Scaling-aware rating of count forecasts

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Abstract
Forecast quality should be assessed in the context of what is possible in theory and what is reasonable to expect in practice. Often, one can identify an approximate upper bound to a probabilistic forecast’s sharpness, which sets a lower, not necessarily achievable, limit to error metrics. In retail forecasting, a simple, but often unconquerable sharpness limit is given by the Poisson distribution. When evaluating forecasts using traditional metrics such as Mean Absolute Error, it is hard to judge whether a certain achieved value reflects unavoidable Poisson noise or truly indicates an over-dispersed prediction model. Moreover, every evaluation metric suffers from precision scaling: Perhaps surprisingly, the metric’s value is mostly defined by the selling rate and by the resulting rate-dependent Poisson noise, and only secondarily by the forecast quality. For any metric, comparing two groups of forecasted products often yields “the slow movers are performing worse than the fast movers” or vice versa, the naïve scaling trap. To distill the intrinsic quality of a forecast, we stratify predictions into buckets of approximately equal predicted value and evaluate metrics separately per bucket. By comparing the achieved value per bucket to benchmarks, we obtain an intuitive visualization of forecast quality, which can be summarized into a single rating that makes forecast quality comparable among different products or even industries. The thereby developed scaling-aware forecast rating is applied to forecasting models used on the M5 competition dataset as well as to real-life forecasts provided by Blue Yonder’s Demand Edge for Retail solution for grocery products in Sainsbury’s supermarkets in the United Kingdom. The results permit a clear interpretation and high-level understanding of model quality by non-experts.

Keywords: Data science, Forecasting, Applied Probability, Monitoring forecasts
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1. Introduction: The unsolved problem of forecast rating under precision scaling

Forecasting is an application of Information Technology. Just like any other technology – think of the speed of rockets or the efficiency of power plants – forecasting performance is constrained by fundamental natural bounds. This aspect is often downplayed in practice, and the unattainability of a perfect deterministic forecast is taken as witness of forecasting problems (Bower, 2023). However, even assuming perfect input data quality, what a forecast can achieve in terms of precision and accuracy is not defined by its subscriber’s wishes and its creator’s skills but mainly by noise: “Forecasts characterize and reduce but generally do not eliminate uncertainty”, as succinctly distilled by Gneiting et al. (2007). A probabilistic forecast should make the strongest, yet true statement possible. In the words of Gneiting and Katzfuss (2014), “probabilistic forecasting aims to maximize the sharpness of the predictive distributions, subject to calibration, on the basis of the available information set”.

1.1. Problem statement

For a meaningful forecasting evaluation, it is therefore indispensable to quantify the uncertainty that is ideally reachable under a given set of available covariates and to view the achieved forecasting performance in that context. This is, however, not at all what is done in practice. Metrics that are used in business and operations, such as Mean Absolute Error (MAE) and their normalized variants (errors divided by the mean observation, discussed in detail in Section 2.4) do not answer at all to which extent the forecast matches the statistical ideal of maximal sharpness under calibration. Forecasters routinely ask and answer “what MAE do I achieve?”, but, in our experience, they don’t ask “what MAE could I ideally achieve?”. The answer to the first question, however, cannot be interpreted properly without answering the second!

As we will show below, even skilled experts cannot develop a reliable intuition that allows them to intuitively answer the second question, due to precision scaling: The possibly achievable MAE depends strongly on the forecasted value itself. When precision scaling is ignored, one falls into what we name the “naïve scaling trap” – an ostensible effect in forecast quality is not real, but only a consequence of precision scaling.

As a consequence, the problem of rating forecast quality beyond relative comparisons (one forecast is better than some competing method, when evaluated on a fixed set of data) remains largely open. Comparisons across subsets of data (e.g. comparisons across product groups), which are being routinely performed by directly comparing scaling-infected raw metrics (Bower, 2023), are doomed to be infected by the naïve scaling trap and lead to erroneous conclusions (“the slow-moving shoes are performing worse than the fast-moving dairy products”).

Here, we solve the problem of forecast rating by a well-defined sequence of data processing and visualization techniques. These account for scaling and put the achieved values of traditional metrics into the context of what is ideally possible. By explicitly answering the second question (“what MAE could I ideally achieve”), the user then obtains a vivid picture of the true quality of a forecast. This allows them to focus
improvement efforts on those subsets of data that truly bear improvement potential instead of those that only ostensibly perform weakly.

In a nutshell, our method consists of segregating the set of predictions and actuals into buckets that share approximately the same prediction. All predictions in one bucket then naturally come with the approximately same expectation value for any forecasting metric. For each bucket, we establish which is the theoretical lower bound to the forecasting metric, and which reference values of that metric can still be considered “excellent”, “good”, “OK”, “fair”, “insufficient” and “unacceptable”. Reference values are scaled to allow transfer between different buckets so the grades only need to be defined for one reference bucket, which we will elaborate in Section 3.3. The forecasting metric is then evaluated for each bucket on the actual observations and compared to the reference values to provide a rating of the quality in the respective bucket. The thereby achieved ratings per bucket are re-aggregated to provide an overall rating (“good”, “OK”, “fair”...), or reference rating values for the overall achieved metric (“the overall MAE amounts to 5.4, which is ‘good’”). Thereby, we answer the question “what MAE could I ideally achieve?” and set the measured value into that context.

1.2. Precision scaling

Forecasting professionals have to handle business stakeholders who sometimes set overly ambitious goals that are driven by gut feeling instead of quantitatively corroborated benchmarks. The reason for the confidence with which forecasting goals are set is that metrics such as percentage errors seem to speak for themselves: 5% appears to be good, 20% fair, and 70% unacceptable. As we show below, such assessment is meaningless, for every metric, unless the ideally achievable distribution of targets (for example, Poissonian) and the scale are set: Normalized and absolute errors, even for ideal forecasts, depend strongly on the predicted value itself, since the variance of distributions that describe non-negative integer counts depends on their expectation value. For example, the variance of the Poisson distribution is equal to its mean. This is in stark contrast to continuous quantities, for example, temperature, for which one can envisage a truly homoscedastic forecast, such as a normal distribution with constant, temperature-independent variance.

This common but often overlooked property is precision scaling. As a consequence, any metric evaluated on a group of forecasted mean values of about 1 (which retailers call “slow-movers”) averages to a different value than for a group of forecasted mean values of about 100 (the “fast-movers”). This difference is often not due to the forecast being systematically “better” or “worse” in the slow- or fast-moving regimes in the sense that certain systematic omissions of features or certain mistakes were done in one or the other regime, but mainly because of the heteroscedasticity of counting distributions.

In short, precision scaling makes it challenging to make meaningful statements about some data-subsets performing “better” or “worse” than others. Any metric reflects, in the first place, the different typical scale of errors for different velocities, and only secondarily reveals the quality of the forecast.
1.3. Literature background

There has been much debate on the usage of the “correct” evaluation metric (Davydenko and Fildes, 2014; Hyndman and Koehler, 2006; Gneiting, 2011; Fildes et al., 2022; Wheatcroft, 2022; Petropoulos, F. et al., 2022; Kolassa, 2016; Hewamalage et al., 2023). From a practitioner’s point of view, it is important to bear in mind that forecast quality is not a goal by itself, but only the means to achieve the optimal business decision, which makes forecast quality unimportant in certain cases (Koutsandreas et al., 2021; Robette, 2023; Kolassa, 2023b).

In general, there is a gap between the statistics state of the art and what is done in practice. Statisticians naturally see forecasts as probabilistic, falsifiable statements, and use the tools of probability theory to evaluate them (Gneiting and Katzfuss, 2014). This approach leads, among others, to the notions of calibration and sharpness (Gneiting and Raftery, 2004; Gneiting et al., 2007) and optimal point forecasts (Gneiting, 2011). Many superficially surprising or paradoxical properties of metrics can then be understood best when they are investigated in terms of those concepts (Gneiting, 2011; Kolassa, 2020).

The effect that we focus on here, precision scaling, is due to the scaling properties of the sharpest possible distribution in a given forecasting context. To our knowledge, precision scaling has not been investigated so far, and computing expectation values of metrics (or other statistics) under a certain distribution is not common in forecasting practice, in great contrast to physics, for example.

Many dimensionless metrics have been proposed. For example, when the popular Mean Absolute Error is divided by the mean observation (Kolassa and Schütz, 2007), one obtains the dimensionless normalized MAE. Other attempts to obtain scale-free measures are MASE (mean absolute scaled error) (Hyndman and Koehler, 2006). Mean Absolute Percentage Error (MAPE) remains popular due to its superficial simplicity, despite strong criticism (Kolassa and Martin, 2011; Tichy, 2023a; Kolassa, 2023a). Among other problems, MAPE is unbounded when over-forecasting, but bound by 100% when under-forecasting. This asymmetry lets it favor biased point forecasts. The symmetric MAPE introduced by Makridakis (1993) resolves this asymmetry only ostensibly, as pointed out by Goodwin and Lawton (1999). Other adaptations of MAPE that aim at overcoming its asymmetry use the logarithm of the forecast-to-actual ratio (Tofallis, 2013). All these dimensionless metrics remain scale-dependent in the sense that they assume different values for slow and for fast velocities under a maximally sharp and calibrated forecast.

The prevalence of precision scaling in all distribution-agnostic metrics is not at all surprising: Precision scaling is a property of how the family of sharpest distributions behaves when the parameter reflecting the expected value is changed, not a property of the metric. Any attempt to judge the achieved values of metrics therefore needs to explicitly incorporate the sharpest possible distribution.

Ex post, the lack of interest in precision scaling is surprising, given the enormous impact that this effect has on forecasting KPIs and the simplicity of computing expectation values. It exemplifies the wide gap between statisticians and practitioners, which we hope to help close.
1.4. Precision scaling in the Poisson distribution

For the Poisson distribution, absolute errors increase and normalized errors (absolute errors divided by the mean observation) decrease with increasing Poisson process rate. This is why current approaches such as the one described by Bower (2023), a “basic heat map or listing of worst-case forecast error – at either the item or product-family level” are heavily infected by the naïve scaling trap, the mis-interpretation of a symptom caused by precision scaling as a genuine signal of model quality. We will see below upon both an academic example (a baseline model on the the M5 dataset, Section 4) and upon a real-world example (the forecasts provided by Blue Yonder using their Demand Edge for Retail solution to UK retailer Sainsbury’s, Section 5) that the naïve scaling trap is the prevalent effect when evaluating different groups against each other. We thus need to make the forecast judgment procedure aware of the underlying ideal distribution by judging each bucket of similar predictions separately. For a Poisson-limited forecast, a normalized error (mean absolute error divided by mean observation) of about 70% for a slow-mover of selling rate of about 1 per day can be judged as “excellent”, while a 20% normalized error for a fast-mover that sells about 100 pieces per day is disappointing. Similar velocity-dependent behavior also prevails in other contexts.

To judge a forecast within a real-world business process, abstract statistical tools will not suffice, but industry-dependence needs to be accounted for. The method we propose in this paper, scaling-aware forecast rating, allows to parametrize the users’ expectation to forecasting quality and handles the precision scaling problem. The thresholds for judging that a forecast is “excellent“, “good enough“ or “needs intervention“ then reflect the typical degree of (un)certainty in a specific industry and on a certain forecast horizon.

1.5. Forecast comparison use cases

Different situations require users to compare forecasts:

- Model comparison: For a given fixed dataset, different models are compared to select the better one (“Which model shall we choose in production? From what forecasting vendor shall we buy?”).

- Dataset comparison: For a given fixed model, subsets of data are compared, e.g., to prioritize model improvement efforts (“On what products does the model perform better, on which departments shall we focus for our data cleaning efforts?”).

Model comparison is the least problematic application of evaluation metrics: Precision scaling artefacts that affect a metric affect the two (or more) competing models in similar ways. When a business-relevant overall metric, which quantifies the business impact (Robette 2023), has improved on a given fixed dataset for a new model, aside from pathological edge cases, one can typically conclude that the newer model yields better performance. Nevertheless, it is helpful to be given additional information such as at which selling rates the improvement has occurred, and whether that improvement was evenly applied across all velocities, or favors the slow- or fast-movers.

Dataset comparison is a routine for practitioners (Bower 2023) and suffers immensely from precision scaling: Given two datasets, one of them certainly contains more slowly
moving items than the other. The difference in metrics then reflects this difference between the datasets, which is often erroneously interpreted as an indication for “better” or “worse” model performance (“your forecast fails for slow-movers!”).

Hybrid comparisons involve both model and dataset changes, and a naïve comparison of metrics across different industries (“grocery retailer A achieves 5% error with a time-series model, softlines retailer B achieves 10% error using a machine-learning model”) is doomed to fail, as elaborated by Kolassa (2008).

Scaling-aware forecast rating sheds more light on model comparison, allows dataset comparison and, to some extent, hybrid comparison. It is intended to allow a judgement of forecasts by closing the gap between distribution-based statistical approaches and business applications. The reasons why the forecast on one dataset might be “far away from Poissonian”, however, might be solvable or unsurmountable, depending on the situation. Our aim is not to make strong general statements about forecastability – we deem such general claims impossible, since forecastability requires a lot of situational and domain-specific knowledge – but to provide simple tools to tackle domain-independent precision scaling. Only if these purely statistical effects are under control can we tackle more involved question of forecastability.

1.6. Outline

A sandbox model for comparing forecast accuracies is introduced in Section 2 along with the metrics and the M5-competition dataset (Makridakis et al., 2022a,b). Our method is motivated and described in Section 3. The technique is applied to different forecast comparison situations on the M5 dataset in Section 4, and on Blue Yonder’s Demand Edge for Retail forecast for the UK retailer Sainsbury’s in Section 5. Several possible future avenues are discussed in the Conclusions, Section 6.

2. Forecasting and evaluation setup

Consider a retailer that offers many different products in many locations, for which a forecast for $n > 1$ different product-location-day combinations is generated. We remain with that illustrative example throughout the rest of the paper, but our argument applies to any countable forecast target.

2.1. A simple approximate upper sharpness bound: The Poisson distribution

Establishing the sharpest possible forecast under given circumstances can be challenging, but one can nevertheless sometimes find approximate benchmarks. In non-personalized grocery retail, it is helpful to employ the Poisson distribution as a simple and intuitive working horse (Tichy, 2023c). Let us recapitulate the prerequisites that go into the Poisson assumption: Consider the number of sold items $s$ of a product in a store on a given day, for a well attended supermarket with many ($N > 100$) customers per day. Our forecast predicts the total number of customers $N$, and the average probability that they buy a given product, which we assume to be small, $p < 0.05$. Moreover, no customer buys two or more pieces of the same item. We then deal with $N$ independent
Bernoulli-events with success probability $p$, such that the probability to sell $s$ items on that day is binomial,

$$P_{\text{binomial}}(s|N, p) = \binom{N}{s} p^s (1-p)^{N-s}, \quad (1)$$

which, for all practical purposes, is indistinguishable from the Poisson distribution of rate $\mu = N \cdot p$,

$$P_{\text{Poi}}(s|\mu = N \cdot p) = \frac{e^{-\mu} \mu^s}{s!}, \quad (2)$$

since it holds

$$\sum_{s \geq 0} |P_{\text{Poi}}(s|\mu = N \cdot p) - P_{\text{binomial}}(s|N, p)| \leq 2p^2 N. \quad (3)$$

We will therefore assume that the forecaster performs a training with Poisson loss function and extracts the expectation value of the Poisson distribution, which is then published as a forecast for the expectation value (Snyder et al., 2012; Gneiting et al., 2007; Czado et al., 2009).

2.2. Corrections to the Poisson distribution

In practice, the Poisson assumption laid out above is never perfectly fulfilled. Several effects both increase and decrease the width of the distribution that one could ideally achieve to predict.

In the first place, perfectly knowing the number of visitors $N$ and the average probability $p$ to buy an item is a strong assumption – not all factors that influence demand can be known in practice. Hence, this item-level buy probability $N \cdot p$ is itself randomly distributed. If that distribution is a gamma-distribution, the resulting demand distribution becomes gamma-Poisson, that is, negative-binomial.

The forecast could, however, also exploit personal information about individual customers, that is, instead of assuming an average probability $p$ for the individual buy, this might be personalized to $p_1 \ldots p_N$. Le Cam’s theorem (Le Cam, 1960) sets a bound to the total variation distance between the resulting Poisson-binomial and Poisson distributions. Under plausible assumptions, the potential of added forecast sharpness via personalization is severely limited: An extraordinary degree of individualization (predictions of individual customer buy probabilities need to convey a lot of information, i.e. $p_j \ll 1$ or $p_k \approx 1$, for the same product) would be necessary to achieve a clearly-sharper-than-Poisson distribution.

The assumption that only one item is bought per customer – underlying the Bernoulli process – is also slightly broken in practice: At Sainsbury’s UK, about 87.5% of sold items are bought alone, 9.8% are bought in pairs of two pieces, about 1.5% in triplets of three pieces, four or more pieces are also occurring. The variance of the resulting distribution is thereby increased. In certain areas of retail, a buy of many pieces of the same item is not the exception but the rule: In hardware stores, bathroom tiles of a certain kind are not sold at all on most days, until someone wants to refurbish their
bathroom and buys a large number. For such products, the Poisson assumption is clearly unsuitable. In grocery retail, however, the effect amounts to a manageable correction.

Finite stocks censor demand values that are larger than the available stocks. This right censoring effectively decreases the width of the observed distribution, which can give a false impression when evaluating a forecast for unconstrained demand against actual constrained sales.

A priori, it is difficult to estimate the strength of these effects. For the datasets that we employed—M5 and Sainsbury’s—we will see, ex post, that the Poisson limit remains a meaningful first-order benchmark, and that the effects mentioned in this section can be considered “second-order terms”, well behind precision scaling. Quantifying their impact more precisely is certainly desirable, and we plan to do so in future works. In general, if a more suitable benchmark distribution is available, which may include effects such as over-dispersion, zero-inflation, and right censoring, our method can easily be adapted, mutatis mutandis.

Hence, to focus on our main contribution, the scaling-aware rating of forecasts across selling rates, our working assumption is that a Poisson forecast constitutes the best possible, that is, the sharpest possible, prediction.

2.3. Forecasting model

Under the Poisson assumption, a product-location-day combination \(j\) (\(1 \leq j \leq n\)) is governed by a true selling rate \(t_j\), which will forever remain unknown and is only under control in the numerical experiments conducted here. That is, the sales \(s_j\) are random samples from the Poisson distribution with rate \(\mu\) assuming the true value \(t_j\):

\[
s_j \sim \text{Poi}(s = s_j | \mu = t_j) = \frac{e^{-t_j}t_j^{s_j}}{s_j!}
\]

The forecast for that product-location-day \(j\) is an estimated selling rate \(r_j\), that is, the forecaster believes that each observed sales value \(s_j\) behaves as if it were drawn from a Poisson distribution with rate \(\mu = r_j\). The forecaster is self-confident and believes that the forecasted rates are indeed perfectly known Poisson-rates. Our considerations are fully independent of how the forecaster constructed the \(r_j\): They might use a time-series approach, a machine-learning model, hybrid approaches, or any other method (Petropoulos, F. et al., 2022).

2.4. Evaluation metrics

To evaluate the quality of their forecast, the forecaster relates the observed sales \(\vec{s} = (s_1, \ldots, s_n)\) to their previously predicted rates \(\vec{r} = (r_1, \ldots, r_n)\) by evaluating a metric \(M(\vec{r}, \vec{s})\), where \(M\) can take many different forms.
2.4.1. Loss functions on point estimates

The forecaster can use several loss functions to assess the forecast, including Mean Absolute Error (MAE),

\[
\text{MAE}(\vec{r}, \vec{s}) = \frac{1}{n} \sum_{j=1}^{n} |s_j - \text{median}(P_{Poi}(\mu = r_j))|, \tag{5}
\]

where the optimal point estimator for MAE, the median of the predicted distribution, replaces the mean \(r_j\) (Schwertman et al., 1990; Gneiting, 2011). That is, the forecaster is aware that their forecast \(r_j\) is not a universal best guess that can be fed into any metric, but that, depending on the chosen metric, an appropriate point estimate needs to be extracted (Kolassa, 2020). MAE naturally takes larger values for larger predictions, a first attempt towards a scale-free metric is the dimensionless normalized MAE (Kolassa and Schütz, 2007), often called Weighted Mean Absolute Percentage Error (WMAPE),

\[
\text{WMAPE}(\vec{r}, \vec{s}) = \frac{\sum_{j=1}^{n} |s_j - \text{median}(P_{Poi}(\mu = r_j))|}{\sum_{j=1}^{n} s_j} = \frac{\text{MAE}(\vec{r}, \vec{s})}{\frac{1}{n} \sum_{j=1}^{n} s_j}, \tag{6}
\]

which turns out below to be scale-dependent as well.

The still-popular Mean Absolute Percentage Error (MAPE) comes with many flaws (Hyndman and Koehler, 2006; Kolassa and Martin, 2011; Kolassa and Schütz, 2007). In particular, it is undefined for observations \(s = 0\), whose treatment heavily affects the achieved MAPE-values (Kolassa, 2023a). These problems are complemented by the particularly complicated precision scaling of MAPE under the Poisson distribution described by Tichy (2023a), in which a simple removal of events with \(s = 0\) is assumed.

2.4.2. Ranked Probability Score

MAE has an immediate business interpretation: A planner who orders \(\text{median}(r_j)\) pieces will, on average, have an excess or under-stock of MAE pieces. The necessity to use the median of the distribution, however, leads to unpleasant discontinuous behavior (the median of the Poisson distribution jumps from 0 to 1 at the rate \(r = \log 2 \approx 0.693\)). To provide a business interpretation while acknowledging the probabilistic nature of the forecast, the forecaster also evaluates the Mean Ranked Probability Score (MRPS), which is the mean over all product-location-days of the discrete Ranked Probability Score (RPS) (Epstein, 1969; Gneiting and Raftery, 2007; Tichy, 2022).

\[
\text{RPS}(s_j, r_j) = E(|x - s_j|)_{x \sim P_{Poi}(s=x|\mu=r_j)} - \frac{1}{2} E(|x - y|)_{x \sim P_{Poi}(s=x|\mu=r_j), y \sim P_{Poi}(s=y|\mu=r_j)}, \tag{7}
\]

\[
\text{MRPS}(\vec{s}, \vec{r}) = \frac{1}{n} \sum_{j} \text{RPS}(s_j, r_j). \tag{8}
\]
MRPS generalizes MAE from simple point estimates to distributions: For a deterministic zero-width forecast that states that \( r_j \) occurs with certainty, the first term in Eq. (7) becomes \(|r_j - s_j|\) and second term vanishes \((\text{Gneiting and Raftery, 2007})\). We define a normalized version of MRPS, the Normalized Mean Ranked Probability Score (NMRPS),

\[
\text{NMRPS}(\vec{s}, \vec{r}) = \frac{\text{MRPS}(\vec{s}, \vec{r})}{\frac{1}{n} \sum_j s_j} = \frac{\sum_j \text{RPS}(s_j, r_j)}{\sum_j s_j}.
\]

Precision scaling affects (N)MRPS in a similar way as MAE/WMAPE. The zoo of metrics is large, the sample that we chose here shall illustrate the ubiquity of precision scaling in different metrics and simultaneously cover the most popular ones.

2.5. Goals of forecast judgement

To make our discussion more tangible, we use the dataset for the validation period, 2016-04-25 to 2016-05-22, of the M5-competition dataset \((\text{Makridakis et al, 2022b,a})\). Our forecaster is interested in the overall performance of their model (“is that a good model?”), and in systematic patterns in the performance (“are we doing better in Hobbies or in Household?”), and therefore evaluates the forecast not only globally, but also segregated by department (or by other properties known at the moment of the forecast). Their goal is to judge the performance across strata to take high-level decisions: Shall data scientists focus on a certain product group, on a certain location, on certain patterns in time \(\text{Bower, 2023}\)? Which improvement can be expected? We aim at answering these questions in a non-defensive way that allows a clear operational interpretation (“the forecast is close/far from the Poisson ideal”), without falling into the na"ive scaling trap, i.e., misinterpreting inevitable precision scaling effects due to the distribution’s heteroscedasticity as true differences in forecast quality.

2.6. Baseline model and na"ive scaling trap

To obtain a maximally sharp and calibrated baseline model, we construct predictions \(r_{\text{baseline}}^j\) via in-sample Expectation Maximization (EM) of the Poisson likelihood, applied on the entire set of sales \(\vec{s}\) \((\text{see Appendix A})\). That is, the EM procedure ingests all outcomes \(s_j\); using the empirical frequencies \(P(s)\) and the Poisson distribution assumption \((4)\), it computes a prior distribution of rates \(P(r)\), from which it samples the rate \(r_{\text{baseline}}^j\) for each outcome \(s_j\) via \(P(r|s_j) = P(s|r_j)P(r)/P(s)\). The rates \(r_j\) that are then generated could hence have led to the outcome \(s_j\) under a Poisson distribution. By construction, we thereby obtain a “perfect” model: For every set of rates around some value \(r\), the associated observations seem to be sampled from a Poisson distribution of that rate \(r\).

The Expectation Maximization procedure does not use any feature or any structure in the data (e.g., whether two sales values \(s_j, s_k\) belong to the same product in different stores or to the same product-store-combination for different days is not accounted for). For our purpose of setting a benchmark, this baseline “forecast” \((\text{given that it is generated and evaluated using the same dataset, it is not, technically speaking, a forecast of some future event})\) \(r_{\text{baseline}}^j\) is suitable, since the sales \(s_j\) are Poisson distributed for each prediction \(r_{\text{baseline}}^j\).
Figure 1: Calibration diagrams for ideal baseline model, based on the heuristic of Appendix A. Distribution of observed sales frequency (orange) vs. predicted probability (blue, this color refers to the ideal Poisson case throughout this article). Upper left: Overall marginal calibration diagram (for visual convenience, we mapped the observation 0 to 0.5 to fit into the log-log-scale). Upper right: Cut on $1.8 < r < 2.2$. Lower left: Cut on $4.8 < r < 5.2$. Lower right: Cut on $9.8 < r < 10.2$.

As an alternative to using true sales data and artificially generated sales rates, one could have worked on a fully synthetic dataset. In that case, the selling rates themselves would be sampled randomly, e.g. from a gamma-distribution, and the observations $s_j$ would be Poisson-sampled for each rate $r_j$. Our point is, however, to exemplify the value of our method upon real-world datasets, which is why we use the M5 dataset as a starting point and need to generate an “ideal forecast”.

The calibration diagrams of Figure 1 confirm that the baseline $\vec{r}_{\text{baseline}}$ is an excellent model, since the unconditioned and the conditioned distributions of actually observed sales $s$ match the Poisson prediction well. It is fair to say that this baseline forecast is the best that one could possibly expect under the given set of product-related covariates (Tichy, 2023c). We will see in Section 5 that the state-of-the-art in commercial machine learning indeed comes close to the baseline performance.

In Table 1 for the total, and for each of three selected departments (“Household 1”, “Hobbies 2”, “Foods 3”), all achieved metrics are finite when evaluated on the baseline forecast. Remember, however, that the forecast was fabricated to be ideally Poissonian for the entire assortment – by construction, it has the same quality in every subset of
data. We know beforehand the result of a meaningful analysis: It should produce the result “there is no significant difference in forecast quality between the departments”.

Hence, the following intuitive conclusions are erroneous: “We need to improve Foods 3! Fast-movers are performing worse than slow-movers!” – even though it’s corroborated by MAE and MRPS, “We must improve Hobbies 2! Slow-movers are performing worse than fast-movers!” is backed by WMAPE and NMRPS. The mere inconsistency of these conclusions is already striking and shows that the judgement of forecast quality across departments is not straightforward. The reason for the reversed conclusions is, again, the precision scaling of metrics: When increasing the selling rate, the typical values that unnormalized metrics (MAE, MRPS) achieve increase, the typical values of normalized (WMAPE, NMRPS) metrics decrease.

A direct interpretation of the forecast metrics across departments (Bower, 2023) (or any other way of segregating the data) is deeply problematic and is doomed to misalign efforts for forecast improvement. Since the “effect” that one sees is infected by precision scaling, we propose to designate such distortion as the naïve scaling trap.

3. Method: Rating metrics in a scaling-aware way

3.1. Revealing and avoiding the naïve scaling trap

The reason why the metrics in Table 1 amount to different values in the different departments lies in the scaling of the variance of the Poisson distribution with the rate \( \mu \): Each individual prediction \( r_j \) comes with a different expectation of how \( s_j \) should be distributed (Eq. (4)), and which metric value \( M(r_j, s_j) \) should be achieved on average. The widespread assumption that the same metric value is achievable for each category is therefore flawed. The value of any metric primarily reflects the unavoidable noise that also the sharpest-possible forecast exhibits. Only secondarily does the metric value reflect forecast quality.

It is therefore useful to compute the expected value of the metric, given the predictions, under the assumed sharpest possible forecast. These ideal values constitute a

| Number of predictions \( n \) | All | Foods 3 | Hobbies 2 | Household 1 |
|-------------------------------|-----|---------|-----------|-------------|
| Total sales \( \sum_j s_j \) | 1'231'764 | 564'926 | 13'302 | 222'327 |
| Total prediction \( \sum_j r_j \) | 1'236'224 | 566'248 | 13'469 | 223'244 |
| Mean sales \( \sum_j s_j/n \) | 1.44 | 2.45 | 0.32 | 1.49 |
| Bias factor \( \sum_j r_j/\sum_j s_j \) | 1.0036 | 1.0023 | 1.0126 | 1.0041 |
| MAE | 0.653 | 0.901 | 0.244 | 0.740 |
| WMAPE | 0.453 | 0.368 | 0.766 | 0.496 |
| MRPS | 0.461 | 0.633 | 0.182 | 0.520 |
| NMRPS | 0.319 | 0.258 | 0.570 | 0.348 |

Table 1: Metric values for baseline model. The level of confidence that is expressed by these metrics values without context depends on the interpretation of the forecast user.
context for the actually achieved metrics. A marginal discrepancy would confirm that
the forecast is (almost) as good as it could possibly be. A large difference motivates an
investigation whether it is possible to close that gap.

If all predictions are aggregated globally, the resulting expectation value of the metric
will be mostly borne by fast-movers. Therefore, a segregation by selling rate is useful:
We group the pairs of predictions \( r_j \) and observations \( s_j \) into buckets, the bucket of
a pair is chosen according to the prediction value \( r_j \). If one chose the outcome \( s_j \) to
define the bucket, one would suffer from a hindsight selection bias (Tichy and Feindt
2022), which leads to the forecaster’s dilemma (Lerch et al. 2017; Tichy 2023b, 2024).
Logarithmically spaced buckets are useful in retail to avoid cases in which one bucket
contains the majority of all predictions, i.e., we group by

\[
R_j = \frac{1}{n_{\text{bins}}} \text{round} \left( n_{\text{bins}} \log_{10}(r_j) \right),
\]

(10)

where we round to integers such that there are \( n_{\text{bins}} \) bins between two powers of 10.
For example, for \( n_{\text{bins}} = 4 \), the \( R_j \) that can take the values 0, 0.25, 0.5, 0.75, 1, 1.25 etc.
Buckets are referred to by their common rounded logarithmic prediction value \( R \). In
practice, we have found that \( n_{\text{bins}} = 2 \) constitutes the absolute minimum of buckets, and
choosing \( n_{\text{bins}} = 5 \) is typically a reasonable choice. The computed ratings also weakly
depend on the number of buckets, such that it should be chosen as a constant within
one analysis.

The predictions \( r_j \) that belong to the same bucket, indexed by \( R \), come with simi-
lar expectation values of any metric. Ideally, when the prediction is truly Poissonian,
we expect the achieved average metric in a bucket to match the metric’s computed ex-
pectation value. The number of bins \( n_{\text{bins}} \) should thus solve the compromise between
having sufficiently many predictions and observations in one bucket so we can expect
measurement and expectation value to match approximately (i.e. a possible deviation
to be considered significant), while ensuring that all predictions in the bucket behave
approximately equally. Figure 2 shows the result of this procedure for the baseline model
as circles and \( n_{\text{bins}} = 4 \), and superimposes the expectation value of the metrics under
the Poisson distribution as solid line. The circle sizes reflect the square-root of the num-
ber of measurements in order to make small buckets still visible. The positions of the
circles match the solid lines, confirming the Poissonian nature of our baseline forecast
from Figure 1. Figure 3 differentiates further by retail department for the NMRPS: The
differences in the metric values exhibited in Table 1 are driven by the differences in the
distribution of rates, not by a different performance. For a given rate bucket \( R \), the
performance is the same for all departments, but the different buckets have different
populations. Skepticism that calls into question the equal model performance in the
different departments, which might have been induced by the different metric values in
Table 1 can now be clearly dismissed.

The intricate shapes that metric values exhibit as a function of the predicted rate
in Figure 2 make comparisons of traditional metrics between strata of data (Bower
2023) deeply problematic. The “best case” value of a metric depends strongly on the
Figure 2: Ideal Poisson metric values (solid lines) and achieved metric values for baseline model (circles). The size of the circles reflects the square root of the number of sales grouped in one bucket.

Figure 3: Poisson-achievable NMRPS (solid lines) and achieved NMRPS (circles) for three selected departments, for the baseline model. While the overall NMRPS per department differs substantially (see Table 1), the differentiation by prediction bucket shows that the performance is equally good.
prediction itself. Faster-selling departments naturally achieve higher values of MAE and MRPS, and lower values of WMAPE and NMRPS, as exemplified by Figure 3. Therefore, statements like “we achieve 38% WMAPE on Foods 3, but 78% WMAPE on Hobbies 2, we need to improve Hobbies 2” are almost always infected by the naïve scaling trap and need to be dismissed.

3.2. Necessity for imperfect reference forecasts

The setting so far was artificial: The baseline model is ideally Poissonian, calibrated, and unsurprisingly matches the theoretical expectation. We merely confirmed that the Expectation Maximization algorithm of Appendix A works. Figure 3 explains why model performance across departments is only ostensibly different. Characterizing the ideal case is, however, not sufficient for judging forecasts in practice: One needs to quantify how “close” the model performance is to ideal, and rate this distance to conclude whether the departure from the ideal case is acceptable or problematic. For this purpose, we need to define imperfect benchmarks that set the standard for “excellent”, “good”, “OK”, “fair”, “insufficient” and “unacceptable” performance, both regarding the level of bias and the level of noise.

Perfect performance is uniquely defined, there is one way to be “right”: Given a Poisson-noise-limited situation, the best possible value of any metric is the value achieved for outcomes drawn from the Poisson distribution. For the bias (the quotient mean prediction / mean observation), the ideal value is one. For other metrics, analytic formulae are often available (see Wei and Held (2014) for WMAPE/MAE and (N)MRPS); in the worst case, one can recur to numerical computation by Monte Carlo sampling.

There are, however, many ways to be wrong: A forecast can be biased, it can be affected by noise, suffer from both, or by more complicated artefacts (e.g., finite-stock effects, zero-inflation...). No general “worst benchmark model” can be found for unbounded metrics that we are dealing with: Given a forecast, one can construct arbitrarily incompatible (“bad”) observations; given a set of observations, one can construct arbitrarily terrible forecasts. A universal definition of “excellent” to “unacceptable” models that would fit all industries is unlikely to be ever possible (Kolassa, 2008). We therefore expose the industry-specificity by few parameters within a simple model for imperfect reference forecasts. As a “bad” reference forecast, we use a collective global forecast which does not individualize at all, but produces the same value for all products, locations and days.

3.3. Scaling of imperfect reference forecasts

Given predictions of \( r_j \approx 10 \) (\( R = 1 \)), we ideally expect the histogram of observations to match the Poisson distribution with rate 10. To simplify judging the deviation from that ideal, we use negative-binomial distributions of different variances to set reference ratings, with examples in Figure 4. That is, we assume that the forecaster produces a Poisson-forecast, but that the observations are generated by a negative-binomial process, such that they observe an over-dispersed distribution of outcomes. We assign the variances at prediction \( r_j = 10 \) and the overall bias factors (mean prediction / mean observation) a qualitative score:
Figure 4: Over-dispersed distributions that define the reference forecasts “perfect” (Poissonian, blue), “excellent” (light green), “good” (dark green), etc., for a predicted rate $\mu = 10$. When the forecaster predicts a Poisson-distributed target, we rate their performance with respect to a metric by referring to the metric values achieved by the respective negative binomial distributions shown in the figure.

| Rating       | Perfect | Excellent | Good | OK | Fair | Insufficient | Unacceptable |
|--------------|---------|-----------|------|----|------|--------------|--------------|
| Variance at $r_j = 10$ | 10      | 18        | 26   | 37 | 48   | 73           | 136          |
| Bias factor  | 1.0     | 1.015     | 1.03 | 1.07 | 1.2  | 2           | 4            |

The ratings for bias and for variance are independent, that is, a forecast can enjoy Poisson-like variance, but be quite biased, or vice versa. The 12 parameters fully define our rating, they were chosen such that a collective global forecast for the M5 competition data is rated “Insufficient” by most metrics.

For a given metric $M$, we need to compute reference values $M^{(R)}_{\text{quality}}$ for each quality (“Excellent”, “Good” etc.), and for each bucket of predictions $\bar{r}^{(R)}$. The achieved value $M^{(R)}_{\text{actual}} = M(\bar{r}^{(R)}, \bar{s}^{(R)})$ can then be set into that context to rate each individual bucket. When moving to smaller or larger predictions, the question arises how the variance scales: Having established that for rate 10, a variance of around 30 is judged as “good”, which variance is “good” when the prediction is 1, 100, or 10,000? This problem is illustrated in Figure 5, where the distribution of observations for a “good” forecast of 10 is shown on the upper left panel in red and compared to the blue Poisson distribution.

Super-Poissonian variance can scale with the rate in different ways. We parametrize the variance as function of the rate $\mu$ as follows:

$$\text{variance} = \mu + f\mu^\gamma,$$

where $f$ quantifies the strength of the noise. Setting $\gamma = 2$ corresponds to quadratic over-dispersion (a Negative Binomial-2-process), and $\gamma = 1$ to linear over-dispersion (a Negative Binomial-1-process) (Hilbe, 2012; Cameron and Trivedi, 2005). In general, the variance $\sigma^2_\mu$ of the underlying distribution of the rate $\mu$, here assumed to be a $\Gamma$-
distribution, is inherited by the variance of the resulting count process via \( \text{variance} = \mu + \sigma^2 \) (Karlis and Xekalaki, 2005). The upper right panel of Figure 5 shows the NMRPS as a function of the predicted rate, for different exponents \( \gamma = 1, 1.5, 2 \). Quadratic \( \gamma = 2 \) is unrealistic as a candidate for "equal quality": It is too benevolent for large rates and becomes too strict for small rates. \( \gamma = 1 \) exhibits the opposite behavior: It judges large rates strictly and small rates too benevolently. The intermediate value \( \gamma = 1.5 \) provides a reasonable compromise, corroborated by this argument: When summing negative binomial-distributed random numbers, \( \gamma = 1 \) corresponds to uncorrelated noise, \( \gamma = 2 \) to perfectly correlated noise, and \( \gamma = 1.5 \) to partial correlation, which is realistic in the retail setting: The number of visitors of a supermarket has some uncertainty that leads to correlated, product-independent noise on the demand, but also each individual product suffers from uncorrelated, product-dependent noise.

The resulting rate-dependent reference values for the exponent \( \gamma = 1.5 \) are shown in Figure 6. For a given prediction bucket, one reads off what the achieved metric means.
in terms of model quality (“excellent”, “good”, etc.) by locating the bucket in the respective plots. By assigning “perfect” the numerical score 100% and “unacceptable” the score 0%, and interpolating between reference lines, each bucket gets assigned a numerical score $S(R)$. We will see below that empirical real-life forecasts match the behavior described by $\gamma = 1.5$ very well.

A given metric value, e.g., WMAPE=0.6, can be considered “perfect” for a predicted rate of 2, or “unacceptable” for a predicted rate of 100. By applying the method to different sets of data from different industries, we have experienced that “large” buckets (containing many, more than 100, predictions and observations) that are found outside the “insufficient” region are indeed rare and point the user to severe model or data quality issues. The significance of “small” buckets (containing less than about 100 predictions and observations) is, however, questionable: Each metric inherits the randomness of the process, and will therefore be distributed around its expectation value. An unexpected metric value that a small bucket assumes is not necessarily significant, i.e. it can also be due to chance.
3.4. Summarizing bucket-wise to overall ratings

It is desirable to summarize the information contained in a set of buckets by a single number. There are two possibilities to perform this summary: One can aggregate the ratings per bucket via taking the mean of the bucket-wise scores $S^{(R)}(\text{weighted by the number of observations per bucket})$, which yields an overall score $S_{\text{overall}}$ between 0% (all buckets are “unacceptable”) and 100% (all buckets are “perfect”). Alternatively, one can judge the overall aggregated metric, e.g., the overall achieved WMAPE, by setting it into the context of the metric that would have been achieved if the model quality were “excellent” to “good”. In other words, one can compute the expectation value of the metric under reference models of imperfect quality, answering the important question “what values of the metric could I possibly achieve?”, which we had put forward above in Section [1.1].

3.5. Ostensibly better-than-Poisson measurements

One will sometimes find buckets $R$ that achieve a “better-than-Poisson” metric, $M^{(R)} < M^{(R)\text{ perfect}}$. This unexpected behavior can be due to different causes:

- Insufficient statistics: The significance of such “better-than-Poisson”-measurement is jeopardized by a too small number of prediction-observation-pairs in a given bucket.

- Sub-Poissonian process: The Poisson assumption might be too pessimistic.

- Finite capacity: If stocks are finite, not all demand peaks are reflected by corresponding sales values, but censored. Fewer events with $s_j \gg r_j$ will then be observed than were predicted, and, consequently, metrics such as MAE, WMAPE will achieve lower values.

- Systematic over- or under-prediction. When a bucket is systematically over- or under-predicted ($r^{(R)\text{ total}} > s^{(R)\text{ total}}$ or $r^{(R)\text{ total}} < s^{(R)\text{ total}}$), the resulting position in the rate-KPI-diagram will be shifted to the left or right, possibly ending at a “better than Poisson” position. A good noise rating for a bucket should only be taken as a sign that the forecast is good if the bucket is also unbiased.

- Overfitting. When rating a model in-sample, a sub-Poissonian performance can witness overfitting. E.g., trivially setting $r_j = s_j$ gives an implausible WMAPE of 0.

Depending on which of these non-exclusive causes are realistic in a given application, one can decide to rate buckets for which $M^{(R)} < M^{(R)\text{ perfect}}$ with some critical quality to ensure that these buckets do not push the overall rating to good values, obfuscating some problems. The method of rating the overall aggregated metric is especially prone to cancellation between smaller-than-ideal and larger-than-ideal values, which would result in an overall “excellent”-looking metric. Just like negative and positive bias in slow- and fast-movers may cancel to yield an ostensibly good global bias, “better-than-Poisson” and “worse-than-Poisson” buckets may result in an ostensibly good overall noise. Therefore, we prefer to aggregate the bucket-wise-scores as a primary tool of investigation.
4. Application to M5-competition-models

As a first application, we apply our scaling-aware rating to several models for the validation period of the M5 competition (Makridakis et al., 2022a, 2022b; kaggle, 2022a).

- **Baseline.** The baseline model described in Section 2.6 (details in Appendix A) is calibrated and maximally (Poisson) sharp.

- **Naïve-1-day-ahead.** A mediocre model uses a one-day-ahead heuristic that takes yesterday’s observed sales value of every product-location combination as prediction for today’s sales, effectively a one-day-horizon.

- **Simple-28-day.** This simple model (kaggle, 2022c) uses the last 28 days and averages the sales per weekday for each product. It predicts on horizon one (first day of the validation period) to horizon 28 (last day of the validation period).

- **Global forecast.** An undifferentiated collective forecast produces the overall average of the Simple-28-day-model for every SKU and every day.

- **LightGBM.** The LightGBM model (kaggle, 2022b) implements a tree-based learning algorithm within the gradient boosting framework (Ke et al., 2017). It was trained with a Poisson objective and RMSE as a metric, and uses some additionally engineered features such as lagged sales. It predicts on horizon one to horizon 28, and scored an above-average performance on the public leaderboard.

To rate the models, we follow the procedure described above in Section 3. We bucket predictions by their value, and plot the bias and the NMRPS for each bucket in Figure 7. We clip small predicted rates at 0.01 (which gives predictions with \( r_j < 0.01 \) an advantage because they are under-predicting, on average), and we clip the bias factor at 10 and 1/10 (giving the otherwise even more heavily biased prediction buckets an advantage) to fit into the visual representation. The plots allow us to judge the models and discuss their strengths and weaknesses.

4.1. Model comparison: Rate-stratified representation

In the bias scatter plot in the upper panel of Figure 7, the baseline (blue) is almost perfect by construction, and LightGBM (orange) exhibits a good level of bias for moderate predicted rates of 1 and above. For smaller rates \( r < 0.1 \), LightGBM-predictions are typically under-predictions, witnessing overfitting. The Naïve-1-day-ahead prediction (green) contains many 0-predictions (which were clipped to 0.01), which are often under-forecasts: When a product is not sold on a given day, the naïve-1-day-ahead-prediction for the next day is zero, but the expected value of its sales for the next day is larger than zero. This regression-to-the-mean also manifests in forecasts that follow days with abnormally many sales, these are then typically over-predictions. The green circles consequently all lie above the ideal line for predictions larger than 1 (the overall average daily selling rate is about 1.4). The Simple-28-day-model is not as unbiased as LightGBM, but it is still less biased than the naïve-1-day-ahead model. The global
model produces 1.39 for every SKU, a slight under-forecasting, since the overall mean sales are about 1.44.

The stratified noise plot in the lower panel of Figure 7 clearly separates the five models: The baseline fits the Poisson-ideal perfect line (reproducing Figure 2). For every prediction bucket, LightGBM is more noisy, rated in the “good” range. The Simple-28-day model is, again, slightly worse, populating the “OK” area. The scattered circles follow the parametrized corridors with $\gamma = 1.5$ remarkably well – any departure of these corridors is, thus, a forecasting abnormality that should be investigated. The naïve-1-day-ahead model follows with some distance to the others on the boundary to “insufficient”. The largest two prediction buckets of that model leave the bulk – the regression to the mean is especially pronounced for the largest predictions, which correspond to the largest observations on the day before. Not surprisingly, the collective global model exhibits a large degree of noise, close to “unacceptable”.

Even though NMRPS is superior to WMAPE in its handling of slowly moving predictions, it nevertheless does not allow us to differentiate between models in the ultra-slow-regime well, where all the lines converge. It remains imperative to always consider bias and noise together.

We summarize Figure 7 by the following scaling-aware ratings (computed following Subsection 3.4):

| Model              | $S_{overall}$ (NMRPS) | Quality | $S_{overall}$ (Bias) | Quality | Bias factor |
|--------------------|-----------------------|---------|----------------------|---------|-------------|
| Baseline           | 99.9 %                | Perfect | 98.2 %               | Perfect | 0.9937      |
| LightGBM           | 66.4 %                | Good    | 87.6 %               | Excellent | 0.9904      |
| Simple-28-day      | 57.4 %                | OK      | 53.1 %               | OK      | 0.9626      |
| Global             | 16.4 %                | Insufficient | 71.3 %       | Good     | 0.9626      |
| Naïve-1-day-ahead  | 41.0%                 | Fair    | 36.9 %               | Fair    | 1.0001      |

With Figure 7 and the resulting ratings at hand, one gains the confidence that the LightGBM-model is consistently better than Simple-28-days, across all prediction buckets and in a stable fashion. The rating of bias per bucket is more stringent than the overall bias alone: The Naïve-1-day-ahead method is, by construction, overall unbiased (it uses the sales of the last day before and of the first 27 days of the validation period), but only the bucket with $R \approx 1$ is unbiased, resulting in a “Fair” overall bias performance. The “insufficient” rating of the global collective model reflects its lack of any individualisation among SKUs and days.
Figure 7: Prediction-bucketed representation of bias (upper panel) and noise (NMRPS, lower panel) for five exemplary models. The size of the circles reflects the total number of sales contained in the respective buckets.
4.2. Dataset comparison

Scaling-aware model rating adds more details to model comparison, but often reproduces the verdict of scaling-unaware metrics. For dataset comparison, the added value of scaling-aware rating is more evident: Here, the conclusion $M_A < M_B \Rightarrow “A$ is better than $B” must not be drawn, because it is almost always affected by precision scaling. Scaling-aware rating then helps identify those data-subsets that require special attention, avoiding the naïve scaling trap.

The bucket scatter plot is shown for LightGBM for three selected departments in Figure 8. The following table summarizes the achieved overall ratings, which complements the metrics displayed in Figure 9:

| Department | $S_{\text{overall}}$(NMRPS) | Quality (NMRPS) | $S_{\text{overall}}$(Bias) | Quality (Bias) | Bias factor |
|------------|----------------------------|----------------|--------------------------|----------------|-------------|
| Foods 3    | 64.9%                      | Good           | 91.3%                    | Excellent      | 1.0         |
| Household 1| 75.6%                      | Excellent      | 83.7%                    | Excellent      | 1.0249      |
| Hobbies 2  | 63.5%                      | Good           | 46.3%                    | OK             | 0.8553      |

The comparison of unscaled metrics and scaling-aware ratings per department allows us to appreciate the added insight from the scaling-aware approach: From the WMAPE, MAE, (N)MRPS alone (displayed in Fig. 9), one cannot make any statement about which department is performing better or worse – context is required. Using the rating, and from the lower panel of Figure 8, we see that “Household 1” has less noise than “Foods 3”, and both these departments perform better than “Hobbies 2”. “Foods 3” enjoys a lack of overall bias (quotient 1.0) and an excellent bias rating (91.3%), reflecting the lack of substantial bias in every bucket. The largest predictions in “Household 1” are clearly deteriorated in both the bias and the noise plots. In “Foods 3”, only the small predictions ($r < 0.3$) stand out as being under-forecasting, a general minor mis-calibration of the LightGBM model (see upper panel of Figure 7).

The systematic negative bias in “Hobbies 2” is reflected by the mediocre bias score (46.3%), while the rating on NMRPS is benevolent (63.5%) – a systematic negative bias shifts the reference values for noise to larger values. For the overall KPIs, the reference values of normalized metrics (WMAPE and NMRPS) are rather benevolent (such that the achieved KPIs lie in the “Fair” and “OK” range, respectively), while absolute metrics (MAE and MRPS) are more strict (the KPI ends up in the “unacceptable” and “insufficient” range, respectively). When bias is negligible, the ratings of the different departments speak a clear message: “Household 1” performs better than “Foods 3”, while the scaling-unaware MAE, MRPS, WMAPE, NMRPS only provide inconclusive, mixed messages. The contextualization achieved by adding the expected metric values for the different qualities reproduces this message in Figure 9: “Foods 3” finds itself in the “good” range, “Household 1” reaches the edge to “excellent”. Clear operational conclusions follow: One should focus on “Hobbies 2”, then improve “Foods 3” and finally tackle the outliers in “Household 1”.

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Figure 8: Prediction-bucketed representation of bias (upper panel) and noise (NMRPS, lower panel), for three exemplary departments (LightGBM model). Circle sizes reflect the square root of the total number of sales contained in the respective buckets.
Figure 9: Overall achieved KPIs for the three departments, and reference KPI values for “excellent”, “good” etc. Due to different selling velocities in the three departments, the reference values differ. An interpretation of the scaling-unaware metrics is misleading, the context added by the reference values is essential. The WMAPE of 57% in “Foods 3” can be considered worse than the WMAPE of 66% in “Household 1”, which is aligned with the visual impression from the scatter plot in the lower panel of Figure 8 as well as with the achieved $S_{overall}(NMRPS)$ (Subsection 4.2).
5. Application to Demand Edge for Retail predictions at Sainsbury’s

The M5 dataset discussion in the previous section has the great advantage to be reproducible by anyone. On the other hand, one might argue that the setup is artificial, since one deals with a curated dataset that does not contain all data that are used in production (e.g. the price of the product). To complement our discussion and prove the applicability of our method in practice, we apply the scaling-aware rating to the predictions of the Demand Edge for Retail forecasts produced by Blue Yonder for the UK retailer Sainsbury’s. Demand Edge for Retail is based on a causal machine-learning model that computes the contribution of features such as price, weather, day-of-the-week, holiday, promotion and many others to the expected demand of a product (Wick et al., 2019, 2021). It produces a forecast that consists of a prediction for the expected mean as well as for the variance, technical details can be found in (Wick [a,b,c]). That is, the prediction itself estimates the super-Poissonian uncertainty. To simplify the discussion and make it comparable to the M5 dataset discussion, we only use the predicted mean, and interpret it as predicted mean of the Poisson distribution.

We analyse the next-day-forecasts (horizon 1) for Monday, October 2nd to Saturday, October 28th, 2023, excluding Sundays. We exclude items that are not sold in units of pieces but volume (liters) or mass (KGs), predictions that are smaller than 0.1 / product-location-day, and unsold product-day-location combinations for which the prediction was 20 or larger. The latter filter is motivated as follows: A Poisson prediction of \( \mu = 20 \) forecasts the probability to sell 0 items to be about \( 2 \cdot 10^{-9} \). Thus, even if the model were grossly overestimating, a sales value of 0 is absolutely unexpected and clearly indicates an out-of-stock situation. After data cleaning, we remain with a total of more than 250 million product-location-day combinations, corresponding to more than 500 million sold items. The global over-prediction of the model is less than 1%.

Figure 10 shows the bucketed scatter plots for bias and noise for the Sainsbury’s dataset. It confirms the calibration of the forecast (outside the ultra-slow-movers), and the only slightly super-Poissonian noise over three orders of magnitude.

The direct comparison of scaling-unaware metrics would not be meaningful (Kolassa, 2008), but we can compare Figures 7 and 10 to perform a hybrid comparison: We see how the Demand Edge for Retail model outperforms the M5 competition models in the sense that, for a given rate bucket, it produces lower values in NMRPS. This is also reflected by the excellent values \( S_{overall}^{NMRPS} = 84.7\% \) and \( S_{overall}^{Bias} = 78.7\% \) on the Sainsbury’s dataset.

Our approach hence confirms that, on a global level, the prediction is comparable to what could possibly be expected from first principles (Tichy 2023c), as discussed in Section 2.1. A more in-depth investigation confirms that the reasons for non-Poissonian behavior described in Section 2.2 above are indeed relevant in this scenario (there are, for example, substantial “multi-buy” events when customers buy several pieces of one item). A thorough quantitative understanding of the mild departure from the Poisson ideal case based on those effects would be highly appreciated, but remains beyond the scope of the current work.

As a more in-depth analysis, one can segregate data by different dimensions and ask...
questions as the following: Are certain stores predicted better than others? Are certain product groups predicted better than others? Are certain days or weekdays predicted better than others? All of these comparisons risk to induce the naïve scaling-trap, which is resolved by our approach. As an instructive example, we can differentiate by the highest-level product group and focus on the three largest assortments (Fresh, Ambient, Non foods). The resulting metrics are shown in Table 2.

Similarly to our artificial example earlier in Section 2.6, the naïve scaling trap is lurking. Large differences between the three product groups could lead an analyst to conclude a dramatically different forecasting performance. More insight can be gained via the prediction-bucketed representation in Figure 11. The upper panel reflects the low bias per prediction bucket (although predicted slow movers are slightly underestimated). The lower panel confirms that the differences in forecast quality between the product groups is small, the difference of achieved KPIs in Table 2 is mostly borne by the different selling velocities (Fresh sells at about twice the mean velocity, Non foods sells at about one third of the mean velocity). When asking for improvement potential, it is worth noting that Fresh is performing slightly better than Ambient and Non foods in the slow- to mid-mover regime (mean prediction between 1/day to 10/day). The outliers in Ambient for fast movers (mean prediction >100/day) are worth investigating, but constitute only a tiny fraction of Ambient product-location-days and sales. Since the circle size scales with the square-root of the number of measurements, small buckets are visually over-emphasized.

The bucketed representation is summarized by the following table, which contains the ratings regarding NMRPS and bias, using the same parameters as for the M5 competition to make the numbers comparable (Blue Yonder uses internally different sets of industry-specific parameters to define the quality grades):

| Product group     | $S_{overall}$(NMRPS) | Quality (NMRPS) | $S_{overall}$(Bias) | Quality (Bias) |
|-------------------|----------------------|-----------------|---------------------|-----------------|
| Fresh             | 88.7%                | Excellent       | 84.3%               | Excellent       |
| Ambient           | 80.2%                | Excellent       | 72.5%               | Good            |
| Non Foods         | 79.3%                | Excellent       | 64.4%               | Good            |

Table 2: Metrics for Blue Yonder’s Demand Edge for Retail forecasts at Sainsbury’s. Due to non-disclosure reasons, we neither show the absolute total sales nor the sales per item-location-day, but state the sales per product-location-day normalized to the overall mean velocity. Similarly to Table 1, the scaling-unaware metrics lead to ambiguous interpretations: Judging from the MAE and MRPS, one would focus to improve the Fresh assortment, based on WMAPE and NMRPS, one would investigate Non foods.
In practice, applying scaling-aware forecast rating at Sainsbury’s has resolved many ambiguous situations in which stakeholders were unsure whether certain achieved KPIs were expected or out of range. When introducing model changes, the prediction-bucketed representation was handy to give confidence that changes have a global positive impact, across all velocities. For the forecast provider Blue Yonder, adhering to a clear methodology on how to judge models has streamlined and improved support capabilities. For the forecast consumer Sainsbury’s, the scaling-aware analyses have helped tackling data quality problems in a focused manner to improve the forecast, and given the visibility and confidence of how it is performing.

In conclusion, applying scaling-aware rating on different data-subsets such as departments, locations, days-of-the week helps focus development efforts on those specific forecasts that exhibit the largest gap to the Poisson ideal case. These are often *not* those that exhibit extremal values of scaling-unaware metrics! Scaling-aware rating accounts for the most important influence on metric values, the precision scaling of the Poisson distribution. It thereby avoids the naïve scaling trap so the user can focus on actual quality differences between models and between datasets.
Figure 10: Prediction-bucketed representation of bias (upper panel) and noise (NMRPS, lower panel), for Demand Edge for Retail predictions by Blue Yonder for UK retailer Sainsbury’s. Circle sizes reflect the square-root of total number of sales contained in the respective buckets.
Figure 11: Prediction-bucketed representation of bias (upper panel) and noise (NMRPS, lower panel), for Demand Edge for Retail predictions by Blue Yonder for UK retailer Sainsbury’s, differentiated by the highest-level product group. Circle sizes reflect the square-root of total number of sales contained in the respective buckets.
6. Conclusions and outlook

Applied forecast judgement methodology should reconcile prevailing business practice (Carvallo, 2021; Bower, 2023) with the state of the art in probabilistic forecasting (Czado et al., 2009; Gneiting, 2011; Kolassa, 2016; Wei and Held, 2014; Gneiting and Katzfuss, 2014). Scaling-aware forecast rating provides a framework to tackle the problem of benchmarking and precision scaling in a systematic way. The counting statistics – the scaling of the Poisson distribution and of the noise of forecast quality references – can be modelled universally. This allows to separate these general effects and natural bounds from industry-specific aspects, which are captured by the parameters that define “excellent” to “unacceptable”. By adapting the parametrization introduced in Section 3.3, one can apply scaling-aware forecast rating to different forecasting problems, e.g. to industries in which bad data quality is common or certain influencing factors are known to be uncontrollable.

6.1. Applicability

Given a forecast, our method permits users to judge the status quo and whether desired improvements in accuracy are realistic at all: If forecast performance is close to the ideal Poisson prediction (or another distribution known to be the sharpest possible for the task at hand), the ask for a substantial improvement needs to be refused. If a gap to the Poissonian case emerges, the reason for this gap needs to be understood. Forecast creators should then elaborate whether it is an unrealistic expectation – the assumed “ideal” case is unreachable due to reasons of forecastability, or whether the forecast can actually be improved.

When several models compete, using scaling-aware methods makes their comparison more transparent and insightful: A given improvement of a metric can be understood holistically by analysing the respective bucket scatter plot as in Figures 7, 8, 10, 11. Two competing models can be placed onto a meaningful scale from “perfect” to “unacceptable”, providing context and orientation whether a certain improvement is substantial and solid, or little more than a mere coincidence.

The greatest added value of our method lies in dataset comparison: A comparison of the values of scaling-unaware metrics such as MAE, WMAPE or (N)MRPS for two sub-datasets of the same model is useless, since the naïve scaling trap almost always snaps. Comparing scaling-aware ratings for different departments, locations, or promotion types instead allows users to focus development efforts on those data subsets for which the gap to the ideal behavior is most pronounced. For example, when monitoring forecasting performance in time, usual metrics will change due to increasing or decreasing overall volume. Thresholds on scaling-unaware metrics (“if WMAPE increases above 70%, trigger an alarm!”) are prone to false positives and false negatives, which scaling-aware ratings remedy.

6.2. Future venues

Our proposed method – computing expectation values of metrics, and bucketing by similar predictions – can be applied to all probabilistic forecasts for which one can
characterize the ideal case. Within the application in retail, several generalizations are thinkable: In a business context, it is advisable to judge the forecast by its actual economic impact (Syntetos et al., 2010), which can be rated in the context of what could be possibly achieved. A certain out-of-stock or waste loss, expressed in currency, can then be given a rating (“perfect”, “excellent”, etc.). The visibility of the financial impact of forecasting error, together with the knowledge about what could be achieved if the forecast were improved to a certain quality level, will help allocate resources efficiently. The question naturally emerges to which extent human overrides of forecasts (Khosrowabadi et al., 2022) intuitively incorporate precision scaling.

The exponent $\gamma = 1.5$ in the parametrization in Eq. (11) works remarkably well and quite universally, for many different applications and industries (compare the M5-analysis to the Sainsbury’s analysis – models on both datasets follow the shape of the reference lines very well, across several orders of magnitude). Nevertheless, a better theoretical explanation or empirical corroboration of that value would be appreciated. Our hope is fuelled by the fact that scaling laws often reflect underlying system structures (West, 2018; Athanasopoulos and Kourentzes, 2022).

As a simple (one could even say: simplistic) first-order approximation to a benchmark, we used the Poisson distribution. For a second-order approximation, the distribution should be corrected for multi-buys (a single customer buys several items of the same product), possible finite capacity and other effects. The gap between empirical data and Poisson ideal in the bucketed representation of the Sainsbury’s dataset is, to a great extent, borne by multi-buy-effects.

For non-individualized retail, it is straightforward to identify the Poisson distribution as an ideal, maximally sharp distribution (Tichy, 2023c). For other processes, it might be much more involved to work out the sharpest possible forecast. Also, it is not guaranteed that the segregation by buckets of similar predicted means is sufficient: More in general, one will need to bucket in a way that “similar forecasts” (in terms of the distributions that they describe) are grouped and evaluated together. The size and number of the buckets should then ensure a good balance between precision (many small, homogenous buckets) and sufficient statistics per bucket (few large more inhomogeneous buckets). The general idea put forward in this article, segregating a large and heterogeneous group of forecasts into homogeneous buckets, is independent of the Poisson distribution.

On the conceptual side, it is worthwhile connecting the present work with the concepts of aleatoric and epistemic uncertainty (Hüllermeier and Waegeman, 2021): Any error metric value is the sum of an aleatoric, unavoidable error, and of an epistemic error, which may be decreased by incorporating further knowledge into the model. Using the Poisson distribution as baseline essentially states that the Poisson process describes the aleatoric part, while the possibly not perfectly known rate of that Poisson process is subject to epistemic uncertainty.

A forecast judgement summarizes a vast number of predictions and observations into a few summary statistics. Without having set a rigorous benchmark, achieved metrics lack context and evade interpretability. Scaling-aware forecast rating, applied on a bias- and on a noise-related metric, reduces the likelihood of such paradoxical situations by
setting a context and avoiding the na"ıve scaling trap. We hope that it will help users handle and judge forecasts in practice, in a statistically corroborated way.

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Declaration of competing interest

Blue Yonder has filed patent applications covering the intellectual property associated with this paper.

Appendix A. Heuristic algorithm for non-parametric Expectation Maximization

For the purpose of discussing our rating methodology on “perfect” Poissonian forecasts for a given dataset of sales values \(s_j\), we construct predictions using the following heuristic, which is equivalent to Expectation Maximization: Given the set of observed sales \(s_j\), we assume that these integer outcomes are the results of Poisson processes with rates \(t_j\), i.e. that Eq. (4) holds. Given the known observations \(s_j\), we need to evaluate \(P(t|s)\), i.e. the conditional probability to have predicted \(t\) given that we do observe \(s\), to create plausible predictions \(t_j\) (Tichy, 2023b). Remember that Eq. (4) states the conditional probability to observe \(s\), given that \(t\) is predicted. Bayes’ rule gives

\[
P(t|s) = \frac{P_{\text{Poi}}(s|t) P_{\text{rate}}(t)}{P_{\text{observation}}(s)},
\]

where \(P_{\text{Poi}}(s|t) = \frac{e^{-t/s} t^s}{s^s!}\) is the Poisson distribution. The probability of a certain outcome \(P_{\text{observation}}(s)\) can be easily retrieved from the dataset (it is the histogram of sales values). To evaluate \(P(t|s)\), we need an approximation to \(P_{\text{rate}}(t)\), the prior probability density function that describes the distribution of the rates.

We approximate this prior by starting with a first guess, an exponential distribution,

\[
P_0(t) = \frac{1}{\langle s \rangle} e^{-\frac{t}{\langle s \rangle}},
\]

where \(\langle s \rangle\) is the mean observed sales

\[
\langle s \rangle = \sum_{s=0}^{\infty} s P_{\text{observation}}(s).
\]
We denote by \( P^{(k)}_{\text{observation}}(s) \) the resulting probability to observe \( s \), given the prior for the rates \( P_k(t) \), i.e.

\[
P^{(k)}_{\text{observation}}(s) = \int_{t=0}^{\infty} P_k(t) P_{\text{Poi}}(s|t),
\]

where the index \( k \) will denote the iterations of the algorithm. For the first guess and before the first iteration, \( P^{(0)}_{\text{observation}}(s) \) and \( P_{\text{observation}}(s) \) typically differ substantially.

We apply the following update rule to improve the probability density function \( P_k(t) \) iteratively:

\[
\tilde{P}_{k+1}(t) = P_k(t) \sum_{s=0}^{\infty} P_{\text{Poi}}(s|t) \frac{P^{(k)}_{\text{observation}}(s)}{P^{(k)}_{\text{observation}}(s)},
\]

where, intuitively speaking, we boost the probability density that contributes to those observations that are currently under-predicted. The updated probability distribution \( \tilde{P}_{k+1}(t) \) is unnormalized, such that we perform the simple normalization

\[
P_{k+1}(t) = \frac{\tilde{P}_{k+1}(t)}{\int_{x=0}^{\infty} dx \tilde{P}_{k+1}(x)},
\]

before the update rule (A.5) is applied again.

To provide a set of plausible predictions \( t_j \), we sample one prediction \( t_j \) for each observation \( s_j \) via \( t_j \sim P(t|s_j) \). Applying the iterative procedure on all data categories (product groups, locations...) separately, we obtain unbiased Poissonian forecasts that could have yielded the set of sales \( s_j \). We have experienced that a dozen iterations is typically sufficient to reach a good artificial forecast, i.e. a forecast for which \( P^{(k)}_{\text{observation}}(s) \approx P_{\text{observation}}(s) \) while, for each prediction \( t \), the resulting sales \( s \) are Poisson-distributed, as shown in Figure 1. In practice, we discretize the space of rates \( t \) into a granular array of length \( G = 5'000, t_0 \ldots t_G \), such that (A.5) is performed on all \( P_k(t_0) \ldots P_k(t_G) \) in a numerical fashion.

Appendix A.1. Treatment of improbable observations

In the update rule (A.5), the cases \( P^{(k)}_{\text{observation}}(s) = 0 \) and/or \( P_{\text{observation}}(s) = 0 \) need special attention, especially in practice, when one recurs to numerical discretization of the space of rates \( t \).

- When \( P^{(k)}_{\text{observation}}(s) = P_{\text{observation}}(s) = 0 \), we set the quotient of \( P_{\text{observation}}(0) \) and \( P^{(k)}_{\text{observation}}(0) \) to 1: The probability 0 for the observation \( s \) predicted by \( P^{(k)}_{\text{observation}}(s) \) matches the empirical frequency \( P_{\text{observation}}(s) \), no adjustment is needed.

- When \( P_{\text{observation}}(s) = 0 \) but \( P^{(k)}_{\text{observation}}(s) > 0 \), the quotient is kept at 0: The empirical frequency is 0, hence, the components that contribute to that observation can be decreased.
• When $P_{\text{observation}}(s) > 0$ but $P_{\text{observation}}^{(k)}(s) = 0$ (or very close to 0), we need to “boost” components that contribute to that observation $s$, hence we set the otherwise undefined (or very large) quotient to 2.

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