Negative Results for Software Effort Estimation

Tim Menzies, Ye Yang, George Mathew, Barry Boehm, Jairus Hihn

Abstract  Context: More than half the literature on software effort estimation (SEE) focuses on comparisons of new estimation methods. Surprisingly, there are no studies comparing state of the art latest methods with decades-old approaches.

Objective: To check if new SEE methods generated better estimates than older methods.

Method: Firstly, collect effort estimation methods ranging from “classical” COCOMO (parametric estimation over a pre-determined set of attributes) to “modern” (reasoning via analogy using spectral-based clustering plus instance and feature selection, and a recent “baseline method” proposed in ACM Transactions on Software Engineering). Secondly, catalog the list of objections that lead to the development of post-COCOMO estimation methods. Thirdly, characterize each of those objections as a comparison between newer and older estimation methods. Fourthly, using four COCOMO-style data sets (from 1991, 2000, 2005, 2010) and run those comparisons experiments. Fifthly, compare the performance of the different estimators using a Scott-Knott procedure using (i) the A12 effect size to rule out “small” differences and (ii) a 99% confident bootstrap procedure to check for statistically different groupings of treatments).

Results: The major negative results of this paper are that for the COCOMO data sets, nothing we studied did any better than Boehm’s original procedure.

Conclusions: When COCOMO-style attributes are available, we strongly recommend (i) using that data and (ii) use COCOMO to generate predictions. We say this since the experiments of this paper show that, at least for effort estimation, how data is collected is more important than what learner is applied to that data.

Categories/Subject Descriptors: D.2.9 [Software Engineering]: Time Estimation; K.6.3 [Software Management]: Software Process

Keywords: effort estimation, COCOMO, CART, nearest neighbor, clustering, feature selection, prototype generation, bootstrap sampling, effect size, A12.

T. Menzies, G. Mathew
CS, North Carolina State Univ., USA E-mail: tim.menzies@gmail.com, E-mail: george.meg91@gmail.com

Y. Yang
SSE, Stevens Inst., USA E-mail: ye.yang@stevens.edu

B. Boehm
CS, Univ. of Southern California, USA E-mail: barryboehm@gmail.com

J. Hihn
Jet Propulsion Laboratory/California Institute of Technology, USA E-mail: jairus.hihn@jpl.nasa.gov
1 Introduction

This paper is about a negative result in software effort estimation—specifically:

− For project data expressed in a certain way (the COCOMO format [7]);
− Despite decades of work into alternate methods;
− Best predictions from that data come from a parametric method proposed in 2000 [7].

This conclusion comes with two caveats. Firstly, not all projects can be expressed in terms of COCOMO— but when there is a choice, the results of this paper argue that there is value in using that format. Secondly, our conclusion is about solo prediction methods which is different to the ensemble approach [35, 48, 53, 54]—but if using ensembles, this paper shows that parametric estimation would be a viable ensemble member.

For pragmatic and methodological reasons, it is important to report negative results like the one described above. Pragmatically, it is important for industrial practitioners to know that (sometimes) they do not need to waste time straining to understand bleeding-edge technical papers. In the following, we precisely define the class of project data that does not respond well to bleeding-edge effort estimation techniques. For those kinds of data sets, practitioners can be rest assured that it is reasonable and responsible and useful to use simple traditional methods.

Also, methodologically, it is important to acknowledge mistakes. According to Karl Popper (a prominent figure in the philosophy of science [63]), the "best" theories are the ones that have best survived vigorous debate. Having been engaged in some high-profile debates (in the field of software analytics [47]), we assert that such criticisms are very useful since they help a researcher (1) find flaws in old ideas and (2) evolve better new ideas. That is, finding and acknowledging mistakes should be regarded as a routine part of standard operations procedure for science.

Given the above, it is troubling that there are very few negative results in the field of software analytics. What does happen, occasionally, are reports of small corrections to prior work. Given the complexity of software analytics, this absence of such failure reports is highly suspicious. For examples of such reports, see (e.g. as done in [49, 58]).

Why are these reports so rare? There are many possible reasons and here we speculate on two possibilities. Firstly, such negative reports may not be acknowledged as "worthwhile" by the community. Forums such as this special issue are very rare (which is why this issue is so important). Secondly, it is not standard practice in software analytics for researchers to benchmark their latest results against some supposedly simpler "straw man" method. In his textbook on Empirical Methods in AI, Cohen [13] strongly advises such "straw man" comparisons, since sometimes, they reveal that the supposedly superior method is actually overly complex. Hence it always useful to compare methods against simpler alternative.

That said, in some cases no such method is available making such benchmarking impossible. Although as a domain starts to become more mature, these comparisons can be conducted; see, e.g. the many experiments on defect prediction [65] or tag inference for Stack Overflow posts [71]. Accordingly, this paper checks an interesting, but currently unexplored aspect of effort estimation. We check if there exists data sets from which very old methods do just as well as anything else. We consider data expressed in terms of the COCOMO ontology: 23 attributes describing a software project, as well as aspects of its personnel, platform and product feature. We will show that (given this diverse sample of data types collected from a project) Boehm’s 2000 model works as well (or better) than everything else we tried. Hence, we strongly recommend that if that kind of data is avail-

1 For full details on these attributes, see §4 of this paper.
able, then it should be collected and it should be processed using Boehm’s 2000 COCOMO model.

To guide our exploration, this paper asks four research questions. These questions have been selected based on our experience debating the merits of COCOMO vs alternate methods. Based on our experience, we assert that each of the following questions has been used to motivate the development of some alternate to the standard COCOMO-II model:

**RQ1:** Is parametric estimation no better than using just Lines of Code measures? (an often heard, but rarely tested, comment).

**RQ2:** Has parametric estimation been superseded by more recent estimation methods? We apply our “best” learner, as well as case-based reasoning and regression trees.

**RQ3:** Are the old parametric tunings irrelevant to more recent projects? COCOMO models are learned by “tuning” the default model parameters using local project data. COCOMO-II shipped with a set of parameters learned from a particular set of projects from 1995 to 2000. We apply those COCOMO-II tunings, without modification, to a wide range of projects dating from 1970 to 2010.

**RQ4:** Is parametric estimation expensive to deploy at some new site? We try tuning estimation models on small training sets as well as simplifying the specification of projects.

To explore these questions, we use COCOMO since its internal details have been fully published [7]. Also, we can access a full implementation of the 2000 COCOMO model.

![Fig. 1: Projects used by the learners in this study.](image1)

![Fig. 2: Projects in this study. COC81 is a subset of COCOMO-II. Note that NASA’93 and COC’05 and NASA’10 have no overlap with the data used to define the version of COCOMO used in this paper.](image2)
Further, we have access to numerous interesting COCOMO data sets: see Figure 1 and Figure 2. With one exception, our learning experiments do not use the data that generated standard COCOMO. That exception is the COC81 data– which lets us compare new methods against the labor intensive methods used to make standard COCOMO– see Figure 2.

Using that data, the experiments of this paper conclude that the answer to all our research questions is nearly always “no”. The RQ1 experiments show that good estimates use many variables and poorer estimates result from some trite calculation based on KLOC. As to the other research questions (RQ2, RQ3, RQ4), those results imply that the continued use of parametric estimation can still be endorsed– at least for data expressed in terms of the 23 COCOMO attributes.

For a sample of our data see the NASA10 data set in Figure 3.

### 2 About Effort Estimation

#### 2.1 History

Accurately estimating software development effort is of vital importance. Under-estimation can cause schedule and budget overruns as well as project cancellation [70]. Over-estimation delays funding to other promising ideas and organizational competitiveness [35]. Hence, there is a long history of researchers exploring software effort estimation; e.g. [4, 5, 9, 19, 20, 22, 25, 46, 61, 65, 68, 73, 74, 76]. In 2007, Jorgensen and Shepperd reported on hundreds of research papers dating back to the 1970s devoted to the topic, over half of which propose some innovation for developing new estimation models [25]. Since then, many such papers have been published; e.g. [14, 30, 32, 34, 36, 37, 40, 42, 43, 51, 55].

In the 1970s and 1980s, this kind of research was focused on parametric estimation as done by Putnam and others [4, 5, 19, 20, 74, 76]. For example, Boehm’s COnstructive COst MOdel (COCOMO) model [5] assumes that effort varies exponentially on size as seen in this parametric form: $effort \propto a \times KLOC^b$. To deploy this equation in an organization, local project data is used to tune the $(a, b)$ parameter values. If local data is unavailable, new projects can reuse prior tunings, with minor tweaks [50]. COCOMO is a parametric method; i.e. it is a model-based method that $(a)$ assumes that the target model has a particular
structure, then (b) uses model-based methods to fill in the details of that structure (e.g. to set some tuning parameters).

Since that work on parametric estimation, researchers have innovated other methods based on regression trees [68], case-based-reasoning [68], spectral clustering [45], genetic algorithms [9, 15], etc. These methods can be augmented with “meta-level” techniques like tabu search [14], feature selection [11], instance selection [34], feature synthesis [51], active learning [36], transfer learning [37], temporal learning [44, 55], and many more besides.

### 2.2 Current Practice

In her keynote address to ICSE’01, Mary Shaw [67] noted that it can take up to a decade for research innovations to become stable and then another decade after that to become

---

**Fig. 4: COCOMO-II attributes.**

`Table 1: COCOMO-II attributes.`

| Definition  | Low-end = (1,2) | Medium = (3,4) | High-end = (5,6) |
|-------------|----------------|----------------|------------------|
| Scale factors: |                 |                |                  |
| Flex | development flexibility | some guidelines, | only general goals |
|      | rigorously defined | which can be    | defined           |
| Pmat | process maturity | CMM level 1 | CMM level 3 |
| Prec | precededness | have never built | somewhat new |
|      | software before | thoroughly familiar |
| Resl | architecture or risk resolution | few interfaces defined or few risks eliminated | most interfaces defined or most risks eliminated | all interfaces defined or all risks eliminated |
| Team | team cohesion | very difficult interactions | basically cooperative | seamless interactions |
| Effort multipliers |                 |                |                  |
| acap | analyst capability | worst 35% | 35% - 50% | best 10% |
| aexp | applications experience | 2 months | 1 year | 6 years |
| cplx | product complexity | e.g. simple read/write statements | e.g. use of simple interface widgets | e.g. performance-critical embedded systems |
| data | data documentation | many life-cycle phases not documented | extensive reporting for each life-cycle phase |
| lex | language and tool-set experience | 2 months | 1 year | 6 years |
| pcap | programmer capability | worst 15% | 50% | best 10% |
| pcon | personnel continuity (% turnover per year) | 48% | 12% | 3% |
| plex | platform experience | 2 months | 1 year | 6 years |
| pvol | platform volatility | 12 months | 9 months | 4 weeks |
| rely | required reliability | errors are slight inconvenience | errors are easily recoverable | errors can risk human life |
| ruse | required reuse | none | multiple programs | multiple product lines |
| sced | dictated development schedule | deadlines moved to 75% of the original estimate | no change | deadlines moved back to 160% of original estimate |
| site | multi-site development | some contact phone mail | some email | interactive multimedia |
| stor | required % of available RAM | N/A | 50% | 95% |
| time | required % of available CPU | N/A | 50% | 95% |
| tool | use of software tools | edit, code, debug | integrated with life cycle |

Effort

*construction effort in months*

*1 month = 152 hours (includes development & management hours).*
widely popular. Given that, it would be reasonable to expect commercial adoption of the
1990s estimation work on regression trees [68] or case-based-reasoning [68]. However, this
has not happened. Parametric estimation is widely-used, especially across the aerospace
industry and various U.S. government agencies. For example:

- NASA routinely checks software estimates in COCOMO [16].
- In our work with the Chinese and the United States software industry, we saw an almost
  exclusive use of parametric estimation tools such as those offered by Price Systems
  (pricesystems.com) and Galorath (galorath.com).
- Professional societies, handbooks and certification programs are mostly developed around
  parametric estimation methods and tools; e.g. see the International Cost Estimation and
  Analysis Society; the NASA Cost Symposium; the International Forum on COCOMO
  and Systems/Software Cost Modeling (see the websites http://tiny.cc/iceaa
  http://tiny.cc/nasa_cost http://tiny.cc/csse).

2.3 But Does Anyone Use COCOMO?

Two of the myths of effort estimation is that (1) no one uses model-based estimation like
COCOMO; and (2) estimates are always better done using expert-based guess-timation.

These myths are misleading. As seen above, model-based parametric methods are widely
used in industry and are strongly advocated by professional societies. Also, while it is true
that expert-based estimation is a common practice [6], this is not to say that this should be
recommended as the best or only way to make estimates:

- Jorgensen [26] reviews studies comparing model- and expert- based estimation and con-
  cludes that there is no clear case that expert-methods are better.
- In 2015, Jorgensen further argued [23] that model-based methods are useful for learning
  the uncertainty about particular estimates; e.g. by running those models many times, each
time applying small mutations to the input data.
- Valerdi [72] lists the cognitive biases that can make an expert offer poor expert-estimates.
- Passos et al. show that many commercial software engineers generalize from their first
  few projects for all future projects [62].
- Jorgensen & Gruschke [24] document how commercial “gurus” rarely use lessons from
  past projects to improve their future expert-estimates. They offer examples where this
  failure to revise prior beliefs leads to poor expert-based estimates.

Much research has concluded that the best estimations come from combining the predictions
from multiple oracles [3, 12, 35, 72]. Note that it is much easier to apply this double-check
strategy using expert+model-based methods than by comparing the estimates from multiple
expert teams. For example, all the model-based methods studied in this paper can gener-
ate estimates in just a few seconds. In comparison, expert-based estimation is orders of
magnitude slower– as seen in Valerdi’s COSYSMO expert-method. While a strong pro-
nponent of this approach, Valerdi concedes that “(it is) extremely time consuming when large
sample sizes are needed” [72]. For example, he once recruited 40 experts to three expert
sessions, each of which ran for three hours. Assuming a 7.5 hour day, then that study took
3 * 3 * 40/7.5 = 48 days.

COSYSMO is an elaborate expert-based method. An alternate, more lightweight expert-
method is “planning poker” [57] where participants offer anonymous “bids” on the comple-
tion time for a project. If the bids are widely divergent, then the factors leading to that dis-
agreement are elaborated and debated. This cycle of bid+discuss continues until a consensus
has been reached.

While planning poker is widely advocated in the agile community, there are surpris-
ingly few studies assessing this method (one rare exception is [57]). Also, planning poker is
used to assess effort for particular tasks in the scrum backlog— which is a different and sim-
pler task than the initial estimation of large-scale projects. This is an important issue since,
for larger projects, the initial budget allocation may require a significant amount of intra-
organizational lobbying between groups with competing concerns. For such large-estimate-
projects, it can be challenging to change the initial budget allocation. Hence, it is important
to get the initial estimate as accurate as possible.

2.4 COCOMO: Origins and Development

These concerns with expert-based estimation date back many decades and were the genesis
for COCOMO. In 1976, Robert Walquist (a TRW division general manager) told Boehm:

“Over the last three weeks, I’ve had to sign proposals that committed us to budgets
of over $50 million to develop the software. In each case, nobody had a good expla-
nation for why the cost was $50M vs. $30M or $100M, but the estimates were the
consensus of the best available experts on the proposal team. We need to do better.
Feel free to call on experts & projects with data on previous software cost.”

TRW had a previous model that worked well for a part of TRW’s software business [76],
but it did not relate well to the full range of embedded software, command and control
software, and engineering and scientific software involved in TRW’s business base. Having
access to experts and data was a rare opportunity, and a team involving Ray Wolverton, Kurt
Fischer, and Boehm conducted a series of meetings and expert exercises to find the relative
significance of various cost drivers. Combining local expertise and data, plus some prior
results such as [4, 20, 65, 74], and early versions of the RCA PRICE S model [19], a model
called SCEP was created (Software Cost Estimation Program). Except for one explainable
outlier, the estimates for 20 projects with solid data were within 30% of the actuals, most
within 15% of the actuals.

After gathering some further data from subsequent TRW projects and about 35 projects
from teaching software engineering courses at UCLA and USC along with commercial short
courses on software cost estimation, Boehm was able to gather 63 data points that could be
published and to extend the model to include alternative development modes that covered
other types of software such as business data processing. The resulting model was called
the COnstructive COst MOdel, or COCOMO, and was published along with the data in the
book Software Engineering Economics [5]. In COCOMO-I, project attributes were scored
using just a few coarse-grained values (very low, low, nominal, high, very high). These
attributes are effort multipliers where an off-nominal value changes the estimate by some
number greater or smaller than one. In COCOMO-I, all attributes (except KLOC) influence
effort in a linear manner.

Following the release of COCOMO-I Boehm created a consortium for industrial orga-
nizations using COCOMO. The consortium collected information on 161 projects from
commercial, aerospace, government, and non-profit organizations. Based on an analysis of
those 161 projects, Boehm added new attributes called scale factors that had an exponential
impact on effort (e.g. one such attribute was process maturity). Using that new data, Boehm
and his colleagues developed the tunings shown in Figure 5 that map the project descrip-
tors (very low, low, etc) into the specific values used in the COCOMO-II model (released in
2000 [7]):

\[
\text{effort} = a \prod_i E M_i \times KLOC^{b+0.01 \sum_j SF_j}
\]  

Here, EM, SF are effort multipliers and scale factors respectively and \(a, b\) are the local
calibration parameters (with default values of 2.94 and 0.91). Also, effort measures “de-
velopment months” where one month is 152 hours of work (and includes development and
= None; Coc2tunings = [\
    # scale factors:
    ['Flex', 5.07, 4.05, 3.04, 2.03, 1.01, _],
    ['Fmat', 7.86, 6.24, 4.68, 3.12, 1.56, _],
    ['Freq', 6.20, 4.96, 3.72, 2.48, 1.24, _],
    ['Risl', 7.07, 5.65, 4.24, 2.83, 1.41, _],
    ['Team', 5.48, 4.38, 3.29, 2.19, 1.01, _],
    # effort multipliers:
    ['acap', 1.42, 1.19, 1.00, 0.85, 0.71, _],
    ['aexp', 1.22, 1.10, 1.00, 0.88, 0.81, _],
    ['cplx', 0.73, 0.87, 1.00, 1.05, 1.14, 1.28],
    ['data', 0.82, 0.91, 1.00, 1.11, 1.23, _],
    ['item', 1.20, 1.09, 1.00, 0.91, 0.84, _],
    ['pcon', 1.34, 1.15, 1.00, 0.86, 0.76, _],
    ['pcap', 1.64, 1.39, 1.00, 0.88, 0.83, _],
    ['plex', 1.79, 1.62, 1.00, 0.88, 0.83, _],
    ['lcon', 1.22, 1.03, 1.00, 0.88, 0.83, _],
    ['ltex', 1.20, 1.09, 1.00, 0.88, 0.83, _],
    ['pvol', 1.14, 0.96, 1.00, 0.88, 0.83, _],
    ['rely', 1.19, 1.09, 1.00, 0.88, 0.83, _],
    ['site', 1.22, 1.03, 1.00, 0.88, 0.83, _],
    ['ruse', 1.19, 1.09, 1.00, 0.88, 0.83, _],
    ['stor', 1.22, 1.03, 1.00, 0.88, 0.83, _],
    ['time', 1.22, 1.03, 1.00, 0.88, 0.83, _],
    ['tool', 1.19, 1.09, 1.00, 0.88, 0.83, _]
]

def COCOMO2(project, a = 2.94, b = 0.91, # defaults
    tunes= Coc2tunings): # defaults
    sfs,ems,kloc = 0, 5, 22
    scaleFactors, effortMultipliers = 5, 17
    for i in range(scaleFactors):
        sfs += tunes[i][project[i]]
    for i in range(effortMultipliers):
        ems *= tunes[i][project[i]]
    return a * ems * project[kloc] ** (b + 0.01*sfs)

Fig. 5: COCOMO-II: effort estimates from a project. Here, project has 5 scale factors plus 17 effort multipliers plus KLOC. “Xhigh” is show for “extremely high”. Each attribute except KLOC and effort is scored using the scale very low = 1, low=2, up to xhigh=6. Note all attributes extend across the entire range very low to extremely high since, in Boehm’s modeling work, not all effects extend across the entire range. For an explanation of the attributes shown in green, see Figure 4.

management hours). For example, if effort=100, then according to COCOMO, five developers would finish the project in 20 months.

2.5 COCOMO and Local Calibration

COCOMO models are learned by “tuning” the default model parameters using local project data. When local data is scarce, approximations can be used to tune a model using just a handful of examples.

For example, COCOMO’s local calibration procedure, adjusts the impact of the scale factors and effort multipliers by tuning the $a, b$ values of Equation [1] while keeping the other values of the tuning matrix constant as shown in Figure [5]. Effectively, local calibration trims a 23 variable model into a model with two variables: (one to adjust the linear effort multipliers, and another to adjust the exponential scale factors).

Menzies’ preferred local calibration procedure is the COCONUT procedure of Figure [6] (first written in 2002 and first published in 2005 [50]). For some number of repeats, COCONUT will ASSESS some GUESSES for $(a, b)$ by applying them to some training data. If any of these guesses prove to be useful (i.e. reduce the estimation error) then COCONUT will recurse after constraining the guess range for $(a, b)$ by some amount (say, by 2/3rds). COCONUT terminates when (a) nothing better is found at the current level of recursion
def COCONUT(training, # list of projects
    a=10, b=1, # initial (a,b) guess
deltaA = 10, # range of "a" guesses
deltaB = 0.5, # range of "b" guesses
depth = 10, # max recursive calls
    constricting=0.66): # next time, guess less
    if depth > 0:
        useful,a1,b1= GUESSES(training,a,b,deltaA,deltaB)
        if useful: # only continue if something useful
            return COCONUT(training,
                a1, b1, # our new next guess
deltaA * constricting,
deltaB * constricting,
depth - 1)
    return a,b

def GUESSES(training, a,b, deltaA, deltaB,
    repeats=20): # number of guesses
    useful, a1,b1,least,n = False, a,b, 10**32, 0
    while n < repeats:
        aGuess = a1 - deltaA + 2 * deltaA * rand()
        bGuess = b1 - deltaB + 2 * deltaB * rand()
        error = ASSESS(training, aGuess, bGuess)
        if error < least:
            useful,a1,b1,least = True,aGuess,bGuess,error
    return useful,a1,b1

def ASSESS(training, aGuess, bGuess):
    error = 0.0
    for project in training: # find error on training
        predicted = COCOMO2(project, aGuess, bGuess)
        actual = effort(project)
        error += abs(predicted - actual) / actual
    return error / len(training) # mean training error

Fig. 6: COCONUT tunes a, b of Figure 5’s COCOMO function.

def RIG():
    DATA = { COC81, NASA83, COC05, NASA10 }
    for data in DATA: # e.g. data = COC81
        errors= ()
        for learner in LEARNERS: # e.g. learner=COCONUT
            for n in range(10): # ten times repeat
                for project in DATA: # e.g. one project
                    training = data - project # leave-one-out
                    model = learn(training)
                    estimate = guess(model, project)
                    actual = effort(project)
                    error = abs(actual - estimate)/actual
                    errors[learner][n] = error
        print rank(errors) # some statistical tests

Fig. 7: The experimental rig used in this paper.

or (b) after 10 recursive calls – at which point the guess range has been constricted to
$(2/3)^{10} \approx 1\%$ of the initial range.

3 Experimental Methods

In this section, we discuss the methods used to explore the research questions defined in the introduction.
3.1 Choice of Experimental Rig

“Ecological inference” is the conceit that “what holds for all, also holds for parts of the population” \cite{45, 64}. To avoid ecological inference, our rig in Figure 7 runs separately for each data set.

Since some of our methods include a stochastic algorithm (the COCONUT algorithm of Figure 6), we repeat our experimental rig \(N = 10\) times (10 was selected since, after experimentation, we found our results looked the same at \(N = 8\) and \(N = 16\)).

It is important to note that Figure 7 is a “leave-one-out experiment”; i.e. training is conducted on all-but-one example, then tested on the “holdout” example not seen in training. This separation of training and testing data is of particular importance in this study. As shown in Figure 1, our data sets (NASA10, COC81, NASA93, and COC05) contain information on 17, 63, 92, and 93 projects, respectively. When fitted to the 24 parameters of the standard COCOMO model (shown in Figure 4), there may not be enough information to constrain the learning—which means that it is theoretically possible that data could be fitted to almost anything (including spurious noise). To detect such spurious models, it is vital to test the learned model against some outside source such as the holdout example.

We assess performance via Standardized Error (SE); i.e.

\[
SE = \frac{\sum_{i=1}^{n} (\text{abs}(\text{actual}_i - \text{predicted}_i))/n}{\sum_{i=1}^{n} (\text{abs}(\text{actual}_i - \text{sampled}_i))/n} \times 100
\]

This measure is derived from Standardized Accuracy (SA) defined by Shepperd & MacDonnell \cite{69} \((SE = 1 - SA)\). In Equation 2, \text{actual} represents the true value, \text{predicted} represents the estimated value by the predictor and \text{sampled} is a value drawn randomly from a list of random samples (with replacement) from the training data. Shepperd and MacDonnell also propose another measure that reports the performance as a ratio of some other, much simpler, “straw man” approach (they recommend the mean effort value of \(N > 100\) random samples of the training data).

3.2 Choice of Learners

Our LOC(n) “straw man” estimators just uses lines of code in the \(n\) nearest projects. For distance, we use:

\[
dist(x, y) = \sqrt{\sum_{i} w_i (x_i - y_i)^2}
\]

where \(x_i, y_i\) are values normalized 0..1 for the range min..max and \(w_i\) is a weighting factor (defaults to \(w_i = 1\)). When estimating for \(n > 1\) neighbors, we combine estimates via the triangle function of Walkerden and Jeffery \cite{73}; e.g., for \(\text{loc}(3)\), the estimate from the first, second and third closest neighbor with estimates \(a, b\) and \(c\) respectively is

\[
\text{effort} = \frac{(3a + 2b + 1c)}{6}
\]

We also baseline the COCOMO-II and COCONUT models using the Automatically Transformed Linear Baseline Model (ATLM) proposed by Whigham et al. \cite{75}. ATLM is a multiple linear regression model of the form

\[
\text{effort}_i = \beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i} + \ldots + \beta_n x_{ni} + \epsilon_i
\]

where \(\text{effort}_i\) is the quantitative response (effort) for project \(i\) and \(x_i\) are the independent variables of describing the project. The prediction weights \(\beta_i\) are determined using a least square error estimation. Transformations are also employed on the independent
variables \(x_i\) based on their nature. If the variable is continuous in nature, either a logarithmic, a square root transformation or no transformation is employed such that the skewness of the independent variable in the training set is minimized. If the variable is of categorical nature, no transformation is performed on the model.

Apart from the LOC “straw man” and the ATLM baseline we also compare COCOMO-II and COCONUT with CART and Knear\((n)\) as they proved their value in the 1990s [68, 73]. That said, CART and Knear\((n)\) still have currency: recent results from IEEE TSE 2008 and 2012 still endorse their use for effort estimation [17, 32, 35]. Also, according to the Shaw’s timetable for industry adoption of research innovations (discussed in the introduction), CART and Knear\((n)\) should now be mature enough for industrial use. Further, to account for some of the more recent work on effort estimation, we also use TEAK and PEEKING2 [34, 60].

CART [8] is an iterative dichotomization algorithm that finds the attribute that most divides the data such that the variance of the goal variable in each division is minimized. The algorithm then recurses on each division. Finally, the cost data in the leaf divisions are averaged to generate the estimate.

Knear\((n)\) estimates a new project’s effort by a nearest neighbor method [68]. Unlike LOC\((n)\), a Knear\((n)\) method uses all attributes (all scale factors and effort multipliers as well as lines of code) to find the \(n\)-th nearest projects in the training data. Knear\((3)\) combines efforts from three nearest neighbors using Equation 3. Knear\((n)\) is an example of CBR; i.e. case-based reasoning. CBR for effort estimation was first pioneered by Shepperd & Schofield in 1997 [68]. Since then, it has been used extensively in software effort estimation [2, 28, 30–33, 39–42, 68, 73]. There are several reasons for this. Firstly, it works even if the domain data is sparse [59]. Secondly, unlike other predictors, it makes no assumptions about data distributions or some underlying parametric model.

TEAK is built on the assumption that spurious noise leads to large variance in the recorded efforts [34]. TEAK’s pre-processor removes such regions of high variance as follows. First, it applies greedy agglomerate clustering to generate a tree of clusters. Next, it reflects on the variance of the efforts seen in each sub-tree and discards the sub-trees with largest variance. Estimation is then performed on the surviving examples. PEEKING2 [60] is a far more aggressive “data pruner” than TEAK and combines data reduction operators, feature weighting, and Principal Component Analysis(PCA). PEEKING2 is described in Figure 8. One important detail with TEAK and PEEKING2 is that when they prune data, they only do so on the training data. Given a test set, TEAK and PEEKING2 will always try to generate estimates for all members of that test set.

### 3.3 Choice of Statistical Ranking Methods

The last line of our experimental rig shown in Figure 7 ranks multiple methods for learning effort estimators. For this paper, those multiple methods are the range of \(l\) treatments of size \(ls = |l|\) explored within each research question. For example, RQ1 studies the differences in output produced by \(ls = 4\) methods: two COCOMO variants and two others that just use lines of code counts.

This study ranks methods using the Scott-Knott procedure recommended by Mittas & Angelis in their 2013 IEEE TSE paper [56]. This method sorts a list of \(l\) treatments with \(ls\) measurements by their median score. It then splits \(l\) into sub-lists \(m, n\) in order to maximize the expected value of differences in the observed performances before and after divisions. For example, for RQ1, we would sort \(ls = 4\) methods based on their median score, then divide them into three sub-lists of of size \(m, n, n\in\{(1, 3), (2, 2), (3, 1)\}\). Scott-Knott would declare one of these divisions to be “best” as follows. For lists \(l, m, n\) of size \(ls, m, n\) where
PEEKING2’s feature weighting scheme changes \( w_i \) in Equation 3 according to how much an attribute can divide and reduce the variance of the effort data (the greater the reduction, the larger the \( w_i \) score).

PEEKING2’s PCA tool uses an accelerated principle component analysis that synthesises new attributes \( e_1, e_2, \ldots \) that extends across the dimension of greatest variance in the data with attributes \( d \). PCA combines redundant variables into a smaller set of variables (so \( e \ll d \)) since those redundancies become (approximately) parallel lines in \( e \) space. For all such redundancies \( i, j \in d \), we can ignore \( j \) since effects that change over \( j \) also change in the same way over \( i \). PCA is also useful for skipping over noisy variables from \( d \)– these variables are effectively ignored since they do not contribute to the variance in the data.

PEEKING2’s prototype generator clusters the data along the dimensions found by accelerated PCA. Each cluster is then replaced with a “prototype” generated from the median value of all attributes in that cluster. Prototype generation is a useful tool for handling outliers: large groups of outliers get their own cluster; small sets of outliers get ignored via median prototype generation.

PEEKING2 generates estimates for a test case by finding its nearest cluster, then the two nearest neighbors within that cluster (where “near” is computed using Equation 3 plus feature weighting). If these neighbors are found at distance \( n_1, n_2, n_1 < n_2 \) and have effort values \( E_1, E_2 \) then the final estimate is an extrapolation favoring the closest one:

\[
\text{estimate} = \frac{n_1 E_1 + n_2 E_2}{n_1 + n_2}
\]

Scott-Knott then checks if that “best” division is actually useful. To implement that check, Scott-Knott would apply some statistical hypothesis test \( H \) to check if \( m, n \) are significantly different. If so, Scott-Knott then recurses on each half of the “best” division.

For a more specific example, consider the results from \( l = 5 \) treatments:

\[
\begin{align*}
rx1 & = [0.34, 0.49, 0.51, 0.6] \\
rx2 & = [0.6, 0.7, 0.8, 0.9] \\
rx3 & = [0.15, 0.25, 0.4, 0.35] \\
rx4 & = [0.6, 0.7, 0.8, 0.9] \\
rx5 & = [0.1, 0.2, 0.3, 0.4]
\end{align*}
\]

After sorting and division, Scott-Knott declares:

- Ranked #1 is rx5 with median= 0.25
- Ranked #1 is rx3 with median= 0.3
- Ranked #2 is rx1 with median= 0.5
- Ranked #3 is rx2 with median= 0.75
- Ranked #3 is rx4 with median= 0.75

Note that Scott-Knott found little difference between rx5 and rx3. Hence, they have the same rank, even though their medians differ.

Scott-Knott is better than an all-pairs hypothesis test of all methods; e.g. six treatments can be compared \( \binom{6}{2} - 6 = 15 \) ways. A 95% confidence test run for each comparison has a very low total confidence: \( 0.95^{15} = 46\% \). To avoid an all-pairs comparison, Scott-Knott only calls on hypothesis tests \( H \) after it has found splits that maximize the performance differences.

For this study, our hypothesis test \( H \) was a conjunction of the A12 effect size test of and non-parametric bootstrap sampling: i.e. our Scott-Knott divided the data if both bootstrapping and an effect size test agreed that the division was statistically significant (99% confidence) and not a “small” effect (A12 \( \geq 0.6 \)).
For a justification of the use of non-parametric bootstrapping, see Efron & Tibshirani [18, p220-223]. For a justification of the use of effect size tests see Shepperd & MacDonell [69]; Kampenes [29]; and Kocaguneli et al. [38]. These researchers warn that even if an hypothesis test declares two populations to be “significantly” different, then that result is misleading if the “effect size” is very small. Hence, to assess the performance differences we first must rule out small effects. Vargha and Delaney’s non-parametric A12 effect size test explores two lists $M$ and $N$ of size $m$ and $n$:

$$A12 = \left( \sum_{x \in M, y \in N} \begin{cases} 1 & \text{if } x > y \\ 0.5 & \text{if } x == y \end{cases} \right) / (mn)$$

This expression computes the probability that numbers in one sample are bigger than in another. This test was recently endorsed by Arcuri and Briand at ICSE’11 [1].

4 Results

4.1 COCOMO vs Just Lines of Code

This section explores RQ1: is parametric estimation no better than using simple lines of code measures?

An often heard, but not often tested, criticism of parametric estimation methods is that they are no better than just using simple lines of code measures. As shown in Figure 9, this is not necessarily true. This figure is a comparative ranking for LOC(1) LOC(3), COCOMO-II and COCONUT. The rows of Figure 9 are sorted by the SE figures. These rows are divided according to their rank, shown in the left column: better methods have lower rank since they have lower SE error values. The right-hand-side column displays the median error (as a black dot) inside the inter-quartile range (25th to 75th percentile, show as a horizontal line).

The key feature of Figure 9 is that just using lines of code is not better than parametric estimation. If the reader is surprised by this result, then we note that with a little mathematics, it is possible to show that the results of Figure 9 are not surprising. From Equation 1 recall that the minimum effort is bounded by the sum of the minimum scale factors and the product of the minimum effort multipliers. Similar expressions hold for the maximum effort estimate. Hence, for a given KLOC, the range of values is given by:

$$0.18 \times KLOC^{0.97} \leq \text{effort} \leq 154 \times KLOC^{1.23}$$

Dividing the minimum and maximum values results in an expression showing how effort can vary for any given KLOC:

$$154/0.18 \times KLOC^{1.23-0.97} = 856 \times KLOC^{0.25}$$  \hspace{1cm} (6)

Equation 6 explains why just using KLOC performs so badly. That equation had two components: KLOC raised to a small exponent (0.25), and a constant showing the influence of all other COCOMO variables. The large value of 856 for that second component indicates that many factors outside of KLOC influence effort. Hence, it is hardly surprising that just using KLOC is a poor way to do effort estimation.
NASA10 (new NASA data up to 2010):

| rank | treatment   | median | IQR |
|------|-------------|--------|-----|
| 1    | COCOMO-II   | 37     | 57  |
| 1    | COCONUT     | 39     | 54  |
| 1    | loc(3)      | 47     | 93  |
| 1    | loc(1)      | 75     | 98  |

COC05 (new COCOMO data up to 2005):

| rank | treatment   | median | IQR |
|------|-------------|--------|-----|
| 1    | COCOMO-II   | 12     | 52  |
| 2    | loc(1)      | 21     | 56  |
| 2    | loc(3)      | 22     | 55  |
| 2    | COCONUT     | 22     | 89  |

NASA93 (NASA data up to 1993):

| rank | treatment   | median | IQR |
|------|-------------|--------|-----|
| 1    | COCONUT     | 12     | 48  |
| 1    | COCOMO-II   | 15     | 50  |
| 2    | loc(1)      | 23     | 63  |
| 2    | loc(3)      | 35     | 65  |

COC81 (original data from the 1981 COCOMO book):

| rank | treatment   | median | IQR |
|------|-------------|--------|-----|
| 1    | COCOMO-II   | 3      | 21  |
| 1    | COCONUT     | 4      | 24  |
| 2    | loc(3)      | 14     | 36  |
| 2    | loc(1)      | 19     | 42  |

Fig. 9: COCOMO vs just lines of code. SE values seen in leave-one-studies, repeated ten times. For each of the four tables in this figure, better methods appear higher in the tables. In these tables, median and IQR are the 50th and the (75-25)th percentiles. The IQR range is shown in the right column with black dot at the median. Horizontal lines divide the “ranks” found by our Scott-Knott+bootstrapping+effect size tests (shown in left column).

4.2 COCOMO vs Other Methods

This section explores RQ2: Has parametric estimation been superseded by more recent estimation methods? and RQ3: Are the old parametric tunings irrelevant to more recent projects?

Figure 10 compares COCOMO and COCONUT with standard effort estimation methods from the 1990s (CART and Knear(n)) as well as ATLM (the baseline effort estimation method proposed in 2015 by Whigham et al. [75] (a method defined by its authors to better define effort estimation experiments— and perhaps to encourage more repeatability in these kinds of studies). In that comparison, COCOMO-II’s error is not ranked worse than any other method (sometimes COCONUT had a slightly lower median SE but that difference was small: ≤2%).

Figure 11 compares COCOMO and COCONUT to more recent effort estimation methods (TEAK and PEEKING2). Once again, nothing was ever ranked better than COCOMO-II or COCONUT.

From these results, we recommend that effort estimation researchers take care to benchmark their new method against older ones.

As to COCONUT, this method was often ranked equaled to COCOMO-II. In several cases COCOMO-II and COCONUT were ranked first and second and the median difference in their scores is very small.

From this data, we conclude that it is not always true the parametric estimation has been superseded by more recent innovations such as CART, Knear(n), TEAK or PEEKING2.
Also, the COCOMO-II tunings from 2000 are useful not just for the projects prior to 2000 (all of COC81, plus some of NASA93) but also for projects completed up to a decade after those tunings (NASA10).

### 4.3 COCOMO vs Simpler COCOMO

This section explores RQ4: Is parametric estimation expensive to deploy at some new site? To that end, we assess the impact a certain simplifications imposed onto COCOMO-II.

#### 4.3.1 Range Reductions

The cost with deploying COCOMO in a new organization is the training effort required to generate consistent project rankings from different analysts. If we could reduce the current six point scoring scale (very low, low, nominal, high, very high and extremely high) then there would be less scope to disagree about projects. Accordingly, we tried reducing the six point scale to just three:
- **Nominal**: same as before;
- **Above**: anything above nominal;
- **Below**: anything below nominal.

| NASA10: (new NASA data up to 2010): | rank | treatment | median | IQR |
|-----------------------------------|------|-----------|--------|-----|
| COCOMO-II                         | 1    | 34        | 55     |     |
| COCONUT                           | 1    | 41        | 61     |     |
| CART                              | 1    | 46        | 55     |     |
| Knear(1)                          | 1    | 49        | 89     |     |
| Knear(3)                          | 2    | 71        | 104    |     |
| ATLM                              | 3    | 90        | 77     |     |

| COCO5: (new COCOMO data up to 2005): | rank | treatment | median | IQR |
|-------------------------------------|------|-----------|--------|-----|
| COCOMO-II                          | 1    | 13        | 51     |     |
| CART                                | 1    | 14        | 48     |     |
| Knear(1)                           | 2    | 22        | 51     |     |
| Knear(3)                           | 2    | 22        | 54     |     |
| COCONUT                            | 2    | 22        | 81     |     |
| ATLM                                | 3    | 94        | 47     |     |

| NASA93: (NASA data up to 1993):     | rank | treatment | median | IQR |
|-------------------------------------|------|-----------|--------|-----|
| COCONUT                             | 1    | 13        | 48     |     |
| COCOMO-II                           | 1    | 15        | 50     |     |
| Knear(1)                            | 2    | 33        | 71     |     |
| Knear(3)                            | 2    | 34        | 63     |     |
| CART                                | 2    | 34        | 63     |     |
| ATLM                                | 3    | 53        | 56     |     |

| COC81: (original data from the 1981 COCOMO book): | rank | treatment | median | IQR |
|---------------------------------------------------|------|-----------|--------|-----|
| COCOMO-II                                         | 1    | 3         | 20     |     |
| COCONUT                                           | 1    | 4         | 25     |     |
| CART                                               | 2    | 13        | 37     |     |
| Knear(3)                                          | 2    | 19        | 48     |     |
| Knear(1)                                          | 3    | 30        | 75     |     |
| ATLM                                               | 4    | 75        | 42     |     |

Fig. 10: COCOMO vs standard methods. Displayed as per Figure 9.
| rank | treatment   | median | IQR |
|------|-------------|--------|-----|
| 1    | COCONUT     | 37     | 58  |
| 1    | COCOMO-II   | 38     | 56  |
| 2    | TEAK        | 87     | 118 |
| 2    | PEEKING2    | 100    | 67  |

**COCO5 (new COCOMO data up to 2005):**

| rank | treatment | median | IQR |
|------|-----------|--------|-----|
| 1    | COCOMO-II | 13     | 55  |
| 1    | COCONUT   | 20     | 86  |
| 2    | TEAK      | 33     | 84  |
| 2    | PEEKING2  | 34     | 79  |

**NASA93 (NASA data up to 1993):**

| rank | treatment   | median | IQR |
|------|-------------|--------|-----|
| 1    | COCONUT     | 12     | 50  |
| 1    | COCOMO-II   | 15     | 49  |
| 2    | TEAK        | 37     | 84  |
| 2    | PEEKING2    | 43     | 76  |

**COC81 (original data from the 1981 COCOMO book):**

| rank | treatment   | median | IQR |
|------|-------------|--------|-----|
| 1    | COCOMO-II   | 3      | 21  |
| 1    | COCONUT     | 4      | 24  |
| 2    | TEAK        | 15     | 61  |
| 2    | PEEKING2    | 19     | 58  |

Fig. 11: COCOMO vs newer methods. Displayed as per Figure 9.

To do this, the tunings table of Figure 5 was altered. For each row, all values below nominal were replaced with their mean (and similarly with above-nominal values). For example, here are the tunings for time before and after being reduced to below, nominal, above:

| range | vlow | low | nominal | high | vhigh | xhigh |
|-------|------|-----|---------|------|-------|-------|
| before| 1.22 | 1.09| 1.00    | 0.93 | 0.86  | 0.80  |
| reduced| 1.15 | 1.15| 1.00    | 0.863| 0.863 | 0.863 |

### 4.3.2 Row Reductions

New COCOMO models are tuned only after collecting 100s of new examples. If that was not necessary, we could look forward to multiple COCOMO models, each tuned to different specialized (and small) samples of projects. Accordingly, we explore tuning COCOMO on very small data sets.

To implement row reduction, training data was shuffled at random and training was conducted on all rows or just the first four or eight rows (denoted r4, r8 respectively). Note that, given the positive results obtained with r8 we did not explore larger training sets.

### 4.3.3 Column Reduction

Prior results tell us that row reduction should be accompanied by column reduction. A study by Chen et al. [10] combines column reduction (that discards noisy or correlated attributes) with row reduction. Their results are very clear: as the number of rows shrink, better estimates come from using fewer columns. Miller [52] explains why this is so: the variance of a linear model learned by minimizing least-squares error decreases as the number of columns in the model decreases. That is, as the number of columns decrease, prediction reliability can increase (caveat: if you remove too much, there is no information left for predictions).
Accordingly, this experiment sorts the attributes in the training set according to how well they select for specific effort values. Let \( x \in a_i \) denote the list of unique values seen for attribute \( a_i \). Further, let there be \( N \) rows in the training data; let \( r(x) \) denote the \( n \) rows containing \( x \); and let \( v(r(x)) \) be the variance of the effort value in those rows. The values of “good” attributes select most for specific efforts; i.e. those attributes minimize 

\[
E(\sigma,a_i) = \sum_{x \in a_i} \left( \frac{n}{N} \times v(r(x)) \right)
\]

This experiment sorted all training data attributes by \( E(\sigma,a_i) \) then kept the data in the lower quarter or half or all of the columns (denoted \( c0.25 \) or \( c0.5 \) or \( c1 \) respectively). Note that, due to the results of Figure 9, LOC was excluded from column reduction.

4.3.4 Results

Figure 12 compares results found when either all or some reduced set of ranges, rows, and columns are used. Note our nomenclature: the COCONUT:c0.5,r8 results are those seen after training on eight randomly selected training examples reduced to below, nominal, above, while ignoring 50% of the columns.

Figure 12 suggests that it is defensible to learn a COCOMO model from just four to eight projects. Most of the r8 results are top-ranked with the exception of the COC81 results (but even there, the absolute difference between the top r8 results and standard COCOMO is very small: just 2%).

Overall, Figure 12 suggests that the modeling effort associated with COCOMO-II could be reduced. Hence, it need not be expensive to deploy parametric estimation at some new site. Projects attributes do not need to be specified in great detail: a simple three point scale will suffice: below, nominal, above. As to how much data is required for modeling, a mere four to eight projects can suffice for calibration. Hence, it should be possible to quickly build many COCOMO-like models for various specialized sub-groups using just a three-point scale

5 Threats to Validity

Questions of validity arise in terms of how the projects (data-sets) are chosen for our experiments. While we used all the data sets that could be shared between our team, it is not clear if our results would generalize to other as yet unstudied data-sets. One the other hand, in terms of the parametric estimation literature, this is one of the most extensive and elaborate studies yet published.

To increase external validity, all the data used in this work is available on-line in the PROMISE code repository. Also, our use of a leave-one-out experimental rig plus the public availability of three of our four data sets (NASA93, COC81, NASA10) means that other researchers would be able to reproduce exactly our rig on exactly the code used in this study.

One source of bias in this study are the learners used for the defect prediction studies. Data mining is a large and active field and any single study can only use a small subset of the known data mining algorithms. Any case studies in SE data mining can only explore a small subset of options, selected by the biases of the researcher. The best any researcher can hope to do is state their biases and make some attempt to compensate for them. Accordingly:

– The biases of the authors of this paper made us select a parametric modeling method (COCOMO) as the main modeling method.
– We then made a conscious decision to reverse those biases and explore non-parametric methods (PEEKING2 and TEAK) as well as decision-tree methods (CART).
6 Conclusion

The past few decades have seen a long line of innovative methods applied to effort estimation. This paper has compared a sample of those methods to a decades-old parametric estimation method.

Based on that study, we offered a negative result in which a decades old effort estimation method performed as well, or better, as more recent methods:

- **RQ1**: just using LOC for estimation is far worse that parametric estimation over many attributes (see §4.1);
- **RQ2**: new innovations in effort estimation have not superseded parametric estimation (see §4.2).
– **RQ3**: Old parametric tunings are not out-dated (see §4.2);
– **RQ4**: It is possible to simplify parametric estimation with some range, row and column pruning to reduce the cost of deploying those methods at a new site (see §4.3);

Hence, we conclude that in 2016, it is still a valid and a recommended practice to first try parametric estimation. In these experiments, four to eight projects were enough to learn good predictors (and we are exploring methods to reduce that even further). This is an important result since, given the rapid pace of change on software engineering, it is unlikely organizations will have access to dozens and dozens of prior relevant projects to learn from.

Our take-away message here is that the choice of data to collect may be more important than what learner is applied to that data. Certainly, it is true that not all projects can be expressed in terms of COCOMO. But when there is a choice, we recommend collecting data like Figure 3, and then processing that data using COCOMO-II.

### 7 Future Work

The negative results of this paper makes us question some of the newer (and supposedly better) innovative techniques for effort estimation. The unique and highly variable characteristics of SE project data place great limitation on the results obtained by naively applying some brand-new algorithm. Perhaps one direction for future direction is to investigate how innovative new techniques can extend (rather than replace) existing and successful estimation methods.

Having endorsed the use of parametric methods such as COCOMO, it is appropriate to discuss current plans for new versions of that approach. Recent changes in the software industry suggest it is time to revise COCOMO-II. The rise of agile methods, web services, cloud services, parallelized software on multi-core chips, field-programmable-gate-array (FPGA) software, apps, widgets, and net-centric systems of systems (NCSOS) have caused the COCOMO II developers and users to begin addressing an upgrade to the 14-year-old COCOMO II. Current discussions of a potential COCOMO III have led to a reconsideration of the old COCOMO 1981 development modes, as different development phenomena appear to drive the costs and schedules of web-services, business data processing, real-time embedded software, command and control, and engineering and scientific applications.

Additionally, while calibrating COCOMO II model and developing COCOMO III, we were also seeing time-competitive Agile projects in well-jelled, domain-experienced rapid development organizations, which demonstrates tremendous effort reduction and schedule acceleration [21]. Finally, the emerging community-based software development, i.e. software crowd sourcing [27], challenges the underlying assumptions of traditional software estimation laws. Access to external workforce and competition factors are becoming critical development influential factors and need to be further investigated.

Efforts to characterize these models and to gather data to calibrate models for dealing with them are underway. Contributors to the definition and calibration are most welcome.

### Acknowledgements

The research described in this paper was carried out, in part, at the Jet Propulsion Laboratory, California Institute of Technology, under a contract with the US National Aeronautics and Space Administration. Reference herein to any specific commercial product, process, or service by trade name, trademark, manufacturer, or otherwise does not constitute or imply its endorsement by the US Government.

### References

1. A. Arcuri and L. Briand. A practical guide for using statistical tests to assess randomized algorithms in software engineering. In *ICSE’11*, pages 1–10, 2011.
2. Martin Auer, Adam Trendowicz, Bernhard Graser, Ernst Haunschmid, and Stefan Biffl. Optimal project feature weights in analogy-based cost estimation: Improvement and limitations. IEEE Trans. Softw. Eng., 32:83–92, 2006.
3. Dan Baker. A hybrid approach to expert and model-based effort estimation. Master’s thesis, Lane Department of Computer Science and Electrical Engineering, West Virginia University, 2007. Available from https://eidr.wvu.edu/etd/documentdata.eTD?documentid=5443.
4. R. Black, R. Curnow, R. Katz, and M. Bray. Bcs software production data, final technical report radc-tr-77-116. Technical report, Boeing Computer Services, Inc., March 1977.
5. B. Boehm. Software Engineering Economics. Prentice Hall, 1981.
6. B. Boehm. Safe and simple software cost analysis. IEEE Software, pages 14–17, September/October 2000.
7. Barry Boehm, Ellis Horowitz, Donald Reifer, Bradford K. Clark, Bert Steece, A. Winsor Brown, Sumita Chulani, and Chris Abts. Software Cost Estimation with Cocomo II. Prentice Hall, 2000.
8. L. Breiman, J. H. Friedman, R. A. Olshen, and C. J. Stone. Classification and Regression Trees. 1984.
9. C.J. Burgess and Martin Lefley. Can genetic programming improve software effort estimation? A comparative evaluation. Information and Software Technology, 43(14):863–873, December 2001.
10. Zhihao Chen, Barry Boehm, Tim Menzies, and Daniel Port. Finding the right data for software cost modeling. IEEE Software, 22:38–46, 2005.
11. Zhihoa Chen, Tim Menzies, and Dan Port. Feature subset selection can improve software cost estimation. In PROMISE ’05. Available from http://menzies.us/pdf/05/fsscocomo.pdf.
12. S. Chulani, B. Boehm, and B. Steece. Bayesian analysis of empirical software engineering cost models. IEEE Transaction on Software Engineering, 25(4), July/August 1999.
13. P.R. Cohen. Empirical Methods for Artificial Intelligence. MIT Press, 1995.
14. A. Corazza, S. Di Martino, F. Ferrucci, C. Gravino, F. Sarro, and E. Mendes. How effective is tabu search to configure support vector regression for effort estimation? In Proceedings of the 6th International Conference on Predictive Models in Software Engineering, PROMISE ’10, pages 4:1–4:10, 2010.
15. R. Cordero, M. Costamagna, and E. Paschetta. A genetic algorithm approach for the calibration of cocomo-like models. In 12th COCOMO Forum, 1997.
16. J. B. Dabney. Return on investment for IV&V, 2002-2004. NASA funded study. Results Available from http://sarpregress.ivv.nasa.gov/ViewResearch/24.jsp.
17. Karel Dejaeger, Wouter Verbeke, David Martens, and Bart Baesens. Data mining techniques for software effort estimation: A comparative study. IEEE Transactions on Software Engineering, 38:375–397, 2012.
18. Bradley Efron and Robert J Tibshirani. An introduction to the bootstrap. Mono. Stat. Appl. Probab. Chapman and Hall, London, 1993.
19. F. Freiman and R. Park. Price software model - version 3: An overview. In Proceedings, IEEE-PINY Workshop on Quantitative Software Models, IEEE Catalog Number TH 0067-9, pages 32–41, October 1979.
20. J. Herd, J. Postak, W. Russell, and J. Stewart. Software cost estimation study-study results, final technical report, radc-tr-77-220. Technical report, Doty Associates, June 1977.
21. Dan Ingold, Barry Boehm, and Supannika Koolmanojwong. A model for estimating agile project process and schedule acceleration. In ICSSP 2013, pages 29–35, 2013.
22. R. Jensen. An improved macrolevel software development resource estimation model. In 5th ISPA Conference, pages 88–92, April 1983.
23. M. Jørgensen and T.M. Gruschke. The impact of lessons-learned sessions on effort estimation and uncertainty assessments. Software Engineering, IEEE Transactions on, 35(3):368–383, May-June 2009.
24. M. Jørgensen and M. Shepperd. A systematic review of software development cost estimation studies, January 2007. Available from http://www.simula.no/departments/engineering/publications/J(\O)l\ot\o\rgensen.2005.12.
25. Magne Jørgensen. A review of studies on expert estimation of software development effort. Journal of Systems and Software, 70(1-2):37–60, February 2004.
26. M. Li K. Mao, Y. Yang and M. Harman. Pricing crowdsourcing-based software development tasks. In ICSE, New Ideas and Emerging Results, pages 1205–1208, San Francisco, CA, USA, 2013.
27. G. Kadoda, M. Cartwright, L. Chen, and M. Shepperd. Experiences using casebased reasoning to predict software project effort, 2000.
30. Jacky Wai Keung. Empirical evaluation of analogy-x for software cost estimation. In *ESEM ’08: International Symposium on Empirical Software Engineering and Measurement*, pages 294–296, New York, NY, USA, 2008. ACM.

31. Jacky Wai Keung and Barbara Kitchenham. Experiments with analogy-x for software cost estimation. In *ASWEC ’08: Proceedings of the 19th Australian Conference on Software Engineering*, pages 229–238, Washington, DC, USA, 2008. IEEE Computer Society.

32. Jacky Wai Keung, Barbara A. Kitchenham, and David Ross Jeffery. Analogy-x: Providing statistical inference to analogy-based software cost estimation. *IEEE Trans. Softw. Eng.*, 34(4):471–484, 2008.

33. C. Kirsopp and M. Shepperd. Making inferences with small numbers of training sets. *IEEE Proc.*, 149, 2002.

34. E. Kocaguneli, T. Menzies, A. Bener, and J. Keung. Exploiting the essential assumptions of analogy-based effort estimation. *IEEE Transactions on Software Engineering*, 38:425–438, 2012. Available from [http://menzies.us/pdf/llbeak.pdf](http://menzies.us/pdf/llbeak.pdf).

35. E. Kocaguneli, T. Menzies, and J.W. Keung. On the value of ensemble effort estimation. *Software Engineering, IEEE Transactions on*, 38(6):1403–1416, Nov 2012.

36. Ekrem Kocaguneli, Tim Menzies, Jacky Keung, David Cok, and Ray Madachy. Active learning and effort estimation: Finding the essential content of software effort estimation data. *IEEE Transactions on Software Engineering*, 39(8):1040–1053, 2013.

37. Ekrem Kocaguneli, Tim Menzies, and Emilia Mendes. Transfer learning in effort estimation. *Empirical Software Engineering*, pages 1–31, 2014.

38. Ekrem Kocaguneli, Thomas Zimmermann, Christian Bird, Nachiappan Nagappan, and Tim Menzies. Distributed development considered harmful? In *ICSE*, pages 882–890, 2013.

39. Jingzhou Li and Gunther Ruhe. A comparative study of attribute weighting heuristics for effort estimation by analogy. *International Symposium on Empirical Software Engineering*, page 74, 2006.

40. Jingzhou Li and Gunther Ruhe. Decision support analysis for software effort estimation by analogy. In *PROMISE ’07: Proceedings of the Third International Workshop on Predictor Models in Software Engineering*, page 6, 2007.

41. Jingzhou Li and Gunther Ruhe. Analysis of attribute weighting heuristics for analogy-based software effort estimation method aqua+. *Empirical Softw. Engg.*, 13:63–96, February 2008.

42. Y. Li, M. Xie, and Goh T. A study of the non-linear adjustment for analogy based software cost estimation. *Empirical Software Engineering*, pages 603–643, 2009.

43. C. Lokan and E. Mendes. Cross-company and single-company effort models using the isbog database: a further replicated study. In *The ACM-IEEE International Symposium on Empirical Software Engineering*, November 21-22, Rio de Janeiro, 2006.

44. C. Lokan and E. Mendes. Applying moving windows to software effort estimation. In *Empirical Software Engineering and Measurement, 2009. ESEM 2009. 3rd International Symposium on*, pages 111–122, 2009.

45. Tim Menzies, Andrew Butcher, David R. Cok, Andrian Marcus, Lucas Layman, Forrest Shull, Burak Turhan, and Thomas Zimmermann. Local versus global lessons for defect prediction and effort estimation. *IEEE Trans. Softw. Eng.*, 39(6):822–834, 2013. Available from [http://menzies.us/pdf/12localb.pdf](http://menzies.us/pdf/12localb.pdf).

46. Tim Menzies, Zhihao Chen, Jairus Hihn, and Karen Lum. Selecting best practices for effort estimation. *IEEE Transactions on Software Engineering*, November 2006. Available from [http://menzies.us/pdf/06coseekmo.pdf](http://menzies.us/pdf/06coseekmo.pdf).

47. Tim Menzies, Alex Dekhtyar, Justin Distefano, and Jeremy Greenwald. Problems with precision. *IEEE Transactions on Software Engineering*, September 2007. [http://menzies.us/pdf/07precision.pdf](http://menzies.us/pdf/07precision.pdf).

48. Tim Menzies, Ekrem Kocaguneli, Leandro Minku, Fayola Peters, and Burak Turhan. Chapter 20 - Ensembles of Learning Machines. In *Sharing Data and Models in Software Engineering*, pages 239–265, 2015.

49. Tim Menzies, Fayola Peters, and Andrian Marcus. Ooops... (errata report for “Better Cross-Company Learning”). In *MSR’13*, 2013. [http://www.slideshare.net/timmenzies/msr13-mistake](http://www.slideshare.net/timmenzies/msr13-mistake).

50. Tim Menzies, D. Port, Z. Chen, J. Hihn, and S. Stukes. Validation methods for calibrating software effort models. In *Proceedings, ICSE*, 2005. Available from [http://menzies.us/pdf/04coconut.pdf](http://menzies.us/pdf/04coconut.pdf).

51. Tim Menzies and Martin Shepperd. Special issue on repeatable results in software engineering prediction. *Empirical Software Engineering*, 17(1-2):1–17, 2012.

52. A. Miller. *Subset Selection in Regression (second edition)*. Chapman & Hall, 2002.

53. Leandro L. Minku and Xin Yao. A principled evaluation of ensembles of learning machines for software effort estimation. In *Industrial Management & Data Systems*, volume 106, pages 9:1–9:10, 2011.
54. Leandro L. Minku and Xin Yao. Ensembles and locality: Insight on improving software effort estimation. In *Information and Software Technology*, volume 55, pages 1512–1528, 2013.

55. Leandro L. Minku and Xin Yao. How to make best use of cross-company data in software effort estimation? In *ICSE’14*, pages 446–456, 2014.

56. Nikolaos Mitsas and Lefteris Angelis. Ranking and clustering software cost estimation models through a multiple comparisons algorithm. *IEEE Trans. Software Eng.*, 39(4):537–551, 2013.

57. Kjetil Melokken-Pstvold, Nils Christian Haugen, and Hans Christian Benestad. Using planning poker for combining expert estimates in software projects. *Journal of Systems and Software*, 81:21062117, December 2008.

58. Emerson Murphy-Hill, Chris Parnin, and Andrew P. Black. How We Refactor, and How We Know It. *IEEE Transactions on Software Engineering*, 38(1):5–18, 2012.

59. Ingunn Myrteit, Erik Stensrud, and Martin Shepperd. Reliability and validity in comparative studies of software prediction models. *IEEE Trans. Softw. Eng.*, 31(5):380–391, May 2005.

60. Vasil Papakontri. Data carving: Identifying and removing irrelevancies in the data. Master’s thesis, Lane Department of Computer Science and Electrical Engineering, West Virginia University, 2013.

61. R. Park. The central equations of the price software cost model. In 4th COCOMO Users Group Meeting, November 1988.

62. Carol Passos, Ana Paula Braun, Daniela S. Cruzes, and Manoel Mendonca. Analyzing the impact of beliefs in software project practices. In *ESEM’11*, 2011.

63. K.R. Popper. *Conjectures and Refutations*. Routledge and Kegan Paul, 1963.

64. D. Posnett, V. Filkov, and P. Devanbu. Ecological inference in empirical software engineering. In *Proceedings of ASE’11*, 2011.

65. Giuseppe Scanniello, Carmine Gravino, Andrian Marcus, and Tim Menzies. Class level fault prediction using software clustering. In Automated Software Engineering (ASE), 2013 IEEE/ACM 28th International Conference on, pages 640–645. IEEE, 2013.

66. L. Putnam. A macro-estimating methodology for software development. In *Proceedings, IEEE COMP-CON76 Fall*, pages 38–43, September 1976.

67. M. Shepperd and C. Schofield. Estimating software project effort using analogies. *IEEE Transactions on Software Engineering*, 23(12), November 1997. Available from [http://www.utdallas.edu/~rbanker/SE_XII.pdf](http://www.utdallas.edu/~rbanker/SE_XII.pdf).

68. Martin J. Shepperd and Steven G. MacDonell. Evaluating prediction systems in software project estimation. *Information & Software Technology*, 54(8):820–827, 2012.

69. C. Walston and C. Felix. A method of programming measurement and estimation. *IBM Systems Journal*, 16(1):54–77, 1977.

70. Peter A. Whigham, Caitlin A. Owen, and Stephen G. Macdonell. A baseline model for software effort estimation. *ACM Trans. Softw. Eng. Methodol.*, 24(3):20:1–20:11, May 2015.

71. R. Wolverton. The cost of developing large-scale software. *IEEE Trans. Computers*, pages 615–636, June 1974.