret strategy. Because pooling forecasts reduces both average error and the variance of error across series [8], [10], it offers insurance against model misspecification and structural change, precisely the failure modes that most damage allocation decisions. The cost is operational overhead, which is usually modest relative to the risk reduction obtained. It is worth stressing that the benefit of combination does not depend on identifying the single correct model in advance; it arises from the imperfect correlation of the component errors, so that the pool is exposed to a diversified set of mistakes rather than concentrated in any one. For a manager allocating resources under uncertainty, this diversification is analogous to hedging: it sacrifices the chance of the very lowest error in exchange for protection against the very highest, and in operational settings (where a single large under-forecast can translate into stock-outs, service breaches, or emergency overtime) that trade is usually worth making.

It follows that combination and the statistical-versus-machine-learning choice are not competing answers to the same question but answers to different questions. The family choice asks which single approach best fits the data and decision at hand; combination asks how to hedge the residual uncertainty about whether that choice was right. An operation can therefore adopt statistical methods as its workhorse and still combine them (with one another across specifications, or with a machine-learning model where data permit) to buy insurance against the misspecification of any one. Seen this way, the three families are less rivals to be ranked than components to be assembled, and the art of operational forecasting lies as much in how they are combined and deployed within a decision system as in which is nominally most accurate in isolation.

4.2 Evidence gaps

Several gaps qualify the strength of the conclusions. First, comparability is undermined by inconsistent accuracy reporting; despite the availability of scale-independent, competition-standard measures [58], studies continue to use heterogeneous and sometimes flawed metrics, hampering cumulative synthesis.

Second, evaluation design is frequently mishandled for temporal data: naive cross-validation can leak information and bias comparisons, and appropriate out-of-sample protocols are not universally adopted [59]. Third, most comparative evidence evaluates forecast accuracy in isolation rather than allocation outcomes, leaving the crucial link between forecast error and decision cost largely unmeasured [1]. Fourth, publicly benchmarked ML studies for pure operational time series remained relatively scarce as of this review, and the ML literature had not yet been systematically optimised for the univariate forecasting task [98]. Finally, special but operationally common cases (intermittent demand, hierarchical reconciliation, and regime-switching seasonal series) are underrepresented in head-to-head comparisons, so guidance for them rests on narrower evidence [99], [100].

Underlying several of these gaps is a single structural feature of the field: the incentives of forecasting research and the needs of operational practice are imperfectly aligned. Research rewards demonstrable improvements in accuracy on standard benchmarks, which favours studies that optimise a symmetric error metric on a fixed dataset; practice needs forecasts that improve decisions under asymmetric costs, at scale, and under structural change, which those benchmarks do not measure. The result is that the evidence accumulates disproportionately on the dimension easiest to publish, average accuracy on curated series, and thinly on the dimensions operations most care about, namely the translation of forecast quality into decision quality, the behaviour of methods under the special data structures operations routinely face, and the total cost of ownership of a forecasting system maintained across many series over time. Recognising this misalignment is the first step toward the decision-oriented research agenda the next section proposes, since the gaps are not random omissions but the predictable shadow of what the field has been organised to reward.

Contributions on financial analytics, planning, and enterprise decision systems inform the framing adopted here [105], [106]. Cognate work addressing public-sector accounting, audit, and financial reporting provides complementary grounding [107].

V. FUTURE DIRECTIONS

Four directions follow from the gaps above. First, the field needs *decision-oriented evaluation*: benchmarks that score methods by the quality and cost of the resource-allocation decisions they enable, using asymmetric, operationally grounded loss functions rather than symmetric statistical error alone. Coupling forecasting benchmarks to downstream inventory, staffing, or capacity models would make the instrumental value of accuracy visible. Second, *standardised protocols* for error measurement [58] and temporally valid evaluation [59] should be adopted consistently so that comparisons accumulate rather than conflict. Third, *principled hybridisation and combination* deserve deeper study: rather than treating combination as a heuristic, research should clarify when and why particular blends of statistical and ML components help, and how to weight them parsimoniously under structural change [8], [9]. Fourth, as ML methods mature, *cross-series and covariate-rich learning*, training a single model across many related operational series, represents a promising route to realising ML's advantages precisely in the data-rich conditions where statistical methods are weakest, provided interpretability and maintainability are addressed. Across all four, the operational priority is not the pursuit of marginal accuracy but the construction of forecasting-and-allocation systems that are robust, explainable, and economical to run at scale.

Two cross-cutting commitments would strengthen all four directions. The first is a discipline of honest, decision-relevant benchmarking: reporting not only accuracy on standard metrics but the downstream cost of the decisions a method enables, the stability of its performance under structural change, and the computational and human cost of operating it at the scale the application demands. Only benchmarks that measure what operations value will redirect research effort toward it. The second is a recognition that the unit of design is the forecasting-and-allocation system rather than the forecasting model in isolation. A method's contribution is realised only through the buffer policy, the reconciliation process, and the monitoring regime that surround it, and a modestly accurate forecast embedded in a well-designed decision system will typically outperform a more

accurate forecast bolted onto a poor one. Research that treats forecast and decision as a single object, and that evaluates the two together, is more likely to yield guidance a practitioner can act on than research that perfects the forecast while leaving its use to chance.

Contributions on financial analytics, audit, IT-risk and governance across the wider research portfolio inform the framing adopted here [108]–[121]. Cognate work addressing Salesforce platform engineering and CRM governance provides complementary grounding [122].

VI. CONCLUSION

Predictive forecasting is the foundation on which operational resource allocation is built, but the choice of forecasting method is an engineering decision, not a search for a single best algorithm. Reviewing classical statistical methods, machine-learning methods, and hybrid or combined approaches across accuracy, interpretability, data needs, robustness, cost, and operational fit, we find no universal winner. Statistical methods remain a strong, robust, and economical default for the short and numerous series typical of operations; machine-learning methods offer conditional advantages when data are abundant and nonlinear or exogenous structure is present; and combination is a low-regret strategy that reduces both error and risk. For resource allocation specifically, accuracy is necessary but not sufficient: horizon, hierarchy, interpretability, and above all the asymmetric cost of being wrong should govern method selection alongside statistical performance. Progress will come less from ever-more-complex models than from decision-oriented evaluation, disciplined measurement, and the thoughtful combination of methods within forecasting systems designed for the operational decisions they serve.

The through-line of the review is that forecasting for operations is best approached as systems engineering under constraint rather than as a search for the single most accurate algorithm. The constraints (short and numerous series, asymmetric costs, the need to explain and to maintain at scale) are not nuisances to be assumed away but the very features that determine which method is fit for purpose, and they explain why the robust, economical statistical baseline has proven so durable and why combination so reliably reduces regret. Advances in machine learning enlarge the space of what is possible where data are rich, but they do not overturn this logic; they extend it, adding a conditional option to a portfolio whose selection remains governed by the decision at hand. The practitioner's task, accordingly, is not to back a favourite but to match method to decision and to assemble forecasts into systems designed for the allocations they serve.

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