

# On the Value of Parametric Software Effort Estimation

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## ABSTRACT

Despite decades of research into software effort estimation, industry still makes most use of parametric methods developed in the 1970s. Why is there so little adoption of innovative estimation methods?

One explanation is an absence of results showing that (1) parametric estimation is no longer useful and that (2) supposedly more innovation methods are comparatively better. Accordingly, this paper tries to demonstrate these two points. We were unsuccessful.

From this study we conclude that (a) it is still valid and recommended practice to try parametric estimation rather than other methods that are supposedly more innovative; (b) it is useful to augment parametric estimation with some local calibration and column pruning (which are techniques discussed in this paper). Also, given the small size of effort estimation data sets, (c) the *details of data collection* must be given due consideration.

**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.

## 1 Introduction

Accurately estimating software development effort is of vital importance. Under-estimation can cause schedule and budget overruns as well as project cancellation [61]. Over-estimation delays funding to other promising ideas and organizational competitiveness [33]. Hence, there is a long history of researchers exploring software effort estimation; e.g. [4, 5, 9, 18, 19, 21, 23, 44, 53, 56, 59, 63–65]. 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 [23]. Since then, many more such papers have been published; e.g. [13, 28–30, 32, 34, 35, 38, 40, 41, 46, 48].

In the 1970s and 1980s, this kind of research was focused on *parametric estimation* as done by Putnam and others [4, 5, 18, 19, 64, 65]. For example, Boehm’s COConstructive 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 [45].

Since that work on parametric estimation, researchers have innovated other methods based on regression trees [59] case-based-reasoning [59], spectral clustering [43], genetic algorithms [9, 14], etc. These methods can be augmented with “meta-level” techniques like tabu search [13], feature selection [11], instance selection [32], feature synthesis [46], active learning [34], transfer learning [35], temporal learning [42, 48], and many more besides.

In her keynote address to ICSE’01, Mary Shaw [57] noted that it can take up to a decade for research innovations to become stable and then another decade after that to become widely popular. Given that, it would be reasonable to expect commercial adoption of the 1990s estimation work on regression trees [59] or case-based-reasoning [59]. 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 [15].
- In our work with the Chinese and 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<sup>1</sup>.

This lack of adoption of innovative estimation methods could be explained by the absence of results showing that (1) parametric estimation is no longer useful, or that (2) supposedly more innovation methods are comparatively better. To explore those absent results this paper asks five research questions:

**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?** We apply the old COCOMO-II tunings from 2000 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 very small training sets as well as simplifying the specification of local projects.

**RQ5: Are parametric estimates unduly sensitive to errors in the size estimate?** In the context of RQ4, we check what happens if there are large errors in the KLOC estimate.

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<sup>1</sup>See the websites [goo.gl/u3q9Nq](http://goo.gl/u3q9Nq), [goo.gl/8jxPrb](http://goo.gl/8jxPrb), [goo.gl/O01Pc6](http://goo.gl/O01Pc6).

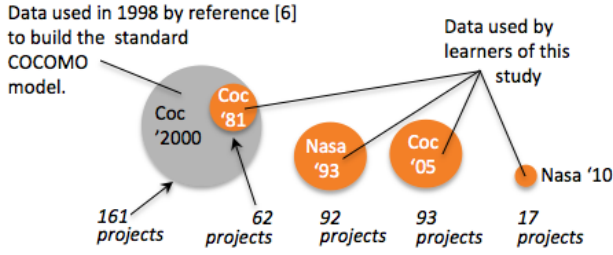


Figure 1: Projects in this study (COC81 is a subset of COC'2000).

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 standard 1998 COCOMO model. 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.

Using that data, the experiments of this paper conclude that the answer to all our research questions is “no”. The RQ1 experiments show that good estimates use many variables and poorer estimates result from some trite calculation based on KLOC. Hence, we can make some degree of error in our KLOC estimates without damaging the overall estimation process (see RQ5).

As to the other research questions (R2,R3,R4), those results mean that the continued use of parametric estimation can still be endorsed. What COCOMO, or other parametric estimation models, can offer

| Types of projects                | COC81 | NASA93 | COC05 | NASA10 |
|----------------------------------|-------|--------|-------|--------|
| Avionics                         |       | 26     | 10    | 17     |
| Banking                          |       |        | 13    |        |
| Business apps/data processing    | 7     | 4      | 31    |        |
| Control                          | 9     | 18     | 13    |        |
| Human-machine interaction        | 12    |        |       |        |
| Military, ground                 |       |        | 8     |        |
| Misc                             | 5     | 4      | 5     |        |
| Mission Planning                 |       | 16     |       |        |
| SCI scientific application       | 16    | 21     | 11    |        |
| Support (tools, utilities, etc)  | 7     |        |       |        |
| Sys (OS, compilers, sensors,etc) | 7     | 3      | 2     |        |

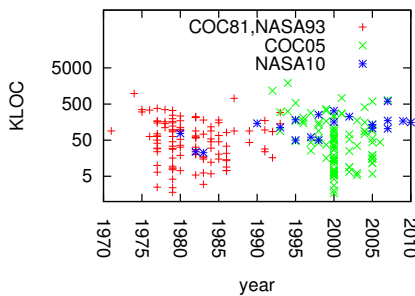


Figure 2: Projects used by the learners in this study. Figure 3 shows project attributes. COC81 is the original data from 1981 COCOMO book [5]. This comes from projects dating 1970 to 1980. NASA93 is NASA data collected in the early 1990s about software that supported the planning activities for the International Space Station. Our two other data sets are COC05 and NASA10 (these data sets are proprietary and cannot be released to the research community). The non-proprietary data (COC81 and NASA93) is available at <http://openscience.us/repo>.

largely contains the fundamentals for making estimation decisions from expert-Delphi, as well as well calibrated tuning factors from over 40 years of industrial data. We would advise researchers *not* to ignore these fundamentals.

More generally, these results make 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 the best future direction is to investigate how innovative new techniques can extend (rather than replace) existing and successful estimation methods. Some experiments in this paper are a starting point for that investigation.

## 2 Background

### 2.1 Parameter- vs Model- vs Delphi-based Estimation

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).

One of the myths of effort estimation is that no one used model-based estimation and that estimates are always better done using expert-based guess-timation, a.k.a. *Delphi-based* methods<sup>2</sup>. As seen in the introduction, model-based parametric methods are widely used in industry and are strongly advocated by professional socients. Also, while it is true that Delphi-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 [24] reviews studies comparing model- and Delphi-based estimation and concludes that there there is no clear case that delph-methods are better.
- Valerdi [62] lists the cognitive biases that can make an expert offer poor Delphi-estimates.
- Passos et al. show that many commercial software engineers generalize from their first few projects to all future projects [54].
- Jorgensen & Gruschke [22] document how commercial “gurus” rarely use lessons from past projects to improve their future Delphi-estimates. They offer examples where this failure to revise prior beliefs leads to poor Delphi-based estimates.

Much research has concluded that best estimations come from *combining* the predictions from *multiple oracles* [3, 12, 33, 62]. Note that it is far easy faster to apply this double-check strategy using Delphi+model-based methods than by comparing the estimates from multiple Delphi teams. For example, all the model-based methods studied in this paper can generate estimates in just a few seconds. In comparison, Delphi-based estimation is orders of magnitude slower— as seen in Valerdi’s COSYSMO Delphi-method. While a strong proponent of this approach, Valerdi concedes that “(it is) extremely time consuming when large sample sizes are needed” [62]. For example, he once recruited 40 experts to three Delphi sessions, each of which ran for three hours. Assuming a 7.5 hour day, then that study took

$$3 * 3 * 40 / 7.5 = 48 \text{ days}$$

COSYSMO is an elaborate Delphi-based method. An alternate, more lightweight Delphi-method is “planning poker” [50] where participants offer anonymous “bids” on the completion time for a project. If the bids are widely divergent, then the factors leading to that disagreement elaborated and debated. This cyclic of bid+discuss continues until a consensus has been reached.

<sup>2</sup>The mythological oracle of Delphi spoke for the god Apollo to answered questions about colonization, religion, and power.

|                    | Definition   | Low-end = {1,2}                                  | Medium = {3,4}                                   | High-end= {5,6}                                   |
|--------------------|--|--|--|---|
| Scale factors:     |  |  |  |   |
| Flex               | development flexibility  | development process rigorously defined           | some guidelines, which can be relaxed            | only general goals defined                        |
| Pmat               | process maturity   | CMM level 1                                      | CMM level 3                                      | CMM level 5                                       |
| Prec               | precedentedness  | we have never built this kind of software before | somewhat new                                     | 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 co-operative                           | seamless interactions                             |
| Effort multipliers |  |  |  |   |
| acap               | analyst capability   | worst 35%  | 35% - 90%  | 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 wid-gets            | e.g. performance-critical embed-ded systems       |
| data               | database size (DB bytes/SLOC)  | 10   | 100  | 1000  |
| docu               | documentation  | many life-cycle phases not docu-mented           |  | extensive reporting for each life-cycle phase     |
| ltex               | language and tool-set experience   | 2 months   | 1 year   | 6 years   |
| pcap               | programmer capability  | worst 15%  | 55%  | best 10%  |
| pcon               | personnel continuity<br>(% turnover per year)  | 48%  | 12%  | 3%  |
| plex               | platform experience  | 2 months   | 1 year   | 6 years   |
| pvol               | platform volatility<br>( $\frac{\text{frequency of major changes}}{\text{frequency of minor changes}}$ ) | $\frac{12 \text{ months}}{1 \text{ month}}$      | $\frac{6 \text{ months}}{2 \text{ weeks}}$       | $\frac{2 \text{ weeks}}{2 \text{ days}}$          |
| rely               | required reliability   | errors are slight inconvenience                  | errors are easily recoverable                    | errors can risk human life                        |
| ruse               | required reuse   | none   | multiple program                                 | multiple product lines                            |
| sced               | dictated development<br>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 multi-media                           |
| 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                        |

Figure 3: COCOMO-II attributes.

While planning poker is widely advocated in the agile community, there are surprisingly few studies assessing this method (one rare exception is [50]). Further, planning poker is used to assessing effort for particular tasks in the scrum backlog— which is a different and simpler task than the *initial* estimation of large-scale projects. This is an important issue since, for larger projects, that 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 that initial estimate as accurate as possible.

## 2.2 COCOMO: Origins and Development

These concerns with Delphi 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 explanation 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 [65], 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 Delphi exercises to find the relative signifi-

cance of various cost drivers. Combining local expertise and data, plus some prior results such as [4, 19, 56, 64], and early versions of the RCA PRICE S model [18], 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, with 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 COConstructive 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 a 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 organizations 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 4 that map the project descriptors (very low, low, etc) into the specific values used in the

```

= None; Coc2tunings = [[
# vlow low nom high vhigh xhigh
# scale factors:
'Flex', 5.07, 4.05, 3.04, 2.03, 1.01, _], [
'Fmat', 7.80, 6.24, 4.68, 3.12, 1.56, _], [
'Prec', 6.20, 4.96, 3.72, 2.48, 1.24, _], [
'Resl', 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, 1.17, 1.34, 1.74], [
'data', _ , 0.90, 1.00, 1.14, 1.28, _], [
'docu', 0.81, 0.91, 1.00, 1.11, 1.23, _], [
'ltex', 1.20, 1.09, 1.00, 0.91, 0.84, _], [
'pcap', 1.34, 1.15, 1.00, 0.88, 0.76, _], [
'pcon', 1.29, 1.12, 1.00, 0.90, 0.81, _], [
'plex', 1.19, 1.09, 1.00, 0.91, 0.85, _], [
'pvol', _ , 0.87, 1.00, 1.15, 1.30, _], [
'rely', 0.82, 0.92, 1.00, 1.10, 1.26, _], [
'ruse', _ , 0.95, 1.00, 1.07, 1.15, 1.24], [
'sced', 1.43, 1.14, 1.00, 1.00, 1.00, _], [
'site', 1.22, 1.09, 1.00, 0.93, 0.86, 0.80], [
'stor', _ , _ , 1.00, 1.05, 1.17, 1.46], [
'time', _ , _ , 1.00, 1.11, 1.29, 1.63], [
'tool', 1.17, 1.09, 1.00, 0.90, 0.78, _]]

def COCOMO2(project, a = 2.94, b = 0.91, # defaults
            tunes=Coc2tunings): # defaults
    sfs ems, kloc = 0, 1, 22
    scaleFactors, effortMultipliers = 5, 17
    for i in range(scaleFactors):
        sfs += tunes[i][project[i]]
    for i in range(effortMultipliers):
        j = i + scaleFactors
        ems += tunes[j][project[j]]
    return a * ems * project[kloc] ** (b + 0.01*sfs)

```

Figure 4: COCOMO-II: effort estimates from a *project*. Here, *project* has up to 24 attributes (5 scale factors plus 17 effort multipliers plus KLOC plus. in the training data, the actual effort). Each attribute except KLOC and effort is scored using the scale very low = 1, low=2, etc. For an explanation of the attributes shown in green, see Figure 3.

COCOMO-II model (released in 2000 [7]):

$$effort = a \prod_i EM_i * KLOC^{b+0.01 \sum_j SF_j} \quad (1)$$

Here,  $EM, SF$  are effort multipliers and scale factors and  $a, b$  are the *local calibration* parameters (with default values of 2.94 and 0.91). Also, *effort* measures “development months” where one month is 152 hours of work (and includes development and management hours). For example, if *effort*=100, then according to COCOMO, five developers would finish the project in 20 months.

Note that, from Equation 1, 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:

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

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

$$154/0.18 * KLOC^{1.23-0.97} = 856 * KLOC^{0.25} \quad (2)$$

### 2.3 COCOMO and Local Calibration

When local data is scarce, approximations can be used to tune a model using just a handful of examples. COCOMO *local calibration* procedure, adjusts the impact of the scale factors and effort multipliers by tuning the  $a, b$  values of Equation 1 while keeping constant the other values of the tuning matrix shown in Figure 4. Effectively, local calibration trims a 23 variable model into a model with two variable: (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 5 (first written in 2002 and first published in

```

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, al, bl = GUESSES(training, a, b, deltaA, deltaB)
        if useful: # only continue if something useful
            return COCONUT(training,
                           al, bl, # 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, al, bl, least, n = False, a, b, 10**32, 0
    while n < repeats:
        n += 1
        aGuess = al - deltaA + 2 * deltaA * rand()
        bGuess = bl - deltaB + 2 * deltaB * rand()
        error = ASSESS(training, aGuess, bGuess)
        if error < least: # found a new best guess
            useful, al, bl, least = True, aGuess, bGuess, error
    return useful, al, bl

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

```

Figure 5: COCONUT tunes  $a, b$  of Figure 4’s COCOMO function.

```

def RIG():
    DATA = { COC81, NASA83, COC05, NASA10 }
    for data in DATA # e.g. data = COC81
        mres = {}
        for learner in LEARNERS # e.g. learner = COCONUT
            n = 0
            10 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)
                    mre = abs(actual - estimate) / actual
                mres[learner][n++] = mre
            print rank(mres) # some statistical tests

```

Figure 6: The experimental rig used in this paper.

2005 [45]). 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 *constricting* 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 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

### 3.1 Choice of Experimental Rig

“Ecological inference” is the conceit that what holds for all, also holds for parts of the population [43, 55]. To avoid ecological inference, our rig in Figure 6 runs separately for each data set.

Since some of our methods include a stochastic algorithm (the COCONUT algorithm of Figure 5), 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 6 is a “leave-one-out experiment”; i.e. training is conducted on all-but-one example, then tested on an “holdout” example not seen in training. This separation of

training and testing data is of particular importance in this study. As shown in Figure 2, 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 3), 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 the magnitude of the relative error; i.e.  $MRE = \frac{abs(actual - predicted)}{actual}$ . Shepperd & MacConnell [60] 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). At first, we used the Shepperd & MacConnell approach for this work but found that their straw man had orders of magnitude larger error than all the results shown here. Hence, we adopt the spirit, but not the letter, of their proposal and compare all our results against the LOC(n) “straw man” method discussed below.

### 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:

$$dist(x, y) = \sqrt{\sum_i w_i (x_i - y_i)^2} \quad (3)$$

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 from  $n > 1$  neighbors, we combines estimates via the triangle function of Walkerdien and Jeffery [63]; e.g.. for  $loc(3)$ , the estimate from the first, second, third closest neighbor with estimates  $a, b, c$  is

$$effort = (50a + 33b + 17c)/100 \quad (4)$$

Apart from the LOC “straw man”, we also compare COCOMO-II and COCONUT with CART, Knear(n), TEAK, and PEEKING2. These methods were selected, for the following reasons. TEAK and PEEKING2 represent recent innovations in effort estimation [32, 52]. CART and Knear(n) are more traditional methods that proved their value in the 1990s [59, 63]. That said, CART and Knear(n) still have currency: recent results from IEEE TSE 2008 and 2012 still endorse their use for effort estimation [16, 30, 33]. 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.

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 is averaged to generate the estimate.

Knear(n) estimates a new project’s effort by a nearest neighbor method [59]. 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 4. Knear(n) is an example of CBR; i.e. *case-based reasoning*. CBR for effort estimation was first pioneered by Shepperd & Schofield in 1997 [59]. Since then, it has been used extensively in software effort estimation [2, 26, 28–31, 37–40, 59, 63]. There are several reasons for this. Firstly, it works even if the domain data is sparse [51]. 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 [32]. TEAK’s pre-processor re-

- 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 used an accelerated principle component analysis that synthesises new attributes  $e_1, e_2, \dots$  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:

$$n = n_1 + n_2; \text{ estimate} = E_1 \frac{n_2}{n} + E_2 \frac{n_1}{n}$$

Figure 7: Inside PEEKING2 [52].

moves 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 [52] is a far more aggressive “data pruner” than TEAK and combines data reduction operators, feature weighting, and PCA. PEEKING2 is described in Figure 7.

### 3.3 Choice of Statistical Ranking Methods

The last line of our experimental rig shown in Figure 6 *rank*s multiple methods for learning effort estimators. This study ranks methods using the Scott-Knott procedure recommended by Mittas & Angelis in their 2013 IEEE TSE paper [49]. 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. E.g. for lists  $l, m, n$  of size  $ls, ms, ns$  where  $l = m \cup n$ :

$$E(\Delta) = \frac{ms}{ls} abs(m.\mu - l.\mu)^2 + \frac{ns}{ls} abs(n.\mu - l.\mu)^2$$

Scott-Knott then applies some statistical hypothesis test  $H$  to check if  $m, n$  are significantly different. If so, Scott-Knott then recurses on each division. For example, consider the following data collected under different treatments  $rx$ :

```
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]
```

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

NASA10 (new NASA data up to 2010):

| rank | treatment | median | IQR |     |
|------|-----------|--------|-----|-----|
| 1    | COCOMO-II | 42     | 35  | —●— |
| 2    | COCONUT   | 47     | 34  | —●— |
| 3    | loc(3)    | 49     | 97  | —●— |
| 3    | loc(1)    | 67     | 44  | —●— |

COC05 (new COCOMO data up to 2005):

| rank | treatment | median | IQR |     |
|------|-----------|--------|-----|-----|
| 1    | COCOMO-II | 46     | 146 | —●— |
| 1    | loc(1)    | 55     | 114 | —●— |
| 1    | loc(3)    | 65     | 99  | —●— |
| 1    | COCONUT   | 65     | 34  | —●— |

NASA93 (NASA data up to 1993):

| rank | treatment | median | IQR |     |
|------|-----------|--------|-----|-----|
| 1    | COCONUT   | 35     | 38  | —●— |
| 1    | COCOMO-II | 38     | 39  | —●— |
| 2    | loc(1)    | 62     | 54  | —●— |
| 2    | loc(3)    | 75     | 102 | —●— |

COC81 (original data from the 1981 COCOMO book):

| rank | treatment | median | IQR |     |
|------|-----------|--------|-----|-----|
| 1    | COCOMO-II | 33     | 35  | —●— |
| 1    | COCONUT   | 37     | 42  | —●— |
| 2    | loc(3)    | 80     | 237 | —●— |
| 2    | loc(1)    | 84     | 100 | —●— |

Figure 8: COCOMO vs just lines of code. MRE 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).

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  $(6^2 - 6)/2 = 15$  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 *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 [17, p220-223]. For a justification of the use of effect size tests see Shepperd&MacDonell [60]; Kampenes [27]; and Kocaguenli et al. [36]. 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?**

NASA10: (new NASA data up to 2010):

| rank | treatment | median | IQR |     |
|------|-----------|--------|-----|-----|
| 1    | COCOMO-II | 42     | 35  | —●— |
| 2    | COCONUT   | 46     | 33  | —●— |
| 3    | Knear(3)  | 50     | 77  | —●— |
| 3    | Knear(1)  | 57     | 49  | —●— |
| 3    | CART      | 61     | 32  | —●— |

COC05: (new COCOMO data up to 2005):

| rank | treatment | median | IQR |     |
|------|-----------|--------|-----|-----|
| 1    | CART      | 42     | 61  | —●— |
| 1    | COCOMO-II | 46     | 146 | —●— |
| 1    | Knear(1)  | 55     | 70  | —●— |
| 1    | Knear(3)  | 63     | 99  | —●— |
| 1    | COCONUT   | 66     | 34  | —●— |

NASA93: (NASA data up to 1993):

| rank | treatment | median | IQR |     |
|------|-----------|--------|-----|-----|
| 1    | COCONUT   | 36     | 38  | —●— |
| 1    | COCOMO-II | 38     | 39  | —●— |
| 2    | CART      | 40     | 55  | —●— |
| 3    | Knear(3)  | 54     | 66  | —●— |
| 3    | Knear(1)  | 56     | 77  | —●— |

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

| rank | treatment | median | IQR |     |
|------|-----------|--------|-----|-----|
| 1    | COCOMO-II | 33     | 35  | —●— |
| 1    | COCONUT   | 36     | 42  | —●— |
| 2    | CART      | 66     | 95  | —●— |
| 3    | Knear(3)  | 85     | 260 | —●— |
| 3    | Knear(1)  | 87     | 236 | —●— |

Figure 9: COCOMO vs standard methods. Displayed as per Figure 8.

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 8, this is not necessarily true. This figure is a comparative ranking for LOC(1) LOC(3), COCOMO-II and COCONUT. The rows of Figure 8 are sorted by the median MRE figures. These rows are divided according to their *rank*, shown in the left column: better methods have *lower rank* since they have *lower MRE* 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 8 is that just using lines of code is *not* better than parametric estimation. Also, when LOC(n) goes wrong, it goes very wrong indeed (see the COC81 results: LOC(3) produces double the median MRE error generated by COCOMO-II).

Equation 2 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. Is this absolutely right? Hence, it is hardly surprising that just using KLOC is a poor way to do effort estimation.

Another observation from Figure 8 is that, measured in terms of median MRE, COCONUT’s local calibration is not better than untuned COCOMO. In only one data set (NASA93) did COCONUT have a lower median MRE than COCOMO-II but even in that case, Scott-Knott declared there was no significant difference between the COCOMO-II and COCONUT results.

On the other hand, sometimes the local calibration results exhibited far less variance than those of COCOMO-II. For example, in Figure 8’s COC05 results, the IQR ranges for COCOMO-II and COCONUT were 146 and 34 respectively. This result (that local calibration reduces variance) repeats enough times in the subsequent experiments to make us recommend local calibration as a method for taming high variance in effort estimation.

NASA10 (new NASA data up to 2010):

| rank | treatment | median | IQR |   |
|------|-----------|--------|-----|---|
| 1    | COCONUT   | 34     | 14  | ● |
| 1    | COCOMO-II | 43     | 35  | ● |
| 2    | TEAK      | 73     | 80  | ● |
| 2    | PEEKING2  | 74     | 51  | ● |

COC05 (new COCOMO data up to 2005):

| rank | treatment | median | IQR |   |
|------|-----------|--------|-----|---|
| 1    | COCOMO-II | 46     | 134 | ● |
| 1    | COCONUT   | 62     | 38  | ● |
| 1    | TEAK      | 84     | 110 | ● |
| 2    | PEEKING2  | 87     | 140 | ● |

NASA93 (NASA data up to 1993):

| rank | treatment | median | IQR |   |
|------|-----------|--------|-----|---|
| 1    | COCONUT   | 36     | 38  | ● |
| 1    | COCOMO-II | 39     | 39  | ● |
| 1    | TEAK      | 50     | 81  | ● |
| 2    | PEEKING2  | 65     | 165 | ● |

COC81 (original data from the 1981 COCOMO book):

| rank | treatment | median | IQR |   |
|------|-----------|--------|-----|---|
| 1    | COCOMO-II | 32     | 33  | ● |
| 1    | COCONUT   | 33     | 42  | ● |
| 2    | TEAK      | 93     | 128 | ● |
| 3    | PEEKING2  | 131    | 569 | ● |

Figure 10: COCOMO vs newer methods. Displayed as per Figure 8.

## 4.2 COCOMO vs Other Methods

This section explores **RQ2: has parametric estimation been superseded by more recent estimation methods?** and **R3: Are the old parametric tunings irrelevant to more recent projects?**

Figure 9 compares COCOMO and COCONUT with traditional effort estimation methods from the 1990s (CART and Knear(n)). In that comparison, nothing was ever ranked better than COCOMO-II (sometimes CART or COCONUT had a slightly lower median MRE but that difference was small:  $\leq 4\%$ ).

Figure 10 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 usually ranked equal to COCOMO-II. In several case COCOMO-II and COCONUT were ranked first and second but the median difference in their scores is very small: see NASA10 of Figure 9 and NASA93, COC81 of Figure 10. Also, many other methods often had much larger variances. Hence, we can recommend some form of local calibration as a variance reduction tool (e.g. compare COCONUT with COCOMO-II in COC05 of Figure 10).

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 200 (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

One 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.

To do this, the tunings table of Figure 4 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 |
|         | <i>below</i> |      |         | <i>above</i> |       |       |

### 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 [47] 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  $E(\sigma, a_i) = \sum_{x \in a_i} (n/N * v(r(x)))$

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 8, LOC was excluded from column reduction.

### 4.3.4 Results

Figure 11 compares results found when use either *all* or some *reduce* set of ranges, rows, and columns. 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.

In Figure 11, all the *r4* results are ranked comparatively worse than the other treatments. That is, these results do not condone learning from just four projects.

On the other hand Figure 11 suggests that it is defensible to learn a COCOMO model from eight projects. All the *r8* results are top-ranked with the exception of the COC81 results (but even there, the absolutely difference between the top *r8* results are standard COCOMO is very small).

Overall, Figure 11 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



NASA10 (new NASA data up to 2010):

| rank | treatment         | median | IQR |     |
|------|-------------------|--------|-----|-----|
| 1    | COCOMO-II         | 42     | 35  | —●— |
| 1    | COCONUT:c*0.5,r=8 | 43     | 35  | —●— |
| 2    | COCONUT           | 46     | 34  | —●— |
| 3    | COCONUT:c1,r4     | 48     | 41  | —●— |
| 3    | COCONUT:c1,r8     | 50     | 33  | —●— |
| 3    | COCONUT:c0.25,r8  | 51     | 35  | —●— |
| 3    | COCONUT:c0.5,r4   | 53     | 38  | —●— |
| 3    | COCONUT:c0.25,r4  | 57     | 41  | —●— |

COC05 (new COCOMO data up to 2005):

| rank | treatment        | median | IQR |     |
|------|------------------|--------|-----|-----|
| 1    | COCOMO-II        | 46     | 146 | —●— |
| 1    | COCONUT:c0.5,r8  | 51     | 58  | —●— |
| 1    | COCONUT:c0.25,r8 | 61     | 56  | —●— |
| 1    | COCONUT:c0.5,r4  | 61     | 58  | —●— |
| 1    | COCONUT          | 68     | 34  | —●— |
| 1    | COCONUT:c1,r4    | 64     | 60  | —●— |
| 1    | COCONUT:c1,r8    | 74     | 45  | —●— |
| 1    | COCONUT:c0.25,r4 | 80     | 58  | —●— |

NASA93 (NASA data up to 1993):

| rank | treatment        | median | IQR |     |
|------|------------------|--------|-----|-----|
| 1    | COCONUT          | 36     | 38  | —●— |
| 1    | COCOMO-II        | 38     | 39  | —●— |
| 1    | COCONUT:c0.5,r8  | 44     | 53  | —●— |
| 1    | COCONUT:c0.5,r4  | 49     | 61  | —●— |
| 1    | COCONUT:c0.25,r4 | 52     | 67  | —●— |
| 2    | COCONUT:c1,r8    | 52     | 61  | —●— |
| 2    | COCONUT:c1,r4    | 54     | 70  | —●— |
| 2    | COCONUT:c0.25,r8 | 55     | 52  | —●— |

COC81 (original data from the 1981 COCOMO book):

| rank | treatment        | median | IQR |     |
|------|------------------|--------|-----|-----|
| 1    | COCOMO-II        | 33     | 35  | —●— |
| 1    | COCONUT          | 37     | 42  | —●— |
| 2    | COCONUT:c1,r8    | 45     | 44  | —●— |
| 3    | COCONUT:c0.5,r8  | 59     | 43  | —●— |
| 3    | COCONUT:c0.25,r8 | 61     | 51  | —●— |
| 4    | COCONUT:c1,r4    | 76     | 60  | —●— |
| 4    | COCONUT:c0.5,r4  | 78     | 30  | —●— |
| 4    | COCONUT:c0.25,r4 | 82     | 65  | —●— |

Figure 11: COCOMO vs simpler COCOMO. Displayed as per Figure 8.

point scale will suffice: *below*, *nominal*, *above*. As to how much data is required for modeling, the results from COCONUT:c0.5,r8 are ranked either the same as COCOMO-II or (in the case of COC81) fall very close to the median and IQR seen for COCOMO-II. That is, a mere 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

That said, some column pruning should be employed when working with very small training sets (e.g. the eight rows used in Figure 11. Note that in all data sets that generated multiple rankings, the *c1* results (that used all the attributes) were not top-ranked. That is, in a result that might have been predicted by Miller or Chen et al., when working with just a few rows it is useful to reflect on what columns might be ignored.

#### 4.4 COCOMO with Incorrect Size Estimates

This section explores **RQ5: Are parametric estimates unduly sensitive to errors in the size estimate?**

Before endorsing a KLOC-based estimation method, it is important to understand the effects of noise within the KLOC samples. Test projects to be estimated may have noisy KLOC values if the development team incorrectly guessed the size of the code. Training data may have noisy KLOC for many reasons such as

- How was reused code accounted for in the KLOC?
- Was LOC measured from end-statement or end-of-line symbols?
- Or how were lines of comments handled?

Another factor that introduces noise into training and test data are

NASA10 (new NASA data up to 2010): c

| rank | treatment            | median | IQR |     |
|------|----------------------|--------|-----|-----|
| 1    | COCOMO-II            | 42     | 35  | —●— |
| 1    | COCONUT:c*0.5,r=8n/4 | 46     | 36  | —●— |
| 1    | COCOMO-II n/2        | 50     | 47  | —●— |
| 2    | COCONUT:c*0.5,r=8    | 54     | 34  | —●— |
| 2    | COCONUT:c*0.5,r=8n/2 | 55     | 45  | —●— |
| 2    | COCOMO-II n/4        | 58     | 45  | —●— |

COC05 (new COCOMO data up to 2005):

| rank | treatment            | median | IQR |     |
|------|----------------------|--------|-----|-----|
| 1    | COCOMO-II            | 46     | 146 | —●— |
| 1    | COCONUT:c*0.5,r=8n/2 | 60     | 53  | —●— |
| 1    | COCOMO-II n/2        | 63     | 203 | —●— |
| 1    | COCONUT:c*0.5,r=8n/4 | 65     | 42  | —●— |
| 1    | COCONUT:c*0.5,r=8    | 77     | 51  | —●— |
| 2    | COCOMO-II n/4        | 110    | 280 | —●— |

NASA93 (NASA data up to 1993):

| rank | treatment            | median | IQR |     |
|------|----------------------|--------|-----|-----|
| 1    | COCOMO-II            | 38     | 39  | —●— |
| 1    | COCOMO-II n/2        | 41     | 43  | —●— |
| 1    | COCOMO-II n/4        | 47     | 45  | —●— |
| 1    | COCONUT:c*0.5,r=8n/2 | 54     | 50  | —●— |
| 1    | COCONUT:c*0.5,r=8    | 57     | 57  | —●— |
| 1    | COCONUT:c*0.5,r=8n/4 | 58     | 45  | —●— |

COC81 (original data from the 1981 COCOMO book):

| rank | treatment            | median | IQR |     |
|------|----------------------|--------|-----|-----|
| 1    | COCOMO-II            | 33     | 35  | —●— |
| 2    | COCOMO-II n/2        | 47     | 46  | —●— |
| 2    | COCOMO-II n/4        | 48     | 44  | —●— |
| 2    | COCONUT:c*0.5,r=8n/2 | 53     | 45  | —●— |
| 2    | COCONUT:c*0.5,r=8n/4 | 57     | 44  | —●— |
| 2    | COCONUT:c*0.5,r=8    | 65     | 51  | —●— |

Figure 12: LOC noise results. MRE values. Displayed as per Figure 8.

systems built from multiple languages. To make estimates from those kind of systems, KLOC in one language needs to be translated (in a possibly incorrect way) to KLOC in another language.

In theory, the problem of noisy KLOC measures seem particularly acute in our work. The core of COCOMO is an estimate that is exponential on KLOC. This means that KLOC will be magnified in a non-linear way. Also, if we train on just eight rows, as proposed above, then any noise in that small training data could be highly detrimental to the estimation process.

To check the effects of noise, we repeated the reduction experiments of the last section while also injecting noise into the KLOC values. That is, as above, (1) the ranges were reduced to three; (2) half the columns were reduced; (3) we trained on only eight randomly selected projects; and (4) prior to train and test, all KLOC values were adjusted to

$$KLOC = KLOC * ((1 - n) + (2 * n * r))$$

where  $n \in \{0.25, 0.5\}$  is the level of noise we are exploring and  $r$  is a random number  $0 \leq r \leq 1$ .

In Figure 12, any result marked with  $n/2$  or  $n/4$  shows what happens when the KLOCs were varied by 50% or 25% respectively. In only one case (COC81) were the noisy results statistically different from using data without noise. That is, the parametric estimation method being recommended here is not unduly effected by noise where the KLOC values vary up to 50% of their original value.

## 5 Discussion

From the above, there are several open issues. Firstly, how can an estimation method based on lines of code be immune to errors in measures to those lines of code? Secondly, all the above assumes that it is possible to collect project data using the COCOMO attributes of Figure 3. What if that is not true?



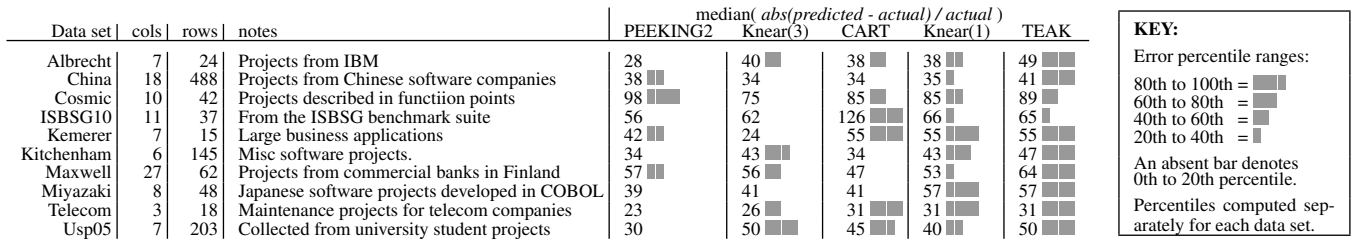


Figure 13: Median errors seen in leave-one-out studies on non-COCOMO data sets. Gray bars show error discretized into 20th percentile ranges from min to max. The PEEKING2 learner has fewest error bars; i.e. it is the best learner seen in this sample. All data available from <http://openscience.us/repo/effort>.

## 5.1 KLOC Noise Immunity

At first glance, the above KLOC noise immunity result seems strange. However, it can be shown that this result can be explained by the underlying theory of COCOMO. Equation 2 commented that there is much more to effort estimation than just KLOC. Recall that that equation showed how effort varies for a given KLOC. While KLOC was a factor in that equation, all the other COCOMO variables have a large influence (by a factor on 856) on effort estimation. This means that even if there are errors in the KLOC measure, then the other COCOMO variables can “step in” to comment on effort estimation. for that second component

Another thing to be said about Figure 12 is that that noise does degrades predicted performance to some degree (observe how the non-noisy result from COCOMO-II always have better medians that once noise is injected). So it is not true to say that KLOC noise has no effect on effort estimation. That said, it should also be noted that statistically, the size of that effect is usually very small or not a significant (as defined by the statistical tests of §3.3). This could mean that our statistical tests are failing. However, looking at the large variances of the COCOMO-II results in Figure 12, another explanation might be that that large variances seen with COCOMO-II shows the inherent imprecision of effort estimation.

These large variances are an important and open issue in effort estimation. It is hard to make precise statements about effort estimation when the estimates themselves are so imprecise. Perhaps the research community should spend a few years working on variance reduction, after which time, they can better explore reducing median error values.

## 5.2 For Non-COCOMO Data

This study was highlighted the benefits of estimating projects using COCOMO. Shepperd [58] notes that one of the disadvantages of COCOMO is that it demands projects be described in terms of the attributes of Figure 3. This is an issue since effort estimation often requires reflecting on numerous projects, some of which may no longer be active. For such historical projects, it can be difficult to uncover information such as Figure 3. Hence, it is appropriate to consider what to do for non-COCOMO projects.

Accordingly, we applied all the learners described above to the ten data sets of Figure 13. In terms of this discussion, the key feature of these data sets is COCOMO and COCONUT could not be applied to these since, apart from some effort measure, this data is described using attributes with little (if any) similarity to Figure 3.

The results of that application were not as clear cut as the results shown in Figure 8 to Figure 12. Nearly all the results had the form of Figure 14— the median results are similar and the IQR variances are large enough to make Scott-Knott declare that all learned have the same  $rank=1$ . This “all learners earned the same ranking” was repeated over all our non-COCOMO data sets. The reason for this “same-rank” effect was mentioned above: it is hard to make precise statements about effort estimation when the estimates themselves

Telecom: Maintenance projects for telecommunications.

| rank | treatment | median | IQR |  |
|------|-----------|--------|-----|--|
| 1    | PEEKING2  | 23     | 34  |  |
| 1    | Knear(3)  | 26     | 33  |  |
| 1    | TEAK      | 31     | 28  |  |
| 1    | CART      | 31     | 22  |  |
| 1    | Knear(1)  | 31     | 35  |  |

Figure 14: Example result of applying learners to non-COCOMO data.

are imprecise. Once again, we call on the SE research community to spend more time of the variance problem in effort estimation.

Given that the quantitative statistics analysis of §3.3 could not rank learners for non-COCOMO data, we turn to a more qualitative statement. The right-hand-side columns of Figure 13 shows median errors seen in leave-one-out experiments on our ten non-COCOMO data sets. The gray bars of that figure show the median error results of a leave-one-out study discretized into 20th percentile ranges from min to max (so the *more* the gray bars, the *worse* the learner). On these non-COCOMO data sets, standard linear regression usually performed worse and PEEKING2 usually performed best. Hence, for non-COCOMO data sets we recommend PEEKING2 (but stress this qualitative recommendation is not as strong as our previous endorsement of COCOMO/COCOUNT, which can be defended via a more rigorous statistical analysis).

Why is PEEKING2 best for non-COCOMO data and not for COCOMO data? The answer, we conjecture, is that model-based effort estimation involves *data collection* followed by *model generation*. Perhaps if the data collection is very broad and covers all aspects of a project such as the process, product, personnel, platform information collected by COCOMO, then the subsequent model generation can be very simple (just the COCOMO equation of Figure 4). On the other hand, if data collection is more myopic and focuses on just the data that is serendipitously available, then the subsequent model generation task is more challenging. For such challenging tasks, we would expect:

- The generator needs to be sophisticated; e.g. not something as simplistic as the linear regression used in Figure 13 (hence, PEEKING2 did well in that figure);
- No matter how sophisticated the model generator, it may not perform well as models generated from a broader range of data (hence, PEEKING2 performed worse than COCOMO in Figure 10).

## 6 Threats to Validity

The above results were based with certain settings for some experiments on some data. For example, in the previous section, we used noise at levels 25% and 50%. Clearly, these results may not hold if a wider range of settings for (e.g.) noise are explored.

Another 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.

Questions of validity also 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.

## 7 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.

We found that:

- **RQ1:** just using LOC for estimation is far worse than 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);
- **RQ5:** Parametric estimation methods like COCOMO that assume effort is exponential on lines of code are *not* unduly sensitive to errors in the LOC measure (see §4.4);

Hence, we conclude that in 2015, it is still a valid and recommended practice to *first* try parametric estimation, perhaps augmented with a local calibration method like COCONUT and column pruner of §4.3.3. One reason to endorse COCOMO-style estimation comes from the row reduction experiments of §4.3.2. Using COCONUT (plus some column reduction), we found that adequate estimates can be generated using just a handful of prior projects. In these experiments, eight projects were enough for COCONUT (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.

There is one more reason to strive to collect COCOMO-like project data and then perform COCOMO-style estimation. Our results show that COCOMO's performance is usually ranked better than other estimation methods. The same cannot be said for non-COCOMO data where, in §5.2, we found it difficult to make a rigorous statistical case than learnerX was better than learnerY.

## 8 Future Work

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 multicore 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, during 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 [20]. Finally, the emerging community-based

software development, i.e. software crowdsourcing [25], 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 modes and to gather data to calibrate models for dealing with them are underway. Contributors to the definition and calibration are most welcome.

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