The Ethnographic Study of Visual Culture in the Age of Digitization

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Writing about a group of people is hard work. Even if the researcher vowed to take an objective stance and simply describe what people did, how they did it, and why they did it, he or she would eventually run into problems of representation. Whose perspective do you take? What activities are important enough to include in the ethnographic record? Who gets to decide what counts as an important activity? In grappling with these questions—either implicitly or explicitly—the ethnographer does more than produce an objective text about a group of people; he or she produces an account of culture that is, itself, culture-laden. Because no ethnographic account is ever completely straightforward, Van Maanen (1988) eloquently describes how readers of an ethnographic text learn as much about the culture in which the ethnographer is embedded through the choices he or she makes in the presentation and description of data as they do about the culture of the informants about whom the author writes.

This chapter is a cultural tale about how ethnographers ply their trade in writing about groups of people whose work constitutes a “visual culture” in an age where visual representations are rendered digitally. A visual culture is one in which people’s work and social interactions are intimately connected to the drawings, sketches, animations, and other images they use daily. Put another way, in a visual culture, people traffic in representations; they use visual representations to persuade, to convey meaning, and, simply, to accomplish their work tasks. Members of a visual culture develop a shared visual literacy that dictates what visual representations are appropriate in their work, how and when those
representations should be used, and how they should be interpreted. As Latour (1986: 9–10) suggests, “A new visual culture redefines both what it is to see and what there is to see.”

I have spent the last several years as an ethnographer of the visual culture of engineering. Hence, my examples in this chapter will focus on engineering work, though I suspect that the arguments I make will be applicable to ethnographers who find themselves dealing with any occupation whose members’ work is enabled and constrained by their use of visual representations. Over the last two decades, numerous authors have described engineering as an increasingly prime example of a visual culture (Bucciarelli 1994, 2002; Collins 2003; Downey 1998; Ewenstein and Whyte 2009; Fleischer and Liker 1992; Henderson 1999; Sims 1999; Vaughan 1996; Vinck 2003). Engineers do not simply work with drawings, plans, and sketches; rather, they think with and think through them, they communicate with other engineers by modifying them, and they develop engineering knowledge by studying and experimenting with them. Interaction with representations of physical systems so permeates the work of most contemporary engineers that Henderson (1999: 27–28) writes, “The visual culture of engineers is not made up of school-learned drafting conventions but rather the everyday practices of sketching, drawing, and drafting that construct their visual culture—a visual culture that in turn constructs what and how design engineers see.”

Today, it is unlikely that a researcher would enter an engineering organization and see blueprints or sketches strewn across tables and desks. In fact, on my first visit to an engineering organization as a field researcher, I was amazed by the lack of paper in the office. What I saw, instead, was the back of a lot of heads. Engineers were seated in front of their computers staring at digitized drawings, sketches, and animations on the screen. Occasionally, I saw two or more engineers huddled around one computer, all looking at the same image on one person’s screen. The visual representations of physical objects (like an engine) or physical processes (like a car crash) with which contemporary engineers work are almost exclusively digital. To be sure, many scholars have documented how engineers frequently work with digitized drawings as opposed to paper and pencil drawings (Adler 1990; Becky 2003; Liker, Haddad, and Karlin 1999; Malhotra et al. 2001), and much recent research has documented engineers’ turn toward the use of computer-based simulations (most often in
three dimensions) to make recommendations for the performance of physical systems (Boland, Lyytinen, and Yoo 2007; Carlile 2004; Dodgson, Gann, and Salter 2007; Leonardi and Bailey 2008). Authors like Turkle (2009), Loukissas (2009), and Bailey, Leonardi, and Barley (2012) argue that the shift toward digitization in engineering design and analysis portends deep changes in not only how engineers work, but what they know and how they know it. In short, understanding how engineers and other technical workers make the shift to a world of digital, visual representations may be key to explaining society’s transition from a mechanical to a computational infrastructure.

For ethnographers of visual culture, digitization poses new challenges. This chapter is focused on discussing how field researchers can deal with some of the issues of studying visual culture in the age of digitization. My goal is to examine how an ethnographer might enter a physical space like an office or a testing facility and observe, document, and analyze people’s interactions with digital artifacts in those settings. I am not, in this chapter, concerned with ethnographies of digital spaces, like social network sites, games like World of Warcraft, or virtual worlds like Second Life. For discussions of how ethnographers deal with the particularities of data collection in these digital spaces, I recommend that the interested reader consult books by Boellstorff (2010), Nardi (2010), and Pearce (2009).

I divide this chapter into three parts. The first part focuses on issues of access to the site. How does one gain approval from a university’s institutional review board and negotiate entrée to a site where individuals work intensively with digital artifacts? The second part focuses on observation and analysis. How does a researcher record people’s interactions with visual representations with which they work on their computers? And how can these representations be used in data analysis? Finally, the third section explores the difficult job of writing and publishing one’s findings based on such data.

Rather than discuss several different research projects, I will constrain my focus to only one and interrogate it deeply. The experience on which I draw in this chapter comes from my research into the development, implementation, and use of a new computer-simulation technology for crashworthiness engineering work at Autoworks (a pseudonym), a major automaker located in the United States. Crashworthiness (a vehicle’s ability to absorb energy through structural deformation) is assessed by
engineers who conduct physical crash tests and computer simulations. Because the cost of administering and recording the data for physical crash tests (building a prototype vehicle, loading it with instrumentation, crashing it into an object, and interpreting the findings) takes so long, Autoworks encourages its engineers to use finite element simulation models to predict a vehicle’s crashworthiness and provide recommendations for how a vehicle structure can be changed to increase performance in an impact. Finite element analysis is a computational technique that divides the actual geometry of a part into a bounded (hence finite) collection of discrete elements. The elements are joined together by shared nodes. Nodes are the locations where values of unknowns (usually displacements) are approximated. This collection of nodes and elements is commonly called a “mesh” (see figure 5.1).

The vast majority of an engineer’s time is spent in iterative cycles of building finite element simulation models, analyzing their performance, and making suggestions for changes in design. To reduce the time and effort it took engineers to set up and analyze simulation models, engineers in Autoworks’ research and development (R&D) labs developed a new software tool called CrashLab. CrashLab is used for preprocessing (setting up mesh models in ways that can be analyzed by a solver to produce desired results) the finite element models that safety and crashworthiness engineers use to make predictions about how a vehicle will respond in a crash. The idea for CrashLab emerged out of Autoworks’ R&D
organization in 1995. After nearly 10 years of developmental work, CrashLab was deployed into the user community in September 2005. I followed the effects that CrashLab had on engineers’ work from its time of deployment through 2007. The main research question of the study was as follows: how do organizations and technologies co-evolve through activities of development, implementation, and use?

Data on CrashLab and the work system in which it was embedded were obtained through the use of ethnographic techniques. I spent nine full months over a two-year period (2004–2006) conducting interviews and observations with people who were involved in all aspects of CrashLab’s lifecycle. I began by identifying key informants I knew were involved in CrashLab’s development, and through these interviews acquired the names of others who were also involved. Fortunately, Autoworks maintained a very detailed and accurate database of employee contact information and I was able to use this database to track down informants even if they had moved into new positions within the company. I conducted interviews with members of five different organizations within Autoworks. Additionally, I conducted interviews with informants in three technology supplier organizations who helped, at various points in time, to build CrashLab. I also conducted several interviews with other individuals who were involved with CrashLab but who were not members of any of these organizations, such as Autoworks senior management, consultants from other supplier firms, and professors at nearby universities. In total, I conducted 58 interviews with people involved in changing CrashLab’s features.

I also collected data on the work of crashworthiness engineers before CrashLab was implemented, the activities of developers, trainers, and managers during implementation, and the work of engineers after CrashLab was implemented. During each of these activities I utilized four primary data sources: Observations made of informants at work, artifacts that informants produced or used in their work (e.g., screenshots of computer simulations, test request forms, best-practice guidelines), interviews conducted with informants about their work, and logs kept by informants tracking their use of CrashLab. These data collection methods resulted in 134 observations (each of which was three hours or more in duration) with crashworthiness engineers, 51 interviews with engineers who used CrashLab, 17 additional interviews of people such as managers and
implementers who were also in some way involved with CrashLab, and more than 500 digital artifacts used by engineers. My analyses of these data have resulted in several publications (Leonardi 2009, 2010a, 2010b, 2011a, 2011b).

Access

In any workplace ethnography, the researcher’s access to the phenomena he or she wishes to study is regulated by at least three gatekeepers: The university’s Institutional Review Board (IRB), the management or leadership of the organization under study, and the individuals who actually conduct the work. Reaching an agreement for access to the research site through each of these three gatekeepers requires a distinct approach. Although one might think that the IRB is the place to begin negotiating access, I would argue that it is really in the researcher’s best interest to start with the organization’s management. Without the consent of management, all the IRB approval in the world will do little good. Thus, there is little point to devote one’s efforts to the IRB process unless the researcher knows that he or she will be able to “get in” the door at the organization. For this reason, I begin by discussing the negotiation of access with the organization of interest.

The Organization

Autoworks is a large company. It has more than 300,000 employees spread across engineering, financial, and manufacturing centers around the globe. Thinking about gaining access to a company of this size can be quite daunting. Of course, the ideal strategy would be to solicit help from the CEO and have him or her open doors for you all the way down to the people you would like to observe. Like most researchers, I do not hobnob with too many CEOs, especially those who run multibillion-dollar companies, so this approach was not feasible. One strategy I have often used to great success is to begin by finding somebody in the organization’s R&D labs. Why the R&D labs? Well, in most firms, members of R&D typically share what Dougherty (1992) calls a “thought world” with the researcher. Unlike individuals in sales, marketing, advertising, product development, finance, and so on, individuals who work in corporate
research are devoted to advancing science. They hope, of course, that their research will culminate in useful and profitable products for the company. But this goal notwithstanding, they understand what it is that you are after because they are often in the same shoes. Finding a member of R&D who understands the principles of academic research and who is excited about the topic you are interested in exploring is a solid way to get your foot in the door at the organization.

Many corporate R&D labs have internship and fellowship programs available for outside researchers. These programs are centrally administered and provide outside researchers with two very important items (I will come back to these later): ID badges and email addresses. From their perspective, internship and fellowship programs are great ways of tapping talent at relatively low cost. From the researcher’s perspective, affiliation with one of these programs provides institutional legitimacy (or insider-ness) that is hard to achieve by any other means.

My route into Autoworks came through the company’s R&D labs, with which my university had an ongoing relationship. After some investigation around campus, I found a mechanical engineering professor (not at all my discipline) who had close relationships with people in Autoworks’ R&D labs. I met with this professor and outlined my research objectives. It was clear that he had no clue what I was talking about or what it meant to conduct an ethnography. He listened politely and, by the end of our hour-long conversation, he seemed to understand, generally, what I wanted to study. He told me that the topic sounded interesting, but that he would not make the contact for me within R&D because he had made a connection for another student whom he did not know well, the student performed very poorly, and his relationship with key players in Autoworks’ R&D was damaged as a result.

I understood his concerns and took it upon myself to show him that I was trustworthy. To do that, I procured letters from two organizations at which I had conducted ethnographic research in the past. Both letters spoke to the fact that I was a responsible researcher, and each letter writer mentioned that they thought more highly of the university and its capabilities once I had finished my research project. I sent these letters to the mechanical engineering professor, and, apparently, they were enough to persuade him to take a gamble on me. Two days after I sent the letters to him, he copied me on an email in which he introduced me to his contact
at Autoworks. With that initial contact in hand, I navigated to a researcher who was a director of R&D’s “work systems group”—a group that was responsible for improving engineering process at Autoworks, as opposed to developing new technologies for cars and trucks (e.g., GPS devices or airbags). Through a series of phone calls and formal applications with the director of the work systems group, I eventually wound up as an R&D “intern” for a six-month stint. The agreement I made to secure this internship was that I could study the topics in which I was interested, but I had to provide monthly reports to R&D and to the product development groups I studied that summarized my findings and made recommendations for how those groups could improve their work processes. I reasoned that these deliverables were a fair trade for access to an interesting research setting.

The central problem with entrée to the organization through R&D, however, is that because they are engaged in research and often do not contribute directly to the firm’s profits, the research labs are often low-status players in the corporate environment (Thomas 1994; Workman 1995). For this reason, at most firms, R&D cannot simply say, “We’re studying this process. We’ll start observing you next week.” Instead, R&D has to ask permission to conduct a study and find a “project champion” (this is what Autoworks called it) within a product development division who will help negotiate access to the people with whom you really want to spend time.

The reason why I was so keen to conduct this study at Autoworks was that I had seen a presentation at my university in which an R&D engineer discussed a new simulation technology under development at the company and expected to be released to the crashworthiness engineering community soon. Both the technology and the timeframe of its release sounded ideal. The technology was supposed to “revolutionize” crashworthiness engineering work by automating simulation analyses, and it was in its final months of development and soon to be deployed. Thus, if I could follow this technology, I would be able to witness not only part of its development, but its implementation and use, too. Upon arriving at Autoworks, I did some initial leg work to make sure the technology was going to be implemented when it was supposed to be and that crashworthiness engineering was the right organization to observe. After verifying that the technology, its time frame, and its user community would be ideal for the
study, I sought out a “project champion”—someone who could see the benefit of my study and would ensure I was able to carry it out.

The project champion I identified was the director of Autoworks’ Safety and Crashworthiness division. To convince this director to let me study his engineers, I had to give a series of presentations in which I discussed my research question and the methods I would use, and, most importantly, described what benefit all of this research would be to the division. The trickiest item was the explanation of data collection. I explained to the directors that I would sit behind engineers as they worked. I explained that I would, with their permission, audio record their dialogue with each other and things they said out loud. I described how I would follow the engineer I was observing to meetings and to tests at the proving grounds. No one objected to any of these methods. But when I said I would be asking engineers for screenshots of the computer-models with which they worked and copies of PowerPoint presentations they prepared to summarize their recommendations, the directors became very uneasy. Questions surfaced about whether or not I would leak confidential information to competitors. (I thought about joking that the sales data suggested no one would be interested in stealing their designs, but I thought this joke would be unwise.) Instead, I discussed how I would only ask for copies of digital artifacts that were of interest for the project, and I would make sure that those artifacts had images or descriptions that were generic enough so that outsiders could not identify the vehicle they referenced. That calmed nerves some, but not a lot. The saving grace came when I explained to them that I was being employed by Autoworks as an official “intern” and as part of the internship process I had to sign nondisclosure agreements for protected company information. I also assured them that my internship “boss” (the director of the R&D work systems group) had the right to review any paper that I published from the data I collected at Autoworks. I am confident that it was because I was a (temporary) “inside man” that the directors finally agreed to allow me to collect digital artifacts from the engineers I studied.

The Institutional Review Board

After lining up access at the field site, the researcher must next secure approval for the study through his or her university’s institutional review
board. IRB applications can be, at times, quite onerous. I have found that once I have negotiated access to the site I wish to study and have received a letter from the site endorsing the study, the IRB process is much more manageable. Sometimes, an observational study like the one I conducted at Autoworks can even qualify for an exempt review if that support letter from the organization is presented as part of the application. An exempt review (as opposed to an expedited review) means that only one member from the IRB has to review the application; the application does not have to undergo deliberation at the monthly board meeting. The upshot is that exempt reviews result in faster decisions and typically require less revision than an expedited or full review. Research projects qualify for exempt review if they undertake observation in “public settings” and the names of the informants are not recorded anywhere in the observer’s notes, in legends or keys made of those notes, or on an informed consent form. The letter from the organization helps to establish the research site as a “public setting,” because the researcher is now invited into a community and can observe people at work, just as one’s coworkers can. Another advantage of an exempt status through IRB is that the researcher must only secure “verbal consent” from the informants, as opposed to formal “written consent,” which must be documented on an informed consent form. The verbal consent is an enormous advantage for a workplace field study. On more than one occasion, I have found myself confronted with an informant who refused to read or sign a written consent form, but was happy to be observed. To avoid this situation, it is much easier to simply read the informant a quick statement, ask for their verbal consent, and move on.

I can only imagine the alarm bells that would sound if I wrote on my application to the university’s IRB that I would be collecting screenshots and PowerPoint presentations from my informants. How would I explain to them the delicate negotiations that occurred with the director of the Safety and Crashworthiness division? Would it suffice to say that I signed a nondisclosure agreement as part of my “internship” papers? I do not know with certainty the answers to these questions, but I suspect that I would not like those answers if I heard them. When it comes time in the IRB application to write about the collection of digital artifacts, I usually indicate my plan for the collection of these data sources by writing something like: “I plan to collect work artifacts used by informants if they wish
to share them. I will only collect artifacts that do not identify the informant in any way. These artifacts will be stored either in a locked cabinet in the researcher’s office or on a password protected, non-networked hard drive that is kept in the researcher’s locked office.” Such phrasing is truthful, but a bit vague. I have never received a question from an IRB (on the Autoworks study or any other) using this language.

The Worker

With IRB access secured, travel plans made, and the houseplants watered, the researcher can finally go “into the field” to conduct the long-anticipated research. The problem, at this point, is that although management may endorse your study and even announce to workers that they should participate in it (as they did at Autoworks), workers still must be convinced that you are not a threat to their productivity. This is a valid concern. Informants typically have a lot to do. If they spend time during their day talking with researchers, they might not complete all of their assigned tasks. Not completing tasks on schedule could force them to stay late at work to catch up, which disrupts personal time, or it could get them in trouble with the boss. Typically, these fears on the part of the worker can be easily overcome in the first few minutes of observation. The more difficult challenge is convincing the worker that you are really interested in learning how he or she works and that you are not interested in how they could work better. I often experienced this latter concern at Autoworks, and it slowed my initial research progress.

For many organizations that were born during the second industrial revolution, time and motion studies are still a fresh memory. Time and motion studies are the most popular byproduct of America’s first major business fad: scientific management. This new orientation toward work was promoted by efficiency experts of the day, most notably Fredrick Taylor (1911/1998) and Frank Gilbreth (1911/1993). In the mid-to-late 1800s, U.S. industrial enterprise was heavily dependent upon the knowledge and skills of craft workers (Thompson 1967). Craft workers spent years learning a particular trade and taught that trade to apprentices and others who were willing and able to learn. As Gilbreth (1911/1993: 94) observed, in the mode of craft production, “all excellent methods or means were held as ‘trade secrets’ sometimes lost to the world for
generations until rediscovered.” From an organizational management perspective, the idea that one person would hold both the knowledge and skill to complete a particular task proved problematic, because productivity depended entirely on that worker and few others like him or her. With increase in mass production in the United States, craftwork was increasingly proving not only impractical, but also burdensome to management, who, without knowledge of the craft themselves, were beholden to the worker’s time constraints and wage demands.

With the development of scientific management, Taylor aimed to solve these problems. He suggested that by extracting the knowledge required for a specific task from a worker, a management scientist could examine, evaluate, modify, improve, and teach it to countless others. Not only did Taylor claim that an individual who had the time to evaluate a task could improve it more effectively than someone performing the task, he also asserted that:

In almost all of the mechanic arts the science which underlies each act of each workman is so great and amounts to so much that the workman who is best suited to actually doing the work is incapable of fully understanding this science, without the guidance and help of those who are working with him or over him. (Taylor 1911/1998: 9)

To extract knowledge from the worker and evaluate the performance of that work, Taylor and Gilbreth, among others, pioneered a method that would become known as the “time and motion study.” The method was quite simple but powerful in its effects. The observer, typically someone unfamiliar with the work of the laborer, would directly observe the laborer in action, recording his every movement and timing how long he took to perform those movements. Learning the details of a job and discerning the possibilities for improving it involved investing an enormous amount of time. The observer (or scientific manager) had to watch the laborer perform an activity enough times to know that, as Taylor suggested, the laborer was not simply “putting on a front” for the observer. Moreover, the observer had to watch multiple laborers in order to determine which actions constituted “best practices.” Once the data collection phase was complete, the observer retreated from the shop floor or construction site to his office to analyze the findings. Thus, as Gilbreth (1911/1993) suggested, the methodology of a time and motion study should include both direct observation and analysis that compares
practices across a variety of workers to make recommendations for optimal performance of work. With such an analysis complete, the scientific manager would not only have abstracted from the worker the knowledge necessary to perform a task, but could also modify the practice to improve performance and efficiency and teach it to new workers.

During my first few days at Autoworks, before informants understood why I was observing their work, they considered my interests suspicious. In fact, many engineers thought I was in their midst sent by management to perform a time and motion study. One of my first interactions with an informant (I) illustrated these concerns and the misconceptions informants had about why someone would want to watch them work:

I: Is this a time and motion study?
Me: No, it’s not a time and motion study.
I: So you’re not like Fredrick Taylor or something?
Me: No, I’m just here to learn about your work.
I: I’m sorry.
Me: Why is that?
I: Because you’re going to be bored.
Me: Well I don’t know anything about crashworthiness work, so it will all be exciting for me.
I: Even if I’m just optimizing element sizes?
Me: See, that sounds exciting already.
I: What if I just check my email?
Me: Well, will you let me read it with you?
I: Sure.
Me: Then that will be exciting, too.
I: Will you follow me to the bathroom?
Me: Okay, the excitement’s gone now.
I: [laughing] So, you don’t have a stop watch, right?
Me: [laughing] Do you want to check my bag?
I: No, I’m just kidding. So you said this was, what did you call it?
Me: An ethnography.
I: Oh, like an anthropologist.
Clearly, the concept of ethnography was more foreign to engineers at Autoworks than that of a time and motion study. Informants were certainly justified in their confusion, since, from the point of view of the person who is being observed, the ethnography does look remarkably like a time and motion study. Within the anthropological tradition, the basis for any ethnography is fieldwork. Pioneers of the method, such as Malinowski (1922) and Mead (1928), suggested that to understand the nature of a social system, researchers had to immerse themselves in the daily actions of those they were studying. Malinowski went to the archipelagos of New Guinea and Mead to Samoa to spend a considerable amount of time living and working with the individuals they were studying. Thus, like that of the scientific manager, the ethnographer’s method involves observing (Spradley 1980), documenting (Van Maanen 1988), and analyzing (Lofland and Lofland 1995) the routine actions of those under study. While most pundits argue that the most important tool in the ethnographer’s toolkit is his notebook, in which he writes field notes to record his observations, many researchers are now also finding value in collecting data in other ways, such as through the acquisition of digital artifacts.

I have found that the best way to conquer these misconceptions, or the ignorance, surrounding the goals of an ethnographic study is to do a really good job with the first person you observe. Make them feel comfortable around you. Do not ask too many questions that distract them from their work. And, most importantly, listen. People love to talk about their work, and they rarely have an audience content to listen to them do so. During my first observations at a site like Autoworks, listening well proved tremendously advantageous. Even though I was tempted to ask questions in my first observations because there were things I did not understand, I refrained. I listened when the informant talked and probed only in the areas in which he most clearly wanted to discuss. I have found that by following this practice, informants warm up to the researcher quite quickly. They tell their coworkers “this observation thing is not so bad,” and others become interested in being observed. If using this strategy, the researcher should plan that the first few observations will be directed at generating goodwill and buy-in from informants, and not necessarily collecting the highest-quality data.
Observation and Analysis

Why spend so much time observing?
Observation of people conducting routine activities lies at the core of the ethnographic method (Barley 1990a; Fine 1993; Geertz 1973; Lofland and Lofland 1995; Spradley 1980). Organizational ethnographers typically make two assumptions about people’s engagement with their work that undergirds support of observation. The first assumption is that although interviews are useful for uncovering attitudes and beliefs about work and for eliciting histories of and people’s interpretations of events, direct observation is necessary to capture a holistic understanding of the work culture in which people are embedded. Research has repeatedly shown that people have a difficult time articulating what work they do and how they do it (Collins 1974; Dreyfus and Dreyfus 1986; Orr 1996). Therefore, people’s actual conduct of work is the most revealing source of data an ethnographer can collect about the contours of a social system. Individuals’ actions produce and perpetuate a social system, but they are also influenced by it. Therefore, observing the actions people take to complete their work, when they take those actions, and how those actions are performed not only provides a descriptive understanding of “how” people work, but it can also help explain “why” they work in certain ways.

The second assumption is that an informant who is being observed in an organizational context performs certain actions because he or she believes such actions are necessary to fulfill his or her work role. In this sense, people’s actions do not lie. Although an informant may inadvertently or even purposefully alter his or her actions when under the watch of the ethnographer, people are notoriously bad at maintaining a facade for long. They are bad at it because trying to perform actions in ways that violate a normal routine often proves to be too much of a cognitive load for people to handle (Louis and Sutton 1991) and because, at some point, they have to get their work done and therefore cannot afford to dissimulate their practice for long (Roy 1959). The upshot is that even though they know they are being watched, people act the way they have to in order to get their jobs done. Howard Becker (1996: 62) provides a compelling example:

When we watch someone as they work in their usual work setting ... we cannot insulate them from the consequences of their actions. On the contrary, they
have to take the rap for what they do, just as they ordinarily do in everyday life. An example: when I was observing college undergraduates, I sometimes went to classes with them. On one occasion, an instructor announced a surprise quiz for which the student I was accompanying that day, a goof-off, was totally unprepared. Sitting nearby, I could easily see him leaning over and copying answers from someone he hoped knew more than he did. He was embarrassed by my seeing him, but the embarrassment didn’t stop him copying, because the consequences of failing the test (this was at a time when flunking out of school could lead to being drafted, and maybe being killed in combat) were a lot worse than my potentially lowered opinion of him. He apologized and made excuses later, but he did it.

In fact, most ethnographers seem to agree that, when in their work settings, even the informants who prove most theatrical after meeting the researcher slip back into their normal set of actions after about 30 minutes of observation. Thus, long, repeated stints of observation can reliably capture the “normal” and “routine” actions informants engage in to accomplish their work.

I typically capture the interactions that occur in each observation in a number of ways. I sit behind the informants at their desks while they work, and I follow them when they go to meetings and talk informally with colleagues. I accompany informants, like the engineers at Auto-works, to places like the corporate proving grounds to watch physical crash tests, and I also go along with them to vehicle tear-downs, where they are able to inspect the state of the parts after a physical test. During all of these activities I take notes on my small laptop computer, indicating the types of activities the informants are conducting, why they are conducting them, and with whom or what they are interacting. Additionally, I record talk occurring during all observations on a digital audio recorder. Using audio recordings allowed me to document the conversations informants have and to capture their personal thoughts about different matters, which I encourage them to speak out loud as they work. I also let the audio recorder run when engineers are working silently at their computers. All of the audio recordings are later transcribed verbatim. I integrate these audio recordings of dialogue with the field notes. By using the digital time stamp on the audio recorder in conjunction with the observation records, I am able to document how long informants work on particular tasks. The combined observation records (field notes with corresponding dialogue) for one observation are normally between 20 and 30 pages of single-spaced text. I have found that attending to all the details of an
engineer’s work typically makes it difficult to observe longer than three or four hours a day.

Why collect digital artifacts?
For ethnographic data collection of a visual culture, it is also important to obtain the copies of the digital artifacts with which informants work. Why not just describe the documents in the field notes? The example below helps to illustrate why securing the actual digital representations is so important.

I had spent two months at Autoworks trying to understand, from the engineers’ perspective, why they needed and wanted a new technology like CrashLab. At the beginning of my third month, I began to see a pattern in my observations that no one had articulated explicitly in any of the interviews I conducted. At Autoworks, design engineers (DEs) were responsible for generating the architectural drawings for a few parts for which they were fully responsible, but crashworthiness engineers functioned in an integration capacity. When it came time for an engineer to test a vehicle’s crashworthiness via simulation, he or she accessed the corporate parts database, found the computer-aided drafting (CAD) files for all of the parts that needed to be included in the model, and then assembled all of the parts into a model representing the complete vehicle.

DEs drafted and updated parts at many different junctures in the engineering process. A DE responsible for part A might change the shape of his or her part, while another DE, responsible for part B, might not. If the changes in the shape of part A made the part larger, it might then infringe on the space in which part B was located. If this happened, the DE responsible for part B would also have to redesign his or her part to accommodate the changes in part A. In a perfect world, a change in part A’s design would instantly trigger a series of changes to all of the other affected parts. However, many times DEs did not communicate with each other about changes in their parts, and they were often even unaware that changing a part would affect the design of another. As a result, when crashworthiness engineers went to download parts from the shared database, not all parts were in the same stage of development. As one engineer observed:

A lot of times I get the parts ready to assemble, and I find out that they don’t fit. Like two parts are occupying the same vehicle coordinates. That can’t happen.
Different matter can’t occupy the same space. You know? So obviously something is wrong. One DE updated one part and, who knows why, but the other DEs didn’t update their parts to match. So you have maybe two or three parts all intersecting and overlapping by maybe 10 or 20 millimeters. That’s a problem that happens a lot.

In the math-based environment of simulation, two parts can occupy the same space in a coordinate system, but in a physical environment that is not feasible. If this occurred, a crashworthiness engineer would discover, to his or her dismay, that the parts overlapped one other.

Many of the models that crashworthiness engineers assembled using CAD files from the shared database contained overlapping—or “penetrating”—parts, which significantly affected the solver’s ability to predict the results of a particular test. If a penetration was substantial, the solver would return an error message indicating that it could not solve the model until the overlapping parts were fixed. If a penetration was minor (just a few millimeters), the solver most likely would be unable to detect it, and the software would solve the model anyway. In that case, the simulation would produce results that could not be achieved in a real-world test scenario, because in the physical world, as the crashworthiness engineer I quote above observed, two parts cannot occupy the same space. To assure a high degree of correlation between simulation and test, it was essential for engineers to fix any major penetrations in their models that would affect their results. Identifying and resolving overlapping parts required, like so many other engineering practices, a series of trade-offs. Fixing every penetration in a model could take an engineer upward of a week—if he or she could even identify them all.

But penetrations did not always occur because of the lack of communication among DEs. Once a version of a particular part was drafted in CAD, the DE would place the electronic file on the shared server for the crashworthiness engineer to download. Engineers opened the CAD files and exported the line data (which represents the geometry of the part) into a finite element preprocessing tool. With these tools, engineers could discretize CAD geometry into finite elements, producing a workable mesh for analysis. Due to preprocessors’ discretization limits in computational power and constraints on the shapes elements could take, it was often difficult for an engineer to create a mesh that followed the geometry of a part with complete accuracy. Instead, most meshes looked similar to the
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mesh of the flange presented in figure 5.2. In this particular example, the geometry of the flange (indicated by a thin, light-colored line) specifies a hole in the center of the part. When generating the shapes of the elements, the preprocessor is not capable of exactly following the curvature of the hole, so the mesh extends past the geometry into what should be open space. When examining one part on its own, separate from the others in the system, the discrepancy between geometry and mesh that resulted from the discretization of a CAD file into a finite element file did not present much of a problem. However, when it came time to assemble a complete vehicle model, the hole in the flange had to be filled by another part. Since the mesh extended past the geometry of the flange, there was likely a penetration with whatever part was placed into the hole. Depending on the location, such a penetration could significantly alter the results of the analysis, making correlation between simulation and test (in which the flange and the part going through its hole could not physically occupy the same space as it could in a math model) difficult to achieve.

What I discovered during my observations was that DEs and crashworthiness engineers often got into fights with each other over part penetrations. Crashworthiness engineers would blame DEs for not enacting

Figure 5.2
Discrepancy between mesh and geometry (potential cause of part penetration).
some version-control mechanisms for their parts, and DEs would blame crashworthiness engineers for doing a sloppy job converting CAD drawings to finite element meshes. From my observations, I knew that this “blame game” caused many problems at occupational boundaries. For example, engineers from one occupation would blame engineers from another for delaying the launch of a vehicle or for causing the vehicle to fail critical government tests. What was unclear, however, was whether or not the blame that each group foisted upon the other was indeed warranted. Explaining how penetrations arose and whether people were justified in assigning blame would, eventually, turn out to be very important in predicting whether engineers would use the new CrashLab technology. Collecting digital artifacts like the one in figure 5.2 proved necessary, later during analysis, to make an empirical statement about the proportionality of blame to causes of part penetrations. By having copies of the parts in my database, I was able to locate instances in the field notes where blame occurred, and then index instances of blame to particular screenshots of parts. I could then analyze those parts, like the flange in figure 5.2, to determine whether the penetration occurred because of mesh translation or poor version control. Without these images, this analysis would not have been possible, and I would not have been able to explain a crucial factor for why CrashLab was accepted by some groups and rejected by others.

To index field notes to digital artifacts requires a great deal of discipline in the writing up of field notes. The method I have found most successful is illustrated in figures 5.3a, b, and c. Figure 5.3a is an excerpt from the observation record of two engineers at work: Damen and Balaji. This excerpt shows text taken from field notes interspersed with lines of dialogue transcribed from digital audio recordings. The field notes were taken in real time on my laptop as informants worked, and the dialogue was captured with a digital audio recorder. At the end of the day, I transcribed the audio and inserted it into the appropriate location in the field notes. Each time an informant uses an artifact, a note is made in the observation record indexing that particular artifact. Figure 5.3b is a reproduction of a printed file from a Microsoft Excel spreadsheet, and Figure 5.3c is a screenshot of a part the engineers were working with.

To move easily between the observation record and the digital artifact, I place all of the artifacts into a database (which I usually get a research
12:25

Damon picks up a piece of paper off the table to the left of his monitor. The paper has a table on it. There are four columns and over ten rows. There is handwriting along the right side of the table. [See Att. # 5]

Me: What’s on that piece of paper?
D: It’s a matrix of the iterations. I’m just making sure I have all the changes necessary for a specific run.
Me: Are these runs you still have to do, or ones you have done already?
D: I’ve done [he counts with his finger] five. Today Bilaji did the bottom four and there are five in the middle I’m preparing. It just happened that these three are very easy to do once you get the first one done. The second one’s kind of stand alone. The next four depend on the first two, so they kind of come in bunches.

Damon turns back to his computer. He moves through several menus at the bottom of the Hypermesh screen. He brings up a new image. The images of the CBS bar, with two inserts attached to the brackets Damon has been welding on. [See Att. #6]

12:30 pm

Damon calls over to Balaji who is working at his computer in the adjoining cube.

D: Hey Bilaji, these two are almost done. Do want to give me a control file or do you want me to create it.
B: Take it from the V-store.
D: Just grab one?
B: Yes.
D: Once I do it can I put it back in?

Damon and Bilaji swivel in their chairs to face each other. Bilaji picks up a piece of paper off his desk. It appears to be the same paper Damon has. They both hold their papers low over their laps and look down at them. Although they face each other, they do not look up. Bilaji glances up at Damon occasionally, but Damon remains looking down.

B: Just put it from where the rest are.
D: That way they’ll be in the same spot.
B: Yes.
D: In 4a, what’s the difference between that and 2b?
B: 2b is going to be [missing] brace.
D: Yes.
B: Yeah. Don’t worry about 4a. Did you finish 4a?
D: No, because –
B: Don’t worry about 4a. You can call this point 7. See, that’s the one I yesterday deleted the footprint.
D: How about I called it 4a then so there’s no 2b.
B: Yeah.
D: So 4a is 0 point 7. [Writes on the piece of paper]
B: Correct. Now you see the bottom two ones?
D: The bottom two? Yes.
B: I submitted those yesterday where I modified that.
D: Do you want it modified? 4a?
B: Probably not. I think we get enough information about this.
D: Because all you did was to cut a flange.
B: Yeah, I cut the footprint really close to the rail. I deleted those elements too.
D: It’s up to you.
B: I think we’ll get enough information from the rest. Make it point 7 and see if it [missing].
D: Ok. Hey, all the jobs we submitted yesterday went in. None of them crashed.
B: No, that’s good.
D: Yeah.

Figure 5.3a
Excerpt from completed observation record.
Figure 5.3b
Photo copy of attachment 5.

assistant from computer science to create). I then place hyperlinks in the observation record that will transport the researcher to the artifact in the "artifact database." In this way, I am able to keep close tabs on both the action and the artifacts that help comprise the action. By arranging observation records and artifacts in this way, the analysis of the image presented in figure 5.2 is made possible.

By taking such care to integrate field notes, recorded dialogue, and digital artifacts into a single observation record, the researcher is poised
Figure 5.3c
Screenshot of attachment 5.

for a more fine-grained and trenchant analysis of ethnographic data than a simple interpretive coding allows (see, for discussion, Lincoln and Guba 1985; Taylor and Trujillo 2001). Such “analness,” as my students sometimes call it, allows the researcher to engage in an “action approach” to data analysis. Such an approach takes as its primary unit of analysis the actions informants take as they conduct their work.

Actions are small but concrete types of behavior. Reading an error message from a computer screen is an action; calling someone on the phone is an action; placing an accelerometer on a model is an action; and submitting a math model to a solver is an action. Actions can be observed directly and recorded by taking notes. Once they are in written form, they can be read, their text can be pointed to, and they can be given a code. Concrete actions can be grouped together into events—the collection of multiple, smaller actions (Becker et al. 1961: 26–28). Thus, the actions of
picking up a piece of paper and calling someone on the phone can be grouped into a trying-to-discover-why-a-model-bombed-out event, just as the actions of strategically placing an accelerometer and submitting a model to a solver can be grouped into a preprocessing-a-model event. These events can then be subjected to the kinds of techniques of analysis (e.g., Strauss and Corbin 1998) that allow the researcher to understand why they occur, what their consequences are, and how they connect to other events.

What is the point of such a detailed coding of actions? Triangulation. Becker (1958: 656) suggests that if observations are coded at the level of actions, they can be standardized in a form “capable of being transformed into legitimate statistical data.” Counting the number of actions taken by informants in the course of their work can be a simple way to triangulate the findings made through the qualitative interpretations of the data. For example, Barley (1990a) suggests that as he began to sort his data in a qualitative manner, he noticed a subtle shift in the interactions among his informants. The analysis seemed to suggest that the new computerized imaging modalities he watched informants use were shifting the power relations among actors in the social order of the two hospitals he observed. By counting identifiable actions from his notes (e.g., “usurping the controls” or “giving someone directions”), Barley was able to verify quantitatively the insights generated by his qualitative sorting of the data (Barley 1986, 1990b).

Although an “action approach” to data analysis can allow the analyst to sum qualitative data in quantitative form, which can help confirm or deny the theories that emerge from analysis, it can also help reveal more subtle and consistent patterns in the data. By coding specific actions from the data, an analyst can construct data matrices that can be statistically analyzed. For example, using simple tests such as an analysis of variance, or ANOVA, can help determine whether or not actions occurred more frequently during one period than during another, or if they were conducted more consistently by one group than another. Thus, by coding actions, the ethnographer can triangulate the descriptive and inductive findings of qualitatively coded data with simple quantitative analyses of mean frequencies, which might help uncover patterns in the data that would otherwise be overlooked due to its sheer volume.
To analyze the data I collected on crashworthiness engineers’ work and the activities of those who implemented CrashLab, I followed the steps outlined above. I began by coding all field notes in the qualitative software analysis program Atlas.ti for the specific actions informants conducted in their work. I coded more than 100,000 lines of text. My codes revealed over 500 different kinds of actions. I then sorted and grouped the actions together into larger events of which they were constitutive, revising categories when I uncovered more data that suggested revision was necessary and after discussing my emerging categories with my informants (Miles and Huberman 1984). I did these discussions informally at the end of observations and, more formally, during the monthly presentations I was required to make under the auspices of my “internship.” Once this basic coding and sorting process was complete, I followed the more advanced, theoretically grounded steps outlined by Strauss and Corbin (1998). I then followed Glaser’s (1978: 120–126) method of theoretical sorting to determine which events were most important in explaining how CrashLab merged with the social systems of crashworthiness engineers at Autoworks. After identifying a number of ideas and theories about relationships in the data, I took counts of those activities and/or events that seemed most important. I used several statistical techniques to compare the occurrence of these actions and events across time and across groups.

How to collect digital artifacts
All of the above discussion presupposes that the researcher will be able to acquire the digital artifacts with which informants work. How is this done? The answer to this question has two parts, what I call the pitch and the carry.

In the first part, the researcher has to make a pitch to the informant about why he or she needs the artifact. I have found this is often hard to do if the researcher does not explain at the observation outset that he or she will be asking for artifacts. I typically begin my observation with a given informant by letting them know I am trying to understand how they do their work. I remind them that they carry out much of their work by using digital representations and other digital artifacts and so, at times, I may want to have copies so I can have a full understanding of the complexities of their job. At this point, informants fall into two typical
categories of response. The first category represents the acceptors. After this pitch they usually say “okay” and then start work. The second category represents the critics. After the initial pitch, the critics are concerned that giving you copies of their digital artifacts will somehow get them into trouble. This is where the ID badge and the email address that I spoke of above come in so handy. When a critic responds with his or her concern, I point to my ID badge and remind them that although I work for the university, this study is supported by the company’s management and they are paying me as an employee (both true statements). I then tell them that all of the artifacts they provide me are protected under a confidentiality agreement (a true statement) and that they can give me the documents by emailing them to me at my company-sponsored email address. 

The email address is one more stamp of the researcher’s legitimacy and provides the illusion that all of the data that they send will remain at the company (not exactly true). Typically, these actions put even the critics at ease. I find it best not to clear up the ambiguity in their interpretation that the artifacts they give will stay behind the company firewall. My negotiation with the company’s management is explicit that I can take artifacts with me. I have found, over the years, that it is easier and more beneficial not to volunteer too much information about all of this for fear of alarming the critics. If (and this has happened) the organization will not provide me with a corporate email account, I will ask the direct manager if he/she will create a temporary folder in his or her email client to store artifacts that people will send. I then have the critic send the artifact via email to their manager (they have no problem doing this) with my name in the subject line, the manager files it away, and later I arrange with the manager to take those artifacts off his or her hands.

As should be clear from above, I find that sending digital artifacts via email is the easiest way to carry them with you. But it does require some logistical maneuvering to get all of the artifacts that someone works with. My normal strategy is to ask informants upon our first meeting to create a temporary folder on their computer desktops. I typically ask them to name it something like “Files for Paul.” Throughout the observation sessions, whenever I ask for a screenshot or copies of a presentation, I ask them to save a copy to this folder. Most engineers know how to make screenshots because they use these shots often for reports. But some informants do not know how to make screenshots, and I need to teach them
how to do so. At the end of the observation session, I ask informants to attach as many artifacts as will fit into an email and send them to me in as many emails as are needed. Doing things in this way is less of a burden to the informant and increases one’s likelihood of carrying away the digital artifacts one deems necessary to fully understanding the work.

Writing it Up

Writing it up is the most difficult part. How do you summarize months of observation and mountains of data into a short article? This is the dilemma that many ethnographers face. To deal with it, many ethnographers publish books where there is more room to present data and tell a wider story. But many social scientists do not find themselves in “book fields.” Instead, they are expected to publish the results of their ethnographic study in journal articles where they are normally limited to 20–30 pages of double-spaced text (don’t forget the one-inch margins!). On top of these normal constraints, ethnographers of visual cultures, like engineering, are faced with yet another obstacle: what to do with all of these digital artifacts?

When I first began writing up my findings for publication in journal outlets, I made a mistake that is common to all ethnographers. I assumed that because I learned it in the field, the reader needed to know it too. The result of this mistake was that I found myself including more excerpts from my field records and more screenshots of digital artifacts than were necessary to convey the key points I was exploring in my article. Very quickly, journal editors pushed back. They said things like, “Why do you need all these images?” “We can’t publish all of these figures.” “Do you really need the screenshot to tell your story?” In reflecting on these comments, I discovered that I often mistook my needs for analysis with my needs for data exposition in the article. In other words, having a digital artifact like the one presented in figure 5.2 was essential to my ability to do a good analysis of the data. But I did not need to show the image to tell the story.

To give a concrete example, my article entitled “Innovation Blindness” (Leonardi 2011a) chronicled the development of CrashLab at Autoworks. The thesis was that various groups often fight over ideas for new technologies because they do not recognize that they each believe the
technology should solve a different organizational problem. When the
groups get together, they often bring prototypes of potential technological
solutions to their specific organizational problems as a means of focusing
conversation. But these prototypes focus conversation around features of
the technology and obscure the fact that those features were created with
the hopes of solving a particular problem that may be different from the
concerns held by other constituents. The result is that the development of
the technology is mired in disagreement. To help illustrate this process—
and to explain why it took 10 years and several million dollars to develop
CrashLab—I included in the submission three images of various proto-
types that engineers brought to their meetings. Here is what two of the
reviewers had to say:

**Reviewer 1:** I understand your point about the differences in features. But why
do we need these images? I don’t think they really add anything. They just show
three prototypes that all have different features. I don’t think there is really any
value-add with these images.

**Reviewer 2:** The images are cool, and they really help to illustrate your point.
But I doubt the journal will let you publish them. I would find some way to take
them out and replace them with a table or something. I know it’s less engaging to
the reader, but I just can’t see the journal putting these in for space reasons. Also,
did you even get permission to publish these? Would “Autoworks” be upset if they
saw these images?

The senior editor concurred with the reviewers and asked me to remove
the images. I ended up publishing a simple table that compared the vari-
ous features of the different prototypes and putting an asterisk next to the
features from those prototypes that eventually made it into the final ver-
sion of CrashLab. What is interesting about these reviewer comments is
that they show people's reticence and discomfort with the publication of
images. Looking back, I can see that a table worked well enough to con-
voy the message I was trying to send. It is interesting, though, that the
concerns about publishing images were not confined to whether they
added value or not, but, as reviewer 2 suggested, it is unclear to most how
to handle images. Will the journal have space? How do you indicate that
you received permission to publish them?

I provide this example to underscore the point that although a
researcher may have worked hard to gather digital artifacts during his or
her fieldwork, spent egregious amounts of time writing up the observa-
tion record in a way that indexes those images, and slaved through a
detailed analysis of the data that included an informant’s use of the images, one should not be under the illusion that these images will appear in print. The digital artifacts an ethnographer collects provide essential information about a visual culture. This information is of primary value for analysis rather than exposition of data. I believe this is an important point to remember.

Confidentiality Matters

There are times when the presentation of some visual image is both warranted and acceptable by editors. When presenting images, the researcher has to be careful to maintain the anonymity of the research site. Many times, names of files that are particular to the company appear in screenshots, or the numbers in a table or the data points in a graph are confidential information. At Autoworks, I ran into the issue that the company’s legal team was very concerned whenever I would present a graph that summarized the results of a simulation analysis. They were concerned because sometimes, as engineers worked through various design iterations, they generated solutions that would provide outstanding performance—for example, a five-star frontal crash test rating from the National Highway Traffic Safety Administration. But because of the politics of the design process, the overall structure of the vehicle might evolve to favor fuel economy rather than occupant safety. For this reason, the legal team was very concerned that if I showed actual data that a safer vehicle was possible than the one that was eventually built, Autoworks would be open to a lawsuit.

To deal with this issue, I had to make some alterations to the story and the data to maintain the anonymity of the research site and those involved when writing about CrashLab. This entailed changing names, dates, relationships between people and departments, and, in some cases, even descriptive statistics about the company or about the technology or vehicle-development process. As Kunda (1992) suggests, I maintained relations between numbers to illustrate analytic points. To be completely explicit, I fabricated all of the vehicle impact-performance data that I have ever presented in charts and figures in articles or chapters. I always provide a note indicating that the data are fabricated in the publication. I have maintained the original shape of the curves or the magnitude of the
results in such cases, but the data are not by any means the real data with which informants were working. Thus, to include digital artifacts and present the data within a limited number of pages while still achieving a readable and understandable narrative, researchers will have to, as I did, take a bit of liberty with the presentation of events in order to construct for the reader what Fine (1993: 273) calls the “illusion of verisimilitude.” This means that the researcher will have to relate some events out of sequence, positioning them in the story as though they occurred at roughly the same time. Additionally, he or she will have to make deliberate choices about which parts of the story to tell and which parts to leave out. So, although the researcher attempts to provide rich data, there is always more to the story than meets the eye (Golden-Biddle and Locke 1997).

Conclusion

I believe the ethnographic method can provide important insights into worlds we know little about, while at the same time helping us abstract findings from those worlds in ways that help explain others. These insights can only be derived from intently watching people—studying them in the course of their everyday work. In a visual culture like engineering, watching people at their jobs also means dealing with the reality that they work constantly with digital artifacts. As I have tried to convey in this chapter, these images thoroughly shape the way people think, see, reason, and interact in the workplace. I have tried to provide some lessons I have learned through trial and error while conducting ethnographic studies in visual cultures.

Although I have spent a good deal of time explaining how to deal with the realities of collecting and analyzing digital data in a physical space, I would encourage ethnographers of visual culture to remember that without careful watching and recording of what informants do, even the richest research site will bear little intellectual fruit. Watching is tedious, but enlightening. Informants regularly joked with me that I had watched them for so long that I could do their jobs. Indeed, I did feel a bit like the Cantaleño apprentice loom operators in Manning Nash’s (1958) vivid portrait of the industrialization of a Guatemalan village, who learned how to work the looms by sitting quietly behind the experienced operators for months at a time without ever touching the machine. I will always
be amused by my informants’ disbelief that I could actually enjoy what I was watching. As two informants joked with me one day:

I1: How can you sit there and watch us so long? I mean it’s like you must be bored out of your mind. It’s like on TV, what kind of shows do you see? There’s hospital shows, lawyer shows, and detective shows. You never see any engineering shows. How come? Because it’s boring, man! I wouldn’t watch an engineering show.

I2: Hell no! Can you imagine it? “Vijaykumar P.E. The world’s most dangerous performance engineer.” He doesn’t drive a cool car, but he designs one.

I1: Yeah, you probably write, “This guy maximizes a window, minimizes a window, wiggles the mouse, and maximizes the first window again.” You must be falling asleep. What can you learn from that?

I2: Maybe you should forget this whole ethnography thing and just do a time and motion study on us. Then you would only be bored for like a week and you could go home and lay on the beach in the sun.

They were right. It would have been much easier to collect data if I were conducting a time and motion study. But the visual culture of engineering is so rich with the potential for understanding fundamental sociological questions that watching, learning, and struggling with digital data makes even the prospect of a sunny day at the beach a little less exciting.

Notes

1. A solver is a program run on a supercomputer that applies equilibrium equations to each element in a finite element model and constructs a system of simultaneous equations that is solved for unknown values to produce measures of a vehicle’s performance given the parameters specified in the setup of a simulation.

2. For a discussion, see Barley and Kunda (1992) and Shenhav (1995).

3. Finite element preprocessors have several automated routines to control the shape of elements. Element shape is an important concern for the accuracy of a model because elements are used to ensure quality.

4. Diane Bailey at the University of Texas at Austin School of Information invented this system. I am indebted to her for teaching it to me.

5. Vijaykumar is the informant’s last name (pseudonym of course). He is mocking the popular 1980s TV show Magnum PI.
References


