4
CHAPTER

Evaluation of Text Entry Techniques

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4.1 INTRODUCTION

New methods of text entry appear with great frequency these days. This is due in large part to the recent and heightened interest in pen-based computing, mobile computing, and short message service (SMS) messaging on mobile phones. The statistics for SMS messaging are particularly staggering: world volumes are now in excess of 1 billion messages per day (*Economist,* 2004)! In that such quantities of text are entered using the small and limited keypad on mobile phones, it is no wonder that researchers are investigating new techniques for the efficient entry of text.

However, there is an unfortunate reluctance on the part of researchers to engage the user community as a final and essential step in the creative process. Great ideas too often remain inadequately tested or untested. To simply invent a new text entry technique and then declare it is fast, efficient, and easy to learn, without testing such claims with users, falls short of the standards of research expected today.

The focus in this chapter is the evaluation of text entry techniques. Our goal is, first, to build a case for the need for evaluation and, second, to lay out strategies and methodologies to do it. We begin with two examples from the literature in which evaluations were not performed, but where a need was clearly evident.

In reviewing a new and interesting text entry technique in 1988, Potosnak wrote, "no tests (other than analyses comparing the number of wipes to number of keystrokes required on traditional keyboards) have been reported to determine performance or preference effects with this keyboard" (Potosnak, 1988, p. 491). The design referred to is the *Wipe-Activated Keyboard,* patented in 1980 and presented in *IEEE Computer* in 1982 (Montgomery, 1980, 1982). An evaluation was not reported, and this deficiency in the research did not escape the scrutiny of Potosnak. The idea, shown in Fig. 4.1, was for a keyboard with a flat touch-sensitive surface and no moving parts. The design also included a nonstandard letter arrangement to facilitate an

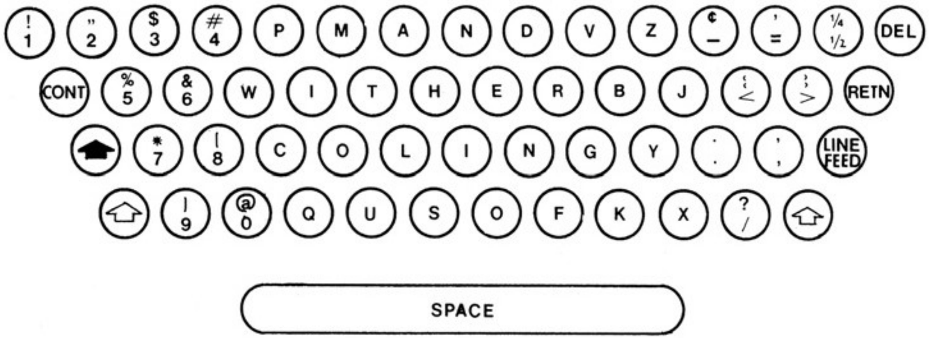


FIGURE 4.1 Montgomery Wipe-Activated Keyboard (from Montgomery, 1982).

alternative interaction: wiping. Letters may be entered in the usual way—by touch-tapping keys with one's finger—or with wiping gestures where consecutive letters are neighbors.

The letters were arranged to maximize opportunities for wipe-activated input for the most common words in English, as verified in Fig. 4.1 for THE, OF, AND, TO, IN, IS (one wipe each); THEY, THEIR, THEM (one wipe plus one tap each); and so on. Despite building a working prototype, the inventor acknowledged that the design was "yet to be tested" (Montgomery, 1982, p. 18)[1].

The example above is by no means unique. More recently, within the discipline of human–computer interaction (HCI), *Quikwriting* was developed as a gesture-based text entry technique using a stylus (Perlin, 1998). The idea involves a 3 × 3 grid on which characters are entered with gestures beginning in a central home zone and moving through one or two adjoining zones, returning to the home zone (see Fig. 4.2). The symbols in the top center and bottom center zones invoke different modes. Letters that occur more frequently in English are given the shortest gestures. For example, "i" in Fig. 4.2 is entered by moving in and out of the bottom right zone. Less frequent letters have longer gestures ("k" requires a move into the upper left zone, then across to the upper right zone, and finally back to home).

The inventor wrote, "while we have yet not done systematic user testing, anecdotal experience to date is consistent: Users well practiced in both *Graffiti* and *Quikwriting* consistently find the latter to be about three times faster" (Perlin, 1998, p. 216)[2]. "Three times faster" combined with the "quick" self-reference in Quikwriting leaves

[1] To this author's knowledge, no user test has ever been performed on this keyboard, nor has it appeared in any commercial product.

[2] Graffiti is the stylus-based single-stroke per symbol text entry method used on Palm's Personal Digital Assistants (www.palm.com).

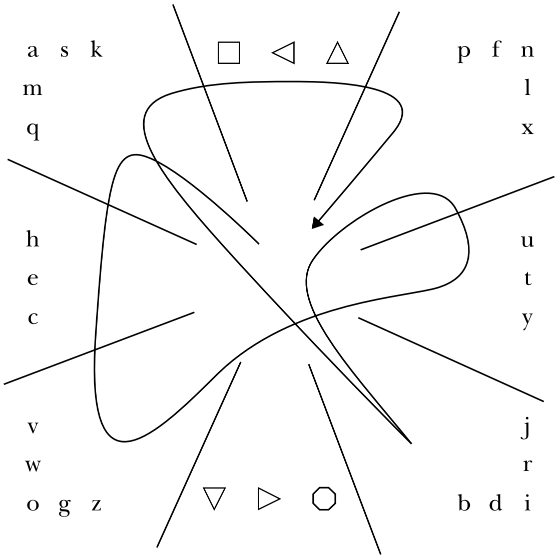


FIGURE 4.2 Quikwriting input of “quick” (based on Perlin, 1998).

little doubt as to the promise of this text entry technique. Yet no formal or informal empirical results were given. A follow-on, independent evaluation of Quikwriting was performed, however, with walk-up entry rates measured at just 4 words per minute (WPM) (Isokoski & Raisamo, 2004). Entry speed increased to 16 WPM, but only after 5 hours of practice. Considering that human handwriting speeds are 20+ WPM (Bailey, 1996, p. 287), “quick” is not a word that comes to mind for Quikwriting. Empirical tests of Graffiti, on the other hand, report entry speeds in the range of 7–10 WPM initially, with rates rising above 20 WPM with practice (Fleetwood *et al.,* 2002; Koltringer & Grechenig, 2004; MacKenzie *et al.,* 2006). It remains a challenge to reconcile the just-cited empirical results with the anecdotal report in the original publication.

What do you think of Quikwriting or the Wipe-Activated Keyboard? They are interesting ideas, for sure. Will they support fast and accurate text entry? Will they do so with modest practice or do they require prolonged practice? What are the expected entry speeds or error rates? Will users like the techniques or will they find them frustrating? How do they compare to alternative text entry techniques? Are there aspects of the designs that could be modified in some way to improve performance? The questions just posed are the central motivation in this chapter. While analysis through modeling might assist in predicting answers to these questions (Card *et al.*, 1978, 1980; MacKenzie, 2003), the final, incontestable answer lies in the community of people that ultimately employ a technique: users. Waiting for the collective response of the public following deployment of a product is risky, albeit not unheard of

(Cooper, 1999)[3]. A more reliable and safer approach is to engage a subset of the user community as participants in an empirical evaluation of the text entry technique in question. Let us see what this entails.

4.2 COMPARATIVE EVALUATION OF TEXT ENTRY TECHNIQUES

A word has crept into the title of this section that is missing in the chapter's title: "comparative." Indeed, evaluation in the absence of comparison is of limited value. When a new text entry technique is proposed, it is usually offered as an improvement over an existing technique, e.g., a new method for handwritten input or a new keyboard for keyed input. At the very least, comparing a new technique with an established technique with known performance characteristics serves as a check on the methodology. That is, if a similar and accepted result is reported for an established technique, then a differential result for a new technique in the same evaluation is given greater credence than if the latter result were reported alone. A slightly different motivation is simply to establish which of two or more design alternatives is better. In any event, a comparative evaluation is recommended.

The framework for a comparative evaluation is an experiment conforming to established standards and methods in the field of experimental psychology. After all, we are dealing with humans interacting with technology. In a sense, HCI is the beneficiary of this more mature field with a history of refined methods of experimentation involving humans. Answering research questions, such as in the preceding section, using a concocted methodology, customized and tailored as might seem reasonable, falls short of good research. In experimental psychology, it is sometimes stated that research questions must pass the "ROT" test (Martin, 2004). Questions must be repeatable, observable, and testable. "Repeatable" refers to the need to describe the apparatus and procedure with sufficient detail to allow others to repeat the research, to verify or refute results. "Observable" refers to the need to test human responses that are directly measurable, such as the time to enter a phrase of text. "Testable" refers to the need to pose questions about numbers; e.g., is the speed of entry faster for one technique than another? Meeting the testable criterion is more difficult for research questions of a qualitative nature, since users' feelings, opinions, or attitudes are not directly measurable. In this case, the accepted approach is to solicit responses from users via a questionnaire. If the responses use a numeric rating scale, e.g., from 1, strongly agree, to 5, strongly disagree, then the testable criterion is met by analyzing the coded responses.

[3] The mockery made of the handwriting recognition on Apple's Newton by cartoonist Garry Trudeau in *Doonesbury* is a specific example (see MacKenzie & Soukoreff, 2002b).

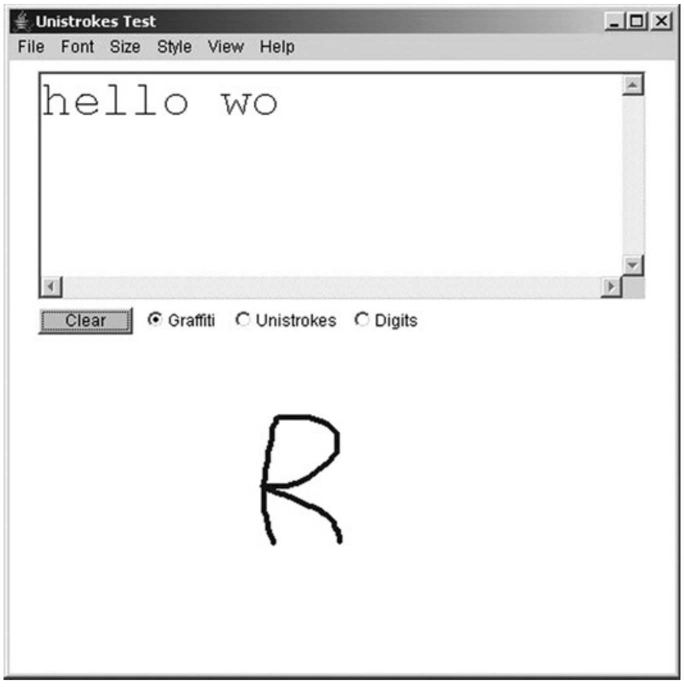
In the following sections, the steps in performing a comparative evaluation of a text entry technique are given.

4.2.1 Preexperimental Testing

The first order of business is to build a working prototype. With a working prototype, the user enters text, with the result appearing for inspection on a display. For research, it is best to keep the design separate from target applications such as e-mail clients or word processors. An example of a handwriting recognition prototype system, created by the author, is shown in Fig. 4.3.

Once a prototype is operational, substantial preexperimental testing is possible—to rid the code of bugs and to refine the design until satisfactory performance is achieved. Since entry speed is the performance variable of greatest interest, it is important to get a rough sense of the technique's potential early on. A good way to do this is to measure the time to enter a phrase of text and to compute entry speed in words per minute. A common phrase for this purpose is

the quick brown fox jumps over the lazy dog



hello wo

● Graffiti ○ Unistrokes ○ Digits

FIGURE 4.3 A working prototype. In this case, text is entered using handwriting recognition with results appearing in a text field in the prototype software.

The phrase contains 43 letters, including spaces, and, notably, each letter of the English alphabet. How fast can this phrase be entered? No need to get fancy at this stage. Timing with a wall clock or wristwatch will suffice. If the phrase is entered in, say, 45 seconds, then the entry speed is $(43/5)/(45/60) = 11.4$ WPM. Dividing the number of characters by 5 yields "words," and dividing the number of seconds by 60 yields "per minute[4]."

Such a cursory evaluation as just described exceeds that reported in the two papers cited at the beginning of this chapter, and, arguably, there is no excuse for this. Even a short paper or poster presentation should give at least preliminary expectations in terms of text entry speeds achievable with simple tests as just described.

As a precaution, we note that the inventor or researcher is so close to the design and interaction requirements that the entry rates achieved by that individual may not reflect those achievable by a broader community of users. Therefore, it is recommended to have arms-length users try the technique. The results may be surprising!

The prototype in Fig. 4.3 is actually software running on a desktop computer. While such mock-ups are relatively simple to implement and convenient to test, the results obtained are limited. Certainly, the data are better than no data at all; but, it is important to remember that the physical affordances of the intended device may be very different (e.g., a mobile phone or PDA). Testing, at some point, should be performed on a device similar to the final product in shape, size, weight, and so on. In the same vein, we, as researchers, need to be aware of the important role of aesthetic qualities in the overall success of products. It is not just entry speed!

4.2.2 The Evaluation Task

Early on in the development and testing of the prototype, focus shifts to the ensuing experimental evaluation. An important consideration is the task: will users create their own text or will they copy text provided to them? The distinction is important. Although text creation is closer to typical usage and, thus, improves the external validity of the experiment, this approach generally is not appropriate for an empirical evaluation. This is explained as follows.

First, we introduce "focus of attention" (FOA). Consider an expert touch-typist using a Qwerty keyboard to copy text from a nearby sheet of paper. This is a text-copy

[4] It has been a convention since about 1905 to standardize the computation of text entry speed in "words per minute," in which a word is defined as five keystrokes (Yamada, 1980, p. 182). This includes letters, spaces, punctuation, and so on. Although some researchers use actual word size (e.g., James & Reischel, 2001), this is inappropriate since it allows entry speed to be inflated by testing with phrases containing predominantly short words. It also hinders comparisons between languages with different mean word sizes.

task. Because the typist is an expert, she attends to only the source text. This is a single FOA task. However, if input is via a stylus and soft keyboard, the typist must also attend to the keyboard because a soft keyboard cannot be operated "eyes free." Therefore, stylus typing in a text-copy task, even for expert users, is a two-FOA task. If the typist is less than expert and corrects errors, she must look at the display to monitor results. This is now a three-FOA task: the typist attends to the display, to the keyboard, and to the source text. Clearly, the feedback channel is overburdened in a three FOA task.

So, why are text-copy tasks preferred over text-creation tasks if the former increase the attention demand? There are several reasons. One is that text creation tasks are likely to include behaviors not required of the interaction technique per se. Examples include pondering ("What should I enter next?") or secondary tasks (e.g., fiddling with system features). The measurement of text entry performance—particularly speed—is compromised if such behaviors are present.

A second difficulty with text-creation tasks is identifying errors—it is difficult to know exactly what was intended if the text is generated by the user. Even if the message content is known a priori, errors in spelling or memory recall may occur, and these metalevel mistakes are often indistinguishable from errors due to the interface itself.

A third difficulty is the loss of control over the distribution of letters and words entered. The task should require the subject to enter a representative number of occurrences of characters or words in the language. However, it is not possible to control for this if the subject is generating the text.

The main advantage of a text-creation task is that it mimics typical usage. The disadvantages just cited, however, are significant enough that most researchers choose a text-copy task, despite the increased FOA. One way to mitigate the effects of increased FOA is to dictate the source text through the audio channel, although other compromising issues arise (Ward *et al.,* 2000).

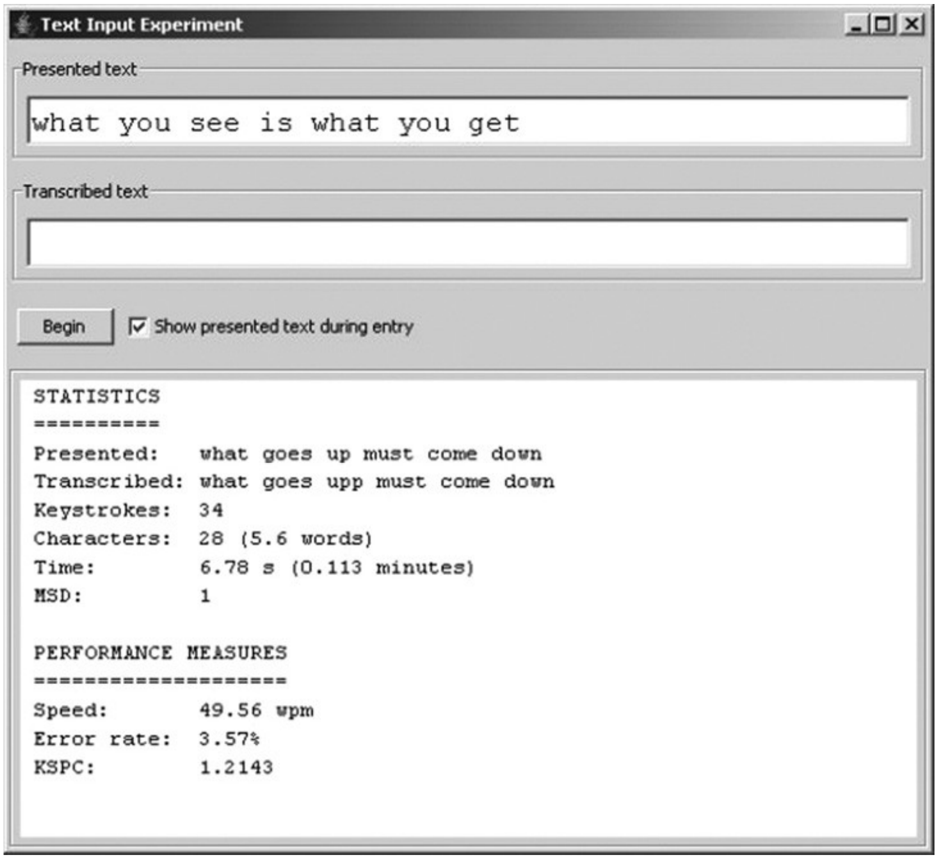
A carefully designed experiment may capture the strengths of both a text-creation task and a text-copy task. One technique is to present subjects with short, easy-to-memorize phrases of text. Subjects are directed to read and memorize each phrase before entering it. Memorization is enforced by hiding the source text during entry. Entry in this manner benefits from the desirable property of a text-creation task, namely, reduced FOA. As well, the desirable properties of a text-copy task are captured, that is, control over letter and word frequencies and measurement of behaviors limited to the text-entry task. There are numerous examples of this approach in the literature (e.g., Alsio & Goldstein, 2000; MacKenzie *et al.,* 1994; MacKenzie & Zhang, 1999; Rau & Skiena, 1994).

As for the choice of text to enter, a custom set of phrases may be used, for example, as appropriate for non-English users (Majaranta *et al.,* 2003), or a standard set of phrases may be employed. MacKenzie and Soukoreff (2003) developed a phrase set for this purpose. The set contains 500 short phrases of English text. Phrases vary from 16 to 43 characters (mean length 28.6 characters). The letter frequency correlation with an English corpus is high at $r = 0.954$. This phrase set, sometimes with a few modifications, has proven useful, as other researchers have used it in their evaluations (Agarwal & Simpson, 2005; Gong *et al.,* 2005; Hwang *et al.,* 2005; Koltringer & Grechenig, 2004;

Lyons *et al.,* 2004; Read, 2005; Spakov & Miniotas, 2004; Wigdor & Balakrishnan, 2004; Wobbrock *et al.,* 2004).

4.2.3 Data Collection and Display

Assuming the experimental task is the input of phrases of text, the prototype software must be modified to include the phrases, perhaps reading them from a file, and presenting them to users for input. Random selection and presentation are probably the best choice. It is at this stage that the software should include data collection and display. An example created by the author is shown in Fig. 4.4. The top field contains a



Presented text: what you see is what you get

Transcribed text:

```
STATISTICS
==========
Presented:   what goes up must come down
Transcribed: what goes upp must come down
Keystrokes:  34
Characters:  28 (5.6 words)
Time:        6.78 s (0.113 minutes)
MSD:         1

PERFORMANCE MEASURES
====================
Speed:       49.56 wpm
Error rate:  3.57%
KSPC:        1.2143
```

FIGURE 4.4 User interface for a text-entry experiment. Data are collected with performance statistics displayed following each entered phrase. Note that the statistics and performance measures are for the previous phrase entered and appear after ENTER is detected, signaling the end of the phrase.

phrase of text to enter, the “presented text.” Below that is the “transcribed text.” An optional checkbox toggles between hiding and showing the presented text during entry.

The software for the interface in Fig. 4.4 is not tied to any particular text entry method. Some customization is needed depending on the method’s input mechanism and output display. Variations on this approach appear in the form of screen snaps in several published evaluations (e.g., MacKenzie, 2002; MacKenzie *et al.,* 2001; Oniszczak & MacKenzie, 2004; Pavlovych & Stuerzlinger, 2003; Soukoreff & MacKenzie, 2003). A distinguishing feature of the software is the immediate presentation of performance data upon detection of ENTER at the end of each entered phrase.

The performance measures in Fig. 4.4 are for entry speed (words per minute), error rate (%), and keystrokes per character (the number of keystrokes divided by the number of characters produced). These are important summary statistics but others may be as important for different entry methods. Showing these in a pop-up or integrated window after each phrase facilitates demonstration of a text entry method and also helps in an experiment, for example, to maintain the interest and motivation of users.

The data for the statistics and performance measures in Fig. 4.4 were gathered by the experimental software and saved in a file for subsequent analyses. For this purpose, it is useful to save two streams of data. Figure 4.5 shows an excerpt of an “sd1” file, wherein low-level, raw data are written (“sd” is for “summary data”). For each phrase, both the presented and the transcribed text are shown as a time stamp and key code for each keystroke. Of course, this file may be customized as appropriate for the entry technique. In an eye typing experiment, for example, we found it useful also to log time stamps for “read-text events”—shifts in the point of gaze to review the transcribed text field (Majaranta *et al.,* 2003).

We save higher level summary data in an “sd2” file, in which each line has summary statistics on a per-phrase basis, rather than on a per-keystroke basis. In Fig. 4.6, the variables are shown in a header line, with the measurements following in a series of lines, one per phrase entered in a block of testing.

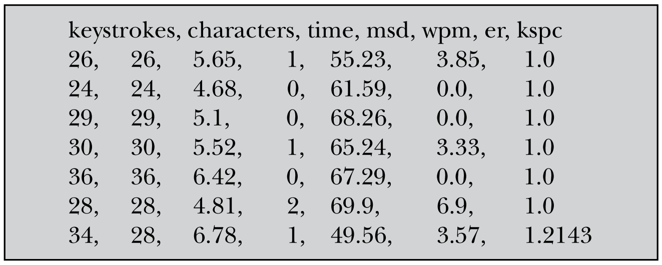
It is also useful to add columns in the data files to identify the participant, the entry technique, the block number, and so on, although this information also may be coded in the file name.

4.3 EXPERIMENT DESIGN

At this point, we have a prototype that adequately implements the text entry technique of interest and perhaps some variations on the technique. The prototype also includes mechanisms to present phrases of text to users, to collect data, and perhaps to display summary results after each phrase of entry. It is time to experimentally evaluate the text entry technique and compare it with design alternatives. In this section we identify the steps for this.

```
what goes up must come down
what goes upp must come down
0     w
130   h
240   a
350   t
510   space
841   g
941   o
1071  e
1241  s
1412  space
1642  u
1872  p
2133  p
2533  space
2744  m
2944  u
3024  s
3154  t
3485  space
3945  d
4086  o
4296  m
4777  backspace
4937  backspace
5067  backspace
5227  c
5337  o
5468  m
5668  e
5828  space
5998  d
6098  o
6309  w
6509  n
6779  enter
#
```

FIGURE 4.5 Example summary data file for one phrase (sd1). The first two lines contain the presented and transcribed text, respectively. The remaining lines contain time stamps (in milliseconds) and key codes. Note the presence of presses of BACKSPACE to correct input. The data are for the phrase in the bottom of Fig. 4.4.



```
keystrokes, characters, time, msd, wpm, er, kspc
26,    26,    5.65,     1,   55.23,      3.85,     1.0
24,    24,    4.68,     0,   61.59,      0.0,      1.0
29,    29,    5.1,      0,   68.26,      0.0,      1.0
30,    30,    5.52,     1,   65.24,      3.33,     1.0
36,    36,    6.42,     0,   67.29,      0.0,      1.0
28,    28,    4.81,     2,   69.9,       6.9,      1.0
34,    28,    6.78,     1,   49.56,      3.57,     1.2143
```

FIGURE 4.6 Example summary data file for one block of phrases (sd2). The first line is a header identifying the summary statistics in subsequent lines. The last line shows the summary statistics for the phrase in Fig. 4.4.

4.3.1 Participants

Users tested in an experiment are called "participants[5]." Most empirical evaluations of text entry techniques include a dozen or so participants, although the exact number to use is debatable. If too few participants are used, large performance differences may occur, but these may not be statistically significant. If too many participants are used, small performance differences of no practical interest may show statistical significance. The best advice, perhaps, is to study the literature and use approximately the same number of participants as found in other research using a similar methodology (Martin, 2006, p. 215).

Ideally, participants are selected randomly from the population of people in the intended community of users. In practice, participants are solicited from the pool of people conveniently available, for example, students at a local university or colleagues at work. Obviously, if the research is aimed as a specific community, such as children or the handicapped, then participants must be solicited accordingly. In some cases, prescreening is necessary if, for example, users with a specific level of expertise or experience with aspects of the interaction are required. This includes having no prior exposure, if such is important in the evaluation. In any event, it is customary to describe the participants by collecting and reporting demographic data in the form of counts, means, or ranges. Examples include age, gender, and prior or current experience using computers or related devices. Additional data may be sought as deemed appropriate, such as handedness, use of corrective lenses, or experience with handwriting recognition or SMS messaging.

[5] Prior to about 1996, the term "subjects" was used. The shift to "participants" reflects a degree of respect and gratitude for the contribution made by those who assist in research by participating as subjects.

4.3.2 Procedure

What will participants do in your experiment? No doubt, they will enter phrases of text. But, how many phrases? Was the text entry technique demonstrated by you, the investigator, beforehand? Did the investigator use any aides in explaining or demonstrating the technique? Were practice phrases allowed before data collection? Were the phrases organized in blocks or sessions with rest breaks between? These questions need to be considered and reported. (Remember the "R" in ROT?)

A particularly important aspect of the procedure is the instructions given to participants. What were participants trying to do? Were they to proceed "as quickly as possible," "as quickly and accurately as possible," or "without making mistakes" or were instructions worded in some other manner? Was it possible to correct errors? Were participants *required* to correct errors or required *not* to correct errors, or was some degree of discretion permitted? There is no single correct answer to these questions, as the instructions given and procedure used depend on the text entry technique and on the objectives in the experiment. Essential, however, is the need to report accurately on the procedure used in the experiment.

One further point on instructions is warranted. Use the same instructions for all participants. Sounds simple enough, but when instructions are given, participants often ask for clarification. It is important to constrain such discussions to ensure all participants proceed similarly. Sometimes instructions are read to participants to enforce consistency. In any event, it is important not to bias some participants by giving them additional information that may cause them to act differently from other participants.

On the potential to bias participants, it also worth commenting on the demeanor of the investigator during testing and while greeting the participants and introducing the research. The investigator should strive to make participants feel comfortable. A nervous manner or an attitude of indifference are sure to affect the way participants take to the task. Be calm, neutral, and encourage participants to feel comfortable.

4.3.3 Experimental Variables

It is important and useful to think about and organize your experiment around two classes of variables: independent variables and dependent variables.

Independent Variables

Independent variables are the conditions tested. They are so named because they are under control of the investigator, not the participant; thus, they are "independent" of the participant. The term "independent variable" is synonymous with "factor." The settings of the variable or factor are called "levels." For example, if three keyboard layouts were compared, say, Qwerty, Alphabetical, and New, the factor is "keyboard layout" and the levels are "Qwerty," "Alphabetical," and "New." Perhaps there is also an interest in the effects of two different sizes of keyboards. In this case, there is an additional factor, "keyboard size," with two levels, "small" and "large." Combining these two factors, the design is referred to as a 3 × 2 design. This nomenclature signals that there are two factors, with three levels on the first and two levels of the second, six test conditions in all.

Dependent Variables

Dependent variables are the measured behaviors of participants. They are dependent because they "depend on" what the participants do. The most obvious example is text entry speed, measured, for example, in words per minute. Note in the preceding sentence the distinction between the variable ("text entry speed") and the units for the variable (words per minute). For clarity, it is important to distinguish the two.

Make sure the measurement is clear. When did the timing for a phrase begin? When did it end? Did timing begin with the first character or with a START button or some other signal before the first character? Did timing end with the last character or by pressing ENTER after the last character? If timing begins with the first character and ends with the last character, then, arguably, the first character should not count, since timing excludes the time leading up to the entry of the first character. The important point is to clearly describe the measurement of all dependent variables.

The other dependent variable of common interest is "accuracy" or error rate, usually reported as the ratio of incorrect characters to total characters. As it turns out, error rate is tricky to measure, since error-ridden passages often contain a confusing and conflicting collection of insertion errors, deletion errors, substitution errors, transposition errors, and so on. The reader is directed to previous work that adequately describes the measurement and categorization of text entry errors (Gentner *et al.,* 1983; MacKenzie & Soukoreff, 2002a; Matias *et al.,* 1996; Soukoreff & MacKenzie, 2001, 2003, 2004) as well as to Chap. 3 herein.

In addition to coarse measures of speed and accuracy, researchers often devise new dependent measures as appropriate for an entry technique under investigation, as noted earlier for read-text events as a dependent variable in an eye typing experiment (Majaranta *et al.,* 2003). As another example, consider Wigdor and Balakrishnan's (2003) *TiltText,* a technique for mobile phone text entry that uses the orientation of the device to resolve the ambiguity of letters on keys on the mobile phone keypad. In addition to conventional error rate analyses, they defined and used "button error" and "tilt error" as dependent variables. Button errors were the ratio of errors due to pressing the wrong button, and tilt errors were the ratio of errors due to tilting the device in the wrong direction. McQueen, MacKenzie, and Zhang's (1995) *PiePad* is a gesture-based entry method for numeric entry. Gestures were stylus strokes conforming to a clock metaphor: right for 3, down for 6, left for 9, and so on. In addition to analyzing the time to make gestures, they defined and used "preparation time" and "scripting time" as dependent variables. Preparation time was the time between gestures, from stylus up after the previous character to stylus down for the current character. Scripting time was the gesturing time, from stylus down to stylus up.

Devising specialized dependent variables is particularly useful for text entry methods that use word prediction or other language-based acceleration techniques. As users engage acceleration opportunities, they exhibit a variety of behaviors that can be measured. In evaluating a stylus-based predictive system, MacKenzie *et al.* (2006) logged all stylus strokes and taps, but also categorized these by function, as shown in Fig. 4.7. The various stroke categories served as useful dependent variables in examining participants' usage patterns with the available acceleration aides.

Read-text events, button errors, tilt errors, preparation time and scripting time, keystroke savings, and so on are examples of dependent variables devised by researchers to gain insight into performance nuances of an entry technique. As one final comment, if a dependent variable is a count of the occurrence of an observable event, then it is useful to normalize it by converting the count into a ratio, for example, the number of such events *per character of input*. This facilitates comparisons across conditions and potentially across studies. If, for example, we are interested in participants' error-correcting behavior and decide to use presses of the BACKSPACE key as a dependent variable for such, then it is more instructive to analyze and report on "BACKSPACE key presses per character" than the raw counts of presses of the BACKSPACE key.

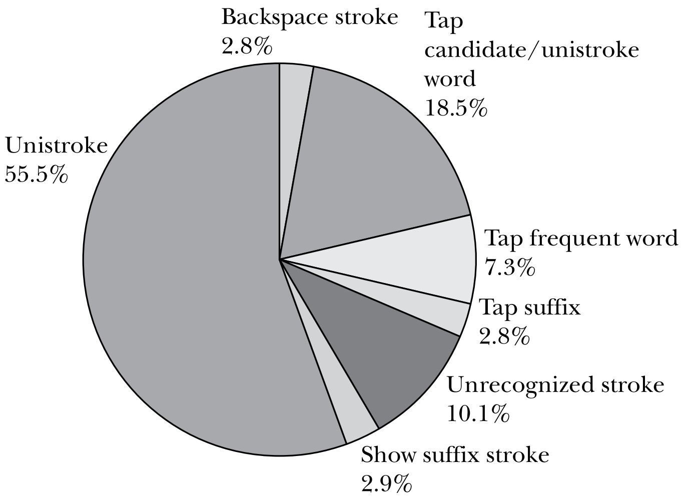


FIGURE 4.7 Seven categories of stylus strokes or taps used as dependent variables in the evaluation of a handwriting recognition system with predictive aides (from MacKenzie *et al.,* 2006).

Other Variables and Internal vs External Validity

While consideration of the test conditions, or independent variables, is crucial in the design of an experiment, researchers need to be aware of three other types of variables: random, control, and confounding. Any circumstance that could affect performance is potentially an independent variable in an experiment: standing vs sitting vs lying down, before lunch vs after lunch, quiet vs noisy, sun lighting vs room lighting vs dim lighting vs no lighting, left-handed users vs right-handed users, males vs females, big keys vs small keys, keys that click vs keys that do not click, and so on. There is no limit to the possibilities. There are three choices: (a) to engage a circumstance as an independent variable, (a) to ignore a circumstance and expect that it varies in a random and representative way within the experiment (random variable), or (c) to limit a circumstance to one setting throughout the experiment (control variable). The first choice was discussed above. Of the random or control choices, there is a trade-off. This is best articulated in terms of two important concerns in experimental research: internal validity and external validity. *External validity* refers to the extent to which the effect observed is generalizable to other persons and situations. *Internal validity* is the extent to which the effect observed is due to the test conditions. To clarify, "effect observed" is the difference observed in a dependent variable across the settings of an independent variable, for example, observing an entry speed of 12 WPM for method A compared to 15 WPM for method B. Was the difference due to the methods or to something else (internal validity)? Is the same outcome likely with other people in other situations (external validity)? Table 4.1 summarizes.

The third type of additional variable to consider is a confounding variable. A confounding variable is any circumstance that varies systematically with an independent variable. There are no advantages to confounding variables. They are an insidious defect in an experiment, and all researchers need to be wary of the possible presence of them. Here is an example. Let us say two keyboard layouts are compared and one is found to perform better on a dependent variable. If the keys were larger on one keyboard than on the other, then "key size" is a confounding variable because it varies systematically with "keyboard layout" (large for one, small for the other). Perhaps the effect observed was due to key size, not to keyboard layout. The solution is to ensure that the keys are the same size on the two keyboards or to include key size as an independent variable and to test both keyboard layouts with both key sizes.

Variable	Advantage	Disadvantage
Random	Improves external validity by using typical situations and people	Compromises internal validity by introducing additional variability in the measured behaviors
Control	Improves internal validity since differences in measured behaviors are more likely due to the test conditions	Compromises external validity by limiting responses to a specific circumstance or a specific type of person

TABLE 4.1 Relationship between random and control variables and internal and external validity.

4.3.4 Within-Subjects vs Between-Subjects Designs

If the evaluation is comparative, as is typically the case, then there is at least one factor in the experiment and this factor has at least two levels. The levels are the test conditions. If all participants are tested on all levels of a factor, the design is called a "within-subjects design" or a "repeated-measures design." In other words, the measurements gathered under one condition are repeated on the same participants for the other condition(s). The alternative to a within-subjects design is a between-subjects design. In this case, each participant is tested under one condition only. One group of participants is tested under condition A, a separate group under condition B, and so on.

There are a number of issues to consider in deciding whether an experimental factor should be assigned within subjects or between subjects. Sometimes there is no choice. For example, "handedness" might be a factor in an experiment in which a text entry technique is thought to have a bias favoring, say, right-handed users over left-handed users. In this case, handedness must be assigned between subjects, because a participant cannot be both left handed and right handed! Conversely, if an experiment seeks to investigate the acquisition of skill over multiple blocks of practice, then "practice" is a factor in the experiment, with levels "block 1," "block 2," and so on. Clearly, the only option for practice is within subjects. No two ways about it!

In many situations, however, there is a choice. If so, a within-subjects design is generally preferred because effects due to the disposition of participants are minimized. Such effects are likely to be consistent across conditions. For example, a participant who is predisposed to be meticulous will likely exhibit such behavior consistently across experimental conditions. This is beneficial because the variability in measurements is more likely due to differences among test conditions than to behavioral differences between participants (if a between-subjects design were used).

Despite the above-noted advantage of a within-subjects design, a between-subjects design is sometimes preferred due to interference between conditions. If the conditions under test involve conflicting motor skills, such as typing on keyboards with different arrangements of keys, then a repeated-measures design may be a poor choice because the required skill to operate one keyboard may inhibit, block, or otherwise interfere with the skill required for the other keyboard. Such a factor may be assigned between subjects (see MacKenzie *et al.*, 2001, for an example).

If interference is not anticipated, or if the effect is minimal and easily mitigated through a few minutes of practice when a participant changes conditions, then a within-subjects design is the best choice.

4.3.5 Counterbalancing

An additional effect must be accounted for in within-subjects designs: learning. For example, if two techniques, A and B, are compared and all participants used technique A first, followed by B, then an improvement with B might occur simply because participants benefited from practice on A[6]. The solution is counterbalancing, or splitting the participants into groups, testing half on A, first followed by B, and half on B first, followed by A. Learning effects, if any, should cancel. Counterbalancing is useful for more than two conditions also, and in this case grouping is done according to a Latin Square. The most common arrangements are shown in Fig. 4.8. Note that in each case a condition appears precisely once in each row and column. For a balanced Latin Square, it is also true that each condition occurs before and after each other condition an equal number of times, thus improving the balance (see Fig. 4.8d).

Obviously, the number of groups required must divide evenly into the total number of participants. If three conditions are counterbalanced over 15 participants, each group has 5 participants.

4.3.6 Testing Research Questions

The "T" in ROT (see above) is for testable. Not only must research questions be repeatable and observable (we have dwelled on these in detail already), they must be testable. In text entry research, we often just want to know if a new entry technique is any good or if it is better than an existing or alternative technique. Fair enough, but "Is it good?" and "Is it better than...?" are not testable questions. We can evaluate techniques and ask participants their opinions, and this is certainly part of the process; but, usually, "good" or "better than" follows from the outcome on observable, measurable behaviors. That is, if participants enter text using method A "faster than" or "with fewer errors than" method B, it is usually true that their opinions align with their performance.

So, testable research questions are usually of the form "Is method A *faster than* method B?" or "Is method A *more accurate than* method B?" However, we are not quite there yet. Let us say we develop a new text entry technique ("A") and conduct an experiment according to the steps above, comparing the new technique with an alternative method ("B"). The data collected reveal 14.5 WPM for method A and 12.7 WPM for method B. Method A is about 15% faster than method B. Fair enough, but where is the test? The test lies in establishing whether the difference is significant or just a chance outcome. Since we are testing humans, variability in responses is inevitable. If the experiment were run again, no doubt the results would be slightly different. But, how much different? Would method B be faster? Clearly, this is a concern.

To answer the question, "Is method A faster than method B?" we use a statistical test called an analysis of variance (ANOVA)[7]. It is a test to determine if two or more mean scores are the same based on the variance in the constituent measures. More

[6] The alert reader will notice that learning, as just described, is a confounding variable.

[7] If only two means are compared, then a "*t* test" is the same as an ANOVA. In practice, usually more than two conditions are compared (as, for example, six in the 3 × 2 design discussed earlier), so the discussion here uses an ANOVA.

(a)

A	B
B	A

(b)

A	B	C
C	A	B
B	C	A

(c)

A	B	C	D
D	A	B	C
C	D	A	B
B	C	D	A

(d)

A	B	C	D
B	D	A	C
D	C	B	A
C	A	D	B

FIGURE 4.8 Latin Squares. (a) 2 by 2, (b) 3 by 3, (c) 4 by 4, (d) balanced 4 by 4. The distinguishing feature in the balanced Latin Square is that each condition occurs an equal number of times before and after each other condition.

precisely, an ANOVA is a test of a null hypothesis, in this case “The entry speed for method A *is the same as* that for method B.” Let us see how this is done. Consider the two hypothetical data sets in Fig. 4.9. The outcome in both cases is the same, 14.5 WPM for method A and 12.7 WPM for method B.

Figure 4.10 illustrates the difference in the data sets. An important embellishment in the charts is error bars showing, in this case, ±1 standard deviation in the constituent measures. Standard deviation is the square root of the variance. Even though the variance is tested, the standard deviation is more commonly articulated, since it has the same units as the mean. Either way, the error bars in Fig. 4.10 clearly show the increased dispersion in the responses of the participants in example b. Further indication of this is evident by comparing the individual participant measures in Figs. 4.9a and 4.9b.

The ANOVA is performed with a statistics program, the full details of which are beyond the scope of this chapter. For the data in Fig. 4.9a, an ANOVA produces a table as in Fig. 4.11[8]. In a research paper, the results would appear somewhat as follows: “The text entry speeds were 14.5 WPM for method A and 12.7 WPM for method B, suggesting about a 15% performance advantage for method A. The difference was statistically significant ($F_{1,9} = 19.0$, $p < 0.005$), as determined using an analysis of variance.”

The ANOVA result is given in parentheses. First, the *F* statistic appears along with the degrees of freedom for the effect and the residual sources of variation (method = 1, method * subject = 9, respectively). Following the *F* statistic is *p,* the probability that the hypothesis is true. (Remember that the hypothesis is expressed in the null form; namely, that there is no difference.) For example a, evidently there is only a 0.0018 probability that the entry speed for method A is the same as that for method B. That is pretty low;

[8] Created by *Statview,* now marketed as *JMP* (www.jmp.com).

(a)

Example #1		
Participant	Method	
	A	B
1	15.3	12.7
2	13.6	11.6
3	15.2	12.1
4	13.3	11.5
5	14.6	13.0
6	14.1	15.0
7	14.0	14.0
8	15.0	11.6
9	15.2	12.5
10	15.1	12.6
Mean	14.5	12.7
SD	0.73	1.12

(b)

Example #2		
Participant	Method	
	A	B
1	17.3	12.7
2	13.6	10.6
3	15.2	12.1
4	10.3	7.5
5	16.6	14.5
6	14.1	15.5
7	10.0	15.0
8	15.0	9.6
9	18.2	15.5
10	15.1	13.6
Mean	14.5	12.7
SD	2.71	2.72

FIGURE 4.9

Hypothetical data sets for a text entry experiment. The mean entry speeds are the same in both data sets. In (a) the difference is significant. In (b) the difference is not significant. SD is the standard deviation in the measures used to compute the mean. See text for discussion.

thus we reject the hypothesis and conclude that the difference in the means is statistically significant. The threshold for significance is typically any value of p lower than 0.05 or a 1 in 20 chance. Instead of reporting a precise value, p is rounded to the more conservative of the thresholds 0.05, 0.01, 0.005, 0.001, 0.0005, or 0.0001. So, in this case, p is rounded from 0.0018 to 0.005. By the way, the difference in the entry speed for method A and method B in example b was not statistically significant ($F_{1,9} = 3.83$, $p > 0.05$).

As a final note, “statistically significant” does not imply that the difference is important, useful, or meaningful. Whether a 15% performance advantage is important for design success or commercial viability is not within the purview of statistical tests.

4.4 LEARNING

We spoke earlier about learning as a behavior to balance out, to cancel, as though it were an undesirable human condition. While learning can confound other effects in within-subjects designs, the human trajectory in skill acquisition is often the most crucial aspect of a text entry method. How quickly can a method be learned, and with

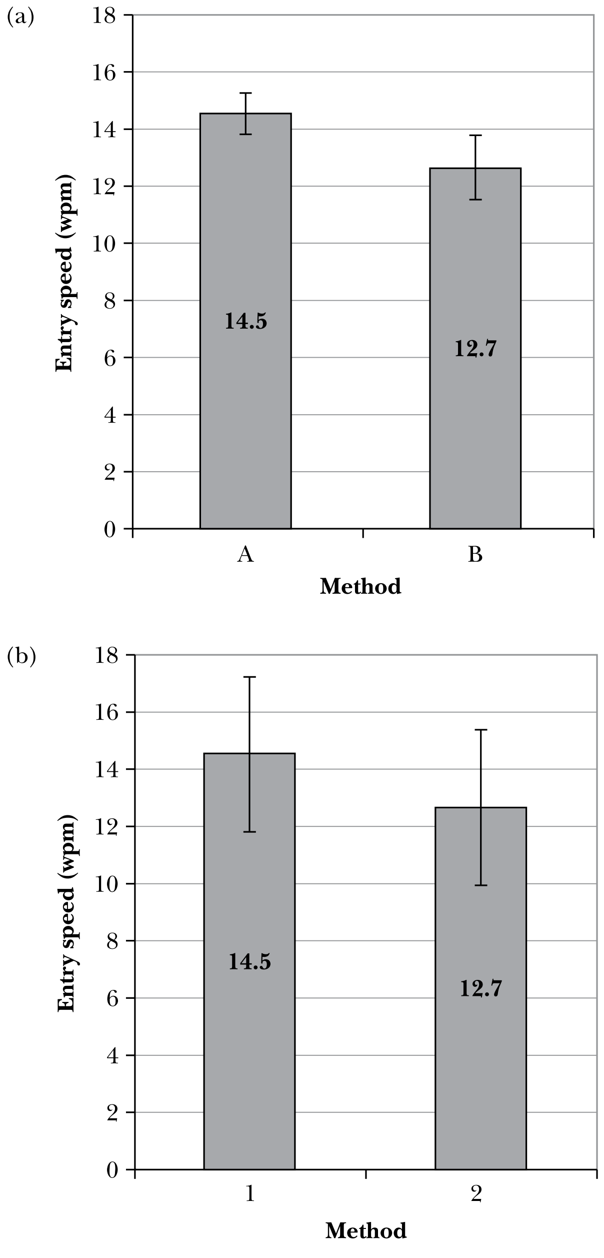
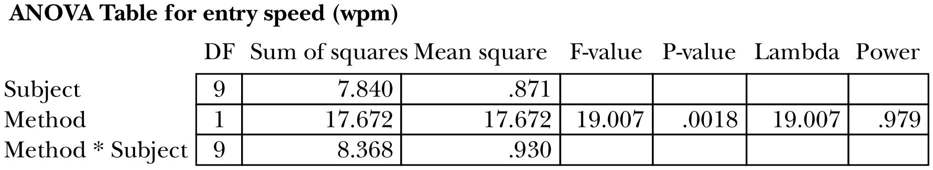


FIGURE 4.10

Charts for data in Fig. 4.9. The vertical lines on the bars show ±1 standard deviation in the measures forming the means. See text for discussion.



ANOVA Table for entry speed (wpm)

	DF	Sum of squares	Mean square	F-value	P-value	Lambda	Power
Subject	9	7.840	.871				
Method	1	17.672	17.672	19.007	.0018	19.007	.979
Method * Subject	9	8.368	.930				

FIGURE 4.11 ANOVA table for data in Fig. 4.9a.

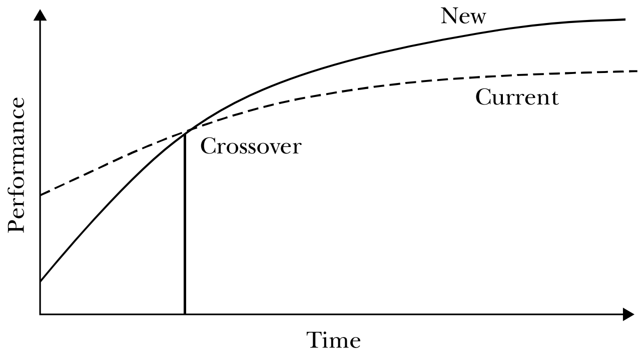


FIGURE 4.12 Improved performance with practice. A new technique outperforms an existing technique, but not initially.

what performance outcomes? Are the intended users consumers with high expectations for immediate usability? Are they high-use experts willing to invest considerable effort in learning the method? In recognition of this diversity in the user community, researchers often investigate learning in their research program.

The trajectory of human's learning a skill is shown in Fig. 4.12. As our emphasis is on comparative evaluation, two curves are shown, one for a new technique and one for a current or alternative technique serving as a point of comparison.

The curves show a performance metric, perhaps text entry speed, plotted against the time invested in learning the techniques. Curves following a power function are shown, but logarithmic scales are sometimes used for the axes, and this produces a plot with straight lines (e.g., Card *et al.,* 1978). The curves reveal a crossover point, wherein one technique, after sufficient practice, outperforms the other.

The equation for the performance–time relationship in Fig. 4.12 is built using linear regression, by regressing the observed performance data on the block or session number in which the data were gathered. Gathering the data is labor intensive because a longitudinal study is required, wherein testing proceeds over a prolonged period of time. This is shown in Fig. 4.13 for an experiment comparing text entry speeds for two soft keyboard layouts over 20 sessions of practice (MacKenzie & Zhang, 1999).

(a)

Session	Entry speed (wpm)	
	QWERTY	OPTI
1	28.44	16.98
2	29.98	21.06
3	31.56	24.80
4	32.62	27.26
5	33.20	29.18
6	34.52	31.04
7	34.14	33.08
8	34.68	34.16
9	35.66	34.72
10	36.77	35.89
11	36.95	37.22
12	37.36	38.75
13	38.50	39.95
14	38.65	40.73
15	39.12	40.68
16	39.00	41.85
17	39.20	42.74
18	39.85	43.55
19	40.16	43.91
20	40.30	44.29

(b)

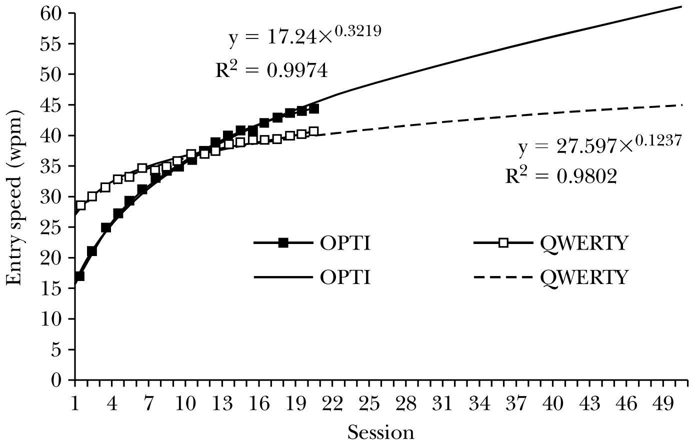


FIGURE 4.13 Power law of learning for an experiment comparing two soft keyboard layouts. (a) Data. (b) Learning curves and extrapolations to the 50th session (from MacKenzie & Zhang, 1999).

The equation for each condition is shown beside its curve in Fig. 4.13b. The equation is in the power form and is derived using a statistics application or a spreadsheet such as Microsoft *Excel*. The crossover phenomenon in Fig. 4.12 appears at the 10th session in Fig. 4.13. An extrapolation beyond the period of practice is also shown, although this obviously bears increasing inaccuracy the farther it extends beyond the test period.

While the plots in Fig. 4.13b are instructive, often the most significant point is the first one—the measured performance during initial exposure to the technique. This point reflects the walk-up usability (a.k.a. immediate usability) of the technique. Researchers are often seeking to develop a text entry technique that improves on a current, established, but less-than-ideal technique. If there is a community of users familiar with the current technique, it is a tough sell to convince them to switch. Performance advantages of a new technique are often expected with minimal effort. In view of this researchers have developed evaluation methodologies designed specifically to capture the immediately usability of a technique, for example, by limiting and tightly controlling participants exposure to the technique. In one experiment, participants were given just 1 minute of exposure before performance measures were gathered (MacKenzie & Zhang, 1997). A second round of testing was done following an additional 5 minutes of exposure. Others have adopted similar evaluation methods (Koltringer & Grechenig, 2004; MacKenzie & Zhang, 2001; Wobbrock *et al.,* 2005).

4.5 SUMMARY AND FURTHER READING

This chapter on evaluation techniques has been crafted in the language of text entry research. It has also been pared down for delivery in a single chapter. Indeed, there is considerably more to evaluation than can fit in this space. We have, for example, not commented on the potential problems of asymmetric skill transfer in within-subjects designs or post hoc comparison tests to determine which conditions differ significantly from which others (when more than two conditions are tested). Other issues in experiment design are just as important, such as participant consent, the method of selecting participants, and sequencing of conditions in more complex experiment designs. Two references on experimental methods the author finds particularly useful are the *Publication manual of the American Psychological Association* (American Psychological Association, 2001) and Martin's *Doing psychology experiments* (Martin, 2004). It is also worthwhile just to study research papers that present experimental evaluations that are comparative and empirical, both in text entry and on other topics in human–computer interaction.

Quite a few papers are cited in the pages above, but, in closing, there are a few others to consider that present experimental methodologies for text entry that include learning models similar to Fig. 4.13 (Bellman & MacKenzie, 1998; Isokoski & Raisamo, 2004; Lyons *et al.,* 2006; MacKenzie *et al.,* 2001; McQueen et al., 1995; Wigdor & Balakrishnan, 2003; Wobbrock *et al.,* 2006).

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