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Frandsen et al. provide insights to PubMed coverage across all Cochrane Review Groups, but more in-depth analyses would further help inform database choices

We congratulate Frandsen et al. [1] on their contribution to the evidence base on PubMed coverage across systematic review topics, especially with regard to their comprehensive sample. Their study provides further evidence that PubMed coverage is generally high, but variable across topics, defined by the scope of Cochrane Review Groups (CRGs).

Frandsen's analyses of included studies and associated publications present a useful estimate of a CRG's average PubMed coverage as well as its variability across reviews. CRGs with an average PubMed coverage of 90%, whose variability across reviews is small (hypertension, heart, breast cancer, neonatal, HIV/AIDS, EPOC, epilepsy, oral health, tobacco addiction, metabolic and endocrine disorders, childhood cancer, and multiple sclerosis), are shown to be retrieving a high proportion of their studies in PubMed. Thus, for these CRGs, searching more bibliographic databases than those mandated by Methodological Expectations of Cochrane Intervention Reviews standards [2] would appear to have limited impact on the results of their systematic reviews.

Unfortunately, Frandsen et al.'s dataset is not provided as supplementary material. It would be valuable to have descriptive information about the dataset, including the total number of reviews analyzed, as well as information on the average number of reviews per CRG and studies per review. These data would facilitate conclusions on the applicability to reviews with a low or high number of included studies.

For information specialists and systematic reviewers to make appropriate database choices, further analyses are required according to the following categories:

1. Type of intervention

One CRG explored differences according to the type of intervention and found that complementary and alternative medicine as well as dietary supplements were not covered as well in PubMed as other interventions [3]. In another analysis across all CRGs, environmental and health policy interventions showed to be covered less in PubMed compared with clinical and pharmaceutical interventions [4]. Therefore, analyses according to the type of intervention are warranted.

2. Review type

According to our own search of the Cochrane Library, the dataset used by Frandsen et al. potentially comprises 3,699 reviews, of which 3,600 are intervention reviews. Hence, their results primarily apply to reviews of interventions rather than other review types, such as prognosis, diagnosis, qualitative, or methodology reviews. Specifying analyzed review types, as done by Hallday et al. [5], is recommended.

3. Study design

PubMed coverage across CRGs has been investigated in relation to nonrandomized studies [4]. Before and after studies and interrupted time series were found to have lower PubMed coverage compared with cohort and case−control studies. Given this known variability in coverage for some nonrandomized studies, we also recommend that future analyses account for study designs, as done by Hartling et al., who focused on RCTs [6].

In summary, we are pleased to see the evidence base on PubMed coverage per CRG enhanced by Frandsen et al. Nonetheless, we encourage the authors and other information specialists to conduct further in-depth analyses according to the intervention and review type, as well as study design, to support evidence-based and appropriate database selection.

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Statistical thinking, machine learning

We read with interest *"A systematic review shows no performance benefit of machine learning over logistic regression for clinical prediction models"* by Christodoulou et al. [1] appreciating its rigor and importance. We question, however, the conceptual dichotomy of logistic regression (LR) vs. machine learning (ML).

Liberally interpreting Breiman's and Mitchell's works [2,3] among others [4], Christodoulou et al. posit that ML differs from regression in that ML lacks underpinning theory or prior domain knowledge, yet acknowledging the difference more as of a continuum than a clear cut [5].

LR exploits a Bernoulli-distributed outcome as a linear function of predictors through the log-odds, with coefficients estimated by maximum likelihood and Newton's method. Incorporated random or other mixed effects can account for data hierarchies. Variations of LR, from probit to Bayesian LR, historically are not regarded as ML [6]. In LR features are usually chosen by content experts based on empirical knowledge or theory, subject to sample size and event rates. In the presence of collinearity, or when theory is vaguer, techniques for selecting variables can be used, for example, regularization. To some extent, feature selection already falls in the ML realm.

Many ML techniques are not much different to LR other than in name and model fit routines. For instance, a support vector machine (SVM) with linear kernel is similar to LR, minimizing the hinge instead of logistic loss [7]. A naïve Bayes classifier can be considered a particular case of LR [8]. In an artificial neural network, a single-layer neuron with sigmoid activation is also LR [9].

We agree with the authors that theory-driven modeling differs from abductive data-driven discovery [10]. However, in the LR/ML definition given by Christodoulou et al. it is debatable that regularization and boosted/bagged LR are included in the LR family, whereas generalized estimating equations are kept separate, as well as linear/nonlinear SVM approaches are pooled together. Furthermore, optimization methods such as genetic algorithms seem to be mixed up with prediction.

We also think that study design should be more of a defining element. Differences in study design can be instrumental in determining *why* and *how* an approach is chosen. A stratified study might be better approximated by a linear model than a general population sample. As mentioned in the review, the number of predictors and sample size can affect the model applicability and performance; of note, the review compared risk of bias rather than performance.

Given these overlaps of ML and (bio)statistical approaches, a more useful categorization can be linear vs. nonlinear (including higher order interactions in LR) [11,12] or model ranking by complexity [13]. After all, clinicians tend to prefer parsimonious, interpretable models such as linear scores or decision rules with few variables as opposed to black boxes calculating nonlinear functions of many predictors [14,15]. We interpret Brian Ripley's *"machine learning is statistics minus checking of models and assumptions"* (useR! 2004, Vienna) as granting ML a broad, computationally empowered scope, yet necessarily rooted in well-founded causal inference, not isolated, specious prediction.

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